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DISCUSSION PAPER SERIES

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ABSTRACT

What Do We Really Know about the Employment Effects of the UK's National Minimum Wage?*

A substantial body of research on the UK's National Minimum Wage (NMW) has concluded that the the NMW has not had a detrimental effect on employment. This research has directly influenced, through the Low Pay Commission, the conduct of policy, including the subsequent introduction of the National Living Wage (NLW). We revisit this literature and offer a reassessment, motivated by two concerns. First, much of 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 (positive or negative) impacts on employment that are consistent with the data. In our re-analysis 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 small positive impacts of the UK National Minimum Wage on employment. We conclude that the existing data, combined with difference-in-difference designs, in fact offered very little guidance to policy makers.

JEL Classification: C12, C18, J23, J38

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

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1 Introduction

The conduct of minimum wage policy in the UK is unusual for its very formal connection to an evidence base. A body called the Low Pay Commission (LPC) was established in 1999 to advise the UK government on the setting of the National Minimum Wage (NMW). Each year the LPC commissions and funds research on the impacts of the minimum wage, and then uses evidence from those studies to help determine its recommendations to the government. Those recommendations have almost always been adopted. A broad conclusion from this body of research has been that the introduction of the NMW and its subsequent up-ratings did not have detrimental effects on employment (see, for example, Stewart, 2004a,b; Dickens and Draca, 2005; Dickens et al., 2012; Bryan et al., 2013). This body of work was also cited when in 2016 the UK Government introduced the National Living Wage, which effectively raised the minimum wage for workers aged 25 or older by 7.5%, and set a target of having this new rate reach 60% of median earnings by 2020.

The NMW is widely viewed as an exemplary case of evidence-based policy making. For example, in their popular book on "The Blunders of Our Governments", political scientists Ivor Crewe and Anthony King cite the NMW as a counter-example of good policy-making. They note that "although introducing it had been controversial to begin with, there now exists an almost total consensus in its favour" (Crewe and King, 2014).

But there are reasons to question whether the evidence base on impact of the UK NMW on employment is as strong as its influence suggests. First, much of that literature is based on difference-in-difference (DiD) designs (see, for example: Stewart, 2004a,b; Dickens and Draca, 2005; Dickens et al., 2012; Bryan et al., 2013; Dickens et al., 2015). 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 such designs may have very low power when inference is conducted properly (Brewer et al., 2018). Second, the literature on the NMW has focused on the binary outcome of the statistical rejection (or otherwise) of the null hypothesis - that the NMW has no impact on employment rates - without attention to the range of the impacts on employment that are consistent with the data. Instead, what policy-makers ought to be asking is "by how much will employment change if we increase the NMW?" Commentators in both the social and medical sciences (such as Cohen (1994), Sterne et al. (2001), Ziliak and McCloskey (2004), Ioannidis (2005), Spiegelhalter (2017) and McShane et al. (2017)) 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. for example, Spiegelhalter (2017) argues that "we should stop thinking in terms of significant or insignificant as determining a 'discovery', and instead focus on effect sizes." One of the recommendations of the American

Statistical Association's recent statement on p-values is that researchers present confidence intervals, as these summarise what values of the parameters of interest would be rejected (in a statistical sense) by the data (Wasserstein and Lazar, 2016). In economics, Ziliak and McCloskey (2004) emphasize the importance of assessing estimated coefficients in terms of their economic or substantive, rather than statistical, significance.

In this paper we re-evaluate the the impact of the NMW on employment taking full account of these concerns. We use the two most common empirical approaches in the literature. The first estimates the impact of the NMW on transitions from employment using a DiD-style design. Examples of studies using this approach are Stewart (2004a,b), Dickens and Draca (2005), Dickens et al. (2012), Bryan et al. (2013) and Dickens et al. (2015). These studies typically estimate the impact that an up-rating of the NMW has on the transition rate from employment into non-employment; this is identified 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. The second approach exploits geographical variation in the "bite" of the NMW that arises because the NMW take a single value across the UK, while wage levels vary. Examples of this approach are Stewart (2002), Dolton et al. (2012) and Dolton et al. (2015). In these studies, the employment rate in a region is related to the bite of the minimum wage within that region; identification comes from variation in the bite over time.

We develop previous findings in three 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 "there is 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. As a way of communicating the width of confidence intervals, we calculate minimum detectable effects (MDEs), following Bloom (1995), which indicate how large the true impacts on employment would have to be (or how large would the true elasticity of labour demand with respect to the minimum wage have to be) for the methods employed in this literature to detect them with high probability. Third, we show what the estimated coefficients mean for economically-meaningful concepts, such as elasticities of employment with respect to the minimum wage.

The existing literature has consistently failed to reject the null hypotheses that the UK's NMW wage has had no impact on employment or job retention. This study is no different (so, like the past literature, we fail to find consistently evidence of a "statistically significant effect"). However, we show that the data cannot exclude that the NMW has large negative (and also small positive) effects on employment rates. For example, in with our preferred specification for implementing the first empirical approach, 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, we obtain a 95% confidence interval that includes the possibility that a 10% rise in the NMW would lower the job retention rate for NMW workers by as much as 22%. Considered another way, our calculations of MDEs indicate that the tests and data employed in this strand of the literature would have an 80% chance of detecting a non-zero impact of the NMW only if the true effect of a 10% increase in the NMW was to reduce the job retention rate of NMW workers by no less than 16%. We also highlight that this commonly-used specification is not informative about the underlying elasticity of employment with respect to the minimum wage, because it relates employment rates to changes in, not the level of, the NMW. The second empirical strategy, exploiting geographic variation, does provide an estimate of the underlying elasticity of employment with respect to the minimum wage. Having such an estimate is important, not least because it allows for comparison with the international literature. We find that this empirical approach has greater power, but still admits a large range of possible effects.

The rest of the paper proceeds as follows. Section 2 outlines the most important features of the UK NMW, and of past literature related to it, with an emphasis on why this research has been so influential. Section 3 describes the empirical strategies that we use to revise the evidence on the 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. A year before the introduction of the NMW, the coverage of the adult rate was around 5.6% and the youth rate was estimated to affect 8.2% workers aged 18-21.[2] After a month from the introduction of the NMW, the vast majority of employers were meeting their obligation to pay

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

[2] Coverage of the NMW is defined as the proportion of employees in working age paid below the NMW in April preceding the NMW review. Coverage estimates are calculated using Annual Survey of Hours and Earnings (ASHE) data. ASHE is conducted in April of each year.

the proper hourly rate (LPC, 2000).

The level of the NMW is reviewed annually (and we describe this process in more detail below). Table 1 shows the history of the NMW up-ratings. 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 grew 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. Coverage of the adult NMW rate has ranged from between 4% and 6% of working-age employees over most of this period, with a slight upwards trend.

In April 2016 the government applied a new rate to workers aged 25 and above, increasing the minimum hourly wage by 7.5%. With this change, the NMW for those aged 25 and above was relabelled the National Living Wage (NLW) and the Government announced a target of the NLW reaching 60% of median earnings by 2020 (LPC, 2016). These changes are much greater than those taken in most of the 17 years before 2016, and the decision to introduce the NLW was taken partly to offset cuts to the Working Tax Credit, but also was taken on the basis of the evidence that the NLW would not harm employment (Office for Budget Responsibility, 2015).[3]

2.2 Research on the impact of the National Minimum Wage

A large number of studies 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. As noted above, two main approaches have been employed in this research. The first approach uses individual panel data and estimates the effect of a change in the NMW on job retention.[4] The second strategy relies on the fact that the extent to which the NMW binds at the bottom of the wage distribution varies geographically, and it exploits this variation to estimate the effect of the minimum wage on the employment rate.

The studies that examine the impact of the NMW on job retention use a Difference-in-Differences (DiD) design, typically comparing workers whose wage is increased to comply with the NMW (or with an annual increase in the NMW) with an unaffected group of workers with higher wages. 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

[3] In his Summer Budget 2015 speech, Chancellor George Osborne stated "The Office for Budget Responsibility (OBR) today say that the new National Living Wage will have, in their words, only a "fractional" effect on jobs. The OBR have assessed the economic conditions of the country, and all the policies in the Budget. They say that by 2020 there will be 60,000 fewer jobs as a result of the National Living Wage but almost 1 million more in total."

[4] Job retention is defined as one minus the probability of making a transition out of employment

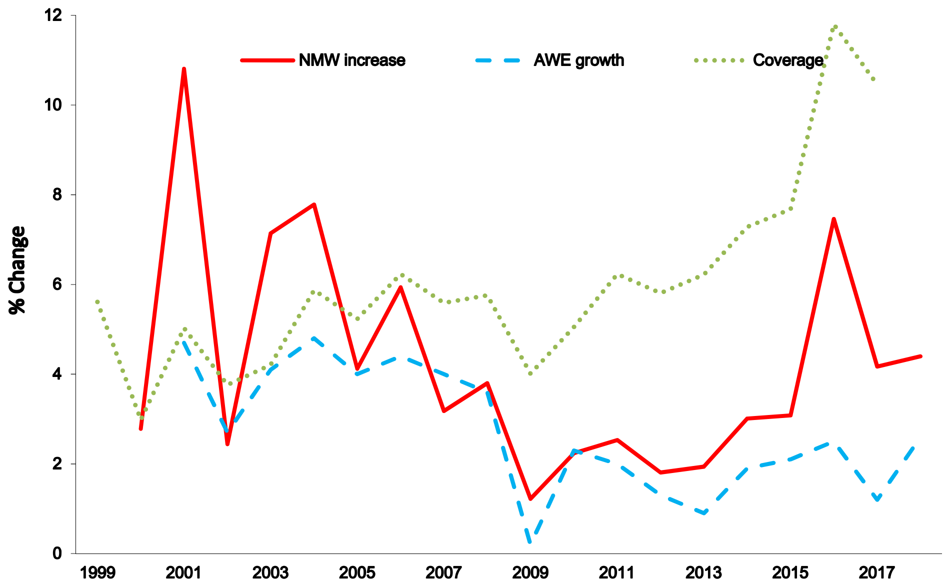
Table 1: The UK National minimum wage for adults

Date	**Adult rate**	**NMW % change**	**Coverage**	**AWE growth**	**Inflation**
Apr 1999	£3.60	-	5.61	-	-
Oct 2000	£3.70	2.78	2.98	-	1.2
Oct 2001	£4.10	10.81	5.03	4.7	1.1
Oct 2002	£4.20	2.44	3.76	2.7	1.4
Oct 2003	£4.50	7.14	4.21	4.1	1.5
Oct 2004	£4.85	7.78	5.87	4.8	1.2
Oct 2005	£5.05	4.12	5.23	4.0	2.3
Oct 2006	£5.35	5.94	6.23	4.4	2.4
Oct 2007	£5.52	3.18	5.59	4.0	2.1
Oct 2008	£5.73	3.80	5.76	3.6	4.4
Oct 2009	£5.80	1.22	4.01	0.2	1.5
Oct 2010	£5.93	2.24	5.05	2.3	3.2
Oct 2011	£6.08	2.53	6.23	2.0	5.0
Oct 2012	£6.19	1.81	5.81	1.3	2.6
Oct 2013	£6.31	1.94	6.22	0.9	2.2
Oct 2014	£6.50	3.01	7.28	1.9	1.3
Oct 2015	£6.70	3.08	7.68	2.1	-0.1
Apr 2016	£7.20	7.46	11.80	2.5	-0.1
Apr 2017	£7.50	4.17	10.46	1.2	2.7
Apr 2018	£7.83	4.40	-	2.6	2.4

The adult rate refers to workers aged 22 and over until 2009, aged 21 and over until 2015 and aged 25 and over afterwards. In April 2016 the National Living Wage replaced the National Minimum Wage for workers aged 25 and over. NMW change, Coverage, AWE growth and Inflation are expressed as percentages. Earnings in ASHE are recorded in April every year. Coverage in 1999, 2016, 2017 is relative to earnings recorded in April of the preceding year.

Sources: Low Pay Commission, Annual Survey of Hours and Earnings (ASHE), CPI series D7BT (ONS), Average Weekly Earnings (ONS) series KAB9.

Figure 1: Annual growth rates of the UK National Minimum Wage, 2000 to 2015.



Sources: Low Pay Commission, Annual Survey of Hours and Earnings (ASHE), Average Weekly Earnings (ONS) series KAB9.

look at employment rates amongst groups of individuals highly likely to be affected by the minimum wage were they to work, such as young low-skilled adults.[5] 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) examine the employment effects of the 2003 uprating of the

[5]Machin et al. (2003) 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.

NMW. 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 impact of the NMW on employment 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) provide 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 (at the time) October increases in 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.

Finally, Dickens et al. (2015) use a DiD design that compares job retention rates of workers affected by the minimum wage and job retention rates of a group of workers with a pay that is slightly higher the forthcoming NMW in a period when the NMW was enacted (1999-2010) and in a base period that precedes the minimum wage introduction (1994-1997). Findings suggest that the NMW reduces job retention and in particular harms part-time women whose job retention decreases by 3 percentage points.

The second of the two main approaches focuses on the impact of the NMW on employment rate and exploits the fact that the minimum wage is set at the national level, but the wage distribution varies across local areas. This strategy recognises that the impact of the NMW on employment will be larger in areas where the NMW has more of an impact on the wage distribution compared to areas where relatively few workers are affected by the minimum wage. The first example of this type of study using UK data is Stewart (2002). Stewart reformulates the model proposed in Card (1992) and estimates a reduced form equation that derives from a structural model of the labour demand. Local area employment changes are explained by the wage variation due to the different impact of the introduction of the NMW across local areas. Stewart (2002) uses the share of workers paid below the minimum wage rate in a local area as an instrument for the endogenous wage variation. He reports positive and negative employment elasticities but none of them is statistically significant at conventional levels. In a second specification, Stewart (2002) uses an indicator for areas with the highest share of workers paid below the NMW as alternative instrument for the endogenous wage variation, but finds that this does not lead to different conclusions.

Dolton et al. (2012) examine the effect of the minimum wage on employment by, in effect, relating the employment rate in a local area to the Kaitz Index in that area - this is a measure of the bite of the minimum wage, or to what extent it acts as a binding wage floor. The Kaitz Index is also interacted with an indicator for each year's uprating, allowing for an effect of the NMW on employment that can change over time. Interestingly, Dolton et al. (2012) find positive and statistically significant effects of the NMW on employment between 2004 and 2006. Dickens et al. (2012) also apply an incremental Difference-in Differences design but they use the share of workers paid below the minimum wage rate rather than the Kaitz Index. Their overall conclusion is that the NMW did not have a statistically significant impact on employment.

Dolton et al. (2015) is the most recent study of the second approach and revisits the incremental Difference-in Differences in Dolton et al. (2012) adding some important contributions. They first present the same static model as in Dolton et al. (2012) but they control for regional shocks to aggregate demand and spatial dependence in the error terms to account for common shocks that affect contiguous areas. Then they implement a dynamic model that includes a lag of the employment rate in the regressors, applying a system GMM IV estimator to deal with the possible endogeneity of the Kaitz Index. Dolton et al. (2015) stress that the incremental Difference-in Differences design disentangles the (negative) underlying effect of the minimum wage and the (positive) effect of the annual upratings. They claim that this divergent effect explain why the literature has found a detrimental effect of the introduction of the minimum wage but also some null or positive effects when a longer period is brought in the analysis.

2.3 The influence of research on the National Minimum Wage

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, that 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, including a recommendation on by how much the NMW should increase, and the government makes its decision shortly after, with the new NMW rate applying from the following October or April. 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, 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, 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, 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" (LPC, 2009).

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 a null hypothesis (Sterne et al., 2001). As Cohen (1994) observes, what policy-makers want to know is how likely is it, given the available data, that policy does not have an adverse effect (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. In the case of the NMW, we might want to know not whether we can reject the null of "the NMW has no effect on employment", but whether, with the available data, we can reject the null of "the NMW has negative, economically-meaningful impacts on employment".

3 Implementation

We now describe the particulars of how we revisit the two main approaches in this literature. Because we are effectively replicating previous work, full details of our sample and data work can be found in the original studies.

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

We proceed by describing the model set out in Bryan et al. (2013), which we choose as a recent example of the method; much of what we say below applies to the other examples cited earlier. This estimates the impact of changes in the NMW on employment transitions with a multi-group, multi-period, difference-in-differences (DiD) design.

The model is estimated on 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, the outcome measure is an individual's transition from employment over a 6 month period using the first and the third observation for each individual. These 6-month intervals either do or do not straddle a NMW increase on 1 October; this is denoted with s. The maintained assumption 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 1 October are not affected by any NMW uprating.[6] Individuals are allocated into one of four different groups, g, according to the starting wage: 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 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 up-rating. [7]

The equation that is estimated is:

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

where y_{igts} is a dummy variable that records whether individuals in work at time t are also in work 6 months later, g subscripts the 4 groups defined according to the individual's initial wage, s denotes whether the 6-month intervals straddle a NMW increase on 1 October, 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 impact on job retention for the treatment group of the year t NMW up-rating effect.

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. This alternative model is:

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

[6] As NMW increases happened always on 1 October in the period we consider, 6-month 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.

[7] There is also 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 ($w_{it} \geq m(NMW_{t+1})$). The 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.

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

Our sample selection and choice of covariates follows Bryan et al. (2013), and we are able to replicate their estimation sample and point estimates, but we estimate different standard errors, following concerns raised in the literature about the accuracy of the inference in DiD designs when using the naïve estimates of the standard errors provided by OLS. The first concern, dating back to Moulton (1990), relates to the grouped error structure. In DiD designs, the error term ϵ_{igts} in Equation 1 is unlikely to be i.i.d., 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. As far as we have been able to work out, 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. A second concern, 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. We describe our approach in further details in Appendix A.

An approach that is commonly used to calculate standard errors that account for the common group structure in the error term is the Donald and Lang (2007) two-step estimator. But, when taking this approach, equations 1 and 2 are exactly identified: in other words, just as standard errors cannot be estimated in the standard 2x2 DiD if there are common group-level shocks, so they cannot be estimated for equations 1 and 2. To proceed, we constrain the impact of the NMW on job retention, β_T, to be time-invariant (see Appendix A for more details). The constrained version of the model that estimates the impact of a NMW uprating on job retention is, then:

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs} + \mathbf{x}_{igts}^{'}\gamma + \epsilon_{igts} \tag{3}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

and the amended model that estimates the impact of a 1% rise in the NMW on job retention is:

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs}\omega_t + \mathbf{x}_{igst}^{'}\gamma + \epsilon_{igst} \tag{4}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

To facilitate comparisons between our estimates and other studies, we translate the estimated coefficients β_T (which estimate the impact of a rise in the NMW on 6-month retention rates) into an estimate of the elasticity of the 6-month job retention rate to the NMW. This elasticity, η_{JR}, is defined as:

$$\eta_{JR} = (\Delta RR/RR) \, / \, (\Delta NMW/NMW) \tag{5}$$

where 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)[8], and $\Delta NMW/NMW$ is 0.049, the average size of the NMW upratings in the 2000-2010 period. [9]

A considerable drawback of the specifications in equations 1-4, though, is that one cannot infer from them what is the underlying relationship between the level of the NMW and the level of the employment rate (or even the retention rate). To calculate an elasticity of employment with respect to the wage or to the minimum wage, one needs to know the shape of the function that relates the level of employment to the level of minimum wages. Instead, equations 2 and 3 relate the *(*level) of employment to the *(*rate of change) of the minimum wage. [10] Another way of seeing this is to consider a sequence of 4 annual changes to the minimum wage of 0%, +25%, +25% and 0%. If the predicted retention rate in the second year, when $\omega_t = 0$ is E^*, then the predicted retention rates in the following 3 years would be E^*, $E^* - 0.25\beta_T$, $E^* - 0.25\beta_T$, E^*. In other words, the equation would predict that the retention rate would fall while the minimum wage was rising, but then would return to its original level even though the minimum wage was over 56% higher.

3.2 Estimating the impact of the NMW on employment using geographical variation in its bite

Here, we present the model in Dolton et al. (2012) and Dolton et al. (2015), which relate employment levels in an area to the Kaitz index, K_{jrt}. The model is:

$$E_{jrt} = \theta_0 + \gamma_j + \lambda_{1r} t + \lambda_{2r} t^2 + \theta_1 Post_t + \theta_2 K_{jrt} + \theta_3 Post_t * K_{jrt} + \mathbf{x}'_{jrt}\delta + \epsilon_{jrt} \tag{6}$$
$$t = 1997, ..., 2010; j = 1, ..., 140; r = 1, ..., 11$$

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

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

[10] It would be possible to write down a version of 2 that included NMW_t as an additional regressor, but it would not be possible to identify the coefficient on such a variable, as it would be collinear with the time effects.

where E_{jrt} is the log of the employment rate in local area j in region r at time t, γ_j is an area fixed effect, $Post_t$ is a dummy that indicates whether the NMW is in place at time t and x_{jrt} are a set of time-varying area characteristics. The Kaitz index K_{jrt}, the ratio of the NMW to the median wage of the local area j, is used to measure the bite of the minimum wage in local area j, with higher numbers indicating that the NMW has a stronger bite.[11] We construct the same data-set as Dolton et al. (2015), using the LFS to calculate the employment rate at local area level, and the Annual Survey of Hours and Earnings (ASHE) to calculate the Kaitz index. To overcome any spurious correlation between the business cycle and the NMW, the model also controls for regional aggregate demand and a quadratic time trend at the regional level. [12] We allow for spatial correlation in error terms between local areas within the same region (allowing for, say, common economic shocks that influence contiguous areas) and allow for serial correlation within error terms of a given local area, and the error ϵ_{jrt} is specified to have three components:

$$\epsilon_{jrt} = \pi_r + \rho\epsilon_{jrt-1} + \nu_{jrt}$$
$$t = 1997, ..., 2010; j = 1, ..., 140; r = 1, ..., 11$$

where π_r denotes a within-region common economic shock, ν_{jrt} is an idiosyncratic term, and serial correlation is modelled with an AR(1) process.

The NMW and the Kaitz index could be endogenous to employment levels, or we could have reverse causation from employment to the Kaitz index, if the UK government set the NMW according to past shocks of employment or any omitted variable correlated with employment. The dynamic model attempts to overcome this issue controlling for the lag of the local area employment rate, E_{jrt-1}. This gives a dynamic model:

$$E_{jrt} = \theta_0 + \gamma_j + \lambda_{1r}t + \lambda_{2r}t^2 + \theta_1 Post_t + \theta_2 K_{jrt} + \theta_3 Post_t * K_{jrt} + \theta_4 E_{jrt-1} + \mathbf{x}'_{jrt}\delta + \epsilon_{jrt} \tag{7}$$

$$t = 1997, ..., 2010; j = 1, ..., 140; r = 1, ..., 11$$

Model 6 and 7 are estimated with feasible GLS, as suggested in Brewer et al. (2018). We first estimate the models with OLS. We then use the residuals to estimate the parameter of the AR(1) process. Finally we apply GLS, clustering the error terms at regional level and using the autoregressive parameter to account for serial correlation.

[11] We use the 140 unitary authorities and counties as our local areas. To calculate the Kaitz index in the years before the NMW was introduced, we use the 1999 value of the NMW, adjusted by growth in average earnings.

[12] We identify 11 regions: North East, North West, Yorkshire and The Humber, East Midlands, West Midlands, East of England, South East, London, South West, Wales, Scotland. Northern Ireland is excluded from the analysis because ASHE does not provide data for this country.

In the models that exploit geographical variation in the NMW bite, deriving elasticities is straightforward: because equation 6 relates log-employment to the Kaitz index K_{jrt}, the resulting employment elasticity with respect to the minimum wage at time t is (see Appendix B.1):

$$\eta_{ER} = (\theta_2 + \theta_3)K \tag{8}$$

where K is the average Kaitz Index.

In the dynamic model, the employment elasticity is (see Appendix B.2):

$$\eta_{ER} = \frac{\theta_2 + \theta_3}{(1 - \theta_4)}K \tag{9}$$

The employment elasticity in the dynamic model differs from the static model by a scale factor $1 - \theta_4$, a measure of how much past employment affects current employment.

3.3 Minimum detectable effects

Following Bloom (1995) and Brewer et al. (2018), we use the concept of Minimum Detectable Effects (MDEs) as a way of illustrating the power of the research designs that have studied the impact of the NMW on employment. The MDE combines the concepts of the significance level α and desired power π with the standard error of the parameter of interest β; it is the smallest true effect that would lead a test with size α to reject the null hypothesis of no treatment with probability κ. 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.

The MDE is defined as:

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

where $\hat{se}(\beta)$ is the estimated standard error for the coefficient β, $t_{\alpha/2}$ is the critical value of the $(\alpha/2)$-th percentile of the t_{C-1} distribution and $t_{1-\kappa}$ is the $(1-\kappa)$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.[13] Of course, the width of the confidence interval is:

$$2 \times \hat{se}(\beta)[t_{\alpha/2}]$$

so that the MDE is $\frac{t_{\alpha/2}+t_{1-\kappa}}{2t_{\alpha/2}}$ times the width of the confidence interval. The MDE therefore is

[13] For the Bryan et al. (2013) method, our implementation of this notes that C is the number of cells in our second stage (i.e. 80), and a standard value for κ in the literature is 0.8, so $t_{1-\kappa}$ turns into the 20th percentile of a t-distribution with 79 degrees of freedom.

another way to think about and communicate the width of the confidence interval.

4 Results

4.1 The estimated impact of the NMW on job retention

Table 2 shows estimates of the impact of an NMW uprating on the probability of remaining employed. We report estimates of β_T from Equation 3 for men in the top panel and for women in the bottom panel. In the first line we conduct inference using the Donald and Lang (2007) two-step estimator; in the other lines we present estimates of β_T that come from estimating Equation 3 with OLS and calculating heteroscedasticity- and cluster-robust standard errors and standard errors with no correction.[14]

For men, OLS point estimates on the micro-data suggest that a NMW uprating increases the probability of remaining employed, while for women the OLS point estimate is a small decrease. When the Donald and Lang (2007) two-step estimator is implemented the estimated impact of an NMW uprating is to increase the job retention rate by 0.4 percentage points for men and cut it by 0.1 percentage points for women.[15]

None of the estimates is statistically different from zero: like the previous UK literature, we fail to find a statistically significant 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 more than twice as large as the OLS standard errors for women, and 88% larger for men: this is consistent with our belief that within-cell correlation in the error terms is an important issue. 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 4.7 percentage points for men, or that it increases the job retention rate by 5.6 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 this DiD design has a low power to detect a

[14] The point estimates in Table 2 do not correspond to the results presented in Bryan et al. (2013). As discussed in Appendix A, 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 8 in Appendix C.

[15] 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 3, and when using OLS on cell-level averages in equation 12 (see Appendix A).

plausibly-sized true impact of the NMW on job retention. Our calculations of the MDEs show that an NMW uprating would need to change the job retention rate for men by 7.3 percentage points to have an 80% chance of being detected, and by 5.0 percentage points for women.

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

Method	**Std. Errors**	β_T	$s(\beta_T)$	**C.I. at 95 %**		**MDE**
Men						
Two Step	-	0.004	0.026	(-0.047	0.056)	±0.073
OLS	Cluster Robust	0.013	0.019	(-0.025	0.051)	±0.054
OLS	Het. Robust	0.013	0.017	(-0.021	0.046)	±0.049
OLS	No Correction	0.013	0.014	(-0.014	0.040)	±0.039
Women						
Two Step	-	-0.001	0.018	(-0.036	0.034)	±0.050
OLS	Cluster Robust	-0.002	0.009	(-0.019	0.015)	±0.024
OLS	Het. Robust	-0.002	0.008	(-0.018	0.014)	±0.023
OLS	No Correction	-0.002	0.007	(-0.016	0.013)	±0.021

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 (14); OLS estimates are from equation (3)

The specification in Equation 3 that estimates the average impact of a NMW up-rating does not take into account that the size of the up-ratings 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, based on the model in Equation 2, and using OLS and the two-step estimator. The point estimates imply 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 10 percentage points for men, and by 1 percentage point for women. Since the average up-rating size over 2000-2011 is 4.9%, the annual review of the NMW reduces on average job retention by 4.9 percentage points for men and by 0.4 percentage points for women.

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 87% larger than the OLS standard errors for men, and 127% larger for women. However, the point estimate for men is large enough to be statistically significant at the 5% significance level. Of course, 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 20 percentage points, or be close to no effect, in response to a 10% rise in the NMW. The estimated MDEs in column (5) indicate that, to have an 80% probability of detecting it, one would need

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

Method	Std. Errors	β_T	$s(\beta_T)$	C.I. at 95 %		MDE
Men						
Two Step	-	-0.010	0.005	(-0.020	-0.000)	$\pm$0.014
OLS	Cluster Robust	-0.010	0.004	(-0.017	-0.002)	$\pm$0.010
OLS	Het. Robust	-0.010	0.003	(-0.016	-0.003)	$\pm$0.010
OLS	No Correction	-0.010	0.003	(-0.015	-0.005)	$\pm$0.007
Women						
Two Step	-	-0.001	0.003	(-0.007	0.006)	$\pm$0.009
OLS	Cluster Robust	-0.001	0.002	(-0.005	0.002)	$\pm$0.005
OLS	Het. Robust	-0.001	0.002	(-0.004	0.002)	$\pm$0.004
OLS	No Correction	-0.001	0.001	(-0.004	0.002)	$\pm$0.004

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 (15); OLS estimates are from equation (2) and are associated to heteroscedasticity-robust standard errors. NMW in 2000 prices.

a true effect on the probability of remaining employed of about 14 percentage points for men, and 9 percentage points for women, in response to a 10% rise in the NMW.

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 that can be compared to those from other studies. The conventional way of summarising effects of wages on employment is through an elasticity, and so in Tables 4, we report the elasticities of job retention to the NMW that are implied by our estimated coefficients in Table 2 and 3, using the formula in equation 5. The elasticities implied by the point estimates for β_T are in the range of 0.11 to -1.15 for men, and -0.02 to -0.09 for women, depending on the specification. The confidence intervals for these elasticities include (substantively) 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 MDEs in Table 4, 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 1.6 for men, and greater than 1 for women.

For comparison, Table 5 reports the range of job retention elasticities observed in the minimum wage literature that uses individual longitudinal data from the US and Canada. Unlike some 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 4 implies a job retention elasticity of -1.15 for adult men, close to Yuen (2003), and larger than what Currie and Fallick (1996),

Table 4: Implied elasticities of job retention with respect to the minimum wage, η_{JR}

Method	Specification	η_{JR}	C.I. at 95 %		MDE
Men					
Two Step	Average Uprating	0.11	(-1.19	1.41)	±1.88
Two Step	1% rise	-1.15	(-2.25	-0.05)	±1.60
Women					
Two Step	Average Uprating	-0.02	(-0.86	0.83)	±1.23
Two Step	1% rise	-0.09	(-0.78	0.60)	±1.00

Elasticities calculated using equation (5). Standard Errors calculated using the delta method.

Neumark et al. (2004), Campolieti et al. (2005) and Sabia et al. (2012) obtain for teenagers and youths (groups that may be more sensitive to minimum wage increases). We conclude, therefore, that the common DiD design used to examine the employment effects of the NMW, when presented as an elasticity, is of similar magnitude to estimates from a North American literature that *does* conclude that minimum wages have adverse job retention effects. This certainly suggests that one should be cautious in drawing the conclusion that the NMW has no detrimental effect on employment.

Table 5: 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)	USA	Workers affected by minimum wage	-0.19 to -0.24
Yuen (2003)	Canada	Teenagers	-0.75 to -0.84
		Young Adults	-1.23 to -1.77
Neumark and Wascher (2004)	USA	Teenagers	-0.12 to -0.17
Campolieti et al. (2005)	Canada	Young Adults	-0.33 to -0.54
Sabia et al. (2012)	USA	Young Adults	-0.7

4.2 The estimated impact of the NMW on employment

Turning now to the second main approach in the literature, in Table 6 we report estimates from our models that exploit geographical variation of the bite of the NMW. For the sake of brevity, we directly report them as them employment elasticities, which are easier to assess and compare to other studies. Standard errors are calculated with the delta method (see Appendix B.1-B.2).

Within each panel of Table 6, in the first line we do not apply any correction to the standard errors. Then in the second line we cluster standard errors at regional level and in the third line we follow Brewer et al. (2018) and account for both cluster and serial correlation in the error term using a combination of feasible generalized least squares (assuming an AR(1) process) and cluster-robust standard errors. In the first panel of Table 6 we report estimated employment elasticities derived from the static model in Equation model 6. These are for our full sample. The second panel of Table 6 reports the employment elasticities derived from the dynamic model in Equation 7.

Table 6: Employment elasticity $\bar{\eta}_{ER(t)}$ with respect to the the year t minimum wage uprating.

Model	**Std. Errors**	$\bar{\eta}_{ER(t)}$	$s_{\bar{\eta}_{ER(t)}}$	**C.I. at 95 %**		**MDE**
All sample						
Static Model (Equation 6)	No Correction	-0.011	0.034	(-0.087	0.045)	±0.096
	Cluster Robust	-0.011	0.027	(-0.074	0.032)	±0.076
	FGLS with AR(1) process	-0.005	0.030	(-0.069	0.049)	±0.085
Dynamic Model (Equation 7)	No Correction	-0.005	0.038	(-0.083	0.065)	±0.107
	Cluster Robust	-0.005	0.026	(-0.061	0.043)	±0.075
	FGLS with AR(1) process	-0.006	0.028	(-0.067	0.043)	±0.079
Low Educated						
Static Model (Equation 6)	No Correction	-0.017	0.050	(-0.131	0.066)	±0.142
	Cluster Robust	-0.017	0.032	(-0.095	0.030)	±0.091
	FGLS with AR(1) process	-0.012	0.032	(-0.085	0.041)	±0.091
Dynamic Model (Equation 7)	No Correction	-0.013	0.054	(-0.130	0.081)	±0.153
	Cluster Robust	-0.013	0.032	(-0.087	0.038)	±0.090
	FGLS with AR(1) process	-0.013	0.031	(-0.085	0.035)	±0.087

FGLS with AR(1) process follows the method presented in Brewer et al. (2018) for small number of clusters.

In the first panel of Table 6, the point estimates imply that a 10% increase in the NMW would reduce the employment rate by between 0.05% and 0.1%. But our preferred confidence interval (reported in the third line) implies that we could not statistically reject that the economy-wide employment rate declines by 0.7% or increases by 0.5% in response to a 10% rise in the NMW. The MDEs for the employment elasticities reflect the relatively large standard errors. The minimum size of the employment elasticity we would have 80% power to detect is around ±0.09 (i.e. a 0.9% change in employment in response to a 10% increase in the NMW).

In the second panel of Table 6, point estimates from the dynamic model are slightly smaller than those from the static model, but there is some loss of precision, and the confidence intervals and MDEs end up being fairly similar.[16]

[16] Interestingly, in Table 6 the uncorrected (OLS) standard errors are larger than the cluster-robust standard errors. Angrist and Pischke (2008) note that this can happen sometimes, because of the high variability of the cluster-robust variance estimator itself; they recommend taking the larger of the OLS and cluster-robust standard errors as a conservative approach.

Table 7: Estimated employment elasticities with respect to the minimum wage, η_{ER}, from studies using US data

Study	Group	η_{JR}
Brown et al. (1983)	Teenagers	-0.23 to -0.02
Card (1992)	Teenagers	-0.06 to 0.19
Neumark and Wascher (1992)	Teenagers	-0.2 to -0.1
	Young Adults (15-24)	-0.2 to -0.15
Neumark and Wascher (2004)	Teenagers	-0.24 to -0.18
	Young Adults (15-24)	-0.16 to -0.13
Dube et al. (2010)	Low Wage Sector Employees	-0.21 to 0.06
Allegretto et al. (2011)	Teenagers	-0.12 to 0.05
Neumark et al. (2014)	Restaurant Employees and Teenagers	-0.15 & -0.05
Meer and West (2015)	Working Age (15-59)	-0.19 to 0.01
Bazen and Marimoutou (2016)	Teenagers	-0.43 to -0.13

In Table 7 we report the range of the employment elasticities of the minimum wage that come from US studies. These estimates are more negative than our point estimates, but they lie well within the confidence intervals reported in Table 6. It is important to note, though, that the literature in the US has focused on the employment rate of groups likely to be at the bottom of the earning distribution and hence most impacted by minimum wage increases. Interestingly, one UK study of a low-wage sector - the residential care homes industry - finds elasticities in the range of -0.15 to -0.40 (Machin et al., 2003), although a later paper finds less robust evidence (Machin and Wilson, 2004). Our design follows Dolton et al. (2012) and Dolton et al. (2015), which both using the employment rate for the working age population. To bridge the gap with the US literature, we re-estimated the models in equations 6 and 7 on the subsample with low levels of education, so as to better capture employment in low wage sectors and jobs. The results are reported in the third and the fourth panels of Table 6. Employment elasticities are indeed larger (that is, more negative) for this group, with point estimates lying between -0.012 to -0.017. However, the confidence intervals and MDEs are still substantively wide or large, reflecting again the large standard errors. For example, to have an 80 % probability of being detected, a 10% increase of the NMW would need to decrease (increase) employment of the low-skilled by 0.9% to 1.6%.

5 Discussion and conclusions

The UK is unusual for the fact that economic research on the impact of the NMW on employment has played a decisive role in the setting of the minimum wage. 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": we fear that policy-makers have wrongly interpreted p-values as telling us how likely it is that the NMW has an adverse effect on employment, and have not paid attention to the range of impacts on employment that also cannot be rejected by the data. This concern is compounded by the fact that much of 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 two most common approaches taken in UK research on the impact of the NMW on employment. Our study first follows Bryan et al. (2013), one of the most comprehensive reports on the impact of NMW increases on employment; as in the UK literature, we also cannot reject the null that NMW up-ratings had no impact on job retention rates. 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 16% 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 with respect to the minimum wage of about -1.2. In the second part of our analysis we follow Dolton et al. (2015)'s approach that exploits geographical variation in the impact of the NMW and calculate employment elasticities. In the preferred dynamic specification, the employment elasticity is -0.006. However, we again find relatively large minimum detectable effects that point to the low power of the design. Indeed, to be detected with an 80% probability, there would need to be a change in employment of at least 0.8% in response to a 10% increase in the NMW.

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. Better evidence may come from the large recent change occurred with the introduction of the National Living Wage, but we also recommend a reconsideration of the use of a DiD designs with existing UK data sources when evaluating the impact of the NMW on employment. More importantly, in this, and other areas of policy-relevant work, researchers should present - and policy makers should demand -

clearer information on what range of effects the data can and cannot rule out. We hope this is something that is considered by the Treasury-commissioned review of the evidence on the employment effects of minimum wages announced in the 2019 Spring Statement. [17]

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[17] See `https://tinyurl.com/yy3bgzm2`

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A Appendix A: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:

$$y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt} d_{gs} \omega_t + \mathbf{x}'_{igts}\gamma + \varphi_{gts} + \xi_{igts} \tag{10}$$
$$i = 1, ..., N; g = C, B, T, A; s = 0, 1; t = 2000, ..., 2011$$

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. (2018)). 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 be 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 96 cells (4 groups, 12 years of data, and 2 transition types). The first stage regression is then:

$$y_{ic} = \mathbf{x}_{ic}^{'}\gamma + \sum_{c=1}^{96} I_c \mu_c + \epsilon_{ic} \tag{11}$$
$$i = 1, ..., N; c = 1, ..., 96$$

where I_c is a dummy variable which identifies the c-th cell, and $\mathbf{x}$ are the controls that vary within-cell.[18]

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:

$$\hat{\mu}_c = \delta_{ts} + \alpha_{gt} + \beta_{gt} d_c + \epsilon_c \tag{12}$$
$$c = 1, ..., 96; t = 2000, ..., 2011$$

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 10. The argument runs as

[18] All the control variables $\mathbf{x}$ in the equation 10 vary within-cell.

follows: in equation 10, 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 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 10, 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 12 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 10 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:

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs} + \mathbf{x}_{igts}^{'}\gamma + \epsilon_{igts} \tag{13}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

and the second stage is:

$$\hat{\mu}_c = \delta_{ts} + \alpha_g + \beta_g d_c + \epsilon_c \tag{14}$$
$$c = 1, ..., 80; t = 2000, ..., 2011$$

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:

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

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

$$\hat{\mu}_c = \delta_{ts} + \alpha_g + \beta_g d_c \omega_t + \epsilon_c \tag{15}$$
$$c = 1, ..., 80; t = 2000, ..., 2011$$

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 9 (Appendix C), which displays the first order auto-regressive coefficients from our estimates of equation (2). 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 12 years, within each of which we observe two 6-month transitions that span an uprating, and two 6-month transitions that do not).

B Appendix B: Derivation of formulas for the elasticity of employment with respect to the minimum wage

B.1 Static Model

The static model in Dolton et al. (2015) is

$$\log(E_{jrt}) = \theta_0 + \gamma_j + \lambda_{1r}t + \lambda_{2r}t^2 + \theta_1 Post_t + \theta_2 K_{jrt} + \theta_3 Post_t * K_{jrt} + \mathbf{x}'_{jrt}\delta + \epsilon_{jrt}$$
$$t = 1997, ..., 2010; j = 1, ..., 140; r = 1, ..., 11$$

Since the dependent variable E is expressed in log- terms and $K = \frac{NMW}{med(wage)}$, the partial derivative of E with respect to NMW is

$$\frac{\partial E}{\partial NMW} = \frac{\theta_2 + \theta_3}{med(wage)} \cdot E$$

Plugging this expression in the definition of employment elasticity

$$\eta_{ER} = \frac{\partial E}{\partial NMW} \cdot \frac{NMW}{E} = \frac{(\theta_2 + \theta_3)E}{med(wage)} \cdot \frac{NMW}{E} = \frac{\theta_2 + \theta_3}{med(wage)} \cdot NMW$$
$$= (\theta_2 + \theta_3)K$$

The variance of η_{ER} is

$$\begin{aligned} Var(\eta_{ER}) &= Var((\theta_2 + \theta_3)K) \\ &= (\frac{\partial \eta_{ER}}{\partial \theta_2})^2 \sigma^2_{\theta_2} + (\frac{\partial \eta_{ER}}{\partial \theta_3})^2 \sigma^2_{\theta_3} + 2\frac{\partial \eta_{ER}}{\partial \theta_2}\frac{\partial \eta_{ER}}{\partial \theta_3}\sigma_{\theta_2\theta_3} \\ &= K^2 \cdot [\sigma^2_{\theta_2} + \sigma^2_{\theta_3} + 2\sigma_{\theta_2\theta_3}] \end{aligned}$$

The standard error of η_{ER} is

$$s_{\eta_{ER}} = K \cdot \sqrt{Var(\theta_2 + \theta_3)}$$

B.2 Dynamic Model

The dynamic model in Dolton et al. (2015) is

$$\log(E_{jrt}) = \theta_0 + \gamma_j + \lambda_{1r}t + \lambda_{2r}t^2 + \theta_1 Post_t + \theta_2 K_{jrt} + \theta_3 Post_t * K_{jrt} + \theta_4 E_{jrt-1} + \mathbf{x}'_{jrt}\delta + \epsilon_{jrt}$$
$$t = 1997, ..., 2010; j = 1, ..., 138$$

where E_{jrt} is the employment of area j.

In the dynamic model the minimum wage at time t has an effect on employment that stretches over time. The partial derivative of E_{t+s} with respect to NMW_t is

$$\frac{\partial E_{t+s}}{\partial NMW} = \theta_4^s \frac{\theta_2 + \theta_3}{med(wage)} \cdot E_{t+s}$$
$$s = 0, ..., \infty$$

and the employment elasticity η_{ER}^{t+s} at time $t+s$ with respect to NMW is

$$\begin{aligned}
\eta_{ER}^{t+s} &= \frac{\partial E_{t+s}}{\partial NMW} \cdot \frac{NMW}{E_{t+s}} = \theta_4^s \frac{(\theta_2 + \theta_3)E_{t+s}}{med(wage)} \cdot \frac{NMW}{E_{t+s}} \\
&= \theta_4^s \frac{\theta_2 + \theta_3}{med(wage)} \cdot NMW = \theta_4^s(\theta_2 + \theta_3)K \\
s &= 0, ..., \infty
\end{aligned}$$

The total employment elasticity η_{ER} with respect to the minimum wage is the sum of the employment elasticities over time

$$\begin{aligned}
\eta_{ER} &= \sum_{s=0}^{\infty} \eta_{ER}^{t+s} \\
&= \sum_{s=0}^{\infty} \theta_4^s(\theta_2 + \theta_3)K \\
&= (\theta_2 + \theta_3)K \sum_{s=0}^{\infty} \theta_4^i \\
&= \frac{\theta_2 + \theta_3}{1 - \theta_4} K
\end{aligned}$$

The variance of η_{ER} is

$$\begin{aligned}
Var(\eta_{ER}) = Var(\frac{\theta_2 + \theta_3}{1 - \theta_4} \cdot K) &= (\frac{\partial \eta_{ER}}{\partial \theta_2})^2 \sigma_{\theta_2}^2 + (\frac{\partial \eta_{ER}}{\partial \theta_3})^2 \sigma_{\theta_3}^2 + (\frac{\partial \eta_{ER}}{\partial \theta_4})^2 \sigma_{\theta_4}^2 \\
&+ 2\frac{\partial \eta_{ER}}{\partial \theta_2}\frac{\partial \eta_{ER}}{\partial \theta_3}\sigma_{\theta_2\theta_3} + 2\frac{\partial \eta_{ER}}{\partial \theta_2}\frac{\partial \eta_{ER}}{\partial \theta_4}\sigma_{\theta_2\theta_4} + 2\frac{\partial \eta_{ER}}{\partial \theta_3}\frac{\partial \eta_{ER}}{\partial \gamma}\sigma_{\theta_3\theta_4} \\
&= (\frac{K}{1 - \theta_4})^2 \cdot \sigma_{\theta_2}^2 + (\frac{K}{1 - \theta_4})^2 \cdot \sigma_{\theta_3}^2 + (\frac{\theta_2 + \theta_3}{(1 - \theta_4)^2} \cdot K)^2 \cdot \sigma_{\theta_4}^2 \\
&+ 2(\frac{K}{1 - \theta_4})^2 \sigma_{\theta_2\theta_3} - 2\frac{K^2}{(1 - \theta_4)^3}(\theta_2 + \theta_3)\sigma_{\theta_2\theta_4} - 2\frac{K^2}{(1 - \theta_4)^3}(\theta_2 + \theta_3)\sigma_{\theta_3\theta_4}
\end{aligned}$$

The standard error of η_{ER} is

$$s_{\eta_{ER}} = K \cdot \sqrt{Var(\frac{\theta_2 + \theta_3}{1 - \theta_4})}$$

C Appendix C: Supplementary results

Table 8: Replication of impact of NMW on job retention presented in Bryan et al. (2013)

Year t	Bryan et al. Men	Replication Men	Bryan et al. Women	Replication 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 9: Estimates of first-order autoregression coefficient of the residuals from equation (2).

	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

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