Essays in the microeconometric evaluation of public policies

A thesis submitted for the degree of Doctor of Philosophy in Economics

by

Federico Zilio

Institute for Social and Economic Research

University of Essex

October 2017

Declarations

No part of this thesis has been submitted for another degree.

Chapter 1 is co-authored with Professor Tom Crossley. Chapter 3 is co-authored with Professor Mike Brewer and Professor Tom Crossley. Chapter 2 is exclusively mine.

Chapter 1 has been previously published in the ISER Working Paper Series (No. 2017-10, August, 2017).

Acknowledgements

I am grateful to the Economic and Social Research Council for the financial support that made writing this thesis possible.

A special thanks to my supervisors, Prof. Mike Brewer and Prof. Tom Crossley, for their daily availability, precious advice, continuous support and strong motivation. I have learned from them a lot and hope to learn more in the future. Thanks to them I have grown as a researcher and as a person. I feel very lucky to have worked with them.

Thanks to ISER for providing me the facilities and all the people working here for giving me help when I needed.

Thanks to all my office-mates, Catalina, Ipek, Yamil, Claudia, Caroline, Mario, Josh, Franco, Victor, for bearing me all the time and making coffee breaks such special moments. I think we are more than an office, we are a group of friends.

Thanks to all the friends I made here in Colchester for all the pleasant discussions and moments. A special thanks to Greta & Adam, Emil, Duygu, Chrysa & Kostas, Elisa & Federico, Alex & Gulcimen, and all the friends at ISER, Department of Economics and Department of Sociology.

Thanks to my family and all my Italian friends. I know that when I go back to Italy I can always count on you and on a warm environment.

Summary

Chapter 1 examines the health benefits of the Winter Fuel Payment (WFP), an unconditional but labelled cash transfer given to elderly people above the female state pension age with the stated intent of help to deal with heating costs. We exploit the eligibility age cut-off to estimate the causal effect of the WFP on self-reports of chest infection, measured hypertension and biomarkers of infection and inflammation, such as C-reactive protein and fibrinogen. We find a robust reduction in the incidence of high levels of serum fibrinogen and some evidence of reductions in other disease markers that point to health benefits.

In Chapter 2, we estimate the incidence of the housing subsidy on subsidised and unsubsidised tenants. Using a reform of the housing subsidies in the UK, we investigate how the exogenous cut in the subsidy affected rents. We find that rents were not significantly reduced by the subsidy cut and the incidence mostly fell on tenants. These findings suggest that the rental market was not originally segmented between subsidised and unsubsidised tenants and the fall in the demand of subsidised tenants was offset by the recent expansion of the private rental market.

In Chapter 3, we revisit and offer a reassessment of the literature on the impact of UK National Minimum Wage on employment. We highlight that this literature has employed difference-in-difference designs, which have significant challenges in conducting appropriate inference and very low power when inference is conducted appropriately. In addition, the literature has focused on the binary outcome of statistical rejection of the null hypothesis, without attention to the range of employment effects. In our reanalysis of the data, we find that the data are consistent with both large negative and small positive impacts of the UK National Minimum Wage on employment offering little guidance to policy makers.

Contents

Introduction **1**

1 The Health Benefits of a Targeted Cash Transfer: The UK Winter Fuel Payment **5**

1.1 Introduction 5

1.2 Data and Methods 9

1.2.1 Data 9

1.2.2 Health Outcomes 11

1.2.3 Regression Discontinuity Design 13

1.3 Results 15

1.4 Discussion and conclusions 27

Appendices **29**

A1 Winter Fuel Payment eligibility 29

2 Does a housing subsidy cut really lower rents? Evidence from a reform in the UK **30**

2.1 Introduction 30

2.2 The incidence of the housing subsidy 34

2.2.1 Rental market models 35

2.2.2 The incidence of a housing subsidy 37

2.3 The Local Housing Allowance reform 39

2.4 Identification strategy 42

2.4.1 The incidence on landlords that let to subsidised tenants . . . 42

2.4.2 The incidence on unsubsidised tenants 46

2.5 Data 48

2.6 Results 49

2.6.1 The incidence on subsidised tenants 49

2.6.2 The incidence on unsubsidised tenants 52

2.7 Discussion 56

2.8 Conclusions 61

Appendices **63**

B1 LHA regime status 63

B2 Equivalence between the IV estimator θ_2 and the two regressions approach 65

B3 Descriptive Statistics 66

3 What do we really know about the employment effects of the National Minimum Wage? **70**

3.1 Introduction 70

3.2 Background 74

3.2.1 The minimum wage in the UK[1] 74

3.2.2 Research on the impact of the National Minimum Wage . . . 77

3.3 Data and Models 83

3.3.1 Two approaches for assessing the impact of the NMW on employment 84

3.3.2 Minimum detectable effects 91

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

3.3.3 From the impact of the minimum wage on job retention to employment demand elasticities . . . 92
3.4 Results . . . 94
3.4.1 The estimated impact on a NMW uprating on job retention . 95
3.4.2 The estimated impact on a 1% rise in the NMW on job retention 97
3.4.3 Implied elasticities of job retention and elasticity of employment 98
3.5 Discussion and Conclusions . . . 105

Appendices **107**

C1 Inference in Difference-in-Differences with Grouped Errors . . . 107
C2 Derivation of formulas for the elasticity of employment with respect to the minimum wage . . . 113
C2.1 Static Model . . . 113
C2.2 Dynamic Model . . . 115
C3 Supplementary results . . . 119

Conclusions **121**

List of Tables

1.1 Descriptive statistics. C-reactive protein, Fibrinogen, Hypertension and Self-Reported Chest Infection. 12

1.2 The impact of the Winter Fuel Payment on predictors of infection. . . 19

1.3 Robustness checks. The impact of the Winter Fuel Payment on predictors of infection. 21

1.4 Falsification Tests: Effect of a "placebo" eligibility at age 55 and age 65, and effect on above median fibrinogen concentration. 22

1.5 The impact of the Winter Fuel Payment on the Poor Health Index. . 25

1.6 Impact of the Winter Fuel Payment on the Poor Health Index in subgroups. 26

A1 Winter Fuel Payment eligibility. 29

2.1 Implementation of the LHA reform by month of claim 41

2.2 Effect of the reform on Log-Subsidy Receipt and Log-Rent and Incidence of the Housing Subsidy 51

2.3 Incidence of the Housing Subsidy in subgroups 52

2.4 Incidence of the Housing Subsidy on unsubsidised tenants 54

2.5 Differences in rents between subsidised and unsubsidised tenants . . 55

B1 LHA regime status d_{it} by month of the interview 63

B2 Descriptive statistics for LHA claimants in the pre- and post- reform period, showing means and t-tests on the equality of means 66
B3 Descriptive statistics for unsubsidised tenants in the pre- and post-reform period, showing means and t-tests on the equality of means . . 68

3.1 The UK National minimum wage for adults 75
3.2 Estimates of Average Impact of a NMW Uprating on Job Retention . 95
3.3 Estimates of Impact of a 1% rise in the NMW on Job Retention . . . 97
3.4 Implied elasticities of job retention with respect to the minimum wage, η_{JR} 99
3.5 Estimated elasticities of job retention with respect to the minimum wage, η_{JR}, from studies using US and Canadian data 99
3.6 Employment elasticity with respect to the the year t minimum wage uprating. Estimates of $\bar{\eta}_{ER(t)}$ derived from the static model of equation 3.5. 101
3.7 Employment elasticity with respect to the the year t minimum wage uprating. Estimates of $\bar{\eta}_{ER(t)}$ derived from the dynamic model in equation C2.2. 103
3.8 Estimated employment elasticities with respect to the minimum wage, η_{ER}, from studies using US data 104
C1 Employment elasticity with respect to the the year 2001 minimum wage uprating. Estimates of $\bar{\eta}_{ER(2001)}$ derived from the static model of equation 3.5. 114
C2 Employment elasticity with respect to the the year 2001 minimum wage uprating. Estimates of $\bar{\eta}_{ER(2002)}$ derived from the dynamic model of equation 3.5. 118
C3 Replication of impact of NMW on job retention presented in Bryan, Salvatori, and Taylor (2013) 119

C4 Estimates of first-order autoregression coefficient of the residuals from equation (3.2). 120

List of Figures

1.1 Effect of the Winter Fuel Payment on the probability of having a C-reactive protein level larger than 10 mg/l. 16

1.2 Effect of the Winter Fuel Payment on the probability of having a fibrinogen level larger than 4 g/l. 16

1.3 Effect of the Winter Fuel Payment on the probability of having had a recent chest infection. 17

1.4 Effect of the Winter Fuel Payment on the probability of having hypertension 17

1.5 Effect of the Winter Fuel Payment on the Poor Health Index. 24

2.1 Housing Subsidy (HS) expenditure in the UK. 2000-2014 40

2.2 Impact of the LHA reform on rents of unsubsidised tenants 47

2.3 Median weekly Log(Subsidy) and Median weekly Log(Rent) in 2008 prices for LHA claimants in the private rental sector, Q2 2008- Q1 2014 50

2.4 Median weekly Log(Rent) for subsidised and unsubsidised tenants in the private rental market, Q2 2008- Q1 2014 53

2.5 Dwelling stock by tenure in Great Britain, 1992-2014 58

2.6 Housing Subsidy (HS) Caseloads, 1992-2014 59

2.7 Vacant Dwellings, 2004-2016 60

3.1 Annual growth rates of the UK National Minimum Wage, 2000 to 2015. 76

Introduction

In the financial year 2016/17 the United Kingdom (UK) spent £216.1 billion on welfare. This is around 11.5 % of the Gross Domestic Product and 28.7 % of the Total Managed Expenditure (Department for Work and Pension, Benefit expenditure and caseload tables 2017). In times of austerity it is very important that policy makers know whether welfare spending achieves the intended goals. It is also important to know whether welfare policies have unintended consequences. The scope of policy evaluation is to answer these questions and this is why it has become increasingly influential among governments. Policy evaluation literature has developed several methods to estimate the causal effects of public policies. In this thesis we work with three of the most common methods of this literature: regression discontinuity design, instrumental variables and difference-in-differences design. We use the methods to evaluate three policies that aim to improve health, housing and employment, three areas that have a big impact on people's well-being. The policy studied in Chapter 1 is the programme known as Winter Fuel Payment (WFP), a cash transfer to elderly people in the UK that costs taxpayers around £2 billion a year. In Chapter 2, we analyse housing subsidies in the UK, on which the UK government spends around £24 billion a year. The third policy is the UK's national minimum wage, one of the policies to increase the wage of low-paid workers.

The health of elderly people is a primary concern of all governments. The UK spent around £133 billion in the financial year 2015/16 on healthcare services (HM Treasury, Country and regional analysis: 2016) and more than two fifths of this expenditure was allocated to people aged 65 or older (Nuffield Trust estimates in Robineau, *The Guardian*, February 1st 2016). Every year, the UK experiences Excess Winter Mortality (EWM) and faces a rise in the demand of care in winter among the elderly. Notably, the UK experiences a higher level of EWM than Scandinavian countries where winters are notoriously harsher. While outdoor temperatures play a decisive role to explain EWM, living in a cold house has a direct impact on the health of elderly people. In the UK, a key policy response to EWM and diseases associated with cold temperatures is the Winter Fuel Payment (WFP), a labelled but unconditional cash transfer to households containing a member aged above the female state pension age. From 2000 to 2009, the WFP was received by households containing an older person above age 60. Previous work has shown that the WFP raises fuel spending among eligible households. In Chapter 1, we study the causal effects of the WFP on health outcomes associated with the main causes of EWM: cardiovascular and respiratory diseases. The outcomes we examine are self-reports of chest infection, measured hypertension, and two biomarkers of infection and inflammation (fibrinogen and C-reactive protein). To estimate the causal effect of the WFP, we use two cross-sectional studies, the Health Survey for England and the Scottish Health Survey, and a longitudinal study, the English Longitudinal Study of Aging. We then employ a regression discontinuity design by comparing our health outcomes just below and above the 60 year old eligibility cut-off. Our regression discontinuity design estimates the causal effect of the WFP on the health of individuals living in households where the oldest member of the household is just over 60 years old. We find a robust and statistically significant reduction in the incidence of high concentration of fibrinogen, one of the biomarkers of infection. Reductions in other health outcomes also point to health benefits from the WFP programme, but the effects are less precisely estimated.

The 1948 Universal Declaration of Human Rights states that everyone should have the right to a suitable living standard, and housing is recognised as one of the basic needs that ensures citizens an adequate well-being. Housing subsidies are one of the policies that aim to make housing affordable. However, the standard theory predicts that when a housing subsidy is in place, the housing demand shifts upwards and, unless the supply is perfectly elastic, rents rise for all tenants regardless of whether they receive a housing subsidy. This change in rents implies that the incidence of the housing subsidies is shared between tenants and landlords. In Chapter 2 we study the incidence of housing subsidies by examining how they affect subsidised and unsubsidised tenants. We exploit a reform implemented in 2011 in the UK that reduced housing subsidies for around 1.5 million claimants. Our analysis uses the exogenous cut in the housing subsidies as an instrument to address the endogeneity of subsidy receipt. The main contribution of this chapter is the link between the effects of the subsidy cut on subsidy recipients and the housing demand of unsubsidised tenants. Using data from the Family Resource Survey and the English Housing Survey, we find that the subsidy cut did not significantly reduce rents for subsidised tenants and there was no spillover effect on rents paid by unsubsidised tenants.

Beside adequate housing, one of the basic principles recognised in the 1948 Universal Declaration of Human Rights is the right to work with a fair remuneration that ensures a suitable living standard. In the UK this is a highly discussed topic based on the perception that a share of the working population does not earn enough to afford a decent living standard. One of the key policies in response to this concern is the National Minimum Wage (NMW) that was first introduced in 1999. Interestingly, successive UK governments' approach to setting the NMW has been evidence-based. A statutory and independent body, the Low Pay Commission (LPC), commissions and funds research on the impacts of the NMW, and then uses this evidence when making its recommendations to government. A substantial body of research on NMW has concluded that the NMW did not have a detrimental effect on employment, and, in the light of this, successive governments have subsequently uprated the NMW and recently introduced the National Living Wage. We revisit and reassess the literature on the employment effect of the NMW on the basis of two concerns. First, much of the literature uses a difference-in-difference strategy, although there are challenges in conducting appropriate inference in such designs, and they can have very low power when inference is conducted appropriately. Second, the literature has focused on the statistical rejection of the null hypothesis of "no employment effect of the NMW", with no attention to the range of employment effects that are consistent with the data. In our reanalysis of the data, we conduct difference-in-difference inference using recent suggestions for best practice, and focus on confidence intervals rather than the binary outcome of whether the null hypothesis of no employment effects can be rejected. We also report job retention and employment elasticities and calculate the Minimum Detectable Effects, the minimum employment effect size we are able to detect with high probability. We find that the data are consistent with both large negative and small positive effects of the UK National Minimum Wage on employment, and so conclude that existing evidence in fact offers little guidance to policy makers.

Chapter 1

The Health Benefits of a Targeted Cash Transfer: The UK Winter Fuel Payment

1.1 Introduction

Each year, the U.K. experiences Excess Winter Mortality (EWM). The Office of National Statistics computes EWM by comparing the number of deaths registered between December and March with the average number of deceases in the foregoing August-November and in the succeeding April-July. In 2014/15 the number of excess winter deaths in England and Wales was estimated at 43,900, the highest level since 1999. EWM has also been documented in Europe (Kunst et al., 1993; Eng and Mercer, 1998; Rose, 1966; Mackenbach et al., 1992; Keatinge and Donaldson, 1995), the USA (Kloner et al., 1999; Lanska and Hoffmann, 1999) and Asia (Cheng, 1993; Ornato et al., 1990). Most EWM occurs among the elderly and is due to respiratory and circulatory diseases (Donaldson, 2010; Lloyd, 2013; ONS Statistical Bulletin 2014/15). [1] In addition to EWM, cold weather is associated with increased demands on health care systems through increased incidence of illness requiring hospitalization or other treatment.

While outdoor temperatures may play a role in EWM, [2] living in a cold indoor environment has a direct impact on the health of elderly people (Wilkinson et al. 2001; Marmot et al. 2011; Dear & McMichael 2011; Rudge & Gilchrist 2007). [3] In the U.K., a key policy response to EWM is the Winter Fuel Payment (WFP). The WFP is a labelled but unconditional cash transfer to households containing an older person (male or female) above the female state pension age. The stated intent of the policy is help the elderly deal with the cost of keeping their dwelling warm (Lloyd, 2013; Kennedy & Parkin, 2016) and the labelling of this cash transfer has been shown to be effective in inducing eligible households to increase fuel spending (Beatty, Blow, Crossley, & O'Dea, 2014). [4] However, the key policy question is whether the WFP improves health of elderly people living in eligible households, and there is little evidence on this point.

The female state pension age, and hence the age cut-off for WFP eligibility, was 60 prior to 2010. It then began to increase so that it will equal the male state pension age of 65 in 2018 and then both will rise to 67 by 2028. Most EWM occurs among

[1] In the winter of 2014/2015 36 % of EWM was attributable to respiratory disease and 22.5 % to cardiovascular disease (ONS, 2015).

[2] The strong association between exposure to outdoor cold temperatures and mortality or morbidity is well documented in the epidemiological literature (Curwen, 1997; Wilkinson et al., 2001; Keatinge, 1986, 1989, 2002).

[3] Wilkinson, Landon, Armstrong, Stevenson, and McKee (2001) show that deaths attributable to cardiovascular diseases are 23 % higher in winter than the rest of the year and give evidence of a positive association of the EWM with the age of the property and the thermal inefficiency of the buildings. Rudge and Gilchrist (2007) find that their fuel poverty index which includes an energy efficiency rates and income is a strong predictor of the excess winter morbidity measured with number of emergency respiratory hospital admissions.

[4] Beatty et al. (2014) estimate that eligible households spend 47 % of their WFP on fuel. If the payment were treated as other income, eligible household would be expected to spend 3 % of the payment on fuel.

individuals over 75, leading Lloyd (2013) to propose that increases in the eligibility age could reduce the financial cost of the WFP with minimal if any reduction in health benefits. But there has been no direct evidence on the health benefits foregone through recent increases in the age cut-off or health benefits that may be lost through further increases.

This paper reports the first tests for health benefits of the WFP based on individual-level data. We measure health outcomes in the Health Surveys for England (HSE), the Scottish Health Survey (SHeS) and the English Longitudinal Study on Ageing (ELSA). These studies include nurse visits allowing us study biomarkers and physical measures as well self-reports. To estimate the causal effect of the WFP, we follow Beatty et al. (2014) in employing a regression discontinuity design (RDD). The RDD is thought to be one of the most convincing of quasi-experimental designs (Lee & Lemieux, 2010). [5] A RDD is possible where there is cut-off in eligibility for treatment, as there is for the WFP: in the period we study, households with no member 60 or above are ineligible. In addition, take up of the WFP is very high, so that there is little difference between eligibility and receipt. [6] A RDD estimates the causal effect of treatment by comparing outcomes just below and above the eligibility cut-off. It estimates a *local* average treatment effect - at the eligibility cut-off. Thus our design estimates the causal effect of the WFP on the health of individuals living in households where the oldest member of the household is 61. These are precisely the individuals who lost any health benefits of the WFP as the eligibility age was incrementally increased from 2010, so our empirical strategy directly answers a key policy question.

To the best of our knowledge, Iparraguirre (2014) is the only prior assessment of

[5] The Regression Discontinuity Design was first introduced in the Education literature (see Thistlethwaite and Campbell (1960)) and has been widely adopted in economics (Lee & Lemieux, 2010)

[6] See Beatty et al. (2014) for further discussion.

the health benefits of the WFP. Using aggregate mortality data, Iparraguirre (2014) documents a decline in EWM in 2000/2001, coincident with the introduction of the universal WFP. EWM fluctuates significantly from year-to-year with changing winter weather conditions and viral environment. Using time-series econometric techniques, Iparraguirre (2014) finds a structural break in the EWM time series for England and Wales in 2000/2001 and estimates that half of the reduction in the EWM in that year can be attributed to the introduction of the WFP. [7] This is an important finding, but it does rest on the ability of the econometric methods to distinguish the policy effects from the very substantial year-on-year fluctuations in EWM. Moreover, the aggregate EWM time-series is necessarily silent on health effects that may precede mortality, and on benefits to particular groups. We add to the evidence base significantly by using a convincing quasi-experimental design in conjunction with individual level data; by considering a variety of measures of circulatory and respiratory illness, including biomarkers [8]; and by testing for health benefits particularly among the group that have been made ineligible by recent changes to the age cut off.

We estimate the effect of the WFP on circulatory and respiratory illness measured four ways:

(i) self-reports of chest infection in the last 3 weeks

(ii) hypertension measured during a visit

(iii) serum values of C-reactive protein (CRP) in excess of 10 mg/l

(iv) serum values of Fibrinogen in excess of 4 g/l.

Repeated exposure to a cold environment results in an increase in the blood pres-

[7] The WFP was introduced in 1997 but it was initially means-tested and the payment significantly smaller. In 2000/2001 it took its current form (a universal payment to all households containing a person above the female state pension age of between 200 and 300 pounds).

[8] Biomarkers have been increasingly drawing attention in the economic literature as an objective measure of health and a complement of self-reported health measures (see Jürges et al., 2013; Evans and Garthwaite, 2014; Michaud et al., 2016)

sure, and high values of systolic and/or diastolic blood pressure (hypertension) is a predictor of heart disease and stroke (Hoffman et al. 1983; Fraser 1986; Wilson et al. 1998; Collins et al., 1985; Collins et al., 1990). CRP is a blood plasma protein that is indicative of inflammation and infection and a risk predictor of cardiovascular disease (Pepys, 2003; Pearson et al., 2003). Fibrinogen is glyco-protein and marker of inflammation. It is strongly associated with exacerbations of chronic obstructive pulmonary disease (Duvoix et al., 2012; Mannino et al., 2015). High values of CRP or fibrinogen are considered evidence of current infection (Pearson et al., 2003).

Our principal finding is that, among those living in a household that just qualifies for the payment, the WFP leads to a six percentage point reduction in the incidence of high levels of serum fibrinogen (on a base of 12 %). This effect is statistical significant ($p < 0.01$) and very robust. For the other health measures we consider, while point estimates suggest health benefits, the estimated effects are less robust to changes in sample or specification, and rarely statistically significant.

In the next section, we provide further detail on our data, outcomes measures, identification strategy and methods. Section 3 presents our results. Section 4 contains additional discussion of the findings.

1.2 Data and Methods

1.2.1 Data

The analysis reported in this paper is based on data from the Health Surveys for England (HSE) (2001, 2003, 2004, 2005, 2006, and 2009) [9] , the Scottish Health Survey (SHeS) (2003, 2008, 2009) and the English Longitudinal Study of Ageing

[9] HSE 2001 contains hypertension measure and self-reports of chest infection only (not the biomarkers).

(ELSA) (wave 2, 2004-05, and wave 4, 2008-09). The HSE and the SHeS are annual cross-sectional surveys of the health conditions of the population in England and Scotland. ELSA is a longitudinal survey that captures the population in England aged 50 and over. The ELSA sample is derived from the 1998, 1999 and 2001 HSE. In the surveys a face-to-face interview is followed by a nurse visit. The sample design of HSE, SHeS and ELSA is at household level. A face-to-face interview is followed by a nurse visit for all members of the household. [10] In the interview the respondents answer questions on their general health, smoking status and alcohol consumption and other individual characteristics such as education and employment status. After the interview an appointment for the nurse visit is arranged. In the visit a trained nurse asks questions on the health condition of the respondent, takes blood and saliva samples and reads the blood pressure and several other measures (height, weight, waist, hip, lung function and grip strength). Blood samples are sent to an external laboratory for analysis. ELSA, HSE and SHeS data contain several biomarkers that are recovered from the analysis of the blood samples. Among the biomarkers reported, there are two that are useful for our analysis because they are correlated with inflammation processes and infection and are markers of circulatory and respiratory illness. These are C-reactive and fibrinogen.

Following Beatty et al. (2014) we restrict the sample to single men and couples in which the man is the oldest in the household. Over the period 2003-2009 the Female State Pension age was 60[11], coincident with the age of eligibility for the WFP. We discard single women and couples in which the woman is the oldest member of the household to avoid any confounding effect of receipt of the state pension on the fuel expenditure and the health outcomes of the elderly.[12]

[10]We study the WFP effect on health of individuals. In this paper, we do not exploit the fact that we have data for individuals of the same household and do not estimate the spillover effects of the WFP.

[11]In the same period the male state pension age was 65.

[12]Our findings are not confounded by any other policy directed at elderly people. Free flu

1.2.2 Health Outcomes

We study four measures of circulatory and respiratory illness:

(i) self-reports of chest infection in the last 3 weeks

(ii) hypertension measured during a visit

(iii) serum values of C-reactive protein (CRP) in excess of 10 mg/l

(iv) serum values of Fibrinogen in excess of 4 g/l.

During the nurse visit, respondents are asked whether they have experienced any respiratory infection in the preceding 3 weeks (influenza, pneumonia, bronchitis or a severe cold). This self-reported outcome is available only in ELSA and SHeS.

Our second outcome is hypertension, a risk factor for strokes and heart attacks. The World Health Organization (WHO) defines hypertension as systolic blood pressure of 140 mm Hg or above and/or diastolic blood pressure of 90 mm Hg or above (WHO, 2013). In our sample, around the cut-off age for WFP eligibility, about 35 % of respondents are hypertensive.

The other outcomes in our study are CRP and fibrinogen, two acute-phase biomarkers. Serum concentrations of these two biomarkers increase sharply during an inflammatory process. CRP is considered an indicator of bacterial infection, pneumonia and tissue damage (Tillet and Francis, 1930; Pepys, 2003; Pearson et al., 2003; Simon et al., 2004). The median of CRP in our sample is 1.7 mg/l but its distribution is highly skewed. The value rises within few hours of disease onset. Inflammation and bacterial infection can produce a rise in CRP values up to 1,000-fold (Pepys, 2003; Gruys et al., 2005). Fibrinogen is a coagulation protein produced by the liver that helps the body in the formation of blood clots. The normal range of fibrinogen is 2-4

vaccination is offered to elderly aged 65 or over (Department of Health, 2000).

g/l, but the concentration increases up to 3-fold in the presence of an inflammatory process, infection or tissue damage (Fenger-Eriksen et al., 2008; Gruys et al., 2005; Schmaier, 2012). High concentrations of fibrinogen are also strongly associated with chronic obstructive pulmonary diseases and moderately with coronary heart diseases (Danesh et al., 2005; Duvoix et al., 2012; Mannino et al., 2015). In the epidemiological literature a value of the C-reactive protein in excess of 10 mg/l is taken as evidence that a person has an active infection or inflammatory process. Epidemiologists have often discarded observations with these high values because of their focus on chronic processes (Pearson et al., 2003). However, as our interest is whether the

Table 1.1: Descriptive statistics. C-reactive protein, Fibrinogen, Hypertension and Self-Reported Chest Infection.

Age Window	**Median**	**90th percentile**	**Prob(Illness)**
C-reactive protein			
			$Prob(CRP > 10mg/l)$
58-63	1.7	7	0.055
Fibrinogen			
			$Prob(Fib \geq 4g/l)$
58-63	3.1	4	0.125
Hypertension			
			$Prob(Hypertension)$
58-63	-	-	0.353
Self-reported Chest Infection			
			$Prob(ChestInfection)$
58-63	-	-	0.101

Sample is pooled data from the English Longitudinal Study of Aging, Health Survey for England and Scottish Health Survey.
Observations with a fractional probability of being eligible to the Winter Fuel Payment are dropped.

WFP plays a role in reducing the incidence of a respiratory or circulatory disease among the elderly, extreme values (in excess of 10 mg/l for CRP) are the appropriate object of our analysis. The epidemiological literature has not defined an equivalent disease threshold for fibrinogen but we take values in excess of the top the standard range (2-4 g/l) as evidence of current infection or inflammation.

1.2.3 Regression Discontinuity Design

The RDD allows health outcomes to vary with the "forcing variable", which is the age of the oldest person in the household of subject i at the time of the interview t. [13] Denote this by (A_{it}). The econometric model includes smooth functions of the value of the forcing variable relative to the cut-off age ($A_{it} - 60$). It also includes an indicator (or dummy) variable, D_{it}, for whether a respondent's household was eligible for the last WFP payment before the interview at which health outcomes were measured. Finally, it includes additional covariates X_{it}, to increase the precision of the estimator by capturing individual background variation in health (unrelated to the WFP). [14] As covariates we use individual characteristics (type of household, gender, smoker status, alcohol consumption , education, income, employment status), anthropometric measurements (Body Mass Index, waist), survey-wave dummies and month of nurse visit. In particular, including month of nurse visit is important because the incidence of illnesses is higher in winter months than in summer months.

Thus the econometric model is:

$$H_{it} = \beta_0 + f(A_{it} - 60) + \tau D_{it} + f(A_{it} - 60) \cdot D_{it} + X_{it}\gamma + \epsilon_{it} \tag{1.1}$$

[13]In health economics, Carpenter and Dobkin (2009) use age as forcing variable in a RDD setting to estimate the effect of the minimum drinking age on mortality.

[14]The assumption behind the RDD is that all health determinants apart from WFP eligibility should evolve smoothly with A_{it}, including (but not limited to) the covariates X_{it}. For the covariates Z_i, we can test this by estimating an RDD in Z_i. There should be no discontinuity in the Z_i at the 60 or, equivalently, observables should be balanced between eligible and not eligible in the region of the cut-off (analogous to covariate balance in a randomized trial). We have tested this and cannot reject for balance for any of the included covariates Z_i (see the notes in Table 1.2 for a complete list). The major concern here is that we can find a discontinuity in employment status at the age of 60 since the WFP eligibility cut-off is coincident with the female state pension age. For our sample of single men and couples in which the men is the oldest the WFP effect on employment status is 0.001 (-0.098; 0.100). This implies that the point estimate of the treatment effect is not affected by employment status. Our robustness checks confirm that covariates do not bias our estimates. The inclusion of employment status and other covariates can, however, improve precision by reducing the unexplained variation in the outcome variable (again, as in a randomized trial).

We employ both linear and quadratic functions for *f( )*. The model is estimated by ordinary least squares. Note that all of the health measures (H_{it}) we consider are binary so that $E[H_{it}] = Prob(H_{it} = 1)$ and this is a linear probability model. The parameter of interest is τ, which measures the local causal effect of the WFP on $Prob(H_{it} = 1)$, around the cutoff. Formally:

$$\tau = \lim_{A \downarrow 60} \mathbb{E}[H_{it}|A_{it} = 60, X_{it} = x] - \lim_{A \uparrow 60} \mathbb{E}[H_{it}|A_{it} = 60, X_{it} = x] \tag{1.2}$$

As the H_{it} are measures of illness, if the WFP improves health τ should be negative. We report standard errors that are robust to heteroscedasticity and clustering by the age in years of the oldest member of respondent's household. [15]

The WFP was between £200 and £300 (about 300-450 USD) during the period 2002-2009 when our data were collected and it was paid in November-December. [16] Eligibility is determined by the age of the old household member in the preceding September. Thus a respondent's household will have received a WFP in the December prior to the nurse visit date only if the oldest member of the household was 60 in the September immediately before the December before that date. All households with an oldest member aged 59 or less at the date of the nurse visit will not have received a WFP. All households with an oldest member aged 62 or more at the date of the nurse visit will have been eligible for at least one WFP, and, given the very high take-up of this benefit, almost surely received it. For households with an oldest member aged 60 or 61, whether they have been eligible for a WFP will depend on both the date of the nurse visit and the birthday of the oldest member of the household. A complication is

[15] We follow the discussion in Cameron and Miller (2015) and cluster standard errors at age of the oldest member level rather than household level because we would otherwise assume independence across households with same age of the oldest member. However, in the analysis we do not apply standard error corrections to account for small number of clusters.

[16] From 2000 to 2007 the WFP was £200 for most eligible households but £300 for households with an over-80s member. In 2008 the WFP was temporarily uplifted to £250 for over 60s and to £400 for over 80s but this increase was reversed in the Budget of March 2011.

that, although the month of the nurse visit is known, ages are recorded in the data in years. That means, the WFP status of some households with an oldest member aged 60 or 61 can only be determined probabilistically. This is described in Table A1 in the Appendix A1. [17] We deal with this in two ways. First, we define D_{it} according to Table A1, so that $D_{it} = 0$ if $A_{it} < 59$, $D_{it} = 1$ if $A_{it} > 61$ and $D_{it} \in [0, 1/12, 2/12, ...1]$ if $A_{it} \in [60, 61]$ following the mapping in Table A1. Second, as a robustness check, we re-estimate the model dropping all observations for which WFP cannot be discretely determined. Note that when these cases are dropped, $D_{it} = \mathbb{1}[A_{it} > 60]$ (exactly), where $\mathbb{1}[.]$ is an indicator function.

1.3 Results

We begin with the now standard graphical presentation of the RDD in Figures 1 through 4. Each figure corresponds to one of our four measures of circulatory and respiratory illness. The vertical axis measures the incidence of illness, after regression-adjustment for covariates at the individual level. These covariates are listed in the notes to the figure but of course exclude age and WFP eligibility. The horizontal axis measures the age of the oldest member of a respondent's household. Each plotted point is the average value of the illness measure for a given year of age (of oldest household member). As our illness measures are binary, this mean is a probability.

The cut-off for WFP eligibility (at age 61) is indicated in Figures 1 through 4 by the vertical line and separate least-squares best-fit lines are plotted to the left and the right of the cut-off. A treatment effect is indicated by a discontinuity between these

[17] For example, if an individual has the nurse visit in January, their household will have received a WFP in December (one month before), as long as the oldest member is more than 60 years and 4 months old, so that they were 60 in the preceding September. If the oldest member of the household reports age 60 and was born in August, the household will have been eligible for a WFP in December. However, if the oldest member of the household reports age 60 and was born in October, the household will not have been eligible for a WFP in December. Of those oldest members of a household aged 60 in years at a given nurse visit, 2/3 will be older than 60 years and 4months, and 1/3 will be 60 years four months or less.

Figure 1.1: Effect of the Winter Fuel Payment on the probability of having a C-reactive protein level larger than 10 mg/l.

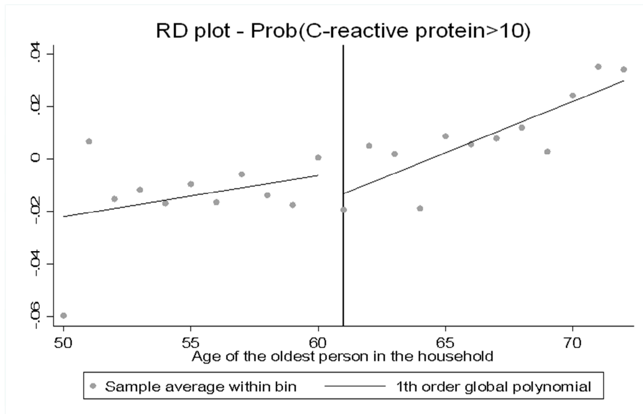


Figure 1.1 plots the residuals of a regression of the $Prob(CRP > 10mg/l)$ on type of household, gender, smoker status, alcohol consumption Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

Figure 1.2: Effect of the Winter Fuel Payment on the probability of having a fibrinogen level larger than 4 g/l.

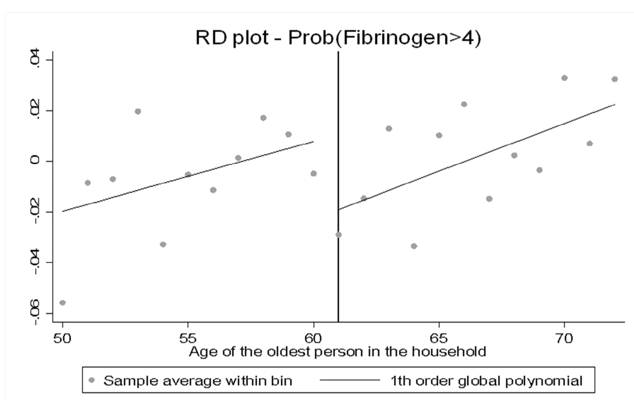


Figure 1.2 plots the residuals from a regression of the $Prob(Fibrinogen \geq 4)$ on type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

Figure 1.3: Effect of the Winter Fuel Payment on the probability of having had a recent chest infection.

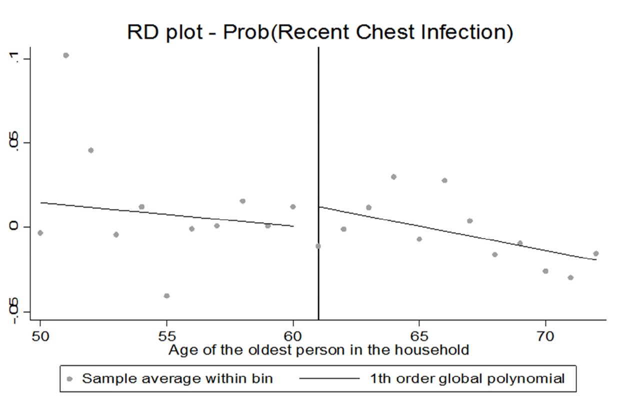


Figure 1.3 plots the residuals from a regression of the Prob (Chest Infection in the last 3 weeks) on type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

Figure 1.4: Effect of the Winter Fuel Payment on the probability of having hypertension

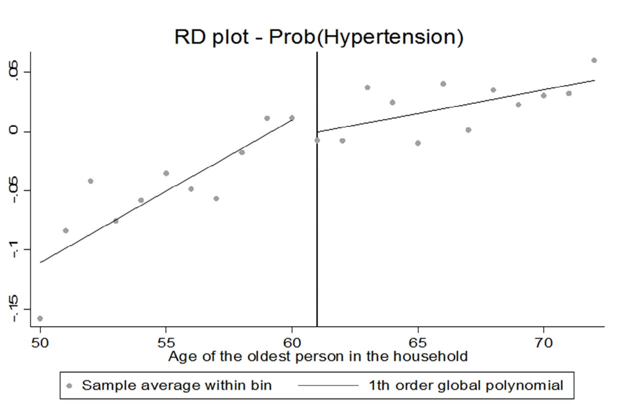


Figure 1.4 plots the residuals from a regression of the $Prob(Hypertension)$ on type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

two best-fit lines at the cut-off. Three of the four figures indicate decline in illness incidence with WFP eligibility. The exception is self-reported chest infection. The plots also indicate that the paths of illness incidence across age of the old household member are quite noisy, with significant year on year fluctuations. Although we have substantial sample sizes (each point represents about 400 observations), we are modelling relatively rare events (in our sample 5.5 % of the elderly just below the cut-off present a CRP value larger than 10 mg/l and 12.5 % a fibrinogen value in excess of 4 g/l, see 1.1). To assess the magnitudes and statistical significance of the discontinuities visible in these figures we turn to formal RDD estimates (as described above). These are reported in Table 1.2.

In Table 1.2 each column gives the estimate of the WFP effect on the health outcomes in our preferred specification with linear functions of the forcing variable, *f()*, covariates, and a sample age window (for the age of the oldest member in the household window) of 55 to 65 years. The point estimates suggest that the WFP improved the health of the elderly at age 61, reducing all our measures of illness. Effect sizes are 1 to 6 percentage points.

However, the WFP only has a statistically significant effect at 5 % level on having a high concentration of fibrinogen. [18] It decreases the probability of a fibrinogen value in excess of 4 g/l by 5.5 percentage points. As 12.5 % of the elderly just below the cut-off have a value of fibrinogen in excess of 4 g/l, our estimate implies a 44 % reduction in the incidence of this measure of illness at the age cut-off. Data on the CRP are noisier and the discontinuity effect at the cut-off is not statistically significant at conventional levels. Nonetheless, the magnitude of the WFP effect (1.2 percentage points) is sizeable and implies a 22 % reduction in the incidence of illness by this measure, again at the cut-off. [19] In Column 3 we report the effect on self-reporting

[18] This finding is robust to the adjustment for multiple testing using the Romano-Wolf algorithm (see the third line in Table 1.2).

[19] The fraction of elderly just below the age 61 cut-off with CRP value larger than 10 mg/l is

Table 1.2: The impact of the Winter Fuel Payment on predictors of infection.

Effect of the WFP on Fibrinogen, C-reactive protein, Self-reported Chest Infection and Hypertension

	Fibrinogen	**C-reactive protein**
Causal Effect of Eligibility	-0.055***	-0.012
(95% Confidence Interval)	(-0.077; -0.033)	(-0.044; 0.020)
Minimum Detectable Effect (at 80% power)	±0.028	±0.040
Unadjusted P-Value (Adjusted for Multiple Testing)	0.000 (0.022)	0.418 (0.221)
Number of observations	3,974	4,517
Age Window	55-65	55-65
	Self-reported Chest infection	**Hypertension**
Causal Effect of Eligibility	-0.024*	-0.018
(95% Confidence Interval)	(-0.049; 0.001)	(-0.049; 0.012)
Minimum Detectable Effect (at 80% power)	±0.031	±0.038
Unadjusted P-Value (Adjusted for Multiple Testing)	0.058 (0.091)	0.207 (0.119)
Number of observations	4,569	6,295
Age Window	55-65	55-65

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Standard Errors clustered by age of the oldest household member. The RDDs have a linear specification in the age of oldest member in the household. Additional covariates in the RDD: type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

a chest infection in the last 3 weeks and in Column 4 we report the effect on the incidence of hypertension. The effects are about 2 percentage points, but neither is statistically significant at 5 % level. The largest point estimate of an effect is for Fibrinogen above the normal range. This is also the most precisely estimate effect. To get an idea of the power of our tests, we calculate, for each outcome, the minimum effect size we would have 80 % power to detect. These are displayed in the third row of Table 1.2. Although we have quite large samples sizes, minimum detectable effects are quite large. This is partly because we are examining the incidence of extreme values, and partly because we only have clean identification of treatment effects at the eligibility cut-off and must model the evolution of illness incidence on either side of the cut-off.

In Table 1.3 we explore the robustness of our results by varying our RDD specification in 5 ways. We first implement a quadratic polynomial for *f( )*, the function of the forcing variable relative to the age cut-off. We then re-estimate the models without including covariates in our specification. We further investigate whether our findings are sensitive to a change in the choice of the sample age window (either wider or narrower). Finally we drop the observations with the oldest member of the household aged 60 or 61 for whom we cannot determine whether they received the WFP or not exactly.

We find that our estimates of the discontinuity effect for the fibrinogen are robust to any of these changes in the RDD specification. The coefficient of the WFP effect is always statistically significant at 1 % level and the effect size lies between 4.4 percentage points and 8.7 percentage points. [20] This implies a reduction of 35 % to 70 % in the incidence of a high serum concentration of fibrinogen at the age cut-off. For the other measures of illness we find a negative coefficient in all the specifications

6.3%.

[20] All estimates are statistically significant at 5 % level after adjusting for multiple testing except for the specification with a quadratic function of the forcing variable, where p=0.074.

Table 1.3: Robustness checks. The impact of the Winter Fuel Payment on predictors of infection.

Effect of the WFP on Fibrinogen, C-reactive protein, Self-reported Chest Infection and Hypertension

	Fibrinogen	**C-reactive protein**	**Self-reported Chest Infection**	**Hypertension**
Quadratic specification in age	-0.079***	-0.030	0.001	-0.089***
of oldest household member	(-0.132; -0.026)	(-0.073; 0.013)	(-0.028; 0.029)	(-0.132; -0.046)
No additional covariates	-0.046***	-0.010	-0.026*	-0.014
	(-0.069; -0.024)	(-0.042; 0.023)	(-0.057; 0.004)	(-0.043; 0.015)
Narrower Age Window:	-0.087***	-0.033**	-0.015***	-0.049***
57-63	(-0.118; -0.056)	(-0.066; -0.001)	(-0.024; -0.006)	(-0.072; -0.025)
Wider Age Window:	-0.044***	-0.015	0.015	-0.022
50-70	(-0.068; -0.019)	(-0.038; 0.008)	(-0.009; 0.040)	(-0.050; 0.005)
Dropping observations whose	-0.060***	-0.012	-0.015	-0.032
eligibility cannot be	(-0.091; -0.030)	(-0.047; 0.024)	(-0.046; 0.017)	(-0.073; 0.009)
discretely determined				

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Standard Errors clustered by age of the oldest member level. 95 % confidence interval in parentheses.

Age window in the quadratic in age of oldest household member specification and no additional covariates specification: 55-65. Additional covariates in the RDD: type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

indicating an improvement in health with WFP eligibility. However, the estimates are variable and rarely statistically significant at conventional levels.

Table 1.4: Falsification Tests: Effect of a "placebo" eligibility at age 55 and age 65, and effect on above median fibrinogen concentration.

	Effect of WFP on Fibrinogen
Cut-off age 55	-0.015
(95% Confidence Interval)	(-0.057; 0.026)
Age Window	50-60
Cut-off age 65	0.004
(95% Confidence Interval)	(-0.052; 0.060)
Age Window	60-70
$\boldsymbol{Prob(Fibrinogen \geq 3.1g/l)}$	0.009
(95% Confidence Interval)	(-0.046; 0.065)
Age Window	55-65

Standard Errors clustered by age of the oldest member level. Additional covariates in the RDD: type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

We now provide some further checks on our main finding of a WFP effect on the incidence of high concentrations of fibrinogen. In Table 1.4 we present falsification tests for an effect at age cut-offs of 55 and 65. As these are not the eligibility cut-off, we should find no effect at these ages. As a further falsification test we check for an effect on the incidence of fibrinogen concentrations above the sample median. The idea here is that having an above-median concentration of fibrinogen is not a marker of disease. If we are measuring a reduction in disease incidence, that effect should be observed only in the upper tail of the distribution (as in our main estimates) and not around the median. [21] As Table 1.4 illustrates, we do not find any evidence of an effect across these specifications. This increases our confidence in the main effect reported in Table 1.2.

We also considered what our estimated effects for the incidence of fibrinogen concentration in excess of 4 g/l imply for levels fibrinogen in the upper tail of the distribution. To do this we estimate a quantile regression version of the RDD. Note that our base specification studies the probability that measured serum Fibrinogen exceeds a specified cut-off, and how this probability differs with WFP eligibility holding the cut-off constant at k:

$$P_{it}(WFP_{it}) = Prob(H_{it}^{Fib} > k|WFP_{it}) \tag{1.3}$$

A quantile regression inverts this relationship, holding the probability constant (at the chosen quantile, *1 - P*) and asking, essentially, how the cut-off varies with WFP eligibility.

$$k_{it} = F_{1-P}(H_{it}^{Fib}|WFP_{it}) \tag{1.4}$$

In our sample $prob(H_{it}^{Fib} = 1) \approx 12\%$ and the RDD estimates show that this falls by 6.1 percentage points with WFP eligibility. In this robustness check we consider how the 85^{th} and 90^{th} conditional quantiles of H_{it}^{Fib} vary with WFP eligibility (corresponding to $P = 0.15$ and $P = 0.1$). We find in both cases a drop in Fibrinogen of about 0.11 g/l. However the effects are less precisely estimated than the probability models. [22]

We finally follow Kling, Liebman, and Katz (2007) and create a Poor Health Index to

[21] The median fibrinogen in the neighbourhood of the age cut-off is 3.1 g/l (see Table 1.1).

[22] Inference for quantile regression is not straight forward. Asymptotic standard errors are not regarded as reliable and we employ a bootstrap procedure. Full details and results are available from the authors on request.

Figure 1.5: Effect of the Winter Fuel Payment on the Poor Health Index.

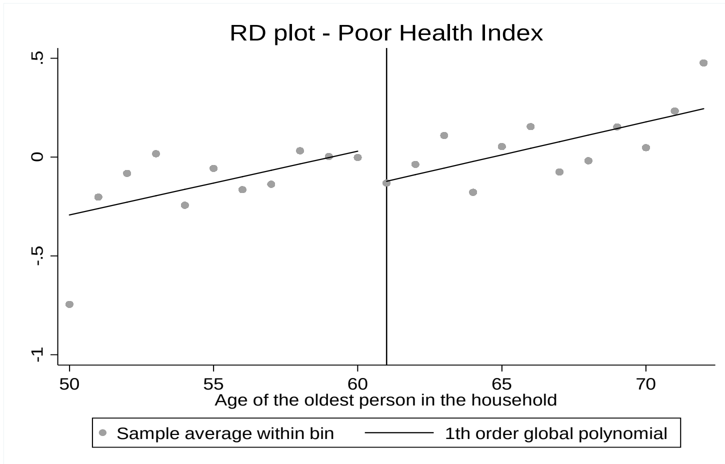


Figure 1.5 plots the residuals from a regression of the Poor Health Index on type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

improve the statistical power of detecting effects of the same sign and implement the RDD on this new measure. The Poor Health Index synthetises in a single measure our indicators of cardiovascular and respiratory diseases. We exclude self-reports of chest infection and focus on our binary objective measures, serum-Fibrinogen in excess of 4 g/l, serum-CRP in excess of 10 mg/l and Hypertension. [23] For each of these outcomes we calculate the *z-scores* subtracting the mean of the group of people just below the eligibility cut-off and dividing by the standard deviation of the same group. The Poor Health Index is the average of the three *z-scores* and a low value of the index is evidence of better health.

We first present the impact of the WFP on the Poor Health Index graphically in Figure 1.5. As in the Figures reported before, we interpret a discontinuity at the eligibility cut-off as evidence of an effect of the WFP on our index. The data in

[23]Self-reports of chest infection is the only measure that does not show a clear pattern in the data and is not reported in HSE.

Table 1.5: The impact of the Winter Fuel Payment on the Poor Health Index.

Linear Specification in age of oldest household member	**Quadratic specification in age of oldest household member**	**Narrower Age Window**	**Wider Age Window**	**Dropping observations whose eligibility cannot be discretely determined**
-0.225**	-0.449***	-0.445***	-0.194**	-0.286**
(-0.418; -0.032)	(-0.642; -0.257)	(-0.607; -0.283)	(-0.376; -0.013)	(-0.563; -0.010)
N= 3,481	N= 3,481	N= 2,343	N= 5,709	N= 3,142
Age Window: 55-65	Age Window: 55-65	Age Window: 57-63	Age Window: 50-70	Age Window: 55-65

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Standard Errors clustered by age of the oldest member level. 95% Confidence Interval in parentheses.

Additional covariates in the RDD: type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status vs age of the oldest person in the household, month of nurse visit, survey-wave dummies.

Figure 1.5 are still noisy as in the analysis of the single objective health outcomes, but there is a visible drop in the Poor Health index around the eligibility cut-off.

In Table 1.5 we show the size of the effect of the WFP on the Poor Health Index implementing various specifications of the RDD. The analysis includes only observations with a valid measure of Fibrinogen, CRP and Hypertension. The reduction of the Poor Health Index is in the range of 0.2 to 0.45 standard deviations with our estimates all statistically significant at 5 % level. Our preferred specification is presented in Column 1 and shows a decrease in the Poor Health Index of 0.23 standard deviations.

Table 1.6: Impact of the Winter Fuel Payment on the Poor Health Index in sub-groups.

Excluding Summer Months	**Low-Income: 1^{st} quartile**	**Low Educated**
-0.367*	-0.039	-0.460***
(-0.772; 0.039)	[-0.433; 0.354]	[-0.681; -0.239]
N= 2,351	N= 786	N= 980

Standard Errors clustered by age of the oldest member level. 95% Confidence Interval. Linear Specification in age of oldest household member. Age Window: 55-65. Summer months excluded : June, July, August and September. Low-Educated highest qualification reported: No Qualification, NVQ level 1, NVQ level 2. Additional covariates in the RDD: type of household, gender, smoker status, alcohol consumption, Body Mass Index, waist, education, income, employment status, month of nurse visit, survey-wave dummies vs age of the oldest person in the household.

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

We replicate the analysis on the Poor Health Index for three sub-samples of observations to investigate whether there are specific groups that drive our findings. In the first Column of Table 1.6, we report the estimates on a sample which excludes observations with a nurse visit in summer. The rational is that we do not expect any beneficial effect of the WFP on health in the summer months. Consistent with this expectation we find a slightly larger reduction in the Poor Health Index than in the full sample (a point estimate of 0.36 standard deviations that is statistically significant at 10 % level.) Second, we estimate the effect of the WFP on the Poor Health Index for low-income respondents. Beatty et al. (2014) show a particularly significant increase in the fuel expenditure for poorer households. These households should therefore experience larger health benefits. We find an effect magnitude of -0.04 standard deviations that is not precisely estimated, in part because of the reduction in the sample size. Finally, we consider the effect of WFP eligibility among low education respondents. Here we find a very large and statistically significant effect of around 0.46 standard deviations.

1.4 Discussion and conclusions

We find evidence to suggest that raising the cut-off age for WFP eligibility had a negative effect on the health of individuals made ineligible. We find a robust and statistically significant effect for only one of the individual illness measures we consider, though point estimates for all the markers we consider point in this direction. Reductions of 1 to 6 percentage points (for fairly rare events) are large effect sizes. Using a Poor Health Index that combines our illness markers, we find particularly large effects for low educated individuals.

For healthcare providers, these results highlight the need to be sensitive to inadequate indoor heating as a potential winter health risk, perhaps particularly among low education individuals who fall just short of eligibility for the WFP.

The eligibility for the WFP is tied to the Female State Pension age. As the Female State Pension age is been rising, eligibility is being tightened to older ages. This will be 65 in 2018.

Our RDD estimates the causal effect of the WFP on the health of individuals living in households where the oldest member of the household is 61. This is very useful because these are precisely the individuals who lost any benefits of the WPF as the eligibility age was incrementally increased from 2010. Our results therefore challenge the assumption of some analysts that raising the eligibility age would reduce program costs without any health cost. At a minimum, further research is needed to determine the loss of health benefits associated with potential future increases in the eligibility age.

Appendix

A1 Winter Fuel Payment eligibility

Table A1: Winter Fuel Payment eligibility.

Month of the nurse visit	Eligibility age	WFP eligibility Aged 60	WFP eligibility Aged 61	WFP eligibility Aged 62
January	60 + 4 months	8/12	1	1
February	60 + 5 months	7/12	1	1
March	60 + 6 months	6/12	1	1
April	60 + 7 months	5/12	1	1
May	60 + 8 months	4/12	1	1
June	60 + 9 months	3/12	1	1
July	60 + 10 months	2/12	1	1
August	60 + 11 months	1/12	1	1
September	61	0	1	1
October	61 + 1 months	0	11/12	1
November	61 + 2 months	0	10/12	1
December	61 + 3 months	0	9/12	1

Chapter 2

Does a housing subsidy cut really lower rents? Evidence from a reform in the UK

2.1 Introduction

In-kind transfers such as housing subsidies and food stamps are provided to encourage consumption of goods that are believed necessary to improve the individual and social well-being. By giving in-kind subsidies rather than cash transfers, governments aim to induce recipients to consume more subsidised goods than they would do voluntarily. This paternalistic justification of in-kind transfers is based on the rational that governments are concerned about the distribution of subsidised goods as well as the reduction of income inequality (Tobin, 1970; Rosen, 1983; Currie & Gahvari, 2008). Economic theory, however, predicts that in-kind transfers come at a cost. Subsidies do shift upward the demand of subsidised goods, but unless the supply is perfectly elastic, the market price of subsidised goods increases in response to a rise in the demand. This change in price implies that part of the benefit of subsidies accrues to suppliers. To evaluate the extent to which the benefit is split between consumers and suppliers, the literature commonly calculates the incidence of a subsidy, a measure of how much the price of the subsidised good rises with a marginal increase in the subsidy unit.

In this paper we focus on the incidence of a housing subsidy, a transfer that governments assign to low-income households to help them to pay for the rent of an adequate property. Governments allocate housing subsidies under the assumptions that low-income households consume less housing than appropriate and housing is an important determinant of quality of life. Indeed, better housing has a positive impact on children educational attainment (Currie & Yelowitz, 2000; Goux & Maurin, 2005) and living in less deprived neighbourhoods improves physical and mental health for adults (Ludwig et al., 2011, 2012; Leventhal & Brooks-Gunn, 2003) and decreases mortality (Jacob, Ludwig, & Miller, 2013).

However the standard theory predicts that housing subsidies affect rents when the supply does not adequately respond to the new housing demand. The change in rents due to the subsidy might not be limited to subsidy recipients and it might extend to the whole rental market (Susin, 2002; Eriksen & Ross, 2015). This is the typical case when some of the incidence of the housing subsidy is on tenants.

This paper investigates the incidence of housing subsidies on subsidised and unsubsidised tenants using a reform implemented in 2011 in the United Kingdom (UK) that aimed at tackling the stark growth in housing assistance outlays. The amount of subsidy that recipients received was to some extent linked to their rent and the UK government believed the housing subsidy outlays were excessive because

> *"some unscrupulous landlords were charging benefit claimants over the odds to make a quick buck at the expense of the taxpayer."*

(Lord Freud, Welfare Reform Minister, 10/2010)

A series of changes in the housing subsidy system were introduced to reduce the generosity of the subsidy and in the intention of the government the reform would

> *"bring an overall downward pressure on rents in the private sector. As these rents come down, more properties would become available to claimants and landlords would have certainty that their income would be protected."*
>
> *(Lord Freud, Welfare Reform Minister, 11/2010)*

However, the incidence of the subsidy might still fall on tenants if other factors than the housing demand of subsidised tenants affect rents. Among them, we highlight three factors that could explain why rents did not fall in response to the subsidy cut in this context. First, landlords might be reluctant to let properties to housing subsidy recipients and can replace them with unsubsidised tenants. The housing supply can be relatively elastic whether the pool of houses is restricted or expanded on the basis of landlords' decisions of letting to subsidy recipients. Second, the rental market and the home owner market are interconnected. When first-home buyers find difficult to purchase a property because of credit constraints or policies that incentivise landlords to invest in properties, the rental market of unsubsidised tenants expands and the chances of landlords of finding a substitute for subsidy recipients increase. The third factor is related to the nominal rigidity in the short term. Some contracts are agreed on a multi-year basis and tenants might not be able to re-negotiate their rent with landlords in the short term. In this latter case the incidence would entirely fall on tenants.

Several studies have examined the incidence of housing subsidies, but they have focused on either subsidised tenants (Gibbons and Manning, 2006; Fack, 2006; Kangasharju, 2010; Viren, 2013; Collinson and Ganong, 2015; Brewer et al., 2014) or unsubsidised tenants (Susin, 2002; Eriksen & Ross, 2015). Research on subsidised tenants have shown that the incidence of the housing subsidy is shared between tenants and landlords. However, empirical estimates are mixed and report an incidence in the range of 10 % and 90 %. In particular, for the UK, Gibbons and Manning (2006) examine the effect of a housing subsidy reform in the mid-90s and find that the incidence of the subsidy on landlords was around 60 % . On the other hand, Brewer et al. (2014) use administrative data exploiting the same reform as this paper and estimate that the incidence was 10 % on landlords. The evidence on the effect of housing subsidies on unsubsidised tenants is also heterogeneous. Analysing data on rents of the US biggest metropolitan areas, Susin (2002) finds that an increase in the number of housing vouchers raised rents in low income neighbourhoods for unsubsidised households on average by 16 %. Eriksen and Ross (2015) report a much smaller increase in rents of medium quality properties and a slight fall in lower-quality properties in response to a higher provision of vouchers. As in the prediction of the standard model, the magnitude of the incidence is, however, sensitive to the supply elasticity in the metropolitan area.

This paper adds to the literature on the incidence offering a link between the studies on the incidence on subsidised tenants and the studies on unsubsidised tenants. Using data from the Family Resource Survey (FRS) and the English Housing Survey (EHS), two cross-sectional surveys that include rich information on property characteristics, we exploit the housing subsidy reform in 2011 as a quasi-natural experiment to estimate the incidence of the subsidy. For subsidised tenants we follow Brewer et al. (2014) and Gibbons and Manning (2006) and run two separate regressions that estimate the effect of the reform on subsidy receipts and rents. The incidence of the housing subsidy is the ratio between the effect of the reform on rents and the effect of the reform on subsidy receipts. A second strategy follows Fack (2006) and uses the reform as an instrument for subsidy receipt. In this case the incidence is recovered

from the second stage where rents are regressed on the predicted values of the subsidy receipt. For unsubsidised tenants, the incidence is estimated through deviations from a time trend that rents would have followed if the reform had not been introduced.
We find that the average subsidy receipt fell by 7 % after the reform, but rents of subsidised tenants did not significantly fall in response to the subsidy cut. The estimated incidence on landlords is 7 % in line with the finding of Brewer et al. (2014). Unsurprisingly, the reform is estimated to have had hardly any impact on the rents of unsubsidised tenants. 33 months after the UK government started to implement the reform, the central estimate is that rents for unsubsidised tenants fell by 0.3 %, and this effect fades away later. In the most deprived areas, where subsidy recipients made up a larger share of the private rental market, the impact on unsubsidised tenants was not significantly larger.
The remainder of the paper proceeds as follows. The next section describes how a housing subsidy affects the rental market. Section 3 provides details of the LHA reform. Section 4 discusses the the empirical strategy to estimate the incidence of a housing subsidy cut. Section 5 describes the data and some descriptive statistics. Section 6 presents the findings on the incidence on subsidised and unsubsidised tenants. Section 7 and 8 conclude providing possible explanations on why the incidence of a housing subsidy mostly fell on tenants.

2.2 The incidence of the housing subsidy

The purpose of a housing subsidy is to make adequate dwellings affordable for low-income households. An housing subsidy is not a plain cash transfer, but it is often related to the rent of a property up to a certain ceiling. In this perspective, a subsidy cut should reduce the housing consumption of low-income households that would otherwise opt for more expensive and higher quality dwellings. Landlords might charge a lower rent in response to the decreased demand of subsidy recipients. However the drop in rents can be lower than the subsidy cut. The share of the subsidy cut that is not covered by the reduction in rents and is paid out of pocket by claimants is the incidence on tenants. The subsidy cut can also affect the equilibrium in the whole rental market. In this case the subsidy has also an incidence on unsubsidised tenants.

The incidence of the housing subsidy depends on the conditions of the rental market. In particular, the incidence is higher when the housing supply is inelastic, the housing demand is elastic and there is no nominal rigidity in rents. In the next sections, I review the rental market models presented in the literature of the incidence of the housing subsidy and describe the model of this paper.

2.2.1 Rental market models

The literature on the incidence of housing subsidies presents different models of the rental market. All these models take the competitive rental market as a benchmark. In a competitive setting, tenants are not discriminated according to whether they receive a subsidy to pay for their rents. Subsidised and unsubsidised tenants therefore pay the same rent for a certain property. When a subsidy is reduced, the housing demand of subsidised tenants decreases and rents fall. Interestingly, the model predicts that the subsidy cut would lower the rent of a property by an equal amount regardless of the type of tenant. The extent of any fall in rents depends on the elasticity of demand and supply for housing. The more responsive is the housing demand and the more inelastic is the housing supply, the higher is the drop in rents caused by the subsidy cut. In addition, rents would fall by more in areas with high concentration of subsidised tenants.

However, several studies provide evidence of frictions in the rental market. Gibbons and Manning (2006) show that a reform implemented in the UK in 1996/97 reduced rents only for recipients affected by the subsidy reduction, and had no significant impact on unsubsidised tenants. This finding supports a matching model where subsidised tenants have some bargaining power in negotiating a rent reduction with landlords. However, the fall in rents for subsidised tenants does not fully cover the subsidy cut because landlords can always opt for letting the property to unsubsidised tenants that are willing to pay higher rents. In the matching model, unsubsidised tenants also benefit from the subsidy cut but the effect on their rents is smaller compared to subsidised tenants. In an analogous paper Fack (2006) assumes that the rental market is divided into two separated sub-markets, one for low-income tenants and the other for high-income tenants. With the introduction of a subsidy, the housing supply in the low-income rental market plays a crucial role because subsidised tenants cannot rent a property in the high income sub-market. If their increased housing demand is not matched by a further and better provision of properties in the low-income segment, the model predicts that the incidence of the subsidy is entirely on tenants. Collinson and Ganong (2016) suggest a model where subsidy claimants have a limited time to find a suitable property in order to keep their subsidy eligibility. They are risk adverse and prefer finding a suitable dwelling in a low-income neighbourhood rather than searching a property in well-off neighbourhoods where landlords are more likely to refuse subsidy claimants. In this setting, a policy that sets a flat rate across neighbourhoods in a certain local area raises rents and traps subsidy recipients in low-income neighbourhoods. On the other hand, a policy that changes the rates in every neighobourhood within a local area leads some subsidy claimants to move from low-income neighbourhoods to higher income neighbourhoods.

Other papers exclusively focus on the spillover effects on unsubsidised tenants. The milestone study is Susin (2002) that suggests a rental market split in three segments according to the income of the residents in the local area: low-, middle- and high- income neighbourhoods. The three rental markets are separated and subsidised tenants are trapped in the low- income neighbourhoods. An increase in the housing subsidy has the effect of bidding up rents only in the whole low-income market. In contrast to Susin (2002), Eriksen and Ross (2015) suggest a rental market where claimants effectively move across neighbourhoods. In their model, housing subsidies increase rents of properties with a pre-subsidy rent near the subsidy rate and decrease rents of low-quality properties. This is because subsidised tenants leave low-quality properties and instead demand properties near the subsidy rate. Moreover, the impact of housing subsidies varies according to the supply elasticities of the local areas. Rents only rise in local areas where constructors cannot adjust the supply of houses to match the increased housing demand.

2.2.2 The incidence of a housing subsidy

Following Susin (2002), we set up a model to show how a housing subsidy affects rents paid by subsidised and unsubsidised tenants in the private rental market. The setting is a competitive market where subsidised and unsubsidised tenants rent their dwellings choosing from the same pool of properties. In a setting with housing subsidy in place, landlords provide Q^S properties and tenants demand Q^D housing . Q^S depends on R^S, the price that landlords receive to supply Q^S:

$$Q^S = A(R^S)^{\varepsilon_S} H^S \tag{2.1}$$

The demand of subsidy recipients Q^D_{Su} and the demand of unsubsidised tenants Q^D_U are

$$Q^D_{Su} = (\frac{R^S}{Sub})^{-\varepsilon_D} H^D \tag{2.2}$$

$$Q_U^D = (R^S)^{-\varepsilon_D} H^D \tag{2.3}$$

where Sub is the housing subsidy and $R_{Su}^D = \frac{R^S}{Sub}$ is rent for recipients and $R_U^D = R^S$ is rent for unsubsidised tenants, H^S and H^D are supply and demand shifters, ε_S and ε_D are the elasticity of supply and demand.

It is convenient to define the housing demand Q^D as the sum of $\ln Q_{Su}^D$, the housing demand of subsidised tenants, and $\ln Q_U^D$, the housing demand of unsubsidised tenants:

$$\ln Q^D = \ln Q_{Su}^D + \ln Q_U^D = \frac{1}{N}(\sum_{i \in N_U} q_i^U + \sum_{i \in N_{Su}} q_i^{Su}) \tag{2.4}$$

Using the log format, the housing demand for subsidised and unsubsidised tenants is expressed in terms of the demand elasticity ε_D :

$$\ln Q_{Su}^D = p_{Su}(h_D - \varepsilon_D \ln(R^S) + \varepsilon_D \ln(Sub) + \eta_{Su}) \tag{2.5}$$

$$\ln Q_U^D = (1 - p_{Su})(h_D - \varepsilon_D \ln R^S + \eta_U) \tag{2.6}$$

where the proportion of subsidised tenants is p_{Su}, the proportion of unsubsidised tenants is $(1 - p_{Su})$ and $h_D = \ln H^D$. The housing demand $\ln Q^D$ is defined as

$$\ln Q^D = p_{Su}(h_D - \varepsilon_D \ln(R^S) + \varepsilon_D \ln(Sub) + \eta_{Su}) + (1 - p_{Su})(h_D - \varepsilon_D \ln R^S + \eta_U)$$

and re-arranging the terms on the right hand side

$$\ln Q^D = h_D - \varepsilon_D \ln(R^S) + p_{Su}\varepsilon_D \ln(Sub) + \eta_D$$

Likewise, using the log format and defining $h_S = \ln H^S$, the housing is supply is

$$\ln Q^S = h_S + \varepsilon_S \ln R^S + \eta_S \tag{2.7}$$

Setting $\ln Q^D = \ln Q^S$ gives the following:

$$a + \varepsilon_S \ln R^S + \eta_S = h_D - \varepsilon_D \ln(R^S) + p_{Su}\varepsilon_D \ln(Sub) + \eta_D \tag{2.8}$$

Rents in the steady state are

$$\ln R^S = \frac{1}{\varepsilon_S + \varepsilon_D} \cdot [h_D - h_S - p_{Su}\varepsilon_D \ln(Sub) + \eta_D - \eta_S] \tag{2.9}$$

and the incidence of the housing subsidy on landlords is

$$I_L = \frac{p_{Su}\varepsilon_D}{\varepsilon^S + \varepsilon^D} \tag{2.10}$$

2.3 The Local Housing Allowance reform

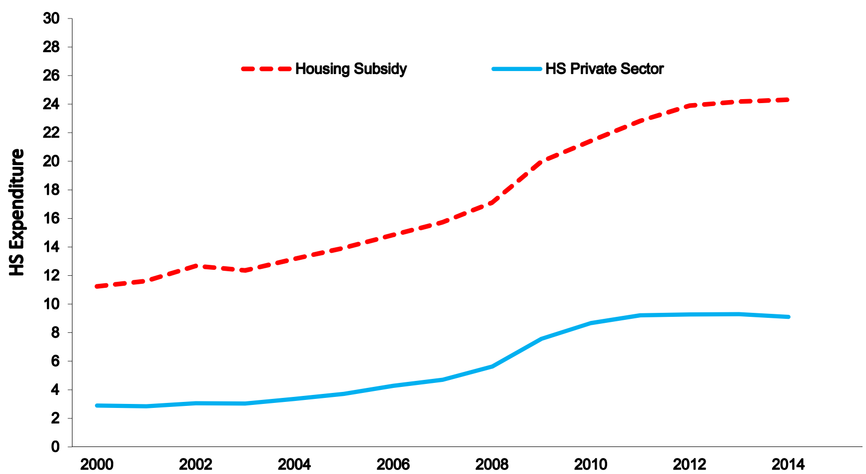
Housing is an important component of the welfare state in the UK. In 2014 the UK Government spent more than £24 billion on housing subsidies which made up 14 % of the total benefit expenditure and 0.5 % of the GDP [1]. Of this amount, the UK government spent nearly £9 billion to subsidise claimants in the private rental

[1]Housing subsidy figures are from the Department for Work and Pension (2015) and data on GDP is from the Office for National Statistics

market, whereas the rest funded social housing (Department for Work and Pension, 2015). In an attempt to control the sharp increase on housing expenditure seen in the first decade of the 21st century (see Figure 2.1), the UK government reformed the housing subsidy system in April 2011 by implementing a series of measures that reduced the generosity of the subsidy.

The amount of housing subsidy that claimants received depended on their income

Figure 2.1: Housing Subsidy (HS) expenditure in the UK. 2000-2014



Source: Department for Work and Pensions.

and the Local Housing Allowance (LHA), a flat rate based on the composition of the household and the local area - the Broad Rental Market Area (BRMA) - where the claimant lived. The boundaries of the BRMAs were defined according to the availability of a series of facilities and services in the local area. For each of the 152 BRMAs, a governmental institution - the Valuation Office Agency (VOA)- periodically released the LHA rates that varied with the number of bedrooms a claimant was entitled to.

In the pre-reform system VOA determined six LHA rates. Single childless customers aged under 25 were entitled to the Shared Accommodation rate, whereas other claimants could be eligible up to the five bedroom rate depending on the size of their household. The LHA rates were set at the median of the BRMA rent distribution and claimants were allowed to keep the excess (up to £15) between the subsidy and their actual rent if the rent charged by the landlord was lower than the LHA rate.

Table 2.1: Implementation of the LHA reform by month of claim

Claim Month	Transitory period	New System Rules
New Claim(April 2011-)	-	Since the claim
April	April 2011-December 2011	January 2012
May	May 2011-January 2012	February 2012
June	June 2011-February 2012	March 2012
July	July 2011-March 2012	April 2012
August	August 2011-April 2012	May 2012
September	September 2011-May 2012	June 2012
October	October 2011-June 2012	July 2012
November	November 2011-July 2012	August 2012
December	December 2011-August 2012	September 2012
January	January 2011-September 2012	October 2012
February	February 2011-October 2012	November 2012
March	March 2011-November 2012	December 2012

With the April 2011 reform the calculation of the maximum entitlement was shifted to the 30th percentile of the BRMA rent distribution, the five bedroom rate was abolished and the Shared Accommodation Rate was extended to cover single childless adults aged between 25 and 34. In addition, the reform established that the LHA rates could not exceed some national caps and claimants were no longer entitled to keep the £15 excess.

The time when claimants were rolled onto the new system differed for old and new claimants (see Table 2.1). LHA claimants that first claimed the subsidy after April 2011 or had a reassessment of their claim after that date were immediately subject to the new rules.[2]. Old claimants that were receiving the subsidy before the implementation of the reform became part of the new system at the first anniversary of their claim. In the first phase, they only lost the entitlement to keep the £15 excess. Nine months after the anniversary of their claim, the transitory period ended and they were fully rolled onto the new system. For example, an old claimant that had made a claim in April 2010 was fully rolled onto the new system in January 2012, whereas the new rules only applied in December 2012 for a claimant whose anniversary was in March 2010.

2.4 Identification strategy

2.4.1 The incidence on landlords that let to subsidised tenants

The empirical analysis of the incidence of a housing subsidy hinges on the quasi-experimental design of the 2011 LHA reform. We first investigate whether the LHA reform lowered the amount of subsidy received, as was the intentions of the UK government. Second, we estimate how the reform affected rents paid by claimants and calculate the incidence of the housing subsidy. Finally, we investigate whether the subsidy cut had a spillover effect on rents paid by unsubsidised tenants.

We implement two strategies to examine the incidence of the subsidy on claimants.

[2] A reassessment of a claim could be triggered by a change in the circumstances or if claimants moved to a new property

The first methodology follows Gibbons and Manning (2006) and Brewer et al. (2015) and estimate the impact of the reform on subsidy receipts and rents with two separate regressions shown in equations (2.11) and (2.12):

$$\ln Sub_{st} = \beta_0 + \beta_1 t_m + \beta_2 d_{st} + \mathbf{x}_{st}^{'}\gamma + \epsilon_{st} \tag{2.11}$$

$$\ln R_{st} = \alpha_0 + \alpha_1 t_m + \alpha_2 d_{st} + \mathbf{x}_{st}^{'}\delta + \eta_{st} \tag{2.12}$$

where the subscript s denotes the subsidy recipient and t denotes the time of the interview, Sub_{st} is the subsidy receipt, R_{st} is the rent paid by the recipient, t_m is a monthly time trend, d_{st} is a binary variable that indicates whether the recipient received the subsidy according to the post-reform rules and x_{st} are control variables such as property and household characteristics. The two regressions are estimated with OLS and standard errors are clustered at BRMA level.

The identification strategy exploits the reform as an exogenous change in the subsidy receipt. Indeed claimants observed before the implementation of the reform differed from claimants observed after the implementation only for the LHA system they were enrolled in.

All claimants observed before April 2011 were not affected by the reform and received the housing subsidies with the pre-reform rules ($d_{st} = 0$). Claimants observed after January 2013 were all enrolled in the new LHA system ($d_{st} = 1$). During the reform enforcement (April 2011- December 2012) claimants could be in the pre-reform system or the post-reform system depending on their claimant status. New claimants were recipients that claimed the subsidy for the first time or had a reassessment of their claim in the period the reform was enforced. They were immediately enrolled onto the post-reform regime ($d_{st} = 1$). Old claimants were recipients that had already claimed the housing subsidy before the reform and were fully rolled onto the new system at the end of the transitory period, nine months after their claim anniversary.

[3] However the data do not have information on the subsidy rules that applied to claimants at the time of the interview. The strategy used to deal with this issue varies with the survey. The FRS releases the month and the year when the last subsidy claim was made. Matching this information with the date of the interview allows to establish whether the claimant was in the pre- or post-reform system. In the EHS the information on the claim anniversary is not available and the LHA regime of the claimant cannot be established. In this case we assign to the post-reform regime only recipients that moved to a new property after April 2011. [4]. For the rest of the sample, the LHA system status is determined parametrically. The reform variable d_{st} is (1/12, 2/12,...,12/12) according to the end-of-the-transitory-period probability. As an example, old claimants observed in January 2012 have $d_{st} = 1/12$ because only claimants whose claim anniversary was in April were rolled onto the new system in that month. The new rules applied to any claimants observed in December 2012 and d_{st} is therefore 12/12 (full details are described in Table B1 in Appendix B1).

The changes of the LHA reform could trigger an anticipatory response from subsidy recipients. For example recipients could move to a cheaper house before the reform to offset the subsidy cut. Or landlords could renew contracts shortly before the reform agreeing lower rents in the wake of the forthcoming subsidy reduction. If there was anticipatory response our estimates of the incidence would be downward biased. However, in our specification we do not deal with anticipation effects.

After estimating the average effect of the reform on subsidy receipts and rents on subsidised tenants, we can calculate the incidence of the housing subsidy. It estimates how the burden of the subsidy is shared between tenants and landlords. A fall in rents in response to the LHA reform implies that landlords are affected by the subsidy cut.

[3] Old claimants had a nine-month transitory period in which they lost only the the entitlement to keep the £15 excess

[4] Claimants that moved to a different accommodation are assigned to the post-reform system because a change in the address led to a reassessment of their claim. We determine whether a claimant moved to a new property after April 2011 matching the information on the length of tenancy and the interview date.

The incidence on landlords is calculated as the proportion of the cut in the subsidy receipts that is covered by the rent decrease. In this specification the coefficient α_2 and β_2 in equation (2.11) and (2.12) represent how much the reform affected rents and subsidy receipts Sub_{st}. The incidence on landlords I_L is estimated through the ratio of these coefficients:

$$I_L = \frac{\alpha_2}{\beta_2} \tag{2.13}$$

My second strategy follows Fack (2006), and regresses rents on subsidy receipt using the reform as an instrument. [5] The amount of subsidy received is endogenous because it is correlated with unobserved characteristics that determine rents. The IV approach is implemented in two stages.

In the first stage I regress subsidy receipts Sub_{st} on the reform d_{st} and control variables as in equation (2.14)

$$\ln Sub_{st} = \rho_0 + \rho_1 t_m + \rho_2 d_{st} + \mathbf{x}'_{st}\mu + \eta_{st} \tag{2.14}$$

The second stage is a regression of rents R_{st} on the fitted values of subsidy receipts $\widehat{\ln Sub}_{st}$ obtained in the first stage.

$$\ln R_{st} = \theta_0 + \theta_1 t_m + \theta_2 \widehat{\ln Sub}_{st} + \mathbf{x}'_{st}\pi + \nu_{st} \tag{2.15}$$

The coefficient θ_2 in the second stage represents the rent percentage change in response to a 1 % increase of the subsidy. θ_2 is therefore an estimate of the incidence of the subsidy on landlords. The classical assumption of the exclusion restriction holds if the reform d_{st} is uncorrelated with the idiosyncratic shock in the second stage.

$$Cov(d_{st}, \nu_{st}) = 0 \tag{2.16}$$

[5] The equivalence of the two regression strategy and the IV approach is shown in Appendix B2.

The reform d_{st} satisfies the exclusion restriction assumption because it caused the exogenous change in housing subsidies occurred in 2011. Although all claimants were affected by the LHA reform, they were not affected evenly by the new rules. The introduction of the national caps affected people living in areas of London where the 30th percentile of the rent distributions was higher than the national LHA limits (2011-2014 LHA rates, VOA). [6] Singles aged between 25 and 34 were no longer entitled to the One Bedroom Rate but instead to the less generous Shared Accommodation Rate, whereas large households faced the abolition of the Five Bedroom Rate. In the equation (2.14) and (2.15) we avoid any source of bias including dummies for these categories in the controls $\mathbf{x}_{it}$.

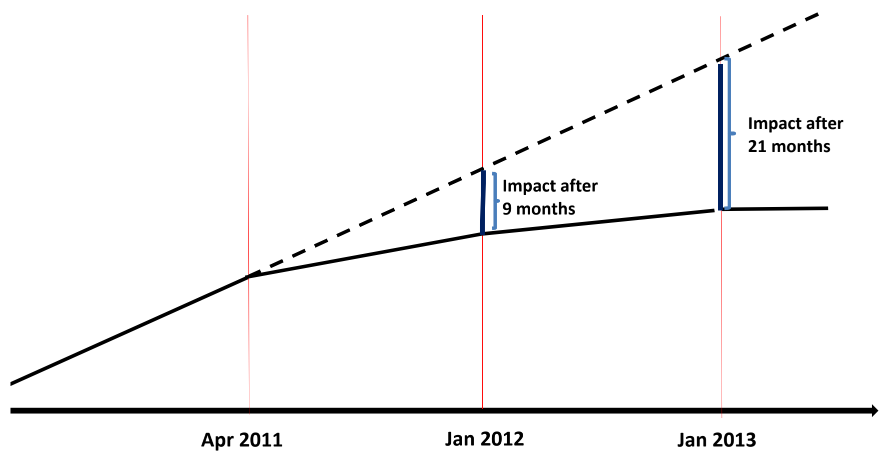
2.4.2 The incidence on unsubsidised tenants

After estimating the incidence of the housing subsidy on subsidised tenants, we examine whether the subsidy cut affected rents of private tenants that do not receive any subsidy. In a competitive rental market there is a single price for subsidised and unsubsidised tenants and a fall in rents for subsidised tenants should lead to a fall in rents for unsubsidised tenants. The effect of the reform for unsubsidised tenants is estimated through deviations from the linear time trend that rents would have had if the reform had not been implemented (see Figure 2.2).

The implementation of the LHA reform can be split in three different time windows: the first nine months after April 2011 when only the new claimants were rolled onto the new system, the period from January 2012 to December 2012 when claimants gradually flowed to the new system depending on their anniversary claim and the period after January 2013 when the reformed system was fully implemented. Thus, we allow for three deviations from the monthly linear trend t_m that rents would have

[6] Over the period 2011-2014 the national caps were binding in Central London and for most of the rates in the BRMAs in Inner London

Figure 2.2: Impact of the LHA reform on rents of unsubsidised tenants



had if the reform had not been implemented (the black dashed line in Figure 2.2):

$$R_{ut} = \alpha_0 + \alpha_1 t_m + \alpha_2 t_a + \alpha_3 t_b + \alpha_4 t_c + +\mathbf{x}_{ut}^{'}\gamma + \epsilon_{it} \tag{2.17}$$

where the subscript u denotes the unsubsidised recipient, R_{ut} are rents paid by unsubsidised tenant, t_a is the deviation from the linear trend t_m due to the first phase of the reform, t_b shows how the rent trend changes because of the gradual flow of claimants in the new system and t_c is the deviation from the linear trend t_m after the full enforcement of the reform changes. The pre-reform linear trend t_m is estimated with data from April 2008 to March 2011. The incidence on unsubsidised tenants is identified under the assumption that the rent time trend would have not changed if the reform had not been introduced.

2.5 Data

In order to estimate the incidence of the housing subsidy on subsidised and unsubsidised tenants, the analysis pools data from the Family Resource Survey (FRS) and the English Housing Survey (EHS). The FRS and the EHS are two annual cross-sectional surveys that collect information on rents, length of tenancy, household and dwelling characteristics. The purpose of using two sources of information is to boost the sample size for LHA claimants. The data we use span from April 2008 to March 2014, an interval of time that gives us enough observations in the pre- and post-reform period to estimate the time trend for rents in the private rental market.

Table B2 in Appendix B3 compares the characteristics of the pre-reform and the post-reform groups. The pre-reform group is made up of claimants who were observed before the implementation of the reform (April 2011) and old claimants who were observed at most 8 months after the anniversary of their claim in the reform phase-in period (April 2011-December 2012).[7] The post-reform group is composed of subsidy claimants who were observed after the full implementation of the reform (December 2012), claimants who were observed at least 9 months after the anniversary of their claim, and claimants who moved to a new property during the phase-in period. Among the claimants observed during the phase-in of the reform, 829 are assigned to the pre-reform group and 440 to the post-reform group. For 735 claimants there is no sufficient information to establish if they received the LHA according to the new or old rules. [8] As we can see from Table B2, claimants before and after the reform pay a very similar rent. However, there is a significant drop of around 10 % in subsidy receipts after the reform. The pre- and the post-reform groups have similar characteristics. The t-tests reject the null hypothesis of equality of the means for

[7]Claimants are fully rolled onto the new LHA regime 9 months after their claim anniversary.

[8]This occurs if claimants do not report information on their claim anniversary or they do not move to a new property in the reform phase-in period.

some variables but the magnitude of the means is not substantially different.

Table B3 in Appendix B3 shows the same descriptive statistics for unsubsidised tenants. Rents are not very different before and after the reform. The other variables present very similar means. When we find difference in the means, this is for the same variables and in the same direction of claimants (see for example the variables lenght of tenancy, HRP age and furnished/unfurnished property). This is reassuring that there is no selection in the post-reform group of claimants.

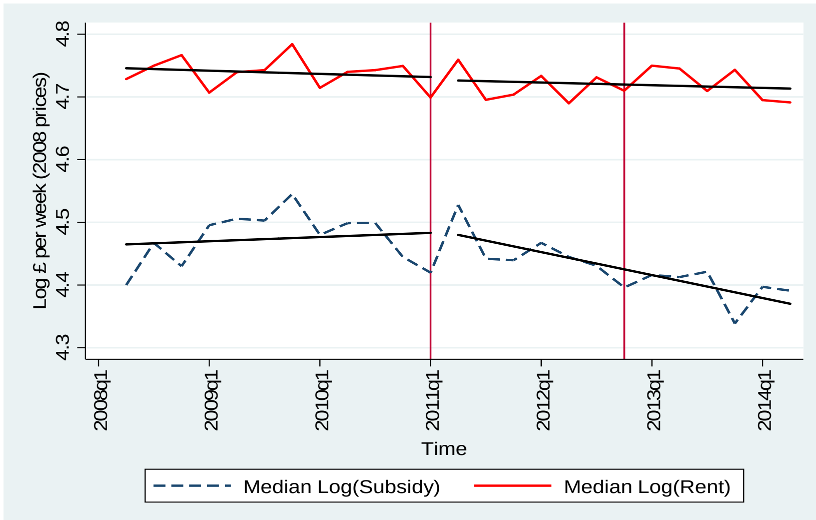
2.6 Results

The purpose of the LHA reform was to lower the aggregate expenditure on housing subsidies. The expectation of the government was that the subsidy cut would reduce rents in the private rental market leading to a minimum loss for recipients and benefits for unsubsidised tenants. In other words, the government assumed the housing subsidy to be mainly incident on landlords. In this section, we first explore whether the effects of the reform matched the expectations of the government with a graphical analysis and then we report the estimates of the incidence of the housing subsidy on subsidised and unsubsidised tenants.

2.6.1 The incidence on subsidised tenants

The graphical analysis shows that the reform only affected the subsidy receipts. Figure 2.3 gives evidence that subsidy receipts remained constant over the pre-reform period and then they fell after the implementation of the subsidy cut. Unlike the expectations of the government rents did not respond to the subsidy cut. Indeed, rents of subsidised tenants were slightly falling in the pre-reform period, and the trend was

Figure 2.3: Median weekly Log(Subsidy) and Median weekly Log(Rent) in 2008 prices for LHA claimants in the private rental sector, Q2 2008- Q1 2014



Source: English Housing Survey and Family Resource Survey.
The two vertical lines mark the introduction of the LHA reform in April 2011 and the full implementation of the reform in December 2012.

not significantly different in the post-reform period.[9]

The upper panel of Table 2.2 gives the estimates of the impact of the reform on subsidy receipts and rents using the equations (2.11) and (2.12). [10] Subsidy receipts and rents are expressed in log terms and the coefficients of the reform can be interpreted as semi-elasticities. In Column (1) and (2) we only condition on the local area fixed effect, housing subsidy rate entitlement and a survey dummy. The estimates confirm the conclusions drawn in the graphical analysis. The reform cut the average subsidy receipt by nearly 7 %, but rents were not affected. The inclusion of property and demographics characteristics and the housing price index reduces the magnitude of the effect on subsidy receipts to 5.5 % (Column (3)), whereas the effect on rents

[9]Subsidy receipts and rents are deflated to 2008 prices with the Consumer Price Index.

[10]Estimates of the reform effects do not account for a possible anticipatory behaviour of subsidy recipients.

Table 2.2: Effect of the reform on Log-Subsidy Receipt and Log-Rent and Incidence of the Housing Subsidy

	Log Subsidy (1)	Log Rent (1)	Log Subsidy (2)	Log Rent (2)
Reform	−0.069***	0.000	−0.054**	−0.004
	(−0.119;−0.019)	(−0.031;0.030)	(−0.104;−0.005)	(−0.034;0.026)
Monthly time trend	−0.001	−0.001	0.000	0.000
	(−0.002;0.000)	(−0.001;0.000)	(−0.001;0.001)	(−0.0010.000)
Incidence	–	0.006	–	0.071
	–	(−0.426;0.437)	–	(−0.446;0.589)
Incidence using IV	–	0.006	–	0.071
	–	(−0.425;0.435)	–	(−0.445;0.587)
N	7629	7629	7085	7085

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$
Confidence Intervals at 95 % in parentheses. Cluster Robust Standard Errors at BRMA level.
Control variables in Specification (1): BRMA, subsidy rate entitlement, survey dummy.
Control variables in Specification (2): controls in Specification (1), shared accommodation with other households, length of tenancy, type of dwelling, rural-urban classification, house rented furnished, type of utility included in the rent, age of the HRP, housing price index, deprived area dummy, number of rooms, number of bedrooms, log-income, other benefits recipient, non-dependant living in the household.
The incidence in the third line is calculated through the ratio between the effect of the reform on rents and the effect on subsidy receipts as shown in equation 2.13. Point estimates and standard errors are based on the delta method using the Stata command *nlcom*.

remained unaltered at around -0.4% (Column (4)). The implied estimate of the incidence on landlords is around 7%, suggesting that the burden of the subsidy cut was mainly borne by tenants. Although the estimate of the incidence is not precisely estimated, findings are consistent with Brewer et al. (2014) that estimate a 10 % incidence on landlords using the same reform of this paper. [11] It is however very different compared to the 60 % estimate in Gibbons and Manning (2006).

In Table 2.3 we replicate the analysis for two subgroups to check whether there is heterogeneity in the incidence of the housing subsidy. The first subgroup are people living in London where the reform had a larger impact due to the introduction of the

[11] The incidence is not precisely estimated regardless of using the two separate regression approach or the instrumental variable strategy. The confidence interval of the incidence contains implausible negative values.

subsidy caps. Brewer et al. (2014) report that the average receipt in London fell by £13.40 per week compared to £6.80 in Great Britain. In the London BRMAs where the caps were binding the average receipt decreased by a much higher £41.90 per week. The point estimate of the incidence in London is 45 %, although it lacks again precision. This suggests that some negotiation between landlords and tenants took place in areas where caps heavily affected subsidy receipts. The second subgroup are recipients that moved to their current property at most 3 years before the interview. The rational to analyse this group is that the subsidy cut might induce recipients to rent cheaper properties leading to a higher incidence on landlords. Column (2) of Table 2.3 however shows that this is not the case suggesting that other factors such as the housing demand of other recipients or unsubsidised tenants prevented them from finding a cheaper accommodation.

Table 2.3: Incidence of the Housing Subsidy in subgroups

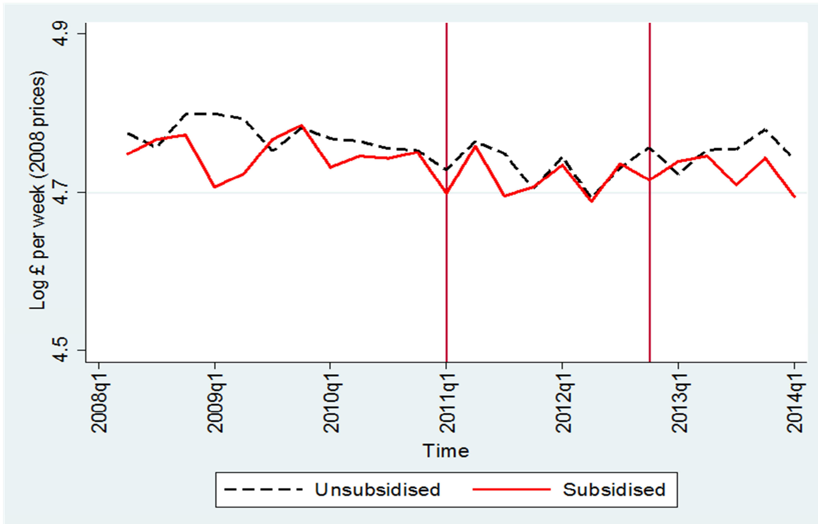
	London	Recently Moved
Incidence	0.448	0.019
	$(-0.119; -0.019)$	$(-0.580 ; 0.617)$
N	1020	4091

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$
Confidence Intervals at 95 % in parentheses. Instrumental Variable approach. Cluster Robust Standard Errors at BRMA level
Control variables: BRMA, subsidy rate a household is entitled to, survey dummy, shared accommodation with other households, length of tenancy,type of dwelling, rural-urban classification, house rented furnished, type of utility included in the rent, age of the HRP, housing price index, deprived area dummy, number of rooms, number of bedrooms, log-income, other benefits recipient, non-dependant living in the household.

2.6.2 The incidence on unsubsidised tenants

We now estimate the incidence of the housing subsidy on non-recipients. Figure 2.4 shows rents of subsidised and unsubsidised tenants. The median rent of unsubsidised

Figure 2.4: Median weekly Log(Rent) for subsidised and unsubsidised tenants in the private rental market, Q2 2008- Q1 2014



Source: English Housing Survey and Family Resource Survey.
The two vertical lines mark the introduction of the LHA reform in April 2011 and the full implementation of the reform in December 2012.

tenants was higher than the median rent of recipients through all the period, but differences were minimal. Until the full implementation of the reform the trend for both series was downward. However, rents paid by unsubsidised tenants began to grow after January 2012, whereas rents were roughly constant for subsidy recipients.

The reform did not lower overall rents for claimants, so no effect is expected on rents for unsubsidised tenants. Table 2.4 reports the effect of the reform on unsubsidised tenants estimated using equation (2.17). Each line is the estimate of the deviation from the pre-reform trend due to the three phases of the reform. Rents for unsubsidised tenants fell by -0.3 % per week after 33 months the LHA reform was implemented.[12] Column (2) shows the estimates for the most deprived areas where

[12]The decrease in rents are obtained by multiplying the coefficients in Table 2.4 with the number of the months elapsed since the reform was implemented. For example the decrease in rents in January 2013-33 months after the introduction of the reform- is 33*(-

Table 2.4: Incidence of the Housing Subsidy on unsubsidised tenants

	Log Rent All Sample	Log Rent Most Deprived Areas
First Reform period	-0.00032 (-0.0088 ; 0.00024)	-.00047 (-0.00163; 0.00068)
Second Reform period	0.00021 (-0.00029 ; 0.0072)	-0.00004 (-0.00103; 0.00095)
Third Reform period	0.00025 (-0.00013 ; 0.00063)	0.00056 (-0.00010 ; 0.00121)
Monthly time trend	0.00028 (-0.00067 ; 0.00124)	-0.00001 (-0.00202 ; 0.00199)
N	16256	2702

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Confidence Intervals at 95 % in parentheses. Cluster Robust Standard Errors at BRMA level.

Control variables: BRMA, , survey dummy, shared accommodation with other households, length of tenancy,type of dwelling, rural-urban classification, house rented furnished, type of utility included, in the rent, age of the HRP, housing price index, deprived area dummy, number of rooms, number of bedrooms, log-income, other benefits recipient, non-dependant living in the households.

a larger share of recipients are expected to live. Their rents fell by 1 % per week 33 months after the implementation of the reform.

Table 2.5: Differences in rents between subsidised and unsubsidised tenants

	Log Rent All Sample
Unsubsidised tenants	-0.174***
	(-0.205; -0.143)
Reform	-0.007
	(-0.043; 0.030)
Reform*Unsubsidised tenants	0.010
	(-.016; .036)
Monthly time trend	-0.0010
	(-.0017; -.0003)
Monthly time trend*Unsubsidised tenants	0.0010
	(.0003; .0018)
N	23330

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Confidence Intervals at 95 % in parentheses. Cluster Robust Standard Errors at BRMA level.

Control variables: BRMA, , survey dummy, shared accommodation with other households, length of tenancy,type of dwelling, rural-urban classification, house rented furnished, type of utility included, in the rent, age of the HRP, housing price index, deprived area dummy, number of rooms, number of bedrooms, log-income, other benefits recipient, non-dependant living in the households

To understand the small incidence on landlords, we supplement the analysis with an equation that aims to explain the differences between rents paid by subsidised and unsubsidised tenants. We pool together data on subsidised and unsubsidised tenants and estimate the equation

$$\ln R_{it} = \delta_0 + \delta_1 UT_{it} + \delta_2 t_m + \delta_3 t_m * UT_{it} + \delta_4 d_{it} + \delta_5 d_{it} * UT_{it} + \mathbf{x}_{st}^{'} \phi + \xi_{st} \quad (2.18)$$

0.00032)+21*0.00021+0.00025*12=-0.003

where UT_{it} is a dummy that indicates whether tenant i is a subsidy recipient. Equation (2.18) allows for different trend in rents and impact of the reform between subsidised and unsubsidised tenants. The price of property characteristics ϕ is assumed constant among the two groups.

Table 2.5 shows that subsidy recipients pay on average 17 % more than unsubsidised tenants for a property. The correlation between recipient status and unobserved property characteristics can bias this estimate. However, it can be interpreted as an indicator that landlords accept subsidised tenants only if they pay a premium to rent their property. If the demand of unsubsidised tenants is high and recipients can be easily replaced with other tenants, landlords can refuse to agree a discount to the premium and ask recipients to pay the full rent.

2.7 Discussion

A considerable number of studies exploit housing subsidy reforms to scrutinize the incidence on landlords of housing subsidies. The reported estimates vary between 10 % and 90 % but the reasons of the large heterogeneity in the estimates are not discussed in the literature. A possible explanation for this range is that every reform has been implemented in a specific rental market and housing subsidies differently affect the housing demand and the decisions of claimants.

Fack (2006) assesses the impact of an early 90s reform in France that extended the eligibility of housing subsidies to low-income singles, couples without children and students. The reform sharply increased the housing demand for these new groups of subsidised tenants. Students that lived with their parents and singles that shared their accommodation with other tenants could claim support to rent an independent accommodation. This substantial rise of the housing demand and the low response of the supply led to only 22 % incidence on landlords. Kangasharju (2010) and Viren

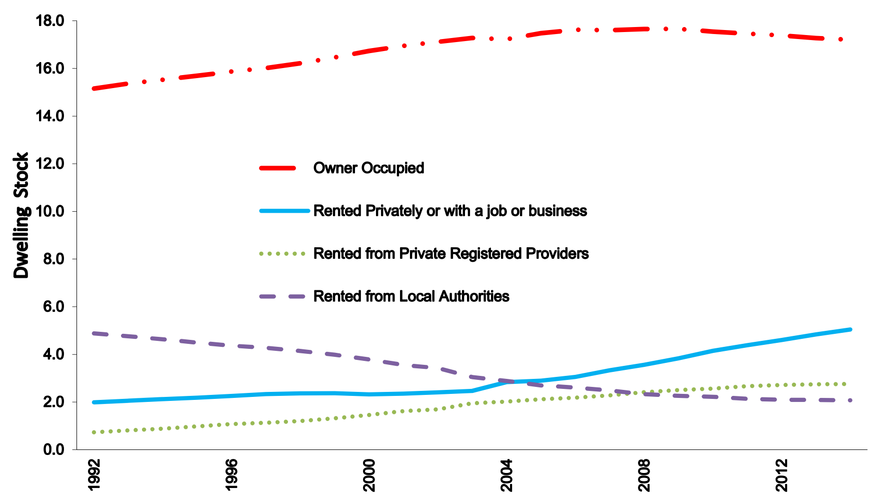
(2013) study the housing subsidies in Finland and find that the incidence on landlords varies between one-third and 50 %. The housing subsidy programme in Finland is peculiar because a share of the rents is always paid by claimants regardless of their income and the payment is made directly to landlords. The reform implemented in 2002 increased the ceiling of the subsidy and accordingly landlords, that held information on the amount of the subsidy, rose rents. 'More recently, Eerola, Lyytikäinen, et al. (2017) estimate the incidence of housing subsidies exploiting discontinuities in subsidy receipts at certain property size and property age thresholds. They find only small differences in rents at the cut-offs in line with this study and Brewer et al. (2014). Collinson and Ganong (2016) examine various housing voucher policies in the US and find that the incidence on landlords is around 80-90 % for tenants that remained in the same property and around 40 % when subsidy recipients moved to a new property. The nominal price rigidity and the active role of the government in bargaining rents with landlords could explain why prices did not soar when recipients stayed in their property.

In the UK Gibbons and Manning (2006) estimate the incidence of the housing subsidy using a reform implemented in the mid 90s. This reform cut the subsidy rates but it did not affect recipients who claimed their subsidy before the reform. The fact that Gibbons and Manning (2006) identify treated claimants as new tenancies that moved to a new property might explain why they find that more than 60 % of the incidence fell on landlords. [13] One factor that helps to understand the much lower incidence found in this paper is the nominal rigidity in the short run. The LHA reform in 2011 reduced the generosity of the subsidy but unlike the reform in the mid 90s it affected new and continuing tenancies. Tenants in multi-year contracts were not able to move to a new property or negotiate a reduced price with landlords. Genesove (2003) show that rents do not change for 29 % of the dwellings within a year and this

[13] Unlike recipients that stay in their properties, recipients that move to a new house have a reservation price for moving that

figure increases to 36 % for continuing tenants. This observation is supported by the evidence in Collinson and Ganong (2016) with rents rising by only 9 cents per dollar of subsidy for recipients that stayed in the same properties after a housing voucher reform.

Figure 2.5: Dwelling stock by tenure in Great Britain, 1992-2014



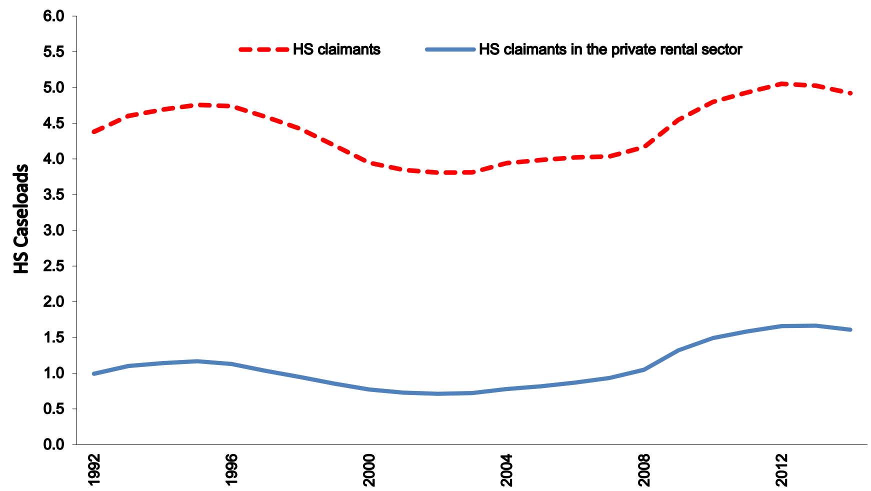
Source: ONS.

If nominal rigidity might be the reason for the low incidence in continuing tenancies, this does not explain why rents did not fall for affected claimants that recently moved to a new property (see Table 2.3). [14] However, there are other important facts that help to figure out the small incidence on landlords for this group. The first fact is the sharp growth of the private rental market that started in 2004 and continued over the period of the LHA reform (Figure 2.5).

This increase in the private rental market compensated for the decline in the social

[14] The assumption here is that claimants affected by the reform that moved to a new property should rent cheaper dwellings to compensate the effect of the subsidy cut.

Figure 2.6: Housing Subsidy (HS) Caseloads, 1992-2014

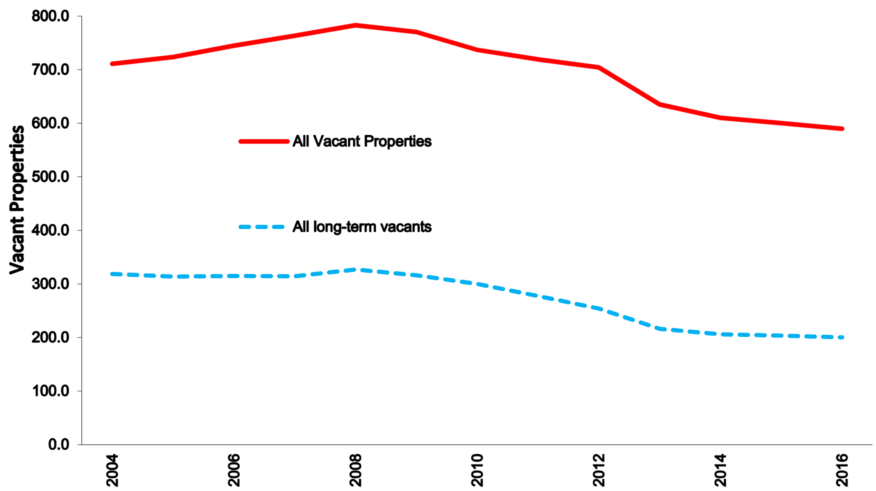


Source: Department for Work and Pensions.

housing sector and the more recent reduction in the home-ownership. The second fact is that the number of subsidy claimants quickly rose in the private rental market after the financial crisis in 2008 and stabilized in 2013 when the reform was fully implemented (Figure 2.6). Therefore recipients that moved to a new property faced the increase in the housing demand generated by unsubsidised tenants and new recipients and they might have not had any bargaining power to negotiate a reduced rent with landlords. These two facts did not occur in the mid 90s, the period analysed in Gibbons and Manning (2006). The number of subsidy recipients started falling when the reform was introduced in 1996 and in that period there was no additional pressure from unsubsidised tenants. New subsidy recipients could therefore rent a cheap property and share the burden of the subsidy cut with landlords.

Another important factor that determines the incidence of the subsidy is the housing

Figure 2.7: Vacant Dwellings, 2004-2016



Source: Department for Communities and Local Government.

supply. In times of high housing demand, landlords could decide to move from the subsidy recipient market to the unsubsidised tenant market. This fact is exacerbated when recipients and non-recipients are not perceived as equivalent. Indeed, subsidy recipients were associated with anti-social behaviour and rent arrears and only 50 % of landlords were eager to let them a property (Private Landlords Survey 2010). The LHA reform increased the reluctance of private landlords to accept claimants and rose the number of evictions and not-renewal of tenancies (Support for housing costs in the reformed welfare system, Fourth Report of Session). In addition, the decline in the number of vacant properties (Figure 2.7) could be an indicator of restrictions in the housing supply and might be a further element to explain why the reform did not reduce rents of subsidised and unsubsidised tenants.

2.8 Conclusions

In 2011 the UK government implemented a housing subsidy reform with the aim of reducing the welfare expenditure on housing and lowering rents in the private rental market. The justification given by the UK government for this reform was that landlords were charging claimants by far more than a fair rent. However, this study shows that the LHA reform did not significantly affect rents for subsidised and unsubsidised tenants and the incidence of the subsidy was mostly on tenants. An explanation for this finding might be the nominal rigidity of rents in the short run. Rents may not adjust because rental contracts are typically renewed annually or on a multi-year basis. In addition, claimants could not have enough bargaining power to obtain a lower rent. This mainly applies in rental markets where the high housing demand from unsubsidised tenants and new subsidy recipients outweighs the decrease in demand due to a subsidy cut and landlords have preferences for letting to unsubsidised tenants.

The findings of this paper are consistent with Brewer et al. (2014) but in contrast with most of the literature on the incidence of the housing subsidy. In the UK context, Gibbons and Manning (2006) use a reform in the mid 90s and find that the incidence on landlords was 60 %, larger than the 7 % estimate of this paper or the 10 % in Brewer et al. (2014). However, their identification strategy and the housing market in the mid 90s were very different. Indeed the reform exploited in Gibbons and Manning (2006) affected only claimants that moved to new properties. Assuming that rents they could afford depended on the reduced subsidy, they targeted on average cheaper houses. Secondly, the rental market in the mid 90s was not characterised by an increase in the demand from unsubsidised tenants and new recipients.

From a policy perspective, it is important to bear in mind the unintended consequences of the 2011 reform. If rents did not respond to the reform, subsidy recipients offset the subsidy cut by cutting consumption of other goods or increasing the labour supply (as Jacob and Ludwig (2012) show in the US). However, a subsidy cut does not only have an economic impact but it can also affect health of recipients. The fear of losing house can indeed cause mental distress and increase the pressure on the health system (Reeves, Clair, McKee, & Stuckler, 2016).

This paper has shown that cutting the housing subsidy is not enough to drive prices in the rental market. A more effective policy could be incentivising the private construction of affordable houses in areas with high housing demand. A second important tool could be increasing the bargaining power of subsidy recipients giving them support when they negotiate a rental agreement.

Appendix

B1 LHA regime status

Table B1: LHA regime status d_{it} by month of the interview

Month of the interview	Old Claimants	New Claimants
Before LHA reform (Jan 2008-Apr 2011)	0	0
April 2011	0	1
May 2011	0	1
June 2011	0	1
July 2011	0	1
August 2011	0	1
September 2011	0	1
October 2011	0	1
November 2011	0	1
December 2011	0	1
January 2012	1/12	1
February 2012	2/12	1
March 2012	3/12	1
April 2012	4/12	1

May 2012	5/12	1
June 2012	6/12	1
July 2012	7/12	1
August 2012	8/12	1
September 2012	9/12	1
October 2012	10/12	1
November 2012	11/12	1
December 2012	1	1
After LHA reform (Jan 2013-Dec 2014)	1	1

B2 Equivalence between the IV estimator θ_2 and the two regressions approach

Define X as a $N \times K$ matrix of the $K-1$ exogenous independent variables and the endogenous regressor (the subsidy S), Z a $N \times K$ matrix of the $K-1$ independent variables and an instrument (the reform dummy d) and $\mathbf{y}$ a $N \times 1$ vector of the dependent variable rent.

The vector of parameters θ from the model 2.15 is obtained through the instrumental variable (IV) estimator. θ is defined as

$$\theta = (Z^{'}X)^{-1}Z^{'}\mathbf{y} = (Z^{'}X)^{-1}(Z^{'}Z)(Z^{'}Z)^{-1}Z^{'}\mathbf{y} \tag{2.19}$$

Since $(Z^{'}X)^{-1}(Z^{'}Z) = ((Z^{'}Z)^{-1}(Z^{'}X))^{-1}$

$$(Z^{'}X)^{-1}(Z^{'}Z)(Z^{'}Z)^{-1}Z^{'}\mathbf{y} = ((Z^{'}Z)^{-1}(Z^{'}X))^{-1}(Z^{'}Z)^{-1}Z^{'}\mathbf{y} \tag{2.20}$$

θ_2 is the coefficient associated to the endogenous regressor S and represents the incidence on landlords of the subsidy. It is obtained by multiplying the second row of the matrix $((Z^{'}Z)^{-1}(Z^{'}X))^{-1}$ by $(Z^{'}Z)^{-1}Z^{'}\mathbf{y}$.

Define $\mathbf{x}$ a $N \times 1$ vector of the endogenous variable S.
The ratio between the vector of parameters β from model 2.11 and α from model 2.12 is defined as

$$\frac{\beta}{\alpha} = \frac{(Z^{'}Z)^{-1}Z^{'}\mathbf{y}}{(Z^{'}Z)^{-1}Z^{'}\mathbf{x}} = ((Z^{'}Z)^{-1}Z^{'}\mathbf{x}))^{-1}(Z^{'}Z)^{-1}Z^{'}\mathbf{y} \tag{2.21}$$

B3 Descriptive Statistics

Table B2: Descriptive statistics for LHA claimants in the pre- and post- reform period, showing means and t-tests on the equality of means

	Pre-Reform	Post-Reform	t-test
Log Rent per week	4.778	4.772	0.006
Log Housing Subsidy per week	4.452	4.354	0.098***
Shared Accomodation Rate	0.014	0.013	0.001
One Bedroom Rate	0.370	0.309	0.061***
Two Bedroom Rate	0.400	0.414	-0.014
Three Bedroom Rate	0.161	0.197	-0.036***
Four Bedroom Rate	0.041	0.052	-0.011**
Five Bedroom Rate	0.014	0.015	-0.000
Share Accomodation	0.007	0.007	0.000
Tenancy $<$ 1 year	0.260	0.230	0.030***
1 year $<=$ Tenancy $<$ 2 years	0.189	0.164	0.026**
2 years $<=$ Tenancy $<$ 3 years	0.144	0.147	-0.002
3 years $<=$ Tenancy $<$ 5 years	0.143	0.174	-0.031***
5 years $<=$ Tenancy $<$ 10 years	0.138	0.180	-0.041***
Tenancy $>=$ 10 years	0.124	0.105	0.019**
Detached House	0.052	0.052	-0.000
Semi Detached House	0.208	0.207	0.001
Terraced House	0.433	0.424	0.009
Flat	0.296	0.309	-0.014
Room	0.008	0.006	0.002
Couple with no Children	0.072	0.068	0.004

Couple with Children	0.170	0.191	-0.021**
Lone Parents	0.370	0.387	-0.017
Single Aged 16 to 24	0.013	0.011	0.002
Single Aged 25 to 34	0.036	0.026	0.010**
Single Over 34	0.253	0.205	0.047***
Other Type of Family	0.087	0.113	-0.026***
HRP Age 16 t o24	0.119	0.093	0.026***
HRP Age 25 to 34	0.256	0.288	-0.031***
HRP Age 35 to 44	0.250	0.254	-0.004
HRP Age 45 to 54	0.150	0.158	-0.007
HRP Age 55 to 64	0.091	0.095	-0.004
HRP Age Over 64	0.133	0.113	0.020**
Furnished	0.118	0.092	0.026***
Partial Furnished	0.189	0.164	0.025**
Unfurnished	0.694	0.744	-0.050***
Male	0.388	0.385	0.003
Number of rooms	4.56	4.52	0.04
Number of bedrooms	2.31	2.35	-0.04
Log income	9.218	9.307	-0.089***
Non dependant in the household	0.128	0.165	-0.038***
Multiple deprivation index	0.292	0.285	0.007
Sample Size	4,170	2,292	

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

The statistics regarding the proportion of subsidised tenants with a certain LHA rate are based on the rules of the pre-reform system.

HRP is the Household Reference Person.

Log Income in 2008 prices.

The Multiple Deprivation is a binary variable that indicates whether the HB claimant lives in the 15 % (FRS data) or 20 % (EHS data) most deprived areas.

Table B3: Descriptive statistics for unsubsidised tenants in the pre- and post- reform period, showing means and t-tests on the equality of means

	Pre-Reform	Post-Reform	t-test
Log Rent per week	4.76	4.79	-0.03***
Share Accomodation	0.099	0.059	0.040***
Tenancy < 1 year	0.379	0.370	0.009
1 year <= Tenancy < 2 years	0.216	0.183	0.033***
2 years <= Tenancy < 3 years	0.139	0.139	0.000
3 years <= Tenancy < 5 years	0.121	0.143	-0.022***
5 years <= Tenancy < 10 years	0.081	0.111	-0.030***
Tenancy >= 10 years	0.064	0.055	0.009**
Detached House	0.096	0.096	0.000
Semi Detached House	0.186	0.188	-0.002
Terraced House	0.328	0.335	-0.007
Flat	0.375	0.362	0.013
Room	0.013	0.014	-0.001
Couple with no Children	0.263	0.235	0.029***
Couple with Children	0.220	0.229	-0.009
Lone Parents	0.051	0.073	-0.022
Single Aged 16 to 24	0.034	0.044	-0.010***
Single Aged 25 to 34	0.079	0.084	-0.005
Single over 34	0.156	0.153	0.003
Other Type of Family	0.197	0.182	0.015**
HRP Age 16 to 24	0.154	0.128	0.026***
HRP Age 25 to 34	0.365	0.381	-0.016**
HRP Age 35 to 44	0.232	0.237	-0.005

HRP Age 45 to 54	0.135	0.146	-0.011*
HRP Age 55 to 64	0.067	0.064	0.003
HRP Age over 64	0.048	0.043	0.005
Furnished	0.231	0.187	0.044***
Partial Furnished	0.189	0.171	0.018***
Unfurnished	0.579	0.642	-0.063***
Isolated	0.027	0.026	0.001
Male	0.648	0.628	0.020**
Number of rooms	4.620	4.530	0.090***
Number of bedrooms	2.38	2.35	0.03*
Log income	10.140	10.139	0.001
Non dependant in the household	0.100	0.146	-0.046***
Multiple deprivation index	0.172	0.156	0.016**
Sample Size	10,062	5,149	

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

HRP is the Household Reference Person.

Log Income in 2008 prices.

The Multiple Deprivation is a binary variable that indicates whether the HB claimant lives in the 15 % (FRS data) or 20 % (EHS data) most deprived areas.

Chapter 3

What do we really know about the employment effects of the National Minimum Wage?

3.1 Introduction

The conduct of minimum wage policy in the UK is unusual for its very formal connection to an evidence base. Each year, a body called the Low Pay Commission (LPC), which was established to advise the UK government on the setting of the NMW, commissions and funds research on the impacts of the NMW, and then uses evidence from those studies to help determine its recommendations to the government. Those recommendations are almost always adopted. A broad conclusion from this body of research has been that the introduction of the NMW and its subsequent up-ratings has not had detrimental effects on employment (Stewart, 2004a,b; Dickens & Draca, 2005; Dickens, Riley, & Wilkinson, 2012; Bryan et al., 2013), with the LPC affirming in 2016 that:

> *The Low Pay Commission is generally thought to have succeeded in its broad aims, ending extreme low pay, without damaging jobs or the wider economy. Businesses have had to respond to a higher pay floor that some have, on occasion, found uncomfortable – but the evidence set out in this and in previous reports shows that they have generally adapted well.*

In light of this evidence and the analysis of a report by the Office for Budget Responsibility (OBR, 2015)[1], in 2016 the Government introduced the National Living Wage, which rose the minimum wage rate for workers aged 25 or older by 7.5 %, and set the target of the new rate at 60 % of the median earnings by 2020.

The NMW is often cited as a good example of policy that reduces inequality with no adverse effect on employment (King, Anthony and Crewe, Ivor, 2014). However, there are reasons to question whether the evidence base on the employment impact of the UK NMW is as strong as its influence suggests. First, much of that literature is based on difference-in-difference (DiD) designs (Stewart, 2004a,b; Dickens & Draca, 2005; Dickens et al., 2012; Bryan et al., 2013; Dickens, Riley, & Wilkinson, 2015). Recent work has highlighted the challenges of conducting appropriate inference in such designs (Bertrand, Duflo, & Mullainathan, 2004; Donald & Lang, 2007; Cameron, Gelbach, & Miller, 2008) and, when inference is conducted properly, such designs may have very low power (Brewer, Crossley, & Joyce, 2013). Second, the literature on the NMW has focussed on the binary outcome of the statistical rejection of the null hypothesis, without attention to the range of employment effects that are consistent with the data. Commentators in both the social and medical sciences (such as Cohen (1994), Sterne, Smith, and Cox (2001), Ziliak and McCloskey (2004) and Ioannidis (2005)) have long noted that an excessive focus on rejecting or failing to

[1] The OBR report in July 2015 forecast a 1.1 million growth in employment by 2020 and a minimal impact on employment of the NLW estimated at only 60,000 job loss.

reject a null hypothesis can result in a very misleading interpretation of the statistical evidence. One of the recommendations of the American Statistical Association's statement is that researchers present confidence intervals, as this summarises what values of the parameters of interest would be rejected (in a statistical sense) by this data (Wasserstein and Lazar, 2016).

In this paper we re-evaluate the employment effects of the UK's NMW taking full account of these concerns. We use the two most common specifications/approaches in the literature. One estimates the impact of the NMW on transitions from employment using a DiD-style design. Examples of studies using this approach are Stewart (2004a,b), Dickens and Draca (2005), Dickens et al. (2012), Bryan et al. (2013) and Dickens et al. (2015). These studies typically estimate the impact that an up-rating of the NMW has on the transition rate from employment into non-employment by comparing outcomes for a treatment group of employees directly affected by a NMW uprating with outcomes for workers in a control group that are located slightly higher up the wage distribution. The other exploits geographical variation in the bite of the living wage that arises because the minimum wage is set for the whole of the UK. Exemplars of this approach are Stewart (2002), Dolton, Bondibene, and Wadsworth (2012) and Dolton, Bondibene, and Stops (2015). In these studies, the employment rate in a region is related to the bite of the minimum wage within that region; Dolton et al. (2015) tackle comprehensively how best to account also for the state of the regional labour market, the endogeneity of the level of the minimum wage, and spatially or temporally correlated errors.

We develop previous findings in four ways. First, we follow recent suggestions for best practice for undertaking inference in DiD designs. Second, we focus explicitly on confidence intervals, rather than reporting p-values or focusing on the binary outcome of whether the null hypothesis of "no impact of the NMW on employment" can be rejected; this means we show, given appropriate inference techniques, what magnitude of effects can be ruled out given the available data. Third, we show what the estimated coefficients mean for economically-meaningful concepts, such as elasticities of employment with respect to the minimum wage. Finally, we calculate minimum detectable effects (MDEs), following Bloom (1995), which indicate how large the true employment effects would have to be (or how large would the true labour demand elasticity have to be) for the methods employed in this literature to detect them with high probability.

The existing literature has consistently failed to reject the null hypotheses that the UK's NMW wage has no impact on employment or job retention. This study is no different (so, like the past literature, we fail to routinely find a "statistically significant effect"). However, we show that the data cannot exclude large negative (and also small positive) effects. Our preferred specification for implementing the approach, in which we follow the recent literature on inference in DiD designs and calculate the standard errors using methods designed to ensure that associated tests have the correct size, gives a 95 % confidence interval within which a 10 percent rise in the NMW could lower the job retention rate for NMW workers by as much as twenty-two percent (or reduce it by as much as 0.5 percent). Considered another way, our calculations of MDEs indicate that the tests and data employed in the literature would have only an eighty percent chance of detecting a non-zero impact of the NMW if the true effect of a ten percent increase in the NMW was to decrease the job retention rate of NMW workers by no less than sixteen percent. We also highlight that this commonly-used specification is not informative about the underlying elasticity of employment with respect to the minimum wage, because it relates employment rates to changes in, not levels of, the NMW. [2] The second empirical strategy does provide an estimate of the underlying elasticity of employment with respect to the minimum wage. Having such an estimate is important, not least because it allows

[2] We therefore think the back of the envelope calculation in the concluding section of Dickens et al. (2015) is incorrect.

for comparison with the rest of the literature. Here, we show that the power of this design is low and so minimum detectable effects are large.

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

3.2 Background

3.2.1 The minimum wage in the UK[3]

The UK's National Minimum Wage (NMW) was introduced on the 1st of April 1999, and it covers all workers who are not self-employed, regardless of industry, size of firm, occupation and region. The adult rate (for workers aged 22 or over) was set at £3.60 per hour, the rate for workers aged between 18 and 21 was £3.00, whereas a "trainee" level for adults who received an accredited training in the first six months of a new work was set at £3.20. Apprentices, workers on a government scheme under age 19 and young workers aged 16-17 were initially exempt. A year before the introduction of the NMW, the coverage of the adult rate was around 5.6% and the youth rate was estimated to affect 8.2% workers aged 18-21. [4] After a month from the introduction of the NMW, the vast majority of employers were meeting their obligation to pay the proper hourly rate (LPC Report 2000).

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

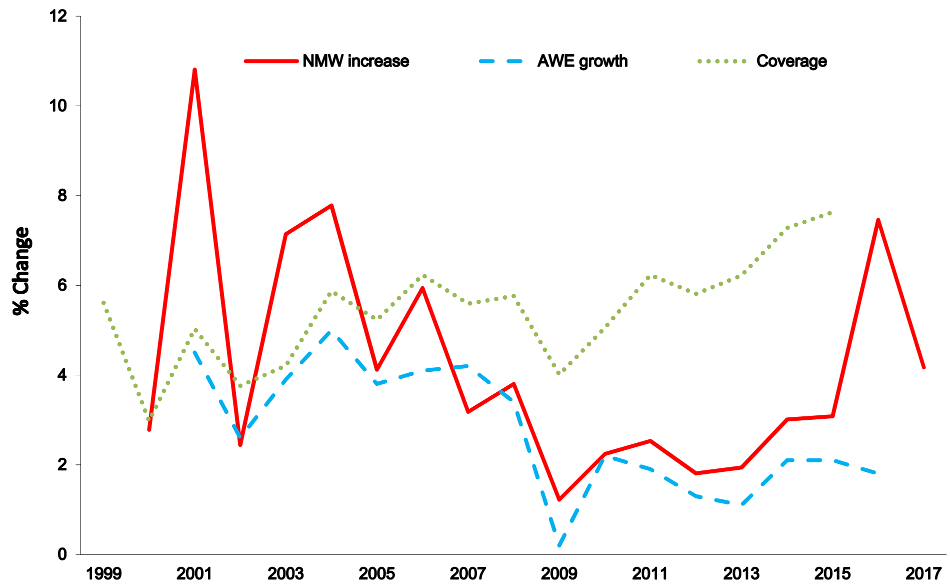
[4]Coverage of the NMW is defined as the proportion of employees in working age paid below the NMW in April preceding the NMW review. Coverage estimates are calculated using Annual Survey of Hours and Earnings (ASHE) data. ASHE is conducted in April of each year.

Table 3.1: The UK National minimum wage for adults

Date	**Adult rate**	**NMW % change**	**Coverage**	**AWE growth**	**Inflation**
Apr 1999	£3.60	-	5.61	-	-
Oct 2000	£3.70	2.78	2.98	-	1.2
Oct 2001	£4.10	10.81	5.03	4.5	1.1
Oct 2002	£4.20	2.44	3.76	2.7	1.4
Oct 2003	£4.50	7.14	4.21	4.0	1.5
Oct 2004	£4.85	7.78	5.87	5.3	1.2
Oct 2005	£5.05	4.12	5.23	4.0	2.3
Oct 2006	£5.35	5.94	6.23	4.3	2.4
Oct 2007	£5.52	3.18	5.59	4.4	2.1
Oct 2008	£5.73	3.80	5.76	3.5	4.4
Oct 2009	£5.80	1.22	4.01	0.2	1.5
Oct 2010	£5.93	2.24	5.05	2.0	3.2
Oct 2011	£6.08	2.53	6.23	2.2	5.0
Oct 2012	£6.19	1.81	5.81	1.1	2.6
Oct 2013	£6.31	1.94	6.22	1.1	2.2
Oct 2014	£6.50	3.01	7.28	2.1	1.3
Oct 2015	£6.70	3.08	7.63	2.1	-0.1
Apr 2016	£7.20	7.46	-	1.8	-0.1
Apr 2017	£7.50	4.17	-	-	-

The adult rate refers to workers aged 22 and over until 2009 and aged 21 and over afterwards. In April 2016 the National Living Wage replaced the National Minimum Wage for workers aged 25 and over. NMW change, Coverage, AWE growth and Inflation are expressed as percentages. Earnings in ASHE are recorded in April every year. Coverage in 1999 is relative to earnings recorded in April 1998.
Sources: Low Pay Commission, Annual Survey of Hours and Earnings (ASHE), CPI series D7BT (ONS), Average Weekly Earnings (ONS) series KAB9.

Figure 3.1: Annual growth rates of the UK National Minimum Wage, 2000 to 2015.



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

The level of the NMW is reviewed annually (and we describe this process in more detail below), with the government announcing each Spring the rate that will apply from the following October. Table 3.1 shows the history of the NMW up-ratings. In the first years after its introduction, the government announced sizeable increases in the NMW: the adult rate rose by nearly 11% in October 2001, and by between 7 and 8% in both 2003 and 2004, at a time when Average Weekly Earnings (AWE) increased by just 4 to 5%. Since the onset of the financial crisis and subsequent downturn, although upratings have not always kept pace with inflation, they have largely outpaced growth in average earnings. Coverage of the adult NMW rate has been most of the years between 4 and 6% of the employees in working age with a slightly increasing trend over time.

In April 2016 the government applied a new rate to workers aged 25 and above, increasing the minimum hourly wage by 7.5%. With this change, the NMW was relabelled as the National Living Wage (NLW) and the government announced a target of setting the NLW at 60% of median earnings by 2020 (LPC Report 2016). These are much larger changes than those between 1999 and 2016 but the decision of the NLW introduction was taken on the basis of the evidence that the NLW would not harm employment and partly to limit the Work Tax Credit outlays.

3.2.2 Research on the impact of the National Minimum Wage

A considerable number of studies have examined whether the NMW has affected employment. Typically, these have made use either of the period just before and after the introduction of the minimum wage, or the variations over time in the level of the minimum wage caused by the annual changes shown in Table 3.1. Two main approaches have been commonly employed in this research. The first approach uses individual panel data and estimates the effect of a change in the NMW on job retention.[5] The second strategy relies on the regional variation of the NMW impact on the wage distribution and estimates the effect of the minimum wage on employment rate.

The studies that examine the impact of the NMW on job retention use a Difference-in-Differences (DiD) design, typically comparing workers whose wage is increased to comply with the NMW with an unaffected group of workers with higher wages. This is done because it allows researchers to identify a group of employees who will be directly affected by a change in the NMW; the main alternative to this is to look at employment rates amongst groups of individuals likely to be affected by the minimum wage were they to work, such as young low-skilled adults.[6] For example,

[5]Job retention is defined as the probability of making a transition out of employment

[6]Machin, Manning, and Rahman (2003) and Machin and Wilson (2004) examine the impact

Stewart (2004b) assesses the impact of the introduction of the NMW on job retention with a DiD design, comparing job retention probabilities of the group who have an initial wage below the 1999 NMW (the treatment group) with a group who earn a slightly higher wage (the control group). He used three different data sources - the Labour Force Survey Data (LFS), the British Household Panel Survey Data (BHPS) and the New Earnings Survey (NES), now known as the Annual Survey of Hours and Earnings (ASHE) - all with their own strengths and weaknesses. Stewart reports mostly positive, but statistically insignificant, effects of the NMW on job retention across all data-sets; only for women, and only in some specifications, does he find disemployment effects of the NMW, and these are statistically insignificant. A companion paper, Stewart (2004a), extends this analysis to the 2000 and 2001 upratings. Again, the study does not find any statistically significant effect of the two subsequent upratings on job retention probabilities.

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

Bryan et al. (2013) provide one of the most comprehensive assessments, as the authors estimate the effect on job retention of each of the NMW upratings from 2000 to 2011. They extend the previous DiD design by not only comparing individuals who

of the NMW on workers employed in residential or nursing care homes, a sector with very high incidence of low rates of pay. They find some evidence of disemployment effects.

were directly affected by a NMW increase to those who earned slightly more (as in Stewart (2004b)), but also comparing job retention rates over periods that do, and do not, span the annual October increase in the NMW: the idea is that retention rates measured over a period that spans October are potentially affected by the NMW uprating, and retention rates measured over a period that does not span October are not affected by any NMW uprating. They find a statistically significant detrimental effect for the 2001 uprating - corresponding to the largest year-on-year increase observed to date, with a rise of over 10% - for men. For some specifications, they also obtain positive coefficients for the 2006 and the 2011 upratings, but the statistical significance of these is not robust across specifications.

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

The second of the two approaches focuses on the impact of the NMW on employment rate and exploits the fact that the minimum wage is set at the national level, but the wage distribution varies across local areas. This strategy assumes that the bite of the minimum wage on employment is larger in areas where the increase of the NMW has an important impact on the wage distribution relative to areas where only few workers are affected by a change in the minimum wage.

In the UK the first exemplar of this type of studies is Stewart (2002). In his paper he reformulates the model proposed in Card (1992) and estimates a reduced form equation that derives from a structural model of the labour demand. Local area employment changes are explained by the wage variation due to the different impact of the introduction of the NMW across local areas. Stewart (2002) uses the share of workers paid below the minimum wage rate in a local area as an instrument for the endogenous wage variation. He reports positive and negative employment elasticities but none of them is statistically significant at conventional levels. In a second specification, Stewart (2002) uses an indicator for areas with the highest share of workers paid below the NMW as alternative instrument for the endogenous wage variation but findings do not lead to different conclusions.

Dolton et al. (2012) examine the effect of the minimum wage on employment with an incremental Difference-in Differences design. They compare employment in a period when the NMW was in place with employment before the NMW introduction regressing the employment rate at local area level on the Kaitz Index -a measure of the bite of the minimum wage on the wage distribution in the local area- area fixed effects and other area characteristics. The Kaitz Index is also interacted with an indicator for the annual NMW uprating allowing for an effect of the minimum wage that changes over time. Interestingly, Dolton et al. (2012) find positive and statistically significant effects of the minimum wage over the years 2004-2006.

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

The research summarised above has had an unusually large influence on policy because of the institutional structure around the NMW, and the particular role played by a body known as the Low Pay Commission (LPC). The LPC is a statutory body, independent of government, and exists to advise the government about policy towards the NMW (it is not responsible for enforcement). As discussed earlier, the decisions about the level of the NMW are made on an annual cycle. The LPC produces a set of recommendations each year (usually in February), including a recommendation on by how much the NMW should increase, and the government makes its decision later in the Spring, with the new NMW rate applying from the following October. [7] Although the government is not obliged to accept its recommendations, successive UK governments have, since 1999, mostly followed the LPC's advice on by how much to increase the NMW in each year.

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

[7] In April 2016 the UK government introduced the National Living Wage, the minimum hourly rate for workers aged 25 and over. Since then the NLW and the other rates for young workers are reviewed in April.

evidence-driven." (LPC Report 2007).

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

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

In 2006, the LPC said that

> *since its introduction in 1999 the minimum wage has been a major success. It has significantly improved the wages of many low earners; it has helped improve the earnings of many low-income families; and it has played a major role in narrowing the gender pay gap. But it has achieved this without significant adverse effects on business or employment creation.* (LPC Report 2006).

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

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

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

3.3 Data and Models

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

We proceed by estimating a model very similar to that in Bryan et al. (2013), one of the most recent and comprehensive studies on the impact of the NMW on employment, and commissioned by the LPC for their 2013 report. Our identification strategy and data are the same as in Bryan et al. (2013), but we depart from that study's approach - and that of previous studies cited in Section 2 - in four ways. First, we follow recent suggestions for best practice for undertaking inference in difference-in-difference designs, and show that the standard errors estimated in (some of the) previous UK literature - although we do not make this claim about Dolton et al. (2015) - are downward-biased. Second, we focus explicitly on confidence intervals, rather than reporting p-values or focusing on the binary outcome of whether the null hypothesis of no impact of the NMW on employment can be rejected. Third, we show what the estimated coefficients mean for economically-meaningful concepts such as labour demand elasticities. Finally, we calculate minimum detectable effects (Bloom, 1995), which indicate how large the true employment effects would have to be (or how large would the true labour demand elasticity have to be) for the methods employed in this literature to detect them with high probability.

3.3.1 Two approaches for assessing the impact of the NMW on employment

In this paper we re-evaluate the employment effects of the UK's NMW taking full account of these concerns, and using the two most common specifications/approaches in the literature. One estimates the impact of the NMW on transitions from employment using a DiD-style design, and the other exploits geographical variation in the bite of the minimum wage. We describe these two approaches below.

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

Studies following this approach typically estimate the impact that an uprating of the NMW has on the transition rate from employment into non-employment by comparing outcomes for a treatment group of employees directly affected by a NMW uprating with outcomes for workers in a control group that are located slightly higher up the wage distribution. Examples of studies using this approach are Stewart (2004a,b), Dickens and Draca (2005) and Dickens et al. (2012), Bryan et al. (2013), Dickens et al. (2015).

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

The model is estimated on data from the Labour Force Survey (LFS), which is comparable to the Current Population Survey (CPS) in the US, and collects information on employment status and other issues for a sample of the UK population. Individuals in the LFS are surveyed in five consecutive quarters, but, as information on earnings is asked only in the first and in the last interview and we need to measure earnings at the beginning of the period over which we measure employment transitions, the outcome measure is an individual's transition from employment over a 6 month period using the first and the third observation for each individual. These 6-month intervals either do or do not straddle a NMW increase on 1 October; this is denoted with s. The maintained assumption by the literature is that retention rates measured over a period that spans 1 October are potentially affected by a NMW uprating, and retention rates measured over a period that does not span 1 October are not affected by any NMW uprating.[8] Individuals are allocated into one of four different groups, g, according to the starting wage: the treatment group is composed of workers who earn a wage w_{it} between the existent NMW enforced in year t and the upcoming year $t+1$ NMW uprating ($NMW_t \leq w_{it} < NMW_{t+1}$), and individuals in the control group have a salary that is slightly higher than the upcoming year $t+1$ NMW ($NMW_{t+1} \leq w_{it} < m(NMW_{t+1})$; we set $m = 1.1$, so workers in our control group earn up to 10% more than the year t upcoming NMW up-rating. There is also a Below NMW group that contains people who report an hourly wage w_{it} below the existent NMW ($w_{it} < NMW_t$), and an Above NMW group that is made up of workers paid more than m times the upcoming NMW ($w_{it} \geq m(NMW_{t+1})$). The specification allows their wage to be affected by changes to the NMW, but the impacts on these groups is allowed to be different from that on the treatment group.

The equation that is estimated is:

$$y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt} d_{gs} + \mathbf{x}'_{igts}\gamma + \epsilon_{igts} \tag{3.1}$$

$$i = 1, ..., N; g = C, B, T, A; s = 0, 1; t = 2000, ..., 2011$$

where y_{igts} is a dummy variable that records whether individuals in work at time t are also in work 6 months later, g subscripts the 4 groups defined according to the individual's initial wage, s denotes whether the 6-month intervals straddle a NMW increase on 1 October, δ_{ts} is an interaction of the year and whether or not the transition straddles a NMW increase, α_{gt} is a time-varying group effect, d_{gs} is a binary policy variable that denotes whether the observation is affected by a minimum wage

[8] As NMW increases happened always on 1 October in the period we consider, 6-month transitions from Q1 to Q3 and from Q4 to Q2 do not span an uprating, and transitions from Q2 to Q4 and from Q3 to Q1 do.

uprating (this varies by group g and whether the transition spans a NMW increase on 1 October, s), and $\boldsymbol{x}$ are individual-level control variables. β_{Tt} can be interpreted as the impact on job retention for the treatment group of the year t NMW up-rating effect.

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

$$y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt} d_{gs} \omega_t + \mathbf{x}'_{igts} \gamma + \epsilon_{igts} \tag{3.2}$$

$$i = 1, ..., N; g = C, B, T, A; s = 0, 1; t = 2000, ..., 2011$$

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

We replicate the sample and point estimates described in Bryan et al. (2013), but we estimate different standard errors, following concerns raised in the literature about the accuracy of the inference in DiD designs when using the naïve estimates of the standard errors provided by OLS. The first concern, dating back to Moulton (1990), relates to the grouped error structure. In DiD designs, the error term ϵ_{igts} in Equation 3.1 is unlikely to be i.i.d., because an individual may have unobservable characteristics that are correlated with other individuals of the same group, or may be affected by common group shocks. In the case of these studies of the minimum wage, members of the treatment group are all located at the bottom of the wage distribution, and so it is highly plausible that they may have some common unobservable characteristics (low ability, low skills, etc.) or are influenced by the same economic shocks. As far as we have been able to work out, none of the research cited in Section 2 addresses this issue: most studies use heteroscedasticity-robust standard errors, but do not allow for any dependence between different individuals. A second concern, as initially noted by Bertrand et al. (2004), is that the level of uncertainty surrounding the estimated policy effect in DiD designs will likely be increased by positive serial correlation in the group-time shocks, as the variable of interest in DiD designs is itself highly serially-correlated. We describe our approach in further details in Appendix C1.

An approach that is commonly used to calculate standard errors that account for the common group structure in the error term is the Donald and Lang (2007) two-step estimator. When we calculate the standard errors with the Donald and Lang (2007) approach equations 3.1 and 3.2 are exactly identified. This requires to place restrictions in the two models and we constraint the impact of the NMW for the treatment group on job retention β_T to be time-invariant (further details in Appendix C1). The constrained version of the model that estimates the impact of a NMW uprating on job retention is

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs} + \mathbf{x}'_{igts}\gamma + \epsilon_{igts} \tag{3.3}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

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

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs}\omega_t + \mathbf{x}'_{igst}\gamma + \epsilon_{igst} \tag{3.4}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

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

The second approach arises because the minimum wage is set for the whole of the UK but the impact or as commonly named in this literature the "bite" of the NMW on the wage distribution varies across geographic areas of the country (Stewart, 2002; Dolton et al., 2012; Dolton et al. 2015). This approach is based on the idea that a larger effect of the NMW on employment is expected in areas where a change in the minimum wage significantly affects the wage distribution compared to more thriving areas where fewer workers earn the minimum rate.

We follow Dolton et al. (2015), the most recent study of this strand with a compelling econometric strategy that accounts for the state of the regional labour market, the endogeneity of the level of the minimum wage and spatially or temporally correlated errors. The effect of the NMW on employment rate at local area level [9] is estimated applying an incremental DiD design that compares a period with annual increases of the minimum wage to a period prior to the introduction of the NMW. The variable that measures the bite of the minimum wage on employment of a local area j is the Kaitz index K_{jt}, the ratio of the NMW to the median wage of the local area j. [10] If the index is nearly 1, the NMW has a strong impact on the wage distribution and possibly on employment of the local area j. Conversely, a small value of the index is an indicator that the wage distribution doesn't significantly vary with the NMW.

To calculate the employment rate at local area level and the Kaitz index, Dolton et al. (2015) use two sources of data: the employment rate is derived from the LFS and the Kaitz index is calculated using data from the Annual Survey of Hours and Earnings

[9] Dolton et al. (2015) report two distinct definitions of local area, the 140 unitary authorities and counties and the 138 travel-to-work areas. Our findings are derived from Dolton et al. (2015)'s estimates that use travel-to-work as definition of local area. Our conclusions do not change when local areas are the unitary authorities and counties.

[10] The Kaitz index in the years preceding the introduction of the NMW is calculated deflating the 1999 NMW by the wage inflation.

(ASHE). The model that estimates the relationship between the two variables is

$$E_{jt} = \pi_0 + A_j + \pi_t \sum_{k=1999}^{t} I_k + \theta_0 K_{jt} + \theta_t \sum_{k=1999}^{t} I_k K_{jt} + \mathbf{x}_{jt}^{'}\rho + \epsilon_{jt} \qquad (3.5)$$

$$t = 1997, ..., 2010; j = 1, ..., 138$$

where E_{jt} is the log of the employment rate in the local area j at time t, A_j is the area fixed effect, I_k is a set of year dummies from the introduction of the NMW in 1999, x_{jt} are a set of time-varying area characteristics. To overcome any spurious correlation between the business cycle and the NMW, the model controls for a measure of the regional aggregate demand at a geographic level broader than the local area defined for employment and Kaitz index. Two parameters identify the effect of the NMW: θ_0 captures the time-invariant effect of the existence of a minimum rate and θ_t picks up the additional effect of the annual uprating on employment rate.
The model allows for correlation between error terms of different local areas. This is to account for spatial correlation generated by commuting patterns and common economic shocks that influence contiguous areas. The error ϵ_{jt} is a weighted average of contemporaneous errors of the other local areas: [11]

$$\epsilon_{jt} = \lambda \sum_{i=1}^{138} w_{ij}\epsilon_{it} + \nu_{jt}$$

$$t = 1997, ..., 2010; j = 1, ..., 138; i = 1, ..., 138; i \neq j$$

where weights w_{ij} measure the strength of the economic relationship between local areas.

A second specification addresses the possible endogeneity of the Kaitz index adding a dynamic component. The minimum wage and accordingly the Kaitz index are endogenous whether the UK government sets the minimum rate according to past

[11] Spatial Error Model (SEMP, Elhorst, 2010)

shocks of employment or any omitted variable correlated with employment. The dynamic model attempts to overcome this issue controlling for the lag of the employment rate at local area level E_{jt-1}. The formal equation of the dynamic model is

$$E_{jt} = \gamma E_{jt-1} + \pi_0 + A_j + \pi_t \sum_{k=1999}^{t} I_k + \theta_0 K_{jt} + \theta_t \sum_{k=1999}^{t} I_k K_{jt} + \mathbf{x}_{jt}^{'}\rho + \epsilon_{jt} \qquad (3.6)$$

$$t = 1997, ..., 2010; j = 1, ..., 138$$

The model is estimated with the System of Generalized Method of Moments (SGMM) to cope with the common issue in the dynamic models of the endogeneity arised by the inclusion of the lag dependent variable in the regressors (Holtz-Eakin et al., 1988; Arellano and Bond, 1991; Arellano and Bover, 1995). The SGMM estimator uses lags of levels and differences of the endogenous variables as instruments. Dolton et al. (2015) consider lags of levels and differences of employment rate and some area characteristics as pre-determined instruments and obtain robust estimates and correct standard errors implementing a two step SGMM procedure with the Windmeijer correction (Windmeijer, 2005).

3.3.2 Minimum detectable effects

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

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

where $\hat{se}(\hat{\beta})$ is the estimated standard error for the coefficient $\hat{\beta}$, c_u is the critical value of the $(1-\alpha/2)$-th percentile of the t_{C-1} distribution and p^t_{1-x} is the $(1-x)$th percentile of the t-statistic under the null hypothesis of no treatment effect. The formula makes clear that either large standard errors, or a "high" threshold for determining statistical significance (i.e. a low value of α) both lead to large MDEs.[12]

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

Studies of the minimum wage effect on employment implement different approaches and report estimates that are rarely comparable. To facilitate comparisons between studies we translate the estimated coefficients from Equations 3.1-3.6 into their implied elasticities.

For the Dolton et al. (2015), this is straightforward: because equation 3.5 relate log-employment to the Kaitz index K_{jt}, the resulting employment elasticity with respect to the minimum wage at time t calculated at the median wage in the static model (derivation in Appendix C2.1) is

$$\bar{\eta}_{ER(t)} = (\theta_0 + \theta_t)\bar{K}_t \tag{3.7}$$

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

In the dynamic model the employment elasticity (derivation in Appendix C2.2) is

$$\bar{\eta}_{ER(t)} = \frac{\theta_0 + \theta_t}{(1-\gamma)} \cdot \bar{K}_t \tag{3.8}$$

The employment elasticity in the dynamic model differs from the static model for a scale factor $1-\gamma$ - a measure of how much past employment affects current employment. A strong relation between current employment and previous employment implies a larger employment elasticity.

For the Bryan et al. (2013) method, we turn our estimates of the impact of the NMW on 6-month retention rates β_T into an estimate of the elasticity of the 6-month job retention rate to the NMW. This elasticity, η_{JR}, is defined in the usual way:

$$\eta_{JR} = \Delta RR/RR/\Delta NMW/NMW \tag{3.9}$$

For the model in which we estimate the average impact of a NMW up-rating, then ΔRR is the coefficient β_T (i.e. the change in the retention rate for the treatment group thanks to an increase in the NMW), RR is the counterfactual retention rate (i.e. the proportion of workers who would have remained in employment if the NMW had not been changed, which we can calculate as the observed retention rate less β_T)[13], and $\Delta NMW/NMW$ is 0.049, the average size of the NMW upratings in the 2000-2010 period. [14]

A considerable drawback of the specifications in equations 3.1-3.4 , though, is that one cannot infer from them what is the underlying relationship between the level of the NMW and the level of the employment rate (or even the retention rate). To calculate an elasticity of employment with respect to the wage or to the minimum

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

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

wage, one needs to know the shape of the function that relates the level of employment to the level of minimum wages. Instead, equations 3.2 and 3.3 relate the level of employment to the rate of change of the minimum wage. [15] Another way of seeing this is to consider a sequence of 4 annual changes to the minimum wage of 0%, +25%, +25% and 0%. If the predicted retention rate in the second year, when $\omega_t = 0$ is E^*, then the predicted retention rates in the following 3 years would be E^*, $E^* - 0.25\beta_T$, $E^* - 0.25\beta_T$, E^*. In other words, the equation would predict that the retention rate would fall while the minimum wage was rising, but then would return to its original level even though the minimum wage was over 56% higher.

3.4 Results

We revisit two papers that are representative of the UK literature on the impact of the minimum wage on employment. We first present the estimates of the NMW effect on job retention using the modified DID-style specification of Bryan et al. (2013) applying the best practices for inference in DID designs and comparing them with standard inference. We report confidence intervals and MDEs and translate the estimates into job retention elasticities providing a benchmark with the rest of the literature. Finally, we calculate employment elasticities for the estimates in Dolton et al. (2015), a study that examines the impact of the minimum wage on employment transitions using geographical variation in its bite.

[15] It would be possible to write down a version of 3.2 that included NMW_t as an additional regressor, but it would not be possible to identify the coefficient on such a variable, as it would be collinear with the time effects.

3.4.1 The estimated impact on a NMW uprating on job retention

Table 3.2 shows estimates of the (weighted) average impact of an NMW uprating on the probability of remaining employed. We report estimates of β_T from equation 3.3 for men in the top panel and for women in the bottom panel. In the first line we conduct inference using the Donald and Lang (2007) two-step estimator; in the other lines we present estimates of β_T that come from estimating equation 3.3 with OLS and calculating heteroscedasticity- and cluster-robust standard errors and standard errors with no correction.

Table 3.2: Estimates of Average Impact of a NMW Uprating on Job Retention

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

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.01$

Control variables are age, gender, marital status, highest level of education, region, ethnicity, number of children, age left education, and whether the respondent has health problems, industry, public sector, occupation, and tenure. Two-step estimates are from equation (3.14); OLS estimates are from equation (3.3)

OLS estimates on the micro-data suggest that a NMW uprating increases the probability of remaining employed. When the Donald and Lang (2007) two-step estimator is implemented (and controls are included in the specification), then the estimated impact of an NMW uprating is to increase the job retention rate by 0.4 percentage points for men and cut it by 0.1 percentage points for women. [16] [17]

None of the estimates is statistically different from zero: like the previous UK literature, we fail to find an impact of the NMW on the probability of remaining employed. But it is extremely instructive to look at the confidence intervals associated with our estimates. These reveal two things. First, the Donald and Lang (2007) two-step standard errors are more than twice as large as the OLS standard errors for women, and 88% larger for men: this is consistent with our belief that within-cell correlation in the error terms is an important issue in the DiD. Second, the confidence intervals in Table 3.2 reveal that large positive and negative impacts of a NMW uprating on employment would also not fail to be rejected by this data at a significance level $\alpha = 0.05$. For example, we cannot reject that an average NMW uprating reduces the probability of remaining employed by 4.7 percentage points for men, or that it increases the job retention rate by 5.6 percentage points. These confidence intervals are wide, and illustrate that the data and the research design are not especially helpful in allowing us to make inferences about the existence of a negative impact of the NMW on job retention. The corollary of these large standard errors is that the DiD designs typically used in the UK NMW studies has a low power to detect a plausibly-sized true NMW effect. Our calculations of the MDEs show that an NMW uprating would need to decrease (increase) job retention rate for men by 7.3 percentage points to have an 80 % chance of being detected, and by 5.0 percentage points for women.

[16] These point estimates of β_T are slightly different under the two methods because the coefficient β_T represents the weighted average of the impact all of the NMW upratings on job retention rates, and the effective weights in this calculation are different when using OLS on the micro-data in equation 3.3, and when using OLS on cell-level averages in equation 3.12 (see Appendix C1).

[17] The point estimates in Table 3.2 do not correspond to the results presented in Bryan et al. (2013). As discussed in Appendix C1, the model estimated in Bryan et al. (2013), corresponding to our equation 3.1, is exactly identified under the two-step approach. However, we are able to replicate results in Bryan et al. (2013) to the 2nd decimal point when we also estimate equation 3.1 with OLS, and calculate heteroscedasticity-robust standard errors: see Table C3 in Appendix C3.

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

The specification in equation 3.3 that estimates the average impact of a NMW uprating does not take into account that the size of the up-ratings has varied over time (see Table 3.1). Table 3.3 therefore presents estimates of the impact of a 1% rise in the NMW on transitions from employment. We present estimates from equation 3.2 using OLS and the two-step estimator. The point estimates are that a NMW increase lowers the probability of remaining employed for both men and women, with a 10 % growth in the minimum wage estimated to reduce the job retention rates by as much as 10 percentage points for men, and by 1 percentage point for women. Since the average up-rating size over 2000-2011 is 4.9%, the annual review of the NMW reduces on average job retention by 4.9 percentage points for men and by 0.4 percentage points for women.

Table 3.3: Estimates of Impact of a 1% rise in the NMW on Job Retention

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

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.01$

Control variables are age, gender, marital status, highest level of education, region, ethnicity, number of children, age left education, and whether the respondent has health problems, industry, public sector, occupation, and tenure. Two-step estimates are from equation (3.15); OLS estimates are from equation (3.2) and are associated to heteroscedasticity-robust standard errors. NMW in 2000 prices.

As in Table 3.2, the size of the standard errors sharply increases when the Donald and Lang (2007) two-step estimator is implemented: in the specification that includes controls, the standard error under the two-step is 87% larger than the OLS standard errors for men, and 127% larger for women. However, the point estimate for men is large enough to be statistically significant at 5 % significance level. In addition, the large standard errors obtained through the Donald and Lang (2007) two-step estimator lead to wide confidence intervals: the range of impacts that cannot be rejected includes that the job retention rate might decline by 20 percentage points, or be close to no effect, in response to a 10 % NMW rise. The estimated MDEs in column (5) indicate that one would need a true effect on the probability of remaining employed of about 14 percentage points for men, and 9 percentage points for women, in response to a 10 % NMW increase to have 80 % probability of detecting it.

3.4.3 Implied elasticities of job retention and elasticity of employment

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

In Tables 3.4, we report the elasticities of job retention to the NMW that are implied by our estimated coefficients in (respectively) Table 3.2 and 3.3, using the formula in equation 3.9. The elasticities implied by the point estimates for β_T are in the range of 0.11 and -1.15 for men, and -0.02 and -0.09 for women depending on the specification. The confidence intervals for these elasticities include large positive and negative elasticities, especially for men. As a result, the estimated MDEs are very large. Even if we take the smaller of the 2 MDEs in Table 3.4, our estimates imply that this DiD design would detect a true effect with 80% probability only if the true job retention elasticity was greater than 1.6 for men, and greater than 1 for women.

Table 3.4: Implied elasticities of job retention with respect to the minimum wage, η_{JR}

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

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.01$
Elasticities calculated using equation (3.9). Standard Errors calculated using the delta method.

For comparison, Table 3.5 reports the range of job retention elasticities observed in the minimum wage literature that uses individual longitudinal data from the US and Canada. Unlike some studies that have used aggregate data and find small *insignificant* positive effects of the minimum wage on employment (e.g. Card, 1992; Card and Kruger, 1994, 1995, 2000), studies using individual longitudinal data tend to find that the minimum wage has a *significant* detrimental impact on job retention rates for workers likely to be affected by a minimum wage hike (e.g. young people).

Table 3.5: Estimated elasticities of job retention with respect to the minimum wage, η_{JR}, from studies using US and Canadian data

Study	Country	Group	η_{JR}
Currie and Fallick (1996)	USA	Workers affected by MW	-0.19 to -0.24
Yuen (2003)	CAN	Teenagers	-0.75 to -0.84
		Young Adults	-1.23 to -1.77
Neumark and Wascher (2004)	USA	Teenagers	-0.12 to -0.17
Campolieti, Fang, and Gunderson (2005)	CAN	Young Adults	-0.33 to -0.54
Sabia, Burkhauser, and Hansen (2012)	USA	Young Adults	-0.7

Our estimate of β_T from equation 3.4 implies a job retention elasticity of -1.15 for adult men, this is statistically significant at 5 % level. Our finding is close to Yuen (2003), and larger than what Currie and Fallick (1996), Neumark, Schweitzer, and

Wascher (2004), Campolieti et al. (2005) and Sabia et al. (2012) obtain for teenagers and youths. The minimum wage literature in the US and Canada that uses individual longitudinal data does not apply techniques to account for within-cell shocks or serial correlation in the treatment status. Despite of following the best practices to conduct inference in DiD designs, we still find a statistically significant job retention elasticity that strengthens our idea that research should be cautious to draw the conclusion that the NMW has no detrimental effect on employment.

We now convert Dolton et al. (2015)'s point estimates of the effect of the NMW on employment into elasticities and calculate standard errors with the delta method (see Appendix C2.1-C2.2). Compared to the models of the transition from employment 3.3 and 3.4, the specifications in Dolton et al. (2015) allow for a time-varying impact of the minimum wage, so we derive the employment elasticity for every year. Table 3.6 presents the employment elasticities using the estimates derived in the static model of equation 3.5. All the elasticities are negative and imply a reduction of the employment rate that varies between 0.17 % and 0.83 % in response to a 10 % increase in the minimum wage.

Although these employment elasticities are relatively small, standard errors are quite precise and we cannot reject the null hypothesis of no effect at the 5 % level in three cases (years 2001, 2006, 2009). And again, for the smallest (in magnitude) of our employment elasticities in Table 3.6, the confidence intervals cannot exclude that the employment rate declines by 0.8 % or increases by 0.5 % with a 10 % rise in the minimum wage. The MDEs for the employment elasticities reflect the relatively small standard errors. The minimum size of the employment elasticity we would have 80 % power to detect is around ± 0.1 that corresponds to a 1 % change in the employment in response to a 10 % increase in the NMW.

Table 3.7 reports the employment elasticities derived from the dynamic model 3.6.

Table 3.6: Employment elasticity with respect to the the year t minimum wage uprating. Estimates of $\bar{\eta}_{ER(t)}$ derived from the static model of equation 3.5.

t	$\bar{\eta}_{ER(t)}$	$s_{\bar{\eta}_{ER(t)}}$	**C.I. at 95 %**		**MDE**
1999	-0.054*	0.032	(-0.117	0.009)	±0.091
2000	-0.041	0.032	(-0.103	0.022)	±0.091
2001	-0.074**	0.033	(-0.140	-0.008)	±0.095
2002	-0.055*	0.033	(-0.120	0.009)	±0.094
2003	-0.023	0.034	(-0.090	0.045)	±0.098
2004	-0.017	0.036	(-0.087	0.053)	±0.102
2005	-0.054	0.037	(-0.127	0.018)	±0.105
2006	-0.077**	0.038	(-0.152	-0.003)	±0.108
2007	-0.049	0.037	(-0.122	0.024)	±0.106
2008	-0.035	0.037	(-0.107	0.038)	±0.105
2009	-0.083**	0.036	(-0.155	-0.012)	±0.103
2010	-0.051	0.037	(-0.124	0.023)	±0.106

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.001$
Standard errors for the employment elasticity are calculated as $s_{\bar{\eta}_{ER(t)}} = \bar{K}_t \cdot \sqrt{\hat{\sigma}^2_{\theta_0} + \hat{\sigma}^2_{\theta_t}}$, assuming $\sigma_{\theta_0 \theta_t} = 0$

Compared to the static model this specification accounts for the autoregressive structure of employment. Although the employment elasticities are small in the range of -0.086 and no effect they are never statistically significant at conventional levels. This is because standard errors are less precise in the dynamic model than in the static model and produce wider confidence intervals. Taking the estimate with the lowest standard error (i.e. 2000), the confidence interval includes a range of employment elasticities that include a reduction by 1.4 % or a rise by 0.8 % in the employment rate with a 10 % growth in the NMW. MDEs are quite large and indicate that we would detect a true effect with 80 % probability only if the true employment elasticity was greater than 0.15.

A note of caution should be done regarding the interpretation of the confidence intervals and MDEs in Table 3.6 and Table 3.7. We calculate standard errors assuming that the parameters involved in the estimation of the elasticities are uncorrelated. In Appendix C2.1 and C2.2 we relax this assumption and set several degrees of correlation between the parameters to calculate standard errors, confidence intervals and MDEs of the elasticity in 2001, one of the years in which we found a statistically significant elasticity at conventional level. Regardless of the degree of correlation the 2001 elasticity derived from the static model is always statistically significant at 10 % level. In the dynamic model we do not reject the null hypothesis of no effect in most of the cases but the confidence intervals can be very large.

In Table 3.8 we report the range of the employment elasticities recovered in studies of the USA literature on the minimum wage. Despite the considerable number of studies, the debate on the employment effect of the minimum wage has not reached an unanimous consensus. Some scholars find employment elasticities in the range of [-0.1; -0.3] as in the series of studies reported in the earliest minimum wage review (Brown, Gilroy, & Kohen, 1983), others point out that some specifications show no

Table 3.7: Employment elasticity with respect to the the year t minimum wage uprating. Estimates of $\bar{\eta}_{ER(t)}$ derived from the dynamic model in equation C2.2.

t	$\bar{\eta}_{ER(t)}$	$s_{\bar{\eta}_{ER(t)}}$	**C.I. at 95 %**		**MDE**
1999	-0.019	0.056	(-0.129	0.091)	±0.159
2000	-0.029	0.054	(-0.136	0.077)	±0.154
2001	-0.082	0.059	(-0.197	0.033)	±0.166
2002	-0.059	0.057	(-0.170	0.051)	±0.160
2003	-0.016	0.063	(-0.140	0.108)	±0.179
2004	-0.023	0.070	(-0.160	0.114)	±0.198
2005	-0.032	0.065	(-0.159	0.095)	±0.184
2006	-0.063	0.068	(-0.196	0.071)	±0.193
2007	-0.029	0.070	(-0.167	0.108)	±0.199
2008	0.000	0.065	(-0.128	0.128)	±0.185
2009	-0.086	0.069	(-0.221	0.049)	±0.195
2010	-0.030	0.073	(-0.174	0.113)	±0.208

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

Standard errors for the employment elasticity are calculated as $s_{\bar{\eta}_{ER(t)}} = \frac{\bar{K}_t}{1-\hat{\gamma}} \cdot \sqrt{\hat{\sigma}^2_{\theta_0} + \hat{\sigma}^2_{\theta_t} + (\frac{\hat{\theta}_0+\hat{\theta}_t}{1-\hat{\gamma}})^2 \hat{\sigma}^2_{\gamma}}$, assuming $\sigma_{\theta_0\theta_t} = 0$, $\sigma_{\theta_0\gamma} = 0$ and $\sigma_{\theta_t\gamma} = 0$

detrimental effect of the minimum wage on employment. The elasticities derived from Dolton et al. (2015) posit between -0.1 and no effect leading to conclude that if any the minimum wage in the UK has had a modest impact compared to in the USA. However, it is important to note that the literature in the USA has focused on the employment rate of groups likely to be at the bottom of the earning distribution such as teenagers and low-wage industries (e.g fast-food and retailer sectors). In the UK a study on a low-wage sector, the residential care homes industry, finds elasticities in the range of -0.15 to -0.40 (Machin et al., 2003), although a later paper finds less robust evidence (Machin & Wilson, 2004). The design in Dolton et al. (2015) correctly estimates the impact of the NMW on employment for the working age population but ignore the fact that the effect is possibly larger for groups likely affected by the minimum wage.

Table 3.8: Estimated employment elasticities with respect to the minimum wage, η_{ER}, from studies using US data

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

3.5 Discussion and Conclusions

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

In this paper, we re-evaluate the UK research on the employment effects of the minimum wage following two common approaches in the literature. Our study first follows Bryan et al. (2013), one of the most recent and comprehensive reports commissioned on the NMW effects on employment and, as in the UK NMW literature, we also cannot reject the null that the NMW up-ratings had no impact on job retention. However, when we apply the Donald and Lang (2007) two-step estimator to conduct correct inference, the range of effects that also cannot be rejected is extremely large, and include large positive and negative values of the NMW impacts on employment. Moreover, using Bloom (1995)'s minimum detectable effects, we find that the DiD design typically used in the literature has low power to detect a real NMW impact. For example, in our preferred specification, one would need that the job retention rate in reality falls by 16 % in response to a 10 % NMW rise to be able to detect it with 80 % probability using the data and research design typically used in the UK work. This impact would correspond to a job retention elasticity with respect to the minimum wage of about -1.2. In the second part of our analysis we use Dolton et al. (2015)'s approach that exploits geographical variation in the impact of the NMW and calculate employment elasticities. In the preferred dynamic specification the employment elasticities vary between -0.083 and 0. However, we find again relatively large minimum detectable effects that point to low power of the design. Indeed, one would need a fall in employment of more than 1% in response to a 10 % NMW rise to detect the NMW effect with 80 % probability.

Our study raises concerns for the routine application of a DiD designs when assessing the impact of the NMW on employment. Although we also do not find any *statistical significant* impact, the confidence intervals we obtain suggest that the standard research design used in the assessment of the UK's NMW is not very informative. In turn, this casts doubt on the consensus that the UK NMW does not harm employment. We therefore recommend a reconsideration of the combined use of a DiD designs with existing UK data sources when evaluating the NMW impact on employment. Better evidence may come from the large recent change occurred with the introduction of the National Living Wage but it would be unfortunate if it turns out to have deleterious effects on employment.

Appendix

C1 Inference in Difference-in-Differences with Grouped Errors

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

The first concern relates to the grouped error structure. In DiD designs, the error term ϵ_{igts} is unlikely to be iid, because an individual may have unobservable characteristics that are correlated with other individuals of the same group, or may be affected by common group shocks. In the case of these studies of the minimum wage, members of the treatment group are all located at the bottom of the wage distribution, and so it is highly plausible that they may have some common unobservable characteristics (low ability, low skills, etc.) or are influenced by the same economic shocks. A comprehensive specification of equation (3.1) which includes common group shocks φ_{gts} is:

$$y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt} d_{gs} \omega_t + \mathbf{x}_{igts}^{'} \gamma + \varphi_{gts} + \xi_{igts} \tag{3.10}$$

$$i = 1, ..., N; g = C, B, T, A; s = 0, 1; t = 2000, ..., 2011$$

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

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

Various standard error corrections have been proposed to account for the common group structure in the random disturbances ϵ_{igts} and thus produce tests of the correct size: these include a parametric adjustment using intra-class correlations (Moulton, 1990), the Liang and Zeger (1986) generalization of the White (1980) heteroskedastic robust covariance matrix, a feasible GLS estimator (Hansen, 2007), and methods based on the bootstrap (Cameron et al., 2008). However, many of these techniques lead to t-statistics for the null hypothesis of no treatment effect that are asymptotically normal distributed only as the number of groups tend to infinity (e.g. Donald and Lang (2007), Angrist and Pischke (2008), Cameron et al. (2008), Brewer et al. (2013)). When the number of groups is small - and we have only 4 - the critical values of the asymptotic normal distribution will be a poor approximation to the critical values for the Wald tests, and using critical values from the standard normal distribution when the number of groups is small will lead us to over-reject the null hypothesis. But a method that does lead to Wald tests with a known distribution in cases with few clusters is a two-step estimator: under specific circumstances where the common group shock φ_{gts} is normal, homoscedastic and uncorrelated between groups and over time, Donald and Lang (2007) show this two-step estimator produces tests of the correct size.

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

In our study, we define a cell as the interaction of group, year and transition-type, giving us 96 cells (4 groups, 12 years of data, and 2 transition types). The first stage regression is then:

$$y_{ic} = \mathbf{x}_{ic}^{'}\gamma + \sum_{c=1}^{96} I_c\mu_c + \epsilon_{ic} \tag{3.11}$$

$$i = 1, ..., N; c = 1, ..., 96$$

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

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

$$\hat{\mu}_c = \delta_{ts} + \alpha_{gt} + \beta_{gt}d_c + \epsilon_c \tag{3.12}$$

$$c = 1, ..., 96; t = 2000, ..., 2011$$

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

As Donald and Lang (2007) observe, standard errors cannot be estimated with this approach for a two-by-two DiD design, as the second step is an exactly-identified

[18] All the control variables $\mathbf{x}$ in the equation 3.10 vary within-cell.

regression, with 4 coefficients (2 time effects, 1 group effect and 1 policy effect) being estimated from 4 data points. Clearly, the same is true for other types of DiD where the second step is an exactly-identified regression. What might not be immediately clear is that this situation also holds when we apply the two-step to the unrestricted equation 3.10. The argument runs as follows: in equation 3.10, identification of the impact of the NMW arises because, in every year, we observe transitions that either do or do not span an uprating, and where these transitions can come from 1 of 4 groups, three of whom are deemed to be affected by the uprating (but in different ways). Accordingly, each year of data effectively provides us with a 4-group, 2-period DiD (if we understand the 2 *periods* to refer to *whether or not a transition spans an uprating*) where the policy affects 3 groups in the second period with impacts that are allowed to be different. This means that applying the two-step method to such data would lead to a second step regression with zero degrees of freedom (with 2 time effects, 3 group effects and 3 policy effects estimated from 8 data-points). Accordingly, equation 3.10, which is the main specification in Bryan et al. (2013), and which allows both for the group effects to be different in each year and for the impact of each year's uprating on the three treated groups to be different, would also give a second-step regression in equation 3.12 with zero degrees of freedom (it is equivalent to estimating 10 separate, exactly-identified, DiDs, where each DiD is a 4-group, 2-period DiD that is producing 8 coefficients). To address this problem, we make two restrictions to the (overly) flexible model in equation 3.10 so as to be able to undertake inference, notably: each group has a constant effect on job retention over time (so we estimate α_g rather than α_{gt}, and the impact of an NMW uprating effect is constant over time (but different for each group) (so we estimate β_g rather than β_{gt}). This gives us the following:

$$y_{igts} = \delta_{ts} + \alpha_g + \beta_g d_{gs} + \mathbf{x}_{igts}^{'}\gamma + \epsilon_{igts} \tag{3.13}$$
$$i = 1, ..., N; g = C, B, T, A; t = 2000, ..., 2011$$

and the second stage is:

$$\hat{\mu}_c = \delta_{ts} + \alpha_g + \beta_g d_c + \epsilon_c \tag{3.14}$$
$$c = 1, ..., 80; t = 2000, ..., 2011$$

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

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

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

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

$$\hat{\mu}_c = \delta_{ts} + \alpha_g + \beta_g d_c\omega_t + \epsilon_c \tag{3.15}$$
$$c = 1, ..., 80; t = 2000, ..., 2011$$

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

A second concern about inference in DiD studies, as initially noted by Bertrand et al. (2004), is that the level of uncertainty surrounding the estimated policy effect in DiD designs will likely be increased by positive serial correlation in the group-time shocks, as the variable of interest in DiD designs is itself highly serially-correlated. Put more directly, if the group-time shocks φ_{gts} exhibit positive serial correlation that is ignored in estimation, then the resulting estimates of the standard errors will likely be biased downwards; it is for this reason that the Bertrand et al. (2004) recommendation is that analysts NOT cluster errors at the level of the group-time interaction, as doing so leads to incorrect inference if there is serial correlation in the φ_{gts}. In principle, this could cause a problem for our approach based on the Donald and Lang (2007) two-step, as we assume that each cell, given by a group-time-span interaction, is independent of the others. But we test for serial correlation by estimating a first order auto-regressive model of the residuals by group. The results are shown in Table C4 (Appendix C3), which displays the first order auto-regressive coefficients from our estimates of equation (3.2). Residuals for the treatment group exhibit small, negative degree of serial correlation for men, although that for women is larger, at 0.40 (the confidence intervals for both span zero). However, our research design is much less subject to problems caused by positive serial correlation in the group-time shocks because our variable of interest is *negatively* serially correlated, as it turns on and off repeatedly (recall our analysis uses data that covers 12 years, within each of which we observe two 6-month transitions that span an uprating, and two 6-month transitions that do not).

C2 Derivation of formulas for the elasticity of employment with respect to the minimum wage

C2.1 Static Model

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

$$\log(E_{jt}) = \pi_0 + A_j + \pi_t \sum_{k=1999}^{t} I_k + \theta_0 K_{jt} + \theta_t \sum_{k=1999}^{t} I_k K_{jt} + \mathbf{x}_{jt}^{'}\rho + \epsilon_{jt}$$
$$t = 1997, ..., 2010; j = 1, ..., 138$$

Since the dependent variable E_{jt} is expressed in log- terms and $K_{jt} = \frac{NMW_t}{med(wage_{jt})}$, the partial derivative of E_{jt} with respect to NMW_t is

$$\frac{\partial E_{jt}}{\partial NMW_t} = \frac{\theta_0 + \theta_t}{med(wage_{jt})} \cdot E_{jt}$$

Plugging this expression in the definition of employment elasticity

$$\eta_{ER(t)} = \frac{\partial E_{jt}}{\partial NMW_t} \cdot \frac{NMW_t}{E_{jt}} = \frac{(\theta_0 + \theta_t)E_{jt}}{med(wage_{jt})} \cdot \frac{NMW_t}{E_{jt}} = \frac{\theta_0 + \theta_t}{med(wage_{jt})} \cdot NMW_t$$
$$= (\theta_0 + \theta_t)K_{jt}$$

The employment elasticity $\bar{\eta}_{ER(t)}$ at the median of the wage distribution of employees in working age $med(wage_t)$ is

$$\bar{\eta}_{ER(t)} = (\theta_0 + \theta_t)\bar{K}_t$$

where $\bar{K}_t = \frac{NMW_t}{med(wage_t)}$

The variance of $\bar{\eta}_{ER(t)}$ is

$$
\begin{aligned}
Var(\bar{\eta}_{ER(t)}) &= Var((\theta_0 + \theta_t)\bar{K}_t) \\
&= (\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_0})^2 \sigma^2_{\theta_0} + (\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_t})^2 \sigma^2_{\theta_t} + 2\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_0}\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_t}\sigma_{\theta_0\theta_t} \\
&= (\bar{K}_t)^2 \cdot [\sigma^2_{\theta_0} + \sigma^2_{\theta_t} + 2\sigma_{\theta_0\theta_t}]
\end{aligned}
$$

Table C1: Employment elasticity with respect to the the year 2001 minimum wage uprating. Estimates of $\bar{\eta}_{ER(2001)}$ derived from the static model of equation 3.5.

$\bar{\eta}_{ER(2001)}$	A	$\sigma_{\theta_0\theta_{2001}} = A * s_{\theta_0}s_{\theta_{2001}}$	$s_{\bar{\eta}_{ER(2001)}}$	**CIs**		**MDE**
-0.074**	0	0	0.033	(-0.140	-0.008)	±0.095
-0.074***	-0.6	-0.0025	0.021	(-0.116	-0.032)	±0.060
-0.074***	-0.3	-0.0013	0.028	(-0.129	-0.019)	±0.080
-0.074**	-0.1	-0.0004	0.032	(-0.136	-0.012)	±0.090
-0.074**	0.1	0.0004	0.035	(-0.143	-0.005)	±0.100
-0.074*	0.3	0.0013	0.038	(-0.149	0.001)	±0.108
-0.074*	0.6	0.0025	0.042	(-0.157	0.009)	±0.120

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.001$
Standard errors for the employment elasticity are calculated as
$s_{\bar{\eta}_{ER(2001)}} = \bar{K}_{2001} \cdot \sqrt{s^2_{\theta_0} + s^2_{\theta_{2001}} + 2\sigma_{\theta_0\theta_{2001}}}$

The standard error of $\bar{\eta}_{ER(t)}$ $s_{\bar{\eta}_{ER(t)}}$ depends on $\sigma_{\theta_0\theta_t}$, the covariance of θ_0 and θ_t. Assuming $\sigma_{\theta_0\theta_t} = 0$ the estimate of $s_{\bar{\eta}_{ER(t)}}$ is

$$s_{\bar{\eta}_{ER(t)}} = \bar{K}_t \cdot \sqrt{s^2_{\theta_0} + s^2_{\theta_t}}$$

where $\hat{s}^2_{\theta_0}$ and $\hat{s}^2_{\theta_t}$ are the standard errors of θ_0 and θ_t. The direction of the bias of

$s_{\bar{\eta}_{ER(t)}}$ depends on the sign of $\sigma_{\theta_0\theta_t}$. $s_{\bar{\eta}_{ER(t)}}$ is overestimated if $\sigma_{\theta_0\theta_t}$ is negative and underestimated if the covariance of θ_0 and θ_t is positive. In Table C1 we show how standard errors, confidence intervals and MDEs of the 2001 elasticity change varying the correlation between θ_0 and θ_{2001} .

C2.2 Dynamic Model

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

$$\log(E_{jt}) = \gamma E_{jt-1} + \pi_0 + A_j + \pi_t \sum_{k=1999}^{t} I_k + \theta_0 K_{jt} + \theta_t \sum_{k=1999}^{t} I_k K_{jt} + \mathbf{x}_{jt}^{'}\rho + \epsilon_{jt}$$
$$t = 1997, ..., 2010; j = 1, ..., 138$$

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

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

$$\frac{\partial E_{jt+s}}{\partial NMW_t} = \gamma^s \frac{\theta_0 + \theta_t}{med(wage_{jt})} \cdot E_{jt+s}$$
$$s = 0, ..., \infty$$

and the employment elasticity $\eta_{ER(t)}^{t+s}$ at time $t+s$ with respect to NMW_t is

$$\begin{aligned}\eta_{ER(t)}^{t+s} &= \frac{\partial E_{jt+s}}{\partial NMW_t} \cdot \frac{NMW_t}{E_{jt+s}} = \gamma^s \frac{(\theta_0 + \theta_t)E_{jt+s}}{med(wage_{jt})} \cdot \frac{NMW_t}{E_{jt+s}} \\ &= \gamma^s \frac{\theta_0 + \theta_t}{med(wage_{jt})} \cdot NMW_t = \gamma^s (\theta_0 + \theta_t) K_{jt} \\ s &= 0, ..., \infty\end{aligned}$$

The employment elasticity $\bar{\eta}_{ER(t)}^{t+s}$ at the median of the wage distribution of employees in working age $med(wage_t)$ is

$$\bar{\eta}_{ER(t)}^{t+s} = \gamma^s(\theta_0 + \theta_t)\bar{K}_t$$

The total employment elasticity $\bar{\eta}_{ER(t)}$ with respect to the minimum wage at time t is the sum of the employment elasticities over time

$$\begin{aligned}
\bar{\eta}_{ER(t)} &= \sum_{s=0}^{\infty} \bar{\eta}_{ER(t)}^{t+s} \\
&= \sum_{s=0}^{\infty} \gamma^s(\theta_0 + \theta_t)\bar{K}_t \\
&= (\theta_0 + \theta_t)\bar{K}_t \sum_{s=0}^{\infty} \gamma^i \\
&= \frac{\theta_0 + \theta_t}{1-\gamma}\bar{K}_t
\end{aligned}$$

The variance of $\bar{\eta}_{ER(t)}$ is

$$\begin{aligned}
Var(\bar{\eta}_{ER(t)}) = Var(\frac{\theta_0 + \theta_t}{1-\gamma} \cdot \bar{K}_t) &= (\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_0})^2 \sigma_{\theta_0}^2 + (\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_t})^2 \sigma_{\theta_t}^2 + (\frac{\partial \bar{\eta}_{ER(t)}}{\partial \gamma})^2 \sigma_{\gamma}^2 \\
&+ 2\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_0}\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_t}\sigma_{\theta_0\theta_t} + 2\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_0}\frac{\partial \bar{\eta}_{ER(t)}}{\partial \gamma}\sigma_{\theta_0\gamma} + 2\frac{\partial \bar{\eta}_{ER(t)}}{\partial \theta_t}\frac{\partial \bar{\eta}_{ER(t)}}{\partial \gamma}\sigma_{\theta_t\gamma} \\
&= (\frac{\bar{K}_t}{1-\gamma})^2 \cdot \sigma_{\theta_0}^2 + (\frac{\bar{K}_t}{1-\gamma})^2 \cdot \sigma_{\theta_t}^2 + (\frac{\theta_0 + \theta_t}{(1-\gamma)^2} \cdot \bar{K}_t)^2 \cdot \sigma_{\gamma}^2 \\
&+ 2(\frac{\bar{K}_t}{1-\gamma})^2 \sigma_{\theta_0\theta_t} - 2\frac{\bar{K}_t^{\,2}}{(1-\gamma)^3}(\theta_0 + \theta_t)\sigma_{\theta_0\gamma} - 2\frac{\bar{K}_t^{\,2}}{(1-\gamma)^3}(\theta_0 + \theta_t)\sigma_{\theta_t\gamma}
\end{aligned}$$

The standard error of $\bar{\eta}_{ER(t)}$ depends on $\sigma_{\theta_0\theta_t}$, $\sigma_{\theta_0\gamma}$ and $\sigma_{\theta_t\gamma}$ the covariances between θ_0, θ_t and γ. Assuming that $\sigma_{\theta_0\theta_t} = 0$, $\sigma_{\theta_0\gamma} = 0$ and $\sigma_{\theta_t\gamma} = 0$ the standard error of $\bar{\eta}_{ER(t)}$ is

$$s_{\bar{\eta}_{ER(t)}} = \frac{\bar{K}_t}{1-\hat{\gamma}} \cdot \sqrt{s^2_{\theta_0} + s^2_{\theta_t} + (\frac{\hat{\theta}_0 + \hat{\theta}_t}{1-\hat{\gamma}})^2 s^2_{\gamma}}$$

where $\hat{\theta}_0$, $\hat{\theta}_t$, $\hat{\gamma}$ are the estimates of θ_0, θ_t, γ and s_{θ_0}, s_{θ_t} and s_{γ} are the standard errors of θ_0, θ_t and γ. In Table C2 we report how standard errors, confidence intervals and MDEs of the 2001 elasticity change when we relax the assumption of no correlation between θ_0, θ_{2001} and γ .

Table C2: Employment elasticity with respect to the the year 2001 minimum wage uprating. Estimates of $\bar{\eta}_{ER(2002)}$ derived from the dynamic model of equation 3.5.

$\bar{\eta}_{ER(2001)}$	$\sigma_{\theta_0\theta_{2001}}$	$\sigma_{\theta_0\gamma}$	$\sigma_{\theta_{2001}\gamma}$	$s_{\bar{\eta}_{ER(2001)}}$	**CIs**		**MDE**
-0.082	0	0	0	0.059	(-0.197	0.033)	±0.166
-0.082**	-0.0006	-0.005	-0.004	0.039	(-0.159	-0.005)	±0.111
-0.082	-0.0006	-0.005	0.004	0.053	(-0.185	0.021)	±0.149
-0.082	-0.0006	0.005	-0.004	0.055	(-0.190	0.025)	±0.156
-0.082	-0.0006	0.005	0.004	0.060	(-0.210	0.046)	±0.185
-0.082	0.0006	-0.005	-0.004	0.051	(-0.182	0.018)	±0.145
-0.082	0.0006	-0.005	0.004	0.062	(-0.204	0.040)	±0.176
-0.082	0.0006	0.005	-0.004	0.064	(-0.207	0.043)	±0.182
-0.082	0.0006	0.005	0.004	0.073	(-0.225	0.061)	±0.207
-0.082	0.006	-0.005	-0.004	0.086	(-0.252	0.088)	±0.246
-0.082	0.006	-0.005	0.004	0.093	(-0.265	0.101)	±0.265
-0.082	0.006	0.005	-0.004	0.095	(-0.268	0.104)	±0.269
-0.082	0.006	0.005	0.004	0.101	(-0.280	0.116)	±0.286

$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.001$
Standard errors for the employment elasticity are calculated as

$$s_{\bar{\eta}_{ER(2001)}} = \frac{\bar{K}_{2001}}{1-\hat{\gamma}} \cdot \sqrt{s^2_{\theta_0} + s^2_{\theta_{2001}} + (\frac{\hat{\theta}_0+\hat{\theta}_{2001}}{1-\hat{\gamma}})^2 s^2_{\gamma} + 2\sigma_{\theta_0\theta_{2001}} - 2\frac{\hat{\theta}_0+\hat{\theta}_{2001}}{1-\hat{\gamma}}\sigma_{\theta_0\gamma} - 2\frac{\hat{\theta}_0+\hat{\theta}_{2001}}{1-\hat{\gamma}}\sigma_{\theta_{2001}\gamma}}$$

where $\sigma_{\theta_0\theta_{2001}} = A * s_{\theta_0} * s_{\theta_{2001}}$, $\sigma_{\theta_0\gamma} = B * s_{\theta_0} * s_{\gamma}$, $\sigma_{\theta_t\gamma} = C * s_{\theta_0} * s_{\gamma}$, $A = -0.01, 0.01, 0.1; B, C = -0.1, 0.1$

C3 Supplementary results

Table C3: Replication of impact of NMW on job retention presented in Bryan et al. (2013)

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

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

Control variables used in the DIDs model are age, gender, marital status, highest level of education, region, ethnicity, number of children, age left education, and whether the respondent has health problems, industry, public sector, occupation, and tenure.

Bryan et al. (2013) results are from their Tables 7-8

Model that identifies the uprating effect is $y_{igts} = \delta_{ts} + \alpha_{gt} + \beta_{gt} d_{gs} + \mathbf{x}'_{igts}\gamma + \epsilon_{igts}$

Table C4: Estimates of first-order autoregression coefficient of the residuals from equation (3.2).

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

We estimated the model $\epsilon_{gt} = \rho_g \epsilon_{gt-1} + \nu_t$
ρ_C is the autoregressive coefficient for control group, ρ_T for treatment group, ρ_A for Above NMW_t group, ρ_B for Below NMW_t group
$^{*}p < 0.10^{**}p < 0.05^{***}p < 0.001$

Conclusions

This thesis sheds light on important aspects of three public policies that are among the cornerstones of the UK welfare system: the Winter Fuel Payment (WFP), the Housing Benefits (HB) and the National Minimum Wage (NMW). In the case of the WFP, we examine whether it has any impact on its ultimate goal: the health of elderly people. For Housing Benefits, we investigate the response of the rental market to a cut in housing subsidies. Studying the NMW, we offer a re-assessment of the literature on the effects of the minimum wage on employment.

In Chapter 1 we employ a regression discontinuity design exploiting the 60 year old eligibility cut-off of the WFP. We estimate the local average treatment effect of the WFP on health outcomes including self-reports of chest infection, measured hypertension and fibrinogen and C-reactive proteine, two markers of infection and inflammation. Overall, the Winter Fuel Payment does appear to have had some health benefit among those just eligible. We find a robust and statistically significant reduction in the incidence of high concentration of fibrinogen. The point estimates for all other markers point to health benefits but they are less precisely estimated. In terms of reduction in the incidence of our measures of illness for people in the early sixties, this varies between 5 % for hypertension and 44 % for fibrinogen. Using a health index that combines our illness markers, we find particularly large effects for low education individuals. Although we could estimate the health benefits only for people at the 60 year old eligibility cut-off, our findings are relevant from a policy perspective. Following the recent increase in the Female State Pension Age, the eligibility age for the WFP is now 64. Therefore the health benefits documented in Chapter 1 will have been eliminated by these increases in the eligibility age. In light of our findings we think that policy makers should be sensitive to ensure a warm indoor environment to elderly people. Adequate indoor heating may prevent illnesses reducing the pressure on the healthcare system through a fall in the number of hospitalizations and visits to physicians.

In Chapter 2 we exploit an exogenous cut in housing subsidies in the UK as a quasi-experimental design to estimate the incidence of housing subsidies on subsidised and unsubsidised tenants. The expectation of the UK government at the time of the reform was that rents would fall, benefiting all tenants. However, the reform of the housing subsidy did not affect rental market prices, and so the incidence fell almost entirely on tenants. This finding may be explained by a series of factors. Among them, there are the nominal rigidity of rents in the short term and the general conditions of the housing market. In recent years we observed an expansion of the demand in the private rental market due to an increase in number of unsubsidised tenants and new subsidy recipients. The bargaining power of tenants might be low if landlords could choose among a large pool of potential tenants. Policies that support tenants in the process of the rental agreement or incentives to supply affordable houses in areas of high demand could prevent that the incidence of housing subsidies falls on tenants.

In the final Chapter we make a point on the NMW. The conduct of the NMW in the UK has been evidence-based, and the research has broadly concluded that the NMW has no detrimental effect on employment. We re-evaluate this literature in light of two concerns. The first concern is that the literature has mostly failed to reject the null hypothesis of "no effect on employment" but has never commented on the range of values that lie within the confidence intervals. The second concern is that the studies have employed a difference-in-differences design, although there are challenges in conducting inference with such designs and this can lead to low power of the designs. In our re-analysis we find that the data are consistent with both large negative and small positive effects of the UK National Minimum Wage on employment. Minimum Detectable Effects also show that difference-in-difference designs used in the literature have low power to detect a real NMW impact. Our findings cast doubt on the consensus that the UK NMW does not harm employment and suggest a reconsideration of the combined use of difference-in-differences design and the existing data in evaluating the NMW effects.

Overall we think we have provided some useful insights on three UK public policies: the Winter Fuel Payment, the Housing Benefits and the National Minimum Wage. We indeed offer policy makers important elements to assess whether the policies have the expected effects and inputs for policy reforms.
Three useful considerations for policy makers can be inferred from this thesis. First, policy makers should clearly outline the goals of policies and conduct a systematic evaluation of the achievements of these policies. If the policy produces unexpected consequences, policy makers should promptly set out appropriate changes. The second suggestion is that policy makers should consider all the environmental conditions. Similar policies could have very different effects in different contexts. The third consideration is that policy makers should monitor the power of the research design. They should focus more on the size of the policy effect rather than whether the effect is *statistical significant*.

References

Allegretto, S. A., Dube, A., & Reich, M. (2011). Do minimum wages really reduce teen employment? Accounting for heterogeneity and selectivity in state panel data. *Industrial Relations: A Journal of Economy and Society*, *50*(2), 205–240.

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

Arellano, M., & Bond, S. (1991). Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. *The Review of Economic Studies*, *58*(2), 277–297.

Arellano, M., & Bover, O. (1995). Another look at the instrumental variable estimation of error-components models. *Journal of Econometrics*, *68*(1), 29–51.

Bazen, S., & Marimoutou, V. (2016). Federal Minimum Wage Hikes Do Reduce Teenage Employment: The Time Series Effects of Minimum Wages in the US Revisited.

Beatty, T. K., Blow, L., Crossley, T. F., & O'Dea, C. (2014). Cash by any other name? Evidence on labeling from the UK Winter Fuel Payment. *Journal of Public Economics*, *118*, 86–96.

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

Blood pressure, stroke, and coronary heart disease. (1990). *The Lancet*, *335*(8693), 827 - 838.

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

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

Brewer, M., Emmerson, C., Hood, A. and Joyce, R. (2014). *Econometric Analysis of the impacts of Local Housing Allowance reforms on existing claimants.* Department for Work and Pensions Research Report no. 871.

Brown, C., Gilroy, C., & Kohen, A. (1983). Time-Series Evidence of the Effect of the Minimum Wage on Youth Employment and Unemployment. *The Journal of Human Resources*, *18*(1), 3-31.

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

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

Cameron, A. C., & Miller, D. L. (2015). A Practitioner's Guide to Cluster-Robust Inference. *Journal of Human Resources*, *50*(2), 317–372.

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

Card, D. (1992). Using regional variation in wages to measure the effects of the federal minimum wage. *Industrial & Labor Relations Review*, *46*(1), 22–37.

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

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

Carpenter, C., & Dobkin, C. (2009). The effect of alcohol consumption on mortality: regression discontinuity evidence from the minimum drinking age. *American Economic Journal: Applied Economics*, *1*(1), 164–182.

Chen, G. (1993). Investigation on the correlation between the mortality of cerebrovascular diseases and the meteorological factors in Zhanjiang City. *Zhonghua liu xing bing xue za zhi= Zhonghua liuxingbingxue zazhi*, *14*(4), 234–236.

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

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

Collins, K. J., Easton, J. C., Belfield-Smith, H., Exton-Smith, A. N., & Pluck, R. A. (1985). Effects of age on body temperature and blood pressure in cold environments. *Clinical Science*, *69*(4), 465–470.

Collinson, R. A., & Ganong, P. (2016). The incidence of housing voucher generosity.

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

Currie, J., & Gahvari, F. (2008). Transfers in Cash and In-Kind: Theory Meets the Data. *Journal of Economic Literature*, 333–383.

Currie, J., & Yelowitz, A. (2000). Are public housing projects good for kids? *Journal of Public Economics*, *75*(1), 99–124.

Curwen, M. (1841). Excess Winter Mortality in England and Wales with special reference to the effects of temperature and influenza. *The Health of Adult Britain*, *1994*, 205–216.

Danesh, J., Lewington, S., Thompson, S. G., Lowe, G., Collins, R., Kostis, J., ...

others (2005). Plasma fibrinogen level and the risk of major cardiovascular diseases and nonvascular mortality: an individual participant meta-analysis. *JAMA: the Journal of the American Medical Association*, *294*(14), 1799–1809.

Dear, K. B., & McMichael, A. J. (2011). The health impacts of cold homes and fuel poverty. *BMJ*, *342*, d2807.

Department for Communities and Local Government. (2011). *Private Landlords Survey 2010*.

Department for Communities and Local Government. (2017). *English Housing Survey, 2008-2014: Secure Access.* ([data collection]. 6th Edition. UK Data Service. SN: 6923, `http://doi.org/10.5255/UKDA-SN-6923-6`)

Department for Work and Pensions. (2017). *Benefit expenditure and caseload tables 2017.* `https://www.gov.uk/government/publications/benefit-expenditure-and-caseload-tables-2017`.

Department for Work and Pensions, Office for National Statistics. Social and Vital Statistics Division, NatCen Social Research. (2016). *Family Resources Survey, 2005/06-2015/16 and Households Below Average Income, 1994/95-2015/16: Safe Room Access.* ([data collection]. 6th Edition. UK Data Service. SN: 7196, `http://doi.org/10.5255/UKDA-SN-7196-8`)

Department of Health. (2000). *Major changes to the policy on influenza immunisation.* CMO's Update 26 May 2000.

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

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

Commission.

Dickens, R., Riley, R., & Wilkinson, D. (2015). A Re-examination of the Impact of the UK National Minimum Wage on Employment. *Economica*, *82*(328), 841–864.

Dolton, P., Bondibene, C. R., & Stops, M. (2015). Identifying the employment effect of invoking and changing the minimum wage: A spatial analysis of the UK. *Labour Economics*, *37*, 54–76.

Dolton, P., Bondibene, C. R., & Wadsworth, J. (2012). Employment, Inequality and the UK National Minimum Wage over the Medium-Term. *Oxford Bulletin of Economics and Statistics*, *74*(1), 78–106.

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

Donaldson, L. (2010). *2009 Annual Report of the Chief Medical Officer.* London: Department of Health.

Dube, A., Lester, T. W., & Reich, M. (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The Review of Economics and Statistics*, *92*(4), 945–964.

Duvoix, A., Dickens, J., Haq, I., Mannino, D., Miller, B., Tal-Singer, R., & Lomas, D. A. (2013). Blood fibrinogen as a biomarker of chronic obstructive pulmonary disease. *Thorax*, *68*(7), 670–676.

Eerola, E., Lyytikäinen, T., et al. (2017). *Housing allowance and rents: evidence from a stepwise subsidy scheme.* VATT Working Papers 88/2017.

Elhorst, J. P. (2010). Spatial panel data models. In M. M. Fischer & A. Getis (Eds.), *Handbook of applied spatial analysis: Software tools, methods and applications.*

Eng, H., & Mercer, J. B. (1998). Seasonal variations in mortality caused by cardiovascular diseases in Norway and Ireland. *Journal of Cardiovascular Risk*, *5*(2), 89–95.

Eriksen, M. D., & Ross, A. (2015). Housing vouchers and the price of rental housing. *American Economic Journal: Economic Policy*, *7*(3), 154–176.

Evans, W. N., & Garthwaite, C. L. (2014). Giving Mom a Break: The Impact of Higher EITC Payments on Maternal Health. *American Economic Journal: Economic Policy*, *6*(2), 258-90.

Fack, G. (2006). Are housing benefit an effective way to redistribute income? Evidence from a natural experiment in France. *Labour Economics*, *13*(6), 747–771.

Fenger-Eriksen, C., Lindberg-Larsen, M., Christensen, A. Q., Ingerslev, J., & Sørensen, B. (2008). Fibrinogen concentrate substitution therapy in patients with massive haemorrhage and low plasma fibrinogen concentrations. *BJA: British Journal of Anaesthesia*, *101*(6), 769-773.

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

Fraser, G. E. (1986). *Preventive Cardiology.* Oxford University Press, USA.

Genesove, D. (2003). The nominal rigidity of apartment rents. *Review of Economics and Statistics*, *85*(4), 844–853.

Gibbons, S., & Manning, A. (2003). *The incidence of uk housing benefit: evidence from the 1990s reforms.* Centre for Economic Performance, London School of Economics and Political Science.

Gibbons, S., & Manning, A. (2006). The incidence of UK housing benefit: Evidence from the 1990s reforms. *Journal of Public Economics*, *90*(4), 799–822.

Goux, D., & Maurin, E. (2005). The effect of overcrowded housing on children's performance at school. *Journal of Public Economics*, *89*(5), 797–819.

Gruys, E., Toussaint, M., Niewold, T., & Koopmans, S. (2005). Acute phase reaction and acute phase proteins. *Journal of Zhejiang University.*, *6*(11), 1045.

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

Hofman, A., Feinleib, M., Garrison, R. J., & van Laar, A. (1983). Does change in blood pressure predict heart disease? *BMJ*, *287*(6387), 267–269.

Holtz-Eakin, D., Newey, W., & Rosen, H. S. (1988). Estimating vector autoregressions with panel data. *Econometrica: Journal of the Econometric Society*, 1371–1395.

House of Commons Work and Pensions Committee. (2014). *Support for housing costs in the reformed welfare system. Fourth Report of Session 2013–14* .

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

Iparraguirre, J. (2014). Have Winter Fuel Payments reduced excess winter mortality in England and Wales? *Journal of Public Health*, *37*(1), 26–33.

Jacob, B. A., & Ludwig, J. (2012). The Effects of Housing Assistance on Labor Supply: Evidence from a Voucher Lottery. *The American Economic Review*, 272–304.

Jacob, B. A., Ludwig, J., & Miller, D. L. (2013). The effects of housing and neighborhood conditions on child mortality. *Journal of Health Economics*, *32*(1), 195–206.

Joint Health Surveys Unit, University College London. (2016). *Scottish Health Survey, 2003.* ([data collection]. 3rd Edition. UK Data Service. SN: 5318, `http://doi.org/10.5255/UKDA-SN-5318-2`)

Jürges, H., Kruk, E., & Reinhold, S. (2013, Apr 01). The effect of compulsory schooling on health—evidence from biomarkers. *Journal of Population Economics*, *26*(2), 645–672.

Kangasharju, A. (2010). Housing Allowance and the Rent of Low-income Households. *The Scandinavian Journal of Economics*, *112*(3), 595–617.

Keatinge, W. (1986). Seasonal mortality among elderly people with unrestricted home heating. *British Medical Journal (Clinical research ed.)*, *293*(6549), 732.

Keatinge, W., & Donaldson, G. (1995). Cardiovascular mortality in winter. *Arctic Medical Research*, *54*, 16–18.

Keatinge, W. R. (2002). Winter mortality and its causes. *International Journal of Circumpolar Health*, *61*(4), 292-299.

Kennedy, S., & Parkin, E. (2011). *Winter Fuel Payment update.* House of Commons Library Standard Note SN/SP/6019. SN.

King, Anthony and Crewe, Ivor. (2014). *The Blunders of our Governments.* Oneworld Publications.

Kling, J. R., Liebman, J. B., & Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, *75*(1), 83–119.

Kloner, R. A., Poole, W. K., & Perritt, R. L. (1999). When throughout the year is coronary death most likely to occur? *Circulation*, *100*(15), 1630–1634.

Kunst, A. E., Looman, C. W., & Mackenbach, J. P. (1993). Outdoor air temperature and mortality in the netherlands: a time-series analysis. *American Journal of Epidemiology*, *137*(3), 331–341.

Lanska, D. J., & Hoffmann, R. G. (1999). Seasonal variation in stroke mortality rates. *Neurology*, *52*(5), 984–984.

Lee, D. S., & Lemieux, T. (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, *48*(2), 281–355.

Leventhal, T., & Brooks-Gunn, J. (2003). Moving to opportunity: an experimental study of neighborhood effects on mental health. *American Journal of Public Health*, *93*(9), 1576–1582.

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

Lloyd, J. (2013). Cold Enough: Excess Winter Deaths, Winter Fuel Payments and the UK's Problem with the Cold'. *London: The Strategic Society Centre*, *27*.

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

Commons, Library.

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

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

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

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

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

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

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

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

Low Pay Commission. (2016). *National Minimum Wage. Low Pay Commission Report Autumn 2016.*

Ludwig, J., Duncan, G. J., Gennetian, L. A., Katz, L. F., Kessler, R. C., Kling, J. R., & Sanbonmatsu, L. (2012). Neighborhood effects on the long-term well-being of low-income adults. *Science*, *337*(6101), 1505–1510.

Ludwig, J., Sanbonmatsu, L., Gennetian, L., Adam, E., Duncan, G. J., Katz, L. F., ... others (2011). Neighborhoods, obesity, and diabetes—a randomized social experiment. *New England Journal of Medicine*, *365*(16), 1509–1519.

Machin, S., Manning, A., & Rahman, L. (2003). Where the minimum wage bites hard: Introduction of minimum wages to a low wage sector. *Journal of the*

European Economic Association, *1*(1), 154–180.

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

Mackenbach, J., Kunst, A., & Looman, C. (1992). Seasonal variation in mortality in The Netherlands. *Journal of Epidemiology & Community Health*, *46*(3), 261–265.

Mannino, D. M., Tal-Singer, R., Lomas, D. A., Vestbo, J., Barr, R. G., Tetzlaff, K., ... others (2015). Plasma fibrinogen as a biomarker for mortality and hospitalized exacerbations in people with COPD. *Chronic Obstructive Pulmonary Diseases*, *2*(1), 23.

Marmot, M., Geddes, I., Bloomer, E., Allen, J., & Goldblatt, P. (2011). The health impacts of cold homes and fuel poverty. *Friends of the Earth*.

Marmot, M., Oldfield, Z., Clemens, S., Blake, M., Phelps, A., Nazroo, J., Steptoe, A., Rogers, N., Banks, J., Oskala, A. . (2017). *English Longitudinal Study of Ageing: Waves 0-7, 1998-2015.* ([data collection]. 27th Edition. UK Data Service. SN: 5050, `http://doi.org/10.5255/UKDA-SN-5050-14`)

Meer, J., & West, J. (2015). Effects of the minimum wage on employment dynamics. *Journal of Human Resources*.

Michaud, P.-C., Crimmins, E. M., & Hurd, M. D. (2016). The effect of job loss on health: Evidence from biomarkers. *Labour Economics*, *41*, 194 - 203. (SOLE/EALE conference issue 2015)

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

National Centre for Social Research, University College London. Department of Epidemiology and Public Health. (2010a). *Health Survey for England, 2001.* ([data collection]. 3rd Edition. UK Data Service. SN: 4628, `http://doi.org/`

10.5255/UKDA-SN-4628-1)

National Centre for Social Research, University College London. Department of Epidemiology and Public Health. (2010b). *Health Survey for England, 2003.* ([data collection]. 2nd Edition. UK Data Service. SN: 5098, http://doi.org/10.5255/UKDA-SN-5098-1)

National Centre for Social Research, University College London. Department of Epidemiology and Public Health. (2010c). *Health Survey for England, 2004.* ([data collection]. 2nd Edition. UK Data Service. SN: 5439, http://doi.org/10.5255/UKDA-SN-5439-1)

National Centre for Social Research, University College London. Department of Epidemiology and Public Health. (2011). *Health Survey for England, 2006.* ([data collection]. 4th Edition. UK Data Service. SN: 5809, http://doi.org/10.5255/UKDA-SN-5809-1)

National Centre for Social Research, University College London. Department of Epidemiology and Public Health. (2015). *Health Survey for England, 2009.* ([data collection]. 3rd Edition. UK Data Service. SN: 6732, http://doi.org/10.5255/UKDA-SN-6732-2)

Neumark, D., Salas, J. I., & Wascher, W. (2014). Revisiting the Minimum Wage—Employment Debate: Throwing Out the Baby with the Bathwater? *ILR Review*, *67*(3_suppl), 608–648.

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

Neumark, D., & Wascher, W. (1992). Employment effects of minimum and subminimum wages: panel data on state minimum wage laws. *ILR Review*, *46*(1), 55–81.

Neumark, D., & Wascher, W. (2004). Minimum wages, labor market institutions, and youth employment: a cross-national analysis. *ILR Review*, *57*(2), 223–248.

Office for Budget Responsibility. (2015). *Economic and Fiscal Outlook. Cm 9088. July.* The Stationary Office.

Office for National Statistics. Social Survey Division, Northern Ireland Statistics and Research Agency. Central Survey Unit. (2017). *Quarterly Labour Force Survey, 1992-2017: Secure Access.* ([data collection]. 10th Edition. UK Data Service. SN: 6727, http://doi.org/10.5255/UKDA-SN-6727-11)

ONS. (2015). Excess Winter Mortality in England and Wales 2014/15 (Provisional) and 2013/14 (Final)'. https://www.ons.gov.uk/peoplepopulationandcommunity/birthsdeathsandmarriages/deaths/bulletins/excesswintermortalityinenglandandwales/201415provisionaland201314final.

Ornato, J. P., Siegel, L., Craren, E. J., & Nelson, N. (1990). Increased incidence of cardiac death attributed to acute myocardial infarction during winter. *Coronary Artery Disease*, *1*(2), 199–204.

Pearson, T. A., Mensah, G. A., Alexander, R. W., Anderson, J. L., Cannon, R. O., Criqui, M., … Vinicor, F. (2003). Markers of Inflammation and Cardiovascular Disease. *Circulation*, *107*(3), 499–511.

Pepys, M. B., & Hirschfield, G. M. (2003). C-reactive protein: a critical update. *Journal of Clinical Investigation*, *111*(12), 1805.

Reeves, A., Clair, A., McKee, M., & Stuckler, D. (2016). Reductions in the United Kingdom's government housing benefit and symptoms of depression in low-income households. *American Journal of Epidemiology*, *184*(6), 421–429.

Robineau, D. (2016, February 1). *Ageing britain: two-fifths of nhs budget is spent on over-65s.* Retrieved from https://www.theguardian.com/society/2016/feb/01/ageing-britain-two-fifths-nhs-budget-spent-over-65s

Romano, J. P., & Wolf, M. (2016). Efficient computation of adjusted -values for resampling-based stepdown multiple testing. *Statistics & Probability Letters*, *113*, 38 - 40.

Rose, G. (1966). Cold weather and ischaemic heart disease. *British Journal of Preventive & Social Medicine*, *20*(2), 97.

Rosen, H. S., et al. (1985). Housing subsidies: Effects on housing decisions, efficiency, and equity. *Handbook of Public Economics*, *1*, 375–420.

Rudge, J., & Gilchrist, R. (2007). Measuring the health impact of temperatures in dwellings: Investigating excess winter morbidity and cold homes in the London Borough of Newham. *Energy and Buildings*, *39*(7), 847–858.

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

Schmaier, A. (2008). Laboratory evaluation of hemostatic and thrombotic disorders. *Hoffman Hematology: Basic Principles and Practice. 5th ed. Philadelphia, Pa: Churchill Livingstone Elsevier*, 319.

Scottish Centre for Social Research, University College London. Department of Epidemiology and Public Health. (2016a). *Scottish Health Survey, 2008.* ([data collection]. 3rd Edition. UK Data Service. SN: 6383, `http://doi.org/10.5255/UKDA-SN-6383-3`)

Scottish Centre for Social Research, University College London. Department of Epidemiology and Public Health. (2016b). *Scottish Health Survey, 2009.* ([data collection]. 5th Edition. UK Data Service. SN: 6713, `http://doi.org/10.5255/UKDA-SN-6713-3`)

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

Stewart, M. B. (2002). Estimating the impact of the minimum wage using geographical wage variation. *Oxford Bulletin of Economics and Statistics*, *64*(supplement), 583–605.

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

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

Susin, S. (2002). Rent vouchers and the price of low-income housing. *Journal of Public Economics*, *83*(1), 109–152.

Thistlethwaite, D. L., & Campbell, D. T. (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology*, *51*(6), 309.

Tillett, W. S., & Francis, T. (1930). Serological reactions in pneumonia with a non-protein somatic fraction of pneumococcus. *Journal of Experimental Medicine*, *52*(4), 561–571.

Tobin, J. (1970). On limiting the domain of inequality. *The Journal of Law & Economics*, *13*(2), 263–277.

United Nations. (1948). *Universal Declaration of Human Rights.* `http://www.un.org/en/universal-declaration-human-rights`.

Viren, M. (2013). Is the housing allowance shifted to rental prices? *Empirical Economics*, *44*(3), 1497–1518.

Wasserstein, R. L., & Lazar, N. A. (2016). The ASA's Statement on p-Values: Context, Process, and Purpose. *The American Statistician*, *70*(2), 129-133.

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

Wilkinson, P., Landon, M., Armstrong, B., Stevenson, S., & McKee, M. (2001). *Cold* *comfort: the social and environmental determinants of excess winter death in England, 1986-1996*. Joseph Rowntree Foundation.

Wilson, P. W. F., D'Agostino, R. B., Levy, D., Belanger, A. M., Silbershatz, H., & Kannel, W. B. (1998). Prediction of Coronary Heart Disease Using Risk Factor Categories. *Circulation*, *97*(18), 1837–1847.

Windmeijer, F. (2005). A finite sample correction for the variance of linear efficient two-step GMM estimators. *Journal of Econometrics*, *126*(1), 25–51.

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

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