What do we really know about the employment effects of the National Minimum Wage? An illustration of the low power of difference-in-difference designs [*]

Mike Brewer[†]
Thomas Crossley[‡]
Federico Zilio[§]

June 2015

Abstract

A substantial body of research on the UK National Minimum Wage (NMW) has concluded that the introduction and subsequent up-ratings of the NMW have not had a detrimental effect on employment. This research has directly influenced, through the Low Pay Commission, the conduct of policy. We revisit this literature and offer a reassessment, motivated by two concerns. First, this literature employs difference-in-difference designs, even though there are significant challenges in conducting appropriate inference in such designs, and they can have very low power when inference is conducted appropriately. Second, the literature has focused on the binary outcome of statistical rejection of the null hypothesis, without attention to the range of employment effects that are consistent with the data. In our reanalysis of the data, we conduct inference using recent suggestions for best practice and consider what magnitude of employment effects the data can and cannot rule out. We find that the data are consistent with both large negative and positive impacts of the UK National Minimum Wage on employment. We conclude that the existing data, combined with difference-in-difference designs, in fact offers very little guidance to policy makers.

JEL Classification: C12, C18, J23, J38.

Keywords: minimum wage, difference-in-difference, power, minimum detectable effects

1   Introduction

The conduct of minimum wage policy in the UK is unusual for its very formal connection to an evidence base. Each year, a body called the Low Pay Commission (LPC), which was established to advise the UK government on the setting of the NMW, commissions and funds research on the impacts of the NMW, and then uses evidence from those studies to help determine its recommendations to the government. Those recommendations are almost always adopted. A broad conclusion from this body of research has been that the introduction of the NMW and its subsequent up-ratings has not had detrimental effects on employment (Stewart, 2004a,b; Dickens and Draca, 2005; Dickens et al., 2012; Bryan et al., 2013), with the LPC affirming in 2008 that they:

> have not found any significant negative effects, either in the work they have done themselves or in the work they have commissioned from others. The LPC have looked long and hard in all the places that are most likely to reveal such an effect. This is not to say that the minimum wage is incapable of negative impact; merely that, so far, the UK minimum wage has successfully avoided the dangers.

However, there are reasons to question whether the evidence base on the employment impact of the UK NMW is as strong as its influence suggests. First, much of that literature is based on difference-in-difference (DiD) designs (Stewart, 2004a,b; Dickens and Draca, 2005; Dickens et al., 2012; Bryan et al., 2013). Recent work has highlighted the challenges of conducting appropriate inference in such designs (Bertrand et al., 2004; Donald and Lang, 2007; Cameron et al., 2008) and, when inference is conducted properly, such designs may have very low power (Brewer et al., 2013). Second, the literature on the NMW has focussed on the binary outcome of the statistical rejection of the null hypothesis, without attention to the range of employment effects that are consistent with the data. Commentators in both the social and medical sciences (such as Cohen (1994), Sterne et al. (2001), Ziliak and McCloskey (2004) and Ioannidis (2005)) have long noted that an excessive focus on rejecting or failing to reject a null hypothesis can result in a very misleading interpretation of the statistical evidence. One common recommendation is that researchers present confidence intervals, as this summarises what values of the parameters of interest would be rejected (in a statistical sense) by this data.

In this paper we re-evaluate the employment effects of the UK's NMW taking full account of these concerns. Our model estimates the impact of the NMW on transitions from employment using a DiD design. Our specifications closely follow those in Bryan et al. (2013)), a recent and comprehensive study of NMW upratings commissioned by the LPC; similar models have also been estimated by Stewart (2004a,b), Dickens and Draca (2005) and Dickens et al. (2012). In these studies, the standard approach is to estimate the impact that an uprating of the NMW has on the transition rate from employment into non-employment by comparing outcomes for a treatment group of employees directly affected by a NMW uprating with outcomes for workers in a control group that are located slightly higher up the wage distribution. But we depart from that study's approach in four ways. First, we follow recent suggestions for best practice for undertaking inference in DiD designs. Second, we focus explicitly on confidence intervals, rather

than reporting p-values or focusing on the binary outcome of whether the null hypothesis of no impact of the NMW on employment can be rejected; this means we show, given appropriate inference techniques, what magnitude of effects can be ruled out given the available data. Third, we show what the estimated coefficients mean for economically-meaningful concepts, such as elasticities of employment with respect to the minimum wage. Finally, we calculate minimum detectable effects (MDEs), following Bloom (1995), which indicate how large the true employment effects would have to be (or how large would the true labour demand elasticity have to be) for the methods employed in this literature to detect them with high probability.

The existing literature has consistently failed to reject the null hypotheses that the UK's NMW wage has no impact on employment or job retention. We replicate the point estimates of these past studies, and, like the past literature, our estimated confidence intervals span zero (so, like the past literature, we fail to find a "statistically significant effect"). However, we also cannot exclude large negative (and also small positive) effects. Our preferred specification, in which we follow the recent literature on inference in DiD designs and calculate the standard errors using methods designed to ensure that associated tests have the correct size, gives a 95 % confidence interval within which a 10 percent rise in the NMW could lower the job retention rate by as much as eighteen percent (or increase it by as much as four percent.) Considered another way, our calculations of MDEs indicate that the tests and data employed in the literature would have only an eighty percent chance of detecting a non-zero impact of the NMW if the true effect of a ten percent increase in the NMW was to decrease the job retention rate by no less than fifteen percent. This would correspondent to an employment elasticity amongst minimum wage workers of (minus) *three*.

The rest of the paper proceeds as follows. Section 2 outlines the most important features of the UK NMW and past literature related to it with an emphasis on why this research has been influential on LPC and Government. Section 3 describes the data and the empirical strategy we use to revise the evidence on UK NMW. Section 4 presents and discusses our findings. Section 5 concludes.

2 Background

2.1 The minimum wage in the UK[1]

The UK's national minimum wage (NMW) was introduced on the 1st of April 1999, and it covers all workers who are not self-employed, regardless of industry, size of firm, occupation and region. The adult rate (for workers aged 22 or over) was set at £3.60 per hour, the rate for workers aged between 18 and 21 was £3.00, whereas a "trainee" level for adults who received an accredited training in the first six months of a new work was set at £3.20. Apprentices, workers on a government scheme under age 19 and young workers aged 16-17 were initially exempt. Around 1.9 million workers were estimated to be affected by the introduction of the NMW, among whom 220,000 were aged 18-21: 8% of all adult employees and 14% of all workers aged between 18 and 21 gained from the NMW implementation (Lourie, 1999). After a month from the introduction of the NMW, it was estimated that the vast majority of employers were meeting their obligation to pay the proper hourly rate (Low Pay Commission Second Report 2000).

The level of the NMW is reviewed annually (and we describe this process in more detail below), with the government announcing each Spring the rate that will apply from the following October. Table 1 shows the history of the NMW upratings. In the first years after its introduction, the government announced sizeable increases in the NMW: the adult rate rose by nearly 11% in October 2001, and by between 7 and 8% in both 2003 and 2004, at a time when Average Weekly Earnings (AWE) increased by just 4 to 5%. Since the onset of the financial crisis and subsequent downturn, although upratings have not always kept pace with inflation, they have largely outpaced growth in average earnings.

---

[1]This section is drawn on Low Pay Commission Report 1998, Lourie (1999), Coats (2007), Finn (2005).

Table 1: The UK National minimum wage for adults

Date	Adult rate	NMW change (%)	AWE growth (%)	Inflation (%)
Apr 1999	£3.60	-	-	-
Oct 2000	£3.70	2.78	-	1.0
Oct 2001	£4.10	10.81	4.5	1.2
Oct 2002	£4.20	2.44	2.6	1.4
Oct 2003	£4.50	7.14	3.9	1.4
Oct 2004	£4.85	7.78	5.0	1.2
Oct 2005	£5.05	4.12	3.8	2.3
Oct 2006	£5.35	5.94	4.1	2.4
Oct 2007	£5.52	3.18	4.2	2.1
Oct 2008	£5.73	3.80	3.4	4.5
Oct 2009	£5.80	1.22	0.2	1.5
Oct 2010	£5.93	2.24	2.2	3.2
Oct 2011	£6.08	2.53	1.9	5.0
Oct 2012	£6.19	1.81	1.3	2.7
Oct 2013	£6.31	1.94	1.1	2.2
Oct 2014	£6.50	3.01	2.1	1.3
Oct 2015	£6.70	3.08	-	-

* The adult rate refers to workers aged 22 and over until 2009 and aged 21 and over afterwards.
Source: Low Pay Commission, CPI series D7G7 (ONS), Average Weekly Earnings (ONS) series KAB9.

Figure 1: Annual growth rates of the UK National Minimum Wage, 2000 to 2015
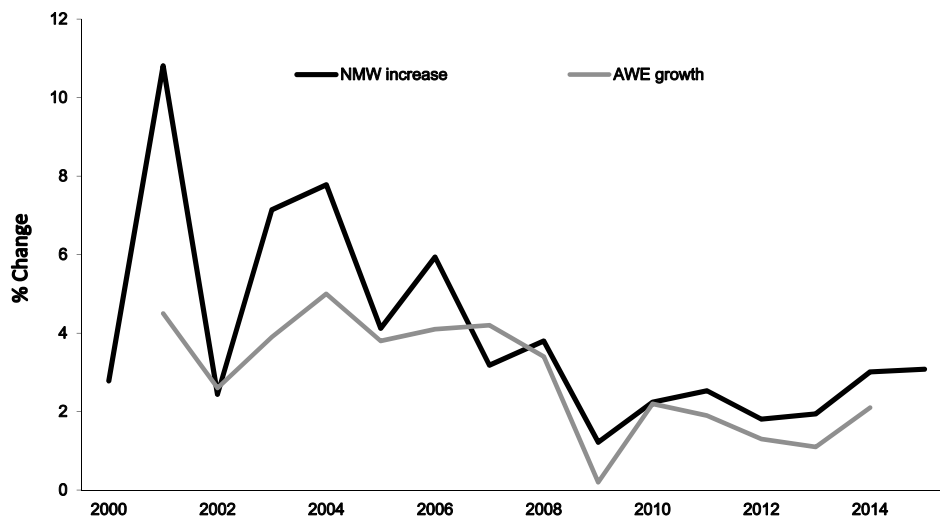
Source: Low Pay Commission, Average Weekly Earnings (ONS) series KAB9.

2.2 Research on the impact of the National Minimum Wage

A considerable number of studies, mostly commissioned and funded by the Low Pay Commission, have examined whether the NMW has affected employment. Typically, these have made use either of the period just before and after the introduction of the minimum wage, or the variations over time in the level of the minimum wage caused by the annual changes shown in Table 1.

Many of these studies use a Difference-in-Differences (DiD) design, typically comparing workers whose wage is increased to comply with the NMW with an unaffected group of workers with higher wages. It is also common to look at the impact of the NMW on job retention (i.e. on the probability of making a transition out of employment). This is done because it allows researchers to identify a group of employees who will be directly affected by a change in the NMW; the main alternative to this is to look at employment rates amongst groups of individuals likely to be affected by the minimum wage were they to work, such as young low-skilled adults.[2] For example, Stewart (2004b) assesses the impact of the introduction of the NMW on job retention with a DiD design, comparing job retention probabilities of the group who have an initial wage below the 1999 NMW (the treatment group) with a group who earn a slightly higher wage (the control group). He used three different data sources - the Labour Force Survey Data (LFS), the British Household Panel Survey Data (BHPS) and the New Earnings Survey (NES), now known as the Annual Survey of Hours and Earnings (ASHE) - all with their own strengths and weaknesses. Stewart reports mostly positive, but statistically insignificant, effects of the NMW on job retention across all data-sets; only for women, and only in some specifications, does he find disemployment effects of the NMW, and these are statistically insignificant. A companion paper, Stewart (2004a), extends this analysis to the 2000 and 2001 upratings. Again, the study does not find any statistically significant effect of the two subsequent upratings on job retention probabilities.

Dickens and Draca (2005) examines the employment effects of the 2003 minimum wage uprating. The authors find no statistically significant effects on employment, although they note that the 2003 uprating affects fewer workers than previous upratings, which they helpfully note diminishes the power of the analysis. Dickens et al. (2012) analyse the NMW effects on employment using individual data and exploiting geographical variation in the bite of the national minimum wage. Using the NES, they find that the introduction of the minimum wage may have had a small negative impact on job retention for women working part-time. However, their conclusions are not consistent across specifications, and are not confirmed using the LFS.

Bryan et al. (2013) provides one of the most comprehensive assessments, as the authors estimate the effect on job retention of each of the NMW upratings from 2000 to 2011. They extend the previous DiD design by not only comparing individuals who were directly affected by a NMW increase to those who earned slightly more (as in Stewart (2004b)), but also comparing job retention rates over periods that do, and do not, span the annual October increase in

---

[2]Machin et al. (2002) and Machin and Wilson (2004) examine the impact of the NMW on workers employed in residential or nursing care homes, a sector with very high incidence of low rates of pay. They find some evidence of disemployment effects.

the NMW: the idea is that retention rates measured over a period that spans October are potentially affected by the NMW uprating, and retention rates measured over a period that does not span October are not affected by any NMW uprating). They find a statistically significant detrimental effect for the 2001 uprating - corresponding to the largest year-on-year increase observed to date, with a rise of over 10% - for men. For some specifications, they also obtain positive coefficients for the 2006 and the 2011 upratings, but the statistical significance of these is not robust across specifications.

The research summarised above has had an unusually large influence on policy because of the institutional structure around the NMW, and the particular role played by a body known as the Low Pay Commission (LPC). The LPC is a statutory body, independent of government, and exists to advise the government about policy towards the NMW (it is not responsible for enforcement). As discussed earlier, the decisions about the level of the NMW are made on an annual cycle. The LPC produces a set of recommendations each year (usually in February), including a recommendation on by how much the NMW should increase, and the government makes its decision later in the Spring, with the new NMW rate applying from the following October. Although the government is not obliged to accept its recommendations, successive UK governments have, since 1999, mostly followed the LPC's advice on by how much to increase the NMW in each year.

And it is the LPC that provides the link between research and policy decisions, as the LPC's recommendations are heavily based on its reading of the research evidence. The LPC has a continuous programme of monitoring and evaluation of the NMW, and in each year since its inception, it has directly commissioned a considerable volume of research on the impacts (in a broad sense) of the UK NMW; typically commissioning some 6-10 projects each year, the results which are published alongside their recommendations to government. Speaking in 2007, the incoming Chairman of the LPC, Paul Myners, declared that his predecessors "established a way of working within the Commission based on partnership, openness and a respect for evidence. I am determined that, under my chairmanship, the Commission will continue to be evidence-driven." (LPC Report 2007).

The research on the effect of the NMW on employment has typically failed to reject the null that the NMW has had no impact on employment, or on job retention probabilities. Crucially, though, this "failure to reject the null" has been interpreted in policy circles as "evidence of no adverse impact". For example, in the LPC's 2003 report, the then-chairman stated that:

> *The National Minimum Wage has brought benefits to over one million low-paid workers. It has done so without any significant adverse impact on business or employment. Far from having the dire consequences which some predicted, the minimum wage has been assimilated without major problems even though it has been a challenge for some businesses. It has ceased to be a source of controversy and become an accepted part of our working life.* LPC Report 2003.

In 2006, the LPC said that

> *since its introduction in 1999 the minimum wage has been a major success. It*

> *has significantly improved the wages of many low earners; it has helped improve the earnings of many low-income families; and it has played a major role in narrowing the gender pay gap. But it has achieved this without significant adverse effects on business or employment creation.* (LPC Report 2006).

Finally, in their 2009 report, the LPC concluded that "a large volume of research has demonstrated that the minimum wage has not had a significant impact on either measures (unemployment and wage inflation) over its first ten years."

And this impression about the benign impact of the NMW on employment is, in general, shared by government. In 2001, the Secretary of State for Trade and Industry at the time said that "the second report of the LPC, published in February 2000, looked at these matters but found no indication so far of significant effects on the economy as a whole as a result of the introduction of the national minimum wage." (House of Commons Debates, *15 May 2000 : Column: 26W*). Announcing the 2004 uprating, the Prime Minister at the time said: "Some people said unemployment would go up as a result of the minimum wage. Actually we have one-and-three-quarter million more jobs in the British economy as well."

Of course, these UK policy-makers are not alone in wrongly interpreting a p-value of more than 0.05 as strong evidence in favour of the null hypothesis that "the NMW has no effect on employment" (Sterne et al., 2001). As Cohen (1994) observes, what policy-makers want to know is how likely is it, given the available data, that the NMW does not have an adverse effect on employment (i.e. $P(H_0|D)$); what a p-value tells us is how extreme the data is if the null hypothesis was true (i.e. $P(D|H_0)$). Furthermore, as Ziliak and McCloskey (2004) argue, we should consider the magnitude of effects when interpreting findings, in order to establish whether findings are *economically* significant. Alternatively, we might want to know not whether we can reject the null of "the NMW has no effect on employment", but whether we can reject the null of "the NMW has economically-meaningful large negative impacts on employment".

3 Data and Models

Our approach is motivated by two concerns. First, we contend that too much weight has been placed on a body of research that has mostly failed to reject the null hypothesis that "the NMW has no effect on employment", with policy-makers wrongly interpreting p-values as telling us how likely it is that the NMW does have an adverse effect on employment. Second, this literature has employed difference-in-difference (DiD) designs, even though there are significant challenges in conducting appropriate inference in such designs, and they can have very low power when inference is conducted appropriately. Our hypothesis is that the existing research has under-stated the statistical imprecision of its key parameter estimates.

We proceed by estimating a model very similar to that in Bryan et al. (2013), one of the most recent and comprehensive studies on the impact of the NMW on employment, and commissioned by the LPC for their 2013 report. Our identification strategy and data are the same as in Bryan et al. (2013), but we depart from that study's approach - and that of previous studies cited in Section 2 - in four ways. First, we follow recent suggestions for best practice for undertaking inference in difference-in-difference designs, and show that the standard errors estimated in the previous UK literature are downward-biased. Second, we focus explicitly on confidence intervals, rather than reporting p-values or focusing on the binary outcome of whether the null hypothesis of no impact of the NMW on employment can be rejected. Third, we show what the estimated coefficients mean for economically-meaningful concepts such as labour demand elasticities. Finally, we calculate minimum detectable effects (Bloom, 1995), which indicate how large the true employment effects would have to be (or how large would the true labour demand elasticity have to be) for the methods employed in this literature to detect them with high probability.

3.1 Estimating the impact of the NMW on employment transitions using difference-in-differences

We investigate the impact of the NMW on transitions out of employment (alternatively, on job retention) over the period 1999q4 to 2010q1, using information on each of the annual upratings in that period.[3] We use data from the Labour Force Survey (LFS), which is comparable to the Current Population Survey (CPS) in the US, and collects information on employment status and other issues for a sample of the UK population. Individuals in the LFS are surveyed in five consecutive quarters, but, as information on earnings is asked only in the first and in the last interview and we need to measure earnings at the beginning of the period over which we measure employment transitions, our outcome measure is an individual's transition from employment over a 6 month period using the first and the third observation for each individual. We focus on individuals aged 22 and over.

We then allocate individuals into one of four different groups according to the their starting

---

[3]Further details of the approach, and of the final sample used, can be found in Bryan et al. (2013).

wage:[4] the treatment group is composed of workers who earn a wage w_{it} between the existent NMW enforced in year t and the upcoming year $t+1$ NMW uprating ($NMW_t \leq w_{it} < NMW_{t+1}$), and individuals in the control group have a salary that is slightly higher than the the upcoming year $t+1$ NMW ($NMW_{t+1} \leq w_{it} < m(NMW_{t+1})$; We set $m = 1.1$, so workers in our control group earn up to 10% more than the year t upcoming NMW uprating.)[5] We then classify all six-monthly transitions according to whether they span a NMW increase (these always happen on 1 October, so transitions from Q1 to Q3 and from Q4 to Q2 do not span an uprating, and transitions from Q2 to Q4 and from Q3 to Q1 do). The maintained assumption by the literature is that retention rates measured over a period that spans 1 October are potentially affected by a NMW uprating, and retention rates measured over a period that does not span an October are not affected by any NMW uprating.

Having done this, one can estimate the impact of the NMW on employment transitions with a multi-group, multi-period, difference-in-differences (DiD) design. Denoting our outcome variable by y_{igts} (a dummy variable that records whether individuals in work at time t are also in work 6 months later), letting the subscript g indicate the 4 groups defined according to the individual's initial wage, and letting the subscript s denote whether or not the transition spans a NMW increase on 1 October, then we get the following model:

$$y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt}d_{gs} + \mathbf{x}'_{igts}\gamma + \epsilon_{igts} \tag{1}$$
$$i = 1, ..., N; g = C, B, T, A; s = 0, 1; t = 2000, ..., 2011$$

where δ_{ts} is an interaction of the year and whether or not the transition straddles a NMW increase, α_{gt} is a time-varying group effect, d_{gs} is a binary policy variable that denotes whether the observation is affected by a minimum wage uprating (this varies by group g and whether the transition spans a NMW increase on 1 October, s), and $\boldsymbol{x}$ are individual-level control variables. β_{Tt} can be interpreted as the year t NMW uprating effect on job retention for the treatment group.

An alternative specification estimates the impact of a 1% rise in the NMW on job retention. The motivation for this is the large variation in the growth rate of the NMW: in 2001, the NMW rose by 10.8%, nine times as much as the rise in 2010 of 1.2%. In this alternative specification, we multiply the binary policy variable d_{gs} by the percentage change in the NMW at time t, ω_t.

---

[4]The LFS provides two measures of wages, a self-reported hourly wage and a derived hourly pay. We follow the literature (e.g. Stewart(2004a,b), Dickens and Draca (2005) and Dickens et al. (2012)) and use the self-reported hourly wage variable, as it is likely to be less affected by measurement error.

[5]We also define a Below NMW group that contains people who report an hourly wage w_{it} below the existent NMW ($w_{it} < NMW_t$), and an Above NMW group that is made up of workers paid more than m times the upcoming NMW ($w_{it} \geq m(NMW_{t+1})$). These latter two groups are kept in the sample to better identify the time effects; our specification allows their wage to be affected by changes to the NMW, but the impacts on these groups is allowed to be different from that on the treatment group.

This alternative model is:

$$y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt}d_{gs}\omega_t + \mathbf{x}'_{igts}\gamma + \epsilon_{igts} \qquad (2)$$
$$i = 1, ..., N; g = C, B, T, A; s = 0, 1; t = 2000, ..., 2011$$

where β_{Tt} is now the estimated impact of a 1% rise in the NMW at time t on job retention.

3.2 Inference in Difference-in-Differences with Grouped Errors

A broad literature has raised concerns about the accuracy of the inference in DiD designs when using the naïve estimates of the standard errors provided by OLS.

The first concern relates to the grouped error structure. In DiD designs, the error term ϵ_{igts} is unlikely to be iid, because an individual may have unobservable characteristics that are correlated with other individuals of the same group, or may be affected by common group shocks. In the case of these studies of the minimum wage, members of the treatment group are all located at the bottom of the wage distribution, and so it is highly plausible that they may have some common unobservable characteristics (low ability, low skills, etc.) or are influenced by the same economic shocks. A comprehensive specification of equation (1) which includes common group shocks φ_{gts} is:

$$y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt}d_{gs}\omega_t + \mathbf{x}'_{igts}\gamma + \varphi_{gts} + \xi_{igts} \qquad (3)$$
$$i = 1, ..., N; g = C, B, T, A; s = 0, 1; t = 2000, ..., 2011$$

and $\epsilon_{igts} = \varphi_{gts} + \xi_{igts}$.

A well-known result is that, in designs where the errors are within-group correlated and where a variable of interest does not vary within the group, the conventional OLS estimates of standard errors are seriously downward biased: this produces t-statistics that are too large and, accordingly, leads analysts to over-reject the null hypothesis of no treatment effect (Moulton, 1990). To the best of our knowledge, though, none of the research cited in Section 2 addresses this issue: most studies use heteroscedasticity-robust standard errors, but do not allow for any dependence between different individuals.

Various standard error corrections have been proposed to account for the common group structure in the random disturbances ϵ_{igts} and thus produce tests of the correct size: these include a parametric adjustment using intra-class correlations (Moulton, 1990), the Liang and Zeger (1986) generalization of the White (1980) heteroskedastic robust covariance matrix, a feasible GLS estimator (Hansen, 2007), and methods based on the bootstrap (Cameron et al., 2008). However, many of these techniques lead to t-statistics for the null hypothesis of no treatment effect that are asymptotically normal distributed only as the number of groups tend to infinity (e.g. Donald and Lang (2007), Angrist and Pischke (2008), Cameron et al. (2008), Brewer et al. (2013)). When the number of groups is small - and we have only 4 - the critical values of the asymptotic normal distribution will be a poor approximation to the critical values for the

Wald tests, and using critical values from the standard normal distribution when the number of groups is small will lead us to over-reject the null hypothesis. But a method that does lead to Wald tests with a known distribution in cases with few clusters is a two-step estimator: under specific circumstances where the common group shock φ_{gts} is normal, homoscedastic and uncorrelated between groups and over time, Donald and Lang (2007) show this two-step estimator produces tests of the correct size.

The two-step estimator consists in retrieving estimates in two stages: in the first step, the dependent variable is regressed on dummies that identify cell membership and all the variables which vary within cells. In the second stage, the set of parameters associated with the cell membership are regressed on the variables which do not vary within cells. In the Donald and Lang (2007) two-step estimator, the concept of cell or cluster is essential: errors within a cell are allowed to correlated, but shocks between cells are assumed to be independent.

In our study, we define a cell as the interaction of group, year and transition-type, giving us 80 cells (4 groups, 10 years of data, and 2 transition types). The first stage regression is then:

$$y_{ic} = \mathbf{x}'_{ic}\gamma + \sum_{c=1}^{80} I_c \mu_c + \epsilon_{ic} \tag{4}$$

$$i = 1, ..., N; c = 1, ..., 80$$

where I_c is a dummy variable which identifies the c-th cell, and $\mathbf{x}$ are the controls that vary within-cell. [6]

In the second stage, the coefficients associated with the cell membership dummies μ_c are regressed on the cell-invariant variables. In our example, this second step is:

$$\hat{\mu}_c = \delta_{ts} + \alpha_{gt} + \beta_{gt} d_c + \epsilon_c \tag{5}$$

$$c = 1, ..., 80; t = 2000, ..., 2011$$

where d_c is a dummy indicating whether the c-th cell is affected by a minimum wage uprating.

As Donald and Lang (2007) observe, standard errors cannot be estimated with this approach for a two-by-two DiD design, as the second step is an exactly-identified regression, with 4 coefficients (2 time effects, 1 group effect and 1 policy effect) being estimated from 4 data points. Clearly, the same is true for other types of DiD where the second step is an exactly-identified regression. What might not be immediately clear is that this situation also holds when we apply the two-step to the unrestricted equation (1). The argument runs as follows: in equation (1), identification of the impact of the NMW arises because, in every year, we observe transitions that either do or do not span an uprating, and where these transitions can come from 1 of 4 groups, three of whom are deemed to be affected by the uprating (but in different ways). Accordingly, each year of data effectively provides us with a 4-group, 2-period DiD (if we understand the 2 *periods* to refer to *whether or not a transition spans an uprating*) where the policy affects 3 groups

---

[6]All the control variables $\mathbf{x}$ in the equation (3) vary within-cell.

in the second period with impacts that are allowed to be different. This means that applying the two-step method to such data would lead to a second step regression with zero degrees of freedom (with 2 time effects, 3 group effects and 3 policy effects estimated from 8 data-points). Accordingly, equation (1), which is the main specification in Bryan et al. (2013), and which allows both for the group effects to be different in each year and for the impact of each year's uprating on the three treated groups to be different, would also give a second-step regression in equation (5) with zero degrees of freedom (it is equivalent to estimating 10 separate, exactly-identified, DiDs, where each DiD is a 4-group, 2-period DiD that is producing 8 coefficients). To address this problem, we make two restrictions to the (overly) flexible model in equation (1) so as to be able to undertake inference, notably: each group has a constant effect on job retention over time (so we estimate α_g rather than α_{gt}, and the impact of an NMW uprating effect is constant over time (but different for each group) (so we estimate β_g rather than β_{gt}). This gives us the following:

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs} + \mathbf{x}'_{igts}\gamma + \epsilon_{igts} \tag{6}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

and the second stage is:

$$\hat{\mu}_c = \delta_{ts} + \alpha_g + \beta_g d_c + \epsilon_c \tag{7}$$
$$c = 1, ..., 80; t = 2000, ..., 2011$$

β_T can then be interpreted as the (weighted) average impact of a NMW uprating on job retention for the treatment group.

For the variant where we estimate the impact of a 1% rise in the NMW on job retention, the amended model is:

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs}\omega_t + \mathbf{x}'_{igst}\gamma + \epsilon_{igst} \tag{8}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

and the second stage in the Donald and Lang two-step estimator is:

$$\hat{\mu}_c = \delta_{ts} + \alpha_g + \beta_g d_c\omega_t + \epsilon_c \tag{9}$$
$$c = 1, ..., 80; t = 2000, ..., 2011$$

where β_T is the (weighted) average effect of a 1% NMW rise on the probability of remaining employed.

A second concern about inference in DiD studies, as initially noted by Bertrand et al. (2004), is that the level of uncertainty surrounding the estimated policy effect in DiD designs will likely be increased by positive serial correlation in the group-time shocks, as the variable of interest in DiD designs is itself highly serially-correlated. Put more directly, if the group-time shocks

φ_{gts} exhibit positive serial correlation that is ignored in estimation, then the resulting estimates of the standard errors will likely be biased downwards; it is for this reason that the Bertrand et al. (2004) recommendation is that analysts NOT cluster errors at the level of the group-time interaction, as doing so leads to incorrect inference if there is serial correlation in the φ_{gts}. In principle, this could cause a problem for our approach based on the Donald and Lang (2007) two-step, as we assume that each cell, given by a group-time-span interaction, is independent of the others. But we test for serial correlation by estimating a first order auto-regressive model of the residuals by group. The results are shown in Table 10 (Appendix B), which displays the first order auto-regressive coefficients from our estimates of equation (8). Residuals for the treatment group exhibit small, negative degree of serial correlation for men, although that for women is larger, at 0.40 (the confidence intervals for both span zero). However, our research design is much less subject to problems caused by positive serial correlation in the group-time shocks because our variable of interest is *negatively* serially correlated, as it turns on and off repeatedly (recall our analysis uses data that covers 10 years, within each of which we observe two 6-month transitions that span an uprating, and two 6-month transitions that do not).

3.3   Minimum detectable effects

Following Bloom (1995) and Brewer et al. (2013), we also use the concept of Minimum Detectable Effects (MDEs) as a way of illustrating the power of these DiD designs. The MDE combines the concepts of significance level α and power π with the standard error of the parameter of interest β, and is the smallest true effect that would lead a test with size α to reject the null hypothesis of no treatment with a certain probability π. We can view high values of the MDE as suggesting that the estimator is low powered, whereas low values show that the analyst should be able to detect even small effects with a certain probability π. The MDE is defined as

$$MDE(x) = \hat{se(\hat{\beta})}[c_{1-\alpha/2} - p^t_{1-x}]$$

where $\hat{se(\hat{\beta})}$ is the estimated standard error for the coefficient $\hat{\beta}$, c_u is the critical value of the $(1 - \alpha/2)$-th percentile of the t_{C-1} distribution and p^t_{1-x} is the $(1 - x)$th percentile of the t-statistic under the null hypothesis of no treatment effect. The formula makes clear that either large standard errors, or a "high" threshold for determining statistical significance (i.e. a low value of α) both lead to large MDEs. Our implementation of this notes that C is the number of cells in our second stage (i.e. 80), and a standard value for π in the literature is 0.8, so p^t_{1-x} turns into the 20th percentile of a t-distribution with 79 degrees of freedom.

3.4   From the impact of the minimum wage on job retention to employment demand elasticities

Our estimated coefficient β_T from equation (7) tells us about the (weighted) average impact of a NMW uprating on 6-month retention rates (and the coefficient from equation (9) tells us about the impact of a 1% rise in the NMW on the same). This is not a very helpful summary of the

way that the NMW affects employment: even if it were the case that researchers and policy-makers considered not just the p-value of β_T but also its confidence interval, we imagine many would find it hard to know whether any particular value of β_T were large or small. Accordingly, and to facilitate comparisons with other studies, we translate our estimated coefficients into implied elasticities. We do this in two ways.

First, we turn our estimates of the impact of the NMW on 6-month retention rates β_T into an estimate of the elasticity of the 6-month job retention rate to the NMW. This elasticity, η_{JR}, is defined in the usual way:

$$\eta_{JR} = \Delta RR/RR/\Delta W/W \tag{10}$$

For the model in which we estimate the average impact of a NMW uprating, then ΔRR is the coefficient β_T (i.e. the change in the retention rate for the treatment group thanks to an increase in the NMW), RR is the counterfactual retention rate (i.e. the proportion of workers who would have remained in employment if the NMW had not been changed, which we can calculate as the observed retention rate less β_T)[7], and $\Delta W/W$ is 0.049, the average size of the NMW upratings in the 2000-2010 period. [8]

Our estimates are even easier to interpret and to compare with other studies if we calculate the implied elasticity of employment with respect to the minimum wage. If we begin with equation of motion for employment:

$$E_t = f(1 - E_{t-1}) + RRE_{t-1} \tag{11}$$

where E_t is the employment rate at time t, f is the job finding probability for unemployed people and RR is the job retention rate for employed people, then, in steady state

$$E_t = \frac{f}{1 + f - RR}. \tag{12}$$

As shown in Appendix A, from this we can express the elasticity of employment with respect to the minimum wage as:

$$\eta_{ER} = \frac{\Delta E/E}{\Delta NMW/NMW} = \frac{\beta}{1 + f - RR}/\frac{\Delta NMW}{NMW} \tag{13}$$

where ΔE is the change in the employment rate, and f is the 6-monthly job finding probability.[9] This calculation assumes that f is not affected by changes in the NMW; Bryan et al. (2013) investigate whether job-finding rates amongst workers who would be paid the minimum wage respond to changes in the NMW, concluding that "Estimates within each method are not always robust to changes in model specification and given the additional differences we see across methods, we conclude that there is no empirical support for the hypothesis that the NMW has had an impact on job entry."

---

[7]We estimate the counterfactual job retention probability RR to be 0.875 for men and 0.902 for women.

[8]For the model in which we estimate the impact of a 1 % rise in the NMW, then ΔRR is the coefficient β_T and $\Delta W/W$ is 0.01.

[9]Our main estimates set f to 0.33 for men and 0.22 for women, but we use 0.2, 0.4 and 0.6 as variants. Given the 6-monthly retention rates that we observe in the LFS, $f = 0.33$ for men corresponds to an equilibrium employment rate of 0.79 and $f = 0.22$ for women corresponds to an employment rate of 0.66.

4 Results

4.1 The estimated impact on a NMW uprating on job retention

Table 2 shows estimates of the (weighted) average impact of an NMW uprating on the probability of remaining employed. In the top panel, we report estimates of β_T from equation (7) where we conduct inference using the Donald and Lang (2007) two-step estimator; in the bottom panel, we present estimates of β_T that come from estimating equation (6) with OLS and calculating heteroscedasticity-robust standard errors.

OLS estimates on the micro-data suggest that a NMW uprating increases the probability of remaining employed. When the Donald and Lang (2007) two-step estimator is implemented (and controls are included in the specification), then the estimated impact of an NMW uprating is to cut the job retention rate by 0.6 percentage points for men and increase it by 0.4 percentage points for women. [10] [11]

None of the estimates is statistically different from zero: like the previous UK literature, we fail to find an impact of the NMW on the probability of remaining employed. But it is extremely instructive to look at the confidence intervals associated with our estimates. These reveal two things. First, the Donald and Lang (2007) two-step standard errors are twice as large as the OLS standard errors for women, and 59% larger for men: this is consistent with our belief that within-cell correlation in the error terms is an important issue in the DiD. Second, the confidence intervals in Table 2 reveal that large positive and negative impacts of a NMW uprating on employment would also not fail to be rejected by this data at a significance level $\alpha = 0.05$. For example, we cannot reject that an average NMW uprating reduces the probability of remaining employed by 6.7 percentage points for men, or that it increases the job retention rate by 5.4 percentage points. These confidence intervals are wide, and illustrate that the data and the research design are not especially helpful in allowing us to make inferences about the existence of a negative impact of the NMW on job retention. The corollary of these large standard errors is that the DiD designs typically used in the UK NMW studies has a low power to detect a plausibly-sized true NMW effect. Our calculations of the MDEs show that an NMW uprating would need to decrease (increase) job retention rate for men by 8.6 percentage points to have an 80 % chance of being detected, and by 5.4 percentage points for women.

---

[10]These point estimates of β_T are slightly different under the two methods because the coefficient β_T represents the weighted average of the impact all of the NMW upratings on job retention rates, and the effective weights in this calculation are different when using OLS on the micro-data in equation 1, and when using OLS on cell-level averages in equation 5.

[11]The point estimates in Table 2 do not correspond to the results presented in Bryan et al. (2013). As discussed in Section 3.2, the model estimated in Bryan et al. (2013), corresponding to our equation (1), is exactly identified under the two-step approach. However, we are able to replicate results in Bryan et al. (2013) to the 2nd decimal point when we also estimate equation (1) with OLS, and calculate heteroscedasticity-robust standard errors: see Table 9 in Appendix B.

Table 2: Estimates of Average Impact of a NMW Uprating on Job Retention

Gender	Method	Controls	β_T	Std. Errors	Confidence Interval		MDE
Men	Two Step	yes	-0.0064	0.0301	(-0.0668	0.0540)	±0.0855
Men	Two Step	no	0.0044	0.0337	(-0.0632	0.0719)	±0.0955
Women	Two Step	yes	0.0036	0.0186	(-0.0337	0.0409)	±0.0528
Women	Two Step	no	0.0064	0.0167	(-0.0272	0.0400)	±0.0475
Men	OLS	yes	0.0049	0.0189	(-0.0321	0.0420)	±0.0535
Men	OLS	no	0.0117	0.0183	(-0.0241	0.0476)	±0.0519
Women	OLS	yes	0.0036	0.0090	(-0.0140	0.0212)	±0.0255
Women	OLS	no	0.0043	0.0089	(-0.0132	0.0217)	±0.0252

$^*p < 0.10^{**}p < 0.05^{***}p < 0.001$

Control variables are age, gender, marital status, highest level of education, region, ethnicity, number of children, age left education, and whether the respondent has health problems, industry, public sector, occupation, and tenure. Two-step estimates are from equation (7); OLS estimates are from equation (6) and are associated to heteroscedasticity-robust standard errors

4.2 The estimated impact on a 1% rise in the NMW on job retention

The specification in equations (6) and (7) that estimates the average impact of a NMW uprating does not take into account that the size of the upratings has varied over time (see Table 1). Table 3 therefore presents estimates of the impact of a 1% rise in the NMW on transitions from employment. We present estimates from equation (8) using OLS, and from equation (9) using the two-step. The point estimates are that a NMW increase lowers the probability of remaining employed for both men and women, with a 10 % growth in the minimum wage estimated to reduce the job retention rates by as much as 6.2 percentage points for men, and by 1 percentage point for women. Since the average uprating size over 2000-2010 is 4.9%, the annual review of the NMW reduces on average job retention by 3 percentage points for men and by 0.5 percentage points for women.

Like the previous literature, none of the point estimates is statistically significant. However, as in Table 2, the size of the standard errors sharply increases when the Donald and Lang (2007) two-step estimator is implemented: in the specification that includes controls, the standard error under the two-step is 57% larger than the OLS heteroscedasticity-robust standard error for men, and 123% larger for women. Again, the large standard errors obtained through the Donald and Lang (2007) two-step estimator lead to wide confidence intervals: the range of impacts that cannot be rejected includes that the job retention rate might decline by nearly 16 percentage points, or increase by more than 3 percentage points, in response to a 10 % NMW rise. The estimated MDEs in column (5) indicate that one would need a true effect on the probability of remaining employed of about 13 percentage points for men, and 8 percentage points for women, in response to a 10 % NMW increase to have 80 % probability of detecting it.

Table 3: Estimates of Impact of a 1% rise in the NMW on Job Retention

Gender	Method	Controls	β_T	Std. Errors	Confidence Interval		MDE
Men	Two Step	yes	-0.0062	0.0047	(-0.0157	0.0032)	±0.0133
Men	Two Step	no	-0.0039	0.0053	(-0.0146	0.0067)	±0.0150
Women	Two Step	yes	-0.0010	0.0029	(-0.0069	0.0049)	±0.0083
Women	Two Step	no	-0.0007	0.0026	(-0.0060	0.0046)	±0.0075
Men	OLS	yes	-0.0050	0.0030	(-0.0109	0.0010)	±0.0086
Men	OLS	no	-0.0030	0.0030	(-0.0088	0.0029)	±0.0085
Women	OLS	yes	-0.0010	0.0013	(-0.0036	0.0016)	±0.0038
Women	OLS	no	-0.0009	0.0013	(-0.0035	0.0017)	±0.0038

$^*p < 0.10^{**}p < 0.05^{***}p < 0.001$

Control variables are age, gender, marital status, highest level of education, region, ethnicity, number of children, age left education, and whether the respondent has health problems, industry, public sector, occupation, and tenure. Two-step estimates are from equation (9); OLS estimates are from equation (8) and are associated to heteroscedasticity-robust standard errors

4.3 Implied elasticities

As discussed in Section 3, it is easier to assess whether the point estimates, and the range of estimates inside the confidence intervals, are large or small if they are expressed in terms of elasticities that can be compared to those from other studies.

In Tables 4 and 5, we report the elasticities of job retention to the NMW that are implied by our estimated coefficients in (respectively) Table 2 and 3, using the formula in equation (10). The elasticities implied by the point estimates for β_T are -0.15 for men, and -0.08 for women. But the confidence intervals for these elasticities include large positive and negative elasticities, especially for men. As a result, the estimated MDEs are very large. Even if we take the smaller of the 2 MDEs across Tables 4 and 5, our estimates imply that this DiD design would detect a true effect with 80% probability only if the true job retention elasticity was greater than 1.5 for men, and greater than 0.9 for women.

Table 4: Implied elasticities of job retention with respect to the minimum wage, η_{JR}, based on the estimated impact of NMW upratings from Table 2

Gender	Method	Controls	η_{JR}	Confidence Interval		MDE
Men	Two Step	yes	-0.15	(-1.55	1.26)	±1.99
Men	Two Step	no	0.10	(-1.47	1.67)	±2.22
Women	Two Step	yes	0.08	(-0.76	0.92)	±1.19
Women	Two Step	no	0.14	(-0.61	0.88)	±1.07

$^*p < 0.10^{**}p < 0.05^{***}p < 0.001$

Elasticities calculated using equation (10).

Table 5: Implied elasticities of job retention with respect to the minimum wage, η_{JR}, based on the estimated impact of a 1% rise in the NMW from Table 3

Gender	Method	Controls	η_{JR}	Confidence Interval		MDE
Men	Two Step	yes	-0.71	(-1.79	0.37)	±1.52
Men	Two Step	no	-0.45	(-1.66	0.76)	±1.71
Women	Two Step	yes	-0.11	(-0.76	0.54)	±0.92
Women	Two Step	no	-0.07	(-0.66	0.51)	±0.83

$^*p < 0.10^{**}p < 0.05^{***}p < 0.001$
Elasticities calculated using equation (10).

For comparison, Table 6 reports the range of job retention elasticities observed in the minimum wage literature that uses individual longitudinal data from the US and Canada. Unlike studies that have used aggregate data and find small *insignificant* positive effects of the minimum wage on employment (e.g. Card (1992), Card and Kruger (1994, 1995, 2000)), studies using individual longitudinal data tend to find that the minimum wage has a *significant* detrimental impact on job retention rates for workers likely to be affected by a minimum wage hike (e.g. young people).

Our estimate of β_T from equation (8) implies a job retention elasticity of -0.7 for adult men, this is not statistically significant. However, it is close to the finding in Sabia et al. (2012), and larger than what Currie and Fallick (1996), Neumark et al. (2004) and Campolieti et al. (2005) obtain for teenagers and youths. The minimum wage literature in the US and Canada that uses individual longitudinal data does not apply techniques to account for within-cell shocks or serial correlation in the treatment status. However, the fact that they find statistically significant job retention elasticities that are considerably smaller than our (statistically insignificant) point estimate strengthens our idea that the DiD used in the UK literature has low power to detect disemployment effects of a realistic magnitude.

Table 6: Estimated elasticities of job retention with respect to the minimum wage, η_{JR}, from studies using US and Canadian data

Study	Country	Group	η_{JR}
Currie and Fallick (1996)	US	Workers affected by the new minimum	-0.19 to -0.24
Yuen (2003)	Canada	Teenagers (16-19)	-0.75 to -0.84
		young adults (20-24)	-1.23 to -1.77
Neumark et al. (2004)	US	Teens (15-19)	-0.18 to -0.24
		Youths (15-24)	-0.13 to -0.16
Campolieti et al. (2005)	Canada	Youths (16-24)	-0.33 to -0.54
Sabia et al. (2012)	US	Youths (16-24)	-0.7

Finally, Table 7 and 8 shows the elasticities of employment with respect to the NMW implied

by our estimates of β_T from Table 2 and 3, calculated using equation (13).[12] Our (statistically insignificant) point estimates of β_T imply employment elasticities of -0.3 for men, and $+0.2$ for women. What is more important, though, is the range of values for the elasticity of employment with respect to the minimum wage within the confidence intervals. For example, the confidence interval for our estimate of β_T is consistent with employment elasticities as large as -3.4 for men and -2.2 for women. Finally, our calculations of the MDEs again support the thesis that, if we use techniques to obtain correctly sized tests, the power of these DiDs designs to detect a real impact of the NMW on employment is extremely low: taking the minimum of our two estimated MDEs, we would need an elasticity of employment with respect to the minimum wage of -2.9 for men and -2.6 for women to spot it with 80% probability.

Table 7: Implied elasticities of employment with respect to the minimum wage, η_{ER}, based on the estimated impact of NMW upratings from Table 2

Gender	Method	Controls	η_{ER}	Confidence Interval		MDE
Men	Two Step Estimator	yes	-0.29	(-2.98	2.41)	±3.82
Men	Two Step Estimator	no	0.20	(-2.82	3.21)	±4.27
Women	Two Step Estimator	yes	0.23	(-2.16	2.62)	±3.38
Women	Two Step Estimator	no	0.41	(-1.74	2.56)	±3.04

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.001$
Elasticities calculated using equation (13) with $f = 0.33$ for men and $f = 0.22$ for women.

Table 8: Implied elasticities of employment with respect to the minimum wage, η_{ER}, based on the estimated impact of a 1% rise in the NMW from Table 3

Gender	Method	Controls	η_{ER}	Confidence Interval		MDE
Men	Two Step Estimator	yes	-1.37	(-3.44	0.70)	±2.93
Men	Two Step Estimator	no	-0.87	(-3.20	1.46)	±3.30
Women	Two Step Estimator	yes	-0.31	(-2.16	1.55)	±2.62
Women	Two Step Estimator	no	-0.21	(-1.88	1.46)	±2.36

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.001$
Elasticities calculated using equation (13) with $f = 0.33$ for men and $f = 0.22$ for women.

---

[12]Table 11 and 12 in Appendix B show the employment rate elasticities for a range of values of the job finding probability f that varies from 0.2 to 0.6. The magnitude of the employment elasticities falls with the job finding probability f: *ceteris paribus*, if finding a job is easier, then job losses would be offset by more non-employed persons who find a job, and the employment rate would be less sensitive to a change in the NMW.

5 Discussion and Conclusions

The UK is unusual for the fact that economic research on the impact of the NMW on employment plays a decisive role in the setting of the NMW each year. Our concern is that too much weight has been placed on a body of research that has mostly failed to reject the null hypothesis that "the NMW has no effect on employment": policy-makers seem to have wrongly interpreted p-values as telling us how likely it is that the NMW does have an adverse effect on employment, and have not paid attention to the range of values that would also not be rejected. And this concern is compounded by the fact that the UK literature has employed difference-in-difference (DiD) designs, even though there are significant challenges in conducting inference appropriately in such designs, meaning that the existing research has likely under-stated the statistical imprecision of its key parameter estimates.

In this paper, we re-evaluate the UK research on the employment effects of the minimum wage. Our study follows Bryan et al. (2013), one of the most recent and comprehensive reports commissioned on the NMW effects on employment and, as in the UK NMW literature, we also cannot reject the null that the NMW upratings had no impact on job retention. However, when we apply the Donald and Lang (2007) two-step estimator to conduct correct inference, the range of effects that also cannot be rejected is extremely large, and include large positive and negative values of the NMW impacts on employment. Moreover, using Bloom (1995)'s minimum detectable effects, we find that the DiD design typically used in the literature has low power to detect a real NMW impact. For example, in our preferred specification, one would need that the job retention rate in reality falls by 15 % in response to a 10 % NMW rise to be able to detect it with 80 % probability using the data and research design typically used in the UK work. This impact would correspond to a job retention elasticity of −1.5 and an employment elasticity with respect to the minimum wage of about −3.

Our study raises concerns for the routine application of a DiD designs when assessing the impact of the NMW on employment. Although we also do not find any *statistical significant* impact, the confidence intervals we obtain suggest that the standard research design used in the assessment of the UK's NMW is not very informative. In turn, this casts doubt on the consensus that the UK NMW does not harm employment. We therefore recommend a reconsideration of the combined use of a DiD designs with existing UK data sources when evaluating the NMW impact on employment.

References

Angrist, J. D. and J.-S. Pischke (2008). *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton University Press.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How Much Should We Trust Differences-in-Differences Estimates? *Quarterly Journal of Economics 119*(1).

Bloom, H. S. (1995). Minimum Detectable Effects: A Simple Way to Report the Statistical Power of Experimental Designs. *Evaluation Review 19*(5), 547–556.

Brewer, M., T. F. Crossley, and R. Joyce (2013). Inference with Difference-in-Differences Revisited. IZA Discussion Paper No. 7742.

Bryan, M., A. Salvatori, and M. Taylor (2013). The Impact of the National Minimum Wage on Employment Retention, Hours and Job Entry. Research Report for the Low Pay Commission. Institute for Social and Economic Research, University of Essex.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-Based Improvements for inference with clustered errors. *The Review of Economics and Statistics 90*(3), 414–427.

Campolieti, M., T. Fang, and M. Gunderson (2005). Minimum wage impacts on youth employment transitions, 1993–1999. *Canadian Journal of Economics/Revue canadienne d'économique 38*(1), 81–104.

Card, D. (1992). Do Minimum Wages Reduce Employment? A Case Study of California, 1987-89. *Industrial and Labor Relations Review 46*(1), pp. 38–54.

Card, D. and A. B. Krueger (1995). *Myth and Measurement: The New Economics of the Minimum Wage.* Princeton University Press.

Card, D. and A. B. Krueger (2000). Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: reply. *American Economic Review*, 1397–1420.

Coats, D. (2007). *The National Minimum Wage: Retrospect and Prospect.* Work Foundation.

Cohen, J. (1994). The Earth is Round ($p \leq .05$). *American Psychologist 49*(12), 997.

Currie, J. and B. C. Fallick (1996). The Minimum Wage and the Employment of Youth Evidence from the NLSY. *The Journal of Human Resources 31*(2), pp. 404–428.

Dickens, R. and M. Draca (2005). The Employment Effects of the October 2003 Increase in the National Minimum Wage. Research Report for the Low Pay Commission. Centre for Economic Performance, London School of Economics and Political Science.

Dickens, R., R. Riley, and D. Wilkinson (2012). Re-examining the impact of the National Minimum Wage on earnings, employment and hours: the importance of recession and firm size. Research Report for the Low Pay Commission.

Donald, S. G. and K. Lang (2007). Inference with Difference-in-Differences and Other Panel Data. *The Review of Economics and Statistics 89*(2), 221–233.

Finn, D. (2005). The National Minimum Wage in the United Kingdom.

Hansen, C. B. (2007). Generalized Least Squares Inference in Panel and Multilevel Models with Serial Correlation and Fixed Effects. *Journal of Econometrics 140*(2), 670–694.

Ioannidis, J. P. (2005). Why Most Published Research Findings Are False. *PLoS medicine 2*(8), e124.

Liang, K.-Y. and S. L. Zeger (1986). Longitudinal Data Analysis Using Generalized Linear Models. *Biometrika 73*(1), 13–22.

Lourie, J. (1999). *National Minimum Wage*. Great Britain, Parliament, House of Commons, Library.

Low Pay Commission (1998). *The National Minimum Wage: First Report of the Low Pay Commission.*

Low Pay Commission (2000). *The National Minimum Wage. The story so far; Second Report of the Low Pay Commission.*

Low Pay Commission (2003). *The National Minimum Wage. Fourth Report of the Low Pay Commission.*

Low Pay Commission (2006). *National Minimum Wage. Low Pay Commission Report 2006.*

Low Pay Commission (2007). *National Minimum Wage. Low Pay Commission Report 2007.*

Low Pay Commission (2008). *National minimum wage: Low Pay Commission report 2008.*

Low Pay Commission (2009). *National minimum wage: Low Pay Commission report 2009.*

Low Pay Commission (2013). *National Minimum Wage. Low Pay Commission Report 2013.*

Machin, S., A. Manning, and L. Rahman (2002). *Where the Minimum Wage Bites Hard: the Introduction of the UK National Minimum Wage to a Low Wage Sector.* Centre for Economic Performance, London School of Economics and Political Science.

Machin, S. and J. Wilson (2004). Minimum wages in a low-wage labour market: Care homes in the uk. *The Economic Journal 114*(494), C102–C109.

Moulton, B. R. (1990). An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Units. *The Review of Economics and Statistics*, 334–338.

Neumark, D., M. Schweitzer, and W. Wascher (2004). Minimum Wage Effects throughout the Wage Distribution. *Journal of Human Resources 39*(2), 425–450.

Sabia, J. J., R. V. Burkhauser, and B. Hansen (2012). Are the Effects of Minimum Wage Increases Always Small-New Evidence from a Case Study of New York State. *Industrial & Labour Relation Review 65*, 350.

Sterne, J. A., G. D. Smith, and D. Cox (2001). Sifting the evidence-what's wrong with significance tests? Another comment on the role of statistical methods. *Bmj 322*(7280), 226–231.

Stewart, M. B. (2004a). The employment effects of the National Minimum Wage. *The Economic Journal 114*(494), C110–C116.

Stewart, M. B. (2004b). The Impact of the Introduction of the U.K. Minimum Wage on the Employment Probabilities of Low-Wage Workers. *Journal of the European Economic Association 2*(1), 67–97.

White, H. (1980). A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity. *Econometrica: Journal of the Econometric Society*, 817–838.

Yuen, T. (2003). The Effect of Minimum Wages on Youth Employment in Canada A Panel Study. *Journal of Human Resources 38*(3), 647–672.

Ziliak, S. T. and D. N. McCloskey (2004). Size matters: the standard error of regressions in the American Economic Review. *The Journal of Socio-Economics 33*(5), 527–546.

Appendix A: derivation of formula for the elasticity of employment with respect to the minimum wage

The law of motion for the employment rate at time t states that it is the sum of in-flows from unemployment, and the stock workers who retain their jobs from the previous period.

$$E_t = f(1 - E_{t-1}) + RRE_{t-1} = f - fE_{t-1} + RRE_{t-1} \tag{14}$$

where E_t is the employment rate, f the job finding probability, and RR the retention rate. Then,

$$E_t + fE_t - RRE_t = f, \tag{15}$$

and the employment rate in steady state is

$$E_t = \frac{f}{1 + f - RR} \tag{16}$$

The definition of the elasticity of employment with respect to the NMW, η_{ER}, is

$$\eta_{ER} = \frac{\Delta E / E}{\Delta NMW / NMW} \tag{17}$$

If we assume that a change in NMW does not affect the job-finding probability f, i.e.

$$\frac{df}{dNMW} = 0 \tag{18}$$

then we can express the change in employment rate in response to a NMW increase, ΔE, as

$$\Delta E = \frac{dE}{dNMW} = \frac{\partial E}{\partial f} \cdot \frac{df}{dNMW} + \frac{\partial E}{\partial RR} \cdot \frac{dRR}{dNMW} = 0 + \frac{\partial E}{\partial RR} \cdot \frac{dRR}{dNMW} \tag{19}$$

and

$$\frac{\partial E}{\partial RR} = \frac{\partial (f/(1 + f - RR))}{\partial RR} = \frac{0 - f(-1)}{(1 + f - RR)^2} = \frac{f}{(1 + f - RR)^2} \tag{20}$$

$$\frac{dRR}{dNMW} = \beta \tag{21}$$

the employment rate elasticity η_{ER} is

$$\eta_{ER} = \frac{\Delta E / E}{\Delta NMW / NMW} = \frac{\frac{f}{(1+f-RR)^2} \cdot \frac{dRR}{dNMW}}{\frac{f}{1+f-RR}} \bigg/ \frac{\Delta NMW}{NMW} = \frac{\beta}{1 + f - RR} \bigg/ \frac{\Delta NMW}{NMW} \tag{22}$$

Two implications of the formula are remarkable: the employment rate elasticity has the same sign as the effect of a NMW increase. If the parameter of interest β is negative (a NMW hike has an adverse effect on job retention), the employment rate decreases. Furthermore, the employment rate elasticity is increasing (decreasing) with job finding probability when the parameter of interest β is negative (positive). In fact, the first derivative of η_{ER} with respect to f is

$$\frac{\partial \eta_{ER}}{\partial f} = \frac{\partial[(\beta/(1+f-RR))/\Delta NMW/NMW]}{\partial f} = \frac{NMW}{\Delta NMW} \cdot \frac{0-\beta}{(1+f-RR))^2} \quad (23)$$
$$= \frac{\Delta W}{W} \cdot \frac{-\beta}{(1+f-RR)^2}$$

Since the denominator is always positive, the sign of the first derivative depends on β.

Appendix B: supplementary results

Table 9: Replication of impact of NMW on job retention presented in Bryan et al. (2013)

| | Bryan et al. (2013) | Replication | Bryan et al. (2013) | Replication |
Year t	Men	Men	Women	Women
2000	-0.040	-0.040	0.012	0.013
2001	-0.177**	-0.179**	-0.028	-0.028
2002	0.069	0.072	0.018	0.018
2003	-0.106	-0.105	-0.010	-0.009
2004	-0.033	-0.074	-0.010	-0.002
2005	-0.002	-0.002	-0.016	-0.016
2006	0.103**	0.104**	0.001	0.001
2007	0.057	0.056	0.037	0.037
2008	0.098*	0.095*	-0.017	-0.016
2009	0.009	0.010	0.037	0.037

$^*p < 0.10^{**}p < 0.05^{***}p < 0.001$
Control variables used in the DIDs model are age, gender, marital status, highest level of education, region, ethnicity, number of children, age left education, and whether the respondent has health problems, industry, public sector, occupation, and tenure.
Bryan et al. (2013) results are from their Tables 7-8
Model that identifies the uprating effect is $y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt}d_{gs} + \mathbf{x}'_{igts}\gamma + \epsilon_{igts}$

Table 10: Estimates of first-order autoregression coefficient of the residuals from equation (8).

	Autoregressive Coefficients			
$\hat{\rho}_C$	-0.12	-0.24	0.34	0.44
$\hat{\rho}_T$	-0.03	-0.17	0.32	0.40
$\hat{\rho}_A$	0.28	0.23	0.49	0.51
$\hat{\rho}_B$	0.26	0.19	0.31	0.43
Gender	Men	Men	Women	Women
Controls	No	Yes	No	Yes

We estimated the model $\epsilon_{gt} = \rho_g \epsilon_{gt-1} + \nu_t$

ρ_C is the autoregressive coefficient for control group,

ρ_T for treatment group, ρ_A for Above NMW_t group,

ρ_B for Below NMW_t group

$^*p < 0.10 ^{**}p < 0.05 ^{***}p < 0.001$

Table 11: Implied employment elasticities, η_{ER}, derived from the estimated impact of NMW upratings in Table 2 with different values of job-finding probability f

Gender	Method	Controls	β_T	f=0.2	f=0.4	f=0.6
Men	Two Step Estimator	yes	-0.006	-0.40	-0.25	-0.18
Men	Two Step Estimator	no	0.004	0.27	0.17	0.12
Women	Two Step Estimator	yes	0.004	0.25	0.15	0.11
Women	Two Step Estimator	no	0.006	0.44	0.26	0.19

$^*p < 0.10 ^{**}p < 0.05 ^{***}p < 0.001$

Elasticities calculated using equation (13).

Table 12: Implied employment elasticities, η_{ER}, derived from the estimated impact of a 1% rise in the NMW in Table 3 with different values of job-finding probability f

Gender	Method	Controls	β_T	f=0.2	f=0.4	f=0.6
Men	Two Step Estimator	yes	-0.006	-1.92	-1.19	-0.86
Men	Two Step Estimator	no	-0.004	-1.22	-0.75	-0.54
Women	Two Step Estimator	yes	-0.001	-0.33	-0.20	-0.14
Women	Two Step Estimator	no	-0.001	-0.22	-0.13	-0.10

$^*p < 0.10 ^{**}p < 0.05 ^{***}p < 0.001$

Elasticities calculated using equation (13).