Ph.D. Thesis

Essays on Minimum Wages: an Evaluation of their Impact on Labour Market Outcomes

Thesis submitted for the Degree of Doctor of Philosophy

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Declaration of work

I declare that this thesis was composed by myself, and the work contained herein is my own, except explicitly stated otherwise. The second and third chapters are based on research conducted in collaboration with Peter Dolton and Jonathan Wadsworth.
The fourth chapter is based on research conducted in collaboration with Peter Dolton.

Part of the work in the thesis resulted in the following academic publications:

Dolton, P. and Rosazza-Bondibene, C. (2012). "An Evaluation of the International Experience of Minimum Wages in an Economic Downturn", Economic Policy, 69: 99-142.

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Abstract

This thesis evaluates the impact of minimum wages on labour market outcomes, exploiting variation in its "bite" across areas and years.

In the UK, a National Minimum Wage (NMW) was introduced in 1999 and has been up-rated each year since. This rather extended length of time since implementation constitutes an opportunity to take a retrospective look at the impact of this policy. Identification is based on variation in the "bite" of the NMW across local labour markets and the different sized year on year up-ratings. An "Incremental Difference-in-Differences" (IDiD) model is used in which each year's change in the NMW is considered as a separate interaction effect. This IDiD procedure allows one to evaluate the year-on-year impact of the up-rating of the NMW on different labour market outcomes.

The effect of the NMW on UK wage inequality is also assessed. In order to identify the effect of this policy on the distribution of earnings, the strategy applied in the US by Lee (1999) and more recently by Autor et al (2010) is used. Variation in the relative level of the NMW across local areas is exploited in order to disentangle the NMW effect from movements in latent wage dispersion.

Finally, new estimates of the employment effects of the Minimum Wage (MW) are produced focusing on a panel of 33 OECD and European countries for the period 1971-2009. Cross-national variation in the level and timing of the MW up-rating is exploited. The panel allows one to take into account the institutional and other policy related differences that might have an impact on employment other than the MW. It also allows one to differentiate the effect of the MW on employment in periods of economic downturn as well as in periods of economic growth, exploiting the exact timing of the recessionary experiences in different countries.

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List of Abbreviations

2SLS	Two Stage Least Square
ALMP	Active Labour Market Policies
APS	Annual Population Survey
ASHE	Annual Survey of Hours and Earnings
DiD	Difference-in-Differences
EPL	Employment Protection Legislation
FE	Fixed Effects
FEIV	Fixed Effects Instrumental Variable
GDP	Gross Domestic Product
HAC	Heteroskedasticity Autocorrelation
IDiD	Incremental Difference-in-Differences
ILO	International Labour Organization
IV	Instrumental Variable
JSA	Jobseeker Allowance
LFS	Labour Force Survey
MW	Minimum Wage
NES	New Earnings Survey
NI	National Insurance
NMW	National Minimum Wage
OECD	Organisation for Economic Co-operation and Development
OLS	Ordinary Least Square
ONS	Office for National Statistics
PAYE	Pay As You Earn
RR	Replacement Rate
TTWA	Travel to work area
UD	Union Density

1. Introduction

The labour market effects on a minimum wage, particularly its effect on employment, are perhaps one of the most contentious issues in labour economics. Contrasting theories (Stigler (1946), Burdett and Mortensen (1998), Manning (2003)) suggest that a minimum wage can have either positive, negative or neutral effects on employment depending on one’s priors and so ultimately the effects must be a matter for empirical verification. Despite more than 30 years of empirical work in this area, the effects are still disputed and vary across space and time generating room for continued work in this area. This thesis in applied labour economics consists of four research chapters that are devoted to the empirical analysis of the impact of the Minimum Wage (MW) on various labour market outcomes, exploiting variation of its "bite"[1] across areas and years in order to try and identify its effects. The decade since the advent of the MW in the UK, in particular, provide new grounds on which to undertake further empirical investigation. Most existing UK studies focus on the short-term effects of the introduction of the MW in the UK and few look at the effects over a longer time horizon when there may be more scope for factor adjustment. We also take advantage of a long cross-country panel in order to extend the analysis to research questions that UK data cannot answer. First of all, using cross-country data we focus on young people, who are more likely to be affected by MW legislation because they are at the margins of employment. Secondly, we use cross-country data to look at whether the effects of the MW change across the business cycle.

All four chapters aim at deepening our understanding of whether the MW has an effect on employment (chapter 2 and chapter 4), on hours of work and unemployment (chapter 3) and on wage inequality (chapter 5). Most of the analysis will make use of UK data at local area level (chapter 2, 3 and 5). Chapter 4 will exploit international variation in the "bite" of the MW.

[1] By "bite" of the MW we mean a variable which captures the impact of the MW and varies across areas and years. Such a measure is especially necessary when looking at the UK, where the MW is national and the units of analysis are geographical areas. One of the most widely used variables in the literature is the Kaitz index, defined as the ratio of the MW to some measure of the average wage. The closer the Kaitz index to unity the "tougher" the "bite" of MW legislation in any area. In this thesis we will use the Kaitz index as a measure of the "bite" of the MW, but we will also look at other measures to test the robustness of our results. Detailed definitions of these measures will be found in the main chapters of the thesis (Chapter 2, section 2.2.1 and Chapter 4, section 4.3 and Appendix 4.C).

Chapters 2 and 3 focus on assessing the impact of the UK National Minimum Wage (NMW) on inequality, employment, unemployment and hours of work over a decade since the introduction of the policy (1999-2007).

The main reason for looking at inequality is that one of the motivations of the introduction of the NMW was to help reduce the trend of rising wage inequality which characterised the British labour market in the 80s and 90s (Low Pay Commission 1998). Since, labour adjustments due to the NMW may take place either at the extensive margins or at the intensive margin looking at how changes in the local area minimum wage incidence are related to changes in the employment rate, the unemployment rate and average working hours in the locality makes sense.

Identification of the NMW impact is based on two sources of variation. The first is to exploit a natural variation in how the NMW "bites" in different geographical locations. In the UK case the NMW is set nationally and so there is no decision to be made at the local level. This means that the natural variation in the way the NMW impacts can be different at each geographical area. The second source of variation is to examine the effect of changes in the up-rating of the NMW over the years since it was introduced. The effects of the NMW are evaluated in each year, using an "Incremental Difference-in-Differences" (IDiD) estimator. Instead of using a simple policy-on/policy-off Difference-in-Differences model, we examine a model in which each year's change in the NMW is considered as a separate effect.

We find that an increased "bite" is associated with a significant fall in wage inequality in the bottom half of the distribution. This suggests that geographical areas where the NMW "bites" more have experienced larger decline in wage inequality than elsewhere. While the overall effect of the NMW on employment rates averaged over its existence is neutral, we do find small positive employment effects from 2003 onwards. Likewise, the association of the NMW with unemployment has been negative in recent years. NMW effects on hours have been mixed, but overall there is no compelling evidence to indicate that the NMW up-ratings have had an adverse effect on full-time total hours of work. Our conclusions are all the more credible in the sense that we got substantially the same results even though we reanalysed the data using different definitions of the geographical unit of analysis and different measures of the "bite" of the NMW.

Chapter 4 is devoted to obtaining new estimates of the employment effect of the MW by focussing on the recessionary experiences across countries over 1971-2009 and on young people.

Results in chapter 2 suggest that overall the impact of the UK NMW on employment is neutral for the period 1999-2007. These results are in line with theories where firms have some degree of monopsonistic power or with the existence of other labour market frictions. They are also consistent with the idea that there may be adjustments along margins other than employment, notably prices, profits, productivity and hours. However, one should bear in mind that the period of analysis is characterised by an economic boom and macroeconomic stability and that the effects of an upgrade of the MW could change across the economic cycle. Economic recessions clearly impose aggregate shocks to employment conditions which may affect the working of the MW.

This motivates chapter 4, which exploits variation of recessionary experiences across 33 countries and 40 years. Moreover, analysis in chapter 2 is mainly conducted for adult workers. Chapter 4 also focuses on young people, who are at the margins of employment, and therefore more likely to be affected by the employment legislation.

Using international data different sources of variation are exploited: cross-national variation in the timing and up-rating of the MW and the exact timing of recessionary experiences in different countries with a panel dataset comprising 33 OECD and European countries for the period 1971-2009. Institutional and other policy-related differences that might have an impact on employment other than the MW will be also accounted for.

The study advances in many ways the earlier cross-country study of the impact of the MW on employment (Neumark and Wascher, 2004). The first advance relates to extending the dataset of Neumark and Wascher (2004), by including more countries and by extending the time period under scrutiny. The second contribution of this study is to generalise the controlling environment to allow for the fact that countries are introducing new employment policies, or changing them very frequently. In the literature to date this has not been adequately modelled. Another area of advance relates to one of the starting point and guiding motivation of this research which is to examine the effects of the interaction between the depth of the recession and the MW. Another contribution is to use different measures of the "bite" of the MW, checking the robustness of the results. As in Neumark and Wascher (2004) and in most of the MW literature, the Kaitz index as the ratio of the MW to an average wage is used. However, the percentile at which the MW "bites" the wage distribution and the MW relative to GDP per head are also looked at. The final area of our robust investigation is that we attempt to explore the difficult problem of the possible endogeneity of the MW or Kaitz variable. The core problem with any MW regression, however formulated, is that arguably the measurement errors of the determination of employment are not independent of the "bite" of the MW. This is true to the extent that any country's government which invokes a MW (or has favourable policies relating to its up-rating in real terms) will also have unmeasured, unobserved attributes which separately affect the employment level. In this sense – one cannot reasonably assume that the variable which measures the MW is a valid exogenous variable to be included on the right hand side of such an equation. For this reason a ‘political complexion of the government’ instrumental variable (the Schmidt Index) is used.

The main finding of this chapter is that there are significant negative employment effects of MW rises for young people – but that there are basically no significant negative employment effects for adults – which are only found when one uses one alternative measure of the "bite" of the MW. It would also appear that there are important additional interaction effects of these policies for young people in times of recession. In policy terms, the findings suggest that there is a potential price to be paid by those who are at the margins of employment, such as young people, especially during recessionary periods. Countries that have not already adopted a separate MW rate for young people should consider doing so. Also our results would suggest that in times of recession it might be prudent not to raise the MW for young people**.**

Chapter 5 aims at deepening the understanding of whether the NMW contributed to the decrease of lower tail wage inequality in the UK, looking at the period from the introduction of the policy in 1999 up to 2010.

One of the motivations for the introduction of the NMW was to help to "make work pay" and address in-work poverty, against a background of rising wage inequality which characterised the British labour market in the 1980s and early 1990s. At the outset, the Low Pay Commission (1998) hoped that the NMW might take "greater inroads into pay equality" without putting jobs at risk. That is why the chapter aims at having a deep insight of the impact of the NMW on wage inequality.

In order to estimate the effect of the NMW in the recent trend towards lower tail wage inequality in the UK, the methodology exploited by Lee (1999) for the US is used. We assume that the level of wages would have been similar across areas (or would have changed similarly), if it were not for the effect of the NMW. Differences in average wage levels across areas, that are assumed to be exogenous, therefore induce useful variation in the real "bite" of the NMW across areas that allows the identification of its effect net of other confounding forces.

Three issues that appear to bias Lee's (1999) work are addressed. Namely, the omitted variable bias due to the absence of area fixed effects (Autor et al, 2010), measurement error in the variables (Bosh and Manacorda, 2010) and simultaneity bias. We address the measurement error in the wage data by instrumenting our measure of the NMW computed with the Annual Survey of Hours and Earnings (ASHE) data with a new NMW measure computed using the Annual Population Survey (APS). To the extent that measurement error in the ASHE data is not correlated with measurement error in the APS data, this procedure will purge the estimates of potential correlation between the included regressors and the error term due to measurement error. We also attempt to solve the simultaneity bias problem in our data using alternative measures of wage inequality in our regression, such as the Atkinson index and the Generalised Gini.

By looking at wage differentials of different percentiles q relative to the median wage ($w^{q}_{it} - w^{50}_{it}$), we find an effect of the NMW on the wage distribution. Using 2SLS estimation, the point estimates tend to become smaller at higher deciles and are statistically significant only up to the first decile, suggesting a negative impact of the NMW on lower tail wage inequality and perhaps some small spill-over effects. By looking also at different measures of wage inequality, such as the Atkinson index and the Generalised Gini, we find again a significant contribution of the NMW in reducing wage inequality. In conclusion, the findings suggest that the NMW has an impact on the UK wage distribution, contributing significantly to the decrease of lower tail wage inequality in past years. In policy terms, the NMW helped to "make work pay" and address in-work poverty, against a background of rising wage inequality which characterised the British labour market in the 1980s and early 1990s.

The following chapters now expand on these issues in more detail.

2. Employment, Inequality and the UK National Minimum Wage over the Medium-Term

2.1 Introduction

It is now more than ten years since the National Minimum Wage (NWM) was introduced in the UK in April 1999. This rather extended length of time since implementation affords us an opportunity to take a retrospective look at the impact of the NMW. Most existing UK studies, (Stewart, 2002, 2004a, 2004b) have focused on the impact of the introduction of the NMW, finding, broadly, that the employment effects of the introduction were negligible. Aside from adjustment along other dimensions such as productivity, profits, hours or prices, or simply that the initial rate was too low in the wage distribution, another possible reasons for this, arguably counter-intuitive employment effect is that any longer-run effects have not been captured by previous studies. Since in the short-run the costs of adjusting inputs tend to be high, the response of employment to NMW increases might not be immediate. As recently pointed out by Neumark and Wascher (2007): “Most of the existing research on the United Kingdom has been limited to estimating short-run effects, and in our view, the question of the longer-run influences of the NMW on UK employment has yet to be adequately addressed”. In this chapter we take a medium to long run look at the impact of the NMW in the UK and its up-ratings and try to assess whether this has had a differential impact across heterogeneous geographical areas.

Since inception, the UK NMW has been administered on a national basis, with both adult and youth rates applying to all parts of the country. However, the issue of whether a national minimum adequately reflects putative regional variation in productivity has recently been mooted in government and in the media.[2] The longstanding geographic variation in wage rates across the UK does indeed have consequences for the "bite" of the NMW in different areas. As Stewart (2002) points out, the NMW reaches further up the wage distribution in certain parts of the country than in others. We therefore make use of both this geographical variation and the variation in the real level that the NMW has been set at over time in order to see how changes in the local area NMW incidence over several years of the minimum wage’s existence are correlated with changes in local area performance. Since the level of the NMW is typically announced 6 months in

[2] Daily Telegraph 23 July 2007, http://www.telegraph.co.uk/news/uknews/1558174/Gordon-Brown-to-vary-minimum-wage-over-UK.html

advance of any up-rating, we also explore issues of advance implementation of employment changes in the dynamic specifications that follow.

While there are a large number of studies on the labour market impact of the NMW, especially on the impact on employment, (see Brown et al (1982) and Card and Krueger (1995) for extensive reviews of the literature), only a few studies evaluate the impact of the NMW by exploiting geographical variation in local or regional labour markets, (See Card (1992) or Neumark and Wascher (1992) for the United States, Stewart (2002) for the UK). This chapter builds on that small literature by examining the impact of the NMW in the UK over the period 1997-2007, comparing the period two years before its introduction with the subsequent history of the NMW and its up-ratings. This enables us to provide an additional insight by distinguishing effects between those in a NMW policy-off period compared to each incremental up-rating of the NMW in subsequent years. Hence instead of using a simply policy-on/policy-off, Difference-in-Differences (DiD) model, we examine a model in which each year's change in the NMW is considered as a separate interaction effect. This 'Incremental Difference-in-Differences' (IDiD) estimator is a logical corollary of the econometric model suggested by Wooldridge (2007) and Bertrand et al (2004) in that it introduces a yearly interaction for each up-rating of the NMW so that we may gauge the impact of each change in the NMW. We use this IDiD procedure to evaluate the year on year impact of the up-rating of the NMW on both employment and inequality.

Secondly, we seek to assess whether the definition of the variable used to capture the impact of the NMW makes a notable difference to the analysis. In the empirical literature there is some debate over the exact definition of which variable to use to measure (or instrument for) the NMW. In our work, three different minimum wage variables are used and compared. Two measures focus on the proportion of workers directly affected by increases in the minimum wage: the minimum wage “share” (the proportion paid at or below the minimum wage) and the “spike” (the proportion paid at the minimum). The third measure is the Kaitz index, the ratio of the minimum wage to average wages in the local area.

Thirdly, we examine whether the definition of the geographical unit used for the analysis matters. Since the definition of what constitutes a 'local labour market' in Great Britain is still open to discussion, the analysis is undertaken at three different levels of geographical aggregation. As in Stewart (2002), the data can be divided into 140 areas comprising unitary authorities and counties. However, the same analysis can be done using 406 unitary authorities and districts. We also look at how the results change if we use the definition of 67% of people living and working in the same geography to capture a local labour market, as now used by the UK national statistics office to define a “travel to work area” (TTWA). We remain agnostic as to what the correct definition of a 'local labour market' is and let the data tell us whether such definitional difficulties matter.

Finally, the chapter examines the robustness of our results with regard to the issues associated with: dynamic specification to incorporate the lagged effects of the impact of the NMW, fixed effects for geographical areas, time and interaction effects, and we also assess whether the estimates differ if we include young people (those aged 16-25) or just use adults separately in the analysis. In this testing we suggest that much of the previous literature is sometimes presented as if the results are in stark contrast to each other. Our take on this literature is that it often estimates fundamentally different parameters and that this explains a large degree of the differences in results.

Previous research in the UK focused mainly on the employment effects of the NMW and for the most part found mainly no impact. However since, one of the motivations of the introduction of the minimum wage was to help reduce the trend of rising wage inequality which characterised the British labour market in the 80s and 90s (Low Pay Commission 1998), we show how changes in the local area minimum wage incidence are related to the extent of wage inequality in the locality along with our employment estimates.

The chapter is organised as follows. Section 2.2 describes the datasets used and the characteristics of the data and contains a description of the maps of the incidence of the minimum wage and the measures of local area performance in each local area. Section 2.3 outlines the methodology for the analysis. The main results of the analysis are presented in section 2.4. Section 2.5 focuses on robustness checks. Section 2.6 concludes.

2.2 Data

The central idea of this chapter is to see whether geographic variation in the "bite" of the minimum wage is associated with geographic variation in employment and wage inequality. Geographical variation in wages in the UK is exploited in order to evaluate the impact of the NMW on employment and inequality. The data used in this study are drawn primarily from three sources. Data on earnings and a restricted number of covariates all disaggregated by geography is provided by the Annual Survey of Hours and Earnings (ASHE) from 1997 to 2007. The survey, conducted in April of each year, employers are asked to provide information on hours and earnings of the selected employees. The geographic information collected for the full sample period used in the chapter is based on workplace rather than residence. This is the only dataset that has hourly wage information from 1997 to 2007 at the various levels of geographical disaggregation used in this chapter. Alongside the hourly wage, the ASHE data enable us to compute different measures of wage inequality at the same geographic level, (we use the $50^{th}/5^{th}$, $50^{th}/10^{th}$ percentiles of the wage distribution. See appendix 2.B for a detailed description of the limitations affecting ASHE dataset).

The geographic variation in wages will reflect the demographic and industrial composition of each local labour market. The changing industrial composition of an area and the extent to which industries are low and high paying will affect the changing incidence of the minimum wage working in a locality. Likewise the skill, age and gender composition of the local workforce. To a certain extent we can control for variation in these factors with a set of time varying local labour market control variables, drawn from either ASHE or matched in from complementary Labour Force Survey (LFS) data. However, the choice of what constitutes a local labour market is open to discussion, therefore the analysis is conducted at three different levels of aggregation. First, the analysis is conducted at unitary authority and district level which includes 32 London boroughs, 238 districts[3], 36 metropolitan districts and the 46 unitary authorities in England. This geography also includes the 22 unitary authorities in Wales and the 32 unitary councils in Scotland, resulting in 406 local areas in Great Britain. The median ASHE sample cell size is 311 and the smallest cell is 37. The second level of analysis is conducted at unitary authority and county level including 34 English counties, 6 English metropolitan counties, 46 English unitary authorities, inner and outer London and finally 52 unitary authorities in Scotland and Wales.[4] This results in 140 local areas in Great Britain. Here the median sample cell size is 575 and the smallest cell is 42. The final level of our analysis is to use a general definition of a TTWA, by aggregating up from district level to create areas in which

[3]The London borough City of London and the district Isles of Scilly are excluded from the analysis due to small sample sizes.

[4]The Orkney Islands, Shetland Isles and Western Isles are aggregated together. The 36 English metropolitan districts are combined into 6 English metropolitan counties. London Boroughs are aggregated into inner and outer London. This allows to have matched geographies in the LFS and in the ASHE, using the definition of the variable “uacnty” in the LFS.

67% of people living and working in the same geography. Since TTWAs are not available for the entire period considered in this study the only option was to attempt to replicate our results for the most 'reasonable' definition of a TTWA that we could manually reconstruct from the data available. This gave us 138 new geographical areas for which we repeated all our analysis. The mechanics of how to do this and the estimated effects using TTWA instead of the formal geographical administrative areas are given in the robustness checks section of the chapter.

We then match local area employment data from the LFS with the minimum wage covariates generated from the ASHE. There is an important feature of the timing of data collection which we exploit in order to try and make sure that our employment variable is measured after the up-rating of the NMW. The ASHE estimates for hourly earnings and therefore the minimum wage variables used in this chapter are recorded in April of each year. Since the minimum wage was first introduced in April 1999 but then up-rated in October of each following year, the NMW variables are therefore generally recorded 6 months after each NMW up-rating. There are however two exceptions: April 1999 which is contemporaneous to the introduction of the minimum and April 2000 which is 1 year from the introduction of the minimum. To reduce simultaneity concerns, the wage data in April of year *t* is regarded as having absorbed any effect of the NMW upgrade in October of year *t-1*. This is in turn matched to employment data taken from June to August of year *t*[5]. This means that the estimated impact effect we identify is a mixture of the impact of the up-rating in year *t-1* and any changes from the already announced anticipation of the effect of the new NMW level in year t[6].

Data on employment at these levels of aggregation derived from the LFS are available via NOMIS for yearly data for 1997 and 1998. For the period 1999-2005 we use employment rates calculated from the quarterly LFS local area data. For the years 2006 and 2007 we use the quarterly LFS Special License data to calculate the employment rate. Data availability means that we can do our analysis separately for three age groups: All workers from 16 years old to retirement age (65 years for men and

[5] For 1997 and 1998, data on employment rates are collected from March 1997 to February 1998 and from March 1998 to February 1999. Quarterly data is not available for these two calendar years. Since LFS Local Area data is only available in seasonal quarters, it is only possible to use the June-August quarter and not a longer period (eg. from May to September).

[6] Swaffield (2008) shows that there is little early upward adjustment in wages in the six months prior to October over several years of data.

60 for women); Adults workers, from 25 years old to retirement age[7]. A detailed definition of the key variables in the analysis is reported in appendix 2.A

2.2.1 Measures of the NMW

One of the most widely used variables in the literature is the Kaitz index, defined as the ratio of the minimum wage to some measure of the average wage. We use the median wage in our study. The closer the Kaitz index to unity the "tougher" the "bite" of minimum wage legislation in any area. However, the denominator can be influenced by factors other than the level of the NMW and so the median wage is arguably more endogenous in an employment regression. For example, a positive correlation between the employment rate and the median wage might be generated by an exogenous labour demand shift. This will create a negative correlation between the Kaitz index and the employment rate. In view of these problems with the Kaitz index, two other minimum wage variables are used in this study. These two measures focus on the proportion of workers directly affected by increases in the minimum wage: the minimum wage “share” proportion paid at or below the minimum wage, and the “spike” (proportion paid at the minimum). The larger the spike or the shares, the more likely the impact of the minimum wage on the local wage. The “shares” and the “spike” should exclude the variation in real minimum wages that results from inflation or other aggregate factors (Neumark and Wascher, 2007).

The logic of our identification strategy is evident in the descriptive statistics in figures 2-1 to 2-3. Figure 2-1 highlights the temporal variation in the NMW, comparing the nominal hourly wage level of the adult NMW over time with the notional level which would have been achieved if the NMW were indexed to average earnings. The figure shows how the NMW started off by being lower than the average rise in earnings and then rose more steeply than this series. Most marked is the rise in this level in both real and nominal terms since 2003. The largest rises in the NMW are in 2001, 2004 and 2006. This is mirrored in the rising level of the Kaitz Index over the same years shown in figure 2-2.

As well as temporal variation in the NMW, there are clear geographic differences in the "bite" of the NMW. The 95% range for the Kaitz index is around 20% points and the spread for the share estimate is around 5 points. This pattern does not change much over

[7]Due to the presence of age bands in the area-level LFS, it is not possible to analyse the impact of the NMW on adults from 22 years up that the actual coverage of the adult rate of the NMW would require. Analysis is therefore restricted to persons from 25 years up.

the 1997-2007 period. While the average value of the Kaitz has risen, there is less evidence that these spreads have risen or fallen consistently over time. Figure 2-3 plots how these patterns of geographical low pay vary across the UK at the inception of the NMW in 1999 alongside the changes in the NMW share over the period 1999-2007. The "bite" of the minimum wage in London tends to be lower than in the rest of the country. Areas particularly affected are the rural periphery of the country and the formerly industrialised urban areas. Over time the map shows that the "bite" of the minimum wage has increased across more areas. The biggest changes in the "bite" occur in parts of the Midlands, Hampshire, Wiltshire and Dorset and parts of Lancashire and the North East. As we show below, these changes are associated with changes in the local area levels of wage inequality. The tougher the NMW "bites", the bigger the effect on local measures of wage inequality.

2.3 Methodology and identification

To understand any of the estimation results relating to the impact of the NMW one must be clear about the exact form of the econometric specification and which parameters the model aims to identify in the model.

Among the first to use panel data to address the question of the impact of the minimum wage were Neumark and Wascher (1992) who used US state data from 1973-1989. They estimated the model:

$$E_{jt} = \alpha + \gamma T_t + J_j + \beta MW_{jt} + \delta X_{jt} + \varepsilon_{jt} \qquad (2.1)$$

Where E_{jt} is employment at time t in State j , MW_{jt} is the level of the MW (adjusted for coverage) at time t in State j, X_{jt} is a set of controlling regressors at time t in State j, T_t is a set of year effects and J_j is a set of State fixed effects. Fixed effect estimation identifies potential causal inferences based on changes in the regressor and regressand given the assumption that the unobserved heterogeneity across areas remains constant over time periods. Later Neumark and Wascher (2004) use the same specification to estimate the impact of the NMW laws across countries with the slight modification that now the MW_{jt} term is similar to the Kaitz index using the ratio of the NMW in country j at time t divided by the average wage in that year[8]. Neumark and Wascher in their various papers, whether at the US state level or at the level of countries, also find a negative employment effect of the NMW.

[8]Usually the Kaitz index is also weighted by some measure of 'coverage' of the NMW in the sense of the fraction of the labour force that the NMW applies to.

The logical critique of this panel model is that it still suffers from potentially all the same sources of potential heterogeneity bias as the simple time series model. Indeed it could even be argued that using geographical states as the unit of observation could potentially have even more problems - if for example - one state legislature's decision to implement or change a minimum wage is heavily influenced by another neighbouring state's policy decision. This concern is less of a problem in the UK context as there is a national NMW rather than a state minimum - in which case the actual level (and change) in the NMW is not under the control of the authorities in any particular location.

A related methodological departure focused on identification is suggested by Card (1992) and Stewart (2002) in which a 'structural' econometric model consists of two equations. The first is a form of labour demand equation which suggests that any change in the employment rate in area *j* is a movement along the labour demand curve which results from a change in the wage level in area *j*.

$$\Delta E_j = \gamma_0 + \eta \Delta W_j + u_{1j} \quad (2.2)$$

The second equation is a form of identity suggesting that the wage increase in area *j* is a function of the proportion in the area who are 'low paid', P_j.

$$\Delta W_j = \alpha_1 + \lambda P_j + u_{2j} \quad (2.3)$$

Substituting equation (2.3) into equation (2.2) we get

$$\Delta E_j = \gamma_0 + \beta P_j + \varepsilon_j \quad (2.4)$$

Where $\beta = \eta\lambda$, with λ assumed to be positive, implying that β has the same sign as η which basic economic theory would suggest is negative if the demand for labour falls as wages rise. According to Stewart (2002) the precondition for identification is that the proportion in the area that are 'low paid', P_j is a predetermined instrument for the endogenous wage change.

The central idea of our chapter is also to see whether geographic variation in the "bite" of the minimum wage is associated with geographic variation in employment. However, we also allow the effect of any treatment to vary over time, given the differential pattern of up-ratings that we observe in the data. This can be done by pooling over the 11 year period and letting the treatment be the measure of the "bite" of the NMW in each area at time *t*, $P_{jt,}$ so that the model estimated is:

$$E_{jt} = \gamma_0 + J_j + \sum_{t=1999}^{2007} \gamma_t Y_t + \theta_0 P_{jt} + \sum_{t=1999}^{2007} \theta_t^{IDID} Y_t P_{jt} + \delta X_{jt} + \varepsilon_{jt} \quad (2.5)$$

Where E_{jt} is a measure of area labour market performance in area j at time t, J_j are area effects, and Y_t is a set of year effects. Area fixed effects are included to control for omitted variables that vary across local areas but not over time such as unmeasured economic conditions of local areas economies that give rise to persistently tight labour markets and high wages in particular areas independently of national labour market conditions. Time fixed effects control for omitted variables that are constant across local areas but evolve over time.

The IDiD coefficients θ_t^{IDiD} on the interaction of the year dummies and the measure of the "bite", capture the average effect of the up-rating of the NMW in each year, starting from the introduction of the policy in 1999 all relative to the 'off period' of 1997 and 1998, provided of course that the proportion in the area who are ‘low paid’, P_{jt} is a valid instrument for the endogenous wage change. The advantage of using the IDiD estimation procedure is that it facilitates the estimation of year on year incremental effects of each year’s up-rating. So even if the average effect over all years is insignificantly different from zero, this does not mean that the effect of any individual year's change in the NMW is also zero. Note that one cannot deduce the longer run effect of all the changes in the NMW by simply summing all the year-on-year IDiD coefficients.[9] The long run effect can only be measured in aggregate by using one DiD coefficient for the whole period. We therefore present both short run IDiD and medium run DiD estimates in what follows.

The literature is silent on how to untangle autocorrelation in panel data with very short time series like ours. An additional concern is the obvious one of spatially contiguous areas giving rise to heteroskedastic errors. With regard to the latter problem one approach is to model the form of these spatial relations. As all our geographical areas have bordering areas then it may well be that there is a clear relationship between these contiguous areas. The complex nature by which these neighbouring areas have local labour markets which are inter-related and how to model these effects is left for future work. In the absence of an appropriate spatial model, we calculate standard errors robust to heteroskedasticity and serial correlation of unknown form, Wooldridge (2002

[9]This is because some additional (untestable) assumption relating to independence of effects over time would be necessary. In addition, since we use a dummy variable interaction term, rather than a normalised metric on how large each increment was then this also makes aggregation of the individual interaction term estimates difficult.

p.275), which gives consistent, if inefficient, estimates. Another alternative is to simply cluster the data by local area. [10]

2.3.1 Identification Issues

One important question to ask is how long it should take the introduction (or changes) in the NMW to have its full effects on employment and other economic indicators (especially since some of the variables in the data are already measured with a lag). From an empirical point of view, this raises the specification issue about including a lagged effect of the minimum wage variable in the regression. The debate on this question is still ongoing. On the one hand, employers might react relatively quickly to increases in minimum wages. Employers might even adapt before the implementation of the minimum wage. Brown et al (1982), regarding employment, argue that: "One important consideration is the fact that plausible adjustment in employment of minimum wage workers can be accomplished simply by reducing the rate at which replacements for normal turnover are hired.", (p.496). Clearly the size of any change to the existing wage bill generated by the NMW matters here. Another reason given by the authors is that minimum wages increases are announced months before they are implemented – typically 6 months in the UK - therefore firms may have begun to adapt before the increase of the minimum wage come effectively into force. On the other hand, it might take time for employers to adjust factors inputs to changes in factors prices. Hamermesh (1995) points out that in the short run capital inputs might be costly to adjust. If firms adjust capital slowly following an increase of the minimum wage, the adjustments of labour input might be slowed as well. The use of a lagged minimum wage measure as well as the inclusion of fixed effects in the regression also helps to decrease the possible endogeneity of the minimum wage variable which occurs from correlation of either the proportion paid at the minimum or, in case of the Kaitz index, the minimum wage and the median wage with labour market conditions or productivity.

A further issue of identification arises from the 'common trends assumption' which, in our context, is that the effect of market conditions will be the same across all geographic units in the absence of the introduction of the NMW. One way of examining this is to consider whether the employment rate has the same underlying trend across all our geographical units before the introduction of the NMW. In our case we cannot do this because the small geography LFS data which we use to construct the

[10] Clustering by local area rather than using the general robustness correction makes little or no qualitative difference to our conclusions.

employment rate does not go back before 1997. However, it is possible to have a longer off-period starting from 1994 and using 95 areas, which correspond to the coding used on the NES (the National Earnings Survey which preceded the ASHE) up to 1996.[11] The results of the test give us some confidence about the internal validity of the model, being unable to reject the null of a common trend at 10% level for the two age groups considered in the study. [12] Whilst this is no proof of the presence of common trends in our data, this gives us some confidence about the internal validity of our model for the full set of more detailed geographies.

The NMW was not the only labour market policy instrument in operation over the period that varied by area and time. It may be that identification of a NMW effect is also compromised by any correlation of these other interventions with changes in the local "bite" of the NMW. The set of area and time varying covariates in the control vector X_{jt} help net out some of the concerns over these issues. [13]

2.4 Results

We begin with a summary of the association between the level of lower tail wage inequality and the "bite" of the NMW in the local area. If there appears to be an impact on the wage distribution then this might suggest there would be effects on other measures of local labour market performance. There is good reason to expect that imposition and then raising of the NMW will have positive effects in reducing wage inequality at the bottom end of the income distribution. If one truncates the income distribution from the left by forcing employers to pay the lowest earners at a specified minimum then automatically one expects that (unless there are large spill-over effects) we would find that inequality would be reduced as the NMW rises, other things equal. Dickens and Manning (2004a) report evidence of these effects in the UK around the

[11]The areas comprise all existent counties, the counties abolished with the 1996 local government reform and the London boroughs. The "City of London" was deleted from the dataset due to small sample size and the Scottish Islands were excluded from the analysis because they are not present in the data across all years.

[12]For adult workers (25 years to retirement) we cannot reject the null of a common trend at the 10% level ($F(94, 285)=1.41$). For all workers (16 years to retirement) we cannot reject the null of a common trend at the 10% level ($F(91, 276)=1.45$) if we omit three areas, all with small sample sizes, (Scottish Borders, Gwynedd and Shropshire). However, omitting these areas from our IDiD regressions does not change our main results.

[13]Employment rates for groups more or less likely to have been affected by the NMW within areas as a means to identification through a triple Difference-in-Differences, could, in principle be disaggregated by local area and industry or education from 2004 onward using the Annual Population Survey, though the level of area disaggregation would have to be larger than that used in the present study because of sample size limitations. Wages could be disaggregated by (macro) region and industry back to 1997.

introduction and other authors report similar findings from the US. (See DiNardo et al (1996), Lee (1999) and Autor et al (2010)).

There are obvious endogeneity concerns here when regressing a measure of wage inequality on another variable linked to wages. For this reason we do not use the Kaitz index as an NMW toughness proxy and the remaining estimates should be seen as indicative only of correlations in the data. Table 2-1 presents our IDiD results using model (2.5) for the effects of the year on year up-ratings of the NMW on local area wage inequality as measured by the log 50-5 and the log 50-10 percentile ratio. The results are given for two different local labour market definitions for all adults aged 16 and over. We have also performed our estimation for the TTWA as defined above. Our results with their TTWA robust counterparts can be summarised in a graphical representation of the estimates coefficients from the underlying regression model. Figure 2-4 graphs the estimated NMW coefficients along with the 95% confidence interval for both the 406 and TTWA area levels of aggregation. The coefficients of our IDiD regression are all negatively significant and increasingly so over time, indicating that lower tail wage inequality fell more in areas where the NMW bit most. It is also important to note that there is a clear overlap in all of the 95% confidence intervals for both these different geographies.

There are also smaller effects moving up the wage distribution, again consistent with the idea that the NMW is driving the fall in inequality. The NMW coefficients for the 50-10 wage ratio are smaller than the equivalent coefficients using the 50-5 ratio. This may also indicate limited spill-over effects of the NMW as the lower percentile used in the measure of inequality moves further away from the percentile at which the NMW "bites". When we repeat the same exercise at 140 areas level of aggregation the results are qualitatively similar. Here the regression coefficients tend to be even more negative than the coefficients for the 406 areas, suggesting there may be a greater degree of attenuation bias in the 406 level of disaggregation.[14]

There is little difference between the estimates when wage inequality rate for all age groups is used as the dependent variable or when only the adult (25 to retirement) rate is used.

We next present estimates of the DiD model (2.1) using (the log of) employment as the labour market outcome of interest to summarise the NMW effect on employment over the medium term, namely the average over nine years since its introduction relative

[14]If we use the 50-20 differential as the dependent variable, the NMW effects are smaller still.

to the base period of 1997/98. Table 2-2 outlines the estimated NMW coefficients . For each NMW toughness measure there are 4 columns. The first column is the estimate from a simple regression of the dependent variable on the NMW measure, effectively establishing the correlation between the two variables. The estimates confirm the long-established fact that employment is lower in low wage areas. The correlation is stronger when 140 areas are used rather than 406. In every regression the estimated coefficients based on the 406 areas are attenuated relative to the higher level of aggregation estimates. This again suggests the presence of a greater degree of measurement error among the more disaggregated data. There is little difference between the estimates when total employment is used as the dependent variable or when the adult (25 to retirement) rate is used. The addition of year specific time dummies makes little difference to the estimates, but the addition of area fixed effects removes the positive association between low wages and low employment. Since any effect is now identified through variations in the NMW "bite" over time across areas, this suggests no overall difference in employment growth rates between areas where the NMW "bites" most compared to areas where the NMW has less impact. The further addition of time and varying area-level covariates has little effect.

Table 2-3 presents the results of the IDiD estimates for several samples based on the model (2.5), with a full set of controls along with time and area fixed effects. The results suggest that the average estimate of no association between the NMW "bite" and employment obscures significant changes over the sample period. Indeed over time, the positive association between low pay and NMW toughness becomes negative, so that in the latter sample period, areas where the NMW bit most experienced higher employment growth. These positive estimates are larger and most significant for the sample of all individuals aged to 16 to retirement, but in 2004 and 2006 there are positive, significant estimates of the NMW "bite" on employment for two of the three NMW measures. These point estimates effects are small in magnitude,[15] but it is clear that they are masked if the simple DiD policy-on/policy-off variable is used. If the standard assumptions of Difference-in-Differences relating to the Stable Unit Treatment are applicable (namely that no other systematic factors are varying across geography and over time) then we can interpret this as a causal impact of the up-ratings to the

[15]For example the point estimate of 0.026 for 2004 implies that employment growth in that year was 0.26% higher in an area where 10% of employees were paid at or below the NMW compared to areas where no-one was paid the NMW compared to the respective growth rates in 1997/98.

NMW. On this basis, if anything, employment rate appears to have risen more in areas where the NMW has more relevance.[16]

Figure 2-5 plots the individual year employment estimates for the 16 to retirement group for both the 406 areas and the TTWA areas. The regression estimates are given in Table C 2-4. Here again we can see clearly that whichever geography is used there are grounds to believe that there were positive employment effects for 2004 and for 2006 with a reasonable possibility that the positive effect also exists for 2003 and 2005. Figure 2-1 suggests that these are all the years in which the up-rating of the NMW kept it above the general rise in average earnings.[17]

2.5 Robustness checks

Table 2-4 offers a set of robustness checks for the employment estimates. To address concerns over measurement error in the construction of the minimum wage variables, we use instead the mandated minimum plus 5 or 10 pence to generate the share, spike and Kaitz variables. This makes very little difference to the estimates, nor does using the mean rather than the median as the denominator for the Kaitz index. A weighted least squares regression, based on the sample sizes of the local areas used to calculate wages, also makes little difference to the overall impression that while the full sample period there is little association between the "bite" of the minimum and employment, there are years toward the end of the sample period when there is a positive association between the "bite" of the NMW and employment.

An alternative way to eliminate fixed unobserved area characteristics and obtain consistent estimates is to estimate the model in differences. Table C 2-1 compares within group estimates of the NMW effect estimated in Table 2-2, averaged over the 9 years, with the estimates in differences. In both models time fixed effects are added to control for omitted variables that are constant across local areas but evolve over time. Both models suggest no overall difference in employment growth rates between areas where the NMW "bites" most compared to areas where the NMW has less impact. Similarly using different dynamic specifications, outlined in Table C 2-2, make little differences to the conclusions drawn from Table 2-2.

The results of the IDiD estimates measured the additional incremental effect of the up-rating of the NMW in each year relative to the off-period of 1997/98. In Table C 2-3,

[16]One concern with the timing of the effects we have found is that the post 2003 period coincides with the change in the sampling frame of ASHE. However, it would seem to us that there is no way to test this.

we run separate Difference-in-Differences regressions year by year, measuring the effect of the up-ratings of the minimum wage in each year relative to the year before. The estimates for the years 1997-8 (before the NMW was introduced), effectively test how our Difference-in-Differences model performs on a 'placebo', fictitious law. The estimated coefficients are not significantly different from zero, independently on the minimum wage measures used and the level of geographical aggregation, giving us confidence about the internal validity of our model. The results for the other year pairings are generally insignificant, excepting the negative and significant estimate of the introduction of the NMW in 1999 using the proportion paid at the NMW. In general then, it seems that the positive employment results we find above are driven mainly by comparisons with local area conditions in the run-up to the introduction to the NMW[18].

The definition of a local labour market is moot, however it is important to test the robustness of our findings to different definitions of a local labour market. We have therefore used a travel to work area approach alongside the other two local area classifications. Since TTWA data do not exist on any administrative data bases that we are aware of for the entire length of our panel, we have had to create an alternative set of local areas that are close to TTWA from the raw ASHE/LFS data that we do have access to as follows. The most recent criteria used by the ONS to define TTWA is that at least 67% of those who live in the area also work there and at least 67% of those who work on the area also live there. We therefore use ASHE data from 2002 where we have information about the local authorities where people work and local authorities where people live. We then compute commuting shares (given by the proportion of people who live in an area and work in another area and the proportion of people who work in an area and live in another one). We than keep all the district and unitary authorities where the ONS definition of travel to work areas holds (around 12% of areas). For the other local authorities, with the help of ArcGis software we overlap the map of ONS TTWA with the map of local authorities and combine Districts and Unitary Authorities into existing TTWA boundaries. With these new geographies we compute the commuting patterns to check the consistency with ONS definition of TTWAs. For the few areas (14%) where the ONS definition of TTWAs still does not hold we aggregate further. Doing this we end up with 138 areas. Some 90% of these are such that at least

[18]Dickens, Riley and Wilkinson (2009) have also recently used an area based approach over the latter half of our sample period. They find statistically insignificant NMW effects on employment growth over this period. This again seems to suggest that the base period is an important reference point underlying the results.

67% of working residents work in the area and at least 67% of workers are resident in the areas. Table C 2-4 shows how changing the definition of geography used in our analysis the main message of the chapter does not change. Similar small positive effects of the NMW are found when we use our TTWA definition.

In Table C 2-5 we present our IDiD results using as a base year either 1997 only or 1998 only. This is mainly because in 1998 there might be already an anticipation of the effect of the introduction of the NMW. The results using either 1997 or 1998 as a base year are similar to our main regressions results, suggesting that the anticipation effect of the introduction of the NMW in 1998 was limited. The coefficients of the interactions between the NMW measure and 1998 as well as 1997 are insignificant.

The regression estimates of Table C 2-6 show our IDiD estimates using a longer off-period from 1993 to 1998 and compares them with our previous estimates. Due to the changing in coding reflecting the local government reorganisation of the mid-1990s, the geography used in previous sections of the chapter cannot be used for a longer period estimation. Instead we use the same 95 areas used to test for common trends. The results in Table C 2-6 again show that the average estimate of no association between the NMW "bite" averaged over the entire sample period obscures significant changes at different points. Comparing the regression results of the 408 and 140 areas with the ones of the 95 areas, over time, the initial (insignificant) negative association between employment and NMW toughness is now statistically significant and then becomes positive and statistically significant.

2.6 Conclusions

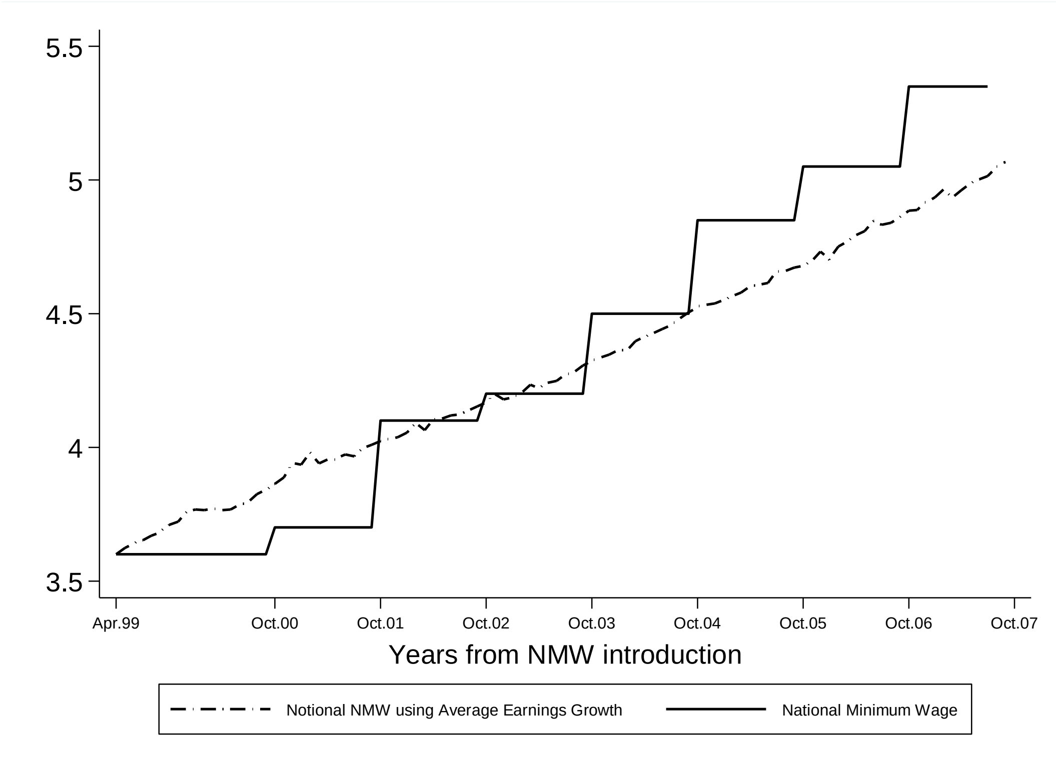
Our starting point was that much of the US debate over the employment effects of the NMW has generated a 'lot of heat but not much light'. This conclusion is warranted to the extent that our examination of the empirical literature made it clear that much of the US controversy and debate over whether the effects on employment are negative or positive is actually arguing about different estimated parameters in the sense that they use different estimation strategies, with different types of data, on widely different samples of people of different ages. The truth is that most of the papers in this literature are estimating different marginal effects.

Our identification strategy was to use two sources of variation to try and identify the effect of the NMW. The first is to exploit a natural variation in how the NMW "bites" in different geographical locations. In our UK case the minimum wage is set nationally and so there is no decision to be made at the local level (in sharp contrast to the US case). This means that the natural variation in the way the NMW works must be different at each geographical area. Our second source of variation was to examine the effect of changes in the up-rating of the NMW over the years since it was introduced. This estimation is based on an IDiD method which allows us to estimate the marginal (interaction) effect of each year change in the NMW. The combination of these two different methods of identification along with the rigorous use of different robustness checks means that we can be more confident about the estimated effect of the impact of the NMW. Our conclusions are all the more credible in the sense that we got substantially the same results even though we reanalyzed the data in three completely different ways using completely different definitions of the geographical units of analysis.

The conclusion from our estimates is that overall there seems to be no significant association of the NMW on employment when we use a conventional Difference-in-Differences estimation for the whole policy-on/policy-off effect. However, when we use of IDiD estimation method we retrieve significant positive effects on employment in recent years. Most specifically in the period 2004-6. These findings are interesting as they are firstly consistent with much of the recent literature focusing on the introduction of the NMW (i.e. since they also get zero or small positive effects) but also because they explain why it may be possible to get both zero and positive effects. What drives these results is open to interpretation and subject to our ability to identify a NMW effect. It may be a realisation that the effects of the NMW on the wage bill may not warrant widespread employment losses, particularly given the level of demand and the ability of UK firms to adjust to labour cost shocks through a combination of hours, prices, productivity and profits documented elsewhere (summarized in Metcalf (2008)).

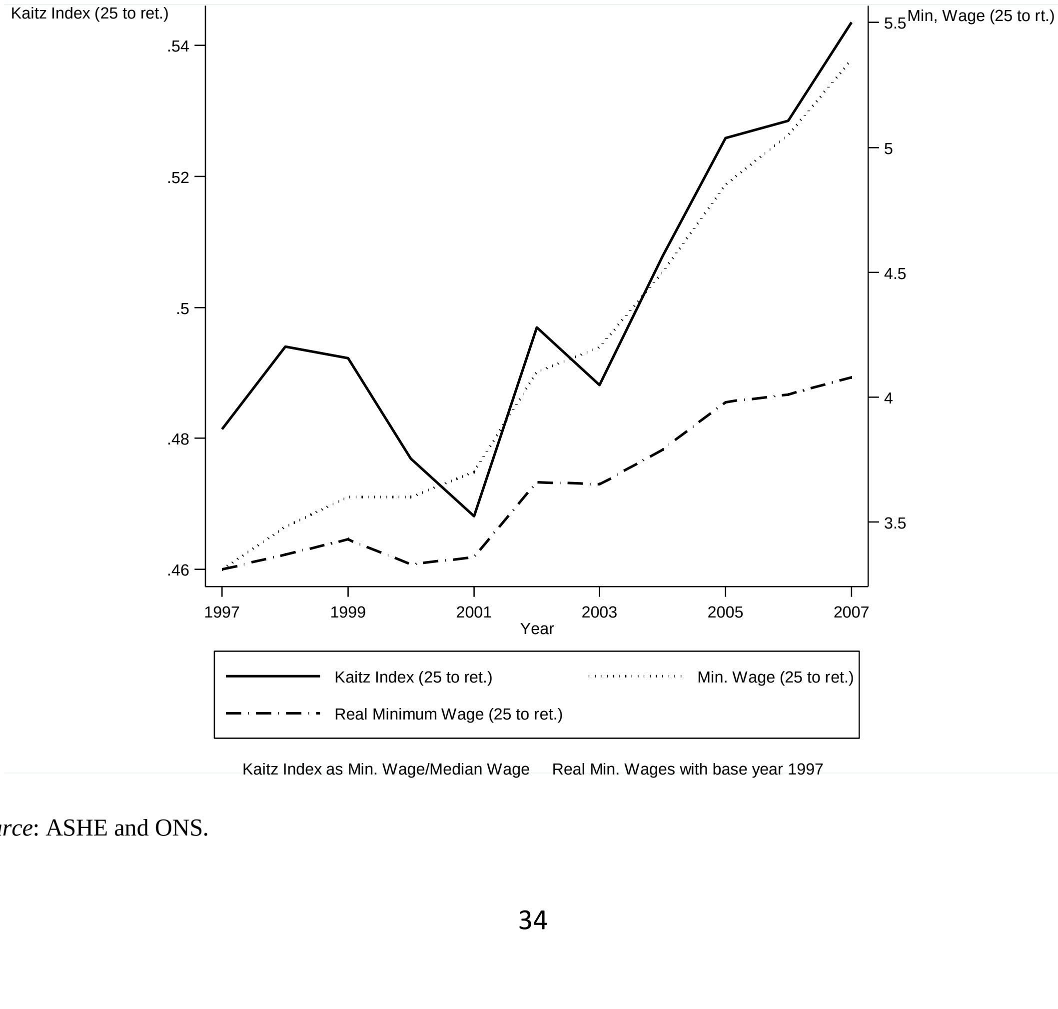
In relation to our findings on inequality it is clear, as one might expect, that raising the NMW is associated with reduced lower tail wage inequality in a systematic way each year since its introduction.

Figure 2-1. Change in the Nominal Hourly Wage Level of the adult rate of the NMW



Source: Low Pay Commission and ONS

Figure 2-2. Change in Estimated NMW & Kaitz Index Over Time, 1997-2007



Source: ASHE and ONS.

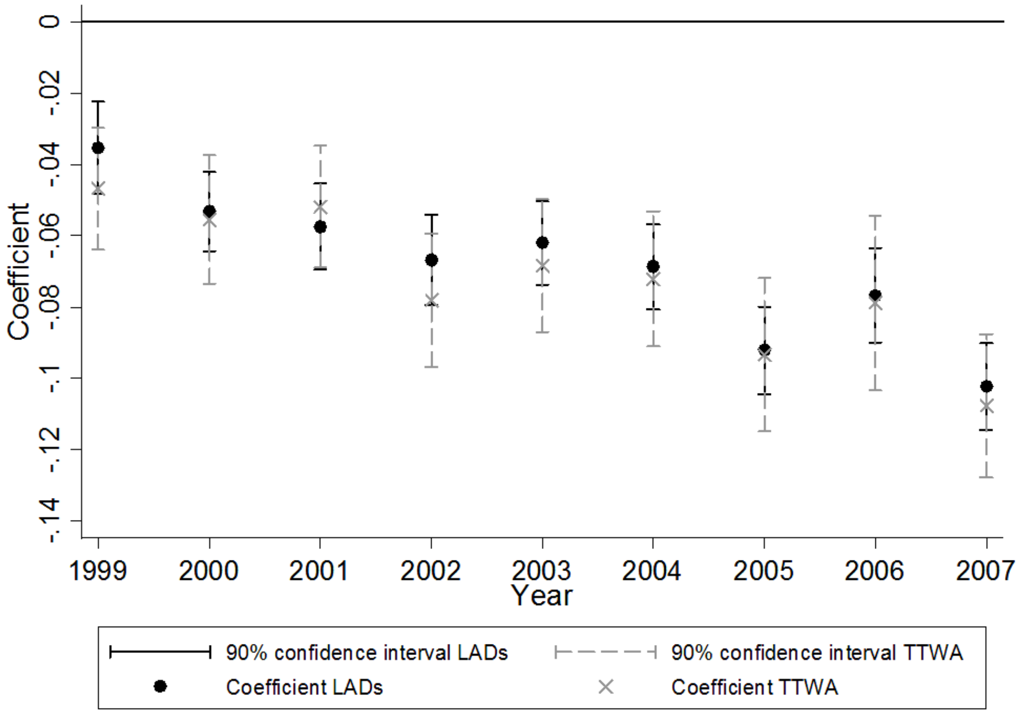
Figure 2-3. Geographical Variation in the Minimum Wage Share (persons of working age)

Level 1999-2001

Change 1999-2007

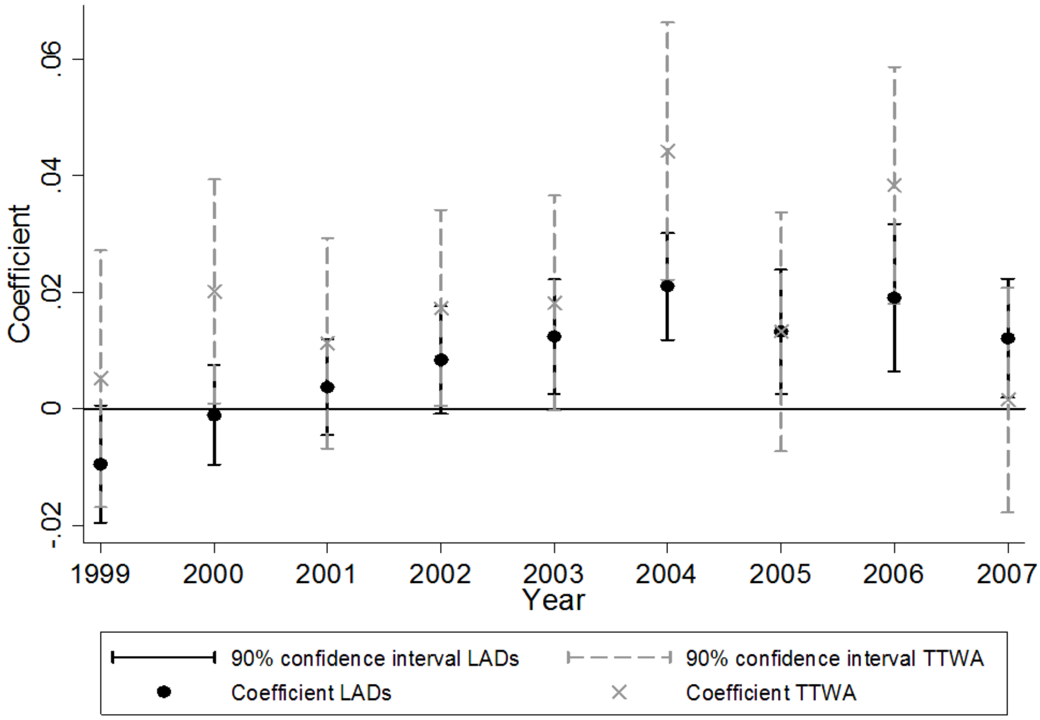
Source: ASHE

Figure 2-4. Incremental Difference-in-Differences wage inequality estimates, age 16-retirement.



Source: ASHE and LFS. Authors' calculations.

Figure 2-5. Incremental Difference-in-Differences employment estimates, age 16-retirement.



Source: ASHE and LFS. Authors' calculations.

Table 2-1. Incremental Difference-in-Differences Wage Inequality Estimates

	Proportion paid at or below NMW				*Proportion Paid at NMW*			
	Total	*Total*	*Total*	*Total*	*Total*	*Total*	*Total*	*Total*
	16-ret.	*16-ret.*	*16-ret.*	*16-ret.*	*16-ret.*	*16-ret.*	*16-ret.*	*16-ret.*
	406 areas	*140 areas*	*406 areas*	*140 areas*	*406 areas*	*140 areas*	*406 areas*	*140 areas*
	50-5		*50-10*		*50-5*		*50-10*	
NMW	0.092***	0.095***	0.054**	0.060**	-0.002	0.004**	-0.001	0.003*
Base Years	(0.006)	(0.010)	(0.005)	(0.007)	(0.006)	(0.002)	(0.005)	(0.002)
NMW*1999	-0.035***	-0.052***	-0.009	-0.007	0.007	-0.006	0.014**	0.008*
	(0.008)	(0.012)	(0.006)	(0.004)	(0.007)	(0.004)	(0.006)	(0.004)
NMW*2000	-0.053***	-0.050***	-0.027***	0.028***	0.006	0.005	0.008	0.005
	(0.007)	(0.010)	(0.006)	(0.009)	(0.007)	(0.004)	(0.006)	(0.005)
NMW*2001	-0.057***	-0.061***	-0.031***	-0.043***	0.002	-0.011***	0.004	-0.011***
	(0.007)	(0.011)	(0.006)	(0.009)	(0.008)	(0.004)	(0.006)	(0.004)
NMW*2002	-0.067***	-0.079***	-0.032***	-0.048***	-0.011	-0.014***	-0.003	-0.009**
	(0.008)	(0.012)	(0.006)	(0.010)	(0.007)	(0.005)	(0.006)	(0.005)
NMW*2003	-0.062***	-0.064***	-0.036***	-0.041***	0.001	-0.011**	0.001	-0.014***
	(0.007)	(0.011)	(0.006)	(0.009)	(0.008)	(0.004)	(0.007)	(0.004)
NMW*2004	-0.069***	-0.073***	-0.043***	-0.049***	-0.005	-0.017***	(0.004)	-0.011***
	(0.007)	(0.011)	(0.005)	(0.008)	(0.007)	(0.004)	(0.006)	(0.004)
NMW*2005	-0.092***	-0.094***	-0.055***	-0.053***	-0.015**	-0.017**	-0.009	0.009*
	(0.007)	(0.012)	(0.006)	(0.009)	(0.017)	(0.006)	(0.006)	(0.005)
NMW*2006	-0.077***	-0.097***	-0.047***	-0.056***	-0.009	-0.025***	-0.007	-0.017***
	(0.007)	(0.012)	(0.006)	(0.010)	(0.007)	(0.006)	(0.006)	(0.006)
NMW*2007	-0.102***	-0.116***	-0.064***	-0.077***	-0.028***	-0.036***	-0.021***	-0.027***
	(0.007)	(0.013)	(0.006)	(0.011)	(0.007)	(0.008)	(0.006)	(0.007)

Notes: All regressions contain year, area effects + controls (education, age, gender). HAC robust fixed effect estimates in brackets. The base year are 1997-1998.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 2-2. Employment Estimates of the NMW over the Medium Term, 1997-2007

	Proportion paid at or below NMW				*Proportion paid at NMW*				*Kaitz Index*			
Total 16-ret.	-0.021***	-0.020***	0.001	0.001	-0.012***	-0.012***	0.001	0.001	0.013	0.015	-0.021	-0.012
406 areas	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.012)	(0.013)	(0.023)	(0.024)
Total 16-ret.	-0.039***	-0.043***	0.009*	0.008*	-0.024***	-0.030***	-0.001	0.002	-0.109***	-0.150***	0.031	0.035
140 areas	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.021)	(0.024)	(0.048)	(0.030)
Adult 25-ret.	-0.023***	-0.022***	-0.001	0.001	-0.013***	-0.016***	0.001	0.001	-0.014	-0.026***	-0.008	-0.006
406 areas	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.010)	(0.011)	(0.022)	(0.023)
Adult 25-ret	-0.038***	-0.042***	0.003	0.002	-0.026***	-0.034***	-0.002	0.003	-0.102***	-0.151***	0.066	0.047
140 areas	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.018)	(0.020)	(0.041)	(0.042)
Year Effects	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Area Effects	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Controls	N	N	N	Y	N	N	N	Y	N	N	N	Y

Notes: see Table 2-1.

Table 2-3. Incremental Difference-in-Differences Employment Estimates

	Proportion paid at or below NMW				*Proportion Paid at NMW*				*Kaitz Index*			
	Total	*Total*	*Adult*	*Adult*	*Total*	*Total*	*Adult*	*Adult*	*Total*	*Total*	*Adult*	*Adult*
	16-ret.	*16-ret.*	*25-ret*	*25-ret*	*16-ret.*	*16-ret.*	*25-ret*	*25-ret*	*16-ret.*	*16-ret.*	*25-ret*	*25-ret*
	406 areas	*140 areas*	*406 areas*	*140 areas*	*406 areas*	*140 areas*	*406 areas*	*140 areas*	*406 areas*	*140 areas*	*406 areas*	*140 areas*
NMW	-0.006*	-0.002	-0.004	-0.006	0.009	-0.001	0.012*	-0.001	-0.041	-0.034	-0.032	-0.009
Base year	(0.003)	(0.007)	(0.003)	(0.006)	(0.006)	(0.002)	(0.007)	(0.002)	(0.026)	(0.050)	(0.025)	(0.045)
NMW*1999	-0.009	-0.011	-0.006	-0.006	-0.025***	-0.021***	-0.023***	-0.013**	-0.029	0.023	-0.009	0.023
	(0.006)	(0.011)	(0.006)	(0.011)	(0.007)	(0.006)	(0.007)	(0.006)	(0.022)	(0.040)	(0.032)	(0.036)
NMW*2000	-0.001	0.002	0.002	0.008	-0.013**	-0.007	-0.014*	-0.006	0.02	0.078**	0.022	0.090***
	(0.005)	(0.010)	(0.005)	(0.010)	(0.007)	(0.005)	(0.008)	(0.006)	(0.021)	(0.038)	(0.020)	(0.034)
NMW*2001	0.004	0.002	0.003	-0.001	-0.009	-0.017***	-0.008	-0.013**	0.01	0.038	0.006	0.035
	(0.005)	(0.010)	(0.005)	(0.010)	(0.007)	(0.005)	(0.007)	(0.005)	(0.019)	(0.042)	(0.018)	(0.037)
NMW*2002	0.008	0.002	0.007	0.001	-0.010	-0.007	-0.009	-0.004	0.048**	0.068*	0.048**	0.036
	(0.006)	(0.010)	(0.006)	(0.010)	(0.007)	(0.005)	(0.008)	(0.005)	(0.020)	(0.035)	(0.021)	(0.034)
NMW*2003	0.012**	0.01	0.007	0.013	-0.008	0.004	-0.013*	0.005	0.074***	0.184***	0.054**	0.128***
	(0.006)	(0.012)	(0.006)	(0.011)	(0.007)	(0.006)	(0.008)	(0.006)	(0.024)	(0.044)	(0.022)	(0.039)
NMW*2004	0.021***	0.026***	0.012**	0.021**	-0.003	0.008	-0.011	0.003	0.078***	0.115***	0.050**	0.079**
	(0.006)	(0.010)	(0.006)	(0.009)	(0.007)	(0.006)	(0.008)	(0.006)	(0.025)	(0.044)	(0.022)	(0.037)
NMW*2005	0.013**	0.023**	0.006	0.017*	-0.004	0.013***	-0.004	0.008	0.072***	0.132***	0.031	0.067**
	(0.006)	(0.011)	(0.006)	(0.010)	(0.007)	(0.006)	(0.007)	(0.005)	(0.028)	(0.036)	(0.023)	(0.032)
NMW*2006	0.019**	0.033***	0.013*	0.023**	-0.001	0.011*	-0.004	0.006	0.077**	0.177***	0.063**	0.142***
	(0.008)	(0.011)	(0.007)	(0.010)	(0.008)	(0.006)	(0.009)	(0.007)	(0.031)	(0.036)	(0.028)	(0.035)
NMW*2007	0.012*	0.020*	0.005	0.012	-0.003	0.011	-0.008	0.002	0.058**	0.143***	0.049**	0.116***
	(0.006)	(0.011)	(0.006)	(0.009)	(0.007)	(0.008)	(0.009)	(0.007)	(0.026)	(0.048)	(0.024)	(0.042)

Notes: see Table 2-1.

Table 2-4. Employment Robustness Checks, 406 areas, total (16-ret.)

	Proportion paid at or below NMW				*Proportion paid at NMW*				*Kaitz Index*			
	Original	*5p*	*10p*	*Cell size*	*Original*	*5p*	*10p*	*Cell size*	*Original*	*5p*	*10p*	*Cell size*
NMW	-0.006*	-0.005	-0.006	-0.009**	0.009	-0.002	-0.006**	0.004	-0.041	-0.041	-0.042	-0.037
Base year	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.007)	(0.026)	(0.026)	(0.026)	(0.028)
NMW*1999	-0.009	-0.011*	-0.010	-0.011*	-0.025***	-0.010**	-0.007	-0.019**	-0.029	-0.029	-0.029	-0.035*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.008)	(0.022)	(0.022)	(0.022)	(0.021)
NMW*2000	-0.001	-0.001	-0.002	0.001	-0.013**	-0.001	-0.001	-0.01	0.02	0.021	0.021	0.027
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.004)	(0.004)	(0.008)	(0.021)	(0.021)	(0.021)	(0.022)
NMW*2001	0.004	0.003	0.001	-0.001	-0.009	0.003	-0.002	-0.01	0.01	0.01	0.01	0.01
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.005)	(0.004)	(0.008)	(0.019)	(0.019)	(0.019)	(0.017)
NMW*2002	0.008	0.006	0.007	0.010	-0.01	0.006	0.005	-0.002	0.048**	0.048**	0.049**	0.058**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.004)	(0.009)	(0.020)	(0.021)	(0.021)	(0.024)
NMW*2003	0.012***	0.012**	0.015**	0.001	-0.008	0.006	0.010**	-0.013	0.074***	0.074***	0.074***	0.007
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.005)	(0.004)	(0.010)	(0.024)	(0.024)	(0.024)	(0.036)
NMW*2004	0.021***	0.021***	0.023***	0.021***	-0.003	0.013**	0.019***	-0.001	0.078***	0.078***	0.079***	0.065**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.009)	(0.025)	(0.025)	(0.025)	(0.029)
NMW*2005	0.013**	0.012*	0.013*	0.021**	-0.004	0.007	0.010*	0.004	0.072***	0.073***	0.073**	0.097*
	(0.006)	(0.007)	(0.007)	(0.010)	(0.007)	(0.006)	(0.005)	(0.009)	(0.028)	(0.028)	(0.028)	(0.050)
NMW*2006	0.019**	0.023***	0.021***	0.028***	-0.001	0.018***	0.019***	0.008	0.077**	0.077**	0.078**	0.100***
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)	(0.008)	(0.031)	(0.032)	(0.032)	(0.037)
NMW*2007	0.012*	0.012*	0.015**	0.012	-0.003	0.010*	0.014**	-0.002	0.058**	0.058**	0.059**	0.077
	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.005)	(0.009)	(0.026)	(0.026)	(0.027)	(0.050)

Notes: see Table 2-1.

Appendix 2.A

Definition of key variables

Employment rate

Total number of employees, self-employed, unpaid family workers and participants in government-supported training and employment programs in working age as a proportion of people in working age in each local area.

This variable has been generated also for adult workers (25 to retirement age).

Data on employment used in this chapter is taken from June to August of each year.

Source: Labour Force Survey. Residence based analysis.

Wage Inequality:

In this study two different measures of wage inequality are used:

- The median wage divided by the 5th percentile of the wage distribution in each local area
- The median wage divided by the 10th percentile of the wage distribution in each local area.

This variable has been computed also for adult workers (25 to retirement age).

Source: ASHE, data recorded in April of each year. Workplace based analysis.

Minimum wage shares

Proportion of workers paid at or below the minimum wage in each local area.

The shares are generated for two age bands in each local area:

- 16 to retirement age

Starting from 1999, the shares are a weighted average of the minimum wage shares of persons from 18 to 21 years and of persons from 22 to retirement age.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the shares are a weighted average of the minimum wage shares of persons of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to retirement age.

- 25 to retirement age

Source: ASHE, data recorded in April of each year. Workplace based analysis.

Spike of the minimum wage

Proportion of workers paid at the minimum wage in each local area.

The spikes are generated for two age bands in each local area:

- 16 to retirement age

Starting from 1999, the spike is a weighted average of the spike of persons from 18 to 21 years and of persons from 22 to retirement age.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the spike is a weighted average of the spike of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to retirement age.

- 25 to retirement age

Source: ASHE, data recorded in April of each year. Workplace based analysis.

Kaitz Index

Kaitz Index, generated as the ratio of the NMW to the median hourly wage in each local area.

The Kaitz index is generated for two age bands in each local area:

- 16 to retirement age

Starting from 1999, the shares are a weighted average of the minimum wage shares of persons from 18 to 21 years and of persons from 22 to retirement age.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the shares are a weighted average of the minimum wage shares of persons of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to retirement age.

- 25 to retirement age

Source: ASHE, data recorded in April of each year. Workplace based analysis.

Appendix 2.B

ASHE Dataset

Even if ASHE is considered to give reliable wage figures through payroll records and it has a relatively large sample size, there are some limitations of this dataset which affect this study.

a) Possible measures of hourly earnings

The Low Pay Commission recommended construction of the hourly pay variable on the ASHE data involves dividing gross pay (excluding overtime, shift and premium payments) by basic paid hours. This variable closely matches the definition of NMW. However, the variable is available in the panel only from 2000. It is therefore necessary to use another measure of hourly earnings in this study which covers the period 1997 to 2007.

The variable used is a "basic hourly wage rate", defined as gross weekly earnings excluding overtime, and divided by normal basic hours. As a result this variable will be slightly larger than the true hourly wage and the measurement error will tend to be larger, the higher shift and premium payments are. This might therefore result in an under-statement of the number of low paid workers.

b) Discontinuities in ASHE dataset across years

Time series analysis has been complicated when the ASHE replaced the NES in 2004 and also by several changes in the ASHE methodology from 2004 to 2007.

First of all, the coverage of employees for the ASHE is greater than that of the NES. The NES surveys employees taken from HM Revenue & Customs PAYE record, excluding the majority of those whose weekly earnings fall below the PAYE deduction threshold. Moreover, this survey does not cover employees between sample selection for a particular year and the survey reference week in April. Thus, mobile workers who have changed or started new jobs between the drawing of the sample and the reference week are excluded. In conclusion, NES understate the proportion on NMW as it does not record the earnings of many low paid workers, especially part-time and mobile workers. In 2004, ASHE survey was introduced to improve on the representation of the low paid: it improved coverage of employees including mobile workers who have either changed or started new jobs between sample selection and the survey reference in April. Also, the sample was enlarged by including some of the employees outside the PAYE system.

In 2005 a new questionnaire was introduced. In particular, the definition of incentive/bonus pay changed to only include payments that were paid and earned in April. Also, a new question including "pay for other reasons" was introduced. This implies respondents might include earnings information which was not collected in the past. Even if results for 2004 have been reworked to exclude irregular bonus/incentive payments and to allow for this missing pay, results from 1997 to 2003 remain inconsistent with the ones from 2004 onwards.

Given that the main source of information on hourly pay in this study includes shift and premium payments and from 2004 "pay for other reasons", estimations of measures of minimum wage and wage inequality might be affected by this discontinuity, with an increase of the average measurement error and the dispersion in the measurement error from 2004 onwards.

Finally, in 2007 the sample size of ASHE was reduced by 20%. ASHE results for 2007 are based on approximately 142,000 returns, down from 175,000 in 2006. The largest sample cuts occurred principally in industries where earnings are least variable, affecting the randomness of the sample.

Consistent series which takes into account of the identified changes has been produced going back from 2006 to 2004 and from 2007 to 2006. For 2004 results are also available that exclude supplementary information, to be comparable with the back series generated by imputation and weighting of the 1997 to 2003 NES data. It is not possible to get consistent datasets for the entire period of this study (1997-2007).

Appendix 2.C

Robustness checks

Table C 2-1.Employment Estimates of the NMW over the Medium Term, 1997-2007. Comparison of results with area fixed effects and in first differences.

	Proportion paid at or below NMW		*Proportion paid at the NMW*		*Kaitz Index*	
	Fixed effects	*Differences*	*Fixed effects*	*Differences*	*Fixed effects*	*Differences*
Total 16-ret	0.001	0.001	0.001	0.001	-0.012	0.004
406 areas	(0.002)	(0.002)	(0.002)	(0.002)	(0.024)	(0.026)
Total 16-ret	0.008*	0.004	0.002	-0.003	0.035	0.005
140 areas	(0.005)	(0.004)	(0.002)	(0.002)	(0.030)	(0.055)
Adult 25-ret	0.001	-0.001	0.001	-0.001	-0.006	-0.009
406 areas	(0.002)	(0.002)	(0.002)	(0.002)	(0.023)	(0.023)
Adult 25-ret	0.002	0.003	0.003	-0.003	0.047	0.011
140 areas	(0.004)	(0.004)	(0.002)	(0.002)	(0.042)	(0.048)
Years Effects	Y	Y	Y	Y	Y	Y
Areas Effects	Y	N	Y	N	Y	N
Controls	Y	Y	Y	Y	Y	Y

Notes: see Table 2-1.

Table C 2-2. Within Group Estimates of Dynamic Specifications of Minimum Wage Effects on Employment Rate (16 years to retirement age), 406 areas.

	Proportion at or below the NMW				*Proportion at the NMW*				*Kaitz Index*			
Independent Variables	*(1)*	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*
Proportion paid at or below the NMW t			0.001	0.002								
			(0.002)	(0.002)								
Proportion paid at or below the NMW t-1	0.002	0.002	0.002	0.002								
	(0.002)	(0.002)	(0.002)	(0.002)								
Proportion paid at the NMW t							0.001	0.001				
							(0.002)	(0.002)				
Proportion paid at the NMW t-1					0.002	0.002	0.002	0.002				
					(0.002)	(0.002)	(0.002)	(0.002)				
Kaitz Index t											-0.014	-0.003
											(0.027)	(0.028)
Kaitz Index t-1									-0.015	-0.011	-0.011	‘-0.010
									(0.025)	(0.025)	(0.026)	(0.025)
Years Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Areas Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060
R-squared	0.013	0.027	0.013	0.027	0.013	0.027	0.013	0.027	0.013	0.027	0.013	0.027

Notes: see Table 2-1.

Table C 2-3. Difference-in-Differences year by year, Employment Estimates.

	Proportion paid at or below NMW		*Proportion paid at NMW*		*Kaitz Index*	
	Total	*Total*	*Total*	*Total*	*Total*	*Total*
	16- ret, 406	*16- ret, 140*	*16- ret, 406*	*16- ret, 140*	*16- ret, 406*	*16- ret, 140*
1997-1998						
NMW*1998	-0.002	0.001	0.012	0.004	-0.002	‘-0.010
	(0.005)	(0.010)	(0.008)	(0.003)	(0.018)	(0.035)
1998-1999						
NMW*1999	-0.007	-0.015	-0.025**	-0.014**	-0.034	0.008
	(0.007)	(0.011)	(0.012)	(0.007)	(0.021)	(0.035)
1999-2000						
NMW*2000	0.003	-0.003	0.010*	0.015**	0.051**	0.081*
	(0.008)	(0.015)	(0.006)	(0.006)	(0.025)	(0.042)
2000-2001						
NMW*2001	0.003	-0.005	0.003	-0.005	-0.004	-0.049
	(0.006)	(0.014)	(0.006)	(0.008)	(0.023)	(0.048)
2001-2002						
NMW*2002	0.011	0.006	-0.002	0.016**	0.040*	0.038
	(0.007)	(0.012)	(0.006)	(0.007)	(0.021)	(0.040)
2002-2003						
NMW*2003	0.011	0.021	0.005	0.011	0.029	0.115**
	(0.008)	(0.015)	(0.007)	(0.007)	(0.027)	(0.047)
2003-2004						
NMW*2004	0.008	-0.001	0.003	0.001	0.006	-0.077
	(0.008)	(0.012)	(0.006)	(0.009)	(0.026)	(0.053)
2004-2005						
NMW*2005	-0.006	0.009	0.001	0.007	-0.005	0.023
	(0.008)	(0.014)	(0.006)	(0.009)	(0.032)	(0.045)
2005-2006						
NMW*2006	0.007	0.011	0.009	0.003	0.002	0.030
	(0.009)	(0.013)	(0.007)	(0.008)	(0.041)	(0.037)
2006-2007						
NMW*2007	-0.009	-0.013	-0.006	-0.006	-0.027	-0.043
	(0.010)	(0.010)	(0.009)	(0.007)	(0.039)	(0.038)

Notes: see Table 2-1.

Table C 2-4. Incremental Difference-in-Differences, Employment Estimates: using TTWAs.

	Proportion paid at or below NMW			*Proportion paid at the NMW*			*Kaitz index*		
	Total	Total	Total	Total	Total	Total	Total	Total	Total
	16 to rt.	16 to rt.	16 to rt.	16 to rt.	16 to rt.	16 to rt.	16 to rt.	16 to rt.	16 to rt.
	406 areas	140 areas	TTWA	406 areas	140 areas	TTWA	406 areas	140 areas	TTWA
NMW	-0.006**	-0.002	-0.014*	0.009	-0.001	-0.001	-0.041	-0.034	-0.107**
Base Year	(0.003)	(0.007)	(0.007)	(0.006)	(0.002)	(0.002)	(0.026)	(0.050)	(0.050)
NMW*1999	-0.009	-0.011	0.005	-0.025***	-0.021***	-0.018***	-0.029	0.023	0.007
	(0.006)	(0.011)	(0.013)	(0.007)	(0.007)	(0.007)	(0.022)	(0.040)	(0.048)
NMW*2000	-0.001	-0.002	0.020*	-0.013**	-0.007	-0.002	00:02	0.078**	0.037
	(0.005)	(0.010)	(0.012)	(0.007)	(0.005)	(0.005)	(0.021)	(0.038)	(0.044)
NMW*2001	0.004	0.002	0.011	-0.009	-0.017***	-0.005	0.010	0.038	-0.017
	(0.005)	(0.010)	(0.011)	(0.006)	(0.005)	(0.004)	(0.019)	(0.042)	(0.036)
NMW*2002	0.008	0.002	0.017*	-0.010	-0.007	-0.008	0.048**	0.068*	0.030
	(0.006)	(0.010)	(0.010)	(0.007)	(0.005)	(0.005)	(0.020)	(0.035)	(0.037)
NMW*2003	0.012**	0.010	0.018	-0.008	0.004	0.010*	0.074***	0.184***	0.100**
	(0.006)	(0.012)	(0.011)	(0.007)	(0.006)	(0.006)	(0.024)	(0.044)	(0.042)
NMW*2004	0.021***	0.026***	0.044***	-0.003	0.008	0.014**	0.078***	0.115***	0.113**
	(0.006)	(0.010)	(0.013)	(0.007)	(0.006)	(0.006)	(0.025)	(0.044)	(0.049)
NMW*2005	0.013**	0.023**	0.013	-0.004	0.013***	0.017**	0.072***	0.132***	0.040
	(0.006)	(0.011)	(0.012)	(0.007)	(0.006)	(0.007)	(0.028)	(0.036)	(0.052)
NMW*2006	0.019**	0.033***	0.038***	-0.001	0.011*	0.002	0.077***	0.177***	0.004
	(0.008)	(0.011)	(0.012)	(0.008)	(0.006)	(0.009)	(0.031)	(0.036)	(0.058)
NMW*2007	0.012*	0.020*	0.002	-0.003	0.011	-0.001	0.058**	0.143***	0.045
	(0.006)	(0.011)	(0.012)	(0.007)	(0.008)	(0.008)	(0.026)	(0.048)	(0.048)

Notes: see Table 2-1.

Table C 2-5. Incremental Difference-in-Differences Employment Estimates, 406 areas: pre-period 1997 only and 1998 only.

	Proportion paid at or below the NMW			*Proportion paid at NMW*			*Kaitz Index*		
	Total (16-ret) Base years '97-98	Total (16-ret) Base year '97	Total (16-ret) Base year '98	Total (16-ret) Base years '97-98	Total (16-ret) Base year '97	Total (16-ret) Base year '98	Total (16-ret) Base years '97-98	Total (16-ret) Base year '97	Total (16-ret) Base year '98
NMW	-0.006**	-0.007*	-0.010**	0.009	0.001	0.016*	-0.041	-0.048*	-0.051*
Base year	(0.003)	(0.004)	(0.005)	(0.006)	(0.004)	(0.009)	(0.026)	(0.029)	(0.028)
NMW*1997			0.003			-0.015			0.003
			(0.006)			(0.010)			(0.022)
NMW*1998		-0.003			0.015			-0.003	
		(0.006)			(0.010)			(0.022)	
NMW* 1999	-0.009	-0.009	-0.006	-0.025***	-0.017***	-0.032***	-0.029	-0.028	-0.025
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.010)	(0.022)	(0.025)	(0.023)
NMW*2000	-0.001	0.001	0.002	-0.013**	-0.005	-0.020*	0.02	0.022	0.025
	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)	(0.010)	(0.021)	(0.025)	(0.022)
NMW*2001	0.004	0.004	0.007	-0.009	-0.001	-0.016	0.01	0.012	0.014
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.010)	(0.019)	(0.023)	(0.021)
NMW*2002	0.008	0.009	0.012*	-0.01	-0.002	-0.017	0.048**	0.050**	0.053**
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.010)	(0.020)	(0.024)	(0.022)
NMW*2003	0.012**	0.013**	0.016**	-0.008	0.001	-0.015	0.074***	0.075***	0.078***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.010)	(0.024)	(0.027)	(0.025)
NMW*2004	0.021***	0.022***	0.024***	-0.003	0.005	-0.010	0.078***	0.080***	0.082***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.010)	(0.025)	(0.028)	(0.026)
NMW*2005	0.013**	0.014**	0.016**	-0.004	0.004	-0.011	0.072***	0.073**	0.076***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.010)	(0.028)	(0.030)	(0.029)
NMW*2006	0.019**	0.020**	0.022***	-0.001	0.007	-0.008	0.077**	0.078**	0.081**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.011)	(0.031)	(0.034)	(0.032)
NMW*2007	0.012*	0.013*	0.015**	-0.003	0.005	-0.010	0.058**	0.059**	0.062**
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.010)	(0.026)	(0.029)	(0.028)

Notes: see Table 2-1.

Table C 2-6. Incremental Difference-in-Differences Employment Estimates: 95 areas regressions results, pre-period 1993-1997.

	Proportion paid at or below the NMW			*Proportion paid at NMW*			*Kaitz Index*		
Total (16-ret)	*406 areas*	*140 areas*	*95 areas*	*406 areas*	*140 areas*	*95 areas*	*406 areas*	*140 areas*	*95 areas*
	Base '97-98	*Base '97-98*	*Base '93-97*	*Base '97-98*	*Base '97-98*	*Base '93-97*	*Base '97-98*	*Base '97-98*	*Base '93-97*
NMW	-0.006**	-0.002	0.001	0.009	-0.001	-0.001	-0.041	-0.034	0.050
Base year	(0.003)	(0.007)	(0.003)	(0.006)	(0.002)	(0.001)	(0.026)	(0.050)	(0.035)
NMW* 1999	-0.009	-0.011	-0.031***	-0.025***	-0.021***	-0.010**	-0.029	0.023	-0.092***
	(0.006)	(0.011)	(0.007)	(0.007)	(0.007)	(0.005)	(0.022)	(0.040)	(0.025)
NMW*2000	-0.001	-0.002	-0.008	-0.013**	-0.007	-0.010**	00:02	0.078**	0.004
	(0.005)	(0.010)	(0.006)	(0.007)	(0.005)	(0.005)	(0.021)	(0.038)	(0.028)
NMW*2001	0.004	0.002	-0.009*	-0.009	-0.017***	0.002	0.010	0.038	-0.032
	(0.005)	(0.010)	(0.005)	(0.006)	(0.005)	(0.004)	(0.019)	(0.042)	(0.021)
NMW*2002	0.008	0.002	-0.012	-0.010	-0.007	-0.004	0.048**	0.068*	0.001
	(0.006)	(0.010)	(0.008)	(0.007)	(0.005)	(0.005)	(0.020)	(0.035)	(0.031)
NMW*2003	0.012**	0.010	0.007	-0.008	0.004	0.001	0.074***	0.184***	0.017
	(0.006)	(0.012)	(0.008)	(0.007)	(0.006)	(0.005)	(0.024)	(0.044)	(0.033)
NMW*2004	0.021***	0.026***	0.014**	-0.003	0.008	0.013**	0.078***	0.115***	0.055
	(0.006)	(0.010)	(0.007)	(0.007)	(0.006)	(0.005)	(0.025)	(0.044)	(0.035)
NMW*2005	0.013**	0.023**	0.011	-0.004	0.013***	0.010	0.072***	0.132***	0.059
	(0.006)	(0.011)	(0.010)	(0.007)	(0.006)	(0.008)	(0.028)	(0.036)	(0.048)
NMW*2006	0.019**	0.033***	0.023**	-0.001	0.011*	0.021***	0.077***	0.177***	0.074*
	(0.008)	(0.011)	(0.009)	(0.008)	(0.006)	(0.009)	(0.031)	(0.036)	(0.045)
NMW*2007	0.012*	0.020*	0.007	-0.003	0.011	0.011	0.058**	0.143***	0.035
	(0.006)	(0.011)	(0.009)	(0.007)	(0.008)	(0.009)	(0.026)	(0.048)	(0.043)

Notes: see Table 2-1.

3. The UK National Minimum Wage in Retrospect, looking at Unemployment and Hours of Work

3.1 Introduction

This third chapter is an extension of chapter 2. We apply again our "Incremental Difference-in-Differences" (IDiD) estimator to look at the effects of the National Minimum Wage (NMW) in each year through its differential impact across local labour markets. In particular, here we examine the association of the NMW on a broader range of labour market measures other than employment and wage inequality, such as unemployment and working hours.

Various recent papers focus on the employment impacts of the introduction of the NMW and its initial upratings, as summarised in Metcalf (2008). These studies suggest that the NMW has had a limited, if any, adverse impact on employment (see for example, Stewart (2002, 2004a, 2004b), Dickens and Draca (2005)). There are also studies that find small positive impact on employment as our previous chapter suggests (see also for example Dickens, Machin and Manning (1999)). Since labour adjustments due to the NMW may take place either at the extensive margin or at the intensive margin[19], looking at how changes in the local area NMW incidence are related to changes in the unemployment rate and average working hours in the locality makes sense.

Moreover, the UK literature generally investigates the impact of the NMW on employment, however, it is generally silent on its impact on unemployment. We find that it is worth to look also at this labour market outcome as a further robustness check for our employment results. Also the effects on unemployment might differ from the effects on employment for several reasons. For example, if we suppose that the NMW causes job losses, some of those who leave the job because of the policy might feel discouraged and become inactive, thus leaving the labour force. These will no longer accounted as unemployed. Furthermore, the NMW could induce an increase in labour supply if additional individuals enter the labour force to search for the now more attractive jobs, this will lead unemployment to increase.

[19] "Extensive" margin refers to the number of inputs that are used. For example, hiring an additional worker would increase an extensive margin. "Intensive" margin refers to the quantity of use extracted within a given extensive margin. For example, treducing production from a given group of workers would diminish the intensive margin.

There are only few papers that investigate the impact of the NMW on hours of work using UK data. First of all, Connolly and Gregory (2002) employ a Difference-in-Differences technique to evaluate the effect of the UK NMW on hours worked by full- and part-time women (who are more likely to be affected by the NMW) for the first three years of NMW existence. They find no significant changes in hours worked by either full- or part-time women. Stewart and Swaffield (2008) also estimate the impact of the introduction of the NMW on the working hours of low-wage employees using Difference-in-Differences estimators. Their estimates suggest that the introduction of the NMW reduced paid working hours of both male and female low-wage workers significantly. For example, using the New Earnings Survey (NES) their estimates of the total effects (ie. initial plus lagged effects) indicate a reduction of between one and two hours per week in basic hours for both men and women, and similarly for total paid hours. More recently, Dickens et al (2009) also looked at the impact of the NMW on working hours testing different methods of analysis such as individual level Difference-in-Differences analysis and aggregate level analysis, exploiting the variation in the pay distribution across different geographical areas. They find little evidence of any effect of the NMW on either basic or total hours.

As already pointed out in the previous chapter, only a few studies evaluate the impact of the NMW by exploiting geographical variation in local or regional labour markets (see Card (1992) and Neumark and Wascher (1992) for the US and Stewart (2002) for the UK). The longstanding geographic variation in wage rates across the UK has consequences for the "bite" of the NMW in different areas. Stewart (2002) points out how the NMW reaches further up the wage distribution in certain parts of the country than in others. This chapter builds on that small literature by examining the impact of the NMW in the UK over the period 1997-2007, comparing the period of two years before its introduction with the subsequent history of the NMW and its up-ratings.

Our additional insight, as in the previous chapter, is to differentiate between a period in which there was no NMW policy and the incremental up-rating of the NMW each year, now afforded by the longer run of data available and to extend the analysis to a broader range of potential channels through which the NMW may have had effects.

Our results suggest that over the medium term, unemployment fell in areas where the NMW had the strongest "bite" during the second half of the sample period. The results on hours of work suggest that hours worked by part-timers grew more in areas more affected by the NMW. When we consider the effect on full-time workers, it would appear that there are no significant effects. However, causal interpretation of the results might be compromised by concomitant policy interventions over the sample period. The simultaneous presence of these policies may have effects that are also correlated with changes in the local "bite" of the NMW.

In the interests of clarity and brevity, the methodology and the issues linked to it are already described in chapter 2. Here, we simply attempt to summarise the main conclusions from our IDiD regression estimates of unemployment and working hours in what follows. Section 3.2 describes the data. Section 3.3 examines the unemployment effects and Section 3.4 considers the effects on hours of work. Section 3.5 presents some robustness checks. Section 3.6 concludes.

3.2 Data

The central idea is to see whether geographic variation in the "bite" of the NMW is associated with geographic variation in indicators of local market performance. Chapter 2 extensively describes the data employed in this analysis. In this section we will therefore briefly focus only on the description of the labour market outcomes that we analyse in this chapter, namely: hours of work and unemployment. In particular, here we focus on average total working hours for part-time and full-time workers separately. Data on working hours is drawn from the Annual Survey of Hours and Earnings (ASHE). The ASHE dataset has the advantage of providing relatively accurate data on hours, being an employer based dataset. However, the ASHE has also a potential drawback as most of the employees earning below the PAYE threshold are excluded from the survey especially in the years before 2004. This could affect particularly part-time workers, who are more likely to earn the NMW. However, from 2004 onwards the ASHE sample was boosted by a sample of firms not registered for PAYE, therefore improving representation of low-paid employees, particularly those that usually work part-time and tend to earn below the PAYE threshold. Therefore, we expect that measurement error in terms of working hours more prevalent in earlier years of the sample. We use 35 basic hours of work as a threshold for part-time and full-time work. In order to have consistent data on unemployment disaggregated at local area level for the entire period of the analysis we use the claimant count data from NOMIS. The Jobseekers Allowance (JSA) claimant count records the number of people claiming JSA and National Insurance (NI) credits at Jobcentre Plus local offices. It should be beared in mind that not everyone who is unemployed is included in the claimant count data. For example, some employees (low paid in particular) that loose their jobs do not have enough NI contributions to be elegible for claimant count NI based JSA. Even if it is not an internationally agreed measure of unemployment, it is the only indicative statistic available at our levels of geographical aggregation for the time period considered in the analysis. In the analysis, the number of claimant resident in an area is measured as a percentage of population in working age resident in that area[20].

3.3 Unemployment and the NMW

Figure 3-1 shows the pattern of change that has taken place in unemployment over the period 1997 to 2007. The figure shows ranges of the claimant count rate across the 406 local authorities and districts at two points in time. In the before-NMW period, the claimant count rate was above 4% in many of the most outlying geographical areas in Scotland, Wales and the North, with those in most of the rest of the country being between 1.7% and 2.7%. However, by 2007, the unemployment rate in most of the country was below 1.6%, with just a few of the outlying geographical areas having a rate of up to 2.7% and a very few being between 3% and 4%.

Table 3-1 gives estimates of the IDiD model used in chapter 2 when the dependent variable is now the claimant unemployment rate in each local area. Together with tables of regression estimates, we also summarise our results in a graphical representation of the estimated coefficients from the underlying IDiD regression model, detailed in chapter 2. This approach facilitates a convenient comparison across years and a simple retrospective look at the effect of the NMW since 1999. Figure 3-2 shows the estimated coefficients along with the 95 per cent confidence interval for both the 406- and 140-areas levels of aggregation when the claimant count rate is regressed on the NMW share variable (the share of people earning at or below the NMW), a set of area fixed effects, time dummies and a set of within-area time-varying controls. The results are for all workers between 16 and retirement age. For all workers (age 16 to retirement), the IDiD estimates in column 1 and 2 of table 3-1, also graphed in figure 3-2, suggest that there may have been some positive association between the NMW and the unemployment rate in the earliest years of the NMW's existence. Areas where the NMW has more "bite" appear to have experienced higher unemployment growth in the early years of the

[20] The claimant count in this chapter is an average of the monthly data for the period May-September of each year. For a detailed explanation of the timing of data collection, please refer to paragraph 2.2 of chapter 2.

NMW. However, the IDiD estimates show significant negative effects in later years: unemployment rates fell more in areas more affected by the NMW after 2002[21].

3.4 Hours of work and the NMW

Our second outcome variable of interest is the level of working hours, since one intensive margin at which change in the NMW may operate is through hours. When confronted by rising costs resulting from a higher NMW, firms may try to cut back on hours, while low-wage workers may seek to compensate by working more hours if the substitution effect dominates the income effect. Thus, a changing NMW may also impact directly on the fraction of workers who move from part-time to full-time employment. Stewart and Swaffield (2004) report small cuts of around one or two hours following the introduction of the NMW for men and women. Connolly and Gregory (2002) find no hours reductions for their sample of female workers. Dickens, et al. (2009) found no evidence of a consistent impact on hours worked.

If one examines the geographical pattern of working hours it is clear that there is a substantial year-to-year shift in the fraction working part-time. This may in part be due to the sampling frame (measurement error) rather than genuine labour supply shifts. Hence, we report in the four-paneled figure 3-3 and in column 3 and 4 of table 3-1 our estimated year-on-year IDiD effects for both the 406 and the 140 geographies for part-time workers and for full-time workers. When looking at the NMW estimates on part-time hours (average total paid hours worked during the reference period, including overtime) for all workers in figure 3-3, all of the coefficients become positive and significant during the second half of the sample period, suggesting that hours worked by part-timers grew more in areas more affected by the NMW[22]. When we consider the effect on full-time workers, it would appear that there are no significant effects. Nevertheless, we suggest these results be interpreted with some caution, bearing in mind data limitations and the difficulty in modeling both hours of work and participation decisions endogenously.

[21] For example, looking at the results in column 2 of table 3-1 for 2004, results suggest that unemployment growth in 2004 was 0.69% lower in an area where 10% of employees were paid at or below the NMW compared to areas where no-one was paid the NMW compared to the respective growth rate in 1997-1998.

[22] For example, looking at the results in column 4 of table 3-1 for 2006, results suggest that average part-time hours growth in 2006 was 0.24% higher in an area where 10% of employees were paid at or below the NMW compared to areas where no-one was paid the NMW compared to the respective growth rate in 1997-1998 (pre-period).

3.5 Robustness checks

One important question to ask is how long it should take the introduction (or changes) in the NMW to have its full effects on economic indicators. From an empirical point of view, this raises the specification issue about using a lagged effect of the NMW variable in the regression. On the one hand, employers might react relatively quickly to increases in minimum wages. On the other hand, it might take time for employers to adjust factor inputs to changes in factor prices. Table 3-2 mirrors table 3-1 in this chapter, but using last year's relative minimum rather than the current year's as the regressor. The table shows that using a lagged minimum wage variable in the regression instead of the current one does not influence the results.

The local wage distribution and the NMW shares depend on local industry and labour force composition effects. For this reason, all the main results in our chapter include controls for education, age and gender, as in chapter 2. Moreover, as a robustness check we replicated our IDiD regressions controlling also for industry composition in the local area (the proportion of workers in the manufacturing sector). The results in table 3-3 are qualitatively similar to those in table 3-1 in this chapter.

3.6 Conclusions

This chapter, similarly to the previous one, summarises our estimated associations of the NMW with an additional set of measures of local labour market performance, focusing on the incremental effects of each up-rating of the NMW since 1999 up to 2007 against a base period prior to 1999 in which no NMW operated. In this chapter particularly, we estimate the effects of the NMW by looking at whether geographic variation in the "bite" of the NMW was associated with geographic variation in unemployment and hours of work.

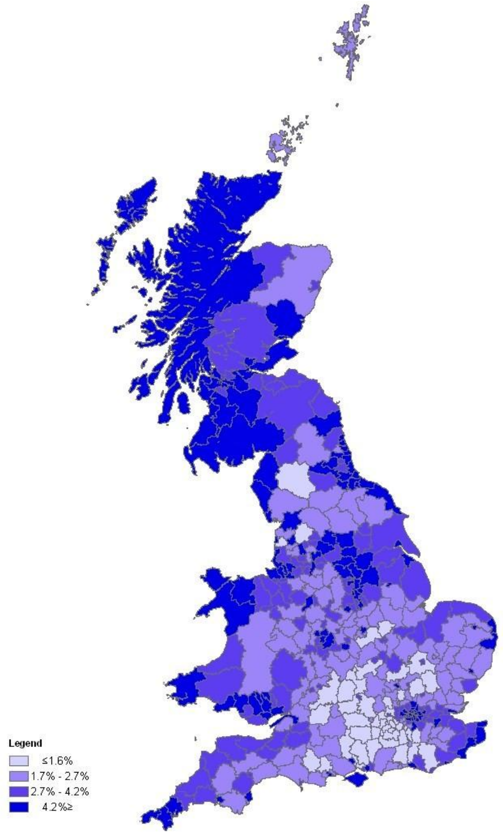
Our estimation strategy uses two sources of variation to try to identify the effect of the NMW. The first is the natural variation in how the NMW "bites" in different geographical locations, since the minimum wage is set nationally but other local wages are not. Our second source of variation is the effect of changes in the up-rating of the NMW over the years since it was introduced. This estimation is based on an IDiD method which allows us to estimate the marginal (interaction) effect of each year's change in the NMW.

As shown in chapter 2, the NMW appears to be associated with a significant narrowing of wage inequality in the bottom half of the distribution. Wage inequality is lower and has fallen further in areas where NMW bit most in the latter half of the sample period. When estimating the marginal effect of each year’s change in the NMW, we find a significant positive association between the NMW "bite" and employment in recent years. Similarly, in the present chapter, the areas where NMW bit most have experienced larger falls in unemployment, particularly in latter half of sample period. The evidence on working hours is mixed, but overall there is no compelling evidence to indicate that the NMW up-rates had an adverse affect on full-time total hours of work and they may have been associated with an increase in hours worked by part-time employees.

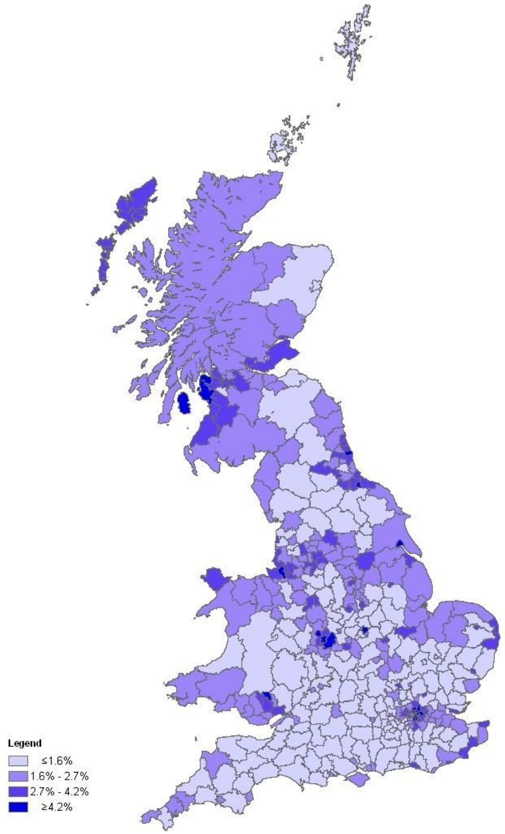
Our findings, consistent with much of the recent literature focusing on the introduction of the NMW, suggest that over the medium term, alongside a significant fall in wage inequality, employment grew (slightly) faster and unemployment fell further in areas where the NMW bit most during the latter half of the sample period. Of course there may have been other policy instruments in operation over the period and it may be that identification of a NMW effect is compromised by any correlation of these other interventions with changes in the local "bite" of the NMW. While, positive employment effects of the NMW are in line with theories where firms have some degree of monopsonistic power or the existence of other labour market frictions, they are also consistent with the idea that there may have been adjustments along margins other than employment, notably prices, profits, productivity and hours. The evidence collected in Metcalf (2008) suggests changes along all these margins in the UK. In the end all we can say that it seems unemployment did not rise over the period in which the NMW was in operation.

Figure 3-1. Claimant count (persons of working age)

1997-1998



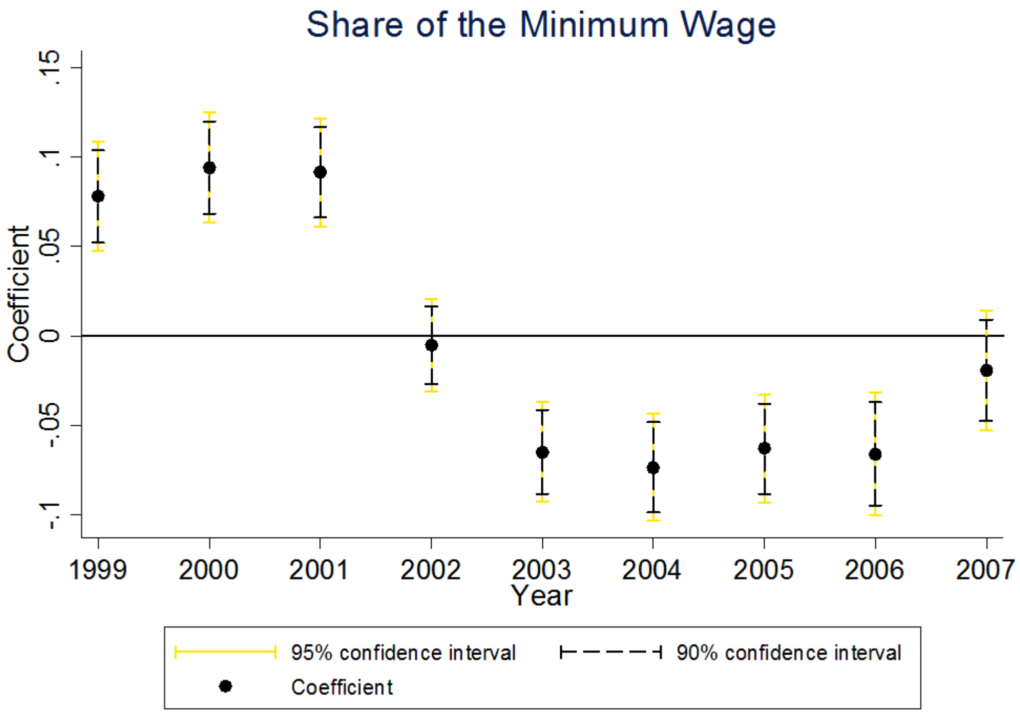
2005 – 2006 - 2007



Source: NOMIS. Authors' calculations.

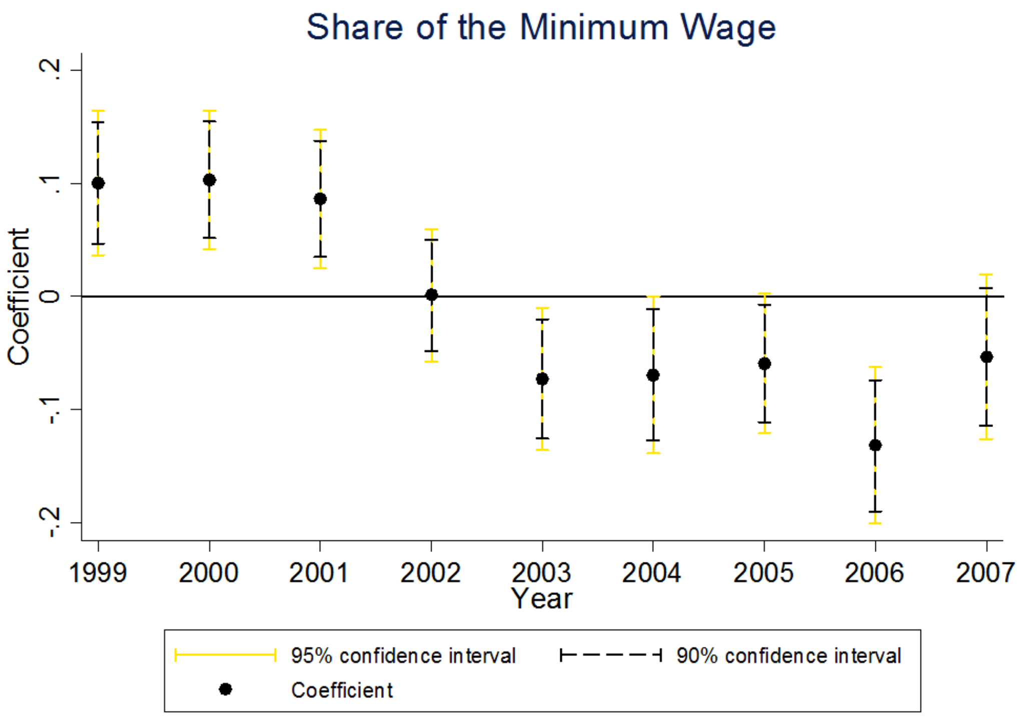
Figure 3-2. Incremental Difference-in-Differences unemployment estimates: All (16 to retirement age)

406 areas



Source: ASHE, LFS,NOMIS. Authors' calculations.

140 areas

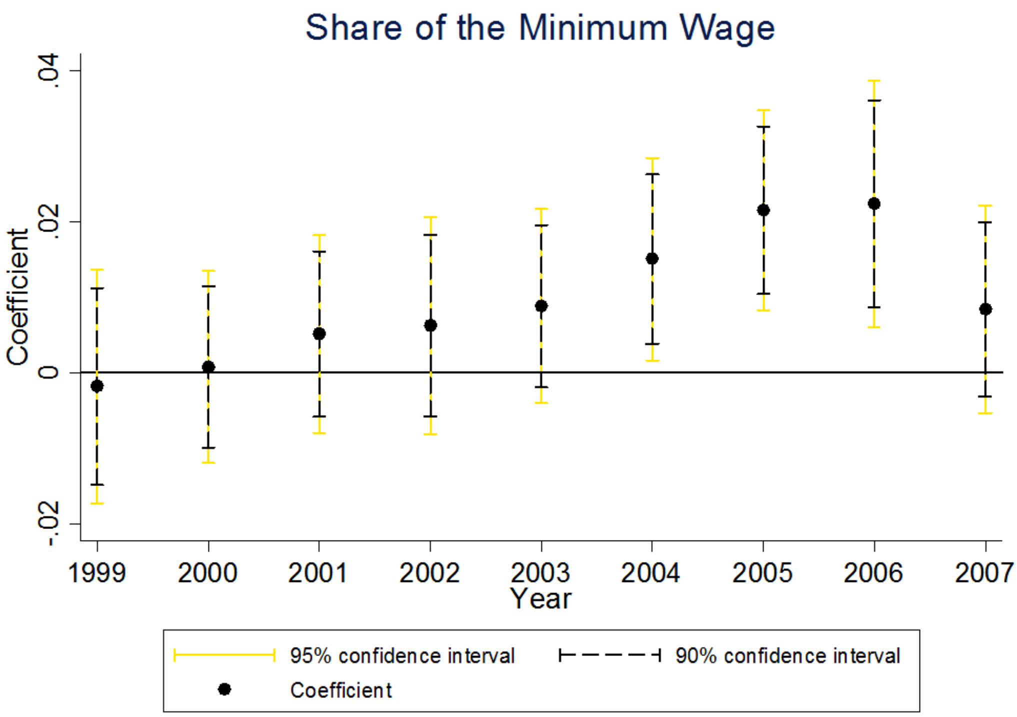


Source: ASHE, LFS,NOMIS. Authors' calculations.

Figure 3-3. Incremental Difference-in-Differences total hours estimates: All (16 to retirement age)

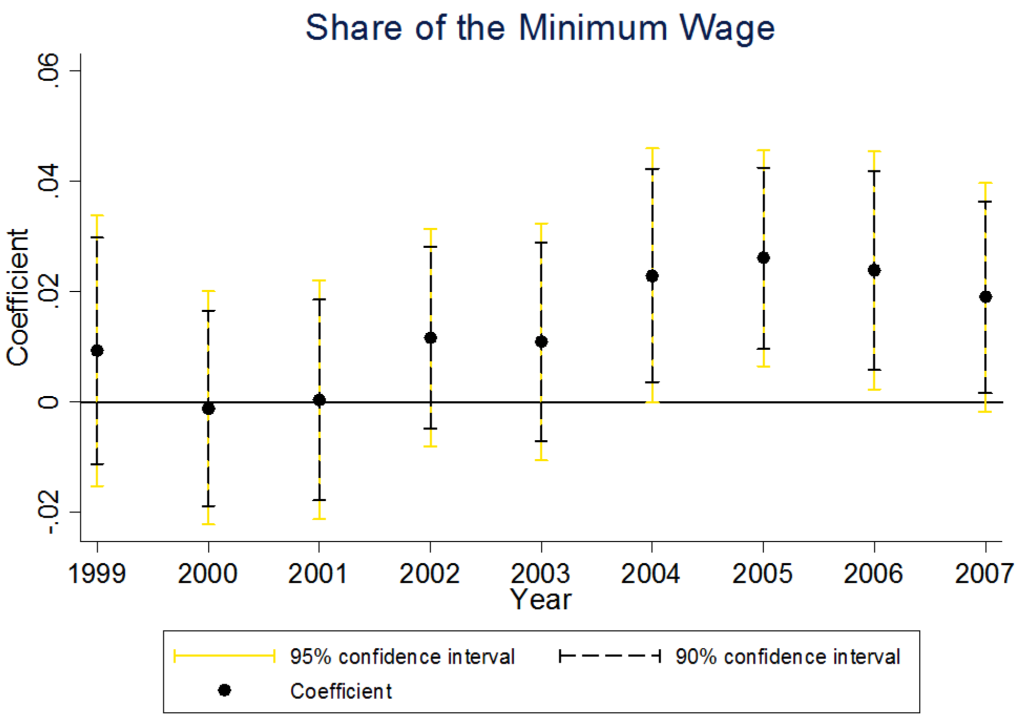
a) Part-Time Employees: All (16 to retirement age)

406 areas



Source: ASHE and LFS. Authors' calculations.

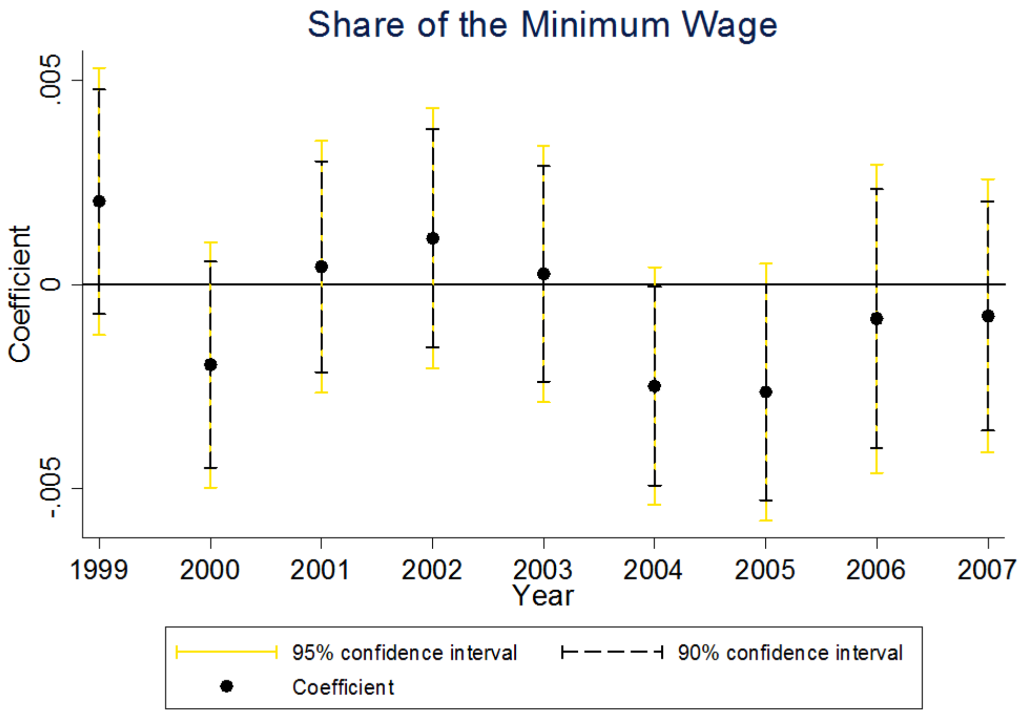
140 areas



Source: ASHE and LFS. Authors' calculations.

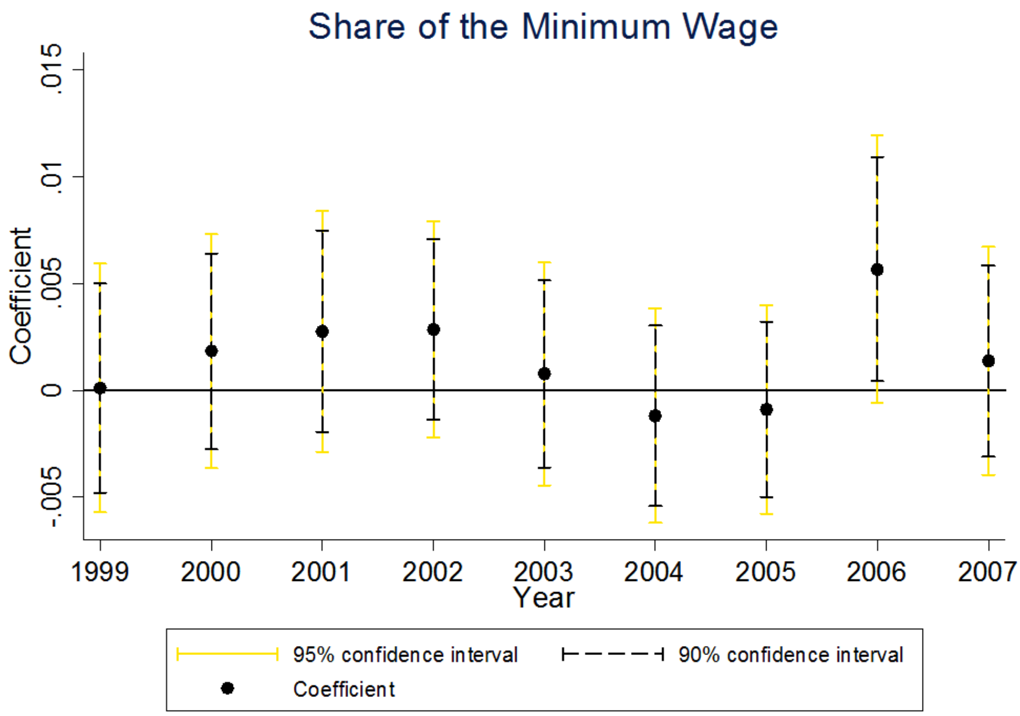
b) Full-Time Employees: All (16 to retirement age)

406 areas



Source: ASHE and LFS. Authors' calculations.

140 areas



Source: ASHE and LFS. Authors' calculations.

Table 3-1. Incremental Differences-in-Difference Unemployment and Hours of Work Estimates

	Proportion paid at or below NMW					
	Total	Total	Total	Total	Total	Total
	16-ret.	16-ret.	16-ret.	16-ret.	16-ret	16-ret
	406 areas	140 areas	406 areas	140 areas	406 areas	140 areas
	Unemployment		***Part-time hours***		***Full-time hours***	
NMW	0.008	0.008	-0.014***	-0.022***	0.003*	0.001
Base years	(0.011)	(0.025)	(0.005)	(0.008)	(0.001)	(0.002)
NMW*1999	0.078***	0.100***	-0.002	0.009	0.002	0.001
	(0.016)	(0.033)	(0.008)	(0.012)	(0.002)	(0.003)
NMW*2000	0.094***	0.103***	0.001	-0.001	-0.002	0.002
	(0.016)	(0.031)	(0.006)	(0.011)	(0.002)	(0.003)
NMW*2001	0.091***	0.086***	0.005	0.001	0.001	0.003
	(0.015)	(0.031)	(0.007)	(0.011)	(0.002)	(0.003)
NMW*2002	-0.005	0.001	0.006	0.012	0.001	0.003
	(0.013)	(0.030)	(0.007)	(0.010)	(0.002)	(0.003)
NMW*2003	-0.065***	-0.073**	0.009	0.011	0.001	0.001
	(0.014)	(0.032)	(0.007)	(0.011)	(0.002)	(0.003)
NMW*2004	-0.074***	-0.069***	0.015**	0.023*	-0.002*	- 0.001
	(0.015)	(0.035)	(0.007)	(0.012)	(0.001)	(0.003)
NMW*2005	-0.063***	-0.059*	0.022***	0.026***	-0.003	- 0.001
	(0.015)	(0.032)	(0.007)	(0.010)	(0.002)	(0.002)
NMW*2006	-0.066***	-0.132***	0.022***	0.024**	-0.001	0.006*
	(0.018)	(0.035)	(0.008)	(0.011)	(0.002)	(0.003)
NMW*2007	-0.019	-0.053	0.008	0.019*	-0.001	0.001
	(0.017)	(0.037)	(0.007)	(0.011)	(0.002)	(0.003)

Notes: All regressions contain year, area effects + controls (education, age and gender). Sample all aged 16 to retirement. NMW variable is the proportion of employees in each area paid at or below the NMW. HAC robust fixed effect estimates in brackets. The base years are 1997-1998.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 3-2. Lagged Incremental Difference-in-Differences Unemployment and Hours of Work Estimates

	Unemployment		***Part-time hours***		***Full-time hours***	
	406 areas	140 areas	406 areas	140 areas	406 areas	140 areas
NMW	-0.002	-0.037*	-0.005	-0.001	-0.001	-0.003*
Base Years	(0.010)	(0.020)	(0.004)	(0.006)	(0.001)	(0.001)
NMW*2000	0.114***	0.166***	-0.001	-0.012	0.002	0.008***
	(0.015)	(0.030)	(0.007)	(0.011)	(0.002)	(0.003)
NMW*2001	0.093***	0.125***	0.006	-0.008	0.002	0.004*
	(0.014)	(0.025)	(0.006)	(0.008)	(0.001)	(0.002)
NMW*2002	-0.024**	-0.004	0.008	0.011	0.001	0.004*
	(0.012)	(0.026)	(0.006)	(0.009)	(0.001)	(0.002)
NMW*2003	-0.077***	-0.067**	0.010	0.010	0.004**	0.004
	(0.014)	(0.030)	(0.007)	(0.010)	(0.002)	(0.003)
NMW*2004	-0.080***	-0.052*	0.011*	0.019*	0.001	0.004*
	(0.014)	(0.030)	(0.006)	(0.010)	(0.001)	(0.002)
NMW*2005	-0.068***	-0.024	0.010	0.021***	-0.001	0.002
	(0.014)	(0.030)	(0.006)	(0.008)	(0.002)	(0.002)
NMW*2006	-0.067***	-0.085***	0.013*	0.024**	0.001	0.008***
	(0.015)	(0.031)	(0.007)	(0.010)	(0.002)	(0.002)
NMW*2007	-0.049**	-0.081**	0.017**	0.022**	0.002	0.004*
	(0.019)	(0.033)	(0.007)	(0.011)	(0.002)	(0.003)

Notes: All regressions contain year, area effects + controls (education, age and gender). Sample all aged 16 to retirement. NMW variable is the proportion of employees in each area paid at or below the NMW. HAC robust fixed effect estimates in brackets. The base years are 1997-1998.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 3-3. Incremental Difference-in-Differences Unemployment and Hours of Work Estimates (prop. if workers in the manufacturing sector included as a control for industry)

	Unemployment		***Part-time hours***		***Full-time hours***	
	406 areas	140 areas	406 areas	140 areas	406 areas	140 areas
NMW	0.008	0.003	-0.014***	-0.022***	0.003**	0.001
Base Years	(0.011)	(0.025)	(0.005)	(0.008)	(0.001)	(0.002)
NMW*1999	0.076***	0.095***	-0.001	0.010	0.002	0.001
	(0.015)	(0.032)	(0.008)	(0.012)	(0.002)	(0.003)
NMW*2000	0.094***	0.108***	0.001	-0.002	-0.002	0.001
	(0.016)	(0.031)	(0.006)	(0.011)	(0.002)	(0.003)
NMW*2001	0.091***	0.089*	0.005	0.001	0.001	0.003
	(0.015)	(0.031)	(0.007)	(0.011)	(0.002)	(0.003)
NMW*2002	-0.005	0.008	0.006	0.011	0.001	0.002
	(0.013)	(0.030)	(0.007)	(0.010)	(0.002)	(0.003)
NMW*2003	-0.065***	-0.070*	0.009	0.011	0.000	0.001
	(0.014)	(0.031)	(0.007)	(0.011)	(0.002)	(0.003)
NMW*2004	-0.076***	-0.061*	0.015**	0.022*	-0.002	-0.002
	(0.015)	(0.035)	(0.007)	(0.012)	(0.001)	(0.003)
NMW*2005	-0.066***	-0.055*	0.022***	0.026**	-0.003	-0.001
	(0.015)	(0.031)	(0.007)	(0.010)	(0.002)	(0.003)
NMW*2006	-0.069***	-0.128*	0.023***	0.023**	-0.001	0.005*
	(0.018)	(0.035)	(0.008)	(0.011)	(0.002)	(0.003)
NMW*2007	-0.021	-0.054	0.009	0.019*	-0.001	0.001
	(0.017)	(0.037)	(0.007)	(0.011)	(0.002)	(0.003)

Notes: All regressions contain year, area effects + controls (education, age, gender). Sample all aged 16 to retirement. NMW variable is the proportion of employees in each area paid at or below the NMW. HAC robust fixed effect estimates in brackets. The base years are 1997-1998.
*** *** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

4. The International Experience of Minimum Wages in an Economic Downturn.

4.1 Introduction

The previous chapters have focused on the medium-run effects of the Minimum Wage (MW) in the UK, a period which covers years of positive growth in the UK. However, the effects of an upgrade in the MW may not be constant across the economic cycle. What should governments do with the level of the MW in times of recession? In an economic downturn when many firms face downward pressure on demand and costs, is it appropriate to let the MW fall, in real or even nominal terms, or are the positive effects of the MW on inequality enough to justify up-rating the MW – and if so - what might be the consequences on a country's employment level?

The purpose of the chapter is to obtain new estimates of the employment effect of the MW by focusing on the recessionary experiences across countries. Using international data we will exploit: cross-national variation in the timing and up-rating of the MW and the exact timing of the recessionary experiences in different countries with a panel data set comprising 33 OECD and European countries for the period 1971-2009. Our panel data allow us to differentiate the effect of MWs on employment in periods of economic downturn as well as periods of economic growth. We will also be able to account for institutional and other policy related differences that might have an impact on employment other than the MW.

Over the last 30 years there has been considerable controversy regarding the impact of MW legislation, particularly on employment levels. From a theoretical point of view, in a perfectly competitive labour market, a MW set above the market-clearing level reduces labour demand and thus decreases employment. However, alternative economic models have been put forward that predict insignificant or positive employment effects of MWs, for example, theories where firms have some degree of monopsonistic power or where labour market frictions exist (see Dolado et al, 1996).

Economic recessions clearly impose aggregate shocks to employment conditions which may affect the working of the MW. So there are good reasons for being concerned about the effect of the economic cycle in an analysis of the effects of the MW on employment. This can easily be seen in a simple elaboration of the standard competitive equilibrium in the labour market. The conventional analysis would suggest the logic in figure 4-1a. Here as a MW, $\hat{w}$ is imposed, equilibrium employment falls from e* to e’. Now consider what happens when a recession occurs at the same time. This can be represented in figure 4-1b. In a recession aggregate labour demand falls shifting D to D’. This means that the competitive labour market would routinely contract and employment would fall from e* to e’. But this is precisely the fall in employment which would have occurred by the imposition of the MW $\hat{w}$. Hence it is impossible to determine whether the fall in employment (e*-e’) is due to the imposition of the MW or to the recession. To make matters worse consider the effect of a simultaneous interaction of a MW and a recession. The recessionary contraction of labour demand moving D to D’ at the same time as an imposition of a MW $\hat{w}$ would reduce still further labour demand to E’’’. Hence there is a potential for a large negative interaction employment effect if a high MW is imposed at the same time as a recession occurs in the labour market. In contrast, consider that we are instead in a time of economic growth with the aggregate demand for labour rising, moving D to D’’. Again assuming that the MW of $\hat{w}$ has been imposed, here we reach a new equilibrium employment level of e’’. Now without controlling for rising employment due to economic growth we could erroneously attribute the growth in employment (e’’-e*) to the MW rather than to the effect of the growth in labour demand. Hence it is quite clear that any time series analysis of the MW across countries should control for changing aggregate demand conditions and the possible interaction between macroeconomic conditions and the MW to prevent any possible misattribution of growth in employment to the MW.

Since economic theory leaves us in an ambiguous position of possibly expecting either a negative or a positive effect of the introduction (or up-rating) of the MW on employment, then any serious study of the effects of the MW must adopt the tools of applied econometric analysis to make any progress on determining what has actually happened. A considerable literature – which we briefly review in the next section – has so far not yielded a consensus and indeed only fuelled the controversy of what the real effects of the MW are on employment. We seek to throw some more light on this controversy using extensively new data over a large number of countries, over a longer time period which also permits the study of the interaction between the MW and economic recessions.

Despite the controversy which surrounds the effects of the MW on employment there seems to be nearly universal consensus on the effects of the MW on inequality. More specifically, the prevailing view from the literature is that increasing the MW reduces inequality. In some respects this is not a surprising conclusion as, if one considers simply truncating the bottom end of the wage distribution by outlawing wages below a certain level, then almost by definition, any measure of inequality will be reduced, other things equal. This simple basic fact is even reflected at the country level when one examines the association between the Gini coefficient (as a measure of inequality) and the level of the Kaitz index (which measures where the MW is, relative to the average wage). In figure 4-2 this basic association is graphed for the countries in our data. The plot shows that even at a crude aggregate level if a country has a higher level of the MW relative to the average wage – a higher "bite" of the MW- then inequality will be lower in the country.

This provides us with the first clue to our policy analysis of the MW. Most importantly if the MW is good for inequality but potentially bad for employment then any government may have a possible trade off which might be more relevant in a recession to resolve in setting the MW. In such a simple framework the question each country's government must ask itself is: Does it want lower inequality enough to trade off the possible higher unemployment to get it? This is the question which must be returned to in any policy analysis of the MW and provides an excellent motivation to study this question.

The reference point for our work is the paper by Neumark and Wascher (2004) which provides some limited, dated, international evidence on the question of the MW impact. We seek to extend and test their results in six ways. Firstly, by extending the set of countries to include an additional 10 countries and extending the time period under investigation both backwards and forwards in time. Secondly, by extending their controlling regressors to a time varying policy context. Thirdly, we seek to explicitly investigate how the results change separately for young people and adults, in times of recession and with different measures of the "bite" of the MW. Fourthly, we have as our guiding motivation, the possible interaction of the timing of MW changes with the prevailing macro-economic position. Fifthly, we use three different measures of the "bite" of the MW. Finally, and perhaps most importantly, we seek to explore the difficult problem of the possible endogeneity of the MW or Kaitz variable in an employment equation.

These advances need some justification and motivation. The first advance relates to extending the data set of Neumark and Wascher (2004), by including more countries and by extending the time period under scrutiny. This is completely justified on the grounds that if the results of Neumark and Wascher are robust then they should hold up over a larger dataset relating to more countries and time periods. An additional justification for this approach is that the most recent time period throws up different macro-economic conditions (particularly in the last two years during the recession). This places the mechanisms through which the MW operates under additional stress. Specifically, for example, if the MW stays constant in real terms whilst the average wage is falling in real terms, then the Kaitz index is rising without the government taking any decisive action. The question then arises of whether this may have the same effect on employment as a conventional rise the real MW.

The second contribution of the chapter is to add more controls in the analysis in order to allow us to net out the effects of changes in policies other than the MW that might affect employment. In the literature to date this has not be adequately modeled. The paper by Neumark and Wascher (2004) only allows these to be fixed country specific characteristics which are immutable across time. In this chapter we are able to control for active labour market policies (ALMP), employment protection legislation (EPL), unemployment insurance programs and union density (see section 4.5.2 below for a detailed explanation of these indicators).

The third area of advance relates to the sensitivity of our estimates to: considering young people and adults separately[23], modeling the interaction of the MW effect with the macro-economic cycle and to different measures of this cycle and of the "bite" of the MW are central to any analysis of its effects. We find all these sensitivity checks reveal important insights concerning the effects of the MW on employment. Most notably: negative effects of the MW on employment are only robust for young people and not adults, the definition of what the MW "bite" is largely immaterial, and that the inclusion of interaction terms on the effect of the MW with the recessionary indicator are important in identifying separate further MW effects in time of recession on young people.

The fourth area of advance relates to the starting point and guiding motivation of this research which is to examine the possible interrelationship between the macroeconomic health of the economy and the impact of changes in the MW. Clearly one might expect

[23] Most of the literature focuses on young workers. This is because most probably a relatively large fraction of young workers are likely to be affected by the policy and potentially pay its price in terms of reduced employment. However, Brown et al (1982) p.512 point out that the literature that focuses on adults is rather scant: “Uncertainty about the effects on adults is a serious gap in the literature, since half of all MW workers are adults (and of course a larger fraction of all workers) are adults”. After 30 years that Brown et al (1982) mentioned this gap in the literature, still there is not much evidence for adults.

that the impact of changes in the MW might be different in a recession rather than a time of expansion in the economy. Evidence of this interrelationship could be crucial in framing policy about what to do with the MW in the current recessionary times.

The fifth contribution is to use different measures of the "bite" of the MW, checking the robustness of the results. As Neumark and Wascher (2004) and in most of the MW literature, we use the Kaitz Index as the ratio of the MW to an average wage. However, we also look at the percentile at which the MW "bites" the wage distribution and the MW relative to GDP per head. We use different measures of the MW to test the robustness of our results and allow for the fact that there are potential flaws in each measure of the MW "bite" (see methodology section). Clearly, if each of these measures offers a very different metric on the toughness of the MW then this constitutes an effective robustness test on our modeling.

The final area of our robustness investigation is the most problematic. The core problem with any MW regression, however formulated, is that arguably the measurement errors of the determination of employment are not independent of the "bite" of the MW. This endogeneity of the MW is most likely to be related to the unobserved heterogeneity across countries. This is true to the extent that any country's government which invokes a MW (or has favourable policies relating to its up-rating in real terms) will also have unmeasured, unobserved attributes which separately affect the employment level. So far in the literature there have been no studies which have discussed this issue. This seems to be a crucial problem in that there must be a strong likelihood that any country which up-rates its MW favourably might also have government which seeks to increase employment and drive down unemployment.

The usual approach to this problem in panel data is to add conditioning regressors and argue that the remaining unobserved heterogeneity is time invariant. We, of course, do this by including controlling regressors on all aspects of employment policy and ALMP. But still there is a suspicion that the unobserved heterogeneity in an employment equation may relate to the "bite" of the MW. The logic would be that whether a country decides to change its MW may well depend on the prevailing state of employment. In the absence of the experimental data the only other recourse to identification is to search for an appropriate instrumental variable (IV). This requires us to find a variable which is correlated with the Kaitz index but uncorrelated with the stochastic factors which determine employment.

Our suggestion is that one determinant of the likelihood of adopting a more generous MW is what political flavour of government is in power. Specifically, a more left wing government, which is, presumably, more averse to wage inequality, is more likely to favour a MW with a higher "bite" relative to the average wage. But the determination of the political makeup of the government is down to the political and electoral process – and the aggregation of votes which determines the political complexion of the government - and this is exogenous to the determination of employment. So specifically, we argue, that the political complexion of the government is independent of the unobserved heterogeneity or stochastic shocks which affect employment. Having already controlled for all the other politically determined employment and ALMP policies, then one might reasonably expect that the remaining unobserved heterogeneity relates to macroeconomic shocks which condition employment and will be independent of where, in the political spectrum, the electorate determines that the government comes from. We explore the extent to which our results are robust to this form of identifying assumption.

In summary, the purpose of the chapter is to obtain new estimates of the employment effect of the MW – but to do so with much more comprehensive and up to date data and to investigate the sensitivity of the results to different econometric identification assumptions. Using international data we exploit: cross-national variation in the timing of the introduction of the MW, the level and timing of its up-rating with a panel data set comprising 33 OECD and European countries for the period 1971-2009. We will also be able to account for institutional and other policy related differences that might have an impact on employment, other than the MW. The large differences across OECD and European countries in the Kaitz index (the MW relative to average wages) are a great potential source of variation in the "bite" of the MW. In common with the rest of the literature, we will evaluate the impact of MWs on employment using the variation in this "bite". What distinguishes this study from most of the literature is that we explicitly use the variation in the MW "bite" across time and countries.

The chapter is organised as follows. Section 4.2 summarizes the literature which precedes and sets the context of our work. Section 4.3 describes the dataset used and the characteristics of the data. Section 4.4 outlines the methodology for the analysis. The main results are presented in section 4.5. Section 4.6 sets out the policy implications of our findings.

4.2 The literature

It is clear that to ascertain the actual effect of MWs on employment, we must resort to empirical econometric research. However, data limitations and econometric identification issues complicate this process. There have been a number of studies which use data from a single country. Some of these studies use time series variation in the MW policy (or its level) over time to try and identify the impact of the policy. The consensus of these earlier studies is summarized by Brown et al (1982). They suggest that these earliest empirical studies, based on time-series data, confirmed standard economic theory showing a negative impact of MW on employment.

However, this debate really began in earnest after the findings of Card and Krueger (1995). In a quasi experimental setting they found that MW increases can, in some circumstances, result in net job gains rather than the losses predicted by conventional wisdom. They used data from fast food restaurants in neighbouring US states in Pennsylvania and New Jersey, where the latter state up-rated its state MW and the former kept it fixed. They argue that this exogenous change in the MW in Pennsylvania constituted a quasi experiment which allowed them to identify a positive causal impact of the MW up-rating.

The work of Card and Krueger has, in turn, been subject to intensive scrutiny and launched a wave of further empirical work on the impact of the MWs on employment. While many assessments of MWs have been carried out on a national basis, there have been few from an international perspective. The large differences across OECD and European countries in the Kaitz index (the MW relative to average wages) are a great potential source of variation in the "bite" of the MW. In common with the rest of the literature we will evaluate the impact of MWs on employment using the variation in this "bite". What distinguishes this study is that we explicitly use the variation in the MW "bite" across time and countries.

Further fuel has been added to this debate by panel data based studies which relate to US States over time or different countries over time (Neumark and Wascher (1992, 2004)). The attraction of this kind of panel study is that different countries (states) will have different policies on the MW and its up-rating, different employment policies and face different macroeconomic conditions. Such natural variation facilitates possible identification of the effect of MW policies on employment. So far, these studies support the view that the "bite" of the MW (as measured by the Kaitz index) has a significant and negative impact on employment. It is important to appreciate that the Neumark and

Wascher results (2004) only applies to the labour market for young people and clearly one important extension to their work involves at looking at adult labour markets, which is what we do below. Of course, the usual identification assumption here is that the unobserved heterogeneity across countries remains fixed over time. To the extent that this identification assumption is justified then these results must be taken seriously. Since the analysis of the labour market impact of the MW is so widely policy relevant (as a lot of countries now have a MW) it merits careful scrutiny. This is the motivation for the present chapter.

There is a related substantial literature that uses cross-country comparisons to investigate the impact of labour market policies generally: for the impact of labour market rigidities on unemployment see Nickel (1997), Blanchard and Wolfers (2000), Nickell, Nunziata and Ochel (2005); for a review of cross-country studies on the impact of Employment Protection Legislation see Addison, J. and Teixieira (2003). However, few studies have used cross-country analysis to estimate the MW effects on employment. Indeed, apart from an older OECD study (1998), Neumark and Wascher (2004) is the only extensive study which looks at how changes in the incidence of the MW across countries are correlated with changes in country specific's youth employment rates, using a panel of countries from 1976 to 2000.

While there are a large number of studies on the labour market impact of the MW, especially on the impact on employment, (see Brown et al (1982), Card and Krueger (1995) and Neumark and Wascher (2008) for extensive reviews of the literature), only a few studies evaluate the impact of the MW by exploiting geographical variation in regional or cross country labour markets, (see Card (1992) or Neumark and Wascher (1992) for the United States, Stewart (2002) for the UK and Baker et al (1999) for Canada). This chapter builds on that small literature by examining the impact of a MW across countries of the world over the period 1971-2009.

There is good reason to expect that imposition and then raising of the MW will have positive effects in reducing wage inequality at the bottom end of the income distribution (see chapter 5). If one truncates the income distribution from the left by forcing employers to pay the lowest earners at a specified minimum then automatically one expects that (unless there are large spill-over effects) that inequality would be reduced as the MW rises, other things equal. Dickens and Manning (2004b) report evidence of these inequality effects in the UK around the introduction of the MW and other authors report similar findings from the US. (See DiNardo et al (1996), Lee (1999) and Autor et al (2010)).

4.3 Data

The data used in this study is necessarily collected from different sources. Most of the data on population, unemployment and employment rates are drawn from the OECD Annual Labour Force Statistics database for OECD members and the European Union Labour Force Statistics for the remaining countries. This allows us to disaggregate our dependent variable into two age groups: young people (15 to 24) and adults (25 to 64 years).[24]

As in the second chapter, we use three different measures of the MW in our analysis: the Kaitz index, the Percentile at which the MW "bites" and the real MW divided by the GDP per head[25]. Data on the Kaitz index is available from the OECD MW database and from the European Union Labour Force Statistics for those countries in which the national MW is set by statute or by national collective bargaining agreement. For those countries in which no national minimum exists, but in which industry- or occupation-specific minimums are set by legislation or collective bargaining agreement, we use summary estimates constructed by Dolado et al. (1996)[26]. OECD and European Union Labour Force Statistics allow us to use as an indicator of the MW the Kaitz Index, the ratio of the MW to the average wage, as measured in this study by the median wage[27]. Using median rather than mean wages in the denominator provides a better basis for international comparison because of differences across countries in the dispersion of earnings. The Kaitz Index is one of the standard indicators used in the literature and it is intended to measure the extent to which the MW "bites" into the wage distribution, and to capture variation in the relative prices of less-skilled and more skilled labour induced

[24] There are few exceptions in the age groups: Italy, where prior to 1993 lower age limit is 14. Portugal, where the lower age limit from 1976 to 1991 is 12 years old and from 1992 to 1997 it is 14 years old. Spain, UK, US and Sweden where the lower age limit in the survey is 16 years. Finally, in Hungary, where up to 1994 the adult and total age groups refer to ages 15 to 74.

[25] Our MW variables measures in common with much of the literature are not adjusted for coverage because of data limitatios.

[26] For these 4 countries the panel length is shorter (Italy, from 1976 to 1991; Germany from 1976 to 1994; Denmark from 1983 to 1994; and finally Sweden from 1976 to 1992) . However, our main results are robust if we omit these countries from the analysis. In these countries no national minimum wage exists, but a set of industry- or occupation specific minimums, the Kaitz index for these countries is computed as an average minimum hourly wage divided by the average wage. We would like to thank Neumark and Wascher for providing us with the data.

[27] Because of data limitations, for a subset of 9 countries, we had to use mean wages in constructing the indices. These countries are: Sweden, Denmark, Germany, Italy, Slovenia, Latvia, Bulgaria, Croazia and Mexico.

by MWs. The closer the Kaitz index is to one the "tougher" the "bite" of MW legislation in any country and specific year (appendix 4.C contains a detailed description of the MW variable by country). Details of the definitions and measurement of the other two alternative measures of the MW are described in the methodology section and appendix 4.C.

Our analysis controls for many other characteristics of the MW systems which might have an influence on employment, so confounding effects of the MW. First of all, we include an indicator of whether the MW is a product of collective bargaining process, with unions, employers and the government all participating in the negotiations or whether MW levels are simply set by statute[28]. Data is drawn from two main sources: the ILO MW database and EUROSTAT. Secondly, we add an indicator of whether countries have youth sub-Minimum Wages or not[29]. Information is taken from the ILO MW database and the Low Pay Commission report (2009). (We refer to appendix 4.D for a list of countries included in the analysis and their characteristics of the MW system.)

We also add controls for the importance of other labour market policies and institutions which might have an influence on employment. Here our data is superior to that used by Neumark and Wascher (2004), as all of our measures are in panel form, i.e. these controls are time varying for each country, whereas those in previous studies have only been cross-sectional form, i.e. constant across countries over time.

The first of these measures is the OECD index of EPL, a constructed indicator of the strictness of regulation on dismissals and the use of temporary contracts. A second measure is the level of public expenditure on ALMP, designed to bring unemployed workers to work, as a percentage of GDP. Again this variable is drawn from the OECD. A third measure again constructed from OECD data is trade union density which is measured by the number of wage and salary earners that are trade union members, divided by the total number of wage and salary earners. The final measure constructed by the OECD is the average of the gross unemployment benefit replacement rates as a percentage of earnings and it is meant to quantify the generosity of unemployment

[28] If the MW is a product of a collective bargaining process, labour unions take part in the decision of the rate of the MW. This could potentially lead to a relatively high rate of the MW that possibly could harm employment more than in other countries where statutory MW exists.

[29] Having a youth sub-minimum lower than the adult rate could potentially help to reduce the negative effects of the MW, if there are any, in the young population, compared to countries where there is only one rate of the MW.

insurance program (a detailed explanation of these variables, their possible effects on employment and our findings are reported in section 4.5.2 below).

As a supply side control we use the relative size of each cohort, such as the population of the regression specific age group to the total population in working age (Brown et al (1982) p.501)[30]. Finally, we include an aggregate demand variable to account for changes in the level of economic activity over the business cycle. Initially we use the adult unemployment rate taken from the OECD Annual Labour Force Statistics database, to ensure comparability with Neumark and Wascher (2004) but we then use other measures of aggregate demand and macroeconomic shocks including our recession indicator (described in the methodology section) and the actual level of GDP growth to test for robustness[31].

Our full sample consists of 33 OECD and European countries, but is reduced to 23 OECD countries when the full set of controls is added into the analysis. We refer to the appendix 4.D for a detailed list of the countries in the analysis (and we clearly show beneath each table the countries which are used in the estimations).

In figure 4-3a countries are ranked by the Kaitz index. As can be seen there is substantial variation in the "bite" of the MW across countries, with the level of the MW ranging from more than 70% of the average (median) wage in Italy to under 30% in Korea. Generally, the continental European countries have the highest levels of the "bite" of the MW. Australia is the only non-European country with a Kaitz index of around 60%. In contrast, US and UK and Japan are towards the bottom, together with Mexico and some new European accession countries such as Estonia, Croatia, Czech Republic and Latvia with a Kaitz index under 40%. Figures 4-3b and 4-3c graph the ranking of countries with our two alternative measures of the "bite" of the MW. Specifically we graph the level of the MW relative to the GDP per head and the Percentile of the MW. These alternative measures are defined and motivated in the methodology section. They represent measures of the "bite" of the MW, normalised, respectively, to the level of aggregate wealth per head in the economy, and by pinpointing where the MW level is in the wage distribution. Looking at the ranking of the countries in figures 4-3b and 4-3c we can see that there is a fair degree of concordance between these 3 rankings, however they are sufficiently different to

[30] It is widely accepted in the literature to use a supply side control, especially in the youth equation. Brown et al (1982) stress the importance of this control on p.501 of his paper.

[31] GDP growth in the employment equation is generally significant (and less so the dummy for downturn). Results do not change much qualitatively with or without GDP growth.

warrant their use as specification checks on the main variable of interest, as they constitute quite different interpretations of what the "bite" of the MW is.

Figure 4-4a shows changes in the Kaitz index across (some selected) countries over the period of our analysis. It is interesting to see how some countries like the Netherlands, (and Belgium, Spain, Australia and Mexico not pictured here) have experienced a decreasing "bite" of the MW over the years. However, other countries such as France, (and the Slovak Republic, Czech Republic, Turkey and Korea not pictured here) show an increasing Kaitz index especially over the last few years. The corresponding figures for the MW/GDP per head variable are similarly informative and follow most of the times a similar trend as the Kaitz with a slightly different metric. The third measure of the Percentile in figure 4-4c is most interesting as it follows a structurally different path to the alternative measures in figure 4-4a and 4-4b, particularly for the USA and the Netherlands prior to 1980. The measure is also useful to graph as it reminds us that the MW in most developed countries only affects between 2-5% of people[32]. In this context, it is asking quite a lot for macroeconomic aggregate data to identify a statistically significant effect of an intervention that affects such a small proportion of the labour force. It is especially demanding to expect the same relationship to be revealed in metrics – like the Kaitz and the Percentile – which exhibit quite a different trend over time.

Changes in the real level of the MW (as measured in US dollar purchasing power parities) by country are very variable as plotted in figure 4-5. Particularly notable is that over the last 10 years, nearly all countries have allowed the MW to increase in real terms. However, in some countries (e.g. Netherlands, US, Canada) there was a substantial erosion of the real value of the MW since the mid-1979s to the late 1980s, which continued into the 2000s in the US. It is also important to point out how, for some countries, changes in real level of the MW do not always correspond to changes in the Kaitz index. For example, Australia has experienced an increase in the real level of the MW but a decrease in the Kaitz index. Also, in Japan, Luxembourg and France the increase in real minimum wages is more marked than the raise in the Kaitz index. The same is true for Ireland and the UK from 2000 onwards. These different movements of the Kaitz index (measured as the MW relative to the median wage) compared to the real

[32] In general, in western countries the MW is generally set to bite the lower tail of the wage distribtuion. It is also true that in some developing countries and in eastern european countries it might be different (for example the bite of the MW could be even lower).

level of the MW could be a result of changes of position of the MW in the distribution of earnings[33].

An indication of the variation in the dependent variable of concern, the youth employment to population ratio is graphed for our selected countries in figure 4-6. We can see that for most of our period this ratio has been declining in some countries, like France, growing in others, like the Netherlands, and roughly constant for others, like the US and the UK[34]. There is also evidence of cyclical variation and considerable variation across years to be explained by our data.

4.4 Methodology

In this section we describe the economic modelling strategy employed in our investigation.

4.4.1 Modeling strategy and baseline regression

Among the first to use panel data to address the question of the impact of the MW were Neumark and Wascher (1992) who used US state data from 1973-1989. Later, Neumark and Wascher (2004) exploited cross-national variation in the MW to estimate the effects of MWs on employment rates using a pooled cross-section data set comprising several OECD countries for the period 1976-2000. We update their investigation by adding a longer time period from 1971-2009 and a larger set of countries. We apply their model to the estimation of the employment rate across countries, in this specification the variable *MW* is assumed to capture the "bite" of the wage condition:

$$E_{jt} = \alpha_1 + J_j + T_t + \beta MW_{jt} + \gamma X_{jt} + u_{jt} \qquad (4.1)$$

Where E_t is the employment to population ratio at time t in country j , MW_{jt} is the Kaitz Index (the MW divided by the median wage)[35] at time t in country j, X_{jt} is a set of controlling regressors at time t in country j, T_t is a set of year effects and, J_j is a set of country fixed effects. Country fixed effects are included to control for omitted variables that vary across countries but not over time such as unmeasured economic conditions of specific countries independently of international labour market conditions

[33] The Kaitz index measures where the MW bites in the wage distribution. By definition in this study the Kaitz index is the MW relative to the median wage. If the MW increases and the median wage does increase more than the MW the the Kaitz index decreases. However, the real MW might increase anyway.

[34] However, even a constant ratio disguises changes in the workforce composition: tipically, a large rise in female employment and a decline in male employment.

[35] Usually the kaitz Index is also weighted by some measure of 'coverage' of the MW in the sense of the fraction of the labour force that the MW applies to.

(government policies as well as cultural and institutional differences across countries). Time fixed effects control for omitted variables that are constant across countries but evolve over time (global shocks or policies that might influence employment rates in all countries at the same time).

We also investigated specifications that include country-specific time trends in order to control for incremental changes in the employment rate associated with longer-term developments in labour force participation or labour demand that are unrelated to changes in a country's MW laws[36]. In all of our reported regressions standard errors are robust to heteroskedasticity and serial correlation of unknown form (Wooldridge, 2010, p.61).

An important issue with panel data and the validity of fixed effect (FE) results is the extent to which omitted regressors may change over time. To the extent that they could vary over time then the FE results could be biased. Our approach to this problem has been prompted by precisely this concern with the paper by Neumark and Wascher (2004)[37]. It is for this reason that we have sought to use all the available data on other controlling regressors, particularly those relating to labour market institutions and how they vary over time. Of course it is still possible that we have not included all the controlling information that one would ideally like (which remains unmeasurable or unobserved). To this extent our (FE) results are valid only under the condition that these other sources of unobserved heterogeneity remain fixed over time.

We seek to model the employment effect for adults and young people rather than just young people. In turn we also address the difficult issue of what constitutes a satisfactory measure of the MW by using three alternatives.

The idea behind using the Kaitz index as a measure for the MW is that the larger the value of this index then the higher up into the wage distribution the minimum will "bite". Of concern in the use of the Kaitz index is its particular sensitivity to movements in the earnings distribution and the variability of this distribution, particularly at the upper end. We explicitly examine the issue of how robust the results are to the use of this variable by also considering the ratio of the MW to the level of GDP per head and the position of the MW in the wage distribution.

A crucial issue which one must consider in modeling the effect of the MW is how this level should be measured. Clearly simply putting this nominal level on the right

[36] See the results section 4.5.1.2 below for a discussion on the results using contry-specific time trends

[37] This is the first time such data has been used in this context as Neumark and Wascher (2004) only used non-time varing regressors to control for these influences.

hand side of a regression is open to the criticism that such a variable is endogenous. Hence, for many years the literature has been dominated by the use of the ratio of the MW to the average or median wage. This Kaitz Index is also vulnerable to many criticisms. Most notably, since it is a ratio, its movement could be heavily influenced by movements in the average wage or indeed in the distribution of earnings – rather than a change in the MW. Clearly, since the wage distribution in many countries in the West has been shifting to the right in the last 20-30 years this Kaitz index could be moving largely as a result of these changes (and figures 4-3 to 4-5 seems to bear this out).

In many microeconometric studies (see Dolton et al (2012), for example) other measures of the MW have been used as they can be computed easily. Specifically we are talking about the percentile in the empirical wage distribution that a MW hits, and the location of the spike in the wage distribution associated with the MW. In international aggregate country data such measures are impossible to compute as we do not have cross section data on individual wages.

In this chapter we use two such alternative measures of the MW. Firstly, the level of the MW expressed as a fraction of the GDP per head level, which is a measure of average income. Our second measure of the MW "bite" is to compute the percentile at which the MW "bites" in the income distribution by estimating this using the level of the MW, the average wage and the estimated Gini coefficient for the country. Using this data and assuming that the income distribution is lognormal then we can retrieve the percentile that MW is paid at in the wage distribution of the economy. (In the appendix 4.C a detailed explanation of how this variable is computed).

4.4.2 Modeling recessions

At the outset of this chapter we set out a key problem in the potential identification of MW effects on employment – specifically whether the shocks experienced by countries in a recession have been adequately captured in existing empirical studies and in addition whether there might not be an interaction between the MW and the timing or the scale of its introduction or up-rating and the prevailing macro-economic conditions.

Our long cross-country panel can be exploited in order to estimate a model that takes into account the different effects of the MW on employment in periods of economic downturn as distinct from periods of economic growth. This can be done by extending our estimation model to analyse the MW effects during economic recessions.

Hence in equation (4.2) we extend the model of equation (4.1) accordingly:

$$E_{jt} = \alpha_2 + T_t + J_j + \beta_2 MW_{jt} + \gamma_2 X_{jt} + \delta C_{jt} + \theta C_{jt} * MW_{jt} + \varepsilon_{jt} \quad (4.2)$$

Where the term in C_{jt} measures the direct effect of the recession on the employment rate (we measure recession in three different ways and compare the results, as clarified and pointed out below in this section). The term $C_{jt} * MW_{jt}$ measures the interaction effect of any recession and the MW. All other variables are as specified in equation (4.1). The coefficient of interest will be θ, which measures the differences of the effect of the MW on employment in periods of recession relative to periods of economic growth. Therefore, the hypothesis being tested here is whether the interaction of a downturn with the "bite" of the MW has an employment effect, over and above, the effect of either the downturn per se (δ) or the imposition of the MW, per se, (β).

The countries in the study have very different patterns of MW changes over time, and very different patterns of recession experiences. Table A 4-1 (in appendix 4.A) lists the timing of recessions across countries and shows how different they are between countries. This observed heterogeneity helps to separate the influences of MWs from the influences of other macroeconomic events affecting employment in multiple countries. We also seek to investigate how robust our estimations are to using this dichotomous recession indicator by replacing it with a continuous measure of the macroeconomic cycle in each country – by using the actual level of GDP growth in the year or the actual level of unemployment in the year.

A measurement issue for this study is how exactly one should measure a recession. The formal accepted definition of a recession is 2 quarters of consecutive negative GDP growth. This definition is clear and unambiguous in the context of quarterly data but leaves us with a difficulty when we work with annual data. We explored three possible definitions of a recession for yearly data:

i) The year contains at least 2 consecutive quarters of negative GDP growth (for example, this would give the years 1980, 1990, 1991 as recessionary years for the UK).

ii) The year contains any 2 two quarters (not necessarily consecutive) of negative growth (for example this would give the years 1979, 1980, 1990, 1991, as recessionary years for the UK).

iii) The year has negative growth on average over all 4 quarters (for example, this would give the years 1980, 1981, 1991 as recessionary years for the UK).

We found that the different possible definitions of a 'recession' as a discrete variable was unimportant and did not change the nature of our conclusions – hence the results we report look only at the effect of a recession defined as per definition ii) above.

The next consideration is that a simple dichotomous variable representing whether a recession was in progress or not may not adequately capture not simply whether a recession was in progress but also the depth of its severity. Hence, the next alternative is to use the level of GDP growth directly, instead of the recessionary indicators defined above. The third logical alternative is to use the level of the youth/adult unemployment as the recessionary indicator for the adult/youth employment equation. Hence, in section 4.5.3 of the text we explore how sensitive our results are to using these different definitions and methods of measuring a recession.

4.4.3 Endogeneity of the MW variable

We now turn to the most difficult potential problem in identifying the effect of the MW on employment. This is that, arguably, the MW variable is itself endogenous in the sense that this is a decision variable subject to change by a government after it observes the employment level and so takes this (and other macroeconomic circumstances) into account when the level is set. So there is a suspicion that the unobserved heterogeneity in an employment equation may relate to the "bite" of the MW. The logic would be that whether a country decides to change its MW may well depend on the prevailing state of employment. In the absence of the experimental data the only other recourse to identification is to search for an appropriate instrumental variable (IV). This requires us to find a variable which is correlated with the Kaitz index but uncorrelated with the stochastic factors which determine employment. So, in this framework we seek to find an appropriate IV, Z_{jt}. Our suggestion is that one determinant of the likelihood of adopting a more generous MW is what political flavour of government is in power.

Specifically, a more left wing government, which is averse to wage inequality, is more likely to favour a MW with a higher "bite" relative to the average wage. But the determination of the political makeup of the government is down to the political and electoral process – and this aggregation of votes which determines the political complexion of the government - must be exogenous to the unobserved stochastic factors which determine employment. So specifically, we argue, that how right or left wing a government is – which is determined by the electorate and the election process – is related to whether or not the government will seek to impose or uprate the MW favourably – but that - the political complexion of the government is independent of the unobserved heterogeneity or stochastic shocks which affect employment. Having already controlled for all the other politically determined employment and ALMP, then one might reasonably expect that the remaining unobserved heterogeneity relates to macroeconomic shocks which condition employment and will be independent of where, in the political spectrum, the electorate determines that the government comes from. We explore the extent to which our results are robust to this form of identifying assumption.

To elaborate the logic of this IV model consider what determines the political complexion of the government in power in any country. In most countries which have either a proportional representation system or a 'first past the post' electoral system the prevailing swing of the electorate – right or left – will determine which political party gets into power. This will then reflect the composition of the ruling body – a government cabinet (or similar configuration) which could be the result of coalitions between more than one political party. So – what we argue is that there is a mapping – albeit a fuzzy one – between the prevailing view of the electorate and the political complexion – right or left – of the resulting government. Now what determines the voting behaviour of the electorate is conditioned on many things – past events, political scandals (Watergate, Monica Lewinsky, MPs expenses etc), outcomes of wars (Vietnam, Iraq, the Falklands), crises in industrial relations (Mr Heath's and Mrs Thatcher's conflict with the miners' unions), one-off popular policies (selling council houses), terrorism attacks (Zapatero's election in Spain the day after the Madrid train bombings), policy announcements (neutrality to wars, Schroeder's election after he declared he was against thr participation of Germany to war in Iraq) or the prevailing state of the world economy and the collapse of major banks. What an electorate is extremely unlikely to be voting on is the stated position of the parties on the MW or the unobserved stochastic factors which determine employment (and remember in our empirical model this cannot include observable implementable employment policies like ALMP or EPL – which we have conditioned on). For an extensive and recent review of the relevant and highly cited literature on voting behaviour, please refer to Bartel (2010). See also Montalvo (2011).

Once the political complexion of the government is determined by the electorate – left or right – then their political position on the MW is likely to be determined by their underlying political philosophy. It is an important feature of Socialist governments that they give a high priority to poverty and income inequality issues. Equally it is an important dimension of most right wing political parties that they favour letting wages and prices be determined by unfettered market forces. It is this logic which suggests our specific IV. Empirical examination of this basic relationship is found in a scatter plot of the Kaitz index and the Schmidt index – which we present in figure 4-8. There is a clear correlation between where the MW "bites" in a country (on average) and how left wing their government is (on average). It is this correlation and its movement over time in each country which we wish to exploit.

Hence our econometric model is now as follows (where we omit the macro-economic recession and its interaction term for notation convenience):

$$E_{jt} = \alpha_3 + J_j + T_t + \gamma_3 X_{jt} + \beta_3 MW_{jt} + u_{jt} \qquad (4.3)$$

$$MW_{jt} = \alpha_4 + J_j + T_t + \varphi Z_{jt} + \gamma_4 X_{jt} + \mu_{jt} \qquad (4.4)$$

Where our notation is as for equation (4.1) except that now there are two different sets of country fixed effects in (4.3) and (4.4). In this context we seek to find an appropriate IV, Z_{jt}. Our suggestion is that one determinant of the likelihood of adopting a more generous MW is what political flavour of government is in power. The estimation procedure in the model set out in equations (4.3) and (4.4) involves using the Fixed Effects Instrumental Variable (FEIV) (see Wooldridge (2010), p 354) which is a two stage procedure estimated in Stata. First, having estimated the country and year fixed effects (which we assume to be constant across both equations) we then estimate the reduced form of this model. Predictions of MW are then used in the second stage estimation of the structural form equation (4.4). In this model the country fixed effects J_j are assumed constant over time and year fixed effects constant across countries. In addition it is necessary that: $E(u_{jt}|Z_{jt}, J_{j,}, T_t) = 0$. Assuming that our IV is valid then these IV estimates of the effect of the MW then provide consistent estimates of the MW effect on employment which potentially have a causal interpretation.

4.5 Results

In this section we will describe our estimation results relating to equation (4.1). We will separately consider: the results for young people and adults (section 4.5.1), the effect or otherwise, of conditioning for government employment policy (section 4.5.2). We then go on to consider the model of equation (4.2) which allows for the possibility of the interaction of macroeconomic shocks and the MW (section 4.5.3). Finally, we consider equations (4.3) and (4.4) jointly which treat the MW as potentially endogenous and examines the consequences of this (section 4.5.4).

As a baseline specification we will focus mainly on results that use the Kaitz index as MW indicator and GDP growth as demand side control. However, we will report also some robustness checks where we test two other MW measures and we assess two other different controls for the business cycle.

4.5.1 Estimates of the MW model

4.5.1.1 Young people aged 16-24

One possible concern is a lack of focus on the outcomes of groups thought to be potentially more at risk, or at the margin of adjustment, following any change in labour costs. Therefore, it is important that we assess whether the estimates differ for young people (those aged 16-24) compared to adults (aged 25 to retirement).

Column 1 of table 4-1 presents results for young people, while column 2 focuses on adults. Each column reports estimates from a regression in which we add fixed year effects to control for global shocks or policies that might influence employment rates in all countries. We also add country fixed effects to capture persistent country-specific unobserved factors that might influence employment rates. Examples of such factors might include government policies as well as cultural or other institutional differences across countries that lead to cross-sectional variation in the propensity to work.

If we concentrate on young people, we find the coefficient of the Kaitz index to be negative and significant and the estimated labour demand elasticity with respect to the MW (shown in the bottom row of the table and evaluated at the sample means) is -0.21, in line with the literature and very close to Neumark and Wascher's (2004).

In table 4-2, panel A, we test the robustness of our results repeating the analysis using two other demand side controls: first, in column 1, our dummy for downturn and secondly, in column 2, adult unemployment rate. This makes very little difference to the estimates confirming the results in table 4-1. The coefficients of the Kaitz index using the dummy for downturn as a control for the business cycle are very close in size to those in table 4-1 with the signs pointing in the same direction. The coefficients of the Kaitz index using adult unemployment rate as a demand side control are as well qualitatively similar, pointing again in the same direction (although they are qualitative larger and this reflects the metric on the unemployment rate compared to the GDP growth rate).

In table 4-2, panel B, we repeat the analysis using two other measures of the MW. An important part of our contribution is posing the question of how the MW should be measured. In the methodology section we suggested that there are 2 alternatives with aggregate countrywide data. Namely, the value of the MW relative to the GDP per head and secondly the Percentile that the MW is paid at in the wage distribution. In column 1, where the MW/GDP per head variable is used instead of the Kaitz index, we see that the negatively significant effect of this MW measure remains, and the coefficients are larger in absolute size, with an estimated MW elasticity of -0.36. The use of the percentile, in column 2, confirms the negative and significant results, even if the coefficient is smaller in absolute size, with an estimated elasticity of -0.17. Hence our overall conclusion is that the effect of a 10% rise in the MW will induce a 2-3% fall in employment of young people and that this effect is invariant to how one measures the MW of controls for demand shocks.

4.5.1.2 Adults

Column 2 of table 4-1 presents the estimation results of the effects of the Kaitz index on employment for the adult age group from 25 to 64 years. The coefficient on the MW variable is negative and significant, although the coefficient is close to zero and much smaller in absolute size compared to young people in column 1. The estimated elasticity for adults is -0.046 compared to -0.214 for young people.

In order to test the robustness of our results, in table 4-2, panel A, we repeat the analysis using other demand side controls: first, in column 3, our dummy for downturn. Secondly, in column 4, the youth unemployment rate. This makes very little difference to the estimates confirming the results in column 2 of table 4-1. The coefficients of the Kaitz index using the dummy for downturn as a control for the business cycle are very close in size and direction to those in column 2 of table 4-1. The coefficients of the Kaitz index using youth unemployment as a demand side control again point in the same direction to those in column 2 of table 4-1.

Finally, in table 4-2, panel B, we repeat the analysis using the other two measures of the MW. In column 3, where the MW/GDP per head variable is used instead of the Kaitz index, we see that the negatively significant effect of this MW measure remains, and the coefficients are larger in absolute size, with an estimated elasticity of -0.195. The use of the percentile, in column 4, confirms the negative and significant results, even if the coefficient is slightly larger in absolute terms, with an estimated elasticity of -0.073.

In both the analysis for the adults and young people we experimented with country specific time trends. This would theoretically capture factors that might influence employment trends within a country. However, we found that some of the estimation results were less stable when we do this. In some cases adding time trends removed the significance of the coefficients, suggesting no overall difference in employment between countries where the MW "bites" most compared to areas where the MW has less impact. Since there is no *a priori* reason to impose these restrictions we favour an unrestricted specification which is more flexible in allowing separate year and country fixed effects. One should also be cautious in interpreting such results because of the loss in terms of degrees of freedom that country-specific time trends might cause. Further, one should be aware that asking this small panel data to recover 33 country effects, 39 year effects and imposing a restriction of a trend to be identical across all countries is simply too restrictive for the model.

4.5.2 The role of other labour market institutions on employment

In the tables of estimation results we present, we also control for the labour market policies and institutions that might affect employment other than the MW. Therefore in this section we summarize the results for these policies and institutions, concentrating on specifications which include controls and both year and country effects (table 4-1, column 1 for young people and column 2 for adults).

The first OECD indicator we consider measures the level of public expenditure in ALMP to bring unemployed workers to work as a percentage of GDP. Therefore, a lower value indicates a lower commitment to such policies and institutions.

In particular, such policies could include public employment services, training, employment incentives (such as recruitment and employment maintenance incentives), supported employment and rehabilitation, direct job creation.

By improving the efficiency of the job matching process and by enhancing the work experience and skills of the unemployed, ALMP can increase employment. However, the efficacy of ALMP has been found to vary significantly between different types of programs and how these programs are designed. Furthermore, the positive effects need to be weighted against the costs of taxes necessary to fund them, which may in turn increase unemployment. Also, certain programs may reduce job search effort amongst the unemployed. In this chapter, a high degree of commitment to ALMP legislation is found to be associated with lower employment prospects for all employment groups.

The second measure provides information on employment protection regulations across countries. This OECD index of employment protection is an indicator of the strictness of regulation on dismissals and the use of temporary contracts. In particular, it measures the procedures and costs involved in dismissing individuals or groups of workers and the procedures involved in hiring workers on fixed-term or temporary work agency contracts. High values are associated with countries having a high degree of employment protection, while low values indicate relative ease in dismissing employees.

Basic economic theory relating to EPL would predict that EPL lowers labour turnover (both hiring and layoff) on the one hand, but increases the length of unemployment spells on the other hand, with ambiguous net effects on aggregate employment and unemployment rates. Econometric estimates of the impact of EPL on the unemployment rate do not clearly provide an unambiguous conclusion on this matter. For example, Nickel (1997) and Nunziata (2002) find no significant effect. However, by reducing turnover, the job prospects of those relatively weakly attached to the labour market, such as young workers, have found to be compromised (OECD 2004).

In this chapter, a high degree of EPL is found to be associated with lower employment prospects for young people (16 to 24) and for adults. However, the coefficient is insignificant for young people and only slightly significant for adults.

The third measure we use as a control is a measure of the generosity of unemployment insurance programs. The summary measure constructed by OECD is defined as the average of the gross unemployment benefit replacement rates as a percentage of earnings.

Relatively high unemployment benefits that are available for a relatively long duration can have adverse effects on labour market performance, by reducing the job-search intensity or by lowering the economic cost of unemployment. In this chapter and in accordance with most of the literature, high unemployment benefits are found to be generally associated with lower employment prospects for all groups.

The final additional measure we use as a control is union density again constructed from OECD data. Trade union density corresponds to the ratio of workers that are trade union members, divided by the total numbers in the labour force.

In theory, strong trade unions have the ability to push wages above market clearing levels, at the cost of lower employment. However, it has long been argued that, in practice, union influence on wage formation varies depending on the structure of collective bargaining. The empirical literature, however, remains inconclusive overall.

In this chapter, high union density is found to be associated with higher employment prospects for young people and adults. However, the coefficient is insignificant for adults and only slightly significant for young people. This finding agrees with some empirical studies (e.g. Boone and Van Ours (2009)), that use cross-country analysis to find negative and significant impact of union density on unemployment. However, the fact that the result largely disappears when other measures of the MW are used (not shown) suggests that the result is not robust to specification analysis and seems to break down whenever the Kaitz is not used as the MW measure and the unemployment level is not used as the demand side control. These findings prompt scepticism about the relationship between trade union density and its effects on employment.

4.5.3 Accounting for differences in MW effects in period of economic downturn and growth

In the fourth part of the analysis, we add interaction terms to distinguish between MW effects on employment in periods of economic downturn relative to periods of economic growth. Table A 4-1 (in appendix 4.A), shows for each country the years in which the dummy variable we use to distinguish between periods of growth (zero) from periods of downturn (one) is equal to one. It is essential for our identification strategy that there is variation across countries in periods of downturn. Also, it is important that countries are entering and exiting from global recessions at different times. Table A 4-1 clearly shows that countries in our sample are facing different periods of economic downturn and it also shows how diverse is the time span of global recessions across countries.

One of the main motivating factors at the outset of this investigation is the extent to which the effects of the MW on employment may or may not be influenced by their timing and interrelation with the prevailing macroeconomic conditions. In the methodology section we describe how these conditions – or more specifically – how a recession, can be measured. Here we adopt the logic of equation (4.2) from our methodology (the logic of this specification is spelt out in more detail in the methodology section). We present our results of this investigation in table 4-3. In the first columns are the results for young people and in the second are the results for Adults.

Looking first at the left hand side of this table and concentrating on the results for young people there is clearly a significant interaction coefficient (which is positive for

GDP growth). In other words the effect of the MW is largely negative on employment for young people but so is the interaction between the MW and the level of the demand shock. Thus the negative effect of the MW is quite large both in terms of its direct impact and its indirect impact via the macroeconomic position.

To facilitate interpretation of the results, in table 4-5 we present some simulations to indicate the size of the effect of the Kaitz index under various cyclical circumstances. In table 4-5 we report (respectively by column) the simulated effect on the elasticity of labour demand with respect to employment when GDP growth is at a minimum of -0.12%, at -0.01%, at 0.0%, at the mean of +0.03% at +0.04% and at the maximum value of +0.11%. The estimated elasticity when GDP growth is equal to the average over the recessionary period (of -0.01% growth) (shown in column 2 and evaluated at the sample means) is equal to -0.26 to -0.41. However, the estimated elasticity varies from -0.510 when GDP growth reaches its sample lowest level (-0.12) to -0.079 when GDP growth reaches its sample highest level (+0.11). Clearly these simulations suggest that the negative employment effect of the MW on young people is around 40% worse in times of recession than when there is average growth (of +0.03%) in the economy. This is a sizeable interaction effect.

In table 4-4, panel A, column 1 and 2, we test the robustness of our results using other demand side controls. When we use the level of the adult unemployment, again a significant interaction coefficient emerges (which is negative consistently with the positive interaction term for GDP growth). When we focus on our dummy for downturn, the coefficient of the interaction term is negative in line with the other demand side controls but insignificant. However, the effects of the interaction between the depth of the recession and the MW are understandably more clearly estimated when one uses a continuous measure of GDP growth rather than the dichotomous measure of the recessionary 0 or 1 indicator.

In table 4-4, panel B, column 1 and 2, we concentrate on other MW measures and we see that the presence of an interaction effect is confirmed for the Percentile, column 2, while it is less significantly so for the MW divided by GDP per head in column 1.

Now looking at the right hand side of table 4-3 we can see that the results for adults are in sharp contrast. Here it would appear that there is no evidence of any interaction effect of the MW with the aggregate demand variable. This is also true, no matter which demand side control we use, table 4-4, panel A, columns 3 and 4 and no matter which

MW measure we use, table 4-4, panel B, column 3 and 4. This suggests that for adults there are no differences in the effect of the MW over the economic cycle.

4.5.4 Examining the endogeneity of the MW

One clear and important concern with all our results relating to the effect of the MW on employment, and indeed the whole literature to date, is that the level of the MW is directly or indirectly a decision variable of the government (or its delegated authority) in power in a given country. Hence, one would expect the government's decision on whether or not to raise the MW to be influenced by what was happening to employment and unemployment. To the extent that this happens the MW in any regression (and in any form on the right hand side of a regression) is endogenous. That is to say, it is hard to argue that it would be independent of the excluded error term. The standard applied econometrician's tool when faced with this problem in the context of non-experimental data is to seek an IV which is correlated with the MW and independent of the stochastic term in the employment equation. The logic of this approach and our motivation is provided in section 4.4 above with the econometric details. In this section we seek to describe the results of our IV estimation.

We repeated our analysis using an instrument for the MW variable, the Schmidt index, that ranks countries from 1960 to 2008 by cabinet composition. More specifically, the indicator ranges from 1 to 5 where higher values indicate an hegemony of social-democratic and left wing parties and lower values indicate dominance of right wing parties[38]. The computation of the Schmidt index is based on the share of social democratic and other left wing parties compared to total cabinet posts, weighted by days. The Schmidt index is then defined as:

1. hegemony of right-wing (and centre) parties (share of left parties=0)
2. dominance of right-wing (and centre) parties (share of left parties<33.3)
3. balance of power between left and right (33.3<share of left parties<66.6)
4. dominance of social-democratic and other left parties (share of left parties>66.6)
5. hegemony of social-democratic and other left parties (share of left parties=100).

We expect the Schmidt index to be positively correlated with the Kaitz Index (countries where left wing parties dominate are the countries where the MW cuts the most into the wage distribution) and uncorrelated with the error term. Figure 4-7 shows what our instrument looks like across countries and years. It clearly demonstrates lots of

[38] The index is taken from the CPDS (Comparative Political Dataset) of the Institute of Political Science of the University of Berne, Switzerland.

variability across countries and within countries over time. This variability is related to the desire to see a lower level of inequality and hence this could be reflected in what happens to the MW. Tables B 4-5 and B 4-6 in appendix B show the average and standard deviation of the Schmidt index by country and by year.

In table 4-6 we report instrumental variable results for our different age groups using the Schmidt index as an instrument for the MW. The first thing to report is that our IV in the first stage regressions is always positively significant – implying that the more left wing a government is the higher will be the "bite" of the MW. This is consistent with our initial hypothesis.

Moving to the examination of the coefficient on the MW variable we see that when we use the IV estimation procedure the coefficient for the Kaitz is still negatively significant for young people, column 1. In contrast, for adults the MW variable is now insignificant, column 2.

If we concentrate on young people, where the effect of the MW is negative and significant similarly to the OLS regression, the estimated elasticity (shown in the bottom row of table 4-6 and evaluated at the sample means) is -0.368, larger than the elasticity for the OLS estimation in table 4-1.

The potential problems of applying the IV procedure is that the IV may be weak and that the two stage least squares standard errors have a tendency to be larger than the FE standard errors. This imprecision in the estimates is, of course, directly related to the strength of the correlation between the IV and the MW variable. This may (or may not be) an acceptable price to pay for tackling the endogeneity and being assured of consistent estimates. Our use of IV methods and the issue is no different from any other study which has used this identifications strategy. In particular, our standard errors reported in our instrumental variable results in table 4-6 are larger than standard errors reported for our FE estimation in table 4-1. However, in order to support our argument, we report in table 4-6 that our IV passes the conventional test associated with weakness of instruments. Specifically, we generally have an F test statistics which indicates that the IV in question is not a weak instrument. However, overall, our conclusion is that since our IV results give us the same conclusion as our FE results then we may be cautiously optimistic that the MW effect we estimate is robust.

Tables 4-7, panel A and B, show that however one chooses to measure the MW or the recession it is the case that the IV estimates of the MW impact on employment is robustly negative and larger than the corresponding estimated effect when the MW measured is not instrumented. It should be stressed that this effect is only apparent for young people and not adults

We suggest that this result is an important contribution to the understanding of modelling the effect of the MW on employment. These results confirm much of the evidence which suggests that the negative effects, where they exist, are clearly important for young people on the margins of employment, but possibly they are insignificant for adults. This accords with the basic fact that, in most countries, the MW affects less than 5% of people and most of these will not be adult workers.

4.6 Conclusions and policy implications

The main purpose of this chapter was to exploit the substantial differences across countries in relative MW levels to obtain new estimates of the employment effects of the MW. Even though an important source of variation is provided by the large differences across countries in the MW relative to average wages, relatively few studies have tested such propositions directly. We also have been able to account for institutional and other policy-related differences that might have an impact on employment other that the MW.

The chapter examined whether the MW has any effect on employment using panel data on 33 countries over the 1971-2009 period. We examined our data to compare it with an earlier study over a shorter period of time with fewer countries.

The main finding of this chapter – which comes directly from our analysis, is that there are significant negative employment effects of MW rises for young people – but that there are less significant negative employment effects for adults – which are only found clearly when one uses the alternative measures of the "bite" of the MW. It would also appear that there are important additional interaction effects of these policies for young people in times of recession. It is worth stressing that these negative employment effects are now found over a much wider set of 33 countries and a much longer period of time – namely 39 years - than previously in the literature. These advances make our conclusions much more generally valuable. It is also worth stressing that these negative employment effects are generally invariant to how one measures recessions and how one measures the level of the MW. However, the effects of the interaction between the depth of the recession and the MW are understandably more clearly estimated when one uses a continuous measure of GDP growth rather than the dichotomous measure of the recessionary zero or one indicator.

An important component of our analysis has been to raise the issue of the potential endogeneity of the MW variable in the standard approach to estimating an employment equation. If one considers that setting or (up-rating) a MW is a choice variable for a government then it is likely to be partially determined by what happens to employment. In this sense – one cannot reasonably assume that the variable which measures the MW is a valid exogenous variable to be included on the right hand side of such an equation. This problem was tackled in this chapter by the use of an Instrumental Variable (IV) identification strategy. This involved using the *de facto* degree of right or left wing political orientation of each country's government as an IV for the MW. In short, we used a 'political complexion of the government' instrumental variable – the Schmidt Index. The motivation was that the electorate will determine the nature of the government and how right or left wing it is. This process will not be driven by the employment rate but the aggregation of the political preferences of voters. However the political complexion of the government will have a direct bearing on how sympathetic the government is towards setting a MW or how generous it is towards uprating it. Specifically, left wing governments are usually much more interested in low pay and the distribution of income in the economy. We establish that (using the conventional tests) this Schmidt Index variable is a valid IV. Using this IV we find that the MW again has a negative impact on employment of young people. This is quite a remarkable and yet simple result. It suggests that there is indeed considerable, time varying, unobserved heterogeneity in the determination of employment and that the Kaitz index is itself endogenous to these processes which are at a country specific level. However if one uses the Schmidt index as an IV we essentially purge ourselves of this endogeneity and reveal the true underlying relationship between the MW and employment. Our examination of the wider set of evidence with robust econometric methods has given a clearer consensus of the negative effect of the MW on employment for young people.

A number of interesting, slightly more methodological, conclusions can be drawn. Firstly, that cross country regressions, even using good panel data, can be plagued by unobserved heterogeneity which gives rise to endogeneity problems. Secondly, that the Kaitz Index, in this context may well be an endogenous variable and that if one takes account of this endogeneity one gets reassuringly stable IV. Finally, most substantively, it would appear that the conclusions regarding the employment effects of the MW are very different for adults and young people. Our analysis was conducted separately for these two groups and we found that when we used the IV identification strategy that there was a clear negative employment effect of MW for young people, but that this effect was not present with adults. It would appear that the most vulnerable groups in the labour market, whose wages are 'closest' to the MW, are most affected by it. One should also be clear that our results suggest that not only does a MW have a negative effect on the employment of young people but that this effect is clearly magnified by a huge margin when there is an economic recession. Overall our elasticity simulation results suggest that over the 2008-9 recessionary period when average growth was only -.01% then a 10% increase in the MW could reduce employment by as much as 2.6-4.1%. This sizeable impact is not mirrored in adult employment.

This leads us to the important problem of possible policy conclusions. The immediate response is that – the literature may well have come full circle and it looks possible that we are back to 'as you were' – with the possibility that higher Minimum Wages could have harmful aggregate employment consequences and that this effect is most clearly delineated for young people rather than adults. This would be a boost for conventional economic thinking but one must hesitate to reach too strident policy inferences from a single set of results. Clearly, more studies of the MW are required that address the possible endogeneity of the MW in an employment equation. In addition, studies are required which will explain the differences in these cross country results and those found in microeconomic datasets where the identification comes from cross time and geographical location variation (see Card and Krueger (1994), Stewart (2002), and Dolton et al. (2012)).

The second logical area of policy implications of this research is that countries that have not already adopted a separate youth MW rate for young people should consider doing so. Our results would suggest that this is a good idea and that in times of recessions it may be prudent not to raise this young people's MW. Further our results would suggest that with separate MW rates for adults and young people – those relating to young people should not be raised if those for adults are raised.

To return to the question which began our motivation. What should a government do with the MW in recessionary times. Unfortunately economic theory cannot provide a single, clear-cut answer. So our only guidance must come from the best empirical evidence that is available. It would appear that raising the MW will, in all probability, lead to a reduction of employment for young people – but it would reduce wage inequality. What happens is that for those who are on low pay – but stay in work when the MW rises – they will be better off - and hence inequality will be reduced. But equally, firms who are at the margin of profitability will try to shed labour (or hire less labour) when the MW rises and so a rising MW – especially in recessionary - times will mean lower employment.

So this means there is a price to be paid by those who are at the margins of employment – like young people – will be more likely to lose their jobs. Hence there is a clear trade-off in choosing to raise the MW. Each government faces the dilemma of raising the MW and reducing inequality and accepting that this will reduce employment levels amongst young people. Things are potentially worse in times of recession for precisely that group and so at the time of writing, governments need to be very careful when raising the MW – especially in these recessionary times. It is possible that in recessionary times, a fixed MW will actually be a rising real level of the MW (assuming that there is deflation) or a rising level of the relative MW compared to average wages. Therefore, the best policy may be, to leave the MW fixed in nominal terms. This means that the government does not face the political storm of a reduction of the MW at a politically sensitive time, but at the same time a falling real value of the MW will not have too adverse a consequence on the employment of young people. In contrast, if the MW is raised at a time of deflation, this would of course generally create a positive effect on inequality but it is likely that this would have a considerable effect on the employment of levels of young people – partly due to the direct effect of the MW level and partly because of the effect of the interaction between the recession and the MW. Hence, in recessionary times, leaving the MW constant in nominal terms (and perhaps thereby falling in real terms) may be the prudent option.

Figure 4-1. MW and movements of the aggregate demand

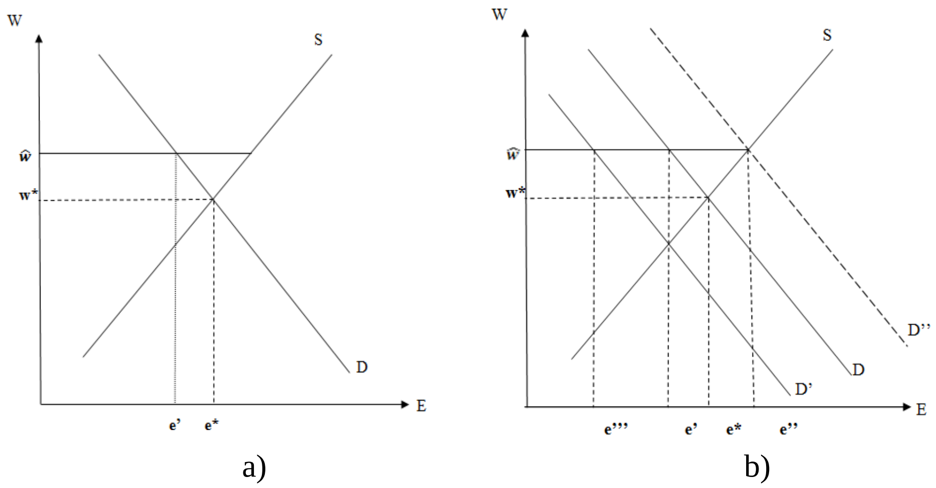
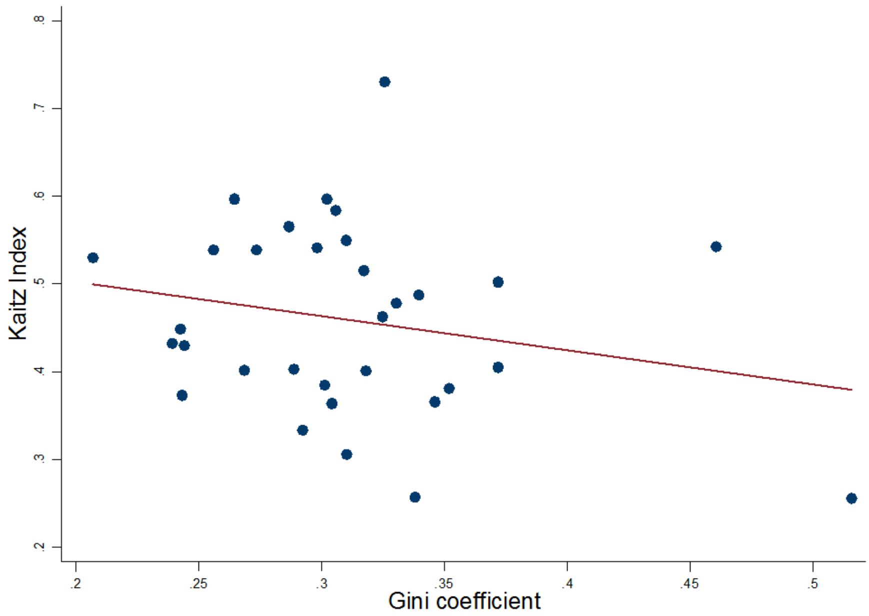


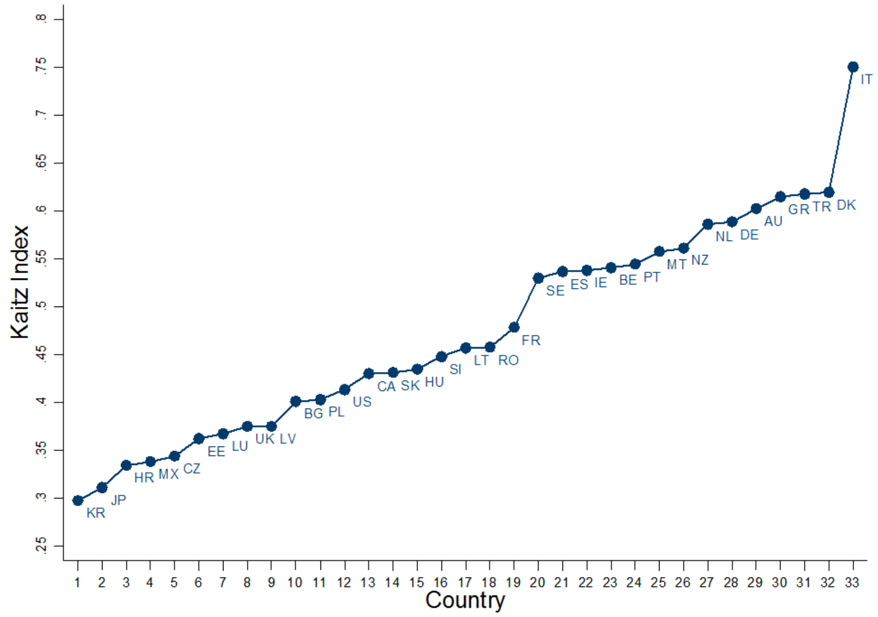
Figure 4-2. Kaitz index and Gini coefficient



Source: Kaitz Index, OECD Minimum Wage database; Gini coefficient, UNU WIDER, World Inequality Database.

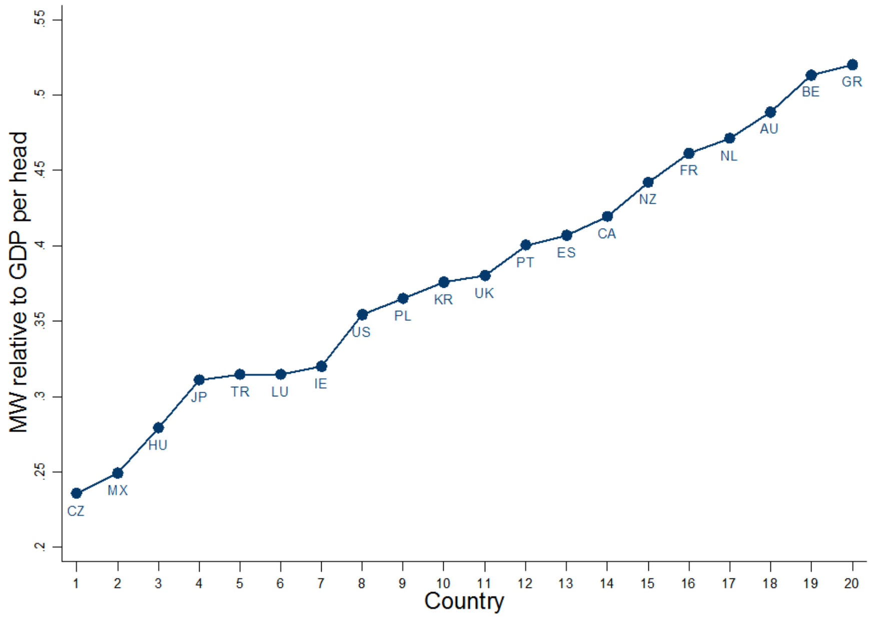
Figure 4-3. MW measures ranked across countries
(for each country, mean of the MW measure across years in the panel)

a. Kaitz Index



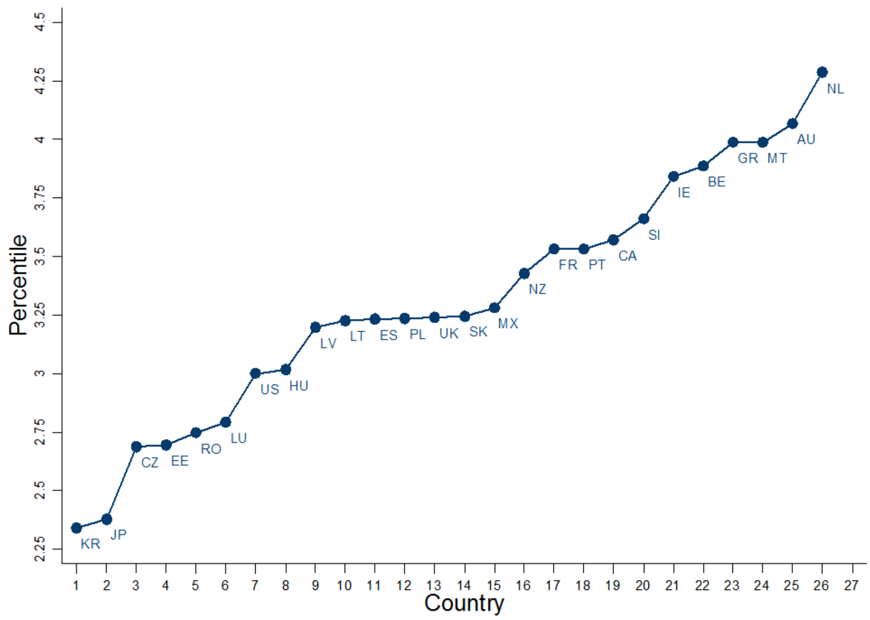
Sources: OECD MW database and EUROSTAT.

b. MW relative to GDP per head



Source: OECD, authors' calculations.

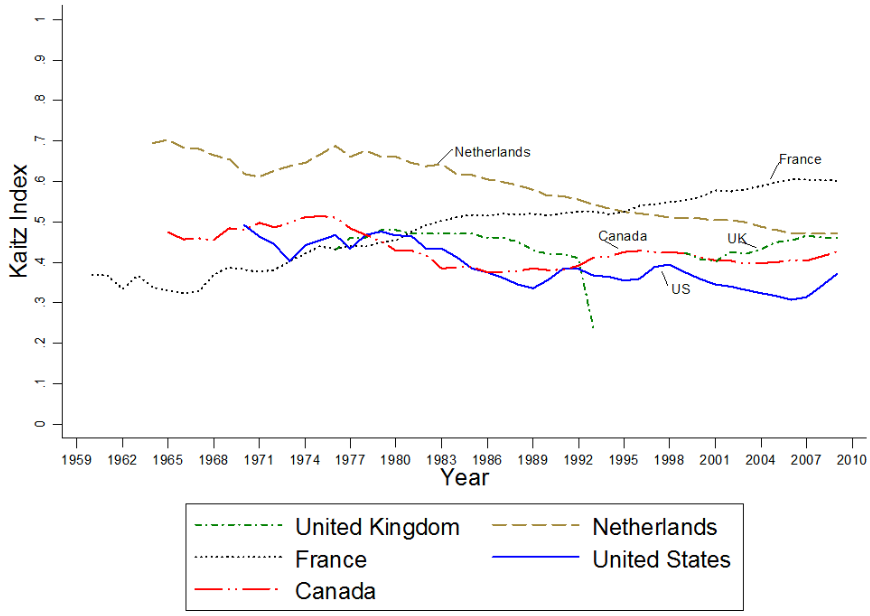
c. Percentile at which the MW "bites"



Source: OECD and UNU WIDER, World Inequality Database. Authors' calculations.
Notes: Australia AU, Belgium BE, Bulgaria BG, Canada CA, Croatia HR, Czech Republic CZ, Denmark DK, Estonia EE, France FR, Germany DE, Greece GR, Hungary HU, Ireland IE, Italy IT, Japan JP, Korea KR, Latvia LV, Lithuania LT, Luxembourg LU, Malta MT, Mexico MX, Netherlands NL, New Zealand NZ, Poland PL, Portugal PT, Romania RO, Slovak Republic SK, Slovenia SI, Spain ES, Sweden SE, Turkey TR, United Kingdom UK, United States US.

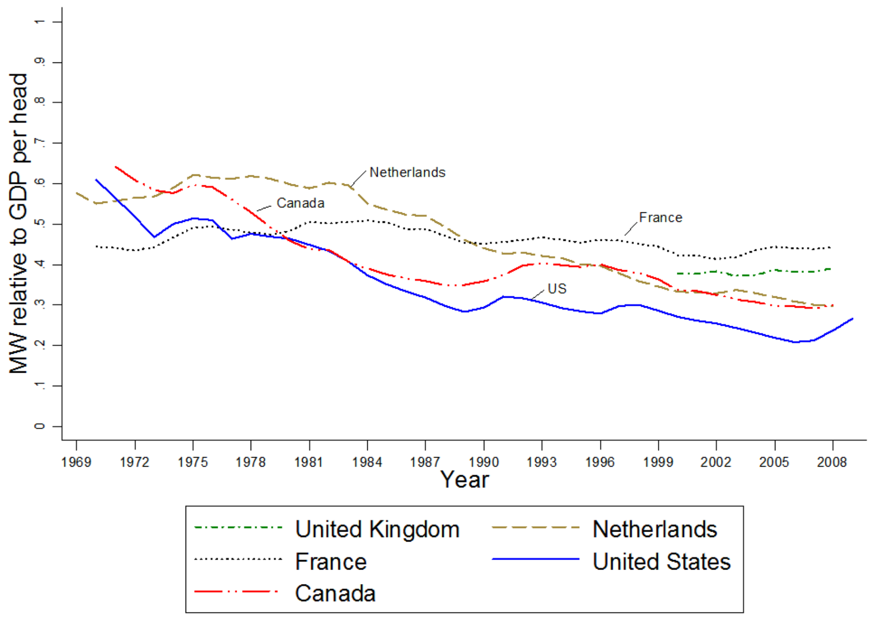
Figure 4-4. MW measures across countries and year

a. Kaitz Index



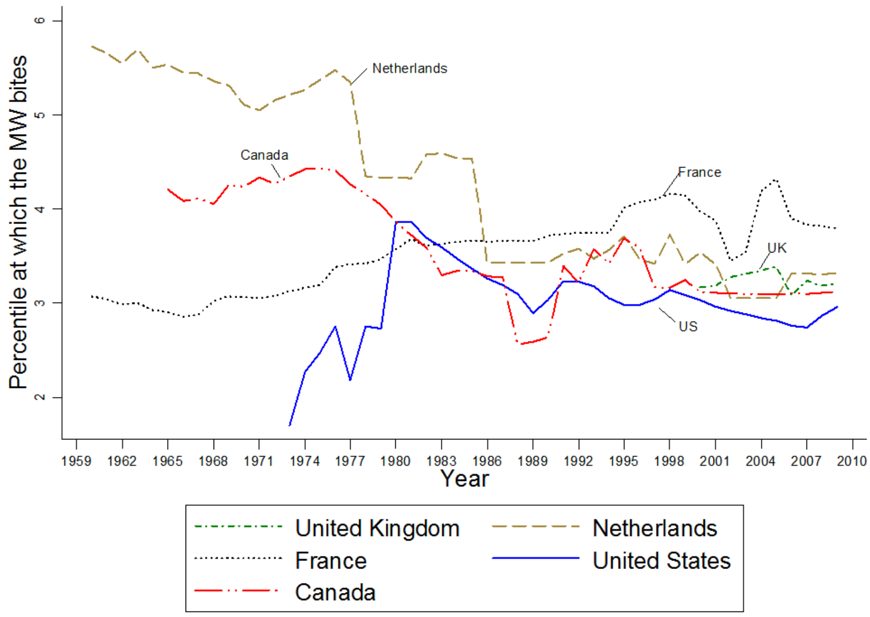
Sources: OECD, MW database. Kaitz index relative to the median earnings.
For UK before 1994, source: Dolado et al. (1996), Kaitz index relative to average earnings.

b. MW relative to GDP per head



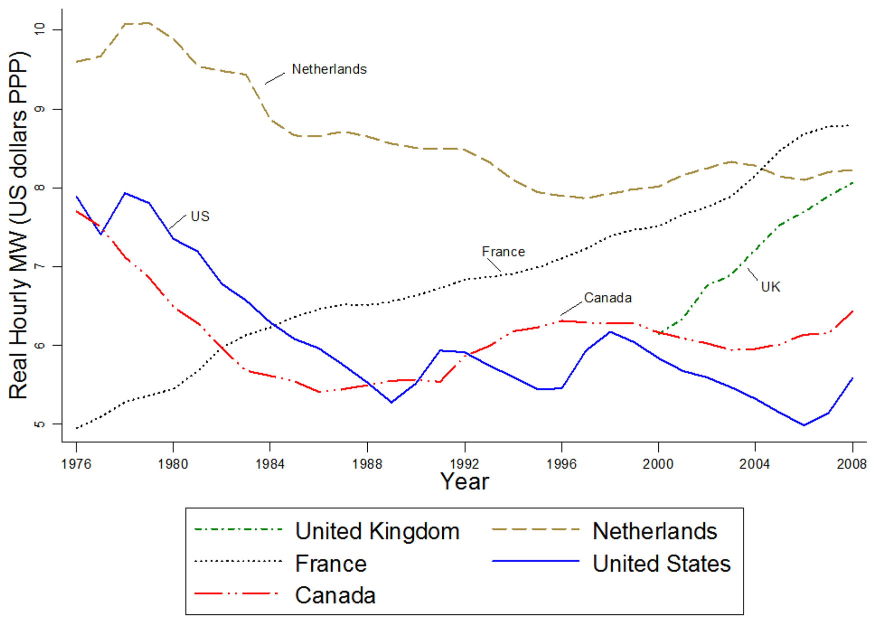
Source: OECD, authors' calculations.

c. Percentile at which the MW "bites"



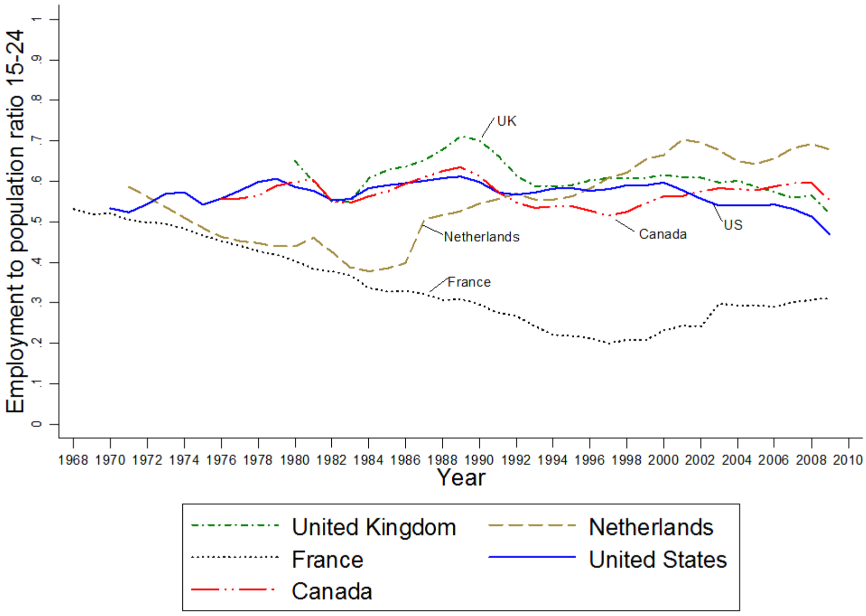
Sources: OECD and UNU WIDER, World Inequality Database. Authors' calculations.

Figure 4-5. Real Hourly MW (US dollars PPP) across countries and years



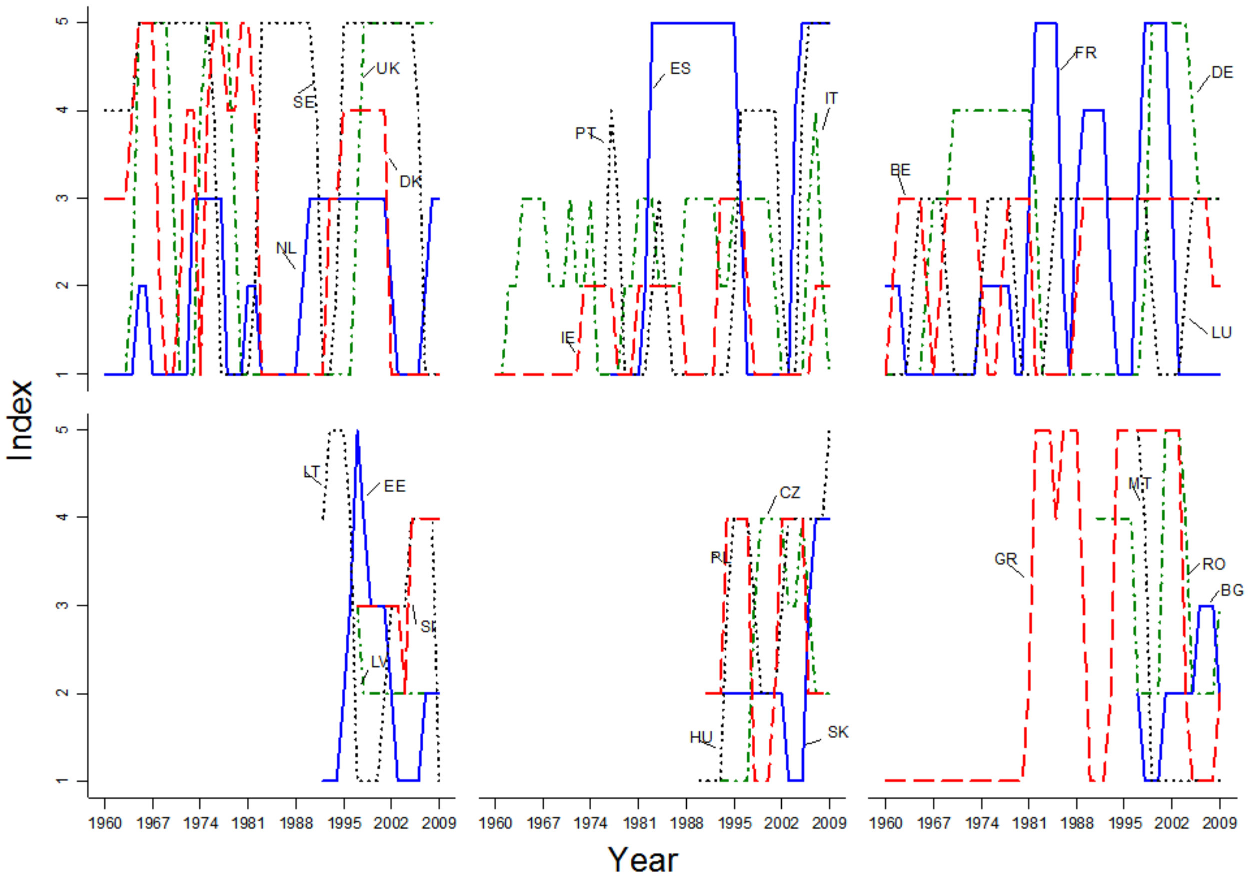
Source: OECD

Figure 4-6. Employment to population ratio (15-24) across countries and years



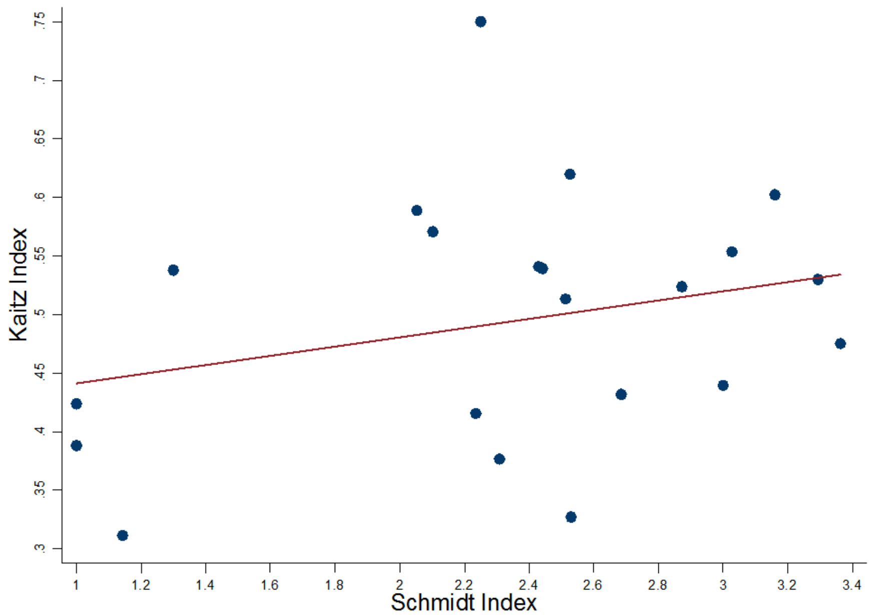
Source: OECD

Figure 4-7. Schmidt Index across countries and years



Source: CPDS (Comparative Political Dataset), Institute of Political Science of the University of Berne, Switzerland.

Figure 4-8. Kaitz Index and Schmidt Index



Source: OECD Minimum Wage database. CPDS (Comparative Political Dataset), Institute of Political Science of the University of Berne, Switzerland.

Table 4-1. Fixed effects estimates of the MW model, Kaitz Index

Variable	*(1)* *FE*	*(2)* *FE*
	Youth (16-24)	***Adults (25-64)***
Kaitz Index (L.)	-0.200***	-0.067***
	(0.043)	(0.020)
GDP growth (L.)	0.245**	0.216***
	(0.099)	(0.058)
Rel. pop.	-0.372**	0.379***
	(0.167)	(0.082)
Bargained Min.	0.077**	0.018**
	(0.016)	(0.007)
Youth Submin.	-0.006	-0.036***
	(0.022)	(0.011)
Act. Policies	-0.021***	-0.006*
	(0.008)	(0.003)
Empl. Prot.	-0.011	-0.010*
	(0.012)	(0.006)
Repl. Rate	-0.076***	-0.014***
	(0.008)	(0.005)
Union Density	0.014*	0.003
	(0.008)	(0.004)
Observations	573	573
R-squared	0.893	0.898
MW Elasticity	**-0.214**	**-0.046**
Years Effects	Y	Y
Country Effects	Y	Y

*Note*s: HAC robust fixed effect estimates in brackets.

The 23 countries included in the analysis are Australia, Belgium, Canada, Czech Republic, Denmark, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Korea, Luxembourg, New Zealand, Netherlands, Poland, Portugal, Slovakia, Spain, Sweden, UK and US.

The sample period is from 1971 to 2009, except for the following countries: Spain (1972-2009); Japan, Portugal (1975-2009); Canada (1976-2009); Germany (1976-1994); Italy (1976-1991); Sweden (1976-1992); Denmark (1983-1994); Belgium, Greece, Luxembourg (1983-2009); United Kingdom (1984-2009); Australia (1985-2009); New Zealand (1986-2008); Korea (1988-2009); Hungary, Poland (1992-2009); Czech Republic (1993-2009); Slovak Republic (1993-2009); Ireland (2000-2009).

*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 4-2. Robustness checks, Fixed effects estimates of the MW model (alternative demand side controls and MW measures)

Variable	*(1)* *FE*	*(2)* *FE*	*(3)* *FE*	*(4)* *FE*
	Youth (16-24)		***Adults (25-64)***	
		Panel A		
Kaitz Index (L.)	-0.205***	-0.230***	-0.071***	-0.061***
	(0.043)	(0.031)	(0.020)	(0.015)
	Downturn (L.)	*Adult unempl.rt*	*Downturn (L.)*	*Youth unempl.rt*
Demand control	-0.003	-1.761***	-0.006***	-0.340***
	(0.005)	(0.094)	(0.003)	(0.021)
Observations	573	573	573	573
R-squared	0.892	0.936	0.895	0.934
MW Elasticity	**-0.220**	**-0.247**	**-0.049**	**-0.042**
		Panel B		
	MW/GDPperhead	*Percentile*	*MW/GDPperhead*	*Percentile*
MW measure (L.)	-0.397***	-0.227***	-0.337***	-0.152***
	(0.048)	(0.042)	(0.020)	(0.017)
Observations	475	485	475	485
R-squared	0.883	0.876	0.925	0.891
MW Elasticity	**-0.364**	**-0.176**	**-0.195**	**-0.073**
Years Effects	Y	Y	Y	Y
Country Effects	Y	Y	Y	Y

*Note*s: HAC robust fixed effect estimates in brackets.
Panel A: See table 4-1.
Panel B: Results using MW/GDP per head as MW indicator: differently from table 4-1: data only goes up to 2008; Germany, Sweden, Italy and Denmark data is missing; UK data goes from 2000 to 2008; Portugal data goes from 1974 to 2008.
Results using Percentile as MW indicator: Differently from table 4-1: data on Germany, Sweden, Italy and Denmark is missing; UK data goes from 2000 to 2009; US data from 1973 to 2009.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 4-3. Differences in MW effects by periods of growth, Kaitz Index

Variable	*(1)* *FE*	*(2)* *FE*
	Youth (16-24)	***Adults (25-64)***
Kaitz Index (L.)	-0.264***	-0.070***
	(0.051)	(0.024)
GDP growth (L.)	-0.553	0.178
	(0.345)	(0.166)
*Kaitz*GDP growth (L.)*	1.789**	0.084
	(0.734)	(0.359)
Observations	573	573
R-squared	0.894	0.898
Years Effects	Y	Y
Country Effects	Y	Y

*Note*s: HAC robust fixed effect estimates in brackets. See table 4-1.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 4-4. Robustness checks, differences in MW effects by periods of growth (alternative demand side controls and MW measures)

Variable	*(1)* *FE*	*(2)* *FE*	*(3)* *FE*	*(4)* *FE*
	Youth (16-24)		***Adults (25-64)***	
		Panel A		
Kaitz Index (L.)	-0.200***	-0.160***	-0.071***	-0.087***
	(0.043)	(0.056)	(0.020)	(0.027)
	Downturn (L.)	*Adult unempl.rt*	*Downturn (L.)*	*Youth unempl.rt*
Demand control	0.019	-1.154***	-0.006	-0.430***
	(0.021)	(0.363)	(0.011)	(0.087)
*Kaitz*Demand contr.*	-0.046	-1.298*	0.001	0.189
	(0.045)	(0.778)	(0.023)	(0.167)
Observations	573	573	573	573
R-squared	0.892	0.936	0.895	0.935
		Panel B		
	MW/GDPperhead	*Percentile*	*MW/GDPperhead*	*Percentile*
MW measure (L.)	-0.426***	-0.305***	-0.330***	-0.175***
	(0.053)	(0.060)	(0.022)	(0.025)
GDP growth (L.)	-0.509	-0.516	0.246	-0.014
	(0.426)	(0.444)	(0.173)	(0.204)
*MW*GDP growth (L.)*	1.764	2.507*	-0.397	0.763
	(1.107)	(1.313)	(0.406)	(0.592)
Observations	475	485	475	485
R-squared	0.884	0.877	0.926	0.891
Years Effects	Y	Y	Y	Y
Country Effects	Y	Y	Y	Y

Notes: HAC robust fixed effect estimates in brackets.
Panel A: see table 4-2, panel A
Panel B: see table 4-2, panel B
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 4-5. Simulations for young people, MW effects under various cyclical circumstances

	GDP growth					
	Min	2008-2009	0	Mean	>=0	Max
	- 0.12	**-0.01**	**0.00**	**+ 0.03**	**+0.04**	**+ 0.11**
Kaitz Index (L.)	-0.477	-0.281	-0.264	-0.210	-0.202	-0.074
Elasticity	-0.510	-0.301	-0.283	-0.225	-0.216	-0.079
Percentile(L.)	-0.603	-0.329	-0.305	-0.226	-0.215	-0.038
Elasticity	-0.468	-0.256	-0.237	-0.175	-0.167	-0.029
MW/GDPperhead (L.)	-0.635	-0.444	-0.426	-0.372	-0.364	-0.238
Elasticity	-0.583	-0.407	-0.390	-0.341	-0.334	-0.218

Table 4-6. IV estimation, Schmidt index as an instrument for the Kaitz Index

Variable	*(1)* *FE*	*(2)* *FE*
	Youth (16-24)	***Adults (25-64)***
First stage	0.014***	0.011***
Schmidt Index	(0.004)	(0.004)
Second stage		
Kaitz Index	-0.381***	0.073
	(0.143)	(0.084)
GDP growth (L.)	0.117	0.192***
	(0.088)	(0.061)
Observations	549	549
Hausman p value	0.368	0.052
F-test	14.879	10
MW Elasticity	**-0.407**	**0.051**
Years Effects	Y	Y
Country Effects	Y	Y

Notes: HAC robust fixed effect estimates in brackets. See table 4-1; Korea data is missing, Portugal data goes from 1976 to 2009.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 4-7. Robustness checks, IV estimation
(alternative demand side controls and MW measures

Variable	*(1)* *FE*	*(2)* *FE*	*(3)* *FE*	*(4)* *FE*
	Youth (16-24)		***Adults (25-64)***	
		Panel A		
First stage	0.014***	0.014***	0.012***	0.011***
Schmidt Index	(0.004)	(0.003)	(0.004)	(0.004)
Second stage				
Kaitz Index	-0.369***	-0.371***	0.088	0.191*
	(0.141)	(0.106)	(0.086)	(0.099)
	Downturn (L.)	*Adult unempl.rt*	*Downturn (L.)*	*Youth unempl.rt*
Demand control	-0.001	-1.638***	-0.006**	-0.326***
	(0.004)	(0.051)	(0.003)	(0.016)
Observations	549	549	549	549
Hausman p value	0.424	0.278	0.030	0.000
F-test	15.107	17.099	10	8.202
MW Elasticity	**-0.394**	**-0.396**	**0.061**	**0.133**
		Panel B		
First stage				
Schmidt Index	0.019***	0.011***	0.020***	0.009***
	(0.004)	(0.002)	(0.004)	(0.002)
Second stage				
	MW/GDPperhead	*Percentile*	*MW/GDPperhead*	*Percentile*
MW measure (L.)	-0.594***	-0.524**	-0.316***	-0.053
	(0.118)	(0.209)	(0.042)	(0.109)
Observations	437	467	437	467
Hausman p value	0.099	0.123	0.544	0.372
F-test	27.498	21.119	24.927	14.173
MW Elasticity	**-0.530**	**-0.407**	**-0.181**	**-0.026**
Years Effects	Y	Y	Y	Y
Country Effects	Y	Y	Y	Y

Notes: HAC robust fixed effect estimates in brackets.
Panel 1: See table 4-6
Panel 2: For Percentile regressions: see table 4-2, panel B. For MW/GDP per head regressions: see table 4-2, panel B. Korea data is missing. Portugal data goes from 1976 to 2009; Spanish data goes from 1977 to 2009.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Appendix 4.A

Table A 4-1. Years of economic downturn across countries
(years in which at least two quarters of downturn per year, 0 otherwise)

Australia	1961,1972,1974,1975,1977,1982,1983,1991
Belgium	1975, 1977, 1980, 1992, 2001, 2008
Canada	1980, 1981, 1982, 1986, 1990, 2008, 2009
Czech Rep.	1991, 1992, 1997, 1998, 2001, 2009
Denmark	1974, 1977, 1980, 1986, 1987, 1989, 1990, 1992, 1993, 1997, 2001, 2002, 2005, 2006, 2008, 2009
France	1975, 1980, 1992, 1993, 2001, 2008
Germany	1967, 1971, 1974, 1975, 1980, 1981, 1982, 1991, 1992, 1998, 2002, 2003, 2008
Greece	1962, 1974, 1975, 1977, 1978, 1979, 1980, 1981, 1982, 1983, 1984, 1986, 1987, 1989, 1990, 1991, 1992, 1994, 1995, 1999, 2009
Hungary	1985, 1988, 1990, 1991, 1992, 1993, 1995, 2007, 2008, 2009
Ireland	1966, 1983, 1985, 1986, 2003, 2007, 2008, 2009
Italy	1964, 1975, 1977, 1982, 1992, 1993, 1996, 1998 , 2001, 2003, 2008, 2009
Japan	1974, 1990, 1993, 1997, 1998, 1999, 2001, 2004, 2008, 2009
Korea	1979, 1980, 1998, 2008
Luxembourg	1967, 1970, 1974, 1975, 1980, 1981, 1992, 1995, 1996, 2008
Mexico	1982, 1983, 1984, 1985, 1986, 1988, 1995, 2001, 2008
Netherlands	1973, 1980, 1981, 1982, 2002, 2008, 2009
New Zealand	1961, 1962, 1966, 1967, 1968, 1969, 1970, 1971, 1972, 1973, 1975, 1977, 1978, 1985, 1986, 1988, 1991, 1997, 2008
Poland	1991, 2001
Portugal	1969, 1974, 1975, 1980, 1983, 1984, 1992, 2002, 2004, 2007, 2008, 2009
Slovak Rep.	1990, 1991, 1992, 1998, 1999, 2002
Spain	1975, 1978, 1981, 1992, 1993, 2008, 2009
Sweden	1965, 1966, 1968, 1971, 1976, 1977, 1980, 1981,1989, 1990, 1991,1992 2008, 2009
Turkey	1979, 1980, 1988, 1994,1998, 1999, 2001, 2008
UK	1961, 1973,1974, 1975, 1979, 1980, 1990, 1991, 2008, 2009
US	1960, 1970, 1974, 1980, 1981, 1982, 1990, 2001, 2008, 2009
Lithuania	1991, 1992, 1993, 1994, 1999, 2008, 2009
Romania	1985, 1988, 1989, 1990, 1991, 1992, 1997, 1998, 1999, 2008, 2009
Slovenia	2009
Malta	2001, 2002, 2003, 2004, 2008, 2009
Latvia	1999, 2008, 2009
Estonia	1999, 2008, 2009
Bulgaria	2009
Croatia	1999, 2008, 2009

Sources: OECD, EUROSTAT.

Appendix 4.B

Table B 4-1. Means of the main variables by country

	Empl. to pop. ratio			*MW measures*		*IV*
	Young (%)	*Adults (%)*	*Kaitz*	*Percentile*	*MW/GDP per head*	*Schmidt Index*
Australia	61.36	67.12	0.60	4.07	0.49	2.87
Belgium	28.96	64.03	0.54	3.89	0.51	2.46
Bulgaria	22.61	64.78	0.40	_	_	2.00
Canada	57.04	71.59	0.42	3.48	0.41	1.00
Croatia	25.99	62.51	0.33	_	_	_
Czech Rep.	36.48	74.52	0.34	2.69	0.24	2.53
Denmark	64.54	77.66	0.62	_	_	2.54
Estonia	30.38	73.68	0.36	2.70	_	2.11
France	32.80	69.99	0.51	3.68	0.46	2.51
Germany	52.78	67.78	0.59	_	_	2.82
Greece	27.29	63.35	0.57	3.81	0.52	3.03
Hungary	27.97	62.52	0.43	3.02	0.28	3.00
Ireland	44.49	61.60	0.54	3.84	0.32	1.62
Italy	29.59	59.73	0.75	_	_	2.26
Japan	43.46	74.43	0.31	2.38	0.31	1.13
Korea	31.79	70.56	0.30	2.34	0.38	_
Latvia	32.28	70.67	0.38	3.20	_	2.08
Lithuania	25.18	72.38	0.46	3.23	_	3.06
Luxembourg	38.91	65.80	0.38	2.86	0.31	2.31
Malta	47.66	56.29	0.56	3.99	_	1.54
Mexico	47.16	65.25	0.34	3.28	0.25	_
Netherlands	54.98	64.27	0.57	3.95	0.47	2.10
New Zealand	57.38	74.38	0.52	3.43	0.44	2.87
Poland	25.43	64.93	0.40	3.23	0.37	2.68
Portugal	48.13	70.71	0.54	3.53	0.40	2.44
Romania	29.58	70.18	0.45	2.75	_	3.26
Slovak Rep.	29.66	68.31	0.43	3.25	_	2.24
Slovenia	34.08	72.53	0.45	3.66	_	3.31
Spain	39.94	58.39	0.51	3.23	0.40	3.36
Sweden	54.58	81.18	0.53	_	_	3.74
Turkey	37.73	54.44	0.61	_	0.31	_
UK	60.89	72.61	0.37	3.24	0.38	2.82
US	56.82	72.97	0.39	3.00	0.35	1.00

Sources: OECD, EUROSTAT, Dolado et al. (1996), UNU WIDER, World Inequality Database, CPDS (Comparative Political Dataset), Institute of Political Science of the University of Berne, Switzerland.

Table B 4-2. Means of the dependent variables by country

	ALMP	*EPL*	*RR*	*UD*	*Adult unempl.*	*Youth unempl.*	*GDP growth*	*Rel. size pop.*
Australia	0.38	1.03	23.28	37.0	4.77	12.48	0.03	23.84
Belgium	1.16	2.82	42.28	52.3	7.36	19.59	0.02	19.91
Bulgaria	_	_	_	_	10.61	24.56	0.04	19.85
Canada	0.51	0.75	17.10	32.2	7.19	14.18	0.03	22.80
Croatia	_	_	_	_	9.70	29.63	0.03	19.43
Czech Rep.	0.18	1.91	6.00	39.2	5.44	13.62	0.02	21.18
Denmark	1.32	2.05	50.67	73.7	5.78	10.00	0.02	19.59
Estonia	_	2.10	_	_	8.63	18.93	0.05	21.65
France	0.83	2.89	34.21	13.0	6.81	18.47	0.02	22.37
Germany	0.87	2.84	27.62	29.9	6.25	7.67	0.02	19.91
Greece	0.23	3.39	10.26	31.3	6.77	26.38	0.03	19.58
Hungary	0.50	1.32	13.00	39.4	6.99	17.24	0.02	20.67
Ireland	1.00	0.96	28.69	45.9	8.92	16.28	0.05	26.35
Italy	0.61	3.10	13.69	40.3	5.54	26.21	0.02	21.39
Japan	0.31	1.71	9.87	26.2	2.74	5.67	0.03	20.55
Korea	0.33	2.51	9.85	13.4	2.85	9.22	0.07	24.35
Latvia	_	_	27.00	_	10.31	20.03	0.05	21.92
Lithuania	_	_	_	_	10.24	21.31	0.01	22.12
Luxembourg	0.34	3.25	_	45.6	2.39	8.30	0.04	18.54
Malta	_	_	_	_	4.68	15.36	0.02	21.45
Mexico	_	3.13	_	21.4	2.69	6.73	0.03	32.24
Netherlands	1.34	2.55	48.62	27.6	4.78	9.94	0.02	22.21
New Zealand	0.71	1.01	28.67	42.6	4.67	12.87	0.02	23.11
Poland	0.28	1.49	10.94	41.5	11.93	31.07	0.04	21.58
Portugal	0.40	3.90	26.10	37.4	4.75	13.93	0.03	23.51
Romania	_	_	_	_	5.48	19.38	0.01	22.41
Slovak Rep.	0.31	1.72	11.38	51.1	12.25	28.54	0.02	23.74
Slovenia	_	2.51	_	_	5.17	14.81	0.03	19.84
Spain	0.53	3.48	30.87	12.5	10.68	27.16	0.03	21.39
Sweden	1.95	2.95	27.54	78.0	3.75	11.63	0.02	18.69
Turkey	_	3.75	9.10	16.4	6.54	17.38	0.04	29.70
UK	0.55	0.63	19.51	38.3	6.16	13.97	0.02	19.05
US	0.21	0.21	13.00	17.3	4.74	12.65	0.03	22.03

Notes: Active labour market policies (ALMP), employment protection legislation (EPL), replacement rate (RR), union density (UD)
Sources: OECD and EUROSTAT.

Table B 4-3. Means of the main variables by year

	Empl. to pop. ratio		*MW measures*			*IVs*
	Young (%)	*Adults (%)*	*Kaitz_median*	*Percentile*	*MW/GDP per head*	*Schmidt Index*
1971	56.41	63.84	0.56	3.81	0.51	1.80
1972	55.64	64.06	0.55	3.76	0.51	2.00
1973	55.42	64.79	0.59	3.55	0.48	2.60
1974	56.01	65.16	0.58	3.63	0.53	2.63
1975	53.79	63.70	0.56	3.96	0.55	2.63
1976	53.17	65.12	0.55	3.60	0.54	2.24
1977	52.53	64.72	0.55	3.58	0.53	2.17
1978	51.84	66.60	0.55	3.57	0.52	1.94
1979	52.44	66.17	0.54	3.54	0.50	1.67
1980	51.77	67.45	0.52	3.59	0.49	1.56
1981	50.65	66.32	0.52	3.66	0.49	1.89
1982	48.47	66.72	0.51	3.55	0.49	2.06
1983	46.38	64.96	0.51	3.61	0.48	2.28
1984	46.48	64.89	0.51	3.59	0.46	2.50
1985	46.73	65.16	0.52	3.61	0.46	2.56
1986	48.18	66.09	0.52	3.57	0.45	2.39
1987	49.06	66.84	0.52	3.65	0.44	2.33
1988	48.89	67.00	0.50	3.42	0.42	2.50
1989	49.27	67.44	0.47	3.40	0.39	2.56
1990	48.67	68.05	0.47	3.38	0.37	2.37
1991	47.81	67.73	0.48	3.26	0.37	2.24
1992	44.93	67.30	0.46	3.03	0.38	2.13
1993	42.89	66.99	0.44	3.34	0.37	2.24
1994	42.34	67.14	0.42	3.25	0.35	2.52
1995	42.28	67.61	0.40	3.27	0.34	2.84
1996	41.98	68.04	0.40	3.19	0.34	2.72
1997	41.43	68.89	0.41	3.14	0.33	2.66
1998	41.10	69.06	0.40	3.18	0.33	2.59
1999	40.66	69.34	0.41	2.88	0.33	2.55
2000	40.40	68.79	0.42	3.11	0.33	2.62
2001	39.74	68.98	0.43	3.16	0.33	2.79
2002	38.67	68.94	0.44	3.10	0.33	2.59
2003	37.81	69.20	0.44	3.23	0.33	2.38
2004	37.48	69.60	0.45	3.23	0.33	2.34
2005	37.42	70.21	0.45	3.21	0.33	2.52
2006	38.01	71.15	0.45	3.24	0.32	2.55
2007	38.61	71.93	0.45	3.22	0.32	2.62
2008	38.53	72.26	0.45	3.26	0.33	2.62
2009	35.42	70.62	0.47	3.33	0.39	2.48

Sources: OECD, EUROSTAT, Dolado et al. (1996), UNU WIDER, World Inequality Database, CPDS (Comparative Political Dataset), Institute of Political Science of the University of Berne, Switzerland.
The sample period is from 1971 to 2009, except for the following countries: Spain (1972-2009); Japan, Portugal (1975-2009); Canada (1976-2009); Germany (1976-1994); Italy (1976-1991); Sweden (1976-1992); Denmark (1983-1994); Belgium, Greece, Luxembourg (1983-2009); United Kingdom (1984-2009); Australia (1985-2009); New Zealand (1986-2008); Korea (1988-2009); Hungary, Poland (1992-2009); Czech Republic (1993-2009); Slovak Republic (1993-2009); Ireland (2000-2009).

Table B 4-4. Means of the dependent variables by year

	ALMP	*EPL*	*RR*	*UD*	*Adult unempl.*	*Youth unempl.*	*GDP growth*	*Rel. size pop.*
1971	0.64	2.36	17.30	40.02	2.16	5.33	0.04	12.56
1972	0.64	2.36	18.04	40.35	1.84	5.47	0.06	11.98
1973	0.64	2.36	18.04	40.70	1.64	5.12	0.07	12.01
1974	0.64	2.36	20.00	41.32	1.74	5.51	0.03	12.07
1975	0.64	2.36	20.00	41.97	3.25	8.73	0.01	12.48
1976	0.62	2.32	20.21	42.90	3.15	9.44	0.05	12.39
1977	0.62	2.32	20.21	43.20	3.73	11.40	0.03	12.69
1978	0.62	2.32	20.33	43.64	3.57	11.98	0.04	12.42
1979	0.62	2.32	20.33	43.52	3.81	12.05	0.04	12.73
1980	0.62	2.32	21.21	43.19	3.85	12.99	0.02	12.86
1981	0.62	2.32	21.21	42.83	4.75	14.28	0.02	13.00
1982	0.62	2.32	21.42	42.69	5.37	16.43	0.02	12.54
1983	0.62	2.32	21.42	42.35	6.67	18.38	0.02	12.43
1984	0.62	2.32	22.50	41.81	6.93	18.62	0.04	12.12
1985	0.62	2.32	22.50	40.92	6.99	18.29	0.03	11.88
1986	0.64	2.32	23.04	40.08	6.64	16.96	0.03	11.86
1987	0.63	2.32	23.04	39.58	6.44	15.95	0.03	11.68
1988	0.63	2.32	22.88	39.24	6.08	15.04	0.04	11.89
1989	0.61	2.31	22.88	39.07	5.90	13.85	0.03	11.65
1990	0.62	2.32	23.25	38.44	5.63	13.46	0.03	11.43
1991	0.67	2.31	23.25	38.60	6.06	14.20	-0.01	11.65
1992	0.81	2.31	23.79	38.31	6.87	15.90	0.00	11.36
1993	0.79	2.28	23.79	37.87	7.40	17.54	0.01	11.13
1994	0.77	2.24	24.21	36.84	7.62	18.01	0.03	11.01
1995	0.76	2.21	24.21	35.38	7.20	17.26	0.04	10.88
1996	0.74	2.20	24.42	33.81	7.07	17.08	0.04	10.76
1997	0.70	2.12	24.42	32.23	6.72	16.69	0.04	10.65
1998	0.72	2.08	25.13	30.80	7.02	16.66	0.03	10.48
1999	0.72	2.04	25.13	29.80	6.92	17.08	0.03	10.34
2000	0.68	2.06	25.08	28.98	6.98	16.51	0.05	10.26
2001	0.67	2.05	25.08	28.24	6.75	17.00	0.03	10.09
2002	0.66	2.04	25.04	27.74	7.06	17.54	0.03	9.99
2003	0.63	2.02	25.04	27.74	6.95	17.84	0.03	9.95
2004	0.62	2.02	23.50	27.17	6.93	17.94	0.04	9.87
2005	0.60	2.03	23.50	26.66	6.58	17.17	0.04	9.81
2006	0.58	2.03	23.13	26.04	5.91	15.88	0.05	9.75
2007	0.54	2.02	23.13	25.39	5.23	14.15	0.05	9.67
2008	0.55	1.99	23.13	24.88	5.20	14.45	0.01	9.52
2009	0.55	1.98	23.13	24.88	7.43	19.91	-0.05	9.35

Notes: Active labour market policies (ALMP), employment protection legislation (EPL), replacement rate (RR), union density (UD).
Sources: OECD and EUROSTAT.
The sample period is from 1971 to 2009, except for the following countries: Spain (1972-2009); Japan, Portugal (1975-2009); Canada (1976-2009); Germany (1976-1994); Italy (1976-1991); Sweden (1976-1992); Denmark (1983-1994); Belgium, Greece, Luxembourg (1983-2009); United Kingdom (1984-2009); Australia (1985-2009); New Zealand (1986-2008); Korea (1988-2009); Hungary, Poland (1992-2009); Czech Republic (1993-2009); Slovak Republic (1993-2009); Ireland (2000-2009).

Table B 4-5. Mean of the Schmidt index by country

	Schmidt Index	*sd*
Australia	2.87	1.92
Belgium	2.46	0.82
Bulgaria	2	0.71
Canada	1	0.00
Croatia	–	–
Czech Rep.	2.53	1.23
Denmark	2.54	1.60
Estonia	2.11	1.18
France	2.51	1.60
Germany	2.82	1.59
Greece	3.03	1.89
Hungary	3	1.30
Ireland	1.62	0.67
Italy	2.26	0.85
Japan	1.13	0.34
Korea	–	–
Latvia	2.08	0.28
Lithuania	3.06	1.51
Luxembourg	2.31	0.89
Malta	1.54	1.33
Mexico	–	–
Netherlands	2.1	0.94
New Zealand	2.87	1.89
Poland	2.68	1.20
Portugal	2.44	1.54
Romania	3.26	1.19
Slovak Rep.	2.24	0.97
Slovenia	3.31	0.63
Spaiò	3.36	1.92
Sweden	3.74	1.77
Turkey	–	–
UK	2.82	1.94
US	1	0.00

Source: CPDS (Comparative Political Dataset), Institute of Political Science of the University of Berne, Switzerland. The sample period is from 1971 to 2009, except for the following countries: Spain (1972-2009); Japan, Portugal (1975-2009); Canada (1976-2009); Germany (1976-1994); Italy (1976-1991); Sweden (1976-1992); Denmark (1983-1994); Belgium, Greece, Luxembourg (1983-2009); United Kingdom (1984-2009); Australia (1985-2009); New Zealand (1986-2008); Korea (1988-2009); Hungary, Poland (1992-2009); Czech Republic (1993-2009); Slovak Republic (1993-2009); Ireland (2000-2009).

Table B 4-6. Mean of the Schmidt index by year

	Schmidt Index	*sd*
1971	1.8	1.32
1972	2	1.36
1973	2.6	1.64
1974	2.63	1.54
1975	2.63	1.54
1976	2.24	1.48
1977	2.17	1.47
1978	1.94	1.35
1979	1.67	1.08
1980	1.56	1.20
1981	1.89	1.23
1982	2.06	1.39
1983	2.28	1.71
1984	2.5	1.72
1985	2.56	1.76
1986	2.39	1.75
1987	2.33	1.78
1988	2.5	1.76
1989	2.56	1.65
1990	2.37	1.61
1991	2.24	1.48
1992	2.13	1.46
1993	2.24	1.36
1994	2.52	1.48
1995	2.84	1.52
1996	2.72	1.34
1997	2.66	1.42
1998	2.59	1.45
1999	2.55	1.50
2000	2.62	1.59
2001	2.79	1.57
2002	2.59	1.55
2003	2.38	1.66
2004	2.34	1.52
2005	2.52	1.60
2006	2.55	1.48
2007	2.62	1.42
2008	2.62	1.42
2009	2.48	1.45

Source: see table B 4-5

Appendix 4.C

Definition of the Kaitz index

Australia

MW: Federal minimum weekly wage (August each year) -- extrapolated from 1997 back to 1985 in line with Metals Industry Award C14 wages and National Wage Case decisions.

Median wage: Median gross weekly earnings of full-time workers in main job (August each year).

Source: OECD MW Database.

Belgium

MW: Minimum monthly wage (annual averages) -- Revenu Minimum Mensuel Moyen Garantie (RMMMG) -- for experienced workers aged 22 and over (21 and over prior to 1992).

Median wage: Median gross monthly earnings of full-time workers.

Source: OECD MW Database.

Bulgaria

MW: Monthly MW

Mean wage: Mean value of the average gross monthly earnings of full-time workers including overtime earnings, regular and irregular bonuses and payments for time not worked. Only enterprises with at least 1 employee are covered (industry and services excluding public administration).

Source: EUROSTAT.

Canada

MW: Weighted (by labour force) average of provincial minimum hourly wage (Can$).

Median wage: Median gross hourly earnings of full-time workers.

Source: OECD MW Database.

Croatia

MW: Monthly MW

Mean wage: Mean value of the average gross monthly earnings of full-time and part-time workers, only enterprises with at least 10 employees are covered (industry and services excluding public administration).

Source: EUROSTAT.

Czech Republic

MW: Minimum gross monthly wage (annual average).

Median wage: Median monthly earnings of employees who worked at least 1 700 hours during the year.

Source: OECD MW Database.

Denmark

The average hourly MW divided by an average hourly wage.

Source: Dolado et al. (1996).

Estonia

MW: Monthly MW

Mean wage: Mean value of the October gross monthly earnings of full-time workers (industry and services excluding public administration).

Source: EUROSTAT.

France

MW: Gross monthly equivalent of the hourly MW -- Salaire Minimum Interprofessionnel de Croissance (SMIC) -- and the Garantie Mensuelle de Rémunération (GMR) for 2000-2005.

Median wage: Median gross annual earnings of full-time workers in the private and semi-private sector.

Source: OECD MW Database.

Germany

Average monthly MW divided by an average monthly wage.

Source: Dolado et al. (1996).

Greece

MW: Minimum monthly wage for an unqualified, single, worker with no work experience (annual average and assuming paid for 14 months).

Median wage: Median gross annual earnings of full-time workers.

Source: OECD MW Database.

Hungary

MW: Minimum gross monthly wage (May each year).

Median wage: Median monthly earnings of full-time employees (May each year).

Source: OECD MW Database.

Ireland

<u>Before 2000:</u>

The average hourly MW divided by an average hourly wage.

Source: Dolado et al. (1996).

<u>From 2000:</u>

MW: minimum gross hourly wage (March each year).

Median wage: median hourly earnings of full-time employees.

Source: OECD MW Database.

Italy

Average minimum monthly wage divided by an average wage.

Source: Dolado et al. (1996).

Japan

MW: Weighted average of prefectural hourly MWs (June each year and weighted by employment).

Median wage: Median gross monthly earnings, including overtime and all special payments, for June of each year (estimated by applying the ratio of mean total to mean scheduled earnings to median scheduled earnings).

Source: OECD MW Database.

Korea

MW: minimum hourly wage (June each year).

Median wage: median gross monthly earnings, including overtime and all special payments, for June of each year.

Source: OECD MW Database.

Latvia

MW: Monthly MW

Mean wage: Mean value of the average gross monthly earnings of full-time workers (industry and services excluding public administration).

Source: EUROSTAT.

Lithuania

MW: Monthly MW

Median wage: Median value of the average gross monthly earnings of full-time workers (industry and services excluding public administration).

Source: EUROSTAT.

Luxembourg

MW: minimum monthly wage -- Salaire Social Minimum (SSM) -- (October each year).

Median wage: median gross monthly earnings of full-time, full-year workers (annual earnings divided by 12).

Source: OECD MW Database.

Malta

MW: Monthly MW (in Malta the MW is fixed at a weekly rate. These hourly or weekly rates have been converted to a monthly rate).

Median wage: Median value of the average gross monthly earnings of full-time workers (industry and services excluding public administration).

Source: EUROSTAT.

Mexico

MW: Weighted average of regional daily MWs (annual average and weighted by employment).

Mean wage: Mean hourly wages of manual workers in manufacturing.

Source: OECD MW Database.

Netherlands

MW: Minimum weekly earnings -- Minimumloon -- for persons aged 23 to 64 (annual average).

Median wage: Median gross annual earnings of full-time employees (including overtime payments).

Source: OECD MW Database.

New Zealand

MW: minimum weekly wage for workers aged 20 and over (February each year).

Median wage: median usual weekly earnings of full-time employees (February each year).

Source: OECD MW Database.

Poland

MW: Minimum monthly wage (September of each year) (Zl).

Median wage: Median gross monthly earnings of full-time workers.

Source: OECD MW Database.

Portugal

MW: Minimum monthly wage -- Salário Mínimo Nacional (SMN) -- for non-agricultural workers aged 20 and over (annual average).

Median wage: Median gross annual earnings of full-time workers.

Source: OECD MW Database.

Romania

MW: Monthly MW

Median wage: Median value of the average gross monthly earnings including non-standard payments (industry and services excluding public administration).

Source: EUROSTAT.

Slovak Republic

MW: minimum monthly wage.

Median wage: median gross monthly earnings of full-time workers.

Source: OECD MW Database.

Slovenia

MW: Monthly MW

Mean wage: Mean value of the average gross monthly earnings of full-time and part-time workers, including 13th month payments (industry and services excluding public administration).

Spain

MW: Salario minimo interprofesional per month (Ptas) for workers aged 18 and over.

Median wage: Median gross annual earnings of full-time workers.

Source: OECD MW Database.

Sweden

The average hourly MW divided by an average hourly wage.

Source: Dolado et al. (1996).

Turkey

MW: Minimum daily wage (TL) for workers aged 16 and over.

Mean wage: Mean daily earnings of manufacturing workers.

Source: OECD MW Database.

United Kingdom

<u>Before 1994:</u>

The average hourly MW divided by an average hourly wage.

Source: Dolado et al. (1996).

From 1999:

MW: national minimum hourly wage.

Median wage: median hourly earnings of full-time adult employees.

Source: OECD MW Database.

United States

MW: Federal minimum hourly wage rate (US$).

Median wage: Median usual weekly earnings of full-time employees.

Source: OECD MW Database.

Definition of the Percentile at which the MW "bites"

The percentile at which the MW "bites" is constructed from the Gini coefficient, real MW and real average wages. The logic is as follows. Let $ln(x) \approx N(\theta, \sigma^2)$ so that x has a lognormal income distribution with parameters θ and σ^2. The median is $\exp\{\theta\}$, the mode is $\exp\{\theta - \sigma^2\}$and the mean is $\exp\{\theta + (1/2)\sigma^2\}$. If $u(p)$ is the value in the $N(0,1)$ distribution at percentile point p (so that $u(1/2)=0$, etc) then $x(p)=exp\{\theta+u(p)\sigma\}$ is the income level at percentile p. The Gini coefficient is $G=1-2u| \sigma/\sqrt{2}|$, or, indeed, twice the area under $N(0,1)$ between the ordinates $u = 0$ and $u = \sigma/\sqrt{2}$. So if you know the Gini coefficient, you can infer σ. And then, knowing the mean (or median or mode) you can infer θ. So if the MW is $\bar{x}$, you can get their average percentile $\bar{p}$ by solving $\bar{x} = x(\bar{p})$.

Data on the Gini coefficient comes from the UNU WIDER, World Inequality Database. We try to use a definition of the Gini coefficient as consistent as possible across countries and years. In most of the series the Gini coefficient is constructed from disposable income. Exceptions are Australia (where the Gini is constructed from monetary income), Latvia, Lithuania, Poland, Slovenia where the Gini is constructed from gross earnings. The income sharing unit is the household and the unit of analysis is the household and the unit of analysis is person, meaning that the needs of different sized households have been taken into account. Exceptions are: Japan and Romania, where the unit of analysis is the household; Lithuania and Latvia where the income sharing unit is the person. The equivalence scale varies among countries being either the OECD scale in most of the countries or per capita when the OECD scale is not available.

Definition of MW relative to GDP per head

Data on MW relative to GDP per head come from different OECD sources.

Data on real hourly MW in US dollars Purchasing Power Parities (PPPs) comes from the OECD Minimum Wage database. This data is calculated by deflating the series using the CPI, taking 2005 as the base year. The series are then converted by the OECD into a common currency unit using PPPs in 2005.

Data on GDP per head current prices, current PPPs is taken from OECD National Accounts data. In order to get data on GDP per head consistent with the real MW data, the GDP series has been deflated using the CPI, taking 2005 as the base year. Moreover, the series has been converted into US dollars using PPPs in 2005.

In order to get data on MW consistent with the GDP per head series, data on real hourly MW has then been multiplied by average annual hours worked per worker from OECD Productivity database.

Appendix 4.D

Table D 4-5. Characteristics of MW systems

Country	*Method of setting*	*System*	*Youth subminimum*
Australia	statute	national-state	yes
Belgium	negotiated	national	yes
Canada	statute	provincial	limited
Czech Republic	statute	national	yes
Denmark	negotiated	sectoral collective agr.	yes
France	statute	national	limited
Germany	negotiated	sectoral collective agr.	some
Greece	negotiated	national	no
Hungary	statute	national	no
Ireland before 2000	Lab. Committees	sectoral collective agr.	yes
Ireland from 2000	statute	national	yes
Italy	negotiated	sectoral collective agr.	some
Japan	statute	regional	no
Korea	statute	national	limited
Luxemburg	statute	national	yes
Mexico	statute	regional	no
Netherlands	statute	national	yes
New Zealand	statute	national	yes
Poland	statute	national	no
Portugal	statute	national	no
Slovak Rep.	statute	national	yes
Spain	statute	national	no
Sweden	negotiated	sectoral collective agr.	yes
Turkey	statute	national	yes
UK before 1994	Wage Councils	sectoral collective agr.	yes
UK from 1999	statute	national	yes
US	statute	national-state	limited
Lithuania	statute	national	no
Romania	statute	national	no
Slovenia	statute	national	no
Malta	statute	national	yes
Latvia	statute	national	yes
Estonia	statute	national	no
Bulgaria	statute	national	no
Croazia	statute	national	no

Sources "Method of Setting": ILO, MW Database; Eurostat, "MWs in January 2009".
Sources "System": The MW Revisited in the Enlarged EU, table 1.1 pg.2; for extra-European countries: ILO, MW data base.
Sources "Youth minimum": ILO, MW database; Low Pay Commission Report 2009.

Appendix 4.E

Institutional control variables
Employment Protection Legislation

Synthetic indicator of the strictness of regulation on dismissals and the use of temporary contracts[39]. High values are associated with countries having a high degree of employment protection, while low values indicate relative ease in dismissing employees.

Source: OECD. The measure is available for 28 of the countries included in the analysis from 1985. We use 1985 values for previous years.

Active labour market programs

Level of public expenditure in ALMP to bring unemployed workers to work as a percentage of GDP.

Source: OECD. The measure is available for 24 of the countries in the analysis from 1985. We use 1985 values for previous years.

Union density

Ratio of wage and salary earners that are trade union members, divided by the total number of wage and salary earners[40].

Source: OECD. This measure is available for 24 of the countries in the analysis.

Replacement rate

Average of the gross unemployment benefit replacement rates as a percentage of earnings and it is meant to quantify the generosity of unemployment insurance programs[41].

Source: OECD. This measure is available for 24 of the countries in the analysis and it is drawn every two years for the entire length of our panel.

[39] See www.oecd.org/employment/protection for details on the methodology and weights used to compiled the indicators.
[40] For more information and full methodology, see http://www.oecd.org/dataoecd/37/2/35695665.pdf.
[41] For further details, see OECD (1994), The OECD Jobs Study (chapter 8) and Martin J. (1996), "Measures of Replacement Rates for the Purpose of International Comparisons: A Note", OECD Economic Studies, No. 26. Pre-2003 data have been revised.

5. The National Minimum Wage and the Decrease in Wage Inequality in the UK

5.1 Introduction

The National Minimum Wage (NMW) was introduced in the UK in April 1999 and subsequently up-rated every October since then. One of the motivations for the introduction of the NMW was to help to "make work pay" and address in-work poverty, against a background of rising wage inequality which characterised the British labour market in the 1980s and early 1990s. At the outset, the Low Pay Commission (1998) hoped that the NMW might take "greater inroads into pay equality" without putting jobs at risk. That is why the chapter aims at having a deep insight of the impact of the NMW on wage inequality. As figure 5-1 shows, from the 1980s to the early 1990s there was a rapid decrease in the log 10:50 hourly wage ratio, this means the 50th percentile was rising relative to the 10th percentile of the hourly wage distribution for most of the period. The trends since the beginning of the 1990s were less marked but have not reversed the earlier decrease. From the mid-nineties the situation reversed with a gradual increase in the log 10:50 wage ratio (a decrease in lower tail inequality).

Applied economics research has put a lot of weight in evaluating the impact of the introduction of the NMW on employment (see chapter 2). However, very few UK studies have been focusing on wage inequality. It is therefore of policy interest to explore the role of the NMW in the trend towards lower inequality in the recent years. The introduction and up-rating of the NMW might affect wage inequality for several reasons. First of all, assuming reasonable levels of enforcement and compliance, the NMW increases the wages of those earning below the wage floor to the level of the minimum, creating a spike in the wage distribution at the minimum. In other words, workers whose wages are bound by the minimum, experience a wage increase. Secondly, an increase in the NMW can also possibly provide some boost to wages for workers who previously earned a bit more than the NMW, leading to changes in wages higher up in the wage distribution. These effects are often called spill-overs. Figure 5-1 shows the trend of lower tail wage equality, as measured by the log 10:50 ratio, from 1975 to 2010 together with the movement of the real value of the NMW from its introduction in 1999. It is quite evident that in some of the years when the real value of the NMW increased the log 10:50 wage differential also increased similarly. Furthermore, in some of the years when the real value of the NMW decreased lower tail wage equality seems to have decreased as well. There are several reasons that could explain why the log 10:50

wage differential moves with the NMW. If the NMW is binding at the 10th percentile of the wage distribution then we might expect the NMW to move together with the log 10:50 wage differential. If the NMW, as in the UK case, is generally binding at lower percentiles of the wage distribution than this co-movement of the NMW and the log 10:50 wage differential could be explained by the presence of some measurement error in our hourly wage data or alternatively by the presence of spill-over effects of the NMW up the wage distribution[42].

American economists have explored the role of minimum wages in the context of rising inequality in the US wages. A few studies, in particular, claimed that the decrease of the real value of the minimum wage was an especially important factor in the rise in wage inequality in the 1980s. The first, by DiNardo, Fortin and Lemieux (1996), used non-parametric density estimation of wage distributions to decompose changes in various measures of between-group and within-group wage inequality into the portions associated with changes in the minimum wage, changes in unionisation, changes in individual attributes, supply and demand influences, and a residual category not explained by any of these factors. The basic strategy of DiNardo, Fortin and Lemieux requires constructing counterfactual, unobserved wage densities that capture how the distribution of wages would have evolved absent a particular change (such as the decline in the value of the real minimum), assuming no general equilibrium effects. Their results indicate that the falling real value of the minimum wage played a major role in the increase in inequality over the 1980s, especially among women.

Lee (1999) also analysed the effects of the minimum wage on changes in wage inequality in the US. Noting that, as we observed in chapter 2, a national minimum allied to local wage distributions generates the potential for area-level variation in wage inequality, he looks at differences between state median wages and the state-level or federal minimum to try and identify a minimum wage effect on wage inequality. He assumes that, in the absence of the minimum wage, wage inequality would have been the same across all US states. The model specifies an identifiable function for the latent wage distribution (the one that would have been observed in the absence of the minimum wage) and it attributes any deviation around this function to the effect of the minimum wage.

Lee concludes that the observed increase in the 50/10 differential from 1979 to 1988 was largely due to the decline in the real value of the minimum wage. He also suggests

[42] The picture looks roughly the same if we use the log 5:50 wage differential.

that, with the exception of men aged twenty-five to sixty-four, the entire increase in the 50/10 differential is attributed to the declining minimum wage.

However, Lee also finds significant effects of the minimum on log 90:50 wage inequality, an effect which is hard to explain. With this in mind, Autor et al (2010) reassess the effect of state and federal minimum wages on US earnings inequality, addressing two issues that appear to bias Lee's (1999) work, namely the omitted variable bias due to the absence of state fixed effects in Lee's model and the inherent measurement error in the variables. Consequently, they introduce state fixed effects in the analysis as needed for consistent estimation. Moreover, they suggest that the presence of measurement error in the hourly wage variables lead to a spurious correlation between the measures of wage inequality and the minimum wage variable, and they therefore instrument their minimum wage variable. They use the differential variation in the minimum wages of the US states over and above the federal minimum wage as an instrument for the measure of the minimum wage in each state. This is a valid instrument to the extent that legislated minimum wages by area do not adjust endogenously to differences in the levels or trends in local latent inequality. Allowing for these factors, the authors find that the decrease of the real minimum wage raises inequality in the lower tail of wage distribution, but the impacts are less than half than those reported by Lee (1999).

A recent paper by Bosch and Manacorda (2010) applies and extends Lee's methodology to urban Mexico. Specifically, it analyses the contribution of the minimum wage to the rise in earnings inequality in Mexico between the late 1980s and the early 2000s, using household micro data from municipalities in Mexico. Bosch and Manacorda borrow from Lee's (1999) and Autor et al's (2010) analyses of the effect of the minimum wage on changes in wage inequality in the United States. Similarly to Autor et al (2010) they observe that any measurement error in a specific percentile of the wage distribution will lead to a spurious positive correlation between different measures of inequality and the minimum wage measure, hence possibly leading to biased estimates of the effect of the minimum wage. To circumvent this problem, they instrument the minimum wage measures by municipality calculated using the ENEU data with the same measure computed using another dataset, Social Security data. They argue that the instrument purges the estimates of potential correlation between the regressors and the error term due to measurement error. They find that a substantial part of the growth in inequality at the bottom end of the distribution is due to the steep decline in the real value of the minimum wage. They show that, at least in the early years, not only did the minimum wage create a floor to the earnings distribution, but it also had spill over effects that spread to higher percentiles of the distribution.

Most of the existing UK literature on inequality and the NMW has focused solely on the year of the introduction of the NMW and provide evidence of whether the NMW creates spikes in the wage distribution at the minimum. Excluding a specific case where measurement error in the hourly wage data might have hidden the presence of a spike in the wage distribution at the minimum (Dickens and Manning, 2004a), there is general agreement that the introduction of the NMW in the UK resulted in a spike at that point in the wage distribution. In particular, Dickens and Manning (2004b) find evidence of such a spike using hourly wages in the low-wage residential home sector. Also Stewart and Swaffield (2002) examining data from the British Household Panel Survey report similar evidence.

The UK literature has also looked for evidence of spill-overs to wages above the wage floor. Regarding the presence of spill-over effects there is less of an agreement. Dickens and Manning (2004a and 2004b) find virtually no spill-over effects of the NMW in the year of its introduction for both the whole economy and the care home sector. Butcher et al (2009) find evidence of cumulative significant spill-over effects for each year relative to 1997 and 1998 (years when the NMW was not yet been introduced). The minimum wage directly affected up to the 6th percentile, at which the spill-over effect was largest, raising wages by about 7 per cent more than in the absence of the minimum wage. This effect stretched up the pay distribution (wages were raised by about 4 per cent at the 10th percentile and still over 1 per cent at the 17th percentile). The effect was larger for women than men. The Low Pay Commission (2009) find evidence suggesting spill-overs for the first years of NMW existence (1998-2004), but a smaller impact for 2004-2008. Nanos (2008) finds a positive NMW spill-over effect in the UK. In contrast, Stewart (2011a and 2011b) using different approaches (such as using a Difference-in-Differences estimator and comparing quantiles of the observed wage distribution after an increase in the minimum wage with those of an estimated counterfactual wage distribution if there had not been an increase in the minimum) find no evidence or, in some cases very small evidence, of spill-over effects in the wage distribution.

None of these papers specifically explore the role of the NMW in the recent trend towards lower tail wage inequality in UK wages. We aim to do precisely this. In order to estimate the effect of the NMW on wage inequality in the UK, in this chapter we borrow from the methodology used by Lee (1999) for the US. We assume that wage inequality would have been similar across areas (or would have changed similarly), if it were not for the effect of the NMW. Differences in average wage levels across areas, that are assumed to be exogenous, therefore induce useful variation in the real "bite" of the NMW across areas that allow the identification of its effect net of other confounding forces.

We also try to address some of the problematic issues that emerges from previous research. As in Lee (1999), we find evidence of a significant effect of the minimum wage variable on upper tail wage inequality, such as the log 90:50 wage differential. We attribute this result to a misspecification in the model and we attempt to address it. First of all, differently from Lee's model, we expect there to be permanent differences in latent wage inequality across areas, and therefore we include area fixed effect in our main specification. Secondly, we also attempt to address simultaneity bias and measurement error problems in our regression equation by instrumenting our measure of the NMW.

We finally test different measures of wage inequality where the NMW might have had a different impact. We do not focus only on the log 10:50 wage differential, but we also look at measures of inequality that, arguably, have a normative basis, derived from a social welfare function which includes values of society regarding equality and justice, such the Atkinson index and the Generalised Gini (for a review of different measures of wage inequality see Cowell, (2011) and Lambert (2001))[43]. When the sample size allows, we look at men and women separately. This is mainly to see whether the relative minimum has a higher impact on lower tail wage inequality for groups traditionally low paid (such as women, who are also most likely to work part-time, itself another significant correlate with low pay).

By looking at log wage differentials of different percentiles *q* relative to the median wage ($w^{q}_{it} - w^{50}_{it}$), we find an effect of the NMW on the wage distribution. Using 2SLS estimation, the point estimates tend to become smaller at higher deciles and are statistically significant only up to the first decile, suggesting a negative impact of the NMW on lower tail wage inequality and perhaps some small spill-over effects. By

[43] The Gini (and Atkinson) indexes are effectively a weighted average of the centiles of the wage distribution, and they can be sensitive to movements in particular parts of the distribution. Therefore, we would not expect the same response as the log 10:50 wage differential. In appendix 5.C a detailed description of how these variable are defined.

looking also at different measures of wage inequality, such as the Atkinson index and the Generalised Gini, we find again a significant contribution of the NMW in reducing wage inequality.

The chapter is organised as follows. Section 5.2 describes the data. Section 5.3 presents the theoretical relation between wage dispersion and the minimum wage. Section 5.4 focuses on the empirical methodology. Section 5.5 shows OLS regression results. Section 5.6 addresses some of the specification problems. Section 5.7 concludes.

5.2 Data

As in chapter 2, this chapter utilises microdata from the Annual Survey of Hours and Earnings (ASHE) for the period 1999 to 2010. The ASHE, developed from the earlier New Earnings Survey (NES), is an employer reported individual-level dataset and it is conducted in April of each year. One of the advantages of the ASHE dataset is that information on earnings and paid hours is obtained in confidence from employers, usually reporting this information directly from their payroll record. It is therefore a relatively accurate record of earnings and hours with which to construct a measure of hourly wages. Secondly, the ASHE sample is relatively large, allowing analysis at different levels of geographic aggregation. Finally, ASHE consists of point-in-time measures of hourly wages, which make it appropriate for a study of the impact of a wage floor on wage inequality.

One of the disadvantages of the ASHE is that it is based on a sample of employees taken from HM Revenue and Customs PAYE records and, in the past, this reduced coverage of those at the lower end of the earnings distribution. However, from 2004 onwards ASHE sample was boosted by a sample of firms not registered for PAYE, therefore improving representation of low paid employees, particularly those that usually work part-time (eg. female) and tend to be below the PAYE threshold.

The ASHE wage variable used for the analysis is defined as average hourly earnings for the reference period, excluding overtime. Specifically, it is average gross weekly earnings excluding overtime divided by basic weekly paid hours worked.

The data used here are restricted to those aged 22 to retirement, meaning that we focus mainly on the adult wage rate. When the sample size allows we focus not only on the pooled sample, but also on women and men separately.

The analysis is undertaken disaggregated by geography, where each cell corresponds to a specific area in the UK. This allows us to exploit variation in our measure of the extent to which the NMW was sweeping up the tail of the wage distribution across areas and years in order to evaluate the effect of the NMW on lower tail wage inequality.

The geographic variation in wages will reflect the demographic and industrial composition of each local labour market. The changing industrial composition of an area over time and the extent to which industries are low and high paying will affect the changing incidence of the NMW working in a locality. Likewise the skill, age and gender composition of the local workforce. We can control for variation in these factors with a set of time varying local labour market control variables, drawn from either ASHE or matched in from complementary Labour Force Survey (LFS) data. The analysis is conducted at Unitary Authority and County level including 34 English counties, 6 English metropolitan counties, 46 English Unitary authorities, Inner and Outer London and finally 52 Unitary authorities in Scotland and Wales.[44] This gives 140 local areas in Great Britain.

Figure 5-2 depicts the Kernel density estimates of the distribution of hourly log-wages from ASHE data for the years 1998 and 2010 for the pooled sample (men and women).

The two vertical lines denote the NMW levels in each year. We assume that the notional NMW in 1998, when the NMW was not been introduced yet, is the nominal NMW in 1999 deflated by the AEI (Average Earnings Index). The figure clearly suggests that in 2010 the NMW had a large effect on hourly wages.

The available time series data on the UK wage distribution also point to a close connection between the NMW and changes in inequality. Figure 5-3, plots changes in percentiles of the wage distribution, relative to the median, for the fifth, tenth, twenty-fifth, seventy-fifth and ninetieth wage percentiles between 1999 and 2010. These measures of wage dispersion as well as the log-difference between the NMW and the median wage (also shown in figure) are normalised to be zero in 1999, the year in which the NMW was introduced. The figure shows that after decreasing in the first years of its introduction, the NMW (relative to the median wage) raised in the period between 2001 and 2010, with a slow down from 2007 to 2009. The reason for the decrease of the NMW (relative to the median wage) in the first years of its existence is that the NMW was not raised in line with average earnings (see figure 2-1 of second chapter).

[44] The Orkney Islands, Shetland Isles and Western Isles are aggregated together. The 36 English metropolitan districts are combined into 6 English Metropolitan Counties. London Boroughs are aggregated into Inner and Outer London. This allows us to match the LFS geographical units with the ones of NES/ASHE, using the same geography as the variable “uancty in the LFS.

The figure illustrates that especially from 2004 on movements in wage dispersion in the lower half of the wage distribution moved in close tandem with changes in the NMW. This is especially true for the 5th percentile of the wage distribution since the average coverage of the NMW from 2004 to 2010 is around 3.5%. Finally, the figure shows that the 90th and 75th percentiles have been pretty stable relative to the NMW in the period of the analysis. However, the NMW is unlikely to have played any part in this particular trend since it "bites" on average only around the 3rd percentile of the hourly wage distribution in the period of analysis. However, no analysis of aggregate time series data will be able to distinguish a NMW effect from a decreasing trend in latent wage inequality (a decrease in wage inequality which would have occurred in absence of the NMW policy), which is why we attempt to distinguish the effects using disaggregated data.

The time series pattern of lower tail wage inequality is not uniform across local areas in the UK. Figure 5-4 plots trends in two average log-wage quantile differentials from 1999-2010 for two groups of local areas: the ten highest-wage local areas[45] and the ten lowest-wage local areas[46]. The low-wage areas experience for the entire period of the analysis a more compressed lower tail as measured by the 5-50 log hourly wage gap, than the high wage areas. Also the figure shows that the log 5:50 differential for both high- and low-wage areas appears relatively stable during the 1999-2010 period[47].

This additional dimension of wage inequality and its relation to the relative level of the minimum wage across areas offers an opportunity to distinguish the minimum wage effect from a national trend in latent wage dispersion. In particular, the interaction of a NMW and wide variation in wages across areas yields variability in the minimum wage bindingness levels.

The NMW highly impacts low-wage areas, while minimally affecting high-wage areas. This is demonstrated in figure 5-5, which plots Kernel density estimates of log-

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[45]High wage areas: Inner and Outer London, Derby UA, Bracknell Forests UA, West Berkshire UA, Reading UA, Slough UA, Windsor and Maidenhead UA, Wokingham UA, Surrey.

[46]Low wage areas: Hartlepool UA, Blackpool UA, Lincolnshire, Herefordshire UA, Cornwall, Torbay UA, Blaneau Gwent, Ceredigion, Pembrokeshire, Moray.

The twenty areas high wage and low wage areas where selected from the area-level panel dataset described in the data section of this paper. Average median wages for the entire period where computed. The ten highest and the ten lowest overall averages were chosen. The differentials are for the entire adult sample.

[47] From Figure 5 it seems there has been an average decrease in lower tail dispersion in low-wage areas. However, the trend in lower tail dispersion for these areas is not significantly different from zero.

wages in 1999 for three groups of areas: high-, medium-, and low-wage areas[48]. The horizontal scale measures the log-wage of each point in the local wage distribution relative to the local median wage. The vertical lines represent the NMW, also relative to each group's relative median. Figure 5-5 gives the impression that low-wage areas are indeed more highly impacted by the NMW than higher wage areas. It is precisely this variation that is exploited in the present study.

5.3 Theoretical relation between wage dispersion and the NMW

In order to identify the effect of the minimum wage on the distribution of earnings, we follow Lee (1999) and more recently Autor et al (2010), who use this strategy for the US.

Following Lee (1999), in each UK's specific local area, there are two wage distributions: the wage distribution that is observed and the latent wage distribution, which is the wage distribution that would have been observed in the absence of the NMW. Let w^q_i the *q-th* percentile of the log wage distribution in area *i* and let the w^{*q}_i the *q-th* percentile of the latent wage distribution. Now suppose there is a sufficiently high percentile p such that workers at this or higher percentiles are unaffected by the minimum wage, ie. $w^s_i = w^{*s}_i \;\; s>=p$. Similarly to Lee (1999) and Autor et al (2010) we assume that for the UK earnings at or above the median are unaffected by the NMW, implying that p=50.

The scope of our chapter is to look at the effect of the real value of the NMW on wage inequality. Wage inequality is measured by wage dispersion, which is proxied by the differential between percentile q and a measure of centrality of the log(wage) distribution, such as the median (w^q_i - w^{50}_i). In a similar way latent wage inequality, or wage dispersion in the absence of a NMW policy, is given by (w^{*q}_i - w^{50}_i). To generate a NMW measure which varies across local areas, we create a measure of the "bindingness"

[48] I rank each area by the mean log-wage. I choose the bottom twenty, middle twenty and highest twenty for the three groups.
Bottom twenty: Hartlepool UA, Northumberland, North Yorkshire, Derbyshire, Lincolnshire, Herefordshire UA, Shropshire, Cornwall, Torbay UA, Devon, Isle of Wight UA, Blaneau Gwent, Carmarthenshire, Ceredigion, Convy, Gwynedd, Povys, The Scottish Borders, Dumfries and Galloway, East Refrewshire.
Middle twenty: Stockton-on-Tees, Lancashire, North Lincolnshire UA, York UA, Leicester UA, Rutland UA, Nottingham UA, Staffordshire, Somerset, Norfolk, East Sussex, Pembrokeshire, Swansea, Torfaen, Clackmannanshire, East Ayrshire, East Dumbartonshire, Highland, North Lanarkshire, West Dumbartonshire.
Highest twenty: City of Bristol UA, South Gloucestershire UA, Swindon UA, Luton UA, Hertfordshire, Inner London, Outer London, Bracknell Forest UA, West Berkshire UA, Reading UA, Slough UA, Windsor and Maidenhead UA, Wokingham UA, Milton Keynes UA, Buckinghamshire UA, Oxforshire, Surrey, West Sussex, Aberdeen City, City of Edinburgh.

of the NMW such as the difference between the log minimum and the median of the log wage distribution ($MW - w^{50}_i$). Following Lee (1999), we will also call this variable "relative minimum" or "effective minimum wage."

If, following Lee (1999), we assume that the shapes of the latent wage distribution in all areas are strictly identical up to percentile q: $w^{*r}_i - w^{50}_i = w^{*r}_j - w^{50}_j$ for all regions and for all percentiles $r <= q$ (in other words, this simply means the absence of stochastic variation in the latent wage dispersion across areas), the theoretical relation between these two quantities is characterized under three scenarios.

1) Censoring (no spill-overs, no disemployment)

In the censoring case, the effect of the minimum wage is to increase the wages of those earning below the wage floor to the level of the minimum. The censoring model implies that the log q and log 50 earnings differential can be expressed as:

$$(w^q_i - w^{50}_i) = (w^{*q}_i - w^{50}_i) \qquad \text{if } w^{*q}_i >= MW \qquad (5.1)$$

$$(w^q_i - w^{50}_i) = (MW - w^{50}_i) \qquad \text{if } w^{*q}_i < MW \qquad (5.2)$$

Equation (5.1) states that the differential between the *q-th* percentile and the 50th percentile of the observed wage distribution in area i equals the latent differential if the latent w^{*q}_i is above the minimum. In other words, in areas with relatively high wages where the latent w^{*q}_i is higher than the minimum and therefore the NMW is not binding, there is no relation between wage inequality and the NMW.

Equation (5.2) states that the differential between the *q-th* percentile and the 50th percentile of the observed wage distribution in area *i* equals the differential between the NMW and the 50th percentile otherwise. In other words, in areas with relatively low wages and therefore where the NMW is binding, there is a linear relation between the relative minimum and the observed wage inequality, given by a 45^0 line.

2) Spill-overs, no disemployment

In this case the NMW may have an effect on the *q-th* percentile even if the *q-th* percentile is in a higher position in the wage distribution than the percentile where the NMW is binding.

In that case therefore we have a non linear relationship:

$$(w^q_i - w^{50}_i) = g(MW - w^{50}_i) \qquad (5.3)$$

where g is an increasing function, with the spill-overs effects monotonically diminishing the higher the wage percentile. In other words, in areas with relatively low wages and therefore where the NMW is likely to be binding, there is a linear relation between the relative minimum and the observed wage inequality, given by a 45^0 line. However, in areas where the NMW does not bind, the relationship is non linear and it depends on how big is the gap between percentile *q* and the NMW (there will be smaller spill-overs effects in areas where this gap is bigger).

3) *Truncation (no spill-overs, full disemployment)*

In this case, the NMW causes a truncation of the wage distribution: it causes job losses for all workers with latent wages below the minimum. It also has no impact on workers with latent wages above the minimum. The loss of the sample in the lower tail of the wage distribution (those paid at or below the minimum) leads to automatic changes in the observed percentiles of the wage distribution.

5.4 Regression specification and identifying assumptions

The three theoretical cases explained above rely on the assumption that there is no stochastic variation in latent wage dispersion across local areas. This assumption was useful in order to make the theoretical explanation simpler and more intuitive. However, in order to be able to empirically estimate the effect of the minimum wage on wage inequality, this assumption is abandoned.

As in Lee (1999), the following equation is the starting point for our empirical specification:

$$(w^{q}_{it} - w^{50}_{it}) = (w^{*q}_{it} - w^{50}_{it}) + \beta_1 (MW_t - w^{50}_{it}) + \beta_2 (MW_t - w^{50}_{it})^2 + \varepsilon_{it} \qquad (5.4)$$

the value of percentile *q* relative to the median is a function of latent wage inequality at percentile *q* and a quadratic of the bindingness of the NMW for area *i* at time *t*.

The quadratic term specifically is included to capture the most important features of the three theoretical cases explained above, and more specifically, that the NMW has a linear and non linear effect on wage inequality.

In order to be able to estimate the effects of the NMW on wage inequality it is necessary to make assumption on the latent wage distribution that cannot be observed.

First of all, the cumulative distribution function gives the latent wage distribution of area *i* at time *t* :

$$F_t\left(\frac{w^{*} - \mu_{it}}{\sigma_{it}}\right) \qquad (5.5)$$

where w^* is the latent wage, μ_{it} is the centrality and σ_{it} the scale parameters. The shape of the latent wage distribution *F(.)* is assumed to be the same for all local areas.

So we can write that the percentile q of the latent wage distribution can be summarized by:

$w^q_{it} = \mu_{it} + \sigma_{it} F_t^{-1}(q)$ (5.6)

and we have normalisation $F^{-1}(50) = 0$ so that μ_{it} is the median wage in area *i* at time *t*.

Secondly, Lee (1999) assumes that conditional on the year, the centrality measure of the wage distribution (the median) is uncorrelated with cross-area variation in latent wage inequality

$\sigma_{it} \perp \mu_{it} \mid t$

On the basis of these assumptions, we would estimate the following equation for the UK:

$$(w^q_{it} - w^{50}_{it}) = \alpha_t + \beta_1 (MW_t - w^{50}_{it}) + \beta_2 (MW_t - w^{50}_{it})^2 + \varepsilon_{it} \qquad (5.7)$$

It is worth noting that the term for latent wage dispersion in equation (5.7) disappears and it is simply substituted by time effects. This is simply because by assumption cross-area variation in latent wage dispersion is uncorrelated with area medians conditional on the year.

If the observed median wage w^{50}_{it} is an adequate measure of μ_{it} and recalling the normalisation $F_t^{-1}(50)=0$, then for any year and percentile *q* of the wage distribution:

$$cov((w^{*q}_{it} - w^{50}_{it}), (MW_t - w^{50}_{it}) \mid t) = cov(\sigma_{it} F_t^{-1}(50), (MW_t - \mu_{it}) \mid t) = 0 \qquad (5.8)$$

The association between wage inequality and the effective minimum in (5.7) is the causal impact of the NMW on wage inequality, if we assume independence between latent local area wage distribution and area wage medians.

An important point of Lee's model in (5.7) is that, subject to the identifying assumptions presented above, one can estimate the effect of the NMW on wage inequality without having to know the latent wage distribution.

5.4.1 Omitted variables bias due to the lack of area specific fixed effects

One of the identifying assumptions of Lee (1999) is that latent local area wage inequality is uncorrelated with median wages. For this reason Lee's (1999) main regression equation only includes years effects in order to approximate latent wage inequality. However, it is reasonable to expect permanent differences in latent wage inequality across areas, justifying the presence of area fixed effects in the main regression equation as Autor et al (2010) point out.

In order to test whether local area latent wage inequality is uncorrelated with the median wage, we regress the *log(60) – log (40)* on the median and time dummies. The latent wage distribution is the wage distribution that would have been existed in the absence of the NMW legislation. If the log (40) is not affected by the NMW legislation and the density function is symmetric around the median, the *log (60) – log (40)* should represent this latent wage distribution. As table 5-1 shows the coefficients on the median wage are always positive and significant suggesting that areas with high median wages are the areas with high latent wage inequality. The results clearly suggest the presence of permanent differences in latent wage inequality across areas, therefore areas fixed effects will be included in our regression specification.

The estimated baseline equation in our study will therefore be:

$$(w^q_{it} - w^{50}_{it}) = \alpha_t + \alpha_i + \beta_1 (MW_t - w^{50}_{it}) + \beta_2 (MW_t - w^{50}_{it})^2 + \varepsilon_{it} \qquad (5.9)$$

In addition to equation (5.7) area fixed effects are added in the analysis. Also, in some specifications we will include area time-varying demographic controls such as the proportion of female in the labour force, the average age and the proportion of highly educated (with NVQ4 or more). We will also investigate specifications that include area-specific time trend in order to control for shocks in the wage distribution that are correlated with changes in the NMW. However, one should be cautious in interpreting such results because of the loss in terms of degrees of freedom that areas-specific time trends might cause.

When data allow, we will report estimates for the overall adult sample and for men and women separately. In all of our reported regressions standard errors are robust to heteroskedasticity and serial correlation of unknown form (Wooldridge, 2010, p.61).

5.5 OLS results

Panel A of table 5-2 reports least squares estimates of equation 5.9 (ie. the relative local wage gap on the effective minimum and its square) using the panel data set of 140 local areas across 12 years from 1999 to 2010 for the entire sample of earners (both men and women). Other than coefficients of the effective minimum and its square, marginal effects are also reported estimated at the average of the effective minimum over all local areas and years.

The first three columns report estimates of the effective minimum and its square on the 10^{th} decile of the wage distribution relative to the 50^{th} decile. The second three columns present estimates of the effective minimum and its square on the 5^{th} percentile of the wage distribution relative to the 50^{th} decile.

The first column is based on an OLS regression with year and area fixed effects. In the second column controls for specific demographic characteristics of each area year cell (such as average education, age and the proportion of women) are also added. Finally, in the third column local area specific time trends are also included to control for shocks to the wage distribution that are correlated with changes in the NMW. Unless otherwise specified, regression are weighted by cell size (local area sample populations) and the standard error in brackets are robust to heteroskedasticity and autocorrelation of unknown form.

It is interesting to note that the coefficients for the quadratic terms are always significantly different from zero, demonstrating the importance of the nonlinear aspect of the relation. The presence of this non-linearity implies that there is a disproportionate response of the wage distribution at the bottom to changes in the NMW.

The coefficients on the marginal effects are always positive and significant, suggesting that an increase in the effective minimum wage reduces wage inequality at the bottom of the wage distribution. For example, the fixed effect regression estimates shown in column 5 of panel A show that a 10 p.p. rise in the effective minimum wage is associated with a statistically significant rise in the 5^{th} and 50^{th} wage differential (and therefore a reduction of lower tail wage inequality) of around 8 p.p.. (0.850*10).

When area specific time trends are added into the analysis, the marginal effects generally become smaller in absolute size even if the differences in the estimates are not statistically significant. Also, as expected, the point estimates tend to become bigger at lower percentiles. This may indicate limited spill-over effects of the NMW as the lower percentile used in the measure of inequality moves further away from the percentile at which the NMW "bites".

Panel B and Panel C of table 5-2 report results for men and women separately. It is not surprising to see how the marginal effects are generally larger in absolute size for women than for men. This is probably due to the fact that women are most likely to earn lower wages and especially to have low paid part-time jobs.

Table A 5-1 and table A 5-2 in the appendix 5.A, shows additional robustness checks. We present a regression in first differences in table A 5-1 and we also present a regression that uses unweighted (as opposed to weighted by cell size) data in table A 5-2. Neither the results reported in first differences nor the weighting scheme make any substantial difference to our OLS conclusion in table 5-2.

Table 5-3 reports the estimated first derivative of each dependent variable with respect to the effective minimum evaluated at the sample mean. Each entry refers to a separate regression, where each row refers to the differential between consecutive deciles of the wage distribution and the 50th decile. As before, column 1 presents a specification that in addition to a linear and quadratic term in the effective minimum wage, includes year and area effects. In column 2 demographic controls are added, and in column 3 area specific time trends are also included. Looking at the lower percentiles, we find large significant effects of the NMW extending throughout all percentiles below the median for the pooled distribution. However, there are reasons to think that this approach is mispecified. As shown in the table 5-3, we also find large significant effects of the NMW at the top of the wage distribution. These results indicate a systematic relationship between the effective minimum and upper wage percentiles of the distribution.

5.6 Specification problems

As shown in table 5-3, when we apply our analysis to upper-tail wage inequality (such as the 90/50 and 75/50 wage differentials), we find evidence that an increase in the effective NMW appears to increase upper-tail wage inequality. This casts doubt on whether we are identifying NMW effects. As with Autor, Katx and Kearney (2006) we could consider this a falsification exercise. If theory predicts an effect of the NMW on lower-tail wage inequality, and such evidence is found, it also worthy to explore whether there is also an effect of the NMW on upper tail wage inequality, for which theory does not predict an effect on upper tail wage inequality. In this specific case, the falsification exercise seems to have failed. One obvious question is to find out what might have generated this spurious correlation.

There are two main issues in our OLS main regression equation. First of all, the median wage is present in both sides of the equation and this could generate simultaneity bias. In other words, some of the association of the effective NMW and wage inequality could be mechanical and arise because both the measure of the NMW and of wage inequality include the median log-wage. A second problem concerns measurement error: one of the limitations of the ASHE dataset is that, certainly before 2004, it does not cover those at the lower end of the earnings distribution because anyone not registered for PAYE was not included in the sample frame. Even if recently the ASHE sample has been boosted since then to improve the representation of low paid employees, the dataset still has this problematic issue.

5.6.1 OLS estimates using alternative measures of wage inequality

One possible way of addressing the simultaneity bias problem is to use an alternative measure of wage inequality in the OLS regression equation which does not include the median wage as denominator. We try two different alternative measures: the Atkinson index (Atkinson, 1970) and the Generalised Gini (Yitzhaki, S., 1983). The use of these two measures will also have two other advantages. First of all, the measure of inequality used by Lee (1999) only looks at two distinct data point in the wage distribution. Conversely, the Atkinson index and the Generalised Gini instead of throwing away the great majority of the data, will rather take into account all parts of the distribution. For this reason, these measures will not violate the Pigou-Dalton condition (or the transfer principle) according to which any (even) transfer from a worse-off to a better-off person worsen inequality. Finally, using these measures it is possible to measure inequality under different value judgments; inequality will have a normative basis, derived from a social welfare function which includes values of society regarding equality and justice. A more technical section with the definition of these measures of wage inequality is included in appendix 5.C. One possible objection of these measures is that since they effectively weight all the quantiles of the wage distribution (and weight vary accordingly to the inequality aversion parameter) than they may smooth out the effects of the NMW at specific points in the distribution. We will also look explicitly at different points in the distribution for evidence of different effects.

In table 5-4 these alternative measures of wage inequality are tested. In columns 1, 2, 3 the Atkinson index as a measure of wage inequality is used as the dependent variable, columns 5, 6 and 7 use the Generalized Gini. Again the results are shown for the pooled sample and separately for men and women. The table reports both the coefficients of the relative minimum and its square and also the derivative implied by the coefficients evaluated at the overall mean of the effective minimum over all local areas and years.

The first column refers to an OLS regression with years and areas effects. The second column adds controls for specific demographic characteristics of each area-year cell (such as average education, age and the proportion of women). Finally, the third column also includes local area specific time trends.

As expected, the marginal effects are always negative and significantly different from zero, suggesting that an increase in the effective minimum wage also causes a reduction in these measures of wage inequality. Generally, the coefficients are smaller in absolute size when area specific time trends are included in the analysis. Also, as might be expected, the table shows that among workers with generally higher wages (men) the relative minimum has a more modest impact on wage inequality than for women.

5.6.2 IV estimates

In order to address the measurement error problem in our analysis, it is necessary to first think about a simple linear model of the type:

$$y^* = x^*\beta + \varepsilon \qquad (5.10)$$

Measurement error in the explanatory variable, x, does result in a bias in the OLS estimate of β. To see this, consider the measured value of x as the sum of the true value x* plus a measurement error ux :

$$x = x^* + ux \qquad (5.11)$$

Substituting x* = x - ux in equation (5.10) we now obtain:

$$y^* = x\beta + (\varepsilon - \beta ux) \qquad (5.12)$$

This is obviously a problem since the presence of ux in the error term generates a mechanical correlation between the error term, ε - βux and the explanatory variable, x $= x^* + ux$.

Since the problem in equation (5.12) is that the error term is correlated with x, the standard solution is to find an instrumental variable for x that is correlated with x, but not with the error term (ε - βux).

In particular, when the problem is that x is only an imperfect proxy for the true x*, there may also be other proxies for x* that are available. For instance consider a second proxy z, where:

$$z = x^* + uz \qquad (5.13)$$

Provided that the measurement error in z, uz, is uncorrelated with the measurement error in x, ux we find that z is a valid instrumental variable since:

$$\text{cov}(z,x) = \text{var}(x^*) > 0 \qquad (5.14)$$

and

$$\text{cov}(z, \varepsilon - \beta ux) = \text{cov}(x^* + uz, \varepsilon - \beta ux) = 0 \qquad (5.15)$$

So the simple solution is to use one proxy as an instrument for the other proxy, and consistently estimate β by two stage least squares.

Therefore, one possible way of addressing the measurement error problem is to instrument the effective NMW (and its square) calculated on the ASHE data with the effective NMW (and its square) calculated using the Annual Population Survey (APS) data. The APS is the official labour market survey that collects detailed labour market information and a large array of socioeconomic characteristics. Since the APS is survey based it will provide error-ridden estimates of average earnings across areas and years. This, however, should not invalidate the IV approach. To the extent that measurement error in the ASHE data is uncorrelated with measurement error in the APS data, this procedure will purge the estimates of potential correlation between the included regressors and the error term due to measurement error (Bosch and Manacorda, 2010).

This instrument might also contribute to reduce the simultaneity bias problem. Assuming that in the regression equation we control for all other factors that might affect inequality other than the NMW, than what we are left with in the error term is the stochastic component of inequality. Since our IV is computed from a different dataset totally exogenous to the regression equation, this new measure of the effective NMW is unlikely to be correlated with the error term.

Table 5-5 report the IV estimates of equation 5.9. Specifically, we instrument the effective NMW (and its square) calculated on the ASHE data with the effective NMW (and its square) calculated using the APS special licence data, similarly to what Bosh and Manacorda (2010) did with Mexican data. Estimates are only reported for the pooled sample (men and women) because of the small sample size at local area level for the APS measure of hourly wages for women and men separately. Because of lack of local area data in the APS, the IV results are only available for the seven year period (from 2004 to 2010). For this reason, we do not report results including area-specific time trends. As mentioned above, one should be cautious in interpreting such results because of the loss in terms of degrees of freedom that area-specific time trends might cause. Further, one should be aware that when doing our IV estimation, the panel become shorter (from 2004 to 2010) and imposing a restriction of a trend to be identical across all areas becomes too restrictive for the model.

The entries in the table refer to the first and second stages of the 2SLS regressions and to the estimated first derivative of each dependent variable with respect to the effective NMW evaluated at the sample mean. The first two columns report estimates of the effective minimum and its square on the 10th decile of the wage distribution and the 50th decile respectively. The second two columns presents estimates of the effective minimum and its square on the 5th percentile of the wage distribution on the 50th decile respectively. The first column refers to an IV regression with year and area effects. In the second column controls for specific demographic characteristics of each area year cell (such as average education, age and the proportion of women) are also added. In general, the F-test on the included instruments for the square of the effective NMW (but not its square root) reported at the bottom of the table is large, implying a predictive power of the instruments. The marginal effects of the fixed effect regression estimates are positive and significant for the 5th percentile of the wage distribution, suggesting that an increase in the effective minimum wage causes reduction in lower tail wage inequality. The fixed effects estimates show that a 10 percentage point increase in the effective NMW is associated with a statistically significant rise in the gap between the bottom fifth percentile and the fifth decile of around 10 percentage points (1.008*10). Coefficients for the 10th percentile of the wage distribution are also positive and significant.

Table 5-6 compares marginal effects of the OLS estimation and the 2SLS estimation for the period 2004-2010 when the two data sets overlap. In contrast with the OLS results, it is quite reassuring that in the 2SLS estimation, the point estimates tend to become smaller at higher deciles and are statistically significant only up to the first decile. The regression coefficients turn from being positive for deciles below the median to being negative for higher deciles, but these are not statistically significant.

Figure 5-6 aims to estimate the contribution of the NMW to the decrease in inequality, comparing actual and counterfactual estimates of the changes of the 5th percentile gap. In practice, we use 2SLS regression results to separate changes in the log(10th) – log(50th) percentile of the wage distribution (solid line) into a term attributable to the fall in the real value of the NMW (dotted line) and a term that ecompasses latent changes in inequality (dashed line).

The solid line of figure 5-6 is obtained from a regression of the 5th percentile of the wage distribution relative to the median on additive years and area dummies. The solid line reports the coefficients of the year dummies relative to their value in the first year of the analysis, 2004. The solid line shows how the coefficients of the year dummies increase throughout the 7 years (2004-2010). This means that, by simply observing the data, there was a decrease in lower tail wage inequality in the period of interest.

The dashed line in figure 5-6 shows the coefficients of the year dummies when a linear and quadratic term of the NMW are also included in the regression. In this case, the coefficients of the year dummies decrease. This suggests that latent lower tail wage inequality (wage inequality which would have occurred without the presence of the NMW following our model) would have increased throughout the period.

Finally, the dotted line in figure 5-6 reports the estimated contribution of the NMW to changes in wage inequality (ie. solid line minus dashed line). The trends in inequality due to the increase in the real value of the NMW are showing clearly how the NMW contributed to the decrease in wage inequality and specifically lower tail wage inequality.

Figure 5-7 repeats the same exercise for the log(10^{th}) – log(50^{th}) percentile of the wage distribution. The patterns of the trend of the actual, latent and the contribution of the NMW are similar to those shown in figure 5-7.

5.7 Conclusions

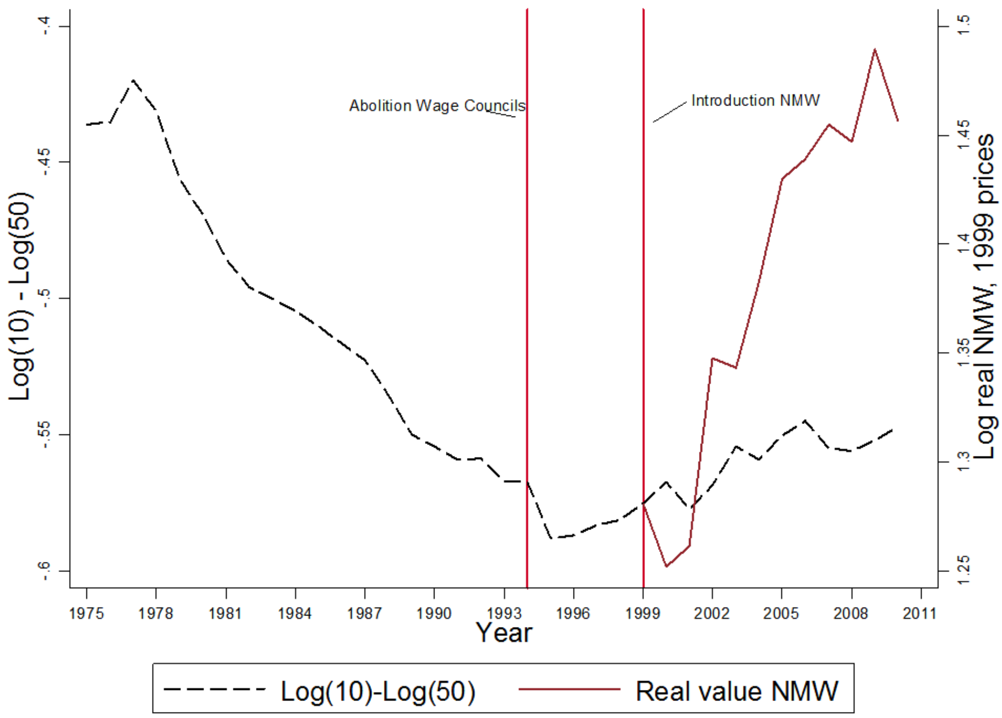
The chapter used the Annual Population Survey micro data from the UK from 1999 to 2010 to analyze the contribution of the NMW in the trend towards lower tail inequality in UK hourly wages. One of the motivations for the introduction of the NMW was to help to "make work pay" and address in-work poverty, against a background of rising wage inequality which characterised the British labour market in the 1980s and early 1990s. At the outset, the Low Pay Commission (1998) hoped that the NMW might take "greater inroads into pay equality" without putting jobs at risk. For this reason the chapter aimed at having a deeper insight of the impact of the NMW on wage inequality.

In order to estimate the effect of the NMW in the recent trend towards lower tail wage inequality in the UK, the methodology exploited by Lee (1999) for the US was exploited. We assumed that the level of wages would have been similar across areas (or would have changed similarly), if it were not for the effect of the NMW. Differences in average wage levels across areas induced useful variation in the real "bite" of the NMW across areas that allowed the identification of its effect net of other confounding forces.

Few issues that appear to bias Lee’s (1999) work were addressed, namely, the omitted variable bias due to the absence of area fixed effects (Autor et al, 2010), measurement error in the variables (Bosh and Manacorda, 2010) and simultaneity bias. We addressed measurement error in the wage data by instrumenting our measure of the NMW computed with the Annual Survey of Hours and Earnings (ASHE) data with a new NMW measure computed using the Annual Population Survey (APS). To the extent that measurement error in the ASHE data is not correlated with measurement error in the APS data, this procedure purges the estimates of potential correlation between the included regressors and the error term due to measurement error. We also attempted to solve the simultaneity bias problem in our data using alternative measures of wage inequality in our regression, such as the Atkinson index and the Generalised Gini.

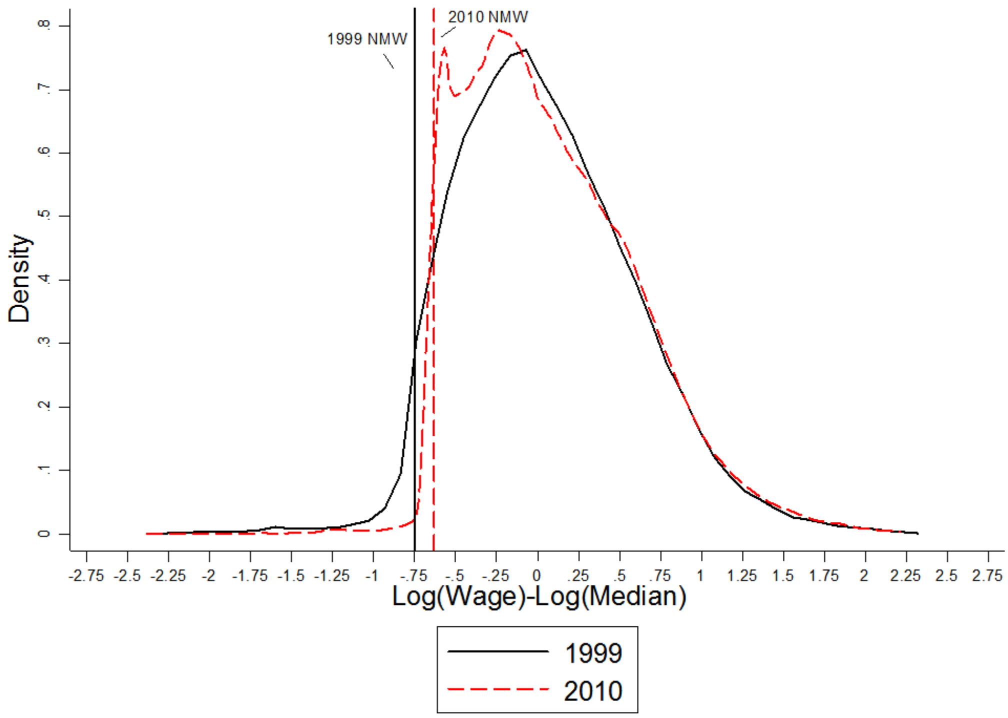
By looking at wage differentials of different percentiles q relative to the median wage ($w^{q}_{it} - w^{50}_{it}$), we find an effect of the NMW on the wage distribution. Using 2SLS estimation, the point estimates tend to become smaller at higher deciles and are statistically significant only up to the first decile, suggesting a negative impact of the NMW just on lower tail wage inequality and perhaps some small spill-over effects of the NMW. By looking also at different measures of wage inequality, such as the Atkinson index and the Generalised Gini, we find again a significant contribution of the NMW in reducing wage inequality.

Figure 5-1. Trends in wage inequality and the real value of the NMW (pooled sample)



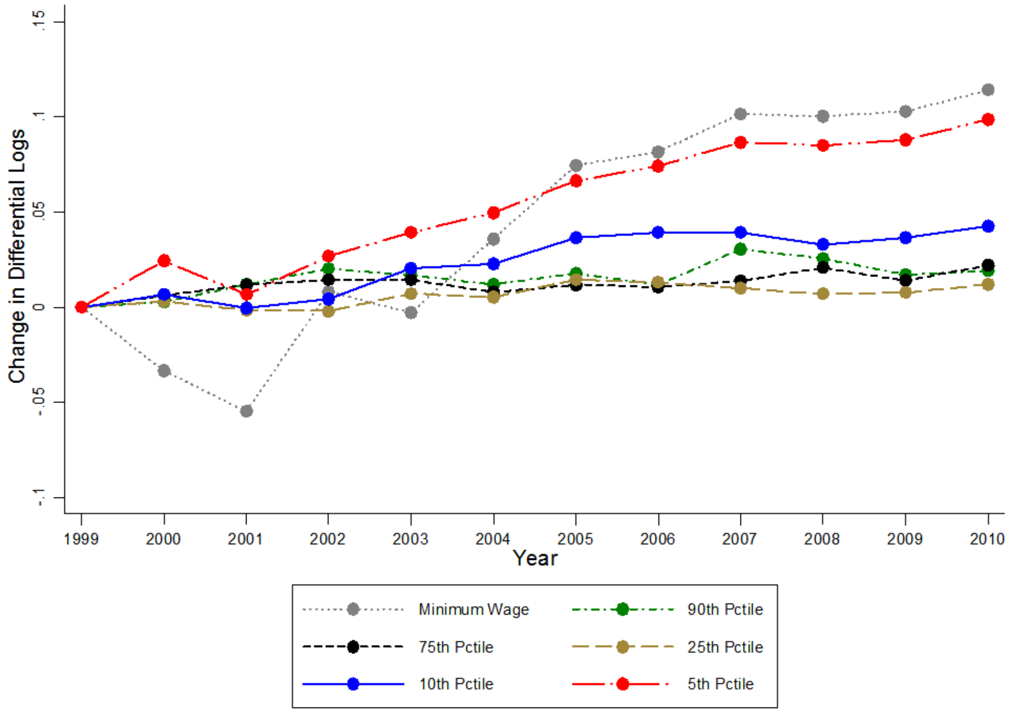
Source: NES (1975-2010). Real value of the NMW in 1999 prices (source RPI all items: ONS). Author's calculations.

Figure 5-2. Wage distribution Density Estimates (pooled sample), 1999-2010



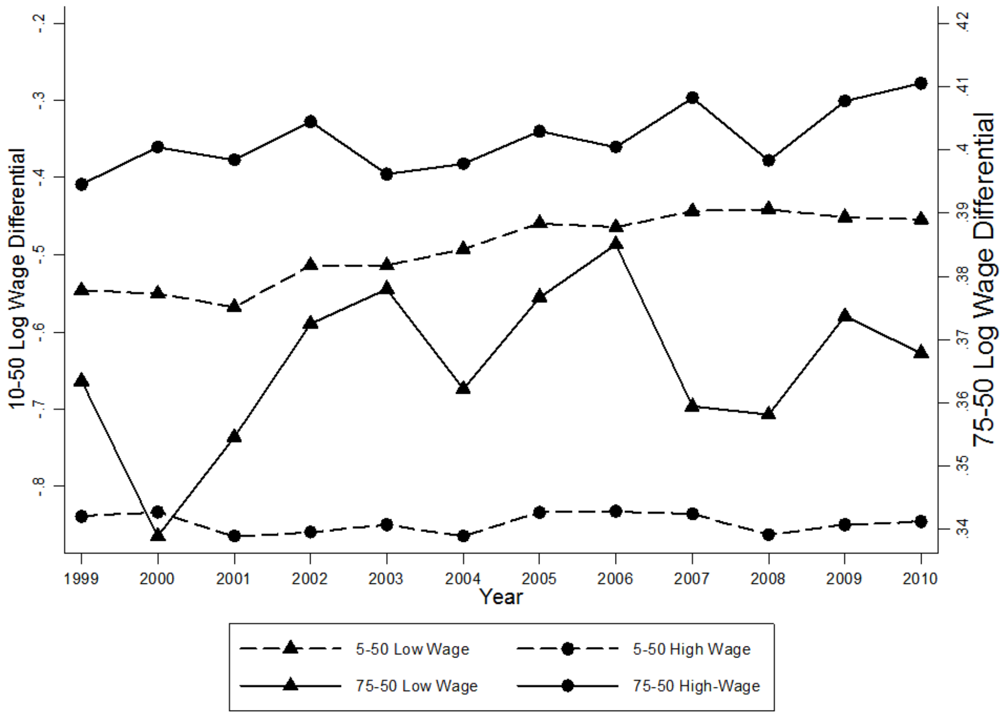
Source: ASHE(1999-2010). Author's calculations.

Figure 5-3. Selected percentiles of the Wage Distribution, Minimum wage relative to the Median: 1999-2009**.** All series are Normalised to be 0 in 1999.



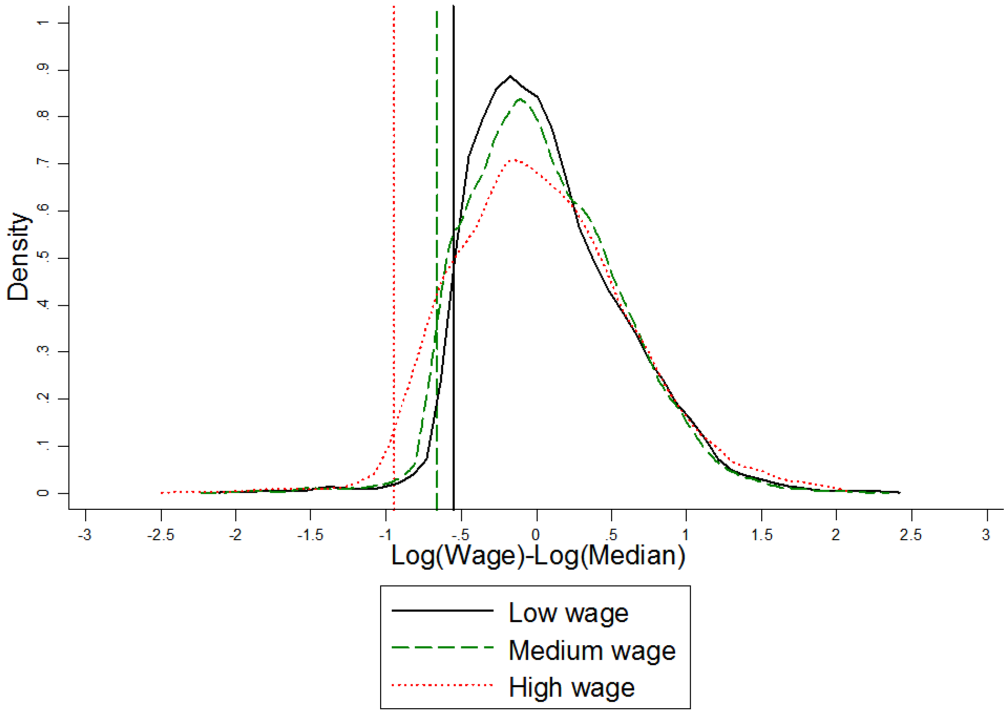
Source: ASHE (1999-2010). Author's calculations. All series are normalised to be 0 in 1999.

Figure 5-4. 5-50, 75-50 Log(Wage) Differentials; High versus Low-wage areas 1999-2010.



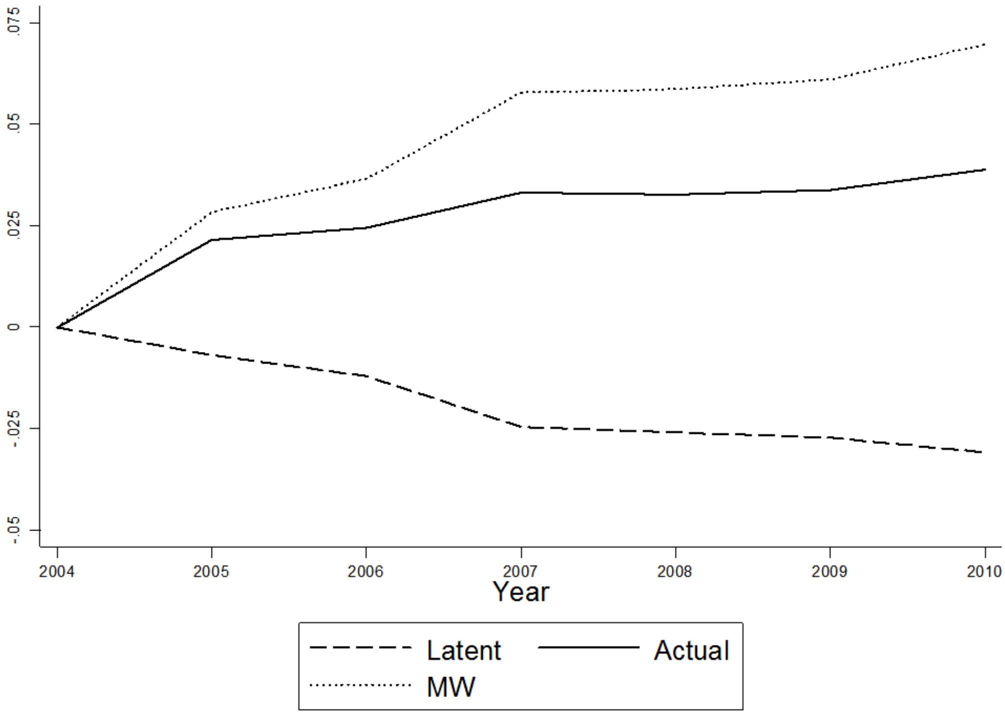
Source: ASHE (1999-2010). Author's calculations.

Figure 5-5. Wage distribution density estimates: Low-, medium- and High-wage Local areas, 1999.



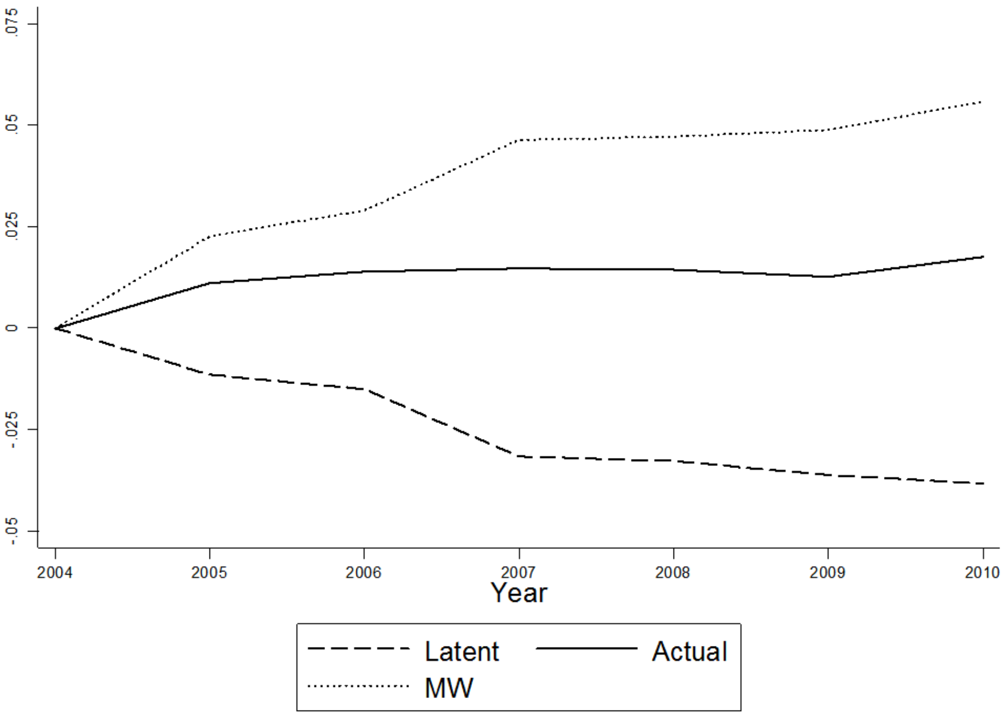
Source: ASHE (1999-2010). Author's calculations.

Figure 5-6. Actual and Latent Trends in Inequality and the Effect of the NMW Log(5^{th}) – Log(50^{th}) percentile of the wage distribution



Source: ASHE (1999-2010). Author's calculations.

Figure 5-7. Actual and Latent Trends in Inequality and the Effect of the NMW
Log(10^{th}) – Log(50^{th}) percentile of the wage distribution



Source: ASHE (1999-2010). Author's calculations.

Table 5-1. Relationship between log(60) and log(40) and log(50) percentiles of the wage distribution: OLS estimates

	Log(60) – log(40)		
	(1)	*(2)*	*(3)*
		Pooled sample	
Median	0.081***	0.084***	0.129***
	(0.009)	(0.022)	(0.021)
Observations	1680	1680	1680
R-squared	0.233	0.629	0.734
		Men	
Median	0.146***	0.070***	0.057**
	(0.007)	(0.021)	(0.024)
Observations	1680	1680	1680
R-squared	0.403	0.672	0.722
		Women	
Median	0.054***	0.030	0.079***
	(0.009)	(0.022)	(0.024)
Observations	1680	1680	1680
R-squared	0.106	0.511	0.632
Year effects	Y	Y	Y
Area effects	Y	Y	Y
Area trends	N	N	Y
Controls	N	Y	Y

Notes: HAC robust fixed effect estimates in brackets. Weighted by observation per area-year.
*** $p < 0.01$ ** $p < 0.05$, * $p < 0.1$.

Table 5-2. OLS estimated relationship between log(q)-log(50) and log(NMW)-log(50)

	10th percentile – Median			*5th percentile – Median*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
			Pooled sample			
NMW - Median	1.583***	1.562***	0.942***	1.900***	1.859***	1.163***
	(0.098)	(0.095)	(0.077)	(0.106)	(0.101)	(0.095)
(NMW - Median)^2	0.700***	0.682***	0.216***	0.813***	0.779***	0.288***
	(0.070)	(0.069)	(0.057)	(0.076)	(0.074)	(0.073)
Marginal effects	0.676***	0.678***	0.662***	0.846***	0.850***	0.789***
	(0.023)	(0.023)	(0.023)	(0.025)	.(0.024)	-0.022
Observations	1,680	1,680	1,680	1,680	1,680	1,680
R-squared	0.961	0.961	0.973	0.979	0.979	0.985
			Men			
NMW - Median	1.434***	1.377***	0.931***	1.691***	1.691***	1.090***
	(0.081)	(0.076)	(0.086)	(0.090)	(0.090)	(0.089)
(NMW - Median)^2	0.517***	0.475***	0.195***	0.604***	0.604***	0.205***
	(0.048)	(0.045)	(0.051)	(0.055)	(0.055)	(0.055)
Marginal effects	0.638***	0.644***	0.630***	0.760***	0.768***	0.774***
	(0.027)	(0.027)	(0.025)	(0.027)	(0.026)	(0.026)
Observations	1,680	1,680	1,680	1,680	1,680	1,680
R-squared	0.933	0.934	0.949	0.950	0.950	0.960
			Women			
NMW - Median	1.454***	1.431***	0.895***	1.692***	1.661***	1.169***
	(0.095)	(0.092)	(0.091)	(0.070)	(0.066)	(0.087)
(NMW - Median)^2	0.703***	0.679***	0.207**	0.776***	0.744***	0.326***
	(0.087)	(0.084)	(0.087)	(0.059)	(0.054)	(0.082)
Marginal effects	0.721***	0.724***	0.680***	0.883***	0.885***	0.829***
	(0.022)	(0.022)	(0.023)	(0.030)	(0.029)	(0.022)
Observations	1,680	1,680	1,680	1,680	1,680	1,680
R-squared	0.960	0.961	0.972	0.972	0.972	0.980
Year effects	Y	Y	Y	Y	Y	Y
Area effects	Y	Y	Y	Y	Y	Y
Area trends	N	N	Y	N	N	Y
Controls	N	Y	Y	N	Y	Y

*Notes:*see table 5-1.

Table 5-3. OLS relationship between log(q)-log(50) and log(NMW)-log(50) for selected percentiles of given wage distribution. Marginal effects reported.

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
		Pooled sample			***Women***			***Men***	
5	0.846***	0.844***	0.797***	0.883***	0.885***	0.829***	0.760***	0.768***	0.774***
	(0.025)	(0.023)	(0.022)	(0.030)	(0.029)	(0.022)	(0.027)	(0.026)	(0.026)
10	0.676***	0.678***	0.662***	0.721***	0.724***	0.680***	0.638***	0.644***	0.630***
	(0.023)	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.027)	(0.027)	(0.025)
20	0.417***	0.432***	0.439***	0.491***	0.491***	0.517***	0.439***	0.440***	0.445***
	(0.025)	(0.026)	(0.025)	(0.023)	(0.023)	(0.024)	(0.032)	(0.033)	(0.040)
30	0.285***	0.300***	0.315***	0.315***	0.314***	0.357***	0.345***	0.344***	0.369***
	(0.020)	(0.021)	(0.022)	(0.023)	(0.023)	(0.022)	(0.023)	(0.023)	(0.026)
40	0.194***	0.209***	0.241***	0.160***	0.160***	0.195***	0.195***	0.196***	0.219***
	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)	(0.015)
75	0.217***	0.205***	0.211***	0.212***	0.213***	0.153***	0.228***	0.232***	0.259***
	(0.028)	(0.030)	(0.031)	(0.029)	(0.029)	(0.031)	(0.025)	(0.025)	(0.027)
90	0.367***	0.341***	0.326***	0.467***	0.466***	0.397***	0.387***	0.391***	0.414***
	(0.043)	(0.045)	(0.044)	(0.042)	(0.042)	(0.045)	(0.036)	(0.036)	(0.039)
Year effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Area effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Area trends	N	N	Y	N	N	Y	N	N	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y

*Notes:*see table 5-1. N= 1680.

Table 5-4. OLS relationship between different measures of wage inequality and log(NMW)-log(50)

	Atkinson Index			*Generalised Gini*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
				Pooled sample		
NMW - Median	-1.174***	-1.199***	-0.632	-0.959***	-0.955***	-0.583***
	(0.240)	(0.251)	(0.388)	(0.063)	(0.064)	(0.065)
(NMW - Median)^2	-0.560***	-0.583***	-0.220	-0.487***	-0.484***	-0.219***
	(0.138)	(0.149)	(0.247)	(0.043)	(0.044)	(0.042)
Marginal effects	-0.449***	-0.445***	-0.346**	-0.329***	-0.329***	-0.299***
	(0.124)	(0.124)	(0.146)	(0.023)	(0.023)	(0.025)
R-squared	1,680	1,680	1,680	1,680	1,680	1,680
Observations	0.629	0.629	0.659	0.939	0.939	0.951
				Men		
NMW - Median	-0.967***	-0.818**	0.404	-0.744***	-0.726***	-0.529***
	(0.325)	(0.336)	(0.473)	(0.050)	(0.051)	(0.065)
(NMW - Median)^2	-0.407**	-0.303	0.441	-0.307***	-0.295***	-0.171***
	(0.187)	(0.196)	(0.272)	(0.027)	(0.029)	(0.035)
Marginal effects	-0.340***	-0.352***	-0.276**	-0.270***	-0.272***	-0.265***
	(0.123)	(0.121)	(0.136)	(0.021)	(0.021)	(0.022)
Observations	1,680	1,680	1,680	1,680	1,680	1,680
R-squared	0.541	0.543	0.588	0.913	0.913	0.930
				Women		
NMW - Median	-1.109***	-1.210***	-1.156**	-0.997***	-0.997***	-0.723***
	(0.280)	(0.287)	(0.455)	(0.079)	(0.080)	(0.105)
(NMW - Median)^2	-0.656***	-0.762***	-0.827**	-0.604***	-0.603***	-0.361***
	(0.207)	(0.218)	(0.400)	(0.063)	(0.064)	(0.090)
Marginal effects	-0.426***	-0.416***	-0.295*	-0.367***	-0.369***	-0.347***
	(0.139)	(0.139)	(0.158)	(0.031)	(0.031)	(0.035)
Observations	1680	1,680	1,680	1,680	1,680	1,680
R-squared	0.588	0.589	0.621	0.878	0.878	0.902
Year effects	Y	Y	Y	Y	Y	Y
Area effects	Y	Y	Y	Y	Y	Y
Area trends	N	N	Y	N	N	Y
Controls	N	Y	Y	N	Y	Y

*Notes:*see table 5-1.

Table 5-5. 2SLS relationship between log(q)-log(50) and log(NMW)-log(50)
IV: median wages from the LFS. Results reported starting from 2004

	10th percentile – Median		***5th percentile – Median***	
	(1)	*(2)*	*(3)*	*(4)*
		Pooled sample		
First stage				
NMW – Median				
NMW(LFS)	-0.079	-0.042	-0.079	-0.042
	(0.072)	(0.072)	(0.072)	(0.072)
NMW (LFS) ^2	-0.089	-0.060	-0.089	-0.060
	(0.060)	(0.060)	(0.060)	(0.060)
(NMW-Median)^2				
NMW(LFS)	0.688***	0.637***	0.688***	0.637***
	(0.175)	(0.169)	(0.175)	(0.169)
NMW (LFS) ^2	0.603***	0.563***	0.603***	0.563***
	(0.147)	(0.143)	(0.147)	(0.143)
Second stage				
NMW - Median	2.062***	2.048***	2.396***	2.353***
	(0.516)	(0.458)	(0.485)	(0.423)
(NMW - Median)^2	1.016***	1.005***	1.132***	1.109***
	(0.155)	(0.149)	(0.120)	(0.107)
Marginal effects	0.830*	0.828**	1.022***	1.008***
	(0.437)	(0.410)	(0.394)	(0.364)
Observations	980	980	980	980
F-test linear	1.790	1.400	1.790	1.400
P-value	[0.167]	[0.247]	[0.167]	[0.248]
F-test quadratic	8.570	7.960	8.570	7.960
P-value	[0.000]	[0.000]	[0.000]	[0.000]
Year effects	Y	Y	Y	Y
Area effects	Y	Y	Y	Y
Area trends	N	N	N	N
Controls	N	Y	N	Y

*Notes:*see table 5-1. Results are presented for a restricted period (2004-2010): hourly wages at local area level in the APS are only available from 2004.

Table 5-6. OLS and 2SLS relationship between log(q)-log(50) and log(NMW)-log(50) for select percentiles of given wage distribution. IV: median wages from the LFS. Results reported starting from 2004. Marginal effects reported.

	OLS from 2004		*2SLS from 2004*	
	(1)	*(2)*	*(3)*	*(4)*
		Pooled sample		
5	0.867***	0.874***	1.022***	1.008***
	(0.024)	(0.024)	(0.394)	(0.364)
10	0.723***	0.727***	0.830*	0.828**
	(0.035)	(0.036)	(0.437)	(0.410)
20	0.428***	0.428***	0.765	0.845*
	(0.030)	(0.031)	(0.497)	(0.485)
30	0.298***	0.300***	0.591	0.623
	(0.024)	(0.025)	(0.425)	(0.403)
40	0.230***	0.230***	0.524	0.549
	(0.018)	(0.018)	(0.366)	(0.346)
75	0.199***	0.202***	-0.048	-0.076
	(0.041)	(0.043)	(0.629)	(0.591)
90	0.303***	0.300***	-1.004	-0.914
	(0.059)	(0.062)	(1.235)	(1.112)
Year effects	Y	Y	Y	Y
Area effects	Y	Y	Y	Y
Area trends	N	N	N	N
Controls	N	Y	N	Y

*Notes:*see table 5-1. Results are presented for a restricted period (2004-2010): hourly wages at local area level in the APS are only available from 2004. N=980.

Appendix 5.A

Robustness checks

Table A 5-1. Robustness checks. Results in first differences

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
		Pooled sample			*Women*			*Men*	
5	0.834***	0.833***	0.824***	0.857***	0.857***	0.859***	0.760***	0.764***	0.761***
	(0.026)	(0.026)	(0.025)	(0.023)	(0.023)	(0.021)	(0.029)	(0.028)	(0.028)
10	0.680***	0.679***	0.672***	0.675***	0.676***	0.672***	0.646***	0.648***	0.648***
	(0.031)	(0.031)	(0.030)	(0.026)	(0.026)	(0.028)	(0.026)	(0.026)	(0.026)
20	0.437***	0.435***	0.433***	0.492***	0.493***	0.493***	0.477***	0.477***	0.483***
	(0.027)	(0.027)	(0.027)	(0.031)	(0.030)	(0.031)	(0.032)	(0.032)	(0.031)
30	0.347***	0.345***	0.346***	0.350***	0.350***	0.351***	0.404***	0.406***	0.411***
	(0.024)	(0.024)	(0.023)	(0.026)	(0.026)	(0.026)	(0.027)	(0.027)	(0.026)
40	0.241***	0.240***	0.242***	0.220***	0.220***	0.222***	0.232***	0.233***	0.238***
	(0.018)	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)	(0.019)	(0.019)	(0.018)
75	0.258***	0.257***	0.253***	0.202***	0.202***	0.199***	0.345***	0.346***	0.348***
	(0.032)	(0.032)	(0.032)	(0.033)	(0.033)	(0.032)	(0.028)	(0.028)	(0.028)
90	0.447***	0.445***	0.445***	0.452***	0.451***	0.450***	0.472***	0.474***	0.480***
	(0.043)	(0.043)	(0.043)	(0.047)	(0.047)	(0.046)	(0.042)	(0.043)	(0.042)
Year effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Area effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Area trends	N	N	Y	N	N	Y	N	N	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y

*Notes:*see table 5-1. N=1540.

Table A 5-2. Robustness checks. Unweighted results

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
		Pooled sample			*Women*			*Men*	
5	0.803***	0.803***	0.806***	0.797***	0.796***	0.840***	0.759***	0.763***	0.775***
	(0.019)	(0.019)	(0.021)	(0.058)	(0.059)	(0.031)	(0.030)	(0.030)	(0.030)
10	0.718***	0.720***	0.732***	0.749***	0.749***	0.767***	0.671***	0.677***	0.659***
	(0.021)	(0.020)	(0.021)	(0.019)	(0.019)	(0.022)	(0.028)	(0.028)	(0.027)
20	0.520***	0.520***	0.531***	0.580***	0.580***	0.613***	0.505***	0.506***	0.502***
	(0.023)	(0.023)	(0.023)	(0.021)	(0.021)	(0.022)	(0.026)	(0.026)	(0.029)
30	0.359***	0.359***	0.385***	0.420***	0.420***	0.453***	0.392***	0.392***	0.412***
	(0.023)	(0.023)	(0.023)	(0.024)	(0.024)	(0.024)	(0.022)	(0.022)	(0.024)
40	0.219***	0.217***	0.249***	0.222***	0.221***	0.255***	0.219***	0.221***	0.239***
	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.022)	(0.017)	(0.017)	(0.019)
75	0.190***	0.193***	0.209***	0.179***	0.178***	0.144***	0.219***	0.225***	0.266***
	(0.033)	(0.033)	(0.034)	(0.043)	(0.043)	(0.049)	(0.030)	(0.030)	(0.031)
90	0.420***	0.419***	0.384***	0.543***	0.543***	0.460***	0.317***	0.324***	0.340***
	(0.050)	(0.051)	(0.050)	(0.049)	(0.049)	(0.049)	(0.047)	(0.047)	(0.049)
Year effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Area effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Area trends	N	N	Y	N	N	Y	N	N	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y

*Notes:*see table 5-1. Results are NOT weighted by observation by area-year. N=1680

Appendix 5.B

Descriptive statistics

Table B 5-3. Mean of the main dependent and independent variables.

		Pooled	*Men*	*Women*	*Pooled*	*Men*	*Women*
		1999-2010 sample			*2004-2010 sample*		
					Panel A		
Wage (in)equality measures							
Log(5) - Log(50)		-0.597	-0.653	-0.496	-0.571	-0.638	-0.473
Log(10) - Log(50)		-0.515	-0.541	-0.435	-0.501	-0.536	-0.422
Log(20) - Log (50)		-0.373	-0.377	-0.326	-0.367	-0.376	-0.322
Log(30) - Log(50)		-0.243	-0.245	-0.218	-0.240	-0.245	-0.216
Log(40) - Log(50)		-0.123	-0.123	-0.109	-0.123	-0.123	-0.107
Log(75) - Log(50)		0.387	0.367	0.372	0.387	0.370	0.374
Log(90) - Log(50)		0.741	0.718	0.740	0.742	0.724	0.739
Aktinson Index		0.553	0.530	0.490	0.526	0.512	0.461
Generalised Gini		0.529	0.539	0.491	0.520	0.534	0.481
					Panel B		
Minimum Wage bindingness							
Log(NMW)- Log(50)	ASHE	-0.648	-0.770	-0.521	-0.607	-0.722	-0.492
Log(NMW)-Log(50)	LFS	_	_	_	-0.572	-0.692	-0.459

Appendix 5.C

Measures of wage inequality

In his seminal paper in 1970, Atkinson suggested an index of inequality that makes it possible to measure inequality under different value judgments. He assumes that introducing social values concerning inequality can be done by using a social-welfare function which simply ranks all the possible states of societies (in this case individual wages) in the order of society's preference. The social welfare-function would be an additively separable and symmetric function of individual incomes:

$$W = \sum_{i=1}^{n} U(y_i) = U(y_1) + U(y_2) + \cdots + U(n)$$

For each specific area and year in our analysis, *W* is the social welfare function, y_i is the hourly wage of the *i*th person. U(y_i) gives an individual's welfare as a function of his wage. It can be called social utility of person i. U(y_i) is assumed to be the same for all individuals, an increasing function and concave. The rate at which this index increases is the social marginal utility or welfare weight U'(y_i). The main function of this marginal utilities is to act as a system of weights when summing the effects of a redistribution of wages over the whole population. If a person wage increases we know from the strict concavity assumption that his welfare weight decreases. But to know by how much, it is important to introduce the concept of inequality adversion parameter ε, which is the elasticity of the social-welfare function and it is supposed to be constant: if a person's income increases by 1%, his welfare weight drops by ε% of its former value. The higher is ε, the faster is the rate of proportional decline in welfare weight to proportional increase in income. Therefore, ε describes the strength of our desire for equality in relation to uniformly higher income for all.

Given these assumptions a simple formula for the Atkinson index can be given by:

$$A_{\varepsilon} = 1 - \frac{y_{\varepsilon}}{\bar{y}}$$

Where $\bar{y}$ is the mean of the actual distribution of income:

$$\bar{y} = \frac{1}{n}\sum_{i=1}^{n} y_i$$

y_e is the equally distributed equivalent level of income or the income corresponding to average social utility:

$$y_\varepsilon = \frac{1}{n}\sum_{i=1}^{n} U(y_i)$$

Since each $U(y_i)$ is taken to be concave (i.e. with non increasing marginal social utility, y_ε cannot be larger than the mean income $\bar{y}$. Further, it can be shown that the more equal the distribution the closer will y_ε be to $\bar{y}$. Obviously, when y_e is equal to $\bar{y}$, wages are equally distributed and the Atkinson index will be zero. Also, for any distribution the value of A_ε should lie between zero and one. As Atkinson observes, if "A_ε falls, the distribution has become more equal- we would require a higher level of equally distributed income relative to the mean to achieve the same level of social welfare as the actual distribution". The value judgment in the Atkinson index is expressed by the inequality adversion parameter ε, which ranges from zero representing indifference to inequality to infinity, representing the Rawlsian criterion, which evaluated distributions according to the income of the poorest in the society. This reflects our relative sensitivity to redistribution from the rich to the not-so-rich *vis à vis* redistribution from the not-so-poor to the poor. If a low value of ε is used (i.e. adversion declines), we are particularly sensitive to changes in distribution at the top end of the parade and the Atkinson gives more weight to the upper end of the income distribution. If a high value is employed, then it is the bottom end of the parade which concerns us most, and the Atkinson measure gives more weight to the lower end of the income distribution.

Another measure of wage inequality is the Gini coefficient, a popular and widely used index of measuring inequality. It can be represented geometrically by the Lorenz curve but unlike Atkinson's index it does not explicitly express the value judgment underlying it. However, an interesting extension of the Gini coefficient proposed by Yitzhaki (1983), has most of the properties of the Atkinson's index. A distributional judgment parameter *v* can be selected by the analyst and the role of *v* is similar in some significant respects to the role of the inequality adversion parameter ε defining the Atkinson index. The behavior of the extended Gini in the extremes, $v \to 1$ and $v \to \infty$, resembles that of the Atkinson index at the extremes $\varepsilon \to 0$ and $\varepsilon \to \infty$ of inequality adversion.

We therefore choose to use these two bottom-sensitive inequality index, such as Atkinson's with ε=5 and the extended Gini with *v*=10.

6. Conclusions

This thesis in applied labour economics consisted of four research chapters that were devoted to the empirical analysis of the impact of the Minimum Wage (MW) on various labour market outcomes, identifying the effect by exploiting variation of its "bite" across areas and years.

Once proved empirically that the MW is binding, or, in other words, that it has an effect on the wage distribution, it was valuable to look at the employment effects of a MW. This is perhaps one of the most contentious issue in labour economics. Contrasting theories (Stigler (1946), Burdett and Mortensen (1998), Manning (2003)) suggest that a minimum wage can have either positive, negative or neutral effects on employment depending on one's priors and so ultimately the effects must be a matter for empirical verification. Despite more than 30 years of empirical work in this area, the effects are still disputed and vary across space and time generating room for continued work in this area.

It was also important to investigate the impact of the MW on other labour market outcomes. First of all, we looked at unemployment as a robustness check for the employment results. Moreover, the effects on unemployment might differ from the effects on employment for several reasons. If we suppose that the MW causes job losses, some of those who leave their job as a result of the policy might feel discouraged and become inactive, thus leaving the labour force. These individuals will no longer counted as unemployed. Furthermore, the MW could induce an increase in labour supply if additional individuals enter the labour force to search for the now more attractive jobs, this will lead unemployment to increase. Secondly, we also looked at working hours. Labour market adjustments due to the MW may take place either at the extensive margin or at the intensive margin. Most of the literature focuses on the extensive margin (employment), it was therefore worthwhile to additionally look at the impact on working hours.

The thesis consisted of four chapters. Most of the analysis focused on the UK. However, one chapter used cross-country data. This allowed us to take into account the business cycle and to focus on young people, who are more likely to be affected by MW legislation.

First of all, the thesis focused on the UK, where a National Minimum Wage (NMW) was introduced in 1999 and up-rated in each year since then. This rather extended length of time since implementation constituted an opportunity to take a retrospective look at the medium-run impact of the NMW on inequality, employment, unemployment and hours of work, where previous studies had only looked at the short-run impacts.

In chapter 2 and chapter 3, identification was based on variation in the "bite" of the NMW across local labour markets and the different sized year on year up-ratings. Instead of applying a simply policy-on/policy-off Difference-in-Differences (DiD) model, we used an "Incremental Difference-in-Differences" (IDiD) model in which each year's change in the NMW was considered as a separate interaction effect. This procedure allowed us to evaluate the year on year impact of the up-rating of the NMW on different labour market outcomes, using data drawn from the Annual Survey of Hours and Earnings (ASHE) and the Labour Force Survey (LFS).

In relation to findings on inequality it appears that raising the NMW is associated with reduced lower tail wage inequality in a systematic way each year since its introduction. Overall there seems to be no significant association of the NMW on employment, however, when we use the IDiD estimation method we retrieve significant positive effects on employment in recent years. Most specifically in the period 2004-6.

Our findings also suggest that unemployment fell in areas where the NMW had the strongest "bite" during the second half of the sample period. Finally, hours worked by part-timers grew more in areas more affected by the NMW. When we consider the effect on full-time hours, it would appear that there are no significant effects.

These two first chapters focused on the medium-run effects of the NMW in the UK, a period which covered years of positive growth. However, the effects of an up-grade in the MW may not be constant across the economic cycle. This was one of the reasons of the analysis in chapter 4 which focused on a panel of 33 OECD and European countries for the period 1971-2009. The purpose of the analysis was to produce new cross-country estimates of the employment effects of the MW. Using international data, cross-national variation in the level and timing of the MW uprating was exploited. The panel allowed us to take into account the institutional and other policy related differences that might had an impact on employment other than the MW. The long panel also allowed us to differentiate the effect of MWs on employment in periods of economic downturn as well as in periods of economic growth, exploiting the exact timing of the recessionary experiences in different countries. The study advanced in many ways the earlier cross-country study of the impact of the MW on employment (Neumark and Wascher, 2004). The first advance related to extending the dataset of Neumark and Wascher (2004), by including more countries and by extending the time period under scrutiny. The second contribution of this study was to generalise the controlling environment to allow for the fact that countries are introducing new employment policies, or changing them very frequently. In the literature to date this has not been adequately modelled. Another area of advance related to one of the starting point and guiding motivation of this research which is to examine the effects of the interaction between the depth of the recession and the MW. Another contribution was to use different measures of the "bite" of the MW, checking the robustness of the results. As in Neumark and Wascher (2004) and in most of the MW literature, the Kaitz index defined as the ratio of the MW to an average wage was used. However, the percentile at which the MW "bites" the wage distribution and the MW relative to GDP per head were also looked at. The final area of our robustness investigation was that we attempt to explore the difficult problem of the possible endogeneity of the MW or Kaitz variable. The core problem with any MW regression, however formulated, is that arguably the measurement errors of the determination of employment are not independent of the "bite" of the MW. This is true to the extent that any country's government which invokes a MW (or has favourable policies relating to its up-rating in real terms) will also have unmeasured, unobserved attributes which separately affect the employment level. In this sense – one cannot reasonably assume that the variable which measures the MW is a valid exogenous variable to be included on the right hand side of such an equation. For this reason a ‘political complexion of the government’ instrumental variable (the Schmidt Index) was used.

A number of interesting conclusions might be drawn from this analysis. First, that cross-country regressions can be plagued by unobserved heterogeneity which gives rise to endogeneity problems. Secondly, that the Kaitz index in this context may well be an endogenous variable and that if ones take into account this endogeneity one gets reassuringly stable IV. Finally, it would appear that the conclusions regarding the employment effects of the MW are very different for adults and young people. When the IV identification strategy is used, results show that there is a small negative employment effect of the MW for young people, but that this effect is no present for adults. It would appear that the most vulnerable groups in the labour market, whose wages are closer to the MW, are most affected by it. Also the negative effect for young people appears to be more pronounced during recessions.

Chapter 5 aimed at deepening the understanding of whether the NMW contributed to the decrease of lower tail wage inequality in the UK, looking at the period from the introduction of the policy in 1999 up to 2010.

In order to estimate the effect of the NMW in the recent trend towards lower tail wage inequality in the UK, the methodology exploited by Lee (1999) for the US was used. We assumed that the level of wages would have been similar across areas (or would have changed similarly), if it were not for the effect of the NMW. Differences in average wage levels across areas, that were assumed to be exogenous, therefore induced useful variation in the real "bite" of the NMW across areas that allowed the identification of its effect net of other confounding forces.

Three issues that appeared to bias Lee’s (1999) work were addressed, namely, the omitted variable bias due to the absence of area fixed effects (Autor et al, 2010), measurement error in the variables (Bosh and Manacorda, 2010) and simultaneity bias. We addressed the measurement error in the wage data by instrumenting our measure of the NMW computed with the Annual Survey of Hours and Earnings (ASHE) data with a new NMW measure computed using the Annual Population Survey (APS). To the extent that measurement error in the ASHE data is not correlated with measurement error in the APS data, this procedure will purge the estimates of potential correlation between the included regressors and the error term due to measurement error. We also attempted to solve the simultaneity bias problem in our data using alternative measures of wage inequality in our regression, such as the Atkinson index and the Generalised Gini.

By looking at wage differentials of different percentiles q relative to the median wage ($w^{q}_{it} - w^{50}_{it}$), the results suggests an effect of the NMW on the wage distribution. Using 2SLS estimation, the point estimates tend to become smaller at higher deciles and are statistically significant only up to the first decile, suggesting a negative impact of the NMW just on lower tail wage inequality, and also some small spillover effects further up the distribution. By testing also different measures of wage inequality, such as the Atkinson index and the Generalised Gini, we find again a significant contribution of the NMW in reducing wage inequality.

In a period of positive growth in the UK (1999 to 2007), while the overall effect on the NMW on employment rate averaged over its existence was neutral, we found small positive employment effects from 2003 to 2007. Similarly, the association of the NMW with unemployment was negative in those years. With the suspicion that the effects of an upgrade in the MW may not be constant across the economic cycle and across different age groups (adults and young) we undertook cross-country analysis to see whether the effect of the MW diverge with the aggregate economic conditions for the young and adult population. We find basically no significant detrimental effect of the MW on adult employment. We also find a small detrimental effect of the MW on the employment of young people, with this effect exacerbated in periods of economic downturn. Finally, we followed Lee (1999) to analyse whether the NMW in the UK contributed to reduce lower tail wage inequality from 1999 to 2010. We find confirmation that the NMW actually contributed to the trend towards lower wage inequality in the UK in the last 12 years.

Different issues could be further analysed in future research. An important component of our analysis was to analyse whether the effect of the MW is different across the business cycle using cross-country data. Up-dating the analysis of the employment impact of the NMW in the UK using more years of analysis would help to further understand the issue of a possible detrimental effect of the NMW in periods of downturn. Also it would help, if possible, to investigate the possibility of including aggregate demand controls in the UK local area level analysis.

Another important component of our analysis has been to raise the issue of the potential endogeneity of the MW variable in the standard approach to estimating an employment equation. Further research should be encouraged in finding appropriate IVs for the MW variable.

Moreover, looking at the effects of the NMW on lower tail wage inequality, we pointed out a simultaneity bias problem which arises when applying Lee’s (1999) methodology. This is due to the presence of the median wage in both sides of the regression equation. Again further research should be encouraged in finding appropriate solutions to this issue.

Finally, further work should focus on firm level datasets, which will allow investigation of other potential channels where the MW might have had an impact, such as productivity, profits, investments and probability of exit.

This thesis aimed to empirically investigate the labour market impact of the MW, possibly one of the most contentious issues in labour economics. We hope to have significantly contributed to the MW debate and also to have identified future areas of research in order to deepen the understanding of this interesting and controversial topic.

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