The impact of the national minimum wage on the labour market outcomes of young workers

Jan Fidrmuc* and Juan de Dios Tena Horrillo†

Abstract

In this paper, we analyze the impact of the national minimum wage (NMW) in the UK on employment of young workers. Our methodological approach is two-fold. First, we address the impact of the age-related increases in the NMW at the ages of 18 and 22: workers below these limits are subject to substantially lower rates than their older counterparts. Second, we look at the impact of the NMW on employment more broadly by considering the impact of NMW increases since its introduction in 1999. Contrary to the previous literature we find evidence of a negative effect of NMW using both approaches.

Keywords: minimum wage; employment; unemployment; young workers

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* Department of Economics and Finance, and Centre for Economic Development and Institutions (CEDI), Brunel University; CEPR, London; and WDI, University of Michigan. Contact information: Department of Economics and Finance, Brunel University, Uxbridge, UB8 3PH, United Kingdom. Email: Jan.Fidrmuc@brunel.ac.uk or jan@fidrmuc.net. Phone: +44-1895-266-528, Fax: +44-1895-203-384. Web: http://www.fidrmuc.net/.

† Departamento de Estadística, Universidad Carlos III, C/Madrid 126. 28903 Getafe (Madrid), Spain. Phone: +34 916241259. Web: www.est.uc3m.es/jtena.

1 Introduction

The imposition of a mandatory minimum wage, whether at national, regional or industry-specific level, is a controversial aspect of economic policy. Standard neoclassical economic theory, based on the assumption of competitive markets, predicts that the minimum wage should either have no effect on employment (if set at a sufficiently low rate) or it should lower employment by preventing the workers at the bottom of the wage distribution from finding work at market-clearing wages that fall short of the minimum wage. Correspondingly, it has been speculated that the high level of minimum wages (relative to average earnings) in Europe contributed to the high unemployment rate there compared to the US. Once we relax the assumption of competitive markets, however, the theoretical prediction can change dramatically. Assuming monopsony in the labour market, for example, can yield a positive employment effect of imposing a minimum wage (Dolado et al., 1996). In particular, a monopsony employer can push wages below the marginal product of labour and thus maximize profits while depressing employment below the level that would prevail under competitive markets. Imposing a wage floor then should reduce the employers' profits and increase overall employment.

To date, the empirical evidence on the employment effect of the minimum wage is equally mixed. A recent review by Neumark and Wascher (2007) concludes that the bulk of studies point to a negative employment effect of introducing (or increasing) the minimum wage, both in the US and in other countries. Workers who are most likely to be affected by the minimum wage, such as young workers and the low-skilled, are said to experience especially large disemployment effects (nevertheless, they find that the negative effect is mitigated somewhat when young workers are subject to different, lower, minimum wage rates). The range of estimated elasticities, however, is very broad: from significantly negative to significantly positive. This resonates with the findings of an earlier overview study by Dolado et al. (1996) who consider the employment effect of minimum wage rules in France, the Netherlands, Spain and the UK. Their results are similarly inconclusive, ranging from negative effects (especially for young workers again) to positive effects.

Hence, the distinguishing feature of the current discourse on the employment effect of the minimum wage is the overwhelming lack of consensus. A popular, though controversial, view posits a positive effect of the minimum wage on employment. A particularly well-known and influential example is the study by Card and Krueger (1994) who find a significant positive employment effect of a minimum-wage increase.

Despite the potential negative employment effects, rules mandating minimum wages are very common at present. Most developed industrialized countries and many less-developed countries impose minimum wages for all or at least some workers. (even Hong Kong, which traditionally espoused a laissez-faire approach to regulation, recently introduced a minimum wage). Moreover, countries that do not have a centrally mandated minimum wage, such as Austria and Germany, often instead put in place a framework for industry-wide wage bargaining with the outcome binding for all firms within the industry. An unprecedented step was taken by Ireland in February 2011 when it reduced its minimum wage by €1 (from €8.65 to €7.65, corresponding to a 12% cut); this move was justified, in part, by the need to increase labour market flexibility and ensure that Irish firms remain competitive during the crises afflicting the country.

The UK introduced its current national minimum wage (NMW) relatively late, in 1999.Until 1993, the Wages Councils had the power to set minimum wages for specific industries. Since 1999, the employment effects of the NMW's introduction and its subsequent

annual increases have been studied by a number of contributors (Stewart, 2004; Dickens and Draca, 2005; Dolton, Rosazza-Bondibene and Wadsworth, 2009; Dickens, Riley and Wilkinson, 2009, among others). The general conclusion of these studies is that there is little evidence to suggest that the introduction of the NMW has had an adverse effect on employment. Similarly, the subsequent NMW increases were found to have had no disemployment effect, despite the fact that the NMW increased significantly faster than average earnings in the UK economy (Dickens, Riley and Wilkinson, 2009).

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 fact that different rates apply to young workers depending on their age. Since its introduction, the UK NMW has featured different rates for those between 18 and 21 years of age (the so-called development rate) and for those who are 22 and above (adult rate). This allows employers to hire young workers at a discount relative to the adult rate: the ratio between the adult rate and the development rate has remained approximately 1.2 since 1999. In 2004, a separate rate was introduced for those aged 16 and 17 (who were exempt from the NMW until then). The ratio between the 18-21 and 16-17 rates has been approximately 1.35. In turn, this also implies that young workers who earn only the NMW rate relevant for their age experience a sharp wage increase upon turning 18 and then again at 22.[1] While productivity is likely to increase with age, workers who are 22 are at best marginally more productive than their 21-year-old counterparts. Therefore, if the NMW affects employment, this should be especially apparent in the case of young workers who move from falling under the reduced development rate to the adult rate.

Our study was undertaken at a time when the UK and most other countries were coming out of a particularly severe recession. Economic decline exerts downward pressure on real wages. Yet, nominal wages tend to be downward sticky. While there are multiple reasons for this, the presence of a legally-binding minimum wage may be one of them. Therefore, even if the minimum wage has little adverse employment effect during periods of healthy growth, it may well have more bite during a contractionary period. This is especially so for young workers who, being less skilled and less productive than older workers, tend to occupy a rather marginal position within the labour market. As such, they should be more vulnerable to adverse effects of labour market tightening. Therefore, we also address the question of whether the employment effect of the NMW depends on the current phase of the business cycle or the tightness of the labour market. In this part of our analysis, we consider not only youth but extend the scope to cover all young and middle-aged workers. The focus of our analysis shifts therefore from the employment effect of age-related NMW increases to the effect of annual increases in the various NMW rates (including, but not limited to, the 18-21 rate).

The impact of the age-related increases in the NMW on the employment of young workers in the UK has been investigated in earlier research by Dickens, Riley and Wilkinson (2010). They apply a regression-discontinuity approach to investigate how the employment status of low-skilled young workers changes when they turn 22. They find, somewhat surprisingly, that low-skilled young workers who turn 22 are significantly more likely to be employed and significantly less likely to be either unemployed or out of the labour force. Such a positive effect of the higher rate for those aged 22 and over is counter intuitive. Nevertheless, they

[1] However, in most of our analysis, we primarily focus on those subject to the 18-21 rate. The labour market position of workers aged 16-17 is substantially different from that of their older counterparts: they are more likely to remain in full-time education and their employability is lowered by restrictions such as not being allowed to sell alcoholic beverages. 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.

attribute this to an increase in labour supply by young workers: if the lower development rate is below the reservation wage of some workers, such workers postpone their labour market entry until after they can be certain of earning a sufficiently high wage. The positive employment effect for low-skilled workers, moreover, is remarkably robust: they find no significant effect when workers reach 21 or 23 years of age (falsification tests) and neither do they find any effect at the age of 22 before the minimum wage was introduced in the UK. However, their result disappears when they consider all workers rather than only the low-skilled ones.[2] This is especially peculiar as the positive effect of the NMW seems to apply to those types of workers, who are most likely to be paid the minimum wage and, according to the evidence surveyed by Neumark and Wascher, should therefore be more adversely affected by the minimum wage than the workers overall.

In this paper, we revisit and explore further the result of Dickens, Riley and Wilkinson (2010) with a somewhat longer data series. In contrast to their analysis, however, we focus primarily on all workers. Young workers are generally more likely to be subject to the minimum wage and this seems more or less independent of their skill level. In fact, Dickens, Riley and Wilkinson report figures that show 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 aged 21 earn less than the adult rate.[3] Therefore, while we also present results for low skilled workers alone, we believe that focusing only on such workers is not justified. In line with their result, we find that when considering all workers, the effect of turning 22 on employment is not significant. Surprisingly, however, we find that male workers who turn 21 are less likely to remain employed. While reaching the age of 21 has no effect on the minimum wage, this finding may be consistent with employers anticipating the wage hike that would occur at 22 and shedding workers approaching that threshold well in advance of their reaching it. When considering the effect of turning 18, we find again a negative effect of turning 18; moreover, the negative effect is found both for males and females.

We then probe the employment effect of the NMW further by implementing a difference-in-difference analysis of minimum wage increases. Our analytical approach is thus similar to those of Stewart (2004) and Dickens and Draca (2005), except we consider all increases between the introduction of the NMW in 1999 and 2009. In contrast to the previous literature, we find that increases in the NMW rate translate into employment losses, while also encouraging labour market entry, thus rendering the net effect potentially ambiguous. When we extend the analysis to allow the minimum wage effect to differ depending on the tightness of the labour market, we find that recessions tend to increase the bite of the NMW, again for both employment and labour market entry.

After briefly discussing the data used in our analysis in Section 2, we present results of the discontinuity analysis in Section 3 and the difference-in-difference findings in Section 4. Section 5 concludes the paper by summarizing the results and suggesting some tentative avenues for further work.

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

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

2 Data

Our analysis is based on the UK Labour Force Survey (LFS). The LFS is a quarterly nationally-representative survey of households across the UK, covering approximately 60 thousand households and over 100 thousand individuals aged 16 and above each quarter. 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, importantly, their labour market outcomes. Since the NMW was introduced in 1999, we use all quarterly datasets available since then. Our data thus span the period from April-June 1999 to October-December 2009, pooling all available LFS waves during this period.

The LFS contains information on the precise date of birth of every respondent.[4] We use this information to compute the age of each individual in months. Crucially, we also have the date the survey was carried out. By comparing these two dates, we can determine the precise age of each respondents in months, on the day when the survey was carried out. We thus know exactly whether a particular individual is 21 or 22 at the time of the survey, even when their birthday falls within the same month in which they were interviewed. As is common in the regression-discontinuity literature, we redefine age so that it takes the value of 0 in the month when the individual reaches the threshold age of 22 (or 18) years.

Our treatment of age differs slightly from that implemented by Dickens, Riley and Wilkinson, who only consider the year and month in which the respondent was born and compare this with the year/month when the survey was carried out. As a result, for each discrete age in months, some respondents are in fact falling short of that age according to their approach while all respondents are correctly aged in our analysis.[5] Dickens, Riley and Wilkinson therefore use the information on age in years, also contained in the LFS, to correctly classify those respondents who appear to have reached the threshold ages of 18 and 22 years, without similarly correcting the age in months of the remaining individuals.[6]

Besides the standard socio-economic characteristics, we also utilize the information on the respondents' employment status and wages. For the latter, the LFS contains two alternative measures of hourly wage: *hrrate* and *hourpay*. Dickens and Manning (2004) and Dickens and Draca (2005) argue that the latter is less accurate as it is subject to measurement error: while *hrrate* is obtained by asking the respondents specifically what their hourly wage rate is, *hourpay* is computed by dividing their weekly earnings by the number of hours worked per week. Therefore, we put more weight to the results obtained with *hrrate*. Finally, the variable *hrrate* was missing in one of the LFS datasets, the one for the first quarter of 2001. Therefore, this quarter is excluded from our difference-in-difference analysis.

[4] 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 making the restricted release of the LFS available to us.

[5] For example, consider the case when a group of respondents, all born in April (of any year) are interviewed on 15th April. When considering only the month and year of birth, it would appear that all of them have already passed their birthday. One needs to therefore use also the date of birth to determine the true age of each individual.

[6] While in principle this methodological difference should be rather innocuous, it may be one of the reasons for some of our results differing from those of Dickens, Riley and Wilkinson

3 Employment Effect of NMW on 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). Specifically, we consider individuals who are within 15 months of their 22nd birthday. Since, as explained above, we redefined age so that it takes the value of 0 when the respondent reaches the relevant threshold, our analysis considers young workers whose ages fall between -15 and 15 months. As a robustness check, we replicate the analysis also for 12 and 6 month intervals.

Our approach is initially similar to that of Dickens, Riley and Wilkinson (2010). We are interested in whether the probability of being employed, unemployed or inactive depends on age and especially whether it differs for individuals who are younger and older than 22. In particular, let y_i be a dummy variable denoting whether the individual is employed (unemployed, inactive). Then, we are estimating the following equation:

$$\mathrm{y_i} = \mathrm{F}\begin{pmatrix} \theta + \alpha_0 * age_i * (1 - dum) + \alpha_1 * age_i^2 * (1 - dum) \\ +\alpha_0^* * age_i * dum + \alpha_1^* * age_i^2 * dum + \beta * dum \end{pmatrix} = \mathrm{F(u)} \quad (1)$$

where F is normal distribution function, age_i is the age in months less the threshold (264 months for 22 years) and *dum* is a dummy taking value of 1 when the individual is 22 or older. For our baseline results, 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 some regressions for females, the quadratic specification was rejected and we therefore also considered a cubic specification). Crucially, we allow for the effect of age to be different before and after the individual passes the threshold age. The effect of reaching the threshold age is thus found by combining the coefficient estimated for the discontinuity dummy with the different effects of age before and after the threshold[7]:

$$F(\theta + \beta) - F(\theta - \alpha_0^* + \alpha_1^* + \beta) - F(\theta) + F(\theta - \alpha_0 + \alpha_1) \quad (2)$$

Table 1 reports the results obtained for the probability of being employed. We present regression results for all individuals and for males and females separately as well as with and without additional control variables. Unlike Dickens, Riley and Wilkinson, we consider all individuals, regardless of their skill level: as we argued above, young workers, whether skilled or unskilled, are nearly equally likely to be paid the NMW. The row denoted *discontinuity* reports the combined effect of the discontinuity dummy and the change in the coefficient estimates for the age polynomial. *Dum*, in contrast, stands for the coefficient estimated for the discontinuity dummy alone. Dickens, Riley and Wilkinson, 2010, focus only on the sign and significance of this latter coefficient, which we believe ignores a potentially important part of the effect of surpassing the age threshold.

As is clear from Table 1, neither the full discontinuity effect nor the dummy on its own are significant. This is in line with the findings of Dickens, Riley and Wilkinson (2010) who also report an insignificant result when they include all individuals rather than only the low skilled ones. Hence, we find no significant effect, whether positive or negative, of turning 22 on young workers' employment.

Next, we seek to replicate Dickens, Riley and Wilkinson's analysis. To do so, we only consider respondents with low qualifications: those who left school at the age of 16 with the

[7] In non linear models, the marginal effect of a change in two interactive variables (age and dum) is not equal to the marginal effect of changing just the interaction term. Moreover, the sign may be different for different observations. Norton *et al.* (2004) explain how to compute interactive effects for probit models and we adapt this procedure to our particular case.

so-called GCSE qualifications and those who report having no qualifications. Recall that they found a significant positive effect of turning 22 for low-skilled workers, suggesting that becoming eligible for the adult NMW rate increases rather than reduced employment. Our results are presented in Table 2. As we emphasized before, while Dickens, Riley and Wilkinson focus on the sign and significance of the discontinuity dummy, we believe it is important to consider the combined effect of both the dummy and the changed estimates for the age polynomial. Our results are broadly in line with those of Dickens, Riley and Wilkinson but appear somewhat weaker.[8] In particular, while the discontinuity dummy is always positive, it is never significant for females; it is significant for males and for all workers but only in the 5-10% range. More importantly, the combined effect of the discontinuity dummy and age polynomial is never even close to being significant.

Tables 3 and 4 present the regression results for unemployment and inactivity, considering all workers regardless of their skill level. Again, the full 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 Dickens, Riley and Wilkinson (2010); as we argue above, accepting this as the true effect of the discontinuity would be wrong as it ignores the fact that the effect of the age polynomial is also allowed to change 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 of our analysis. In Table 5, we present 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 impacts employed and unemployed workers differently: while some of those already employed at 21 may lose their jobs, others may only enter the labour market or intensify their job search when they are 22, attracted by the higher wage. If this were the case, then the overall effect, presented in Table 1, could be insignificant because it combines these different effects. The analysis is again presented separately for males and females (to save on space, we are omitting the results for all workers). In the first two columns of Table 5, we present the estimates for the probability of remaining employed, conditional on being employed already. The estimated effect of turning 22 is negative, especially for men, but it is not even close to being significant at the conventionally accepted levels. Hence, young workers who were employed at the age of 21 are no 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 labour market status of young workers just before they turn 22 makes little difference to our findings.

[8] Note that while we attempt to replicate Dickens, Riley and Wilkinson'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 different or are formatted differently.

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

As the last robustness check, we perform falsification tests for workers turning 21 and 23 (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, employers may seek to dismiss workers in a way that could not be easily construed as motivated by the age-related NMW increase. If this is the case, then we might expect the employment effect to take place at some point before or after the workers turn 22. This is indeed what appears to happen: 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. The fall in employment probability at 21 years 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. Note that this negative result only appears when we consider the combined effect of the discontinuity dummy and the different coefficients for the age polynomial: the dummy alone is not significant. This again highlights the importance of assessing the full effect of the discontinuity and the changed effect of age rather than considering only the coefficient of the discontinuity dummy. Finally, we also replicated the discontinuity analysis at 21st, 22nd and 23rd birthday with 6 and 12 month estimation windows instead of 15 months used to generate the results discussed so far. In general, the regressions generate weaker results (see the Appendix). Those obtained with the 6 month window are never significant. This may be due to the lower number of observations with the shorter estimation window. Moreover, the discontinuity effect may take time to become sufficiently pronounced. The regressions utilizing 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 marginally significant at 10% while the latter is not significant.

The finding of a significant negative effect for males at the age of 21 is interesting and perplexing at the same time. Therefore, we pursue it further and consider the discontinuity for every age in one-month increments between 20 and 23 years. Since we estimate dozens of coefficients, it is more instructive to depict the results graphically. Figure 1 presents the results for males. The solid line captures the employment effect while the dotted lines correspond to the 95% confidence interval. An interesting pattern emerges: the probability of being employed dips in the neighborhood of both the 21st and 22nd birthdays (252 and 264 months, respectively). Only the first dip is significant, however, suggesting that young male workers are significantly less likely to be employed as they approach their 21st birthday. The employment probability rebounds between the birthdays, just after the 20th birthday, then again at approximately at 21.5 years of age and, similarly, around 22.5. The first two of those peaks are significantly different from zero whereas being between 22 and 23 is not associated with a significant increase or reduction in the employment probability.

The estimates for women are strikingly different, as Figures 2 shows: none of the age effects between 21 and 23 years are significant. Moreover, as is also the case in Table 1, 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).[9]

We can only speculate what drives these results. The age-related NMW rates apply equally to men and women yet we only observe age-related effects for the former. This may reflect the fact that the labour market positions of men and women are substantially different from each other. As we hinted above, the negative effect around men's 21st birthday may be due to anticipation effects whereby employers choose to dismiss workers well in advance of the age-related NMW increase. However, it is not clear why they should act in this way already one year in advance of the individual becoming eligible for the higher NMW rate. Similarly, there does not seem to be any obvious reason for the upward blips in the employment probability between birthdays. Nevertheless, the fact that the employment probability dips both around the 21st and 22nd birthday seems to suggest that the age-related NMW increases may lower employment.

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 effect of the NMW threshold at 18 years of age. Recall that those turning 18 become eligible for the 18-21 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. The effect of turning 18 appears significantly negative for both genders: becoming eligible for the higher NMW rate is associated with lower employment probability. Note that again this negative effect becomes apparent only when we consider both the coefficient estimated for the discontinuity dummy and the changing effect of the age polynomial: the dummy itself is not significantly different from zero (except for females). The insignificant coefficient for the dummy is in line with the finding of Dickens, Riley and Wilkinson. 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, however, turning 18 is associated with a host of other important changes besides becoming eligible for a higher NMW rate. For example, the UK law requires those selling or serving alcohol to be 18 or older, which makes those over 18 eligible to work in bars, restaurants and many shops. This makes the negative effect that we found all the more remarkable. Again, 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 stay enrolled for another two years to complete secondary education. Those who do so without enrolling in higher education upon graduating then generally enter the job market when aged 18. Nevertheless, as with the university graduates, few would leave full time education close to their 18th birthday. Rather, the students in their final year of secondary education turn 18 over the course of their last year. Therefore, the negative employment effect is unlikely to be attributable to changes in participation in education.

[9] Because of the insignificant results obtained for ages between 21 and 23 and also in the light of the quadratic/cubic polynomial being rejected, we did not extend the analysis for women to their 20th birthday.

Hence, overall our results suggest that the age related NMW rates may be affecting employment of the young workers. However, the nature of the effect is not entirely straightforward. In particular, it is possible that due to anticipation effects, employers dismiss workers well in advance of them reaching the age when the higher wage is supposed to take effect.

4 Employment Effects of NMW Increases

We now broaden our analysis to consider the effect of NMW on all workers (specifically, we consider workers aged between 18 and 40). To this effect, we implement a difference-in-difference analysis in line with the previous work by Stewart (2004) and Dickens and Draca (2005). We are interested to see whether the annual increases in the NMW rates have had any effect on employment in the UK. Note that there are important differences in our approach now compared to the preceding section. First, our data in this section cover all workers rather than only young workers (although we also present results for young and older workers separately, drawing the line between the two groups at 25 years of age). Second, the NMW increases that we consider in this section are the annual changes to all NMW rates rather than the increase that workers receive when they turn 22. Finally, as the workers included in our analysis can be subject to either the development rate or the adult rate, we always consider the increase relevant to each particular worker.

We thus estimate the following equation:

$$P(e_{t+1}=1|e_t=1)=\alpha*treat+\beta*treat*gap_{it}+\gamma*X \quad (3)$$

where the dependent variable is the probability that the worker is employed conditional on having been employed in the previous quarter. The difference-in-difference methodology involves comparing the employment outcomes of two groups that are a-priori very similar, except one group is affected by the NMW increase (treatment group) while the other is not (control group). Observing any difference in the employment probabilities between the two groups can be interpreted as evidence of the labour market effect of the minimum wage. The variable of interest is therefore *treat*: a dummy denoting workers who belong to the treatment group, defined as workers whose wages are due to increase following the NMW hike:

Treatment group: $nmw_t \leq w_t < nmw_{t+1}$

The control group is defined as workers who already earn as much as or more than the new (age-relevant) NMW rate. Comparing workers subject to the NMW increase with all workers whose wages exceed the new NMW rate would not be correct: those at the bottom end of the wage distribution can have dramatically different employment probabilities than those higher up. Therefore, the control group consists only of those workers whose wages exceed the new NMW rate by no more than *c* percent:

Control group: $nmw_{t+1} \leq w_t < nmw_{t+1}*(1+c)$

We consider three alternative values for *c*: 10%, 30% and 50%. Hence, the workers belonging to the treatment group are those whose wages have to be raised following the NMW increase while the wages of those in the control group, while being similarly low, need not change at all. Using multiple values for *c* is especially important as one of the NMW increases, in October 2001, amounted to a rise of 10.8%; therefore, if looking only at that year, we would not have any control while taking *c* equal to 10%.

Finally, gap_{it} is the difference between the worker's wage and the new NMW rate for the workers in the treatment group and zero for those in the control group:

$gap_{it} = nmw_{t+1} - w_t$ (for the treatment group only, 0 for the control group)

By including the wage gap, we can test whether the effect of NMW increases depends on the size of the increase that applies to each worker: one might expect, for instance, that those whose wages are just below the new rate would see their employment prospects affected the least, if at all.

We only include the treatment-group and control-group workers in our analysis and thus we exclude all those who earn more than *c* percent above the new NMW rate or those who earn less than the old NMW rate (the NMW regulation provides for specific cases when workers can be paid less than the NMW, such as those in apprenticeships or those who live in employer-provided accommodation).

We follow Dickens and Draca (2005) in utilizing two alternative measures of hourly pay available in the LFS data: *hrrate* and *hourpay*. Dickens and Manning (2004) and Dickens and Draca (2005) argue that the latter is more affected by measurement error and therefore less reliable.

The results are summarized in Table 9. We find that NMW increases have had a negative impact on employment. The effect of NMW increases is strongly significant both in regressions with and without additional control variables, especially so when we use *hrrate* to measure earnings. This stands in contrast to previous work: Stewart (2004) considers the effect of the NMW introduction and the increases in 2000 and 2001 while Dickens and Draca (2005) focus on the 2003 increase; neither study finds any significant effect on employment. The different result that we obtain may be due to the fact that our data cover a much longer period: we believe ours is the first study to cover all increases of the NMW since its introduction in 1999 until 2009. It may also be that the NMW only started to have an effect in the more recent years.

Furthermore, and somewhat contrary to our expectations, the size of the increase required to bring the workers in line with the higher NMW rates does not have a significant effect on employment when earnings are measured with *hrrate*. It appears negative and (marginally) significant when using *hourpay*, which is consistent with our expectations: those whose wages need to be raised more suffer a greater disemployment effect.

Increases in the NMW may also encourage the unemployed or those out of the labour-force to enter into employment. We therefore also consider the probability of being employed conditional on not having a job in the previous quarter:

$$P(e_{t+1}=1|e_t=0)=\alpha*treat+\gamma*X \quad (4)$$

A particular problem presents itself here in the fact that we do not have any previous wage information for those who only enter into employment after the NMW increase. In other words, we do not know whether those entering into employment after the increase would have earned more or less than the minimum wage before the increase. Therefore, we follow Dickens and Draca (2005) and define the treatment group as those whose earnings are less than or equal to the (age-relevant) new NMW rate and the control group as those who earn up to *c* percent above the NMW:

Treatment group: $w_{t+1} \leq nmw_{t+1}$

Control group: $nmw_{t+1} < w_{t+1} < nmw_{t+1}*(1+c)$

A somewhat uncomfortable implication of this specification is that the treatment group now includes also those who earn less than the NMW (recall that there are specific cases when this is allowed, for example for apprentices). An alternative specification would entail

constructing the treatment group as including only those who earn the minimum wage after the NMW increase. This, however, would reduce the already low number of observations that we have for this analysis. Furthermore, it would also render our results incomparable with the previous research by Dickens and Draca.

Table 10 reports the results. We find the opposite result as above: job entry responds positively to NMW increases. This is not surprising: higher NMW rate is likely to induce labour market entry by those whose reservation wages exceeded the old NMW rate but are short of the new rate. Hence, the net effect of increasing the NMW combines the two effects which go in opposite directions: disemployment effect for those already in employment coupled with increased job entry by those who were unemployed or inactive previously. These two effects are also approximately similar in size so that it is well possible that they tend to cancel each other out.

Next, we consider young and older workers separately. The labour market positions of different age groups is clearly not the same and therefore the NMW increases need not affect workers in the same way, irrespective of their age. In Tables 11 and 12, we present the result for employment and job entry of workers between 18 and 24 years of age while Tables 13 and 14 feature the results for those in the 25-40 age group. The results are broadly similar to those discussed above. The NMW increases again appear to have a negative effect on employment and encourage job entry. A potentially important difference, however, appears in that the disemployment effect for the young workers, while negative, is not significant. The effect for the older workers remains significant and even increases in its magnitude. This suggests that most of the negative effect discussed above occurs among the older age cohorts.[10]

Note that the fact that NMW increases do not appear to lower the employment of the young workers does not necessarily contradict our findings obtained with the regression-discontinuity analysis. The two components of our analysis measure different phenomena: the discontinuity analysis looks at the impact of age-related MNW increases while the second considers the regular annual increases in all rates. Importantly, the employers of workers who are about to become eligible for a higher age-related NMW rate have the option to hire a younger workers in order to avoid incurring the wage increase. No such alternative is available in the case of annual NMW increases (ignoring the option to move production overseas).

Interestingly, the positive job-entry effect appears significant for both young and older workers, although its magnitude is slightly larger for the latter.

As the final step in our analysis, we turn to incorporating the effect of the tightness of the labour market. In particular, we want to see whether the employment effect of the NMW increases when the labour market is relatively tight. We estimate similar employment and job-entry equations as before but this time the regressions are augmented to include an interaction between belonging to the treatment group and a measure for the tightness of the labour market:

$$P(e_{t+1}=1|e_t=1)=\alpha*treat+\beta*treat*bc_t+\gamma*X \quad (5)$$

where bc_t stands for a business cycle proxy, measured, alternatively, by the regional unemployment rate (computed using the LFS datasets) and the UK-wide economic growth (as reported by the UK Office for National Statistics). The regression equation for job entry is augmented analogously. Treatment and control groups are defined as before.

[10] Nevertheless, the insignificance of the negative effect for young workers can be attributed also to the relatively low number of observations for this group.

Regression results for the probability of remaining employed are reported in Table 15. The business-cycle measure used in this table is the regional unemployment rate. We see that the negative employment impact of the NMW increases disappears in this specification but the interaction term with unemployment is significant and negative (though not significant in every regression). The corresponding analysis of job entry is in Table 16, again using the regional unemployment rate. As with employment, NMW increases on their own lose their significance but the interaction with unemployment is positive and significant. In other words, the NMW increases have a stronger effect when the labour market is tight – and this applies to employment retention and job entry alike.

These results utilize the regional unemployment rate as a measure of labour market tightness. Using the national growth rate instead of unemployment produces mixed and generally insignificant results (not reported but available upon request); this is not surprising as the unemployment rate measured at the regional level is clearly superior to the UK-wide growth rate.

5 Conclusions

The received wisdom concerning the employment effect of the national minimum wage in the UK introduced in 1999 is that it has had little adverse impact. In this paper, we revisit this issue with more recent data and, in our opinion, broader and more comprehensive analysis than any of the previous contributions. We first consider young workers and investigate whether their employment prospects are affected by the fact that different rates apply to different age groups (in particular, the NMW rates are different for those who are 16-17, 18-21 and above 22 years old). Using the regression-discontinuity approach, we find that although the effect of turning 22 is negative, it is not statistically significant. We also revisit the earlier finding of 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 changing effect of age. Specifically, their analysis (as also ours) allows the discontinuity to affect the dependent variable through the coefficient of the discontinuity dummy as well as by allowing age to have a different effect before and after. Dickens, Riley and Wilkinson only consider the former effect. When we account for the combined effect of the dummy and the changed effect of age, we find that turning 22 has no effect on employment of young workers, whether they are unskilled or skilled.

Nevertheless, 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 increase at 22. We find no such increase when workers turn 23. Furthermore, we find a negative effect for both genders at the age of 18.

Besides indicating that the NMW may have an adverse employment effect, our findings also underscore the importance of correctly accounting for the effect of the regression discontinuity: the negative effects at 18 and 21 are only apparent when we consider both the coefficient estimated for the discontinuity dummy and the change in the coefficients for age after the discontinuity.

We complement the analysis of age-related NMW increases with a difference-in-difference analysis of the effect of annual NMW increases over the period from 1999 to 2009. We find evidence that NMW increases depress employment while at the same time they encourage entry into employment by those who were previously unemployed or inactive. The

negative effect on employment appears especially among the older workers, aged 25-40, and is not significant for those aged 18-24. We also find that both effects are strengthened during the periods of tight labour market conditions when unemployment rises.

In summary, our results suggest that the employment effect of the minimum wage in the UK is more complicated than the previous research suggests. In particular, our findings suggest that increases in minimum wage rates, whether they are age-related or apply across the board, may have an adverse impact on employment. Further research may be needed to better understand these effects, however. A more detailed investigation of the interactions involving age, age-related minimum-wage rates and employment seems a particularly fruitful avenue for follow-up work. Our results indicate that the employment probability falls around workers' birthdays between the ages of 20 and 23 and rises in between birthdays. We speculate that this (and especially the significant fall around the 21st birthday) may be due to the anticipation of the age-related minimum wage increase at the age of 22. Nevertheless, there may be another, more innocuous, explanation. Further work should also show whether our finding of a negative employment effect of the annual NMW increases is due to the fact that we use a longer data series or whether it is driven by the inclusion of the more recent years not addressed by the previous studies.

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Table 1 Discontinuity Effect on Employment: All Young Workers

	All	All	Males	Males	Females	Females
	Emp (with covariates)	Emp (without covariates)	Emp (with covariates)	Emp (without covariates)	Emp (with covariates)	Emp (without covariates)
Discontinuity	.00122 (.00244)	.00227 (.00236)	-.00228 (.00331)	.00055 (.00328)	.00368 (.00353)	.00356 (.00336)
No. observations	136,591	136,591	66,582	66,582	70,009	70,009
Chi(5)	26345.97	638.70	15412.56	480.74	12942.46	218.54
Pr>Chi(5)	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
Dum	.00482 (.00800)	.00480 (.00772)	.00567 (.01097)	.00502 (.0107)	.00589 (.01154)	.00348 (.01103)
Chi(25)Test 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: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 2 Discontinuity Effect on Employment: Low Skilled Young Workers

	All	All	Males	Males	Females	Females
	Emp (with covariates)	Emp (without covariates)	Emp (with covariates)	Emp (without covariates)	Emp (with covariates)	Emp (without covariates)
Discontinuity	.00211 (.00418)	.00224 (.00415)	.00214 (.00555)	.00270 (.00561)	.00061 (.00595)	.00193 (.00589)
No. observations	43809	43809	20457	20457	23352	23352
Chi(5)	2686.26	3.24	1621.56	42.32	1174.80	14.47
Pr>Chi(5)	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
Dum	.02940 (.01402)*	.02241 (01386)	.03380 (.01852)	.02807 (.01859)	.02486 (.02002)	.01822 (.01971)
Chi(25)Test 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: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 3 Discontinuity Effect on Unemployment

	All	All	Males	Males	Females	Females
	Unemp (with covariates)	Unemp (without covariates)	Unemp (with covariates)	Unemp (without covariates)	Unemp (with covariates)	Unemp (without covariates
Discontinuity	.00118 (.00126)	.00107 (.00135)	.00190 (.00195)	.00175 (.00212)	.00037 (.00160)	.000200 (.00170)
No. observations	136,591	136,591	66,582	66,582	70,009	70,009
Chi(5)	3489.80	61.34	2721.18	44.54	1170.22	15.95
Pr>Chi(5)	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
Dum	-.008830 (.00425)*	-.00919 (.00452)*	-.01013 (.00659)	-.01104 (.0071)	-.00844 (.00535)	-.00819 (.00565)
Chi(25)Test 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: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 4 Discontinuity Effect on Inactivity

	All	All	Males	Males	Females	Females
	Inact (with covariates)	Inact (without covariates)	Inact (with covariates)	Inact (without covariates)	Inact (with covariates)	Inact (without covariates
Discontinuity	-.00151 (.00160)	-.00347 (.00220)	.00038 (.00249)	-.00252 (.00291)	-.00451 (.00334)	-.00389 (.00323)
No. observations	136,591	136,591	66,582	66,582	70,009	70,009
Chi(5)	29973.84	541.74	20380.64	446.08	13752.84	189.13
Pr>Chi(5)	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
Dum	.00539 (.00698)	.00444 (.00705)	.00695 (.00819)	.00615 (.00919)	.00287 (.01072)	.00474 (.01047)
Chi(25)Test 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: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 5 Probability of Employment Conditional on Employment Status in Previous Quarter

	Males	Females	Males	Females	Males	Females
	Emp from emp (without covariates)	Emp from emp (without covariates)	Emp from unemp (without covariates)	Emp from unemp (without covariates)	Emp from inact (without covariates)	Emp from inact (without covariates)
Discontinuity	-.00184 (.00158)	-.00004 (.00181)	-.01189 (.00936)	.01636 (.01102)	.00030 (.00663)	-.00500 (.00518)
No. observations	27921	26030	3956	2671	6795	11815
Chi(5)	42.09	30.76	7.89	11.21	7.48	10.13
Pr>Chi(5)	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
Dum	.00483 (.00822)	.00114 (.00843)	-.01864 (.04345)	.01636 (.05514)	.03364 (.02418)	.02886 (.01552)

Notes: 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 6 Probability of Employment for Workers Earning Less than Adult Rate

	Males	Females
	Emp (without covariates)	Emp (without covariates)
Discontinuity	.000242 (.01783)	-.00684 (.01279)
No. observations	1365	1931
Chi(5)	4.06	7.96
Pr>Chi(5)	0.5404	0.1582
R2	0.0047	0.0066
Dum	.014173 (.04104)	.008331 (.03483)

Notes: 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

	Males	Females	Males	Females
	21 years	21 years	23 years	23 years
	Emp (with covariates)	Emp (with covariates)	Emp (with covariates)	Emp (with covariates)
Discontinuity	-.00994 (.00326)**	-.001039 (.00349)	.00435 (.00318)	-.00179 (.00336)
No. observations	68324	70647	65206	70622
Chi(5)	17001.14	12155.02	13443.49	14310.83
Pr>Chi(5)	0.0000	0.0000	0.0000	0.0000
R2	0.1947	0.11285	0.1879	0.1602
Dum	-.00764 (.01150)	-.00186 (.01184)	.01043 (.01023)	-.01325 (.01138)

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 8 Discontinuity Effects at 18

	Males	Females	All
	18 years	18 years	18 years
	Emp (with covariates)	Emp (with covariates)	Emp (with covariates)
Discontinuity	-0.01018 (0.00361)**	-.01009 (.00362)**	-0.00984 (0.00255)**
No. observations	67641	65023	132664
Chi(5)	16587.27	9896.45	25665.83
Pr>Chi(5)	0.0000	0.0000	0.000
R2	0.1788	0.1110	0.1410
Dum	-0.00238 (0.01253)	-.0253495 (.01263)*	-.012706 (0.00888)

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 9 Effect of NMW Increases on Employment: Workers Aged 18-40

With Covariates	**Hrrate**			**Hourpay**		
	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	162.19	344.27	521.62	202.22	378.04	580.60
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0366	0.0375	0.0444	0.0439	0.0375	0.0403
Observations	6393	14089	19308	6918	16686	26279
Treatment	-.03267 (.01126)*	-.03856 (.01083)**	-.04449 (.01054)**	-.00820 (.00991)	-.02071 (.00911)*	-.02849 (.00875)*
Wage Gap	.02009 (.04818)	.01244 (.04346)	.01889 (.03958)	-.06798 (.03791)	-.04516 (.03232)	-.03732 (.02838)
Without Covariates	**Hrrate**			**Hourpay**		
	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	10.88	20.96	38.38	11.56	27.85	52.60
Prob > chi2	0.0043	0.0000	0.0000	0.0031	0.0000	0.0000
R2	0.0025	0.0023	0.0033	0.0025	0.0028	0.0037
Observations	6393	14089	19308	6918	16686	26279
Treatment	-.02757 (.01129)*	-.03261 (.01078)*	-.04165 (.01067)**	-.00822 (.01011)	-.01956 (.00924)*	-.03068 (.00905)*
Wage Gap	.00357 (.04722)	.00335 (.0442)	.00311 (.04105)	-.07371 (.03538)*	-.06628 (.03182)*	-.05954 (.02859)*

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 10 Effect of NMW Increases on Job Entry: Workers Aged 18-40

With	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	389.04	1182.14	1744.54	853.82	1543.58	2367.78
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0641	0.0887	0.0986	0.0864	0.0927	0.1056
Observations	5924	13735	19534	10533	19078	27732
Treatment	.05394 (.01105)**	.07560 (.00969)**	.09040 (.00932)**	.00825 (.00757)	.02980 (.00551)**	.04219 (.00498)**
Without	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	29.86	53.89	104.27	6.14	57.00	147.11
Prob > chi2	0.0000	0.0000	0.0000	0.0132	0.0000	0.0000
R2	0.0049	0.0040	0.0059	0.0006	0.0034	0.0066
Observations	5924	13735	19534	10533	19078	27732
Treatment	.05902 (.01092)**	.06610 (.00944)**	.08661 (.00916)**	.01914 (.00766)**	.04217 (.00570)**	.06057 (.00527)**

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 11 Effect of NMW Increases on Employment: Workers Aged 18-24

With Covariates	**Hrrate**			**Hourpay**		
	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	81.83	110.97	153.42	103.75	165.25	214.51
Prob > chi2	0.0321	0.0002	0.0000	0.0007	0.0000	0.0000
R2	0.0749	0.0354	0.0387	0.0770	0.0492	0.0458
Observations	1268	3829	4957	1715	4467	6500
Treatment	-.013645 (.03177)	-.01599 (.02376)	-.01976 (.02294)	.01012 (.02086)	-.01687 (.0186)	-.01949 (.01801)
Wage Gap	-.07296 (.13109)	-.02104 (.08245)	-.01262 (.07938)	-.06276 (.0568)	-.01731 (.05099)	-.01805 (.04835)
Without Covariates	**Hrrate**			**Hourpay**		
	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	0.94	0.60	1.23	0.60	1.76	3.92
Prob > chi2	0.6235	0.7391	0.5396	0.0007	0.4147	0.1410
R2	0.0009	0.0002	0.0003	0.7392	0.0005	0.0008
Observations	1295	3832	4960	1715	4470	6503
Treatment	-.00346 (.02642)	-.00341 (.02181)	-.00885 (.02152)	.01554 (.02184)	-.01496 (.01902)	-.02275 (.01871)
Wage Gap	-.05790 (.08687)	-.03558 (.08167)	-.03470 (.07965)	-.03865 (.05468)	-.01144 (.05065)	-.01092 (.04837)

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 12 Effect of NMW Increases on Job Entry: Workers Aged 18-24

With	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	115.34	286.49	432.43	267.13	481.34	740.67
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0959	0.0800	0.0871	0.1115	0.1086	0.1184
Observations	1107	3268	4690	2379	4588	6712
Treatment	.06809 (.02842)*	.06213 (.02364)*	.07251 (.02281)*	.03282 (.01629)*	.03793 (.01219)*	.04424 (.01127)**
Without	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	4.61	1.12	3.61	6.52	13.14	22.32
Prob > chi2	0.0317	0.2900	0.0573	0.0107	0.0003	0.0000
R2	0.0038	0.0003	0.0007	0.0027	0.0030	0.0036
Observations	1148	3273	4696	2379	4588	6712
Treatment	.05392 (.02531)*	.02248 (.02155)	.03877 (.02099)	.04331 (.01669)*	.04492 (.01263)**	.05370 (.01181)**

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 13 Effect of NMW Increases on Employment: Workers Aged 25-40

With Covariates	**Hrrate**			**Hourpay**		
	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	99.23	186.56	260.17	146.94	196.01	259.95
Prob > chi2	0.0184	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0371	0.0367	0.0400	0.0568	0.0349	0.0321
Observations	4111	8793	12335	4198	10390	16893
Treatment	-.04725 (.01469)*	-.05162 (.01413)*	-.05688 (.01384)*	.00316 (.0134)	-.00750 (.01206)	-.01692 (.01161)
Wage Gap	.10794 (.0688)	.06043 (.05842)	.05317 (.05206)	-.15148 (.07267)*	-.16170 (.06352)*	-.13797 (.05518)*
Without Covariates	**Hrrate**			**Hourpay**		
	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	11.37	28.82	47.10	12.11	26.28	45.36
Prob > chi2	0.0034	0.0000	0.0000	0.0023	0.0000	0.0000
R2	0.0042	0.0057	0.0072	0.0047	0.0047	0.0056
Observations	4111	8793	12335	4203	10400	16910
Treatment	-.04302 (.0145)**	-.05562 (.01431)**	-.06488 (.01431)**	.00050 (.01408)	-.01265 (.01272)	-.02364 (.01243)
Wage Gap	.07761 (.06689)	.06800 (.05862)	.06151 (.05303)	-.17738 (.07424)*	-.15400 (.0645)*	-.13495 (.05653)*

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 14 Effect of NMW Increases on Job Entry: Workers Aged 25-40

With	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	149.38	278.18	390.26	179.85	297.76	512.80
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0398	0.0402	0.0435	0.0334	0.0337	0.0439
Observations	3983	8630	12447	6664	12100	17918
Treatment	.04379 (.01294)**	.08184 (.01114)**	.09609 (.01075)**	-.00622 (.00867)	.02246 (.00623)**	.03848 (.0056)**
Without	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	11.68	67.23	115.91	0.31	20.79	77.18
Prob > chi2	0.0006	0.0000	0.0000	0.5791	0.0000	0.0000
R2	0.0031	0.0097	0.0129	0.0001	0.0024	0.0066
Observations	3983	8630	12447	6664	12100	17918
Treatment	.04311 (.01279)**	.02248 (.02155)	.03877 (.02099)	-.00486 (.00878)	.02856 (.0064)**	.04838 (.00588)**

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 15 Effect of NMW Increases on Employment: Effect of Recession

With	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	162.23	347.19	524.78	208.75	388.71	588.79
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0367	0.0378	0.0446	0.0453	0.0386	0.0409
Observations	6393	14089	19308	6918	16686	26279
Treatment	.015562 (.02904)	.00850 (.02392)	.00393 (.02194)	.06123 (.02382)*	.04488 (.01603)**	.02663 (.01532)
Business cycle	-1.2985 (.83951)	-1.22617 (.70532)	-1.17216 (.63466)	-2.51141 (.80145)**	-2.36101 (.65979)**	-1.81978 (.57427)**
Without	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	15.36	25.44	42.86	21.54	37.83	62.59
Prob > chi2	0.0005	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0035	0.0028	0.0036	0.0047	0.0038	0.0043
Observations	6393	14089	19308	6918	16686	26279
Treatment	.025927 (.02500)	.018807 (.02196)	.00905 (.02119)	.06558 (.02154)	.04519 (.01619)	.03179 (.01491)
Business cycle	-1.53895 (.72216)*	-1.44158 (.67664)	-1.33901 (.62855)*	-2.70200 (.70837)**	-2.43358 (.63865)**	-2.18700 (.57412)**

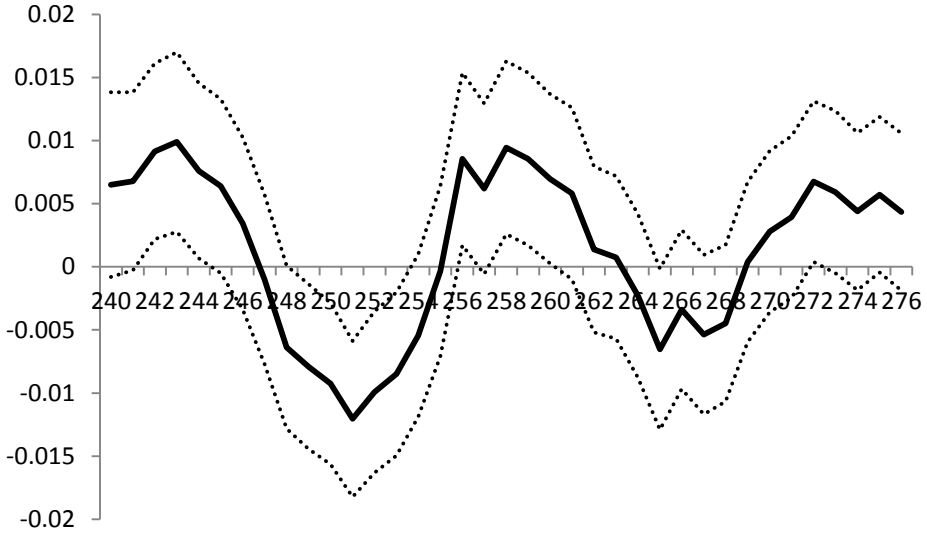
Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Table 16 Effect of NMW Increases on Job Entry: Effect of Recession

With	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (80)	229.67	690.73	1060.10	447.62	834.78	1390.98
Prob > chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R2	0.0418	0.0597	0.0687	0.0528	0.0575	0.0707
Observations	5716	13130	18899	9958	18427	27204
Treatment	-.03891 (.03824)	-.00364 (.03052)	.01735 (.0292)	-.01721 (.02543)	-.00862 (.01852)	.01003 (.01647)
Business cycle	2.31761 (1.07233)*	2.06319 (.83200)*	1.73297 (.73428)*	.58164 (.68791)	.97160 (.52122)	.78619 (.43927)
Without	**Hrrate**			**Hourpay**		
Covariates	c=0.1	c=0.3	c=0.5	c=0.1	c=0.3	c=0.5
LR Chi2 (2)	28.45	62.95	107.12	19.72	52.48	120.44
Prob > chi2	0.0000	0.0000	0.0000	0.0001	0.0000	0.0000
R2	0.0052	0.0054	0.0069	0.0023	0.0036	0.0061
Observations	5716	13130	18899	9958	18427	27204
Treatment	-.05497 (.03156)	-.03085 (.027)	-.01176 (.02591)	-.28705 (.0797005)	-.03674 (.01553)	-.01673 (.01411)
Business cycle	2.7916 (.86446)*	2.53915 (.78648)**	2.3318 (.72234)*	9.13201 (2.093547)**	1.96354 (.45001)**	1.78409 (.40899)**

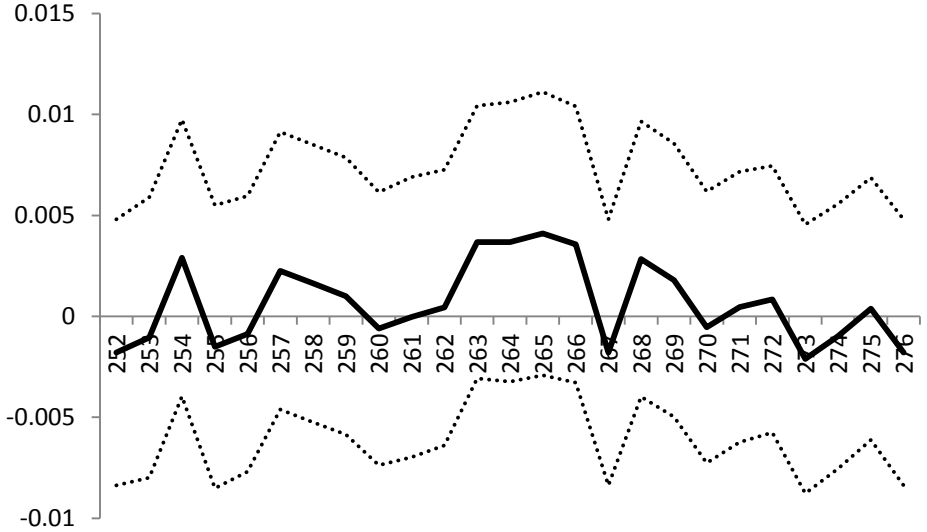
Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.

Figure 1 Discontinuity Effects by Month between Ages 20 and 23: Males



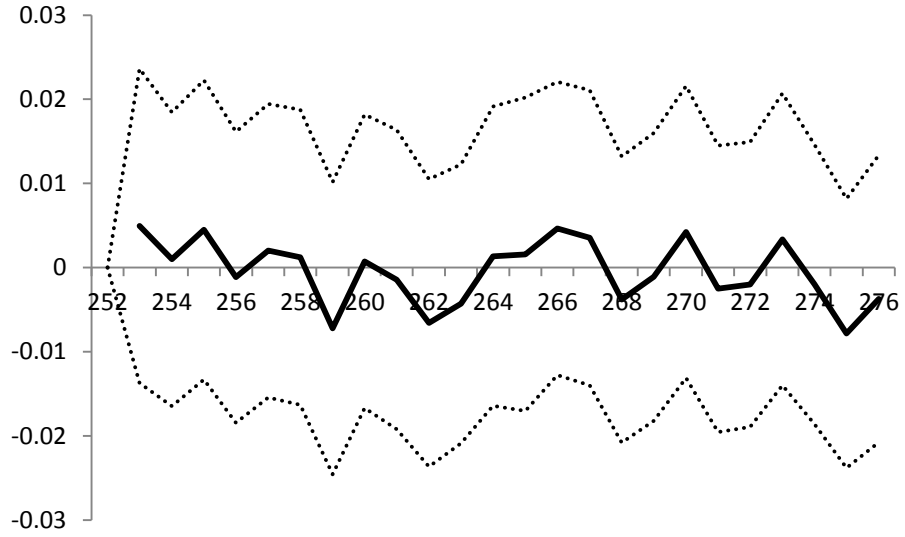
Notes: The points at which birthdays occur are: 20 years (240 months), 21 years (252 months), 22 years (264 months) and 23 years (276 months). Dotted lines represent the 95% confidence interval.

Figure 2 Discontinuity Effects by Month between Ages 21 and 23: Females (quadratic age polynomial)



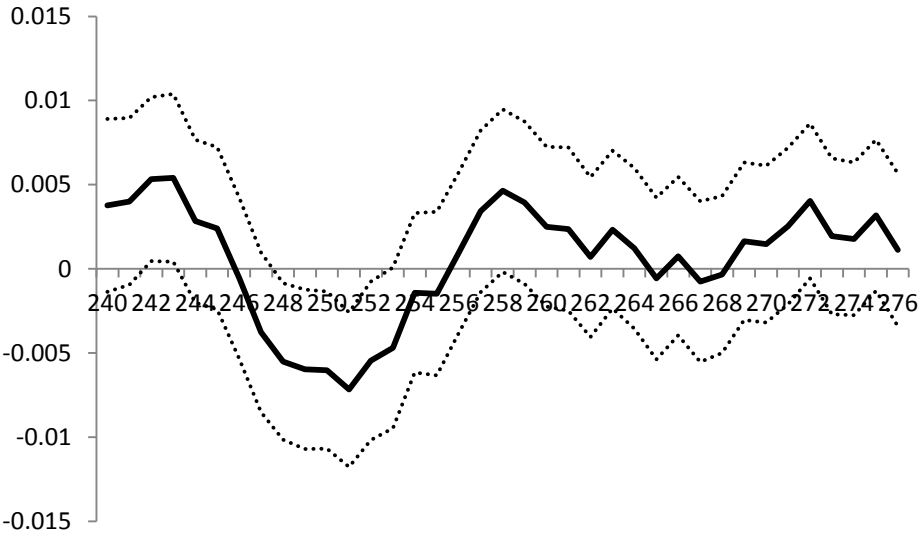
Notes: The points at which birthdays occur are: 21 years (252 months), 22 years (264 months) and 23 years (276 months). Dotted lines represent the 95% confidence interval.

Figure 3 Discontinuity Effects by Month between Ages 21 and 23: Females (cubic age polynomial)



Notes: The points at which birthdays occur are: 21 years (252 months), 22 years (264 months) and 23 years (276 months). Dotted lines represent the 95% confidence interval.

Figure 4 Discontinuity Effects by Month between Ages 20 and 23: Both Genders



Notes: The points at which birthdays occur are: 20 years (240 months), 21 years (252 months), 22 years (264 months) and 23 years (276 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	.00092 (.00969)	-.00461 (.00350)	.00116 (.00965)	-.00045 (.00350)	-.00961 (.00891)	.00096 (.00334)
No. observations	57797	109453	57513	108102	56417	107005
Chi(63)	11048.03	21478.97	11245.37	20836.73	10430.78	19855.19
Pr>Chi(63)	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
Dum	.01341 (.01425)	-.00430 (.00945)	.01026 (.01395)	.01483 (.02617)	-.01239 (.01323)	-.00188 (.00876)

Notes: 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	.01042 (.01352)	-.00883 (.00476)	-.00024 (.00793)	-.00239 (.00479)	.01077 (.01269)	.00532 (.00459)
No. observations	28583	53899	27978	52724	27086	51396
Chi(63)	6610.71	13098.40	6656.79	12248.60	5547.02	10567.76
Pr>Chi(63)	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
Dum	.02918 (.01976)	-.00307 (.01316)	.00052 (.01919)	-.00303 (.01260)	-.00365 (.01750)	.00668 (.01159)

Notes: 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	-.00925 (.01389)	-.00136 (.00508)	-.00665 (.01375)	.01457 (.01321)	-.01932 (.01955)	-.00362 (.00484)
No. observations	29214	55554	29535	55378	29331	55609
Chi(63)	5040.66	9529.44	5505.22	10287.81	5987.72	11228.77
Pr>Chi(63)	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
Dum	-.00170 (.02049)	-.00589 (.01353)	.02335 (.02011)	.00031 (.00506)	-.02845 (.01264807)	-.01020 (.01295)

Notes: Significance levels denoted as * 5% and ** 1%. Source: Labour Force Survey.