Essays in Public and Labour Economics

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Declaration

I, Jonathan Charles Cribb, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis. Chapter 2 was undertaken as joint work with Gemma Tetlow and Carl Emmerson. Chapter 3 was undertaken as joint work with James Banks, Carl Emmerson and David Sturrock. Chapters 4 and 5 were undertaken as joint work with Carl Emmerson. Chapter 6 was undertaken as joint work with Robert Joyce and Andrew Hood.

Signature:

Date: 9th December 2019

Abstract

This thesis contains five papers in public and labour economics, exploring how individuals and families respond to, and are affected by, public policies that form key parts of the pension and tax and benefit systems.

The first paper examines how women change their retirement behaviour in response to an increase in the age at which they can first draw a state pension (the "state pension age"). It considers these changes in a country – the United Kingdom – where there are only very limited financial incentives to retire at the state pension age. Despite this, the paper finds that there are significant increases in labour supply for women in their early 60s, and argues it is likely a result of the pension age being a signal about an appropriate age at which to retire.

Exploiting the policy-driven increase in employment that is studied in the first paper, the second paper examines the effect of women being in paid work in their early 60s on two measures of health: cognitive function and physical disability. Using rich survey data, I find that being in paid work significantly increases cognitive function, particularly for single women. It also ameliorates physical conditions (as measured by walking speed and the number of self reported mobility problems), although not for women who work in the most sedentary occupations.

The third paper examines how individuals saving for retirement can be affected by their employers automatically enrolling them in employer-provided pensions, as employers are now obliged to do in the UK. Automatic enrolment substantially increases pension participation and leads to a rise in pension saving. Surprisingly, many newly-enrolled employees received an employer contribution substantially above the minimum default level. Automatic enrolment also caused the pension participation of those employees who were not obliged to be automatically enrolled to more than double.

The fourth paper seeks to understand better how automatic enrolment drives higher pension saving by examining its effects on the employees of small employers. Automatic enrolment increased pension saving with participation reaching 70% among the employees of small employers - substantially

below the 90% rate seen among the largest employers. Lower participation is not explained by differences in workers' characteristics, or in the pension contributions offered by employers, and therefore it is likely to arise from a combination of differences in the way that small employers administer workplace pensions and the role of peer effects from other employees in the workplace.

Finally, the final paper in this thesis examines how individuals are insured against the reduction in employment and earnings that results from entering the labour market during a recession. By studying the effects of leaving education when unemployment is high on measures of material living standards (household income and expenditure), we assess the role of spousal labour supply, the tax and benefit system, and the role of co-resident parents in providing insurance to young adults. The tax and transfer system plays only a small insurance role. The most important form of insurance, on average, is the income of co-resident parents. Lower educated people see bigger shocks to their labour incomes, but they are also the most likely to benefit from living with parents in the first years of their working lives.

Impact Statement

The research that I have produced in this thesis has the potential to have important impact inside and outside of academia. Inside of academia, the chapters in this thesis help to increase economists' understanding of how public policies affect individual decision making over retirement, health and saving behaviours. Furthermore, the results show that understanding of how people respond to different pensions policies need to account for behaviours that are not easily captured by standard economic models, and how reactions to these policies can have important spillovers – onto individuals themselves, or on people who are nominally unaffected by the policies. I will seek to disseminate the findings of these papers through publication in international peer-reviewed economics journals.

Outside of academia, there is clear potential of this thesis to affect public policy decision making in the United Kingdom and elsewhere. Chapters 2 and 3 explore the effects of a higher pension age in the UK. This is important given that the UK is currently increasing the pension age for men and women to 66, with increases to 67 in coming years. These papers can therefore help guide the UK government in how they undertake future increases, as well as those that are considering similar policies in other countries.

Chapters 4 and 5 explore how saving for a workplace pension changes as a result of automatic enrolment into workplace pensions. These chapters may be particularly impactful as the UK is the first place to introduce such a policy nationwide, but the UK looks set to be followed by a number of European countries and states in the USA. In particular, it will be important for policymakers to take account of the heterogeneity of the effects, and the fact that people's pension saving increased significantly more than would be implied by an ex-ante analysis.

Finally, Chapter 6 has the potential to influence policymakers' thinking on how to respond to a recession or other economic shocks. I highlight that the labour market outcomes of young people with low levels of formal education are hit hardest by leaving education in a recession, but that the living standards of young adults are most affected when they do not have access to any form of insurance

from their parents. This may be the case for young adults who come from remote or rural areas, or areas where job opportunities are particularly limited.

Throughout the course of my PhD, I have spoken to officials at the UK's Department for Work and Pensions and HM Treasury to discuss the results of my research. I will endeavour to continue to do so as I publish these chapters in peer-reviewed journals, and undertake future research in these areas. This will be helped by my being employed as an economist at the Institute for Fiscal Studies in London, which has a strong record of disseminating policy-relevant academic research through its links with media, the civil service, and academics and universities internationally.

Acknowledgements

Throughout the course of my studies in preparation of this thesis, I have been employed as an economist at the Institute for Fiscal Studies (IFS). I am grateful for the opportunity I have had to research and learn alongside some of the most intelligent, thoughtful and generous people I know. This thesis could never have been completed without the help of many of my colleagues at IFS.

I am especially grateful to my supervisors – Richard Blundell and Eric French – who have offered many hours of thoughts, help and advice over the last few years, and without whom I could not have done this. I am also grateful to my co-authors with whom I have enjoyed working: James Banks, Carl Emmerson, Andrew Hood, Robert Joyce and Gemma Tetlow. I look forward to future projects with you. In addition, I want to thank my colleagues Rowena Crawford, Paul Johnson, Rory McGee, Cormac O'Dea, Luke Sibieta and George Stoye for all the support that they have provided throughout my studies. I hope I have been even half as helpful to you as you have to me.

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My last thanks are to my wife, Dimitra Kestoras, and my parents Judith and Nigel Cribb. Dimitra has been unfailingly supportive and understanding over the last few years of work. I am indebted to her for all her love and support. None of this would have been possible without the 30 years of encouragement that I have received from my parents. Thank you.

Data Provision

This thesis makes use of a number of data sources. Chapter 2 uses the Labour Force Survey (LFS). Chapters 2 and 3 use the English Longitudinal Study of Ageing (ELSA). Chapters 3 and 6 use the Family Resources Survey (FRS). Chapters 4 and 5 use the Annual Survey of Hours and Earnings (ASHE), while Chapter 5 also uses the Annual Business Survey (ABS). Chapter 6 also uses the Family Expenditure Survey (FES). Full data citations are provided in the bibliography.

The LFS data are Crown Copyright material and are used with the permission of the Controller of HMSO and the Queen's Printer for Scotland, and were supplied by the UK Data Service.

The ELSA data were also made available through the UK Data Service. ELSA was developed by a team of researchers based at the National Centre for Social Research, University College London and the Institute for Fiscal Studies. The data were collected by the National Centre for Social Research. The funding is provided by the National Institute of Aging in the United States, and a consortium of UK government departments co-ordinated by the Office for National Statistics.

Data from the Family Resources Survey were made available by the Department for Work and Pensions.

The ASHE and ABS data were produced by the Office for National Statistics (ONS) and supplied by the Secure Data Service at the UK Data Archive. These data are Crown Copyright.

Data from the Family Expenditure Survey, the Expenditure and Food Survey and the Living Costs and Food Survey are Crown Copyright, reproduced with the permission of the controller of HMSO and the Queen's Printer for Scotland, and accessed via the UK Data Archive.

Neither the owner nor the distributor of any of these data bear any responsibility for their further analysis and interpretation. Research datasets may not reproduce National Statistics aggregates. Any errors and omissions are the responsibility of the author.

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Chapter 1: Introduction

This thesis is composed of five self-contained papers that address important questions in public and labour economics, or at the intersection between public and labour economics. In each paper, I apply micro-econometric techniques to rich survey data from the United Kingdom to address questions relating to how individuals are affected by, and how their behaviour responds to, public policies that affect the pension and tax and benefit system.

The five chapters can be broadly grouped into three distinct groups, based on the key questions that they seek to answer. 1) In an ageing society, what are the effects of a longer working life and policies that seek to extend working life? 2) To what extent can saving for retirement be encouraged by government policies? 3) How does the tax and benefit system, alongside other forms of insurance, operate to mitigate the effects of economic shocks? In this introduction, I set out the key contributions made by the research in each paper. I then discuss two cross-cutting themes that arise across the five papers.

Responses to an ageing society and the effects of a longer working life

Societies across the developed world are ageing, which – amongst other things – places stress upon public finances due to the cost of providing healthcare and pensions for an older population. In response to this challenge, policymakers in many countries are examining ways to encourage people to extend their working lives, and delay their retirement. Chapters 2 and 3 examine specific questions about how to extend working lives, and the consequences of doing so. One popular way to encourage people to delay their retirement is to raise the age at which people can first claim a pension from the state (the "early retirement age") or the age at which they can claim a full pension (the "normal retirement age"). There has been previous research undertaking either ex-ante simulations of the effects of such pension ages, (Fields and Mitchell, 1984; Gustman and Steinmeier, 1985; Rust and Phelan, 1997; Coile and Gruber, 2000), as well as some ex-post studies of implemented reforms (Staubli and Zweimuller 2013; Atalay and Barret 2015; Rabaté and Rochut 2019).

Chapter 2, which studies the effect of the increase in the earliest age that women can claim a state pension in the UK, is informative because using a difference-in-difference methodology, I show that even in a system where there are very limited financial incentives to retire at the pension age, raising the pension age can have important effects on labour supply, raising the employment of affected women by over 6 percentage points. With little evidence that credit constraints are an important driver of this behaviour, this highlights the importance of focal ages in the benefit or pension system. These focal ages can be important in ways that do not affect the financial pay off (or penalty) from working, but instead they may provide a signal to individuals about a socially appropriate age at which to undertake certain activities, such as retiring.

Societies should not (and do not) only care about the direct financial, or public finance, consequences of policies that encourage a longer working life. However, the full consequences of increased labour force participation at older ages on wellbeing, health and time use are not fully understood. One consequence of a longer working life may be altered levels of health, disability, and cognition, with direct consequences for individuals' wellbeing, and indirect consequences for their subsequent need for services, including those provided publicly. However, previous studies have found no consensus on the size or direction of these effects (see Banks, Chandola and Matthews 2015). This may be particularly complicated by the fact that some jobs may negatively affect certain types of health, others may improve it, and while some retirement lifestyles may be good for health, others may be very poor (Mazzonna and Peracchi 2012, 2017).

Chapter 3 exploits the policy-driven increase in employment that resulted from the increase in the state pension age for women to examine the effect of being in paid work in one's early 60s on the cognitive function and physical disability of older women in England. Using the state pension age reform as an instrumental variable for being in paid work, I find that being in paid work significantly increases cognitive function as measured by two separate tests, and ameliorates physical disability as measured by walking speed and self-reported measures of mobility problems. A lack of increase in social participation, and exercise, in retirement, which do not offset the lost social and physical activity act work, may be one reason for these findings. However, we find that for those women who

work in the most sedentary jobs, such as secretaries and receptionists, staying in paid work significantly reduces their walking speed compared to if they had retired. This shows the importance of understanding the mechanisms that drive health towards the end of working life, and how heterogeneous impacts of work on measures of health can be.

How can be people be encouraged to save for their retirement?

Policymakers have proposed, and economists have studied, a wide range of policies that aim to increase individuals' saving for retirement. Automatic enrolment – which is the where employees are enrolled automatically into an employer-provided (or facilitated) pension plan – is held up as a success story of behavioural economics. Most obviously, automatic enrolment gets around the procrastination problem that leads to people continually putting off saving for retirement until tomorrow (O'Donoghue and Rabin 1999). To date, the implementation of automatic enrolment in a range of American firms has been studied, finding large effects on pension plan participation, and low rates of opting out (Madrian and Shea 2001; Choi et al 2004). But its effects may be different when employers are obliged by government to enrol their employees automatically (as is done in the UK, and is either planned or proposed in a range of other European countries and American states), because employer responses to this obligation may change the effectiveness of the automatic nature of enrolment.

Chapters 4 and 5 study the first nationwide introduction of automatic enrolment – in the United Kingdom – where since 2012 increasing numbers of employers have been obliged to enrol their targeted employees automatically. Chapter 4 exploits the fact that this obligation has been introduced gradually starting with the largest employers in the country, to use a difference-in-difference methodology. Combined a large sample of employer-recorded payroll information in Britain, I find that automatic enrolment increased pension participation in large and medium sized employers by around 36 percentage points to around 90%. The largest increases are for groups – such as lower earners – who had the lowest pre-reform pension participation rates.

However, there are also a number of spillover effects of the policy that would have been very hard to predict ex-ante. First, many newly-enrolled employees received an employer contribution substantially above the (very low) minimum default level, which significantly boosts the effect of the policy on pension saving. Moreover, automatic enrolment also caused the pension participation of those employees who were not obliged to be automatically enrolled to more than double, from 15% to 36%.

Chapter 5 uses the introduction of automatic enrolment to the very smallest employers in the UK (who employ between 2 and 29 employees) to increase our understanding of why automatic enrolment is effective at boosting pension saving. Exploiting the fact that some small firms were, or were not, obliged to introduce automatic enrolment in 2016 based on certain digits of their employer tax code, we find that automatic enrolment substantially increased participation in workplace pensions for targeted employees of small private-sector employers by around 45 percentage points to reach 70%. But, while this is a very large increase, participation is still well below the very high levels (around 90%) seen among the largest employers.

Lower participation among small employers cannot be rationalised by differences in a rich set of observed individual and employer characteristics, including the offer of an employer contribution to the pension. Instead it is likely that a combination of differences in the way that small employers administer their pension arrangements and the role of peer effects from other employees in the workplace that are important in driving the difference in pension participation rates.

Tax and benefit systems and insurance against economic shocks

Government policies, in particular the tax and benefit system, can be particularly important in times of economic difficulty. Taxes on labour income and means-tested benefits (or transfer payments) can act as automatic stabilisers in times of boom and bust (Dolls et al 2012). However, they also interact with other forms of insurance which help to mitigate the effects of economic shocks, such as the labour income of a spouse, the income of co-resident parents, or the existence of savings or credit channels.

In Chapter 6, I seek to examine which of these insurance mechanisms are important for people receiving a particular kind of economic shock: the shock to labour income that results from entering the labour market during a recession, or when unemployment is high.

Previous studies from across the developed world have found that leaving education and entering the labour market has significant and persistent (although generally not permanent) negative impacts on the earnings and/or employment rates of affected young adults. Using long running household survey data from the 1970s to mid-2010s for the UK, I find that these "scarring" effects on labour income of entering the labour market when unemployment is high are substantial, albeit temporary and fade after around 5 years.

However, I find only little evidence of an effect on young adults' material living standards, as measured by their household income and expenditure. One reason for this is that the tax and transfer system plays some role in insuring young adults. However, the most important insurance mechanism is the incomes of co-resident parents. This insurance mechanism is particularly effective because the less educated – whose employment and earnings are most "scarred" by leaving education in a recession – are the group who are most likely to live with their parents.

Applying micro-econometric methods to rich economic data

Across all of the chapters of this thesis, I have applied micro-econometric methods to understand how individuals are affected by, and respond to, public policies in the pension and tax and benefit systems. The results in each of the chapters are a testament to the benefits of undertaking research in this way. I have carefully applied micro-econometric methods, usually by exploiting the variation created in the implementation of government policies.

In particular, using these methods, I have been able to test hypotheses where it would have been hard ex-ante to form a good guide to the expected direction or magnitude of the effect. Indeed, using these methods I have found results that may be considered potentially surprising: a large increase in employment resulting from the increase in the state pension age despite limited financial incentives; substantial increases in cognitive function for women working longer into their early 60s; and the fact

that firm responses to automatic enrolment overall amplified, rather than dampened the effect on pension saving for retirement. The surprising nature of these results helps to underline the value of careful empirical economic analysis in labour economics and public finance.

The importance of heterogeneity in understanding economic behaviour

Finally, another key theme to emerge from the chapters that follow are the importance of considering heterogeneity. Not all people to react to public policies, or other economic changes, in the same way. It is therefore important to consider whether there are groups that are behaving in very different ways to the rest of the population. While a longer working life improves physical disability problems on average, it significantly worsens them for those in the most sedentary jobs. Automatic enrolment sees considerably higher opt out rates among smaller employers. And although less educated people see their labour incomes hit harder by leaving education in a recession, they are potentially more protected by the incomes of their parents (with whom they are more likely to live). The findings in this thesis support the idea that any consideration of changes to policies by government should therefore consider whether the effects of their proposed policies may differ substantially across important groups in the population.

The remainder of this thesis proceeds as follows. Chapters 2 to 5 are each composed of one self-contained paper. Chapter 6 concludes by examining future research questions that build upon the research undertaken in this thesis. Appendices for each of the chapters can be found at the end of the thesis.

Chapter 2: Signals matter? Large retirement response to limited financial incentives

2.1 Introduction

Governments across the developed world have, over recent decades, legislated for increases in the early and normal claiming ages that apply to public pension schemes. Such policies have often been adopted with the explicit intention of strengthening the public finances in the face of rapidly aging populations – not only by reducing payments to pensioners but also by increasing average retirement ages and thus generating additional tax revenues. In this chapter we exploit a recent reform of the State Pension Age (SPA) for women in the UK to estimate the effect on their labour force participation. This provides an important addition to the small existing empirical literature on this topic by examining such a reform in the context of a public pension system that provides minimal financial incentives to exit work at the SPA.

In 1995, the UK government legislated to increase the SPA (known in the UK as the state pension age) for women from 60 to 65 between 2010 and 2020.[1] This chapter uses evidence on labour market behavior in the UK between 2010 and 2014 to examine what impact increasing the SPA from 60 to 62 has had on the economic activity of the affected cohorts of women.

Gruber and Wise (2004) surveyed evidence on eleven developed countries and highlighted the fact that labour force exits are concentrated around legislated early and normal retirement ages and tend to be larger than can be explained by the pure financial incentives associated with retiring at these ages. Most of the early papers that attempted to simulate the impact of moving these early and normal retirement ages on labour force participation relied on using out-of-sample predictions. Papers simulating changes in early and normal retirement ages in the US suggested quite large effects on retirement ages (Fields and Mitchell, 1984; Gustman and Steinmeier, 1985; Rust and Phelan, 1997;

[1] This is the only focal age in the UK state pension system. However, as is explained in more detail below, there is no requirement in the UK for people to retire at the point they claim their pension, and they experience no financial penalty for remaining in paid work.

Coile and Gruber, 2000; French, 2005). For the UK, Blundell and Emmerson (2007) estimate that a three-year increase in the SPA for both men and women (and assuming that defined benefit occupational pension schemes respond with a three-year increase in their normal pension ages as well) would increase retirement ages by between 0.4 and 1.8 years, depending on the specification used.

However, while the effects estimated in these ex ante simulations were quite large, if anything the results of ex post evaluations suggests even larger effects. One of the first papers to examine ex post the impact of a change in SPAs was Börsch-Supan and Schnabel (1999), who looked at evidence from the reduction in the earliest age of pension receipt in Germany from 65 to 63 in 1972. Prior to this reform, the vast majority of men in Germany retired at age 65, whereas after the reform there was a significant shift towards retiring at age 63. More recently, there have been a growing number of reforms around the world, which have increased pension ages. Therefore, ex post evaluations have become more common in the literature, although almost all of these have focused on changes to normal, rather than early, retirement ages (including, among others: Mastrobuoni, 2009; Hanel and Riphahn, 2012; Behagel and Blau, 2012; and Lalive and Staubli, 2014).

The three major exceptions are Staubli and Zweimüller (2013), Atalay and Barrett (2015) and Rabaté and Rochut (2019), who examine the effect of changes in SPAs. The former use administrative data and employ a similar estimation strategy to that used in this chapter to examine an increase in the SPA in Austria. They find that a one year increase in the SPA led to an increase in employment rates of 9.75 percentage points for affected men and by 11 percentage points for affected women, with increases in unemployment rates of a similar size. Manoli and Weber (2016) study the same Austrian reforms and find large delays in job exits and pension claiming caused by the increase in the SPA. However, the Austrian state pension system is different from the UK (and a number of other countries' systems) in several important ways. First, in the Austrian system, individuals' pension benefits are completely withdrawn if their earnings exceed around $500 a month. Second, although the Austrian system provides some increase in pension income for delayed drawing, this is done at a less than actuarially fair rate. Third, the Austrian state pension provides a very high level of earnings

replacement (according to Staubli and Zweimüller (2013), the average net replacement rate of pre-retirement earnings is 75%); public pensions, therefore, provide the main source of income for most pensioners in Austria.

Atalay and Barrett (2015) examine the effect of an increase in the earliest age at which women can access the Australian Age Pension. They find, using cross-sectional survey data, that a one year increase in the eligibility age induced a 12–19 percentage point increase in female labour supply. In Australia (unlike in the UK and many other countries) receipt of the state pension is means-tested against income, which provides a strong incentive for many Australians to retire at the point at which they can become eligible for the pension.[2] Rabate and Rochut (2019) examine the increase in the minimum retirement age in France – a pension system which provides very high replacement rates for its citizens – and find that the raising this age increases employment of affected cohorts by around 21 percentage points.

Importantly, this chapter adds to the evidence provided by these papers, by providing the first evidence from a change in SPA in the context of a system (the UK system) in which there are not strong financial disincentives to working beyond the SPA, and where private pension saving provides a significant fraction of retirement income for many people. In these respects, the UK pension system is more similar to that in the US than either the Austrian or the Australian system.

Women's economic activity could be affected by an increase in the SPA through four main mechanisms. First, increasing the SPA will have some effect on individuals' marginal financial incentives to work, through changing marginal tax rates and eligibility for out-of-work benefits. This channel will be significantly less important in the UK than it is in some other countries because there is no earnings test for state pension receipt in the UK.

[2] There have also been some studies of "early retirement" programs. Vestad (2013) studies the reduction in the age that individuals can take early retirement in Norway and find that 2/3 of pensioners would have been in work at age 63 had the age for early retirement been 64 rather than 62. However, this "early retirement" program was not open to all workers (it excluded half of private sector workers), it involved very high replacement rates (70% of after-tax earnings) and the pension benefits were earnings-tested, meaning the institutional structure is, once again, very different to that seen in the UK.

Second, the increase in the SPA reduces the length of time that individuals receive state pension income for and thus reduces their lifetime wealth; this will tend to increase labour supply. However, if those affected were forward looking and well informed, this response might have manifested as soon as the legislation was passed. Since this policy reform was announced 15 years in advance, we might expect adjustments in employment rates around the SPA to be quite small, as individuals have had a considerable period of time over which to adjust their behavior. However, evidence suggests that – even many years after the legislation was passed – many of the women affected were unaware of it. Crawford and Tetlow (2010) – using data collected in 2006–07 – find that, at that time, six-in-ten of those women who face an SPA somewhere between 60 and 65 were unaware of their true SPA. This suggests that some women may face a significant shock as they approach their SPA and thus may have to adjust their behavior sharply over a short period of time. Previous evidence suggests that individuals respond most strongly to what they believe the rules of the system are, even if their beliefs are incorrect (Bottazzi et al. 2006; Coppola and Wilke, 2014).[3]

Third, individuals who are credit constrained may have to continue working during the period when they are no longer able to receive their state pension in order to finance their consumption.

Fourth, the SPA may provide a signal about the 'appropriate' age at which to retire. The UK Department for Work and Pensions writes to each person who is entitled to a state pension four months before they become eligible to tell them how to claim. Therefore, even if the person is entirely unaware of their eligibility date before this, this communication may provide a strong signal. If the SPA does provide such signals, moving this age could have a greater impact on employment rates than the pure financial incentives would suggest.

There is mixed evidence from previous work about the importance of such signals around retirement ages. Lumsdaine et al. (1996) found that there are excess peaks in retirement in the United States at

[3] Moreover, there is evidence that individuals change their behavior upon receiving correct information about state pension rules. Liebman and Luttmer (2015) run an experiment providing individuals with information on life expectancy and Social Security rules in the US and find labour force participation is 4 percentage points higher than the control group one year later.

age 65 (the Social Security normal retirement age at the time), over and above those explained by the financial incentives generated by Social Security and Medicare, implying that there is an important signal to retire at 65. Kopczuk and Song (2008) find a significant pattern of individuals claiming Social Security in January or on their birthday, either of which might be considered a simple focal point or signal. Behagel and Blau (2012) conclude that non-standard preferences can explain why older Americans responded so strongly to the increase in the normal retirement age in Social Security that occurred in the early 2000s. Conversely, others have found evidence to the contrary – for example, Asch et al. (2005), who examined the retirement behavior of civil service employees in the US, who face different financial incentives to retire from the majority of the population who are covered by Social Security.

We identify the impact of increasing the SPA by comparing cohorts who face different SPAs, while allowing for a flexible specification of cohort, age and time effects. However, the specification we have chosen limits us to identifying only those effects that manifest between the old and new SPAs; other differences in employment rates between treated and control cohorts that occur before or after these points will be subsumed into the cohort effects that are included in our specification.[4]

We find that employment rates of women at ages 60 and 61 increased by 6.3 percentage points when the SPA was increased from 60 to 62; this result is statistically significant at the 1% level. This is equivalent to about a two month increase in the average retirement age and implies that around three-quarters of excess retirements that used to occur at age 60 are explained by that being the SPA. The result is robust to a number of specification tests, including using a linear probability model rather than probit, and variations in the sample chosen to exclude repeat observations on the same individuals, and allowing for serial correlation in employment shocks.

[4] Cribb et al. 2013 explores employment responses prior to the SPA using a method similar to that employed by Mastrobuoni (2009): that is, essentially specifying a functional form for the cohort effects and attributing any deviations from this pattern between cohorts who were affected by the 1995 legislation and those who were not as being the result of the policy change. We find no significant evidence of women having responded at earlier ages.

Subgroup analysis provides some evidence on which mechanisms may be important in explaining the changes in behavior that we observe. There is no significant difference in the response among owner-occupiers and renters, which we interpret as suggestive evidence that credit constraints may not be the primary driver. In addition, the cohort fixed effects included in our model control for differences in state pension wealth across cohorts that are a direct result of the increase in the SPA. Combined with the results of sensitivity analysis, we find no evidence that wealth effects are the driver of the response we see. In contrast to increases in the SPA that have been legislated in other countries (notably Austria and Australia), the absence of any earnings test, means that there are not strong financial incentives to retire at the SPA. Together these suggest that the role of the SPA in providing a signal about the appropriate retirement age may well be an important reason why increasing the SPA feeds through into such a sizeable increase in labour force participation.

The remainder of this chapter proceeds as follows. Section 2.2 describes the institutional setting, the policy reforms we exploit and the data we use, and presents evidence on how employment rates changed around the SPA prior to the reform. Section 2.3 describes the empirical strategy and Section 2.4 presents the results. Section 2.5 concludes.

2.2 Institutional background and data

2.2.1 Institutional details: state pensions

Between 1948 and April 2010, the SPA for women in the UK (that is, the earliest age at which they can receive a state pension) was age 60. There is no earnings test for receipt of the state pension (that is, the amount received is not reduced if the individual also has earned income)[5] but individuals do receive an actuarial adjustment of benefits if they delay claiming beyond the SPA.[6] Those not

[5] The earnings test was abolished in 1989. Disney and Smith (2002) examine the labour supply impact of this change.

[6] Since October 2006 it has been illegal for employers to force individuals aged under 65 to retire on the grounds of age alone. Between October 2006 and March 2011, employers were able to impose compulsory retirement ages at or above age 65, but since April 2011 it has been illegal to impose any compulsory retirement age, unless it can be objectively justified by the demands of the job. It is also very unlikely that employers are

claiming the state pension when they reach the SPA receive a 1% increase for each five weeks of deferral, which is equivalent to a 10.4% increase for each year that they delay claiming. However, in practice (and somewhat surprisingly given the generosity of the deferral rate), very few people choose to delay. Evidence from the English Longitudinal Study of Ageing (Crawford and Tetlow 2010) suggests that only 5% of those aged between the SPA and 75 in 2008–09 had deferred receipt of their state pension.

Given that 95% of people in the UK claim their state pension at the SPA, and a large fraction of people work beyond their SPA[7], it is not appropriate to define "retirement" as claiming a state pension, as is done in some countries. Instead, research into retirement behavior that uses UK data characterizes retirement as an exit from the labour market, rather than claiming a pension. For examples of this, see Meghir and Whitehouse 1997, Blundell and Johnson 1998 and Blundell, Meghir and Smith 2004.

The UK state pension consists of two parts. The first-tier pension (known as the basic state pension) is based on the number of years (but not on the level) of contributions made.[8] The second-tier pension is positively related to earnings across the whole of working life (from 1978 onwards); enhancements are also awarded for periods spent out of work due to some formal caring responsibilities since April 2002. However, historically, the majority of employees has opted out of building entitlement to this second-tier pension and instead build up a private pension entitlement in return for a reduction in payroll taxes.[9]

using temporary contracts to allow older workers to be dismissed at the SPA, as these are very rare in the UK. Only 2.7% of 60 year old women were employed on a "non-permanent contract" in 2009–10.

[7] According to the UK Labour Force Survey 44% of 60 year old women, and 30% of 65 year old men – the SPA for men is 65 – were in work in the 2 years prior to the increase in the SPA for women.

[8] Periods in receipt of certain unemployment and disability benefits and periods spent caring for children or adults can also boost entitlement.

[9] Crawford et al (2013) estimate that 82% of men and women born between 1951 and 1954 had opted out of building entitlement to the second tier pension at some point during their working lives. A full description of the UK state pension system can be found in Bozio et al. (2010).

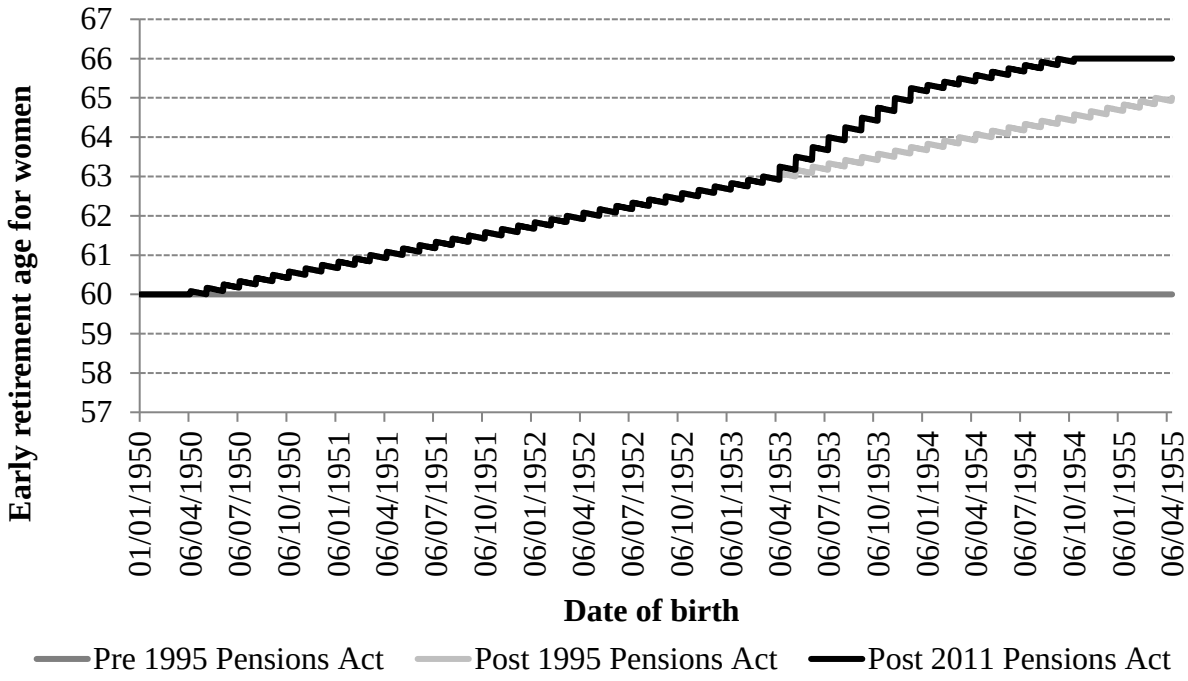
A full basic state pension in 2012–13 was worth £107.45 a week (equivalent to around $170 or 17% of average full-time weekly earnings).[10] Most men and women now reaching the SPA can qualify for the full award. The second-tier pension scheme replaces 20% of earnings within a certain band. The maximum total weekly benefit that could be received in 2012–13 was around $260. However, since most employees opted out of the second-tier pension scheme in the past, the majority of pensioners receive far less than this from the state.

The Pensions Act 1995 legislated for the SPA for women to rise gradually from 60 to 65 over the ten years from April 2010, with the SPA rising by one month every two months for ten years, equalizing with that for men (at age 65) by April 2020. As a result, women born after April 1950 have an SPA of greater than 60.[11] This is shown in Figure 2.1, which also shows the effect of a more recent reform to future SPAs that we do not examine here. The total loss from a one-year increase in the SPA is around $9,000 for a woman who qualifies for a full basic state pension and no additional pension, rising to about $22,400 for a woman who qualifies for a full basic state pension and a full additional pension entitlement.

[10] Women approaching the State Pension Age earn, on average, much less than the economy-wide average and are more likely to work part time. Median earnings for 59 year old women who were in work in the two years prior to the increase in the State Pension Age were about $410 per week, meaning that a full basic state pension is worth around 40% of actual median earnings for this group.

[11] To our knowledge no occupational pension schemes adjusted their normal pension ages in line with the change in the State Pension Age for women. Until recently, the most common normal pension ages were 60 in public sector schemes and 65 in private sector schemes. We are not aware of any schemes that apply a different normal pension age to male and female scheme members.

Figure 2.1: UK State Pension Age for women under different legislation



Source: Pensions Act 1995, schedule 4 (http://www.legislation.gov.uk/ukpga/1995/26/schedule/4/enacted); Pensions Act 2007, schedule 3 (http://www.legislation.gov.uk/ukpga/2007/22/schedule/3); Pensions Act 2011, schedule 1 (http://www.legislation.gov.uk/ukpga/2011/19/schedule/1/enacted).

Notes: the reason that the SPA increases in a "sawtooth" pattern, rather than a smooth line or a "step" pattern, is that women born in a given month are allocated a single "state pension date" at which they are eligible for a state pension. Therefore, women born later in the month have a slightly lower SPA than those born earlier in the month.

State pension entitlements make up a significant fraction of total retirement resources for some individuals, while for many others they are much less important. Table 2.1 shows statistics on the distribution of different types of wealth among the cohorts of women that are the focus of this chapter. On average, these cohorts had accrued just under $210,000 of state pension entitlements by 2010; this figure is calculated as the present discounted value of the estimated future stream of state pension income. However, these women's mean total family wealth is just over $1.3 million. On average, women's own state pension wealth accounted for one-quarter of their family's total wealth; but for one-in-nine women their state pension wealth accounts for more than half their family's total wealth. Since the SPA rises gradually as shown in Figure 2.1, on average, women born in 1950–51 and 1951–52 experience a one year loss of state pension income. A loss of one year of full basic state pension corresponds to a 4% of women's median state pension wealth, and 0.8% of median net family wealth.

Table 2.1: Distribution of wealth for women born between April 1949 and March 1952

$ thousands	Mean	25th percentile	Median	75th percentile
State pension wealth (individual)	206.7	159.6	212.3	259.3
State pension wealth (family)	365.7	273.2	381.1	475.4
Private pension wealth (individual)	145.8	0.0	37.7	169.4
Private pension wealth (family)	401.0	34.9	220.1	531.1
Net financial wealth (family)	136.2	2.3	39.1	146.4
Net housing wealth (family)	325.9	137.3	290.8	452.3
Other physical wealth (family)	90.7	0.0	0.0	7.3
Total net wealth (family)	1,325.5	645.6	1,067.1	1,657.9

Notes: Sample includes all ELSA core sample members born between 1 April 1949 and 31 March 1952. Sample size = 746.
Source: English Longitudinal Study of Ageing, wave 5 (2010–11). Weighted using cross-sectional weights.

A. Institutional details: tax and benefit system

2.2.2 Institutional details: tax and benefit system

In the UK, some features of the direct tax and benefit also change when an individual reaches the SPA and potentially influence incentives to remain in paid. First, employees are work no longer liable for employee National Insurance contributions (i.e. payroll taxes decline); this increases the financial incentive to be in paid employment. Second, instead of being able to claim the unemployment and disability benefits for working-age adults who are out of work,[12] households with one member above the female SPA become eligible to claim the means-tested pension credit guarantee. This is more generous than the equivalent working-age benefits: not only is the amount received higher (it is worth £142.70 per week, or around $230, with greater amounts for those with disabilities) but there are also no requirements for recipients to, for example, seek work or attend work-focused interviews. This reduces the incentive for individuals to be in, or to seek, paid work after reaching the SPA.[13] These

[12] The main working-age unemployment benefit is known as Jobseeker's Allowance (JSA) and is paid at a rate of £71.00 ($115) per week. The main working-age disability-related benefit is known as Employment and Support Allowance (ESA) and is paid at a rate of £99.15 ($160) per week.

[13] However, eligibility for pension credit is determined at the family level: a family is eligible for pension credit if *either* partner is over the State Pension Age for women. Therefore, the higher generosity of pension credit

different effects mean that some women face a reduced incentive to work (as measured by a participation tax rate) at the age of 60 when the SPA rises, while others see almost no change or an increased incentive to work.

2.2.3 Data

We use data from the UK's Labour Force Survey (LFS; see Northern Ireland Statistics and Research Agency et al 2014). This is conducted on a quarterly basis, with all individuals in a household followed for up to five consecutive quarters ('waves') and with one-fifth of households being replaced in each wave. For survey data, the sample size is large and the survey contains information on individual labour market activities combined with background information such as sex, age, marital status, education and housing tenure. Crucially for our study, the data contain month as well as year of birth, and relatively large numbers of individuals are observed from each birth cohort at each age. For example, about 170 individuals born in the first quarter to be affected by the reform (1950Q2) are observed in each quarter of the LFS data that we use in our analysis (which runs from 2009Q2 to 2014Q2). Further details of the achieved sample size by age and cohort are shown in Table A.2 in the appendix.

Since the LFS data are used to produce internationally comparable employment and unemployment statistics, we use the International Labour Organization (ILO) definitions of economic activity. Under these definitions, an individual is categorized as employed if they do any paid work (as an employee or self-employed) in the week of their interview, if they are temporarily away from paid work or if they are on a government training scheme (although this last category is rare for older people). Individuals are considered as being in full-time work if they work 30 or more hours in a usual week. If individuals are not in work, they are categorized as either unemployed (looking for work in the last

relative to working-age out-of-work benefits only gives an incentive to retire at the State Pension Age for single women or women with a partner younger than them (47% of our sample). Moreover, many families are only eligible for lower levels of pension credit (or none at all) due to other sources of income (e.g. state or private pensions) or owning (non-housing) assets above £10,000. For more details on the UK benefit system, see Hood and Oakley (2014).

four weeks or waiting for a job to start and they must be able to start work within the next two weeks), or "economically inactive". Of those who are economically inactive, they can give a number of reasons why they are not in (or actively seeking) paid work. These reasons are being 'retired', 'sick or disabled', or a residual category that includes looking after the family (these are all self-defined).

The pattern of economic activity of older women by age is shown in Figure 2.2. This uses LFS data pooled across the eight years before the female SPA was increased. The percentage of women in paid work (either full-time or part-time) declines with age. Between age 59 and age 60, there is a 13.7 percentage point drop in employment and a 23.5 percentage point increase in the percentage reporting themselves as retired. Both of these changes are bigger than any of the changes observed between other consecutive ages. However, prior to the SPA for women being increased, it was not possible to separate out the extent to which this was an impact of hitting the SPA as opposed to an impact of reaching age 60.[14] The 13.7 percentage point decrease in employment at age 60 compares to a fall of 3.8 percentage points between ages 58 and 59 and of 6.3 percentage points between ages 60 and 61. A simple comparison of these differences would imply there is an excess spike of women leaving employment at age 60 of between 7 and 10 percent of the population.

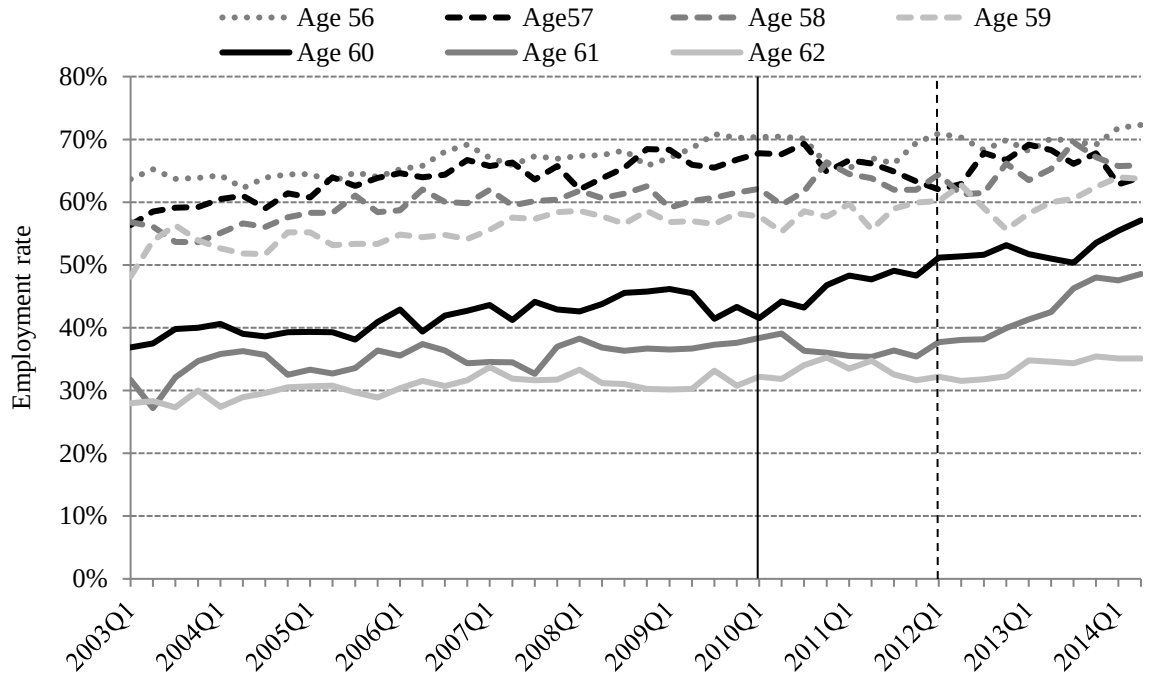
[14] One approach has been to assume a parametric relationship between labour market exit and age (for example, a quadratic in age) and also allow for an additional impact of hitting the State Pension Age. But this assumes that all of the additional retirements that occur at age 60, over and above those explained by the relationship with age measured at earlier and later ages (and other covariates in the model), are due to this age being the State Pension Age. See, for example, Blundell and Emmerson (2007).

Figure 2.2: Economic activity of women prior to State Pension Age reform, by age

Note: Averages over the period 2003Q1 to 2010Q1. *Source:* Authors' calculations using the LFS. Based on 404,428 observations.

An initial indication of what the impact of increasing the SPA on employment has been is provided by Figure 2.3. This shows how employment rates of older women have evolved since 2003 by single year of age. While employment rates at each age have generally been increasing over time (due, at least in part, to more recent cohorts of women having greater labour force attachment), a particularly large increase has been observed for 60-year-old women from April 2010, which is when the SPA started to rise. In 2010Q1 (just prior to the increase in SPA for women), the employment rate of 60-year-old women was 41.5%; by 2012Q2 (the first quarter in which all 60-year-olds were under the SPA), it had increased to 51.4%. This 9.8 percentage point increase is statistically significant at the 1% level. During the same two-year period, the employment rate of 61-year-olds fell slightly (by 0.3 percentage points, from 38.4% to 38.1%, a change that is not statistically significant).

Figure 2.3: Employment rates of women over time, by single year of age



Note: The solid vertical line at 2010Q1 represents the last quarter in which all 60 year olds were under the SPA. The dashed vertical line at 2012Q1 represents the last quarter in which all 61 year olds were under the SPA

Source: Authors' calculations using the LFS, 2003 to 2014. Based on 223,590 observations.

As the SPA rose from age 61 to 62, the employment rate of 61 year old women increased rapidly, by 10.9 percentage points from 37.7% in 2012Q2 (when 61 year olds were over the SPA) to 48.6% in 2014Q2 (when all 61 year olds were under the SPA), while the employment rates of other age groups increased over this period too, it did so by less. The increases in employment rates of 60 year olds from 2010 to 2012 and of 61 year olds from 2012 to 2014 were the largest increases over any two year period shown in Figure 2.3 and provide indicative evidence of a substantial effect on labour supply of older women from increasing their SPA

Sections 2.3 and 2.4 present formal approaches to estimating this effect, controlling in a more sophisticated manner for time effects, cohort effects and differences in observed characteristics between the different cohorts of women.

A description of the background characteristics and the variation in economic statuses by these characteristics, of women close to the SPA immediately before and after it started to rise from age 60 is shown in Table 2.2. Among those not in paid work, the most common reported activities are being

‘retired’, being ‘sick or disabled’ and ‘other’ (which most commonly refers to looking after the home or family). Relatively few women in this group report themselves as being unemployed. Full-time employment is more common among single women than among those in couples. Those who own their own home are much more likely to be in work (either full- or part-time) than those who rent their home, while those in the latter group are relatively more likely to be unemployed or sick/disabled (indeed, almost one-third of renters report being sick or disabled). Employment rates are positively correlated with levels of education, with those with lower levels of education being more likely to report being sick/disabled or having ‘other’ as their main economic activity.

Table 2.2: Economic activity for women born between April 1949 and March 1954, in the period 2009Q2 to 2014Q2

	Percentage of sample in each economic activity						*Sample size*
Sample	Full-time work	Part-time work	Unemployed	Inactive: Sick or disabled	Inactive: Retired	Inactive: Other	
Full sample	26.4	24.1	1.9	11.6	27.6	8.4	60,173
Single women	31.6	19.0	3.0	18.3	22.4	5.7	17,712
Women with a partner	24.3	26.2	1.4	8.9	29.8	9.5	42,461
– *whose partner is older*	23.4	25.4	1.3	8.8	31.4	9.8	31,315
– *whose partner is younger*	26.8	28.6	1.6	9.0	25.4	8.7	8,901
Rent house	20.4	14.9	3.4	29.7	20.9	10.6	11,572
Own house	27.9	26.3	1.5	7.3	29.2	7.8	48,601
Degree or other HE	31.8	25.5	1.7	5.3	30.0	5.9	17,514
Secondary education	28.5	26.3	2.0	9.9	25.8	7.7	29,404
No qualifications	14.8	17.4	1.9	24.0	28.8	13.2	13,255

Notes: Totals may not sum to 100 due to rounding. *Source:* Authors' calculations using the LFS.

2.3 Empirical Methodology

Using data on the labour market behavior of women who face different SPAs allows us to estimate what impact increasing the SPA for women from 60 to 62 has had on labour force participation. To do this, we employ a difference-in-differences methodology. Being under the SPA *(Under_SPA)* is administered to all women but, since the reform was introduced, is administered for longer to women born more recently. Equation 2.1 below sets out the difference-in-differences specification we use to estimate the impact of increasing the SPA.

$$y_{ict} = \alpha Under\_SPA_{ict} + \gamma_t + \lambda_c + \sum_{a=1}^{A} \delta_a [age_{ict} = a] + X_{ict}\beta + \varepsilon_{ict} \qquad (2.1)$$

Our aim is to estimate the effect on an outcome, y, for individual, i, of being below (rather than above) the SPA. Age and calendar time are important determinants of labour force participation, so we control for these variables flexibly: fixed effects are used to control for age (in years and quarters, with 33 dummies included in the model), and time period (γ_t- in year and quarter, with 20 dummies). Since each cohort of women in the UK has higher labour market attachment, we also we want control for fixed differences between cohorts. By including fixed effects for each (financial) year of birth (λ_c), we can control for cohort effects in a flexible way too.[15]

Using this specification, we implicitly assume that there are cohort- and time-constant age effects, time- and age-constant cohort effects and age- and cohort-constant time effects. The last is the usual common trends assumption required for identification in difference-in-differences estimation.

The age- and time-constant cohort effects control in a flexible way for underlying differences in employment patterns between different cohorts of women. However, this comes at the cost of subsuming within this 'cohort effect' any impact of the SPA reform that manifests itself in time-

[15] We could not control for cohort using fixed effects for year and quarter of birth, as that would be perfectly co-linear with the age and time fixed effects. Using (financial) year of birth dummies allows us to control flexibly for fixed differences in labour market participation for different cohorts, without encountering problems of perfect co-linearity.

constant changes in economic activity rates among the affected cohorts before age 60.[16] These cohort effects will also control for differences across cohorts in state pension wealth, meaning that the effect we find of the reform on labour force participation will not be due to wealth effects, unless these manifest in a non-linear way across cohorts that is not accounted for in our specification. The next section contains some sensitivity analysis regarding how we allow for cohort effects.

We also control for a vector of individual characteristics, *X*. These include education, relationship status, housing tenure, ethnicity, and geography, as well as partner's age and partner's education for those with a partner – the full set of covariates included is laid out in Table A.1 in the appendix.

The primary outcome of interest is the effect of increasing the SPA on employment. This is estimated using both ordinary least squares (OLS) and a probit model, calculating the average marginal effects of the treatment.[17] However, we are also interested in the other possible economic states. To assess these, we use multinomial probit models to examine the impact of increasing the SPA on: first, whether an individual is in full-time or part-time work or not in paid work; and, second, whether an individual is in work, or is unemployed or whether they self-report as being retired, sick or disabled, or a residual category.

Since the LFS tracks individuals over up to five consecutive quarters of data, our sample contains multiple observations on the same individuals and so the observations are not independent of one another. We allow for this by clustering standard errors at the individual level and also conduct a sensitivity analysis using only the first observation on each individual; we show that this changes the

[16] Any other policy changes that affect cohorts (and their behavior) differently, but in a time-constant way, will also be absorbed into these cohort effects. This could apply, for example, to the reforms legislated in Pensions Act 2007, which changed the way that pension entitlements were calculated (in a way that made the system more generous on average) for all those born after 5 April 1950.

[17] Since being under the State Pension Age is a function of both a woman's cohort and time, the treatment variable, *T, is* an interaction term. In a non-linear model, calculating marginal effects on an interaction term does not produce a difference-in-differences treatment effect as it does in a linear model. To estimate the treatment effect in a non-linear model, we estimate the model and then, for each observation, look at the difference in the predicted probability of employment if above and below the State Pension Age and then average across all observations to calculate the average marginal effect across the whole distribution of other regressors.

estimated marginal effect very little but increases the standard errors as the sample size is substantially reduced. Our results are also robust to allowing for serially correlated cohort shocks.

2.4 Results

2.4.1 Effect of increasing the SPA on women's employment rates

All the models are estimated on data from 2009Q2 – one year before the SPA started to rise – to 2014Q2 – the first point at which the SPA had reached 62. The cohorts included are those born in 1949–50 to 1953–54, which includes one cohort unaffected by the reform (1949–50) and four cohorts whose SPA was changed by the reform. Of course, as was shown in Figure 2.1, younger cohorts are also affected by the reform, but they are not observed in our data past the old SPA of 60, so they are not included in this analysis. Cohort is controlled for using financial year of birth (e.g. 1950–51 includes those born between 1 April 1950 and 31 March 1951) fixed effects. Time is controlled for using year and quarter fixed effects and there are age fixed effects in years and quarters to control finely for age, which is particularly important in ensuring that the estimate of being under the SPA is not simply capturing the effect of being younger. Calculating whether each individual woman is above or below the SPA involves calculating the date at which she becomes eligible for a state pension, and then comparing it to the date of interview.[18]

Table 2.3 reports the results from estimating Equation 2.1 using a variety of econometric specifications where the dependent variable is being in employment. All of them show that shows that

[18] Under the reform, people born from the sixth day of one month to the fifth day of the next month become eligible for a state pension on the same date. While the exact day of interview is observed in the LFS, only an individual's year and month of birth are available, not their day of birth. This means that those women born between the first and fifth days of any month are allocated a date of becoming eligible to a state pension that is 2 months after they actually reach their State Pension Age. If dates of birth are distributed uniformly within each month, we will have misclassified whether the woman is over or under her State Pension Age for 2.7% of women. Similarly, age in years and quarters may be mismeasured for a small number of individuals, by at most one quarter.

being under the SPA increases the probability of being in work by around 6.5 percentage points, with this impact being statistically different from zero at the 1% level.[19]

This effect may seem small compared to the effects of raising the SPA found in by Atalay and Barrett (2015) in Australia (12-19 percentage points) and Staubli and Zweimulller (2013) in Austria (9.75 to 11 percentage points). However, while there are large changes in the incentives to be in work around the SPA in Austria and Australia there are only minimal changes in financial incentives to work at this point in the UK, due to the absence of the earnings test. In that context, we find a sizeable labour supply response to only a very limited change in the incentive to be in work. Indeed, the magnitude of this estimated effect implies that around three-quarters of the "excess" retirements previously observed at age 60 in the UK (as shown in Figure 2.2) can be explained by it having been the SPA.

Table 2.3: Effect of increasing the State Pension Age from 60 to 62 on women's employment

Specification	*Number of waves*	*Estimated by*	*Standard errors clustering*	*Effect of being under SPA*	*Standard error*	*N*
(1)	5	OLS	Not clustered	0.065***	[0.009]	60,173
(2)	5	OLS	At individual level	0.065***	[0.014]	60,173
(3)	1	OLS	Not clustered	0.066***	[0.020]	13,588
(4)	1	OLS	At cohort level	0.066***	[0.020]	13,588
(5)	1	OLS	Wild cluster bootstrap-t	0.066***	[N/A][a]	13,588
(6)	5	Probit	At individual level	0.063***	[0.014]	60,173
(7 - pseudo reform)	5	Probit	At individual level	–0.007	[0.017]	37,804

[a] Using the wild-cluster bootstrap-t procedure calculates a correct p-value with small numbers of clusters, not standard errors. The estimated p-value using this procedure was 0.006.

Notes: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level, * at the 10% level. Specifications 1–6 estimated on women born in 1949–50 to 1953–54 from 2009Q2 to 2014Q2. Specification 7 ('pseudo reform') estimated on women born in 1947–48 to 1950–51 from 2007Q2 to 2010Q2. Probit models estimated using maximum likelihood estimation, and standard errors calculated by bootstrapping the marginal effect 1,000 times. Cohort-level clusters are at year and month of birth level.

[19] While ethnicity and education (in practice) are fixed for older women, the increase in the State Pension Age could affect relationship status or housing tenure, so these characteristics could be endogenous. Running the model (specification 6) without controls for relationship status, partner's characteristics or housing tenure leads to a coefficient estimate of 0.062, very similar to the estimate including them. As it is unlikely that the increase in the State Pension Age has had any important effects on housing tenure or relationship status, we include these as explanatory variables in our preferred specification.

To test whether the inclusion of multiple waves of data has an impact on our results and whether our clustering is appropriate, we compare specifications estimated by OLS and probit models and it makes very little difference to the results. Using only one wave of data (specification 3) to test the importance of including non-independent observations on the same individuals, the estimated impact is almost unchanged , at 6.6 percentage points, from when using all waves, but we estimate the impact with less precision owing to the considerably smaller sample size (although the estimated impact is still statistically significant at the 1% level). Our preferred approach is, therefore, to include all waves of data, but cluster at the individual level.

A further worry may be that there are shocks at the cohort–time level. If the correlation in employment shocks between people from the same cohort at the same time is positive, this would tend to bias standard errors downwards: in other words, we would be too likely to conclude that raising the SPA affected employment even if it did not (see, for example, Moulton, 1990; Donald and Lang, 2007). We may also worry that there is serial correlation in employment shocks, at the individual and/or cohort level. Ignoring such serial correlation has been shown seriously to bias standard errors (Bertrand et al. 2004; Cameron et al. 2008). To test the implications of these concerns, we first, in specification 4, account for clustering at the cohort (defined here as month and year of birth) level using cluster-robust standard errors (Liang and Zeger, 1986). This makes little difference to the standard error. However, these standard errors are only consistent as the number of clusters tends to infinity, and we have only 60 clusters. Therefore, in specification 5, we implement a wild-cluster bootstrap-t procedure, as suggested by Cameron et al. (2008), to account both for any cohort–time-level shocks and serial correlation in individual and/or cohort–time shocks,[20] and we find the effect is still significant at the 1% level. Therefore, serially correlated cohort–time shocks do not seem to present a problem in estimating standard errors in this case.

[20] Cameron et al. (2008) show that a wild-cluster bootstrap-t procedure can be used to obtain hypothesis tests of the right size even with few clusters.

A further test of the validity of our model is to conduct a placebo test – that is, to test whether there is an effect when we would not expect to see one. One way to do this is to imagine that the reform was introduced in 2008 instead of 2010 and look for the impact of being below, rather than above, this pseudo SPA for these earlier cohorts. We would expect to see no effect of this pseudo SPA and specification 7 shows that there is, indeed, no impact. The size of the marginal effect is small and of the opposite sign to that found for our main specifications, and is not statistically different from zero.

Finally (not presented in the table) we test the sensitivity of our main result (from specification 6) to different ways for controlling for cohort effects. Not including any control for birth cohort led to the estimated effect of being under the SPA on employment increasing slightly to 0.069 (instead of 0.063). A richer specification – in which we allow for financial year of birth cohort effects to differ by year of age (thus exploiting the fact that even within these cells there is variation in the SPA) – is found to lead to a very similar estimated effect of being aged under the SPA on employment of 0.064 (with a larger standard error of 0.018 percentage points, but still sufficiently small for the estimate to be statistically significant at the 1% level).

Table 2.4 presents marginal effects of being under the SPA, estimated separately for different subgroups using OLS. Although there is substantial variation in the point estimates, there are no significant differences in the estimates between subgroups. Single women, if anything, respond more strongly than those in couples. This might be expected given that the latter potentially have an additional margin (their partner's labour supply) on which they can adjust to the loss of state pension.

Women who own their own home have a very similar estimated effect (economically and statistically) to those who rent their home. Home owners are less likely to be credit constrained because they are more likely to have savings or access to credit than renters. This suggests that credit constraints may not play a significant role in determining how women respond to increasing the SPA.

Table 2.4: Effect of increasing the State Pension Age from 60 to 62 on women's employment for different subgroups

	Effect of being under SPA	*Std error*	*N*	*Average employment rate at ages 60 & 61 pre-reform*
Full sample	0.063***	[0.014]	60,173	40.9%
Single women	0.097***	[0.025]	17,712	40.2%
Women with a partner	0.052***	[0.017]	42,461	41.2%
– *whose partner is older*	0.051**	[0.020]	31,315	37.4%
– *whose partner is younger*	0.061*	[0.034]	11,146	49.2%
Rent house	0.051*	[0.029]	11,572	30.5%
Own house	0.071***	[0.016]	48,601	43.1%

Notes: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level, * at the 10% level. All models are estimated using OLS estimated on women born in 1949–50 to 1953–54 from 2009Q2 to 2014Q2 with standard errors clustered at the individual level. Pre-reform employment rate estimated from LFS data in 2008–09 and 2009–10.

2.4.2. Effect of increasing the SPA on broader measures of women's economic activity

The effect of increasing the SPA on employment is important in determining how raising the SPA will affect the public finances by generating additional tax revenues. However, the larger public finance picture and individuals' welfare will also be affected if individuals work full-time rather than part-time or if increasing the SPA increases the number of individuals claiming unemployment or disability benefits. Therefore, we have also examined how increasing the SPA affects the propensity to work full- or part-time or to engage in other economic activities. Figure 2.2 showed that, prior to the reform, at age 60 there was a drop in both full- and part-time employment and the increase in self-defined retirement was larger than the fall in employment.

We first use a multinomial probit model to estimate the impact of being above the SPA on whether a woman is in full-time work, in part-time work or not in paid employment. These results are presented in the top panel of Table 2.5. While both full-time and part-time employment is found to have

increased as a result of increasing the SPA, the impact on full-time employment is slightly larger (at 3.5 percentage points) than the impact on part-time employment (2.9 percentage points).

We also use a multinomial probit model to estimate simultaneously the impact of increasing the SPA on the prevalence of five different economic activities. As the bottom panel of Table 2.5 shows, the estimated impact on being 'retired' (–11.5 percentage points) is larger in absolute terms than the impact on being in paid work (5.5 percentage points).

This model also suggests that there was a significant increase in the proportion of women reporting being unemployed when the SPA was increased (1.2 percentage points). This is a large effect given that the proportion of age 60 year old women who were unemployed prior to the rise in the SPA was only 0.8% and will have reduced the net gain to the public finances from increasing the SPA. The increase in prevalence of unemployment when the SPA is increased could arise because individuals continue actively seeking paid work until they reach SPA, when they qualify for non-employment income sources (such as state pensions), which do not have the same job search requirements as working-age out-of-work benefits, such as unemployment benefit (known as Jobseeker's Allowance). However, it is very small compared to the 12 percentage point increase in the proportion claiming unemployment benefit found to have been caused by the increase in the SPA in Austria (Staubli and Zweimuller 2013).

Table 2.5 also shows that we estimate the increasing the SPA for women increases the probability of 60 and 61 year old women saying they are economically inactive due to being "sick and disabled" by 4.0 percentage points, compared to a pre-reform baseline of 9.0%. This increase may be because women remain eligible for "Employment and Support Allowance" (the UK's version of Disability Insurance – for more details, see Banks et al 2015) until they reach the SPA. Alternatively, it may be that reaching the SPA changes the reason that people identify for not being in (or seeking) work: the social norm is only to identify as "retired" once they have reached the SPA. These findings are in contrast to the findings from Staubli and Zweimuller (2013), who do not find increases in DI when the SPA is increased, despite the fact that rates of receiving DI around the SPA is much higher in Austria.

Table 2.5: Effect of increasing the State Pension Age from 60 to 62 on women's economic activity

	Effect of being under SPA	*Standard error*	*Average prevalence at ages 60 & 61 pre-reform*
Hours of work			
Full-time work	0.035***	[0.013]	17.7%
Part-time work	0.029**	[0.013]	23.2%
Out of work	–0.064***	[0.014]	59.1%
Economic activity			
In work	0.055***	[0.014]	40.9%
Unemployed	0.012***	[0.003]	0.8%
Inactive: Sick or disabled	0.040***	[0.008]	9.0%
Inactive: "Retired"	–0.115***	[0.012]	43.8%
Inactive: Other	0.008	[0.008]	5.5%

Notes: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level, * at the 10% level. There are 60,173 observations in both models. Standard errors are clustered at the individual level and estimated by bootstrapping with 1,000 replications. Estimates were successfully produced on all replications for the multinomial probit with three outcomes and on 997 of these replications for the multinomial probit with five outcomes. Pre-reform economic activities estimated from LFS data in 2008–09 and 2009–10.

2.4 Conclusion

Many countries have legislated to increase early or normal pension claiming ages over the last few decades, partly but not exclusively motivated by a desire to reduce the future cost of publicly-funded pension promises. A number of papers have conducted ex ante simulation of such reforms using out-of-sample predictions, which suggested quite large equilibrium effects in many countries (Fields and Mitchell, 1984; Gustman and Steinmeier, 1985; Rust and Phelan, 1997; Coile and Gruber, 2000; Blundell and Emmerson, 2007). Ex post evaluations of changes to normal and early retirement ages have tended to find, if anything, larger effects than were suggested by the ex ante simulations. Most ex post evaluations of such reforms have focused on changes to normal (rather than early) retirement ages and have found sizeable effects (for example, Mastrobuoni, 2009; Behagel and Blau, 2012; Lalive and Staubli, 2014). Three previous papers have examined the effect of changing the SPA: Börsch-Supan and Schnabel (1999) and, more recently, Staubli and Zweimüller (2013) and Atalay and Barrett (2015). In this chapter, we have used evidence from the UK to add to this small existing

literature, providing the first evidence from a country where there are only very limited financial incentives to exit work at the SPA.

In 1995, the UK government legislated to increase the earliest age at which women could claim a state pension from 60 to 65 between April 2010 and March 2020. This chapter is the first research to examine (ex post) the impact of this policy on women's economic activity at older ages, using data covering the period up to June 2014. Our results, which allow for a flexible specification of cohort effects, suggest that employment rates did increase significantly as a result of the change in SPA – by 6.3 percentage points using our preferred specification. We find statistically significant rises in both full-time and part-time female employment as a result of the reform.

In addition to the impact on employment rates, we find the policy has also led to a 1.2 percentage point increase in the fraction of women who are unemployed and actively seeking work at ages 60 and 61. We also find a 4.0 percentage point increase in the proportion of women reporting themselves to be economically inactive due to sickness or disability. These increases in employment, unemployment, and economic inactivity due to sickness/disability are offset by a large reduction in the proportion self-reporting themselves to be retired.

Subgroup analysis provides some evidence on which mechanisms may be important in explaining the changes in behavior that we observe. There is no significant difference in the response among owner-occupiers and renters, which we interpret as suggestive evidence that credit constraints may not be the primary driver. In addition, the cohort fixed effects included in our model control for differences in state pension wealth across cohorts that are a direct result of the increase in the SPA. Moreover, omitting cohort controls, or including (some) interactions between cohort and age do not materially change our results. Therefore, we can also rule out that these are the major driving force of the response we see. This is not surprising given a one-year loss of a state pension is equivalent to only 0.8% of median family wealth for these women. Finally, in contrast to increases in the SPA that have been legislated in other countries (notably Austria and Australia), the absence of any earnings test in the UK, means that there are not strong financial incentives to retire at the SPA.

Overall, we find a large impact of the increase in the SPA on female labour market behavior in the UK. Together with the evidence on the lack of importance of credit constrains, marginal financial incentives, and wealth effects in explaining the response we see, this suggests that the role of the SPA in providing a signal about the appropriate retirement age may be an important reason why increasing the SPA feeds through into such a sizeable effect on labour force participation.

Chapter 3: The effect of work on cognition and physical disability: evidence from English women

3.1 Introduction

Faced with increasing longevity in populations across the developed world, many governments are responding to the public finance pressures of an ageing population by encouraging individuals to work longer. For example, many countries have increased the eligibility age for public pensions. However, the full consequences of increased labour force participation at older ages on wellbeing, health and time use at older ages are not fully understood. The extent to which governments should continue to encourage individuals to extend their working lives should be based not only on the financial consequences of such policies, but also any knock on effects of a later retirement in other dimensions. One consequence of a longer working life may be altered levels of health, disability, and cognition, with direct consequences for individuals' wellbeing, and indirect consequences for their subsequent need for services, including those provided publicly.

There are a number of challenges when assessing the effects of retirement or work at older ages on health and cognition and there is no clear agreement from previous studies (see Banks, Chandola and Matthews 2015). This is in part because there are a wide range of health outcomes that may be affected by work at older ages (see Atalay and Barrett 2014). Some may be affected quickly and others (such as mortality – see Fitzpatrick and Moore 2018) may only occur a considerable time after retirement, and long term effects may differ from short-term effects (Bertoni et al 2018 and Celidoni et al 2017). Second, there can be substantial heterogeneity in the effect of retirement on health (see Mazzonna and Peracchi 2012, 2017, among others). While some jobs may negatively affect certain types of health, others may improve it, and while some retirement lifestyles may be good for health or cognition, others may be very poor. Finally, identifying causation from correlation can also be a challenge where health and labour supply have the potential to affect each other simultaneously.

Given the challenge of identifying causal impacts in general studies of retirement on many different health outcomes, which may vary for different types of people, jobs and lifestyles, there is an

important role for studies that examine specific mechanisms and outcomes. Our study is one of these and looks at the impact on cognitive function and physical disability of additional years of work? Our chosen health outcomes are measures of health that could plausibly react in the short term to a longer working life for people in their early 60s. To answer this question, we exploit the increase in the UK's "State Pension Age" for women (the earliest age at which they can claim a public pension), which increased the employment rate of 60-63 year old women by 11 percentage points from 2010 to 2017.

We build upon the previous literature in two key ways. First, by exploiting the gradual increase in the State Pension Age – this reform led to women born only a few months apart having different State Pension Ages – we can avoid the concerns arising from use of cross-country variation in pension eligibility as used in Rohwedder and Willis (2010), Bingley and Martinello (2013) and Coe and Zamarro (2011). This is similar to the identification strategy used in Celidoni et al (2017) and Bertoni et al (2018), though we focus on data covering one reform in one country (England), rather than exploiting reforms across 10 european countries. [21]

Second, using rich data from the English Longitudinal Study of Ageing (ELSA), we observe multiple measures of individual's health and cognition. Our analysis combines the memory recall tests used in Rohwedder and Willis (2010), Bonsang et al (2012) and others, with the verbal fluency test of executive function, as used by Coe and Zammaro (2011) and Mazzonna and Peracchi (2012). We also exploit multiple measures of physical disability that are objective but self-reported (questions on problems undertaking certain physical activities) and one that is objective and also independently measured (an individual's walking speed) and therefore not subject to, for example, justification bias. Using walking speed as an objective, independently measured, measure of physical disability builds upon Bertoni et al (2018), who used handgrip strength as a measure of health.

The substantial effects that we find imply that women who continue to work between 60 and their new – higher – State Pension Age as a result of the reform have on average significantly better

[21] Fonseca et al (2017) find that results on retirement and cognitive function are sensitive to inclusion of country fixed effects, suggesting there are important unobserved differences across countries.

cognition and fewer signs of physical disability than if they had retired at age 60. For women aged 60-63, being in paid work is found to increase cognition as measured by a delayed recall test (by around 1.5 words), though there is only a small impact on the immediate recall test. Being in employment also increases verbal fluency by around 6 words (in a minute) – compared to a pre-reform average of 23. One contributing factor to these cognition results could be that retirees lose the social participation and engagement with their colleagues when they retire, but that social participation and engagement outside of work (such as membership of clubs or societies, and seeing friends, family or children) do not rise upon retirement, which is what we find. And this is likely to be particularly relevant for single women, 63% of whom live alone, and therefore have no social engagement within their own home. Indeed, it is for this group that we see larger positive effects of work on cognition, compared to smaller (but still positive effects) on married people.

In terms of measures of physical disability, being in paid work at older ages is found to increase average walking speed by around 0.2 metres per second (m/s, compared to a pre- reform average of 1.0 m/s), results which are corroborated by substantial falls in the probability that individuals report having (moderate) mobility problems. We find evidence that one mechanism consistent with this effect is that older women do not increase the levels of other exercise (e.g. in sports or activities) and hence do not offset the loss of physical activity associated with their work. Looking further into this relationship, we find additional supportive evidence from the heterogeneity in effects across occupational groups. The overall positive effect of work on physical mobility is only a result of positive effects amongst those in more physically active occupations. But for women who work in the most sedentary occupations, being in paid work is found to have a significant negative impact on walking speed – by around 0.3 m/s. For this group we also find that being in employment leads to a reduced likelihood of reporting high levels of total exercise and an increased likelihood of reporting little or no exercise.

Taken together, these results suggest that, on average, there may be additional benefits of extending working lives beyond the financial benefits that accrue through retirement savings margins and such

positive spillovers might should be factored into policy analysis of changing retirement incentives. But any such calculations should be nuanced. Perhaps most importantly, any such effects are going to be differentially distributed across the population, and not present at all for some subgroups. Our evidence suggests work is particularly good for the health of older single women and those who do not work in sedentary occupations. But the effects for married women in sedentary occupations (which is a relatively large group) are less strong. In addition, any longer run effects of work on more distant health outcomes such as cardiovascular disease or other chronic conditions, or mortality, may exhibit different patterns. Further research might profitably identify the key health or cognitive risks and protective factors inherent in different types of jobs and in different types of retirement lifestyles, and then study the distribution of transitions between the two in the older population, in order to generate a more detailed picture.

The rest of this chapter proceeds as follows. Section 3.2 discusses the data we use. Section 3.3 sets out details of the policy reform that we exploit and the empirical methodology that we employ. Section 3.4 sets out our results and Section 3.5 concludes.

3.2 Data

We use data from the English Longitudinal Study of Aging (ELSA; see Steptoe et al 2013 and Marmot et al 2018). This is a longitudinal study of people living in England who are aged 50 and over, with interviews occurring biennially. The first "wave" of ELSA was in 2002–03, and we use data from the third wave in 2006–07, through to the eighth (and most recent) wave in 2016–17. ELSA is similar to related studies in other countries, including the Health and Retirement Study (HRS) in the United States, and Survey of Health, Ageing and Retirement in Europe (SHARE). It contains detailed information on a variety of measures of health, disability and cognitive function, as well as household demographics, economic activity, income, wealth, labour market histories and a range of other information on participation in different activities in society. Importantly, the data contain each individual's precise date of birth. This allows us to calculate the exact date at which they reached their State Pension Age – and the date that they would have reached State Pension Age in absence of

reform. The particular advantages of ELSA – relative to other survey datasets – are the relatively large sample size (for longitudinal household survey data) for individuals around retirement, and the detailed repeat information on measures of cognitive function and physical disability, which are measured in every wave (or nearly every wave) of the data.

As part of the ELSA interviews, the interviewer undertakes a number of tests of the individual's cognitive function. The tests used are carefully chosen such that they are relevant to older people's everyday functioning, sensitive to age-related decline, and that not many people get either the minimum or the maximum score (Huppert et al 2006). The first test is a "verbal learning and recall" test, in which individuals are read a list of ten words to memorise. They are then immediately asked to repeat as many of the words back to the interviewer, and five minutes later they are asked again to repeat the same ten words. This is a test of retrospective memory and is the same measure of cognition used in Rohwedder and Willis (2010) and Bonsang et al (2012), though we examine separately the effects on the immediate and delayed measures.

Steel et al (2003) provide more detailed information on these measures, and show that, on average, women, younger people, and more educated people perform better on them. They also show that as people age, their performance on the delayed recall tests declines at a faster rate than on the immediate recall test. Celidoni et al (2017) find that decline in verbal recall tests in SHARE data are highly predictive of the onset of dementia.

The second test is a test of "verbal fluency", in which individuals are asked to name as many animals as they can in one minute. This measure is studied in Coe and Zamarro (2011) among others. Verbal fluency is a measure of executive functions such as self-initiated activity, categorisation and mental flexibility, rather than memory (Huppert et al 2006). Whitley et al (2016) show average verbal fluency starts to decline rapidly after the age of 60. In addition, we create a "cognitive index" which combines the two memory tests and the verbal fluency test in an equally weighted index that takes values between 0 and 30.

There are also a set of questions in each wave of data relating to disability and physical mobility. We use an objective and independently measured assessment of physical capacity: individuals' walking speed. The interviewer measures this by timing two walks of 8 feet (2.4 metres), and we report walking speed in metres per second (m/s). This is an important outcome not only because it is objective and independently measured, but because it is known to be a measure of physical function that declines dramatically with age (Steel et al 2003).

In addition, there are a set of questions that ask individuals whether they have problems undertaking specific mobility activities, in particular assessing upper and lower limb functions (see Steel et al 2003). Three of these ("pushing or pulling large objects", "stooping, kneeling or crouching", or "climbing several flights of stairs") we categorise as "moderate" mobility problems. There are a further seven problems which we categorise as "severe" mobility problems ("walking 100 yards", "sitting for two hours", "getting up from a chair", "climbing one flight of stairs", "lifting weights", "picking up a 5p coin from a table", and "reaching or extending arms"). We mainly focus on the effects on moderate mobility problems as the group of interest is women aged 60-63 rather than older populations more likely to suffer with more severe problems.

3.2.1 The sample of cohorts of women affected by the increase in the State Pension Age

We select data from waves 3 to 8 of ELSA (from 2006/07 to 2016/17)+ on all women born between April 1948 and March 1957 which are the birth cohorts affected by the reform (a financial year in the UK runs from April to the following March). Our sample contains two birth years (1948–49 and 1949–50) whose State Pension Age was their 60th birthday and then seven years who face a higher State Pension Age (details of the reform which created this variation is provided in the next section). The youngest birth year chosen is 1956–57 as their cohort is the youngest in our sample period who are observed at ages 60 or older. There are approximately 200-250 women observed of each single year of age in each wave of the ELSA data. Overall this leaves a sample of 10,628 person-year for the set of 2,462 women.

Table 3.1 shows the average characteristics of our sample, and the standard deviations. 57% of the sample are in paid work. On average they are 58.7 years old (because we include two waves of pre-reform data), with average ages being slightly higher among those not in paid work than those still in paid work. Those in paid work are more likely to be under the state pension age than those not in paid work. 77% are married or cohabiting, a rate that does not differ across those in and out of paid work. 84% own their own home, a rate which is slightly higher for those in work. 24% of them left school before 16, while only 45% left aged 17 or older and those in work are more educated on average. Table 3.1 also shows differences in our outcomes of interest, with those in paid work, on average, having higher cognitive test score, slightly higher walking speed and reporting fewer mobility problems. Controlling for differences in age, time, birth cohort, education, marital status, and partner's age and education, being in paid work is still significantly associated with having better cognitive test scores, faster walking speed, and fewer mobility problems, as is shown in Appendix Table B.1.

Table 3.1: Descriptive Statistics of ELSA Sample, (waves 3-8, 2006/7 to 2016/17): Women born between April 1948 and March 1957

	All		**In paid work**		**Not in paid work**	
Individual characteristics:						
In paid work	0.57	[0.49]	1.00	[0.00]	0.00	[0.00]
Age	58.7	[4.2]	57.3	[3.7]	60.4	[4.1]
Under state pension age	0.71	[0.45]	0.84	[0.36]	0.53	[0.50]
Married or cohabiting	0.77	[0.42]	0.78	[0.41]	0.75	[0.43]
Home owner	0.84	[0.36]	0.89	[0.32]	0.78	[0.41]
Left school before 16	0.24	[0.43]	0.19	[0.39]	0.31	[0.46]
Left school at 16	0.31	[0.46]	0.32	[0.47]	0.29	[0.45]
Left school 17+	0.45	[0.50]	0.49	[0.50]	0.39	[0.49]
Outcomes of interest:						
Verbal fluency test score	22.6	[6.7]	23.2	[6.5]	21.9	[7.0]
Memory test score – immediate	6.6	[1.6]	6.7	[1.6]	6.4	[1.7]
Memory test score – delayed	5.5	[2.0]	5.7	[1.9]	5.3	[2.1]
Cognitive index score	17.9	[5.2]	18.4	[4.9]	17.2	[5.5]
Walking speed (m/s)	0.95	[0.26]	0.99	[0.23]	0.93	[0.27]
Any moderate mobility problems	0.42	[0.49]	0.34	[0.47]	0.53	[0.50]

Note: Standard deviations presented in brackets. Number of unique individuals: 2,462; Number of person-year observations: 10,628

We supplement our analysis using the UK "Family Resources Survey" (FRS – see Department of Work and Pensions et al 2018), which is an annual cross-sectional household survey of around 20,000 households per year. We only use households living in England to be consistent with the ELSA sample. The FRS includes information on households' incomes, and the economic activity and demographics of all members of the household (but do not contain measures of health). Since 2008–09, the FRS contains date of birth for all individuals (needed to calculate State Pension Age) and it is available up until 2016–17. Using the same birth cohorts of women as in our ELSA sample, there are 17,858 unique women in our sample from the FRS.

3.3 Empirical methodology

Estimating the effect of employment at older ages on physical and cognitive function is complicated by the potential simultaneity of the relationship between employment, and cognitive and physical function. While employment at older ages may affect these outcomes directly, better physical mobility and cognition may also affect when individuals retire. We therefore use an instrumental variables approach to estimate the causal effect of work at older ages on physical disability and cognitive function. We exploit the policy-induced increase in employment of women aged 60 to 63 between 2010–11 and 2016–17 that was caused by the gradual increase in the State Pension Age for women.

3.3.1 The increase in the state pension age for women

The State Pension Age is the earliest age at which an individual can receive a state pension in the UK. Unlike in many social security systems, the State Pension Age is the only focal age in the UK state pension system. This makes it difficult to directly compare our work to Celidoni et al (2017) and Bertoni et al (2018), who distinguish between retirement at "early" and "statutory" retirement ages, as in the UK early and statutory retirement ages are the same.

Between 1948 and 2010, the state pension age for women was 60. At the State Pension Age, the state pension can be claimed, or it can be deferred in return for an increased pension payment, although this is rarely done for long. In 2015–16, a full basic state pension was £116 per week (£6,000 or $7,800

per year).[22] At around 27% of median weekly earnings this provides a relatively low replacement rate for most people. There is no earnings test on the state pension, but it is subject to income tax.

The 1995 Pensions Act legislated to increase the State Pension Age for women gradually from 60 to 65 between 2010 and 2020. The State Pension Age rose by one month for each month of birth after March 1950. The implication of this is that women born only a few months apart have discrete differences in the age that they can first claim a state pension. While those born in March 1950 had a State Pension Age of 60, for those born in April 1951, their State Pension Age was 61, and for those born in April 1952 it was 62. The 2011 Pensions Act accelerated the increase in the State Pension Age to 65 (for those born after 5th April 1953), and increased it to 66 (for those born after 5th October 1954). The resulting State Pension Ages for women born between 1950 and 1955 are shown in Appendix Figure B.1.

3.3.2 Using the increase in State Pension Age as an instrument for employment

We estimate the effect of being in paid work at older ages on disability and cognition using (two-sample) two stage least squares, using an indicator for being under or over her State Pension Age as an instrument for employment. Our first stage is set out in equation 3.1, and our second stage in equation 3.2. In the first stage, we regress an indicator of being in paid work *W*, for individual *i* in period *t*, on an indicator for whether she is under or over her State Pension Age (*underSPA)*, controlling flexibly for age using 18 single-year-of-age dummy variables ($\sum_a \delta_a[age_{it} = a]$), 46 dummy variables for time measured in years and quarters, ($\sum_t \mu_t[time_{it} = t]$), a variable that controls linearly for individuals year of birth (their "cohort" *C*), and a vector of other control variables.[23] This is very similar to the empirical model estimated in the previous chapter, but with a little more

[22] Some qualify for an additional earnings-related state pension, worth up to £160 ($208) per week in addition to the basic state pension, but most employees opted out of this system in return for lower payroll taxes and accumulated a private pension instead.

[23] This comprises five dummies for relationship status (married, cohabiting, widowed, divorced, separated) with baseline of single (never married), a dummy for leaving education at age 16 or later, 10 dummies for region, a dummy for partner's education, a quadratic in partners' age, a dummy for partner being aged 60-64, and a dummy for partner being over his SPA.

structure on the cohort trend, and estimated on different datasets. We present results with and without the cohort variable; but it does not make a substantive difference to the results.

We use a two-sample 2SLS estimator as we use ELSA and FRS data pooled together in the first stage and only ELSA data in the second stage. We estimate our standard errors clustering at the individual level, using the method set out by Pacini and Windmeijer (2016) for robust inference for the two-sample 2SLS estimator.

$$W_{it} = \alpha(underSPA)_{it} + \sum_a \delta_a[age_{it} = a] + \sum_t \mu_t[time_{it} = t] + \pi_1 C_i + \gamma_1 X_{it} + \varepsilon_{it} \quad (3.1)$$

$$y_{it} = \beta \widehat{W}_{it} + \sum_a \varphi_a[age_{it} = a] + \sum_t \mu_t[time_{it} = t] + \pi_2 C_i + \gamma_2 X_{it} + \upsilon_{it} \quad (3.2)$$

Equation 3.1 identifies the effect of being under State Pension Age on employment using difference-in-difference estimation. Being under the State Pension Age is an interaction between a woman's age and the time at which she is observed. We therefore must assume the common trends assumption that – in absence of the reform, the employment rate of women of different ages would have changed in the same way over time. This assumption rules out spillovers of the reform onto otherwise unaffected women, and that no other shocks hit those groups who were affected by the increased pension age but not those whose pension age was unaffected.[24]

The second stage of the instrumental variables methodology is shown in equation 3.2, where the outcome *y* (e.g. cognitive test score) is regressed on the predicted probability in being in work from equation 3.1 ($\widehat{W}$), and the same controls for age, time, cohort and other control variables as in the first stage. Our key assumption is that being under the State Pension Age only affects cognition and

[24] This assumption is untestable in period of the reform. However, to test for common trends prior to the reform, Appendix Table B.4 presents "placebo tests" which imagine that the reform had occurred 4, 6, or 8 years earlier, and tests whether there was an impact on employment of this "placebo reform". We find no effect, suggesting that there are indeed common trends in employment prior to the reform. Pre-reform common trends in employment rates of older women were found in Chapter 2 using a different UK dataset – the Labour Force Survey.

physical disability through its effects on employment, and through no other channel: we discuss the implications of any violation of this assumption alongside the relevant results.

3.4 Results

3.4.1 The effect of the increase in the state pension on the employment of women

Table 3.2 shows our first stage estimates, i.e. the effects of being under the State Pension Age on the probability of being in paid work, as set out in equation 3.1. Columns (1) and (2) show that, using ELSA data only, being under the State Pension Age increases the probability of 60-63 year old women being in paid work by around 10 percentage points, with a standard error of 2.4 percentage points.[25] This compares to a pre-reform (2009–10) employment rate of around 40% for 60-62 year old women. Columns (3) and (4) show that incorporating data from the Family Resources Survey makes little difference to the estimated size of the effect (which is 11 percentage points) but the substantial increase in the number of individuals in our estimation sample means that there is considerably better precision, with a standard error of 1.4 percentage points and F-stat of around 60. In addition whether or not a liner control for birth cohort is included in the model makes very little difference to the estimated size of the effect of being under the SPA on paid work (as shown by comparing column 1 to column 2, and column 3 to column 4).

Table 3.2: Effect of being under the State Pension Age on probability of being in paid work

	(1)	(2)	(3)	(4)
Effect of being under SPA	0.097***	0.095***	0.108***	0.106***
Standard error	[0.024]	[0.024]	[0.014]	[0.014]
F-stat	17.0	16.0	61.9	59.6
Observations	10,628	10,628	28,482	28,482
Controls for linear cohort (financial year of birth)	No	Yes	No	Yes
Datasets	ELSA only	ELSA only	ELSA and FRS	ELSA and FRS

Notes: Control variables included in regression as set out in section 3.3 (though specifications 1 and 3 do not include a linear control for financial year of birth). Estimated by Ordinary Least Squares. Standard errors, shown in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level.

[25] These are similar effects on labour supply to those found by Cribb and Emmerson (2019a) and a little larger than those found in Chapter 2.

Source: Authors' calculations using the English Longitudinal Study of Ageing and Family Resources Survey.

3.4.2 The effect of work on cognition and physical disability

Table 3.3 shows the estimated effect of being in paid work on cognition and physical disability. The results for cognition suggest that there are large and positive effects of being in employment on cognition for those who continue in paid work as a result of a higher State Pension Age. While there is some evidence of positive effects of employment on immediate memory test (0.5 to 0.7 additional words), there are large and significant effects of being in work on the delayed memory test – of around 1.5 to 1.8 additional words. This effect is larger than the effect of work on memory tests found by Bonsang et al (2012) – who found an effect of around 1 extra word recalled when the immediate and delayed tests were combined, but substantially smaller than the effects in Rohwedder and Willis (2010) who found an effect of employment of around 4.7 additional words.

Being in paid work also increases the verbal fluency score (number of animals mentioned) by around 6 animals, compared to a pre-reform average of around 23 animals (and a standard deviation of around 7). This is in contrast to Coe and Zamarro (2011) who do not find a significant effect of retirement on verbal fluency. Overall, the combined cognitive index score increase by around 5.5 points, around a one-standard deviation effect, and compared to a pre-reform average of 18. This large effect is statistically significant at the 1% level.

This effect on the cognitive index is considerable compared to the pre-reform distribution, and larger than the naïve OLS estimate (shown in Appendix Table B.1). Appendix Tables B.2 and B.3 show that the effect is larger than the difference in average cognitive index between the average score for the top and bottom income decile for women in our sample, and similar to the difference between the average score for the top and bottom (non-pension) wealth decile. Although the standard errors show there is some uncertainty around these estimates, the lower 95% confidence interval for the effect on the combined cognitive index is around 3 – meaning that we can rule out not only negative effects on cognition, but also small and even moderate positive impacts on cognition. Of course, these effects

are Local Average Treatment Effects, and therefore one interpretation of these large effects are that women who would be better off in paid work (in terms of their cognition at least) may be those who continue to work in response to the higher State Pension Age.

Table 3.3 also shows that – for those women who work longer as a result of a higher State Pension Age – staying in paid work decreases physical disability. Staying in employment increases walking speed by around 0.2 m/s – compared to a pre-reform average of around 1.0 m/s. This objective and independently measured indicator of physical capacity is corroborated by a substantial decrease in the probability of women reporting having any of the three moderate mobility problems – by around 50 percentage points. These increases in mobility are driven by reductions in people reporting two of the three specific moderate mobility problems: difficulties stooping, kneeling and crouching, and difficulties climbing several flights of stairs. We also look at the effect on the probability of reporting the more severe mobility problems, but find no significant evidence of any effects.

Table 3.3: Effect of being in paid work on measures of cognition and physical disability

Outcome	(1)	(2)	Pre-reform averages for 60-63 year old women	Number of observations
Verbal fluency test score	6.24***	6.23***	23.19	8,490
	[1.86]	[1.89]	[7.38]	
Memory test – immediate	0.50	0.74**	6.61	10,296
	[0.34]	[0.35]	[1.57]	
Memory test – delayed	1.47***	1.76***	5.52	10,309
	[0.43]	[0.45]	[1.95]	
Cognitive index	5.35***	5.78***	18.15	8,480
	[1.38]	[1.43]	[5.40]	
Walking speed (m/s)	0.19***	0.21***	0.98	3,991
	[0.06]	[0.07]	[0.27]	
Probability of any moderate mobility problems	–0.50***	–0.52***	0.49	10,628
	[0.12]	[0.12]	[0.50]	
Controls for linear cohort	No	Yes		

Notes: Exogenous control variables as set out in section 3.3 (though specification 1 does not include a control for financial year of birth). Estimated by Two Sample Two Stage Least Squares. Standard errors, reported in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level and ** at the 5% level.
Source: Authors' calculations using the English Longitudinal Study of Ageing and Family Resources Survey.

The precision of the results suggests that we can rule out not only negative impacts of work on disability, but we can also rule out small and even moderate improvements in physical disability. Appendix Tables B.2 and B.3 suggest that the effects on walking speed are similar to the average difference in walking speed between those in the highest income decile and those in the lowest income decile (0.2 m/s), though smaller than the difference between the top and bottom wealth distribution (0.3 m/s).

These results, which use an instrumental variables methodology, are identified on the base of our assumption that a higher State Pension Age only affects cognition and disability through its effect on employment. There are two potential threats to this assumption. The first is that the higher State Pension Age involves a reduction in state pension income paid to women who are no longer eligible for the state pension at age 60 and it is this income loss that is the cause of the effects observed. Cribb and Emmerson (2019a) estimate that state benefit income (including the state pension) falls by an average of £82 ($107) per week for 60-62 year old women for the period before they reached their higher State Pension Age as a result of the reform. Although this is significant, it is very unlikely that this magnitude of income change to have a material impact on either cognition or disability. A £82 per week fall in benefit income is a little smaller than the £100 per week difference between average income in the 4th and 6th deciles of the income distribution.

But, as is shown in Appendix Table B.2, there the average cognitive index rises by about 0.1 this income range compared to an average index of 17.9. There is also only a 0.04 m/s rise in average walking speeds between these two deciles, compared to an average of 1.0 m/s and essentially no change in reports of moderate mobility problems.[26] Therefore, the fall in state pension incomes because of the reform are very unlikely to affect our results. Moreover, if they did, the falls in state

[26] An alternative calculation is that there is decrease in wealth resulting from a three year increase in the State Pension Age, of around £12,800 (3x £4,300). If people are not credit constrained, it is their wealth that will drive behaviour (Grossman 1972). Appendix Table B.2 shows that differences in (non-pension) wealth which are much larger than this across the wealth distribution are associated with only small differences in – with the exception of the very lowest wealth quintile, who have much worse cognition and disability than the rest

pension income would be likely to worsen cognition and disability, but we find substantial *positive* impacts of employment (driven by the reform) on cognition and disability.

A second possible threat is that there could be an unpleasant surprise among some women that they face a State Pension Age higher than 60, and that there may be a sense of "injustice" at facing a higher State Pension Age than those women who were born a few years earlier (see De Grip et al 2012). This channel may well be relevant for some health outcomes, such as mental health or subjective general health; Carrino et al (2018) find worse mental health as a result of the higher State Pension Age. But it is unlikely that this channel would affect cognition or independently measured physical performance. Again, if it did bias our results, then this would imply that that true effect would be an even larger positive impact of work on cognition and physical disability than we estimate.

In order to check the robustness of these results, we conduct some placebo tests. To do this, we imagine that the reform – the increase in the State Pension Age for women – was implemented either 4, 6 or 8 years previously. We do this by estimating the effect of the instrument (being under the State Pension Age) on the outcomes of interest, where the instrument is defined as if the reform was introduced earlier. The results of these placebo tests – shown in Appendix Table B.4 – find that, while there are positive effects of being under the State Pension Age on cognition and physical disability when the pension age was actually increased, there is no evidence that these outcomes are changing in response to the placebo reforms 4, 6 or 8 years earlier. This helps support our conclusion that our results are not the result of variation in outcomes that happens to correlate with the increase in the State Pension Age.

3.4.3 Mechanisms for paid work improving cognition and physical disability

One reason for the increases in physical mobility could be if paid work involves a degree of physical activity that is not replaced when people move out of work and retire. Table 3.4 provides evidence that is consistent with this. Using the same IV method, it estimates the effect of being in paid work on the probability that individuals report undertaking various levels of exercise (none/low, medium or

high). The question in ELSA asks: "*We would like to know the type and amount of physical activity involved in your daily life. Do you take part in sports or activities that are vigorous/moderately energetic/mildly energetic."* It subsequently asks about the frequency of undertaking of such exercise.

We use these questions on intensity and frequency to categorise people into three groups depending on whether they undertake no/low amounts of exercise, medium, or high amounts. The exact coding of these variables, and the specific activities that are prompted are listed in Appendix Table B.5. The prompting highlights sports (such as swimming or running) or activities (such as gardening or vacuuming) that are usually thought of as leisure activities, or activities of home production, rather than picking up physical activity undertaken at work. We find no statistically significant evidence of changes in the amount exercise as a result of staying in work longer due to the higher State Pension Age, implying that a failure upon retirement to replace physical activity at (or going to) work with physical activity in sports, activities or housework may be one reason that being in employment on average improves walking speed and mobility compared to being retired.

Table 3.4: Effect of being in paid work on probability of undertaking different levels of exercise

Amount of exercise:	(1)	(2)	Pre-reform averages for 60-63 year old women
None or low	0.05	0.10	0.27
	[0.09]	[0.09]	[0.44]
Medium	–0.11	–0.11	0.44
	[0.11]	[0.11]	[0.50]
High	0.06	0.01	0.29
	[0.10]	[0.10]	[0.46]
Controls for linear cohort (financial year of birth)	No	Yes	

Notes: Number of observations: 10,621. Exogenous control variables as set out section 3 (though specification 1 does not include a control for financial year of birth). Estimated by Two Sample Two Stage Least Squares. Standard errors, reported in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level.
Source: Authors' calculations using the English Longitudinal Study of Ageing and Family Resources Survey.

In addition, when many people leave work they lose the social engagement with colleagues who they would previously interact with on a regular basis. Higher levels of social isolation, and lower levels of social participation in societies or clubs, are associated with worse cognitive function in older age (Donovan et al 2015; Bourassa et al 2017). One reason for work maintaining cognitive function might

be that when people retire they do not substitute engagement with colleagues for engagement with other people or participation in other activities. This story is consistent with the results of Table 3.5 which shows that we do not find any statistically significant effect on being in work on seeing children, friends or family, or on the number of clubs or societies (such as social clubs, churches or other religious organisations, sports clubs etc). In addition, we create a "social isolation index", which is a count variable (of between 0 and 4), which the sum of the three dummy variables recording whether they see children, family and friends at least weekly, and a dummy variable for being in a member of at least one society). We find no evidence of an effect of being in paid work on this social isolation index.

Table 3.5: Effect of being in paid work on measures of social participation and isolation

Outcome	(1)	(2)	Pre reform average for 60-63 year old women	Number of observations
See children at least weekly	0.083 [0.103]	0.126 [0.106]	0.533 [0.501]	8,644
See family at least weekly	–0.052 [0.101]	–0.112 [0.105]	0.388 [0.489]	9,144
See friends at least weekly	–0.075 [0.110]	–0.111 [0.113]	0.543 [0.500]	9,203
Number of societies or clubs individual is a member of	–0.198 [0.281]	–0.258 [0.285]	1.33 [1.38]	8,942
Social isolation index	–0.028 [0.235]	0.057 [0.238]	1.91 [1.03]	8,130
Linear controls for cohort	No	Yes		

Notes: Estimated by Two Sample Two Stage Least Squares. Exogenous control variables as set out in section 3.3 (including control for financial year of birth). Standard errors, reported in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level.
Source: Authors' calculations using the English Longitudinal Study of Ageing and Family Resources Survey.

3.4.4 Heterogeneity in the effect of work on cognition and physical disability

All the results so far have shown *average* effects for women aged 60-63 who remained in paid work as the results of a higher State Pension Age. However, as is emphasised by Eibich (2015) and Mazzonna and Peracchi (2017), there is potentially significant variation in the effect of work or

retirement on measures of health at older ages. We therefore investigate the extent to which the effect of paid work on cognition and disability vary by two key characteristics: marital status and the physicality of their occupation (in their current or last job).

To use our Two Sample 2SLS methodology we are restricted to examining heterogeneity that can be measured in both the FRS and ELSA. Given that there is no direct measure of physicality of job in the FRS, we measure the physicality at the 1 digit Standard Occupational Classification level for women in our ELSA sample – as shown in Appendix Table B.6. We class one occupational group (administrative occupations) as "sedentary", with 85% of working women in this group reporting that they have a sedentary job. We class three occupational groups (managerial, professional and associate professional) as "partly sedentary" as between 40% and 60% of working women in each occupational groups report being in sedentary jobs. The remaining five occupational groups have less than 40% of working women reporting being in a sedentary job in each group (and an average of 14% sedentary) and are classed as "non-sedentary". In this way we use the information on individual's occupational group (in either their current or last job), to assign them to a group based on average reported physicality of jobs in that group.

Table 3.6 shows that there is very little difference between the effect of a higher State Pension Age on those in sedentary occupations and those in more physically active occupations, although those in non-sedentary occupations are slightly more likely to have continued to work past age 60 as a result of the higher SPA. It also shows that there is essentially no difference in the effect of a higher State Pension Age on the employment rates of those who are married (or cohabiting) and those who are single (i.e. never married, divorced, separated or widowed).

Table 3.6: Heterogeneous effects of being under the State Pension Age on probability of being in paid work, by marital status and physicality of occupation

	Effect of being under SPA	Standard error	Number of observations	F-stat
All	0.106***	[0.014]	28,482	59.6
Marital/relationship status				
Married	0.104***	[0.016]	21,051	41.2
Single	0.110***	[0.026]	7,431	17.4
Physicality of occupation				
Sedentary occupation	0.110***	[0.035]	4,480	9.7
Partly sedentary occupation	0.096***	[0.030]	6,417	10.4
Non-sedentary occupation	0.127***	[0.026]	8.025	23.8

Notes: Control variables included as set out in section 3.3 (including financial year of birth). Estimated by Ordinary Least Squares. Standard errors, reported in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level.
Source: Authors' calculations using the English Longitudinal Study of Ageing and Family Resources Survey.

Table 3.7 shows heterogeneity in the effects of being in employment on cognition and disability. One disadvantage of splitting the sample is that the standard errors are larger and the estimated effects are less precise. Nevertheless, there are some clear results.

The effect of employment on cognition is consistently more positive for those who are single than those who are married across all of the cognitive test scores. For the verbal fluency test, there is a very large (19.1 points) effect on singles compared to only 1.6 points for married, significantly different from each other at the 1% level. This corroborated by large and statistically significant (at the 1% level) differences in the immediate recall test (3.3 for singles vs –0.3 for married), though the larger effect on singles for the delayed recall is not statistically significant. The effect of work on the combined cognitive index is 11.9 points for singles and 3.7 for those who are married (significantly different from each other at the 10% level): i.e. those without a partner benefit more than those in couples in terms of improvements in cognition arising from remaining in paid work. One possible reason for this is that 63% of single women in our sample also live alone, meaning that they do not

have any social interaction within their household. Potentially unsurprisingly, there are no differences in the effect of work on cognition by the average physicality of the occupation.

Table 3.7 shows that there are also no differences in the effect on physical disability by marital status. However, while there are substantial, and statistically significant, positive effects of work on walking speed for those in non-sedentary, or partly sedentary occupations, there are substantial, and statistically significant, negative effects on walking speed for those working in sedentary occupations (that is administrative occupations, the most common of which for women in our sample are secretary, receptionist and clerk). Being in paid work for sedentary occupations is found to reduce walking speed by 0.29 m/s, compared to increasing it by 0.26 m/s to 0.46 m/s for the less sedentary jobs. This finding is not mirrored by increases in the number of reported mobility problems for this group, which may suggest that the questions on the three moderate mobility problems do not pick up some important changes in physical disability around retirement.

Table 3.7: Heterogeneous effects of being in paid work on cognition and physical disability, by marital status and physicality of occupation

	Marital status		Physicality of occupation		
	Married	Single	Sedentary	Partly sedentary	Non-sedentary
Verbal fluency	1.63	19.05***	6.08	3.84	6.11**
	[2.02]	[6.09]	[4.04]	[3.55]	[2.74]
Memory test – immediate	–0.29	3.26***	0.99	–0.05	0.77
	[0.40]	[1.10]	[0.73]	[0.71]	[0.50]
Memory test – delayed	1.31***	2.63**	1.45	1.18	1.95***
	[0.50]	[1.10]	[0.93]	[0.85]	[0.68]
Cognitive index	3.65**	11.91***	6.31**	3.92	4.85**
	[1.52]	[4.13]	[3.14]	[2.69]	[2.06]
Walking speed	0.12*	0.39**	–0.29**	0.26*	0.46***
	[0.07]	[0.17]	[0.14]	[0.14]	[0.13]
Any moderate mobility problems	–0.54***	–0.60**	–0.65**	–0.34	–0.48***
	[0.15]	[0.27]	[0.30]	[0.23]	[0.18]

Notes: Exogenous control variables as set out in section 3.3 (including financial year of birth). Estimated by Two Sample Two Stage Least Squares. Standard errors, reported in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level.
Source: Authors' calculations using the English Longitudinal Study of Ageing and Family Resources Survey.

Table 3.8 shows that the negative effect on work for those in sedentary occupations could be due to the effect of work (compared to retirement) on the amount of exercise taken. We find that this group of older women are much less likely to undertake high levels of exercise (64 percentage points less likely), and much more likely to undertake little or no exercise (by 67 percentage points) when in work. In comparison, there is no evidence of significant changes in exercise for partly sedentary occupations, and evidence that work leads to more exercise being done for non-sedentary workers, helping to explain the particularly large increase in walking speed arising from them remaining in work.

Therefore with work reducing the amount of exercise for sedentary workers, and little physical activity undertaken at work itself, for these workers, being in a sedentary job is bad for their walking speed. These results are consistent with other studies which find that retirement leads to an increase in physical activity for sedentary workers, but a decrease in physical activity for manual workers (see Barnett et al 2012). It also highlights the importance of recognising that the effects of work (or retirement) on health will be importantly determined by the kind of work undertaken, and the way that people spend their time in retirement, and these may differ substantially across different groups.

Table 3.8: Effect of being in paid work on probability of undertaking difference levels of exercise, by physicality of occupation

	Physicality of occupation:		
Amount of exercise:	Sedentary	Partly sedentary	Non-sedentary
None/low	0.67**	0.14	–0.19
	[0.29]	[0.17]	[0.15]
Medium	–0.03	–0.03	–0.18
	[0.22]	[0.23]	[0.16]
High	–0.64**	–0.10	0.38**
	[0.28]	[0.21]	[0.15]
Number of observations	2,526	3,487	4,153

Notes: Number of observations: 10,621. Exogenous control variables as set out section 3.3 (though specification 1 does not include a control for financial year of birth). Estimated by Two Sample Two Stage Least Squares. Standard errors, reported in brackets, are clustered at the individual level. ** denotes that the effect is significantly different from zero at the 5% level.
Source: Authors' calculations using the English Longitudinal Study of Ageing and Family Resources Survey.

3.5 Conclusion

Many governments have implemented, or are actively considering, policies designed to increase retirement ages, such as increasing state (public) pension claiming ages. Higher employment rates as a result of these kind of reforms may have important impacts on people's health. However, despite a considerable literature on the effect of retirement on health, there is no clear consensus, as health can be measured in many ways, there may be heterogeneous effects and there is the key challenge that health and labour supply decisions are likely to be simultaneously determined. This chapter examines the effect of a longer working life (and therefore a delayed retirement) using an instrumental variables

approach on two key measures of health that can plausibly change quickly in response to retiring: measures of cognitive function and physical disability.

There are two key contributions of this chapter over the existing literature. First, rather than exploiting differences in pension claiming ages across countries, we exploit a reform – the gradual increase in the State Pension Age for women in the UK since 2010 – that means that many women born only a few months apart face very different ages at which they can first receive a state pension. Second, by using detailed data from the English Longitudinal Study of Ageing, we have multiple tests of cognitive function, and use the independently-measured walking speed as a measure of mobility/disability in addition to self-reported mobility difficulties.

Using the increase in employment induced by the increase in the State Pension Age, we find that, for women aged 60-63, being in paid work increases cognitive function by around 5 points on a scale between 0 and 30, equivalent to around a 1 standard deviation effect. We find significantly larger effects for women who do not live with a partner. One contributing factor could be that retirees – particularly those who live alone – lose the social participation and engagement (which is associated with better cognition) with their colleagues when they retire, but that social participation and engagement outside of work (such as membership of clubs or societies, and seeing friends, family or children) do not rise upon retirement.

Paid work at older ages is also found to increase walking speed on average by around 0.2 m/s (compared to a pre-reform average of 1.0 m/s), results which are corroborated by substantial falls in the proportion reporting they have moderate mobility problems. We find heterogeneity, and not just in the magnitude of the impact of being in paid work on mobility but in the direction of the impact: for women working in sedentary occupations, being in paid work *reduces* their walking speed by around 0.3 m/s. This is likely to be caused by the fact that we find that work leads to substantially less exercise for workers in sedentary occupations.

Chapter 4: What happens to workplace pension saving when employers are obliged to enrol employees automatically?

4.1 Introduction

There are concerns about individuals undersaving for retirement in countries across the developed world. In response, policymakers have proposed – and economists have studied – a large variety of schemes designed to increase saving for retirement. One instrument to boost saving for retirement of particular interest is 'automatic enrolment' (sometimes termed default enrolment or 'opt-out' design) where employers enrol employees automatically into a workplace pension scheme, which employees can then choose to leave.

Although automatic enrolment has been prominently highlighted as a particular success story of the real-world implementation of the insights of behavioural economics (Benartzi and Thaler, 2013; Madrian, 2014; Chetty, 2015; Thaler, 2016), until now almost all the evidence on the impact of automatic enrolment on participation in employer-provided pensions and pension saving comes from the voluntary introduction of automatic enrolment by large firms in the United States (Madrian and Shea (2001) and Choi et al. (2004); though Chetty et al (2014) study a case in Denmark). In many cases, these firms have introduced automatic enrolment to comply with the Internal Revenue Service's non-discrimination rules (see Choi et al. 2002 and Butrica and Karamcheva 2015).[27] However, the effects of automatic enrolment on pension saving when employers do not voluntarily choose to introduce it (and are instead are obliged by government to enrol their employees automatically) may be different – particularly if employers take action to limit pension participation, such as by providing low employer pension contributions.

[27] These rules essentially say that employee benefits cannot be only provided to highly-paid employees. Since low-paid employees are less likely to enrol in a pension plan (and receive an employer contribution to the pension scheme) than their higher-paid colleagues, this risks companies failing to comply with the non-discrimination tests. Brady (2007) examines the incentive that firms have to introduce automatic enrolment in order to pay higher-earning employees a larger fraction of their compensation in pension benefits.

This chapter studies the effect of obliging employers to enrol their employees into a workplace pension, using the first ever introduction of such a policy on a nationwide scale. Specifically, we exploit the phased roll-out, by employer size, in the United Kingdom of the obligation for employers to enrol most of their employees into a pension automatically since 2012 to estimate its effect on saving in a workplace pension by private sector employees. The roll-out from largest to smallest employers introduces exogenous variation in the timing of when employers were obliged to enrol their employees in a workplace pension scheme, which we exploit using a difference-in-differences methodology. This chapter provides the first assessment of the impact of automatic enrolment that allows for changes in employer behaviour in response to the policy in a context where those employers did not choose to introduce automatic enrolment. This kind of impact cannot be identified when automatic enrolment is introduced voluntarily by some large employers.

We find that obliging employers to implement automatic enrolment in the UK led to large increases in the pension participation rates and in the total contributions to workplace pensions. For targeted private sector employees, automatic enrolment led to an increase of 36 percentage points in the probability of participating in a workplace pension scheme. By 2015, 88% of targeted private sector employees who were enrolled automatically were a member of a workplace pension. The largest effects on pension participation are for those with the lowest participation rates prior to automatic enrolment: those in their 20s, lower-paid employees, those who have joined their employer more recently, and those employed in industries with low pre-reform rates of pension participation.

The policy is found to have increased the total contribution rate to a workplace pension by 1.05% of earnings, compared with a pre-reform average of 7.0%, therefore significantly boosting pension saving. One of the reasons that automatic enrolment has increased workplace pension savings is that employers' decisions have boosted – rather than mitigated – the impact on pension savings. We find some employers enrolling their employees into pension schemes with employer contributions well above the minimum contributions mandated by the government. The effect on pension saving was also increased due to substantial spillover effects of the policy. We find that the pension participation

rates of workers who were not obliged to be automatically enrolled increased by 18 percentage points on average, thereby more than doubling the pension participation rate among this group.

There are a number of important reasons that we would expect automatic enrolment to increase workplace pension participation. First, by defaulting employees into a pension scheme, automatic enrolment solves the procrastination problem (in which people think that in the future they will save for retirement, but they are naive and continually put it off (O'Donoghue and Rabin 1999)). Second, because automatic enrolment involves default contributions and asset allocations, automatic enrolment makes the decision to participate less complex. Since complexity of a decision is known to lead to individuals putting off the decision to join (Tversky and Shafir, 1992), automatic enrolment should therefore lead to higher participation. Third, Beshears et al. (2009) argue that individuals may see the default options under automatic enrolment as implicit advice on the best course of action. This endorsement (from either the employer or government) may lead to more people participating (and at the default contributions). Finally, there are some individuals who are automatically enrolled in a pension who were not previously offered an employer contribution to their pension, and now are offered at least 1% of qualifying earnings. These individuals may choose to be in their workplace pension in order to be able to receive the (albeit small) employer contribution that they were not offered before.

Previous empirical evidence on the impact of automatic enrolment has been based on its voluntary introduction largely by employers in the US. Madrian and Shea (2001) compare two cohorts of employees at a large healthcare firm in the US, where the cohort hired later were enrolled automatically into a 401(k) scheme with a 3% default employee contribution. They find that participation rates in the pension scheme increased dramatically for the cohort that were enrolled automatically, with 86% of employees enrolled in the 401(k) after 3–15 months, compared with only 37% of those who were not subject to automatic enrolment. Moreover, they find that the 3% default contribution was extremely salient; almost 65% of the cohort eligible for automatic enrolment had contributions equal to the default rate, and the proportion of the cohort with higher contributions fell, implying that automatic enrolment led to some employees contributing less than they would have in

the absence of automatic enrolment. Importantly, Chetty et al. (2014) use Danish administrative data and find that only 15% of people respond actively to automatic pensions contributions (by reducing other saving), implying that, for most people, higher pension saving due to automatic contributions is not offset by reductions in other saving.

These results are of particular interest because automatic enrolment is becoming more popular among public policy makers internationally. Prior to its introduction in the UK, a small number of countries have introduced policies involving automatic enrolment, but they have been either partial or temporary (in Chile) or alongside a raft of other savings policies (in New Zealand). However, by 2017 several states in the United States (California, Connecticut, Illinois and Oregon) had legislated to introduce automatic enrolment (Munnell et al., 2016) while since 2018 new service recruits to the US military have been auto-enrolled into the Thrift Savings Plan (see Executive Office of the President 2016). Many other developed countries are also planning on (or actively considering) introducing automatic enrolment, including Germany, Ireland and Poland.

The remainder of this chapter proceeds as follows. Section 4.2 describes the institutional setting for pensions in the UK and the details of the introduction of automatic enrolment there. Section 4.3 describes the data used in this chapter, Section 4.4 sets out the empirical strategy and Section 4.5 presents the results. Section 4.6 concludes.

4.2 Policy background

4.2.1 The UK pensions policy environment

Here we briefly set out the UK pensions policy environment. Unlike in many European countries, the UK government does not provide a pension at a level that provides high 'replacement rates' for individuals who are retiring after a full working life. A full public pension (known as a 'state pension') in the UK is £155.65 ($202) per week (in 2016–17), equivalent to just under 30% of median

full-time earnings.[28] Entitlement is accrued either by paying a payroll tax or by undertaking other 'qualifying' activities such as caring for children or receiving out-of-work benefits. The state pension received is not related to the earnings of the individual during their lifetime. State pensions are not means-tested or subject to an earnings test, but are taxable and can only be claimed from the 'state pension age', which in April 2016 was 65 for men and 63 for women and is rising over time so that it will reach age 66 for both men and women in October 2020. Those reaching the 'state pension age' with more than 10 years' qualifying activities are entitled to some state pension, with the full amount payable to those who have amassed 35 years of contributions. As a result public spending on state pensions in the UK is 5.6% of GDP, which is slightly lower than the US (6.7%) and substantially lower than most other developed countries in continental Europe (OECD, 2015).

Given this relatively low level of the state pension, income from private pensions makes up a large proportion of income in retirement for individuals in the UK. Crawford and O'Dea (2012) find that, in 2008–09, median private pension wealth of those between 50 and the state pension age was £90,700 ($118,000), while 25% of individuals had private pension wealth of over £237,800 ($309,000). Individuals who contribute to a pension scheme (and/or receive contributions from their employer) typically make contributions to their pension scheme before income tax and any return from the investments is re-invested untaxed in the pension. Income tax is paid upon drawing a pension, although up to one-quarter of the pension pot can be taken free of income tax.

While a large majority of public sector workers are active members of an employer-provided pension scheme (85% in 2012), only 36% of private sector employees were in 2012; the latter figure had fallen from 50% in 1997 (Cribb and Emmerson, 2016). Prior to October 2012 (when automatic enrolment started), all employers with five or more employees were obliged, if requested by an employee, to facilitate participation in a pension scheme where employees' contributions could be

[28] We describe here the state pension system from April 2016, which will be the system for the vast majority of employees currently working in the UK.

deducted directly from employees' pay packets, although employers were not obliged to make contributions to a scheme.

Finally, it should be noted that prior to the introduction of automatic enrolment by the government, it was relatively uncommon for private sector employers to enrol their employees automatically into a pension scheme. McKay (2006) finds that only 4% of private sector employers (representing 16% of private sector employees) enrolled their workers automatically into a scheme in 2005.

4.2.2 Introduction of automatic enrolment in the UK

Following the recommendation of the independent 'Pensions Commission' in 2005, the UK government legislated in the Pensions Act 2008 to oblige employers to enrol most of their employees automatically into a workplace pension scheme with at least a minimum level of contributions. A workplace pension scheme is a scheme that is facilitated – but not necessarily run – by the employer. The introduction of automatic enrolment was recommended due to a falling proportion of employees saving in a pension and fears of undersaving for retirement (see Pensions Commission 2005). The obligation to enrol most employees automatically has been introduced gradually, starting in October 2012. Here we set out the details of the policy as they were implemented by the government.

Employees are 'targeted' if they are aged at least 22, are aged below the state pension age and earn more than a given earnings level.[29] Since April 2014 this level has been set at £10,000 ($13,000) per year. Employers can postpone automatically enrolling new employees for up to 3 months. Once automatic enrolment is introduced by the employer, all targeted employees must be enrolled into a pension scheme, which they are then able to choose to leave at any point, although they will be automatically re-enrolled every three years.

Employers introducing automatic enrolment must enrol their employees into a pension with (at least) minimum levels of contributions. Up to and including March 2018, the minimum employer

[29] The UK government terms 'targeted' employees as 'eligible' for automatic enrolment and those who are not targeted as 'non-eligible'. This is a misnomer because 'non-eligibles' can be, and sometimes are, automatically enrolled.

contribution was 1% of qualifying earnings and the minimum total contribution is 2% of qualifying earnings, where the 'total' is the sum of employee and employer contributions, including any tax relief. 'Qualifying earnings' are earnings in a certain band set by the government. In 2015–16, qualifying earnings were those between £5,824 ($7,600) and £42,385 ($55,100) per year. The minimum total contribution increased to 5% of qualifying earnings in April 2018 (with a minimum of 2% from the employer) and rose to 8% from April 2019 (with a minimum of 3% from the employer). Employers can choose to enrol their employees automatically into schemes with higher (employee and employer) contributions, although they are prevented from setting the employee contribution rate so high as deliberately to encourage a large proportion of employees to opt out, although there is not an explicit cap on this.

The obligation of employers to enrol their targeted employees automatically has been introduced gradually since October 2012. Each employer is given a 'staging date'. Employers must automatically enrol targeted employees from this date, although they can apply to postpone enrolment by up to 3 months from the staging date.[30] This means that any employer at least 3 months past their staging date must have introduced automatic enrolment otherwise they will be in breach of their legal obligations.

An employer's staging date is determined by the number of employees the organisation employs in April 2012, as measured by the number of employees on its Pay-As-You-Earn scheme (the scheme by which income and payroll tax payments are withheld from employees' earnings). Because employer size is measured at a fixed date in April 2012, it cannot be manipulated by employers as automatic enrolment is rolled out. Employers with 120,000 or more employees were the first employers to be affected, with a staging date of 1 October 2012. Over time, the obligation to enrol targeted employees automatically has been rolled out to affect progressively smaller employers. The exact timetable for the roll out is included in Table C.1 in the Appendix. By April 2015 (which is the latest date covered

[30] The exception to this is that if an employer has an open DB pension scheme which all employees are entitled to join then they can delay introduction of automatic enrolment until 30 September 2017. However, there are very few open DB schemes in the private sector.

by the data we use in this chapter), all employers with 58 or more employees are obliged to have introduced automatic enrolment. By February 2018 automatic enrolment applied to all employers.

Finally, there are groups of employees who are not enrolled automatically into a pension scheme, but are nonetheless potentially affected by automatic enrolment. Individuals who earn over the earnings threshold, but are aged 16 to 21 or over the state pension age (but under 75), as well as individuals of all ages earning between £5,824 ($7,600) and £10,000 ($13,000) per year (in 2015–16) do not have to be enrolled automatically; however, they can opt in to join the scheme, and if they do their employer must make minimum contributions. Employees earning below £5,824 ($7,600) per year (in 2015–16) are also not enrolled automatically but can apply to join a pension scheme, although their employer does not have to make a contribution.

4.3 Data

The data used in this chapter are from the Annual Survey of Hours and Earnings (ASHE), which is collected by the UK's Office for National Statistics (see Office for National Statistics, 2018a). ASHE is a panel survey of approximately 1% of employees in Britain. Employees are included in the survey if their National Insurance number ends in a specific pair of digits. The survey is completed by employers in April of each year and data are available from April 1997 to April 2015. The number of responses to this survey was 181,052 in 2015.[31]

The ASHE data include detailed information on the pay and hours of work of each employee and it asks whether the employee was a member of a workplace pension scheme ('run or facilitated by [the] organisation'). From 2004 onwards, it also asks how much the employer and the employee contributed to the pension. The data contain a number of variables on the employee such as age, sex, occupation and job tenure. It also contains information on the employer, such as industry and sector.

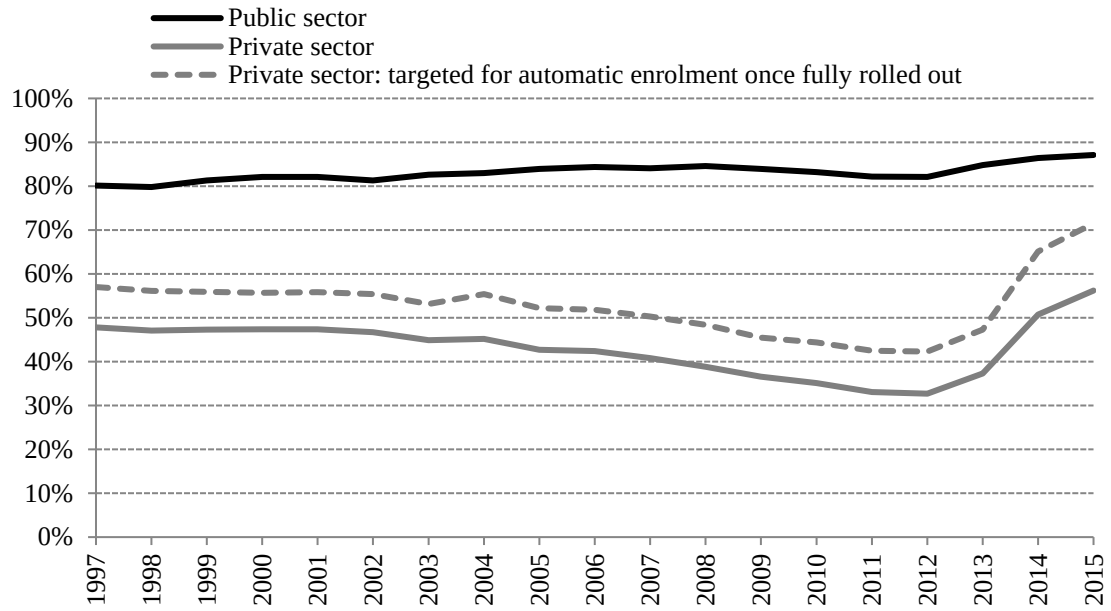
[31] In 2007 and 2008, the ONS cut the sample size (non-randomly) by approximately 20%. The full sample was restored in 2009. For this reason, with the exception of one descriptive chart (Figure 4.1), we restrict our use of these data to the period 2009 to 2015.

Using these data, we can define the set of individuals who are targeted for automatic enrolment: those aged 22 to state pension age, earning more than the automatic enrolment threshold (£10,000 ($13,000) per year since April 2014) and who have been working for their employer for at least 3 months.

Most importantly for our empirical strategy, the data contain a measure of the number of employees in the employer in each year. This measure comes from the UK government's business register. This is crucial because, as was discussed in Section 4.2, it is the number of employees employed in April 2012 that determines when employers are obliged to introduce automatic enrolment. Our main outcomes of interest are the participation in a workplace pension scheme and the pension contribution rates.[32] It is instructive to show the rates of pension participation and the contributions to workplace pensions prior to automatic enrolment being introduced. Figure 4.1 uses the ASHE data to show how pension participation has changed from 1997 to 2015. Public and private sector workers have had very different participation rates in pensions. The proportion of private sector employees in a workplace pension scheme fell from 48% in 1997 to 33% in 2012, before rising to 56% in 2015, indicating that automatic enrolment has potentially increased pension participation. Looking only at those meeting the automatic enrolment eligibility requirements (those aged 22 to the state pension age, earning over the threshold and in work, but not restricting to those employers where automatic enrolment had been introduced), it fell throughout the 2000s, before rising from 42% in 2012 to 72% in 2015. In contrast, pension participation of public sectors is much higher (at least 80% in all years 1997 to 2015), though it has also risen since 2012, to reach 87% in 2015.

[32] We define an individual to be participating in a workplace pension scheme if their employer indicates they are a member of a workplace pension scheme and the employer does not record there being a zero contribution to the pension (from employee and employer combined). We calculate pension contribution rates by dividing the amount contributed by employee or employer by total pay in the pay period. Employee and employer pension contribution rates are top-coded at the 99th percentile of the distribution of the contribution rates of private sector employees who are in a pension scheme in 2012.

Figure 4.1: Workplace pension participation rates among public and private sector employees, 1997 to 2015



Note: 'Targeted' means those aged 22 to state pension age, earnings over automatic enrolment threshold, working for employer for over 3 months. It does not restrict to those employers where automatic enrolment had been introduced.
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

The focus of this chapter is on participation in, and contributions to, workplace pensions among private sector employees. Table 4.1 shows the distribution of pension contribution rates among all private sector employees (including non-participants) from employees, employers and total, prior to automatic enrolment (in April 2012). It shows that there are very few employees with very low, positive contribution rates, which are the levels at which the minimum contributions are set under automatic enrolment. Looking at employee contributions, 24% of employees contribute between 2% and 10%, with very few contributing more than 10%. On the other hand, 14% of employees have an employer contribution of more than 10% of earnings. Looking only at those who are targeted for automatic enrolment working for an employer with 58 or more employees (and therefore auto-enrolled by April 2015), they are much more likely to have higher employer and employee contribution rates.

Table 4.1: Distribution of pension contributions for private sector employees, 2012

Contribution rate	***All***			***All targeted by April 2015: targeted workers in employers with 58+ employees***		
	Employee	*Employer*	*Total*	*Employee*	*Employer*	*Total*
None	73.5%	68.8%	67.9%	59.6%	53.1%	52.0%
0% to 1%	0.3%	0.3%	0.2%	0.5%	0.4%	0.2%
1% to 2%	1.6%	0.8%	0.4%	2.3%	1.0%	0.4%
2% to 5%	12.2%	6.7%	2.9%	18.3%	9.1%	3.7%
5% to 10%	11.4%	9.8%	8.5%	18.0%	15.4%	12.2%
10% to 15%	0.6%	7.8%	6.9%	0.9%	11.4%	11.0%
15% +	0.3%	5.9%	13.2%	0.4%	9.7%	20.6%
All	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

Note: Contribution rates are expressed as weekly contribution to pension scheme divided by gross weekly earnings. For employee and employer contributions, 'None' includes both employees who are in a pension, but where either the employee or employer makes no contribution to it, and employees not in a pension. Columns do not always sum to 100 due to rounding.
Source: Authors' calculations using the Annual Survey of Hours and Earnings

In Appendix Tables C.2 and C.3, we provide descriptive statistics on private sector employees in 2012 working in employers with 58 or more employees who are targeted for automatic enrolment. They have median gross weekly earnings of £460 ($600), 61% are male, 89% work full-time, over 50% of them have worked for their employer for five years or more, they have a median age of 41 and they work across a wide range of industries, of which the largest are 'retail and wholesale' and 'manufacturing', employing 20% and 16% of the employees respectively.

4.4 Empirical methodology

The roll-out of the obligation to enrol targeted employees automatically means that we can estimate the causal effect of automatic enrolment on participation in a workplace pension, and the contributions into it, using a difference-in-differences empirical strategy. Employees (and employers) are affected by automatic enrolment at a given time exogenously based entirely on how many employees there were in 2012 and the roll-out timetable chosen by the government, as described in section 4.2. We only consider private sector employees since public sector employees had much

higher workplace pension participation rates and many of public sector pension plans implemented automatic enrolment prior to the latest reforms coming into effect.

Exploiting the government's roll out, we categorise private sector employees into 7 "employer size groups".[33] The groups are shown in Table 4.2. These groups are determined by rules of the rollout, and aggregate together employers who face the same obligation (to enrol targeted employees automatically) when they are observed in each April. In each year from 2013 to 2015, there are some employers who have passed their staging date, but are not 3 months past their staging date, which means that they could have introduced automatic enrolment but they might not have because they might have postponed its introduction. We term these groups 'partially affected' and control for them in our analysis below.

Table 4.2: Roll-out of automatic enrolment obligations by employer size

Employer size in April 2012	*Was automatic enrolment in place in:*			
	April 2012	*April 2013*	*April 2014*	*April 2015*
30,000+	No	Yes	Yes	Yes
6,000 to 29,999	No	Partially	Yes	Yes
350 to 5,999	No	No	Yes	Yes
160 to 349	No	No	Partially	Yes
58 to 159	No	No	No	Yes
50 to 57	No	No	No	Partially
5 to 49	No	No	No	No

Source: Authors' calculations using http://www.nowpensions.com/auto-enrolment-staging-dates/ and the Annual Survey of Hours and Earnings

Given this roll-out, equation 4.1 sets out the difference-in-difference specification that we use to estimate the impact of automatic enrolment:

$$y_{ift} = \alpha + \beta(autoenrol)_{ift} + \sum_{a=2013}^{2015} \gamma_a [partial\_a = 1] + \theta_f + \mu_t + \delta X_{ift} + \varepsilon_{ift}. \quad (4.1)$$

[33] We exclude individuals who work for employers who had between one and four employees in 2012. This is because the pension participation and contributions rates are unlikely to evolve in a similar way to those for larger employers, partly because many employers with only one employee may be sole proprietors..

We want to estimate the effect of automatic enrolment on an outcome (such as pension participation) *y*, for an individual *i*, working for an employer in 'employer size group' *f*, observed at time *t*. $(autoenrol)_{ift}$ is a dummy variable taking the value 1 if automatic enrolment is in place in the employee's employer when they are observed, and 0 otherwise. β is the coefficient of interest. It is also necessary to control for the fact that some employees work for employers that are 'partially affected'. We therefore introduce a dummy variable for being 'partially affected', which varies for each year that there are people who are partially affected (2013 to 2015): $\sum_{a=2013}^{2015} \gamma_a [partial\_a = 1]$.[34]

We control for fixed differences in the outcome for employees working for employers of different sizes using six 'employer size group' fixed effects θ_f. We control for time using year fixed effects μ_t. Under this specification, we assume that the 'employer size group' fixed effects are fixed over time. This is the usual common trends assumption which says that, in the absence of the reform, affected and unaffected employees would see their pension participation and contribution rates change in the same way.

We also control for a vector of characteristics of employees and the employers they work for, *X*. These include controls for sex, age (in cubic), job tenure (three dummies), dummies for working for a non-profit institution, being in a full-time job, the job not being the individual's 'main' job and the job being temporary, 10 regional dummies, 12 dummies for industry of the employer and 8 dummies for occupational category of the employee. The full list of covariates can be found in Appendix Table C.4.

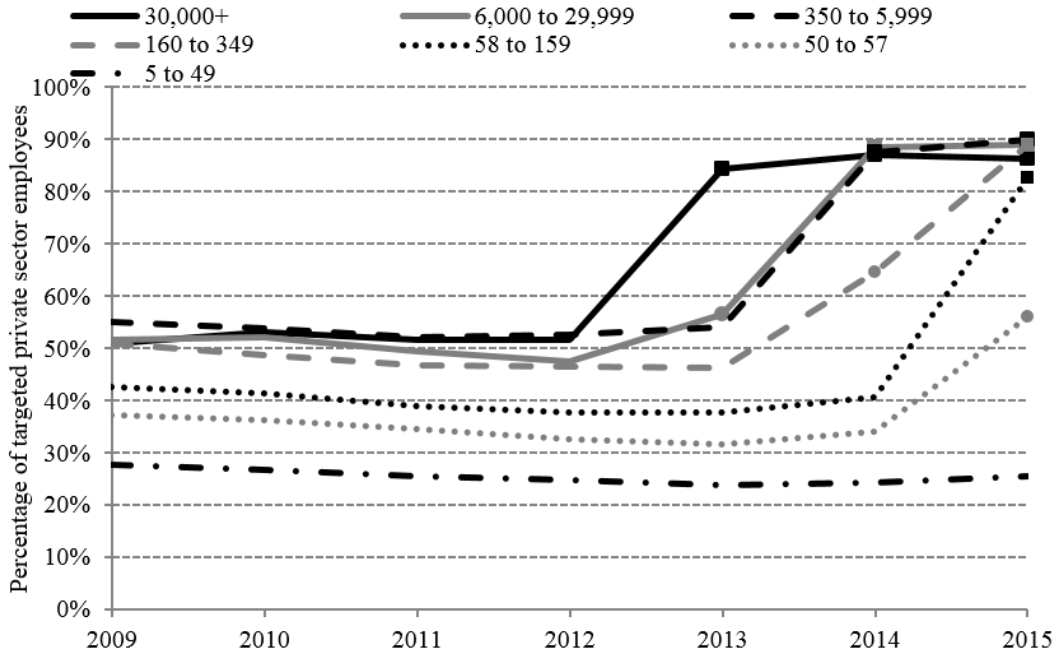
Figure 4.2 provides graphical evidence for the effect of automatic enrolment on the pension participation rate of private sector employees who meet the conditions for auto-enrolment.[35] Each

[34] These coefficients on the partially affected dummies are allowed to vary by year because in different years the average gap between April and the month of the staging date (February, March or April) varies. Restricting these coefficients to be the same does not affect our results.

[35] The data underlying Figure 4.2, and the analysis in the remainder of the chapter, include one observation per job rather than one observation per person. Therefore, if an individual has two jobs, they are included in the data twice. We do not restrict the analysis to main jobs because automatic enrolment operates at the 'job level': if an

series represents employees working for private sector employers of different sizes. Prior to the introduction of automatic enrolment (between 2009 and 2012), the participation rates of each group move in a similar way, although, on average, employees working for larger employers have higher participation rates than those working for smaller employers. This suggests that – at least prior to the reform – our common trends assumption needed for identification of a causal effect is valid.

Figure 4.2: Workplace pension participation rates among private sector employees targeted for automatic enrolment, by employer size in 2012



Note: Square data points indicate periods when employers were at least 3 months past their staging date, and therefore had to enrol their targeted employees automatically. Circular data points indicate employers that are past their staging date, but not 3 months past their staging date and so are 'partially affected'.
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

In Figure 4.2, data points in which employees are enrolled automatically are shown with a square data point. Those periods in which employees are enrolled automatically see far higher pension participation rates than prior to automatic enrolment, with participation rates under auto-enrolment of between 80% and 90%, suggesting that there is a large impact of automatic enrolment on pension

individual is in a job and they meet the requirements for auto-enrolment, they will be enrolled automatically, even if it is not their main job.

participation. Unsurprisingly, partially affected groups (denoted by circles in Figure 4.2) have higher participation than before automatic enrolment, but not as high as when it is fully introduced. The primary outcomes of interest are the effects of automatic enrolment on the probability of participation in a workplace pension scheme and on the level and distribution of pension contributions. We estimate the effect on the probability of pension participation using a linear probability model and a probit model, and the effect on contribution rates (both mean and whether below different thresholds) using ordinary least squares (OLS). The models are estimated on data from April 2011 to April 2015, therefore including two years (2011 and 2012) in which nobody was affected by automatic enrolment and three years in which progressively more employees are enrolled automatically. Our sample size of targeted private sector employees in employers with five or more employees from 2011 to 2015 is 457,443, working for 64,849 employers. There are a small number of employees with missing pension contributions, so the sample size for the effect on pension contributions is slightly smaller at 452,212.

Since automatic enrolment is implemented by employers, and they may implement it in slightly different ways (particularly in terms of how much they offer as an employer contribution), there may be a correlation in the error ε_{ift} between employees working for the same employer. In all our results, we therefore cluster our standard errors at the employer level. We show the number of clusters (employers) as well as the number of observations (employees) underlying each regression in our results section.

4.5 Results

4.5.1 Effect of automatic enrolment on participation of a workplace pension

Table 4.3 reports the results of estimating the effect of automatic enrolment on the proportion of employees who are members of a workplace pension, using equation 4.1, with the dependent variable being a dummy indicating whether the employee is participating in a workplace pension. Our preferred specification is specification 2, which estimates the effect by Ordinary Least Squares (OLS), controls for the characteristics of employees (*X*). We find that automatic enrolment substantially

increases the proportion of employees participating in a workplace pension, by 36 percentage points.[36] This compares with a pre-reform (2012) participation rate of 49% of targeted employees working for employers with 58 or more employees.

The alternative specifications in Table 4.3 show that this result is robust to estimating the model using a probit model (specifications 3 and 4) rather than OLS (as shown in specifications 1 and 2) and to not controlling for control variables *X* (specifications 1 and 3). The full results of the OLS regression (in specification 2) are shown in Appendix Table C.4. In all the results in Table 4.3, the effect of automatic enrolment is highly significantly different from zero (at below the 1% level).

Table 4.3: Effect of automatic enrolment on pension participation rates of targeted private sector employees

	(1)	*(2)*	*(3)*	*(4)*
Effect of automatic enrolment	0.365***	0.361***	0.376***	0.368***
Standard error	[0.016]	[0.016]	[0.018]	[0.017]
Number of observations	457,443	457,443	457,443	457,443
Number of clusters	64,849	64,849	64,849	64,849
Estimated by:	OLS	OLS	Probit	Probit
Control variables (*X*) included?	No	Yes	No	Yes

Note: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. Probit models are estimated using maximum likelihood. Standard errors are clustered at the employer level and, for specifications 3 and 4 are estimated by bootstrapping the average marginal effect of automatic enrolment on pension participation 250 times. Control variables (*X*) are listed in Appendix Table C.4. Sample includes all targeted private sector employees from April 2011 to April 2015.

Source: Authors' calculations using the Annual Survey of Hours and Earnings.

We test the validity of this empirical strategy using a placebo test, in which we imagine that automatic enrolment had been introduced in exactly the same way, but 3 years earlier. We then estimate the same equation 4.1, except using data from 2009 to 2012 (all years are prior to auto-enrolment actually being introduced). The results of this test are shown in Appendix Table C.5. We find that there is no

[36] Almost all of this 36 percentage point increase was due to increased participation in DC pension schemes (a 35 percentage point effect), with only a 1 percentage point increase in participation in DB schemes, an effect which is not statistically significantly different from zero at standard significance levels.

evidence of any effect, with the tiny point estimate of 0.2 percentage points not being close to being statistically significant.

By 2015, targeted employees in employers of the same size had a pension participation rate of 88%. With pension participation rates under automatic enrolment nearing 90%, these are very similar to the rates found by Madrian and Shea (2001) and Choi et al. (2004) in their studies of US firms. However, the increase in participation in workplace pensions caused by automatic enrolment is heterogeneous, which is not surprising because, prior to automatic enrolment, different groups of workers had very different participation rates.

Table 4.4 shows the effect of automatic enrolment on different subgroups. These are the results of estimating equation 4.1 only on given subgroups (using a linear probability model and including control variables). Overall, it shows that those groups that had the lowest pre-reform pension participation rates see the largest impact of automatic enrolment, but that those groups with the highest pre-automatic-enrolment participation rates still have the highest rates after its introduction. For example, automatic enrolment increased pension participation of 22- to 29-year-olds by 52 percentage points, compared with a baseline of 28%, whereas the effect for those in their 40s was 31 percentage points, compared with a base of 56%. By 2015, the participation rate of targeted employees in employers with 58 or more employees was 85% for those in their 20s, compared with 90% for those in their 40s.

Table 4.4 also shows that there is a larger effect for people with low job tenure than for those with high job tenure – increasing the participation rate by almost 54 percentage points for those in their first year with an employer, compared with 27 percentage points for those with 5 or more years with the employer. Before automatic enrolment, job tenure is highly correlated with pension participation, and while there is still a positive relationship after automatic enrolment, it is much less pronounced.

Table 4.4: Effect of automatic enrolment on pension participation rates of different subgroups

	Effect	*Std error*	*Sample size*	*Number of clusters*	*Participation rate in:*	
					2012	*2015*
All	0.361***	[0.016]	457,443	64,849	48.6%	88.1%
Age group						
22 to 29	0.521***	[0.023]	94,294	24,329	27.6%	85.4%
30 to 39	0.372***	[0.017]	116,337	25,480	48.0%	88.4%
40 to 49	0.306***	[0.016]	124,806	25,370	56.4%	89.9%
50 to state pension age	0.279***	[0.013]	122,006	23,570	57.7%	88.0%
Job tenure (years with employer)						
<1 year	0.538***	[0.013]	49,771	23,459	21.6%	81.3%
1 to 2 years	0.494***	[0.016]	54,653	25,773	30.0%	86.1%
2 to 5 years	0.444***	[0.019]	109,154	30,377	38.2%	87.3%
≥ 5 years	0.266***	[0.016]	243,865	34,820	62.0%	90.4%
Earnings quartile						
Lowest quartile	0.539***	[0.035]	114,361	28,007	22.3%	81.1%
Second quartile	0.457***	[0.020]	114,361	28,805	36.0%	86.1%
Third quartile	0.315***	[0.013]	114,362	25,907	55.5%	89.5%
Fourth quartile	0.161***	[0.009]	114,359	19,071	76.6%	93.5%
Sex						
Male	0.356***	[0.016]	275,633	42,758	50.0%	88.7%
Female	0.369***	[0.018]	181,810	32,757	46.4%	87.1%
Industry's pension participation pre-reform						
Lowest third	0.619***	[0.011]	142,384	26,149	18.7%	83.9%
Middle third	0.375***	[0.038]	141,164	21,020	44.7%	86.8%
Highest third	0.151***	[0.010]	141,549	13,915	75.3%	92.3%

Note: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. All models estimated by OLS including control variables (listed in Appendix Table C.4). Standard errors clustered at the employer level. 'Participation rate' in 2012 and 2015 is based only on targeted employees working for employers that had 58 or more employees in 2012.

Source: Authors' calculations using the Annual Survey of Hours and Earnings.

We also divide the sample into quartiles of the weekly earnings distribution in each year (restricting only to targeted private sector employees) and look at the effect on each quartile. The effect for the

lowest earnings quartile of targeted employees (in April 2015, this was composed of those earning between £10,000 ($13,000) and £16,730 ($21,700) per year) is 54 percentage points, compared with only 16 percentage points for the highest quartile, with the participation rate for the highest quartile reaching 94% in 2015. Consistent with this is the fact that those from lower occupational classes see a larger increase in pension participation as a result of automatic enrolment than do those from higher occupational classes (results not reported in the table). There is a slightly larger impact for women than for men (although not statistically significantly different).

The Table also shows that the effects of automatic enrolment were much greater in industries that, prior to the reform (i.e. in 2012), had particularly low pension provision. For those targeted employees working in industries in the lowest third of pension participation, the effect of auto enrolment was to boost pension participation by 62 percentage points, an impact that is even higher than the increase for those with the lowest earnings or shortest job tenure. In comparison, the effect of automatic enrolment for those from high-participation industries was 15 percentage points.

4.5.2 Effect of automatic enrolment on workplace pension contribution rates

While the impact of automatic enrolment on pension participation is clear, the impact on the levels of contributions made into workplace pensions may only be quite small (and could even be negative), if those who are newly enrolled into a pension scheme are enrolled at the legal minima (1% of qualifying earnings from employer, 2% of qualifying earnings in total) and if the introduction of the default means that some employees reduce their contributions to the default level, as is found in Madrian and Shea (2001).

We estimate the effect of automatic enrolment on the mean pension contribution rates (from employee/employer/total) using equation 4.1, estimated by OLS. The results of this are shown in Table 4.5. It shows that there was an increase in the mean employee contribution rate by 0.45% of earnings, from a baseline of 2.1% in 2012 (a 21% increase). The effect on employer contribution rates

was larger, at 0.60% of earnings (although this increase is a lower fraction of the pre-reform mean). The effect on the mean total contribution rate was 1.05% of earnings, compared with an average contribution rate of 7.0% in 2012 prior to automatic enrolment being introduced. All of these impacts are statistically significant at the 1% level. Consistent with the findings in Table 4.4, we find that the increases in total contributions are largest in the groups (such as those with low earnings and younger employees) that had lower contributions prior to the reform.

Table 4.5: Effect of automatic enrolment on mean employee, employer and total contribution rates to workplace pensions among targeted employees

	Effect of auto-enrolment	*Std error*	*Mean contribution rate of targeted employees in 2012*
Employee contribution rate	0.45***	[0.06]	2.1%
Employer contribution rate	0.60***	[0.11]	4.9%
Total contribution rate	1.05***	[0.13]	7.0%

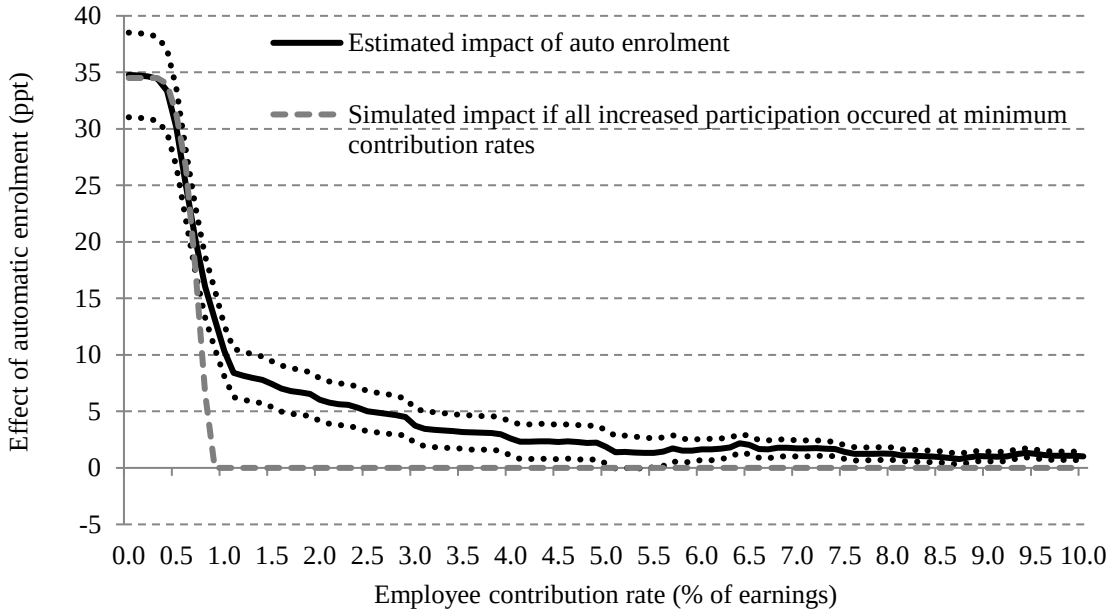
Note: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. Each regression has 452,212 observations and 64,428 clusters (employers) underlying it Estimated by OLS including control variables (listed in Appendix Table C.4). Standard errors clustered at the employer level. Contribution rate is the weekly amount contributed by the employee/employer to the pension, as a fraction of gross weekly earnings. Total contribution rate is the sum of the employee and employer contribution rates.
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

Because the mean contribution rate includes zeros, and is affected by some high contribution rates (generally from Defined Benefit schemes), it is potentially more interesting to estimate the impact on the distribution of contribution rates. We do this by creating a set of dichotomous variables taking the value one if the individual is contributing (strictly) more than a certain share of earnings, and zero otherwise. We do this for all values from zero to 10% of earnings in steps of 0.1% of earnings. This is done separately for employee, employer and total contributions. The impact of automatic enrolment on these outcomes is estimated using equation 4.1, using OLS, and with standard errors clustered at the employer level (analogous to specification 2 from Table 4.2). The resulting estimated coefficients, alongside their 95% confidence intervals, are plotted in Figures 4.3, 4.4 and 4.5.

In addition, on each Figure in a grey dashed line, we show the simulated impact of automatic enrolment on the distribution of pension contributions if every employee brought into a pension scheme as a result of automatic enrolment was enrolled with minimum default contributions as specified by the government (and if there was no other change in response to the policy). Comparing the estimated impact of automatic enrolment (the solid black lines), with this simulated effect allows us to say to what extent employees are making or receiving higher pension contributions than the minimum defaults as a result of automatic enrolment.

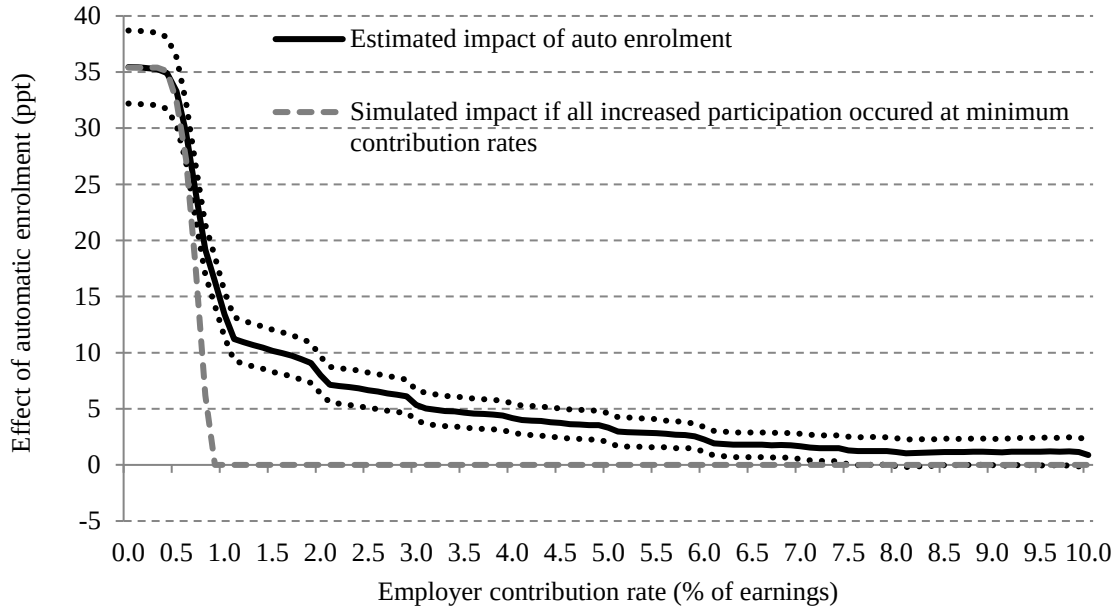
There are very large increases in the proportion with low positive contribution rates, on both the employee and employer side. Figure 4.3 shows that there is an increase of 30.1 percentage points in the proportion of employees making an employee contribution over 0.5% of earnings. The effect on the probability of having an employee contribution rate of over 1% is smaller, at 10.4 percentage points. As shown in Figure 4.4 there is a similar increase in the proportion of employees receiving very low employer contribution rates – an increase of 33.3 percentage points with employer contributions above 0.5% of earnings, but only a 13.4 percentage points increase with contributions in excess of 1% of earnings. This shows that the minimum default of 1% of qualifying earnings as employer contribution has led to a very large increase in the proportion saving small amounts through a workplace pension.

Figure 4.3: Effect of automatic enrolment on probability of employee contribution rate being greater than certain percentages of earnings



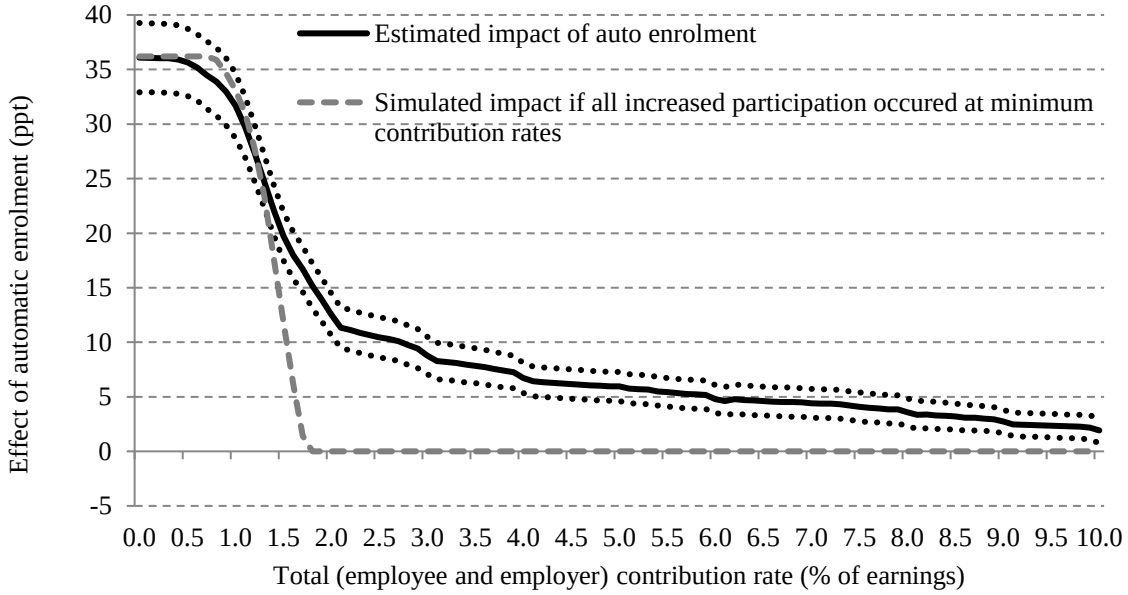
Note: Each of the data points (at each 0.1% of earnings) is the coefficient on auto-enrolment from estimating equation 4.1 where the dependent variable a dummy indicating if the contribution rate is in excess of the specified amount. Each regression is estimated by OLS including control variables (listed in Appendix Table C.4). Standard errors are clustered at the employer level and used to construct the 95% confidence intervals shown in dotted lines. The grey dashed lines show the simulated impact on contribution rates had the entire increase in pension participation occurred at the minimum default contribution rates as specified by government and had no-one else changed their pension saving in response to the reform.
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

Figure 4.4: Effect of automatic enrolment on probability of employer contribution rate being greater than certain percentages of earnings



Note: As Figure 4.3. *Source*: Authors' calculations using the Annual Survey of Hours and Earnings.

Figure 4.5: Effect of automatic enrolment on probability of total contribution rate being above greater than certain percentages of earnings



Note: As Figure 4.3. *Source*: Authors' calculations using the Annual Survey of Hours and Earnings.

However, by comparing the black lines to the simulated grey lines, Figures 4.3 and 4.4 also shows that, on both the employee and employer side, automatic enrolment has caused a large increase in the

proportion of employees saving well over the minimum contribution rates. For employee contributions, automatic enrolment has increased the probability of contributions above 2% of earnings by 6.0 percentage points and above 5% by 1.9 percentage points. These effects are statistically significant at the 5% level. On the employee side, this means that individuals are not responding to the default minimum by reducing their contributions towards it, as is found by Madrian and Shea (2001). Instead, there is an increase in the proportion making contributions that are much higher than the minimum.

This is an important result, because one of the concerns with the introduction of automatic enrolment is the fact that it has (in some settings) led to some people saving less, prompting calls for other policies such as auto-escalation (see Benartzi and Thaler (2013)). Although our results do not rule out this behaviour, they show that if it is present then the lower saving is more than outweighed by the effect of employers and employees starting to contribute more than the minimum. One reason for this could be the fact that employers are enrolling their employees automatically into schemes with much higher employer contribution rates than the minimum and that they also have higher minimum employee contributions. Indeed, there are also significant impacts of automatic enrolment on the proportion of targeted employees who are receiving in excess of 2% in employer contribution (8.0 percentage points) and even on the proportion with employers contributing 5% or more (3.3 percentage points). This also shows that the results are not simply explained by employers choosing to move straight to the long-run minimum contribution rates.

Overall, Figure 4.5 shows the effect of automatic enrolment on the total contribution rate – which is what policymakers may care about most given they are aiming to increase retirement savings. Automatic enrolment has lead to the proportion with a contribution over 1% increasing by 31.7 percentage points, over 2% by 12.5 percentage points, over 5% by 6.0 percentage points and even an increase in the proportion with over 10% of 1.9 percentage points. All of these effects are statistically significant at the 5% level. Overall, therefore, most of the increase in workplace pension contributions comes from very small contributions around the current default minimums, but automatic enrolment also led to considerable increases well above those government minimums.

4.5.3 Effects of automatic enrolment on non-targeted employees

Automatic enrolment potentially has impacts on those who are not targeted for automatic enrolment, for three main reasons. First, as described in Section 4.2, those who are not targeted for automatic enrolment but earn at least £5,824 ($7,600) per year can ask to be enrolled in a pension. They may want to do so (even if they did not want to prior to automatic enrolment) as peer effects have been shown to influence pension plan participation (Duflo and Saez, 2002), so increased participation of targeted workers might encourage non-targeted workers to ask their employers to enrol them in a scheme. Second, employers automatically enrol employees when they are targeted and employees could continue to participate even if they are no longer formally in the targeted group. Although this is not a possible mechanism for some groups of non-targeted employees – specifically those who have not yet worked for the employer for 3 months and those who are aged under 22 – it may be important for those who have variable earnings and are automatically enrolled because at some point they earn over the earnings threshold. Third, employers can decide to enrol automatically employees who are not targeted for automatic enrolment under the legislation. There are a number of reasons that employers might do this, such as a paternalistic desire to provide pensions to all staff, including low earners, or to reduce the administrative burden of monitoring whether staff do or do not earn over the earnings threshold in each pay period.

Table 4.6 shows the effect on different non-targeted groups, by estimating equation 4.1 on non-targeted groups from 2011 to 2015. For the first four rows of the table, we select those who are non-targeted for only one reason (such as being too young, but who would otherwise be targeted). The last row includes employees non-targeted for automatic enrolment for any reason. Table 4.6 shows that there are significant spillover effects of automatic enrolment onto groups that are not targeted for automatic enrolment under the government rules. Auto-enrolment increases participation rates by 20 percentage points for people who have not yet worked for their employer for 3 months and by 28 percentage points for those earning under the earnings threshold. The spillovers on those aged above and below the age cut-offs are smaller, but still sizeable, with auto-enrolment increasing pension participation by 6 percentage points for individuals who are aged under 22 and by 9 percentage points

for those over the state pension age. All of these effects are statistically significant at the 1% level. Taking all non-targeted private sector employees together (who earn at least £5,824 ($7,600) per year), the effect is to increase pension participation by 18 percentage points – thereby more than doubling pension participation among this group.[37]

Table 4.6: Effect of automatic enrolment on pension participation rates of private sector employees who are not targeted for automatic enrolment

Non-targeted group for automatic enrolment	*Effect*	*Std error*	*N*	*Number of clusters*	*Participation rate in:*	
					2012	*2015*
0 to 2 months' job tenure	0.203***	[0.017]	9,478	5,915	10.5%	32.8%
Aged under 22	0.059***	[0.013]	18,476	7,719	11.1%	19.5%
Aged over state pension age	0.087***	[0.016]	11,567	4,253	29.2%	38.8%
Under earnings threshold	0.281***	[0.022]	51,059	14,971	18.4%	52.1%
All not targeted	0.178***	[0.013]	110,554	31,387	14.7%	35.6%

Note: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. All models estimated by OLS including control variables (listed in Appendix Table C.4). Standard errors clustered at the employer level. Analysis restricted to those earning at least the lower earnings limit (LEL) – £5,824 (£7,600) per year in 2015–16 – in the year they are observed.
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

The fact that there are large spillover effects of automatic enrolment on the pension participation rates of non-targeted workers is both interesting and important, even though we cannot distinguish the exact mechanism that is causing it at the moment. If it is that these employees are asking to participate, it cannot be that procrastination was causing them not to enrol previously. However, it could be because the decision is now less complex, because of the endorsement factor or in order to receive the employer contribution. Given that there is evidence of employees not enrolling in pension schemes even when there are no mandatory employee contributions (see Benartzi and Thaler (2007)), it might be unlikely that employees asking to participate is the major driver of this impact.

[37] We do not look at the effect on those earning less than £5,824 ($7,600) per year. This is because historically, the ASHE data have captured those employees earning under this threshold (the Lower Earnings Limit, the threshold at which employers start paying employer payroll taxes) relatively poorly.

On the other hand, it could be that employers are choosing to enrol their non-targeted employees automatically into a pension scheme, even though this will come at some cost to the employer. This would be more evidence of employers choosing to pay more in pension remuneration than is mandated by the legislation introducing automatic enrolment. One piece of evidence for this is that, when looking at the effect of automatic enrolment on employer contributions of non-targeted employees, the proportion of employees receiving more than 1% contribution rose by 5 percentage points, suggesting that many of those who are participating are receiving more than the minimum contribution. This conclusion that these results are not driven mainly by opting in is supported by a survey of employers undertaken by Department for Work and Pensions (2016) that reports only very few (5%) of individuals who are not targeted for automatic enrolment elect to opt in to participating in a workplace pension.

Having said this, it is unlikely that this is simply the result of paternalistic employers automatically enrolling employees (that they do not have to) into pension schemes because they believe it is good for their employees. As is show in Appendix Table C.6, splitting non-targeted employees into three groups based on the pre-reform pension participation of the industry of their employer reveals that there have been large increases (18 percentage points) in the proportion of non-targeted employees participating in a pension even in those industries where pension participation was very low (below 2%) prior to the reform.[38] Given that fewer than 2% of these employees were enrolled in a pension in 2012, it does not seem conceivable that it is paternalism that, in the face of automatic enrolment for targeted employees, leads to these employers automatically enrolling these non-targeted employees into a pension scheme when they are not obliged to do so.

[38] This compares to an increase of 11 percentage points for those employed in industries with high pre-reform participation.

4.6 Conclusion

With concerns about undersaving for retirement across the developed world, there is intense interest amongst economists and policymakers regarding policies that can boost saving for retirement. This chapter has studied the first nationwide introduction of automatic enrolment in which employers are obliged to enrol employees into a workplace pension scheme, which employees can then choose to leave if they wish. We provide the first assessment of the impact of automatic enrolment that allows for changes in employer behaviour in response to the policy in a context where those employers did not choose to introduce automatic enrolment, but instead were obliged to do it. This kind of impact cannot be identified when automatic enrolment is introduced voluntarily by some large employers, such as was studied in Madrian and Shea (2001) and Choi et al (2004).

We exploit the gradual roll-out by employer size of the obligation in the United Kingdom for employers to enrol their targeted employees automatically into a pension between 2012 and 2015 to estimate the effect of automatic enrolment on saving in a workplace pension by private sector employees using a difference-in-differences methodology. We find that the introduction of automatic enrolment substantially increases the probability of participation in a workplace pension scheme, by 36 percentage points. In 2015, after automatic enrolment had been introduced, the workplace pension participation rate for private sector employees who were targeted to be enrolled automatically reached 88%. This is similar to the levels of coverage delivered by automatic enrolment found in Madrian and Shea (2001) and Choi et al. (2004), and is despite the fact that in our setting automatic enrolment is an obligation imposed on employers.

The largest effects on pension participation we find are for those with the lowest participation rates prior to automatic enrolment: those in their 20s, lower-paid employees, those who have joined their employer more recently, and those employed in industries with low pre-reform rates of pension participation. We also find important new evidence that the policy has led to large increases in the participation rate of employees who are not targeted for automatic enrolment, by 18 percentage points

on average, likely driven by employers deciding to enrol non-targeted employees automatically as well as targeted employees.

These increases in pension participation have led to large increases in saving in a workplace pension by employees targeted for automatic enrolment, on average increasing the total workplace pension contribution rate (expressed as a percentage of earnings) by 1.05% of earnings, compared with a pre-reform average of 7.0%. This effect is large in part because a large fraction of employers are making employer contributions above the minimum mandated under the automatic enrolment legislation. While the minimum employer contribution was 1% of (qualifying) earnings, the proportion of employees receiving more than 2% of earnings as an employer contribution rose by 8.0 percentage points, and the proportion receiving more than 5% rose by 3.3 percentage points. This increase in the proportion making contributions that are much higher than the minimum is in contrast to the previous US evidence of individuals reducing their contributions towards the default minimum (Madrian and Shea, 2001). We also find no evidence of employers responding to automatic enrolment by reducing the employer contributions to newly-hired employees.

Given that we find the automatic enrolment leads to an increase in the average amount of employer contributions that are made to their employee's pensions, a key question is whether employees' wages are reduced in response, or whether automatic enrolment is financed in some other way. The increase in average employer contributions (of 0.6% of salary), is small relative to the variance of earnings, and therefore our attempts to use the same methodology and data used in this chapter to look at the effect of automatic enrolment on earnings leaves us with estimates so imprecise that we are unable to rule out wages not falling at all or falling by an amount equivalent to the total increase in employer contributions. Nevertheless, it is a question that it is important for economists and policymakers to answer in the future.

Finally, this chapter only looks at the effect of saving through a workplace pension. It is possible that increases in saving in a workplace pension are offset by reduced saving elsewhere. However, given that one of the reasons that automatic enrolment is thought to increase pension saving is

procrastination (which leads to people not joining in the absence of automatic enrolment and not leaving once they are enrolled), we may not think that many individuals are 'active' enough to reduce their other saving in reaction to automatic enrolment (as is found by Chetty et al. (2014)). Therefore the substantial increases in workplace pension participation and saving that we have found as a result of the UK's implementation of automatic enrolment is likely to lead to many individuals having higher levels of private resources for retirement than they would have had in the absence of this policy.

Chapter 5: What do we learn about automatic enrolment into pensions from small employers?

5.1 Introduction

Automatic enrolment into pensions is an increasingly popular way of encouraging workers to save more for their retirement. Alongside its nationwide introduction in the UK, increasing numbers of US states – specifically California, Connecticut, Illinois, Maryland and Oregon (see Ghilarducci and Fisher, 2017) – have legislated to enact policies that oblige employers to enrol employees into a workplace pension plan. Elsewhere, Germany, Ireland, Poland, and Turkey are planning or actively considering introducing automatic enrolment.[39]

Despite these policy developments, almost all the evidence on the effect of automatic enrolment comes from its introduction in large employers, either in the United States where some employers chose to introduce automatic enrolment (Madrian and Shea, 2001; Choi et al., 2004; Beshears et al., 2010), or in the United Kingdom where – by 2015 – the government had obliged all large and medium-sized employers to introduce automatic enrolment (Cribb and Emmerson, 2019b – see Chapter 4 of this thesis).[40] In both cases, participation rates in workplace pension plans have been shown to be around (or greater than) 90%, with many employees sticking to the default contribution rates.

There is no evidence to date on the effect of automatic enrolment on pension saving when it is introduced by small employers, either voluntarily or because they have been obligated to do it. This is an important evidence gap for two reasons.

[39] In addition, in 2007, New Zealand introduced automatic enrolment into its 'KiwiSaver' scheme. However, this scheme also had a 50% match rate from the government, an initial government contribution of NZD$1,000, and savings could be put towards purchasing a first home instead of being used for retirement.

[40] Choukhmane (2019) combines data from both large firms in the United States and the same nationally representative data from the UK used in Cribb and Emmerson (2019b).

First, large fractions of employees work for small employers. According the OECD (2019), 28% of employees in the United Kingdom work for an employer with fewer than 50 employees, and 16% work for an employer with fewer than 10 employees. The proportion working for these small businesses are also high in the United States (18% and 10%, respectively) and in other developed countries.

Second, in the absence of automatic enrolment, pension participation is substantially lower among employees of smaller private-sector employers. For example, in the United States in 2016, only 33% of private-sector employees working for employers with fewer than 50 employees were enrolled in a workplace pension plan, compared with 76% of those working for employers with 500 or more employees (Bureau of Labor Statistics 2016) with similar gradients seen in the UK prior to automatic enrolment. This means that any policy to enrol employees automatically will disproportionately affect those working for small employers. This paper is the first to provide evidence on the impact of small employers being obliged to introduce automatic enrolment and the potential mechanisms that are driving this impact.

We exploit a feature of the introduction of automatic enrolment among small employers (those with between 2 and 29 employees) in the UK, in which the last two digits of an employer's assigned payroll tax code determined when they had to introduce automatic enrolment. Using employer-reported data on workplace pension savings from April 2016, and exploiting this pseudo-random feature of the rollout, we find that automatic enrolment substantially increased participation in workplace pensions for targeted employees of small private-sector employers by around 45 percentage points (ppts) to reach 70%. Although this is a very large increase, the resulting participation in workplace pensions is still well below the very high rates (around 90%) seen among the largest employers in the United Kingdom and United States.

We find that lower participation among small employers cannot be rationalised by differences in a rich set of observed individual and employer characteristics. This includes the employees' age, occupation, job tenure, hours of work and earnings. Furthermore, while employer pension

contribution rates are somewhat less generous among smaller employers, we find that these explain very little of the gradient in pension participation by employer size that persists after automatic enrolment.

Unless there are unobserved differences between employees working for smaller and larger employers that are very important in driving pension participation, our results imply that a combination of differences in the way that small employers administer their pension arrangements, and the role of peer effects from other employees in the workplace, are driving the difference in pension participation rates.

The remainder of this chapter proceeds as follows. Section 5.2 discusses the related literature, the mechanisms through which automatic enrolment boosts pension participation and why this might differ when applied to small employers. In Section 5.3, we describe the policy background to this chapter, including details of the nationwide rollout of automatic enrolment. In Section 5.4, we describe the data used, and the empirical strategy employed, in this chapter. We present the results in Section 5.5 and we conclude in Section 5.6.

5.2 Literature

Automatic enrolment can increase participation in workplace pensions through a number of mechanisms that have been identified in previous research. First, it exploits inertia as people put off leaving a pension plan rather than putting off joining one (O'Donoghue and Rabin, 1999). Second, with default contributions and asset allocations, automatic enrolment reduces the complexity of the pension-saving decision, which has previously been shown to increase pension participation (Beshears et al., 2013). Third, there may be 'endorsement effects', which mean that participating is seen as implicit advice on the best course of action (Beshears et al., 2009). Fourth, some employees who are enrolled automatically would not have been offered an employer contribution to their pension previously. These employees may therefore choose to stay in their workplace pension in order to receive this (albeit potentially small) employer contribution they are now offered. Most importantly for this paper, there four important reasons that the effect of automatic enrolment on the employees of

small employers might differ from those of larger employers where automatic enrolment has, to date, been introduced.

First, the types of employees working for smaller employers may be systematically different from those working for larger employers in ways that affect how they respond to automatic enrolment. Employees of smaller firms have lower earnings than those working for larger firms (Bloom et al., 2018) and move employers more often (Topel and Ward, 1992), both of which will reduce the attractiveness of pension participation. Workers with lower discount rates or more self-control (see Laibson et al., 1998) may choose disproportionately to work for larger employers who provide more generous pensions, potentially offered specifically to recruit and retain the types of employees attracted by this (Lazear and Shaw, 2007).

Second, small employers – who were much less likely to offer a workplace pension prior to automatic enrolment – might therefore be particularly likely to respond to the new obligation by enrolling employees into an ungenerous pension arrangement with only minimum employer contributions.

Third, small employers may respond to the obligation to enrol their employees automatically into a pension plan in a way that reduces the probability of employees participating.[41] This could arise if the (smaller) human resources functions of small employers are less effective at communicating the benefits of saving in a workplace pension than are larger employers with established plans, or if smaller establishments make it easier to opt out. Alternatively small employers could be more likely to undertake other actions that reduce the participation rate of their employees – for example, by implicitly (and illegally in the UK) offering slightly higher pay on the understanding that employees opt out of their pension plan, thereby reducing the cost of running a small pension plan.

Fourth, peer effects could potentially be important (as shown in Duflo and Saez, 2002). Among large employers that already had established pension plans, previously enrolled employees could be important (or more effective) at explaining to their previously un-enrolled colleagues how workplace

[41] Relatedly there is evidence that the response of smaller employers to the price of providing health insurance is more elastic than for larger employers (Feldman et al., 1997; Gruber and Lettau, 2004).

pension plans work. This would not be possible among employers that were organising a pension plan for the first time with the introduction of automatic enrolment, where levels of understanding or engagement with pensions may consequently be lower.

5.3 Policy Background

We now turn to describe briefly the UK pensions policy environment. Employees accrue entitlement to a flat-rate state pension, which, in 2016–17, was worth up to £155.65 ($202) per week or just under 30% of median full-time earnings. The resulting state pension is received from the state pension age (which, in April 2016, was 65 for men and 63 for women) and, while taxable, is not means-tested or subject to any earnings test.

The longstanding low level of state pension relative to average earnings means private pensions play a significant role in providing retirement income. Contributions to private pensions are made before income tax, and any returns from investments held in the pension are re-invested untaxed. On withdrawal, one-quarter is tax-free while three-quarters are subject to income tax. Employer contributions to private pensions are treated particularly favourably by the tax system as neither they nor the pension income they generate are subject to the payroll tax (known as National Insurance contributions in the UK).

Despite this favourable tax treatment, in 2012 only 36% of private-sector employees were active participants in a workplace pension plan (i.e. one facilitated by, but not necessarily run by, their employer), compared to 85% of public-sector workers (Cribb and Emmerson, 2019b). Prior to 2012, participation in workplace pensions among private-sector employees had been falling: from 48% in 1997 to 36% in 2012. In comparison, in the United States in 2012, 48% of private-sector employees were active participants in a workplace pension plan (Bureau of Labor Statistics, 2012).

This led to concerns that undersaving for retirement was widespread. In response, automatic enrolment into workplace pensions was introduced as part of a package of reforms designed to boost the adequacy of retirement saving (which also included a move from price to earnings indexation of

the state pension to be partially paid for by future increases in the state pension age; see Pensions Commission, 2005).

Details of automatic enrolment

Under automatic enrolment, employers must enrol all their targeted employees into a pension plan, which employees are then able to choose to leave at any point. Certain conditions must be met for employees to be 'targeted' for automatic enrolment:[42] they must be aged at least 22, they must be aged below the state pension age, they must have worked for their employer for at least three months, and they must earn more than a given earnings level. Since April 2014, this earnings level has been set equivalent to £10,000 ($13,000) per year.

Employers must automatically enrol all of their targeted employees into a pension plan with (at least) minimum levels of contributions. Up to and including March 2018, the minimum employer contribution was 1% of qualifying earnings and the minimum total contribution was 2% of qualifying earnings, where the 'total' is the sum of employee and employer contributions, including any tax relief. In 2016–17, qualifying earnings were those between £5,824 ($7,600) and £43,000 ($55,900) per year.[43]

The obligation of employers to enrol their targeted employees automatically has been introduced gradually since October 2012. Each employer is given a 'staging date' from which they must automatically enrol targeted employees, although they can (but do not have to) postpone enrolment by up to three months. This means that any employer at least three months past their staging date must have introduced automatic enrolment otherwise they will be in breach of their legal obligations. Employers that do not comply are issued with fines that increase with the length of time that they are

[42] In the UK government's terminology, targeted employees are known as 'eligible' employees. This is a bit of a misnomer, as eligible employees are those who must be automatically enrolled, while ineligible employees can be automatically enrolled, but do not have to be.

[43] Minimum total contributions rose to 5% of qualifying earnings from April 2018 (with a minimum of 2% from the employer) and will rise to 8% from April 2019 (with a minimum of 3% from the employer). Employers can choose to enrol their employees automatically into plans with higher (employee and/or employer) contributions, although they are prevented from setting the employee contribution rate so high as deliberately to encourage a large proportion of employees to opt out.

non-compliant, and the number of employees affected.[44] According to the Financial Times, in 2017Q2 alone, there were almost 5,000 "fixed penalty notices" of £400 due to non-compliance, and cases of more significant non-compliance have been successfully prosecuted by The Pensions Regulator.[45]

An employer's staging date is determined by information on its Pay-As-You-Earn (PAYE) tax scheme (the scheme by which income and payroll taxes are withheld from employees' earnings), specifically the number of employees as of April 2012 and, for some smaller employers, the last two digits of the employer's PAYE code. Those employing 120,000 or more employees were the first employers to be affected, with a staging date of 1 October 2012. Gradually, the obligation to enrol targeted employees automatically was rolled out to affect progressively smaller employers.

For employers with 29 or fewer employees, staging dates were allocated gradually – from 1 June 2015 to 1 April 2017 – according to the last two digits of the employer's PAYE code (see Appendix Table D1 for the exact details).

There are groups of employees whom employers do not have to enrol automatically into a pension plan, but who are nonetheless potentially affected by automatic enrolment. Employees earning over the earnings threshold, but aged 16–21 or over the state pension age (but under 75), as well as those aged 16–74 earning between £5,824 ($7,600) and £10,000 ($13,000) per year (in 2016–17) do not have to be enrolled automatically. However, they can opt in to join the plan, and both employers and employees must make at least minimum contributions. Employees earning below £5,824 ($7,600) per year (in 2016–17) also do not have to be enrolled automatically but can choose to join a workplace pension plan, although for this group their employer does not have to make a contribution. As with automatic enrolment, employers can postpone enrolment through these 'opt-in' routes by up to three months.

5.4 Data and Empirical Methodology

[44] https://www.thepensionsregulator.gov.uk/en/employers/what-happens-if-i-dont-comply

[45] https://www.ftadviser.com/pensions/2017/09/11/regulator-prosecutes-company-over-auto-enrolment/

The data used in this paper are from the Annual Survey of Hours and Earnings (ASHE). This is a panel survey collected by the UK's Office for National Statistics (2017). Employees are included in the survey if their National Insurance number ends in a specific pair of digits. Therefore, it can be used to follow the same individuals over time and it contains approximately 1% of employees in Britain. The survey began in 1997 is completed by employers in April of each year. The number of responses to this survey is around 180,000 in each year.

The ASHE data include detailed information on the pay and hours of work of each employee and the survey asks the employer whether the employee participated in a workplace pension plan ('run or facilitated by [the] organisation'). From 2004 onwards, it also asks how much the employer and the employee contributed to the pension. The data contain some information on the employee, such as age, sex, occupation and job tenure, and on the employer, such as industry and sector. Most importantly, the data contain the number of employees who worked for the employer in each year they are included in the data.[46] Therefore, we can define the set of individuals who are 'targeted' for automatic enrolment: those aged between 22 and the state pension age, those earning more than the automatic enrolment earnings threshold and those who have been working for their employer for at least three months. As public-sector workers are typically offered very different (defined benefit) pension plans, and had much higher participation rates prior to automatic enrolment, we focus entirely on employees of private-sector organisations in this chapter.

For one year of ASHE data only (2016), the UK's Office for National Statistics has kindly merged into the data the last two digits of the employers' PAYE code – a variable that is not routinely in the data made available to researchers or analysts. This allows us to determine which employers of

[46] For a small number of employers, we match information on their numbers of employees from the Annual Business Survey (see Office for National Statistics, 2018) if data on employer size in 2012 are not available in the ASHE but are available in the Annual Business Survey.

between 2 and 29 employees have introduced automatic enrolment in April 2016 and which have not, given the rules of the rollout set out in Section 5.3.[47]

This pseudo-random variation allows us to compare pension outcomes among employees of small employers who were subject to automatic enrolment (our treatment group), with those observed among employees of otherwise similar small employers who were not yet subject to automatic enrolment (our control group). Because treatment (i.e. being automatically enrolled) in April 2016 is (pseudo-randomly) assigned based on the last two digits of employers' PAYE numbers, we can use the control group as a counterfactual for the treatment group had automatic enrolment not been introduced.

Therefore, we define our treatment group as targeted employees working for employers with between 2 and 29 employees (in 2012) whose staging date was between 1 June 2015 and 1 November 2015.[48] All of these employees would have been more than three months past their staging date by April 2016, and therefore subject to automatic enrolment. Our control group is defined as targeted employees working for employers with between 2 and 29 employees (in 2012) who passed their staging date on or after 1 May 2016. This is because none of these employers would have been past their staging date in April 2016 when the ASHE survey was undertaken.[49] More details on the timing of staging dates for small employers are provided in Table A1 in the Appendix.

[47] Employees working for an employer with only one employee are excluded, as those who are sole-director of their own company and who employ themselves are not obliged to enrol themselves automatically into a workplace pension and these cases will comprise a significant share of employers with just one employee.

[48] We also exclude (from treatment and control groups) any employers that had 100 or more employees in April 2016. This is because we want to focus on small employers, and so do not want our results to be affected by a small number of unusual employers that have seen strong employment growth since 2012.

[49] There are two groups of targeted employees working for employers with between 2 and 29 employees that we do not include in either the treatment group or the control group. First, those with staging dates between 1 February and 1 April 2016 were not three months past their staging date when the ASHE survey was undertaken in April 2016, so we do not know if automatic enrolment had been introduced by then. Second, those who had a staging date of 1 January 2016 will have only just reached the point three months past the staging date in April 2016. Thus, these are excluded as any small delay of a few days in compliance with the obligations would bias our estimates of participation for those who are automatically enrolled.

Having defined our treatment and control groups, equation (5.1) sets out the specification that we use to estimate the impact of automatic enrolment on pension outcomes:

$$y_i = \alpha + \beta autoenrolled_i + \delta X_i + \varepsilon_i \qquad (5.1)$$

We want to estimate the effect of automatic enrolment on an outcome (such as pension participation) y, for an employee i. The variable $autoenrolled_i$ is a dummy variable taking the value of 1 if automatic enrolment was in place (the treatment group) and 0 if it was not in place (the control group). β is our coefficient of interest. We run specifications with and without controls for a vector of characteristics of employees and the employers they work for, X. These controls are for sex, age (quadratic), gross weekly earnings (quadratic), job tenure (linearly and three dummies), weekly hours of work (four dummies), number of employees in 2012, number of employees in 2016, 14 dummies for industry of the employer and eight dummies for occupational category of the employee. The full list of covariates can be found in Appendix Table D2.

The primary outcomes of interest are the effects of automatic enrolment on the probability of participation of a workplace pension plan and on the level and distribution of contributions. Using ordinary least squares (OLS), we estimate the effect on the probability of participation in a workplace pension plan and, for employer, employee and total pension contributions, we look at the effect on the probability of contributing more than several different thresholds.

Because automatic enrolment is implemented by employers, and they may implement it in slightly different ways (particularly in terms of how much they offer as an employer contribution), there may be a correlation in the error ε_i between employees working for the same employer. In all our results, therefore, we cluster our standard errors at the employer level, though in practice this makes little difference as these are all small employers; with a 1% sample of employees, each employer typically only appears in our data once in 2016.

Our key identifying assumption is that, absent automatic enrolment being introduced, the pension outcomes of employees working for employers with between 2 and 29 employees would not vary by

the last two digits of the employers' PAYE tax code. Therefore, a concern could arise if these codes were allocated in some systematic way that is correlated with pension outcomes. Unfortunately, in the earlier years of data we do not observe the last two digits of the employers' PAYE code and therefore we cannot conduct placebo tests to see if pension outcomes were different between the two groups in earlier years before automatic enrolment was introduced.[50] However, we can document the extent to which the characteristics observed in our data of the employees and employers in our treatment and control groups differ. This is shown in Table 5.1.

Table 5.1: Comparison of average characteristics of employees working for private-sector employers with between 2 and 29 employees in April 2012, by treatment and control group

	Treatment group	Control group	Difference
Employee characteristics			
Male	0.584	0.576	0.008
Gross earnings (£ per week)	487	480	6
Total paid hours per week	37.4	37.2	0.1
Job tenure (years with employer)	7.7	6.5	1.3**
Age	42.5	40.9	1.5
High occupational class	0.368	0.369	–0.001
Mid occupational class	0.402	0.41	–0.008
Employer characteristics			
Number of employees in 2012	13.7	13.3	0.3
Number of employees in 2016	15.8	15.0	0.8
Industry: Manufacturing	0.151	0.116	0.035
Industry: Retail	0.192	0.199	–0.007
Industry: Administrative	0.103	0.066	0.037
Industry: Health/social care	0.131	0.106	0.025
Number of observations	291	13,942	N/A

Notes: All averages are means. ***, ** and * denote that the effect is significantly different from zero at the 1%,5% and 10% levels, respectively. The *p*-values are adjusted for multiple hypotheses testing using the stepdown procedure described in Romano and Wolf (2005, 2016), implemented using the STATA program rwolf.
Source: Authors' calculations using the ASHE.

[50] One possibility would have been to link employers over time. However, small employers can only be in our data if they have an employee with a National Insurance number ending in a certain two digits. This leads to a lot of churn among the small employers included in our data from one year to the next, making this approach not possible in practice.

For almost all characteristics we find no evidence of statistically significant differences in average characteristics between our treatment and control groups. More importantly, in no case is the difference observed in our sample economically large enough to drive a substantially different pension outcome. For example, the difference in mean weekly earnings between our treatment and control groups is just £6 per week and is not statistically different from zero. The only difference that is statistically significant is the average job tenure; employees working for the treated employers have slightly longer job tenure (1.3 years). Although there is a slight difference in this variable, when we control for these observed differences our findings are not materially affected. This leads us to conclude that there are not any important differences in the observed characteristics between treatment and control groups. This also helps to justify our assumption that there are also not any important differences in unobserved characteristics between the groups that would drive differences in workplace pension saving.

5.5 Results

5.5.1 The effect of automatic enrolment on pension participation in small employers

Table 5.2 presents our estimates of the effect of automatic enrolment on workplace pension participation among employees working for smaller employers. The first specification shows that among targeted employees, the workplace pension participation rate was 70.1% in our treatment group compared with just 23.2% in our control group. This gives a raw difference of 46.9 ppts. The inclusion of our control variables (gender, age, gross weekly earnings, hours of work, job tenure, employer size in 2012 and 2016, industry and occupation) makes little difference to the estimated impact, as shown by specification 2 of Table 5.2, suggesting that automatic enrolment increased workplace pension participation by 44.0 ppts. This is our preferred estimate of the impact of automatic enrolment on pension participation.

One potential concern with our approach is that our control group could be contaminated by employers choosing to introduce automatic enrolment early. For example, some smaller employers with later staging dates could have decided to implement automatic enrolment a few months early in

order to attract and retain employees who might otherwise work for similar employers who had already reached their staging date.

To test this, we compare targeted employees in our treatment group with those in a different control group. For this alternative control group, we take all targeted employees working for employers with between 2 and 29 employees in April 2015 (i.e. several months before any of them would reach their staging date). As shown in Appendix Figure D.1, there is no evidence of any unusual change in workplace pension participation among this group at that point in time, and therefore no need to use employees in 2014, or earlier, as an alternative control group.

Table 5.2: Effect of automatic enrolment on private-sector pension participation using different methods (for employees of employers with between 2 and 29 employees in April 2012)

Specification number and description	Effect of automatic enrolment	Standard error	Sample size	Unadjusted pension participation in:	
				Control group	Treatment group
Main specification					
(1) Without controls	0.469***	[0.028]	14,756	23.2%	70.1%
(2) With controls	0.440***	[0.027]	14,756	23.2%	70.1%
Alternative specification					
(3) Without controls	0.503***	[0.028]	17,633	19.8%	70.1%
(4) With controls	0.476***	[0.027]	17,633	19.8%	70.1%

Notes: ***, ** and * denote that the effect is significantly different from zero at the 1%, 5% and 10% levels, respectively. Standard errors are clustered at the employer level. Results are from estimating equation 5.1 by OLS. Full list of controls is given in Table D.2. Main and alternative specifications are outlined in the text.
Source: Authors' calculations using the ASHE.

Workplace pension participation among this alternative control group is lower than in the contemporaneous control group. If anything, this suggests that automatic enrolment may have had a larger impact on pension participation than our main specification suggests. Without controls (specification 3) we find a 50.3 ppts impact of automatic enrolment, while with controls (specification 4) this drops slightly to 47.6 ppts. Overall, the use of this alternative control group would not change our conclusion that automatic enrolment boosted pension participation among this group by around 45 ppts.

Overall, we find that 70% of targeted employees working for employers with between 2 and 29 employees participate in workplace pensions once automatic enrolment is in place. This rate is much lower than has been seen elsewhere in the literature. In Chapter 4, we found participation of around 88% for targeted employees working for employers with 58 or more employees in the UK, which is similar to the participation rates seen among the large companies studied in Madrian and Shea (2001). Choi et al. (2004) and Beshears et al. (2009) also find that medium-sized and large companies in the United States with automatic enrolment have pension participation rates in excess of 90%. Our findings imply that, while the boost to pension participation is still extremely large, automatic enrolment for employees of small employers leads to many fewer people participating than when automatic enrolment is introduced by large employers.

In Appendix Table D.3, we examine to what extent the effects on pension participation are heterogeneous. In all cases the point estimates suggest a larger impact on the subgroup with lower pension participation among our control group (men, lower earners, younger employees and those with shorter job tenures, though the differences are only statistically significantly for younger employees compared to older employees, and for those with shorter job tenures compared to those with longer job tenures. Importantly, in none of the subgroups does pension participation reach the very high overall rates (around 90%) seen among employees working for larger employers once automatic enrolment is in place. In addition, among the non-targeted employees, we find a sizeable 13.9 ppt increase in workplace pension participation as a result of automatic enrolment. This is a slightly smaller magnitude than the 17.8 ppt increase in pension participation among non-targeted employees of larger employers as a result of automatic enrolment found in Chapter 4.

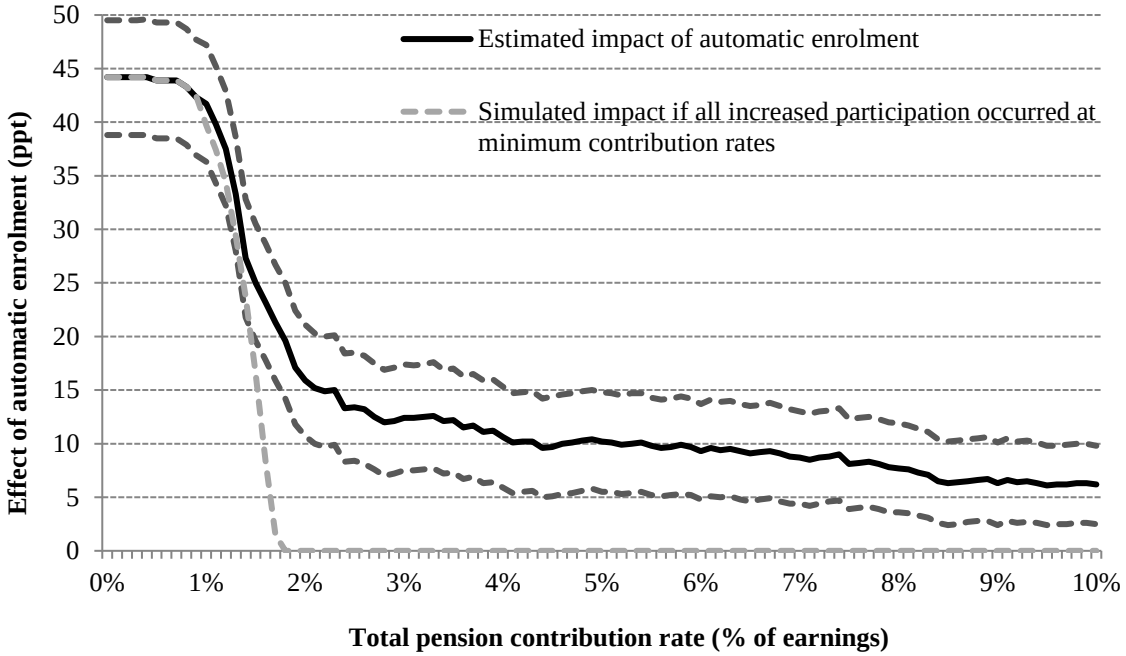
5.5.2 The effect of automatic enrolment on the distribution of pension contributions

Ultimately the adequacy of retirement saving will depend on how much is saved. Therefore, we now turn to look at the impact of automatic enrolment on pension contributions. Specifically, we look at the total contributions made to the pension plan (i.e. the sum of contributions made by the employee and the employer) measured as a share of earnings. We then run a set of OLS regressions to see

whether once automatic enrolment is in place, targeted employees in the treatment group are more likely than those in the control group to have a pension contribution worth more than different thresholds. Specifically, we look at the likelihood of receiving a pension contribution of more than 0% of earnings, then more than 0.1% of earnings, then more than 0.2% of earnings, right up to more than 10.0% of earnings (i.e. a total of 101 separate regressions).

The results of this exercise, presented in Figure 5.1, show that the majority of targeted employees brought into workplace pensions by automatic enrolment are making relatively low levels of contributions, focused on the default minimum (which, in April 2016, was 2% of 'qualifying' earnings). This is shown by the 44 ppts increase in the probability of making contributions of more than 0.5% of earnings, but only a 16 ppts increase in the likelihood of making contributions of more than 2% of earnings.

Figure 5.1: Effect of automatic enrolment on probability of total contribution rate being greater than certain percentages of earnings, for targeted employees of small employers



Notes: Each of the data points (at each 0.1% of earnings) is the coefficient on automatic enrolment from estimating equation 5.1 where the dependent variable is a dummy indicating if the contribution rate is in excess of the specified amount. Each regression is estimated by OLS including control variables (*X*, listed in Table D.2). Standard errors are clustered at the employer level and used to construct the 95% confidence intervals shown by dotted lines. The grey dashed line shows the simulated impact on contribution rates had the entire increase in pension participation occurred at the minimum default contribution rates and had no one else changed their pension saving in response to the reform.
Source: Authors' calculations using the ASHE.

However, we also find evidence that the proportion brought in at significantly higher rates of contributions has also increased as a direct result of automatic enrolment. For example, the proportion of targeted employees contributing more than 5% of their earnings to a workplace pension is increased by 10.2 ppts, while the proportion contributing more than 10% of earnings is increased by 6.2 ppts. All of these effects are statistically different from zero, at least at the 5% level.

As a comparison, the grey dashed line in Figure 5.2 shows the simulated impact of automatic enrolment on the probability that total pension contributions are in excess of different amounts, assuming that every employee enrolled in a pension plan as a result of automatic enrolment (specifically the 44.0% of employees shown in Table 5.2) was enrolled with only the minimum default contributions. By comparing the estimated impact of automatic enrolment (the solid black line) with this simulated effect, we can see that if only the minimum contributions were made, there would be no effect on the probability of having contributions in excess of 2% of salary.

Figures D.2 and D.3 in the Appendix show that this effect is a result of employees having both higher employee contributions and higher employer contributions than the government's minimum defaults. This finding is similar – and, if anything, more apparent – than the equivalent among medium-sized and large employers in the United Kingdom reported by in Chapter 4, where we also found that there were significant increases in the probability of having contributions in excess of the minimums.

5.5.3 Differences in pension participation by employer size under automatic enrolment

It is clear from these results, even with automatic enrolment in place, that employees of smaller employers are significantly less likely to participate in a pension plan than employees of larger employers. This is shown in particular detail in Table 5.3, which looks at private-sector employers that have introduced automatic enrolment. The final column shows the workplace pension participation rate by employer size, for targeted employees in April 2016. (Note that this table measures pension participation by employer size in 2016, whereas the results so far have focused on those employers who had between 2 and 29 employees in 2012.)

The table shows that for employers with 500 or more employees, pension participation is between 87% and 90%. However, for employers with fewer than 100 employees, pension participation is much lower, and it is, in particular, substantially lower for those with fewer than 25 employees.

Specification 1 of Table 5.3 presents the differences in pension participation rates among employees of different sized employers, relative to those working for the largest employers. It confirms that it is among those working for employers with fewer than 100 employees that the differences in pension participation rates are significantly different. We can test some hypotheses as to why – even under automatic enrolment – pension participation is lower among smaller employers than among large employers.

Table 5.3: Differences in pension participation for employees targeted for automatic enrolment working for private-sector employers who have introduced it (April 2016)

	Specification 1		Specification 2		Pension participation rate
	Coefficient	Std error	Coefficient	Std error	
Number of employees					
≥10,000	Baseline group		Baseline group		88.3%
5,000–9,999	0.025	[0.031]	–0.003	[0.024]	90.8%
1,000–4,999	–0.014	[0.030]	–0.033	[0.024]	86.9%
500–999	–0.006	[0.030]	–0.024	[0.023]	87.8%
250–499	–0.019	[0.029]	–0.034	[0.023]	86.5%
100–249	–0.041	[0.029]	–0.052**	[0.023]	84.2%
50–99	–0.117***	[0.029]	–0.122***	[0.024]	76.7%
25–49	–0.217***	[0.030]	–0.218***	[0.025]	66.7%
2–24	–0.257***	[0.038]	–0.248***	[0.034]	62.6%
Controls included	No		Yes		
Number of observations	76,419		76,419		
Number of clusters	21,708		21,708		

Notes: The sample is private-sector employees who are targeted for automatic enrolment and work for employers where automatic enrolment has been introduced. Employer size is measured in April 2016. Results are from regressions with pension participation as the dependent variable, and eight dummies for employer size as the dependent variables, estimated by OLS. Specification 2 includes control variables for gender, age (quadratic), gross weekly earnings (quadratic), hours of work (four dummy variables), job tenure (three dummy variables and linearly), industry (14 dummy variables) and occupation (eight dummy variables). Standard errors are clustered at the employer level. ***, ** and * denote that the effect is significantly different from zero at the 1%, 5% and 10% levels, respectively.
Source: Authors' calculations using the ASHE.

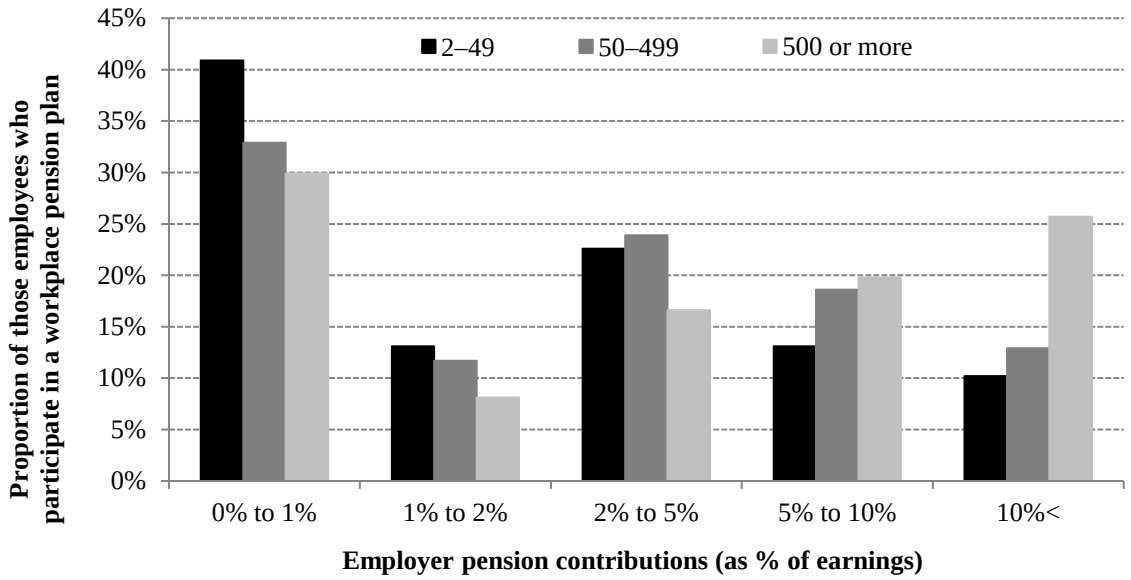
As set out in Section 5.2, one reason that those working for smaller employers may have lower pension participation rates is that the types of people working for smaller employers are

systematically different. For example, it is well known that smaller employers pay less, on average, than larger employers (Bloom et al., 2018). In specification 2 of Table 5.3, therefore, we control for the differences in the rich set of characteristics observed in the ASHE, specifically sex, age, gross weekly earnings, hours of work, job tenure, industry and occupation. The results show that – once automatic enrolment is in place – none of the differences in the pension participation rates between employees of larger and smaller employers is driven by this rich set of observed factors.

There could be unobserved characteristics that vary between the employees of smaller and larger employers that play a role in driving retirement savings and pension participation, such as different levels of self-control (see Laibson et al., 1998) or locus of control (Cobb-Clark et al., 2016). However, given that differences in age, earnings, industry and occupation, job tenure, etc., – all of which correlate strongly with pension participation (as shown in Appendix Table D.2) – play only a very minor role in explaining lower pension participation among smaller employers in an automatic enrolment environment, it would be very surprising if unobserved factors were important in driving the substantial differences in the rates of pension participation.

An alternative reason for lower participation in workplace pensions could be less generous employer contribution offers from smaller employers than from larger employers. Figure 5.2 shows the distribution of employer pension contributions for those participating in a workplace pension plan, for employees working for employers of 2–49, 50–499 and 500 or more employees. It shows that 41% of pension participants working for the smallest employers receive at most 1% of earnings as an employer contribution, compared with 30% of those whose employers employ at least 500 employees.

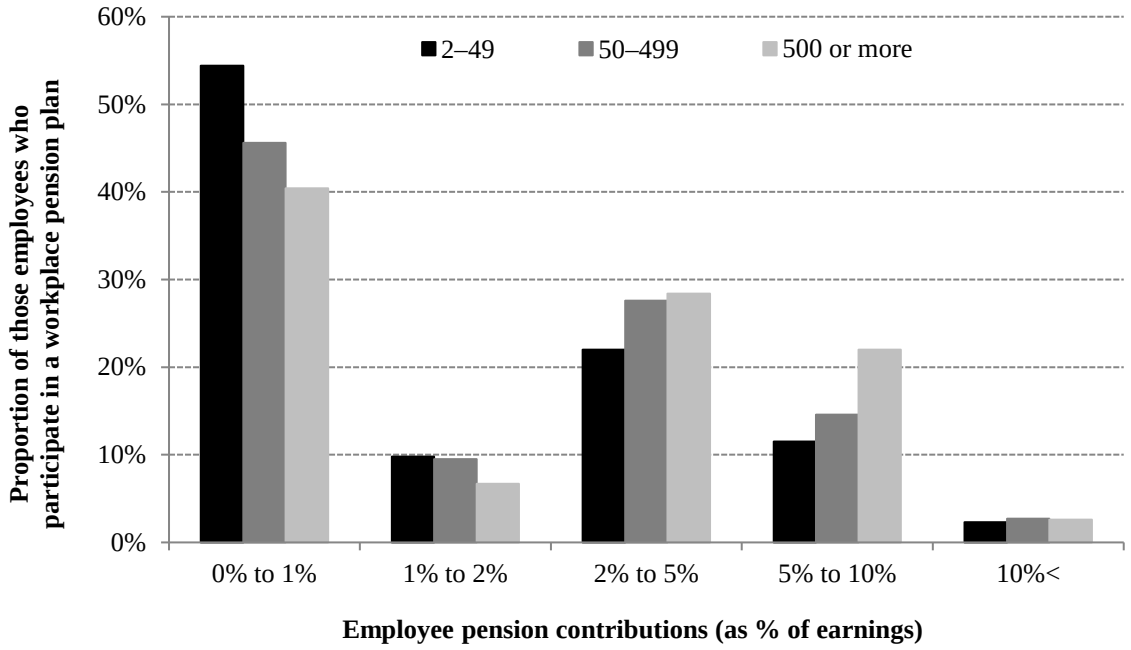
Figure 5.2: Employer pension contributions for private-sector employees participating in a workplace pension plan after automatic enrolment, by employer size in 2016



Notes: For each band (most importantly, 0% to 1%), employees are included in the band if their contribution rate is strictly greater than the lower amount and weakly less than the upper amount. Restricted to those targeted for automatic enrolment.
Source: Authors' calculations using the ASHE.

More generous employer pension contributions from larger employers could potentially rationalise some of the differences in participation rates between large and small employers. However, if employees of larger employers have to make potentially significant employee contributions in order to receive the employer contributions this potential channel might be weaker. Figure 5.3 shows that enrolled employees working for large employers are more likely to make higher *employee* contributions than those working for small employers.

Figure 5.3: Employee pension contributions for private-sector employees participating in a workplace pension plan after automatic enrolment, by employer size in 2016



Notes: Restricted to those targeted for automatic enrolment.
Source: Authors' calculations using the ASHE.

In absence of data on employers' pension *offers*, in order to assess whether differences in offers of pension contributions could be driving the 25 ppts difference in participation between the smallest and largest employers found in Table 5.3, we relate the employer-level average employee and employer contributions among large employers in ASHE to individual pension participation decisions under automatic enrolment.

In Table 5.4, we take a sample employees who work for 134 large private-sector employers that employ at least 5,000 employees (such that there at least 50 observations in our data). We relate the average employer and employee contribution rates (measured among the other targeted employees of the same employer) to the individual's decision over whether or not to opt out of a workplace pension having been automatically enrolled. Specification 1 includes no other controls, and specification 2 controls for a set of employee and employer characteristics.

Higher average employer contributions are indeed associated with higher pension participation, and higher employee contributions are associated with lower levels of pension participation, although the standard errors are quite large as there are only 134 employers in the sample. The association between

the (employer-level) average employer pension contributions is weaker once controlling for employee and employer characteristics such as occupation, earnings and job tenure.

Table 5.4: Relationship between employer-level-average employer and employee pension contributions and pension participations, large employers April 2016

Average contribution rate of the employee's employer	Specification 1		Specification 2	
	Coefficient	Std error	Coefficient	Std error
Employer: 0% to 1%	Baseline group		Baseline group	
Employer: 1% to 2%	0.039	[0.035]	0.034	[0.041]
Employer: 2% to 5%	0.055	[0.037]	0.049	[0.052]
Employer: 5% to 10%	0.100***	[0.028]	0.039	[0.045]
Employer: 10% plus	0.152***	[0.042]	0.084	[0.053]
Employee: 0% to 1%	Baseline group		Baseline group	
Employee: 1% to 2%	0.013	[0.027]	–0.009	[0.034]
Employee: 2% to 5%	–0.032	[0.026]	–0.071*	[0.037]
Employee: 5% plus	–0.079	[0.059]	–0.103*	[0.056]
Controlling for other characteristics	No		Yes	
Number of observations	18,820		18,820	
Number of clusters	134		134	

Notes: The sample is private-sector employees who are targeted for automatic enrolment and work for employers where automatic enrolment has been introduced. Results are from regressions with pension participation as the dependent variable, and dummies for average contribution rates of targeted employees in their employer (excluding the individual themselves in the calculation), estimated by OLS. Specification 2 includes control variables for gender, age (quadratic), gross weekly earnings (quadratic), hours of work (four dummy variables), job tenure (three dummy variables and linearly), industry (14 dummy variables) and occupation (eight dummy variables). Standard errors are clustered at the employer level. ***, ** and * denote that the effect is significantly different from zero at the 1%, 5% and 10% levels, respectively.
Source: Authors' calculations using the ASHE.

If the same relationship that is observed among the employees of large employers also holds for those working for smaller employers, the different offers of pension contributions cannot explain the very large gap in pension participation between large and small employers under automatic enrolment. Indeed, even if all employees of small employers were offered the minimum contributions, and all employees of large employers were offered the highest rate of contribution, this would only lead to an 8 ppts difference in participation. In fact there is only an 11 ppt gap between employees of large and small employers in terms of the probability of receiving the minimum employer contribution.

This analysis finds that differences in a rich set of observed characteristics do not drive the large difference in pension participation between small and large employers. We also find that less

generous employer pension offers are unlikely to be driving lower pension participation for small employers under automatic enrolment. Given that observed differences in job tenure, earnings and age, all of which are correlated with pension participation, do not drive the difference in pension participation, unobserved characteristics seem unlikely to be the main explanation of the difference.

Instead, it is much more likely that a combination of differences in the way that small employers administer their pension arrangements and the role of peer effects from other employees in the workplace are important in driving the difference in pension participation rates. A priority for future research is therefore to investigate the effect of employers' administration of their schemes and intra-employer peer effects on workplace pension saving in the context of automatic enrolment.

5.6 Conclusion

With automatic enrolment increasingly being considered by governments internationally, not least by state governments in the United States, to increase private retirement provision, it is important to understand its effects on those working for smaller employers, who typically have much lower rates of participation in pension plans, and so are much more likely to be directly affected by automatic enrolment.

This is the first paper to assess the impact of obliging small employers to enrol their employees automatically into a workplace pension. By exploiting the pseudo-random timing – based on the last two digits of the employer's payroll tax code – of the obligation for small private-sector employers in the United Kingdom to enrol their employees automatically into a workplace pension plan, we find that automatic enrolment substantially boosts pension participation, by around 45 ppts, to reach around 70% participation among targeted employees. We find that while most employees are automatically enrolled at low minimum default contributions, there are also significant increases in the proportion with pension contributions well in excess of the minimums. There are also increases in the proportion of employees who do not have to be automatically enrolled but who do participate in a workplace pension. Although we do not observe the saving behaviour of employees outside their workplace pensions, if the results of Chetty et al. (2014) are generalisable (who find that most

individuals are sufficiently ‘passive’ not to reduce their saving in other forms), then we can conclude that automatic enrolment does indeed increase overall saving among those working for small employers. Despite automatic enrolment increasing pension participation among those working for small employers to 70%, this remains well below the 90% levels seen among medium-sized and larger employers both in the United Kingdom and the United States. We show that this lower participation among small employers cannot be rationalised by differences in a rich set of observed individual and employer characteristics. This includes the employees’ age, occupation, job tenure, hours of work and earnings. Furthermore, while employer pension contribution rates are somewhat less generous among smaller employers, these can only explain very little of the gradient in pension participation by employer size that persists after automatic enrolment. While there will be unobserved differences in the characteristics of employees working for small and large employers, it is unlikely that these are large enough to drive the differing participation rates. Much more likely is that a combination of differences in the way that small employers administer their pension arrangements, and the role of peer effects from other employees in the workplace, are important in driving the difference in pension participation rates.

Chapter 6: Entering the labour market in a weak economy: scarring and insurance

6.1 Introduction

How are individuals' living standards affected when they are unlucky enough to enter the labour market during a recession? In the aftermath of the Great Recession, there has been renewed interest in the extent to which young adults have been negatively affected by entering the labour market at a bad time. A body of previous research has found large and persistent negative impacts ('scarring') on the earnings and employment rates of people who leave education during a recession. However, when individuals face economic shocks, they may have access to forms of insurance (such as the tax and state transfer system) that mitigate the effects on their living standards. Given that individuals' living standards are of primary interest, it follows that the role of insurance is key to understanding the consequences of labour market scarring.

In this chapter, we provide the first estimates of the impacts of entering the labour market when the economy is weak on the most commonly used measures of households' material living standards – net (post-tax-and-transfer) household incomes, and household consumption expenditures. By first looking at the impacts on the individual labour market outcomes studied in previous work, and then examining in sequence the effects on other outcomes, we identify which insurance mechanisms are important in insuring against the shock of entering the labour market at a bad time. We use long-running and consistent repeated cross-sections of household survey data from the United Kingdom from 1978 to 2016, and exploit the fact that there have been three recessions (and recoveries from them) in that time, meaning individuals entering the labour market a few years apart faced very different initial economic conditions.

We find that there are substantial and persistent negative causal effects of entering the labour market when unemployment is high on the probability of being in work and on the pre-tax earnings of those in work. However, we find only little impact on household net incomes or expenditures, even in the years immediately after leaving education when the labour market impacts are particularly large.

To explain this, we investigate a number of intermediate outcomes, which isolate the role of different insurance mechanisms. The key forms of insurance are the tax and transfer system and, most importantly on average, the incomes of parents, with whom many young people live in the years after they leave education. We show that the interplay between heterogeneity in labour market scarring and in insurance is key to understanding why parental income insures so much of the shock for young adults. On average, it is lower-educated young adults who experience the worst scarring in the labour market; but this is also the group that is most likely to live with their parents in the years after leaving education. Moreover, because most of the labour market scarring fades away after around five years, it does not outlast the period in which a high proportion of young people live with their parents (especially for those whose labour market outcomes are most negatively impacted by entering the labour market in a recession). Because living with parents is such an impact insurance mechanism, this highlights the importance of understanding the degree to which resources are shared between parents and their co-resident children.

This chapter contributes to two literatures in economics that have to date remained distinct, and our main aim is to bring them together. The first is the 'scarring' literature on the persistent impacts of entering the labour market during a recession, or when unemployment is high, on labour market outcomes. Previous work on the impact of entering the labour market during a recession has shown that doing so can lead to significant and persistent negative impacts on individuals' employment and earnings. Oreopoulos et al. (2012), using Canadian data, and Kahn (2010) and Altonji et al. (2016), using American data, all find persistent negative impacts on subsequent earnings of labour market conditions upon entry for college graduates, albeit with varying degrees of persistence.[51] Whereas most papers focus on the experience of young adults in the first decade of working-age life, Rothstein (2019) examines whether there are persistent effects of leaving college education beyond the first 10

[51] Labour market institutions may be important determinants of scarring. Genda et al. (2010) find that scarring impacts of entering the labour market at a bad time are worse in the more rigid Japanese labour market than in the more flexible American system. Of all developed countries, however, the UK probably has the labour market institutions most similar to the US and Canada, making the effects potentially more comparable than if this study focused on a continental European country.

years, and finds permanently lower employment rates for those cohorts that experienced early career recessions.

Research on labour market scarring in European countries which looks not solely at university graduates, but also at those without tertiary education, has found significant and persistent negative impacts on employment probabilities (Burgess et al. (2003) in the UK, Raaum and Røed (2006) in Sweden, Schmillen and Umkehrer (2017) in Germany, Brunner and Kuhn (2014) in Austria and Haaland (2018) in Norway).

Of the many potential mechanisms behind these scarring effects, the literature has suggested that experiencing unemployment itself may worsen later life economic outcomes – for example, through the depreciation of human capital (Pissarides 1992) or becoming psychologically discouraged (Clark et al. 2001). In addition, there is evidence that leaving education in a recession leads to individuals taking lower-skilled work (Kahn 2010), in lower-quality or lower-paying firms (Brunner and Kuhn 2014; Oreopoulos et al. 2012), and to worse matches between workers and firms (Liu et al. 2016).

The second literature to which we contribute seeks to understand how individuals and households respond to economic shocks, and what insurance mechanisms allow their living standards to be insulated against these shocks. This literature has shown that key insurance mechanisms against shocks to wages or earnings include the tax and transfer system, though to a greater extent in Europe than in the US (Dolls et al. 2012), and family labour supply responses (Blundell, Pistaferri and Saporta-Eksten 2016). The tax and transfer system is a particularly important insurance mechanism for low skilled (Blundell et al 2015). These can insure even persistent shocks (though, of course, taxes and transfers cannot insure *permanent* aggregate shocks, of which the Great Recession was almost certainly one, without structurally weakening the public finances). Both mechanisms reduce the extent to which wage or earnings shocks actually result in falls in net household incomes.

In addition, of course, households may be able to self-insure through the use of precautionary saving such that the impact of earnings shocks on their consumption, and hence living standards, is reduced or eliminated. This has also been found to be an important insurance mechanism, though to a lesser

degree for persistent or permanent shocks, and to a lesser degree for low-educated households, who tend to have fewer assets (Blundell, Pistaferri and Preston 2008). Wider family networks, most importantly parents, can potentially play important roles in providing insurance against shocks. Kaplan (2012) finds that the ability of young adults to move back home provides an important insurance mechanism against labour market shocks, though Attanasio et al (2018) find little evidence of insurance provided by family networks once adult children have formed their own households.

Integrating these hitherto distinct literatures, on scarring and on insurance, is essential in order to understand properly the costs of scarring for young adults. Being unlucky enough to start one's career during a recession can be thought of as one type of shock that affects earnings and the probability of employment. It is a shock with a particular degree of persistence, potentially varying by subgroup, and it is a shock that occurs at a very specific stage in the life cycle when the mix of insurance available is likely to be very different from that for the population as a whole (and hence from that studied by previous literature).

For example, young adults at the start of their careers are much less likely to have partners or assets (two potential important forms of insurance), but much more likely to have co-residence with parents as a form of insurance. On the other hand, the prevalence of both of these forms of insurance changes quite quickly (in opposite directions) in the years after labour market entry, meaning that the relationship between the persistence of labour market scarring and the availability of insurance at different stages in young adulthood is a key determinant of the overall impacts of scarring on living standards. In addition, the way in which heterogeneity in labour market scarring relates to heterogeneity in the availability of insurance will be important: it matters whether the most scarred in the labour market are the least or most insured, for example.

For all these reasons, it is difficult to infer much from existing work about the consequences of entering the labour market at a bad time for young adults' living standards. To do that, one needs to study the role of insurance in this specific case. By tracing the impacts of entering the labour market in a weak economy from the traditional 'raw' individual labour market outcomes right through to

household net income and household expenditure, we seek to better understand the true impacts on individuals' living standards and the insurance mechanisms that can mitigate these effects.

The rest of this chapter proceeds as follows. Section 6.2 describes the data and Section 6.3 sets out the empirical methodology that we use to estimate the scarring impacts on measures of earnings, income and expenditure. Section 6.4 presents our results and Section 6.5 concludes.

6.2 Data

We pool data from two long-running repeated annual cross-section UK household surveys – the Family Expenditure Survey (FES, see Office for National Statistics 2002) and its successors (the Expenditure and Food Survey and the Living Costs and Food Survey), and the Family Resources Survey (FRS, see Department of Work and Pensions et al 2018). The FES surveys between around 5,000 and 6,500 households per year. We use FES data starting from 1978, when it began to record the age of leaving education, up to the latest data in 2016. As well as detailed demographic information about each household, the survey asks about all sources of income for all members of the household, and it records household expenditures, asking what all members of the household buy over a two-week period, plus regular payments such as household bills, rent and mortgage payments, and purchases of durable items that do not occur frequently.

The FRS is an annual cross-sectional data set that contains around 20,000 households per year. It is available from 1994–95 to 2016–17. Like the FES, it contains a battery of questions designed to measure individual and household incomes and sources of income, as well as the demographic characteristics of the household and the age at which adults left education. Unlike the FES, it does not measure household expenditures. For analysis of all other outcomes, we use the pooled FES and FRS data, since both data sets contain all other outcomes. The two data sets generally align very closely, but we control for any systematic differences between them (as explained in Section 6.3).

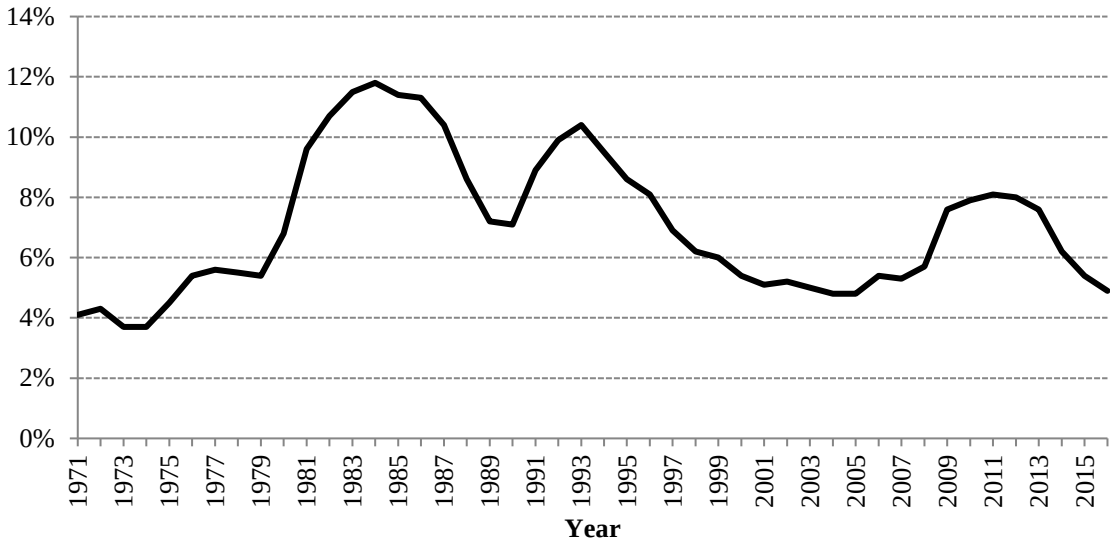
We use the derived income variables available in the data sets, which construct consistent measures of income from different sources before and after tax. These are constructed by the UK's Office for

National Statistics (ONS) for the FES and its successors and by the Department for Work and Pensions (DWP) for the FRS. The derived household income variables based on the FRS are also published separately in a data set known as 'Households Below Average Income' (which, despite the name, includes households of all income levels), and this underpins the official UK statistics on poverty and the income distribution.

We are interested in the impact of the state of the economy at the point at which individuals enter the labour market and how persistent those impacts are. We use the national unemployment rate as the measure of the state of the economy faced by individuals entering the labour market. We also show the robustness of our key results to using a different measure of the economy – the estimated output gap (as a percentage of potential GDP), which is constructed by the UK government's official and independent forecaster, the Office for Budget Responsibility (see Appendix Table E.3).

The measure of unemployment is constructed on a consistent basis under the International Labour Organisation (ILO) definition of unemployment since 1971 by the Office for National Statistics, and its evolution since 1971 is shown in Figure 6.1. It can be seen that there has been a large amount of cyclical variation over this period, including three major recessions and the recoveries from them: the first in the early 1980s, with unemployment peaking at 11.8% in 1984; the second in the early 1990s, with unemployment peaking at 10.4% in 1993; and the third being the 'Great Recession' in which unemployment rose to 8.1% in 2011. On average, the increase in the unemployment rate between the year before the recession and the peak in unemployment is around 4 percentage points.

Figure 6.1: Unemployment rate in the UK



Notes: Aged 16 and over. *Source*: UK Office for National Statistics series MGSX.

We use the FES and FRS data to look at scarring impacts on a set of different outcomes. First, we examine the probability of individuals being in paid employment and the weekly gross earnings of those who are employed or self-employed. We then look at a set of 'family-level' variables – gross earnings of those families with an adult in work, total private income (before taxes are paid and state transfers received), and total net (post-tax-and-state-transfers) income. The definition of a 'family' here is important: it is defined narrowly as an individual, their cohabiting partner (if they have one) and any dependent children of that individual/couple. In the UK, this is sometimes known as a 'benefit unit', reflecting that it is the unit used to assess entitlements to means-tested benefits; it is essentially equivalent to a US 'tax unit'. Hence 'families' in this sense do not necessarily contain all members of a household, or even all related members of a household.

We then examine the same variables at the household level. The household can differ from the 'family' if two or more 'families' live together. Examples of this would be adult children living with their parents, as well as two (or more) adults living together in a house where they do not form a cohabiting relationship/partnership. When we measure household net income, we equivalise using the OECD modified equivalence scale, as this is as a standard income-based measure of household living standards (though equivalisation makes a negligible difference to our results). Using the FES data, we

also look at total (equivalised) household expenditure. This includes both durable and non-durable expenditures and housing expenditures.

All financial amounts are adjusted for inflation and expressed in 2016–17 (the financial year running from April 2016 to March 2017) prices. We adjust for inflation using a modified version of the Consumer Prices Index (which additionally incorporates owner-occupied housing costs). This is the same measure of inflation as UK government uses when constructing its statistics on the income distribution. For all financial variables, we trim the bottom and top 1% of the sample in each year and exclude those observations from the analysis that uses those variables.

We select our sample (from both the FRS and FES data) based on the following criteria. Each individual must have left education between their compulsory school-leaving age (which varies by birth year) and age 25; they must have left education since 1971 (because we only observe the national unemployment rate from this year); and we keep observations for these individuals if they are within 10 years of having left education. We include people who are observed in the same calendar year as they left education (which we call 'zero' years since leaving education), as long as the observation is in July–December (final school examinations, plus almost all university courses, finish by July). This gives a total sample size of 205,349 individuals.

Table 6.1 shows the means of each of the outcomes of interest in the pooled FES/FRS sample, for each of the first 10 years after leaving education. Financial variables are shown in pounds sterling in 2016–17 prices. The table shows that, excluding the year individuals leave education, there is little change in the employment rate after leaving education. In comparison, the experience profile of earnings is steep, increasing from £250 per week in the first year after leaving education to £375 per week four years later. A striking feature of the table is that, while (unequivalised) family-level earnings and incomes also grow steeply in the years after leaving education, household-level incomes and expenditures do not.

Table 6.1: Means of measures of employment, earnings, income and expenditure (£ per week, 2016–17 prices)

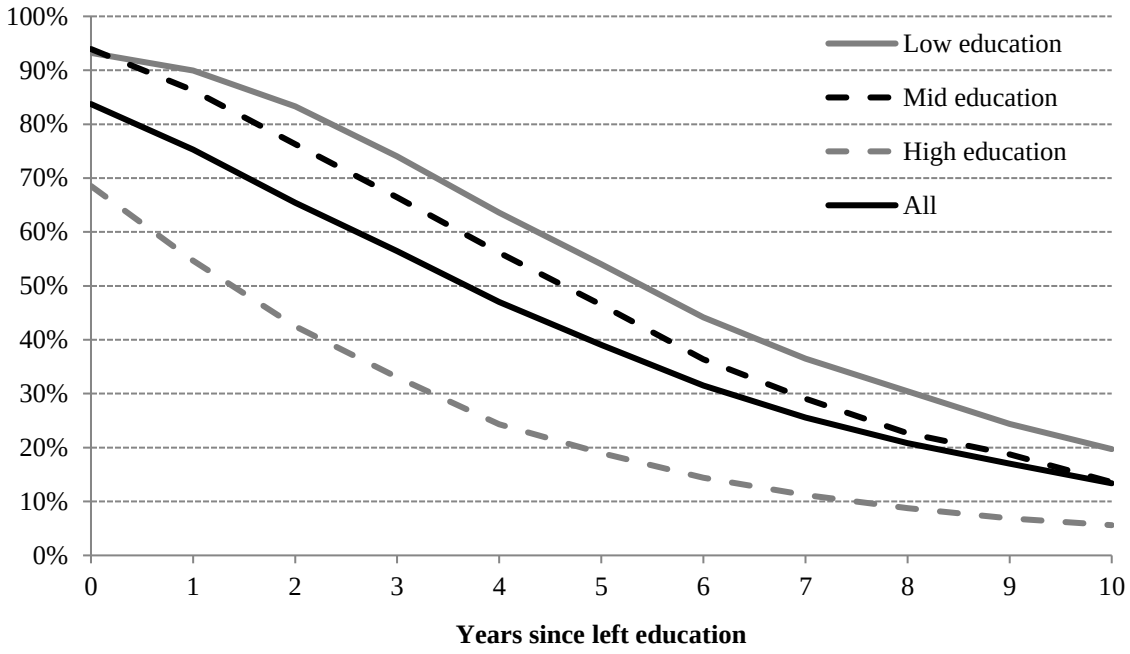
Years since left education	In paid work	Individual earnings for workers	For working families					For all families	
			Family earnings	Family private income	Family net income	H'hold private income	H'hold equiv. net income	H'hold equiv. net income	Total equiv. expenditure
0	0.61	207	246	252	213	945	534	472	435
1	0.74	250	302	307	251	947	544	492	443
2	0.77	288	365	371	297	940	551	504	451
3	0.78	321	427	434	344	927	556	508	448
4	0.77	350	493	501	393	913	566	517	448
5	0.77	375	543	551	431	897	572	523	448
6	0.76	396	593	602	470	872	571	524	447
7	0.76	408	624	634	498	857	571	525	445
8	0.77	425	665	676	531	850	574	530	451
9	0.76	432	687	699	550	840	569	529	457
10	0.76	446	714	727	571	840	570	530	451

Note: 'equiv.' variables are equivalised using the OECD modified equivalence scale and expressed as the equivalent for a childless couple. Inflation is adjusted for using a modified version of the Consumer Prices Index that includes owner-occupied housing costs.
Source: Authors' calculations using the FRS and FES (and successors) from 1978 to 2016.

The main reason for this can be seen from Figures 6.2 and 6.3, which show respectively the percentage of individuals living in the same household as their parents and the percentage living with a partner or spouse, by years since leaving education, and how that varies for different education groups in each case. Pooling all the data used in our analysis, one year after leaving education 75% of individuals lived with their parents and 39% did even five years after. Low-educated individuals are particularly likely to live with their parents, with 90% doing so a year after leaving education and 54% doing so five years after. The lower percentage for higher-educated individuals is driven by the fact that they enter the labour market at an older age.

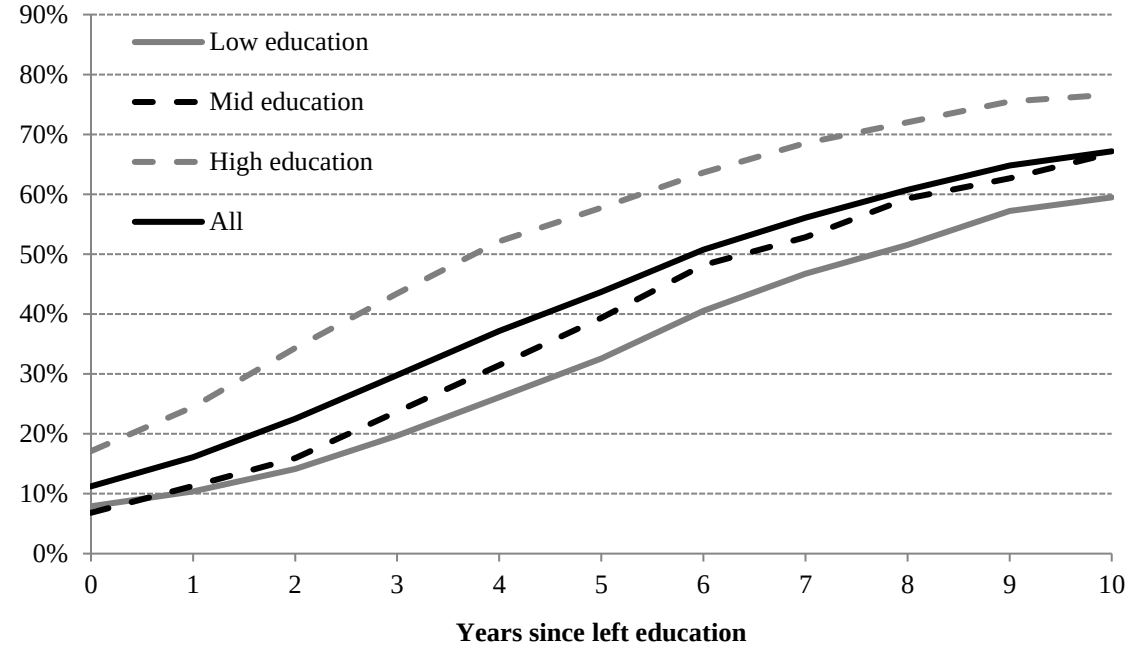
In comparison, relatively few people live with a cohabiting partner (or spouse) soon after leaving education, though of course the fraction increases with time. One year after leaving education, 16% cohabit with their partner, with 44% doing so five years after and 67% 10 years after. Higher-educated people are more likely to be living with a partner in the years after leaving education, again at least in part driven by the older age at which they leave education.

Figure 6.2: Percentage of individuals living in same household as their parents, by years since left education



Note: Low education – left education at age 16 or younger. Mid education – left education at age 17 or 18. High education – left education between ages 19 and 25.
Source: Authors' calculations using the FRS and FES (and successors) from 1978 to 2016.

Figure 6.3: Percentage of individuals living with a cohabiting partner or spouse, by years since left education



Note: Low education: left education at age 16 or younger. Mid education: left education at age 17 or 18. High education: left education between ages 19 and 25.
Source: Authors' calculations using the FRS and FES (and successors) from 1978 to 2016.

6.3 Empirical Methodology

We estimate the impact of entering the labour market when unemployment is high by exploiting variation in unemployment rates in the UK since 1971. This period includes three large recessions, and the recoveries from them, during which the unemployment rate varies substantially even over short periods, as depicted earlier in Figure 6.1. For example, someone leaving education in 1979 faced an unemployment rate of only 5.4%, whereas someone leaving education two years later in 1981 faced an unemployment rate of 9.6%.

Using repeated cross-sections of pooled micro-data from the FRS and the FES, we estimate the following model for different outcomes:

$$y_{ict} = \alpha + \sum_{s=0}^{2}\sum_{e=0}^{10}\gamma_{es}1[experience = e] \times 1[educ = s]$$

$$+ \sum_{e=0}^{10}\beta_e[unemp\_lefted_c] \times 1[experience = e] + f(yearlefted_c) + \mu_t + \theta X_{ict} + \varepsilon_{ict} \quad (6.1)$$

where y_{ict} is an outcome (such as log household net income) for an individual *i* who belongs to year-left-education cohort *c* and who is observed at time *t*. The outcome of interest is allowed to depend on a full set of interactions between experience years (measured as years since left education) and education level (*experience* and *educ*).[52] We find the complete flexibility in experience gradients (for each education group) to be important in this context, as it is difficult to accurately capture the experience profiles of earnings right at the very start of a career with low-order polynomials. The outcome is also potentially affected by the unemployment rate in the year the individual left education (*unemp_lefted*), with the effect potentially varying with the number of years since the individual left education ('experience') to allow for scarring effects to fade over time. The estimated coefficients on these variables, β_e, are the coefficients of primary interest.

[52] The three-category measure of education is defined in the following way: low education – left education at age 16 or younger; mid education – left education at age 17 or 18; and high education – left education between ages 19 and 25.

An important control is $f(yearlefted_c)$, which allows for underlying cohort effects independent of *unemp_lefted* (which also varies at the cohort level). The functional form we use is a set of 'five-year cohort' dummy variables based on when the individual left education, i.e. there is one dummy for people leaving education between 1978 and 1982, another for those leaving education between 1983 and 1987, and so forth.[53] These cohort variables control for any fixed unobservable differences between the five-year cohorts (recall that we are already controlling explicitly for differences in the level of education, measured at the individual level, which is a key observable that differs across cohorts). This is much less restrictive than imposing a more rigid parametric functional form across the large number of birth year cohorts in our sample, such as a linear or quadratic cohort trend. The result of including these five-year cohort dummy variables is that, although we use a long time series of data, identification effectively comes from comparing sets of cohorts that graduated relatively close together. For example, differences between the economic outcomes of those who entered the labour market in 1979 and those who entered in 1981 will drive our results, as will differences between those who entered in 2008 and 2010, but differences between those entering the labour market in 1979 and 2010 will not. The logic underlying this identification strategy is that the economic cycle can and does change quickly, whereas other underlying cohort differences do not. Therefore, cohorts close together but who potentially experience different economic starting conditions can be used to identify scarring effects. Our results are all robust to using different five-year cohort groupings.

We also control for the year that each individual in the data is observed (μ_t). This has an important impact on the interpretation of the coefficients of interest, β_e: we estimate the persistent impacts of having entered the labour market when unemployment is high, after stripping out the effect of the economic conditions at the point that people are observed (which might be correlated with the starting conditions). Note that, although we strip out the potentially confounding effects of *current* conditions,

[53] We choose to define our five-year cohorts as 1978–82, 1983–87 and so forth because 1978 is the first year of data, and so it is the first cohort for which we observe individuals in each of the first 10 years after leaving education. Fortunately, this also means that periods of high unemployment / low unemployment do not systematically appear at the beginning/end of our five-year cohorts, which could be concerning if there were some gradual cohort trend that was not picked up by the five-year dummies that we use. Our results reported in Section 6.4 are robust to changing the boundaries between five-year cohorts.

we do not separately estimate scarring effects from the entire history of a cohort's years in the labour market: we control only for the initial conditions and current ones, and nothing in between. Hence the interpretation of β_e is as explained in Oreopoulos et al. (2012): the estimated scarring effect of having had worse initial conditions, inclusive of any persistent impact from the regular evolution of the economy faced after those bad initial conditions.

In X_{ict}, we control for a small number of other control variables that are plausibly exogenous to the economic conditions at the point people enter the labour market. These are sex and a dummy variable indicating whether the mandatory minimum school-leaving age was 16 rather than 15 (a change that affected those born in the academic year 1957–58 and afterwards) – a change which previous research has shown significantly increased the education and earnings of those affected (see Harmon and Walker 1995). Finally, we include dummy variables to control for any systematic differences between the FRS and FES (differences which in fact are very small) in our pooled data set, allowing such differences to vary over time by interacting them with year.

We first estimate equation 6.1 using the individual labour market outcomes focused on in the literature to date: employment probabilities and the (log) weekly earnings of those in paid employment or self-employment. These can be thought of the 'direct' labour market impacts of entering the labour market in a recession. We then seek to extend our understanding of the impacts of scarring on living standards by incorporating the various potential insurance mechanisms, moving through a sequence of outcome variables as described below. For employment, and other outcomes which are binary variables, we estimate equation 6.1 using a probit model, and report the average marginal effect. For all other variables, we estimate equation 6.1 by Ordinary Least Squares.

We look at the (log) pre-tax earnings of the family (i.e. the individual and their cohabiting partner), so we can assess whether partners mitigate any of the impacts. It is, of course, only possible to estimate this for those living in a family with at least one adult in work. For clarity, we therefore keep that same sample – families with an adult in work – as we move through the next set of outcomes, so as

not to conflate the effects of insurance with the effects of changing the sample, before adding in the workless families in a final step at the end.

Keeping the same sample of working families, we then estimate the impact on log family private income (examining the insurance provided by any unearned private income – importantly excluding state transfers), log family net income (examining the insurance provided by the tax and transfer system) and log household income measured both private and net (examining insurance provided by other members of the household). We then add the workless families back into the sample when showing the impact on log net household income, before finally considering log total expenditure. Comparing effects on household income and expenditure should allow us to understand the ability of borrowing or drawing down on assets to provide individuals with insurance. We equivalise both net household incomes and expenditures, although this does not materially affect any of the results. These latter outcomes are the standard ones used in the empirical economics literature on household living standards, allowing us to bridge the gap between this and the scarring literature. Given that the variation in "unemployment at age left education" varies by year left education – as is standard in the literature – we cluster our standard errors at the "year left education" level. This allows for potential correlation in the errors ε_{ict} between indidividuals leaving education in the same year.

Of course, to some extent, individuals may be able to choose the point at which they leave education and join the labour market, and the state of the labour market may influence that decision. This raises two potential issues for our empirical strategy. First, it could affect the educational composition of labour market entrants at different stages of the economic cycle. However, we control for education level, so our results should be interpreted as net of any indirect impacts of the economic cycle on earnings via education. Second, the composition of labour market entrants may be correlated with the state of the economy with respect to unobserved factors. For example, if the delay of labour market entry in reaction to a recession were concentrated among those with lower ability, those entering the labour market at that time would have relatively high ability and this would attenuate the estimated scarring effects.

We have two main pieces of evidence for why this second issue is not a problem in practice. First, selection of this kind would presumably lead to permanent unobserved differences between cohorts (since cohorts are fixed from the point of leaving education). However, as reported in Section 6.4, our estimated scarring effects on labour market outcomes fade to zero once we follow cohorts for enough years. Hence this looks inconsistent with material selection on unobservables. Second, as found in Altonji et al. (2016), the magnitude of the effect of the cycle on education decisions is simply too small to cause substantial biases in our results.[54] Exploiting time-series variation in the unemployment rate at different ages, we estimate the effect of the unemployment rate at ages 16 and 18 on the probability of remaining in education past those ages (see Appendix E.1 for details). We estimate that a 4 percentage point (ppt) increase in unemployment increases the probability of staying on in education for 16-year-olds by 1.1ppts, although this effect is not statistically different from zero. Effects of this magnitude (or even significantly bigger effects) could not plausibly lead to cohorts graduating in recessions having such different *unobservable* characteristics as to significantly bias our results.

6.4 Results

6.4.1 Effects of entering the labour market when unemployment is high

Table 6.2 reports the estimated effect that a 4 percentage point increase in the unemployment rate has on all of our outcomes of interest in each of the 10 years after leaving education, based on the specification in equation 6.1. In a similar way to Altonji et al (2016), we report the effect of a 4ppt

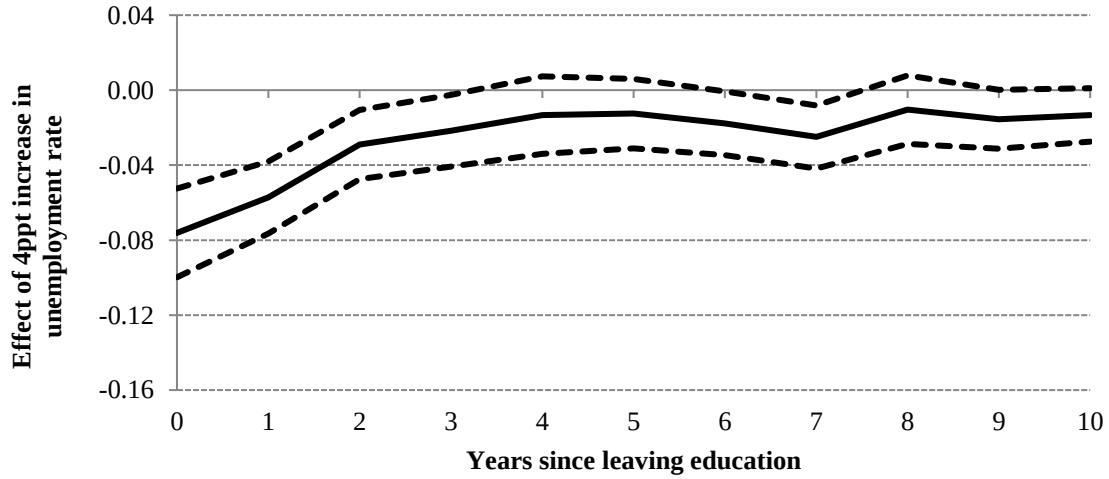
[54] Other authors have used alternative strategies to address this potential issue. Bell et al. (2018) examine the effect of unemployment at a fixed age (16) on the likelihood of committing crimes. But with over 35% of those born in the early 1980s completing a degree, using a fixed age would mean accepting a very large amount of measurement error in economic conditions at labour market entry, attenuating the estimated effects. Kahn (2010) uses year of birth as an instrument for the year individuals left education, and age as an instrument for experience. However, in our sample of people in the first 10 years after leaving education, those who are older are also more educated, so the exclusion restriction would not hold and therefore this is not a valid instrument in this context.

rise in unemployment because, as noted in Section 6.2, this is the average increase in unemployment that has occurred during the last three recessions in the UK. [55]

The first two columns of Table 6.2 show the effect of higher unemployment upon entering the labour market on the probability of being in paid work and on the (log) earnings of workers (including self-employed earnings). These coefficients are also plotted in Figure 6.4, along with the 95% confidence intervals. A 4ppt increase in the initial unemployment rate leads to a decreased probability of being in paid work of 7.6ppts in the year of leaving education. The magnitude of this effect falls rapidly, to 2.9ppts two years after leaving education. There are some small, but mostly statistically significant, negative impacts of between 1 and 2 percentage points on the probability of being in paid work between 3 and 7 years after leaving education. There are also large negative impacts on the earnings of workers, which persist for a few years. A 4ppt increase in the unemployment rate at labour market entry reduces the weekly earnings of workers by 10.3% on average. This large impact dissipates slowly, with a 5.7% negative impact after two years and a 2.8% impact after four years. After this, the effects fade towards zero and are no longer statistically significant at conventional levels.

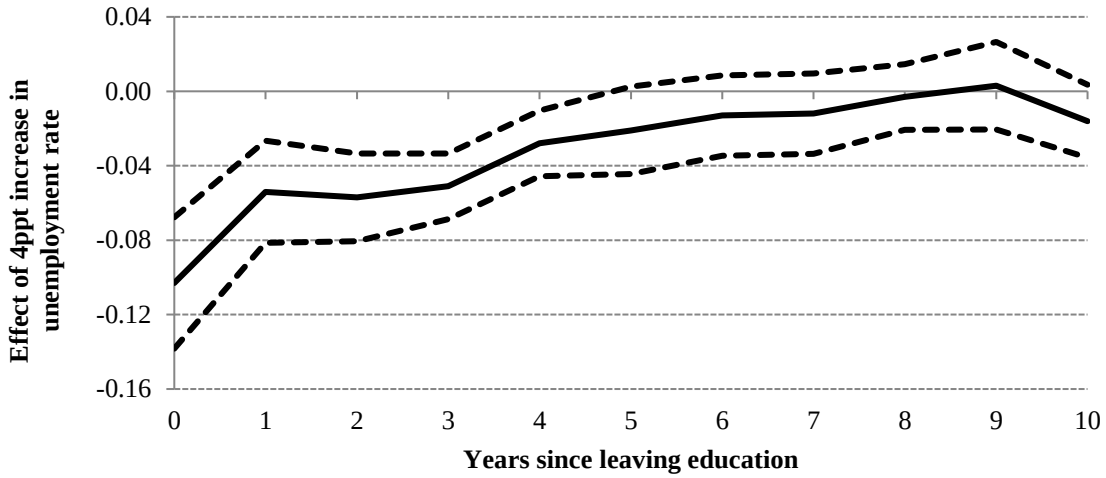
Figure 6.4: Effect of a 4 percentage point increase in unemployment rate upon leaving education on labour market outcomes

Panel A: Probability of being in paid work



[55] In Appendix Table E.3, we show that these results are robust to a slightly different measure of the state of the economy upon leaving education; it looks at the impact of the output gap (positive meaning output is above trend) on the same outcomes.

Panel B: Log earnings from employment or self-employment



Note: Effects are obtained by estimating equation 6.1 using a probit model with a dummy variable for employment as the dependent variable (Panel A) and by OLS with log gross weekly earnings (Panel B) as the dependent variable. Dashed lines are the 95% confidence intervals. Standard errors clustered at the "year left education" level.
Source: Authors' calculations using Family Resources Survey and Family Expenditure Survey from 1978 to 2016.

Table 6.2: Effect of a 4 percentage point rise in unemployment rate upon leaving education on measures of employment, earnings, income and expenditure

Effect, by years since left education			For working families only					For all families		
	In paid work	Log individual earnings of workers	Log family earnings	Log family private income	Log family net income	Log household private income	Log equivalised net household income	Log equivalised net family income	Log equivalised net household income	Log equivalised total expenditure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
0	–0.076***	–0.103***	-0.095***	-0.098***	-0.063***	–0.019	0.011	–0.046	–0.000	–0.017
	(0.012)	(0.018)	(0.021)	(0.021)	(0.018)	(0.021)	(0.014)	(0.031)	(0.012)	(0.014)
1	–0.057***	–0.054***	–0.042***	–0.042***	–0.022	–0.001	0.009	–0.064***	0.002	–0.021**
	(0.010)	(0.014)	(0.015)	(0.015)	(0.014)	(0.014)	(0.010)	(0.017)	(0.014)	(0.010)
2	–0.029***	–0.057***	–0.069***	–0.070***	–0.049***	–0.021	–0.005	–0.055***	–0.017	–0.017*
	(0.009)	(0.012)	(0.015)	(0.015)	(0.012)	(0.015)	(0.011)	(0.016)	(0.013)	(0.010)
3	–0.022**	–0.051***	–0.065***	–0.066***	–0.045***	–0.023*	–0.015	–0.053***	–0.025*	–0.029**
	(0.010)	(0.009)	(0.012)	(0.012)	(0.010)	(0.013)	(0.011)	(0.013)	(0.013)	(0.011)
4	–0.013	–0.028***	–0.023*	–0.023*	–0.012	–0.003	0.003	–0.023	–0.012	–0.026**
	(0.011)	(0.009)	(0.012)	(0.013)	(0.012)	(0.010)	(0.008)	(0.014)	(0.010)	(0.011)
5	–0.013	–0.021*	–0.038**	–0.039***	–0.024**	–0.008	0.000	–0.024	–0.009	–0.004
	(0.009)	(0.012)	(0.014)	(0.014)	(0.011)	(0.013)	(0.010)	(0.016)	(0.012)	(0.009)
6	–0.018**	–0.013	–0.019*	–0.018	–0.012	–0.016	–0.007	–0.017	–0.026*	–0.025***
	(0.009)	(0.011)	(0.011)	(0.012)	(0.010)	(0.011)	(0.009)	(0.014)	(0.013)	(0.009)
7	–0.025***	–0.012	–0.013	–0.014	–0.007	–0.013	0.007	–0.026**	–0.015	–0.011
	(0.009)	(0.011)	(0.013)	(0.013)	(0.012)	(0.011)	(0.010)	(0.013)	(0.012)	(0.009)
8	–0.010	–0.003	–0.015	–0.013	–0.011	0.010	0.010	–0.012	–0.001	0.011
	(0.009)	(0.009)	(0.014)	(0.014)	(0.012)	(0.011)	(0.009)	(0.013)	(0.012)	(0.009)
9	–0.016*	0.003	–0.002	–0.001	–0.005	0.006	0.013	–0.013	–0.003	–0.006
	(0.008)	(0.012)	(0.014)	(0.014)	(0.012)	(0.011)	(0.009)	(0.012)	(0.011)	(0.011)
10	–0.013	–0.016	0.002	0.005	0.002	0.004	0.012	–0.006	–0.003	–0.015
	(0.007)	(0.010)	(0.013)	(0.013)	(0.011)	(0.012)	(0.009)	(0.013)	(0.012)	(0.010)
Observations	205,349	150,614	163,134	162,950	162,119	161,979	161,127	193,870	199,806	73,718

Note: Effects are obtained by estimating equation (6.1) by OLS with the specified dependent variables, with the exception of column (1) which is the results of a probit model. *** indicates that the effect is statistically different from zero at the 1% level, ** at the 5% level and * at the 10% level. Standard errors clustered at the "year left education" level.
Source: Authors' calculations using Family Resources Survey and Family Expenditure Survey from 1978 to 2016.

The effects on earnings are similar in magnitude to those estimated among US and Canadian graduates by Altonji et al. (2016) and Oreopoulos et al. (2012) respectively, though smaller than the effects estimated among US graduates by Kahn (2010). Our estimates for employment are quite similar to those from European studies that, like us, use the whole population – such as Burgess et al. (2003), Raaum and Røed (2006) and Schmillen and Umkehrer (2013) – and smaller than the effects typically found among North American graduates.

Columns 3 and 4 of Table 6.2 show the effects on log (pre-tax) family earnings and on log family private income (i.e. after adding in unearned income other than state transfers, which tends to be small). As explained in Section 6.3 (and shown in Table 6.2), these results – and the next few that follow – are for the common sample of individuals in a family where at least one member is in paid work, so that the comparison of results across outcome variables isolates the impacts of insurance rather than being partly a result of sample changes. The results show that the initial unemployment rate has negative effects on both family earnings and family private income that persist until around five years after leaving education – the same as for individual earnings of workers. After three years, both measures are around 7% lower as a result of a 4ppt higher initial unemployment rate. Given what we have seen, it is unsurprising that moving from the individual to the family level makes little difference: few people live with a partner with whom they can pool resources shortly after leaving education.

However, columns 5 to 7 of the table, show that for those living in a working family, a combination of the tax and transfer system and the incomes of other members of the household (most importantly parents) acts to mitigate essentially all of the negative effects of entering the labour market in a recession on net household income.

Taking first the insurance provided by the tax and transfer system, Column 5 shows that, three years after leaving education, family net income is 4.5% lower as a result of a 4ppt increase in the initial unemployment rate, whereas family private income is 6.8% lower. This difference is the result of lower direct tax payments and higher means-tested transfers. In addition, even where entitlements to

transfers are not increased by lower earnings, the mere existence of another source of income besides earnings acts to attenuate the proportional effects on income of lower earnings. However, the scarring effect on net family income is still clear and persistent: the estimate is both negative and statistically significant for around five years.

In contrast, Column 6 of Table 6.2 shows that the incomes of other members of the household (excluding the partner or spouse) seem to provide large amounts of insurance against the negative impacts on labour market outcomes. Negative effects on household private income are much smaller than the effects on family private income, both in the period immediately after leaving education (when effects on earnings are largest) and at the end of the period of 'scarring' on family income around five years later.

Once both the tax and transfer system and the incomes of other household members are added in, by looking at net household income, impacts of entering the labour market at a time of unemployment are no longer significant.

If we broaden the sample so that we include families in which no adults in work, as in column 9, some negative impacts on net household income are apparent, which is not surprising given that there are small but persistent negative impacts on the probability of being in paid work. Nevertheless, the effects are small relative to those on employment and earnings, and only in a small number of years are they statistically significant. Impacts on household expenditures are similar to those on net household incomes, as shown in column 10. While there are some significant reductions in household consumption expenditure up to 6 years after leaving education, they are very small compared to the falls in employment or earnings, and fade away after to zero after that.

A comparison of the results in column 9 (on net *household* income for all individuals) with those in column 8, which shows the effects on net *family* income for all individuals, again highlights the importance of the potential safety net provided by parents. There are sizeable – and statistically significant – negative effects of entering the labour market in a recession on net family income, particularly in the first five years after leaving education. Hence in cases where the degree of resource

pooling within households is weak, there would still be good reasons to be concerned about the living standards of young adults scarred in the labour market who live with their parents. If, on the other hand, one thinks that parents are likely to pool their income with that of their co-resident children, then net household income and expenditure are good proxies for material standards of living, and this would imply that the impacts on young adults' living standards are insured against to a very large degree. There is an important and sharp contrast between these highly muted effects on typical measures of living standards and the large persistent effects of bad initial conditions on labour market outcomes examined in previous work.

6.4.2 Understanding the insurance mechanisms and their interaction with heterogeneity

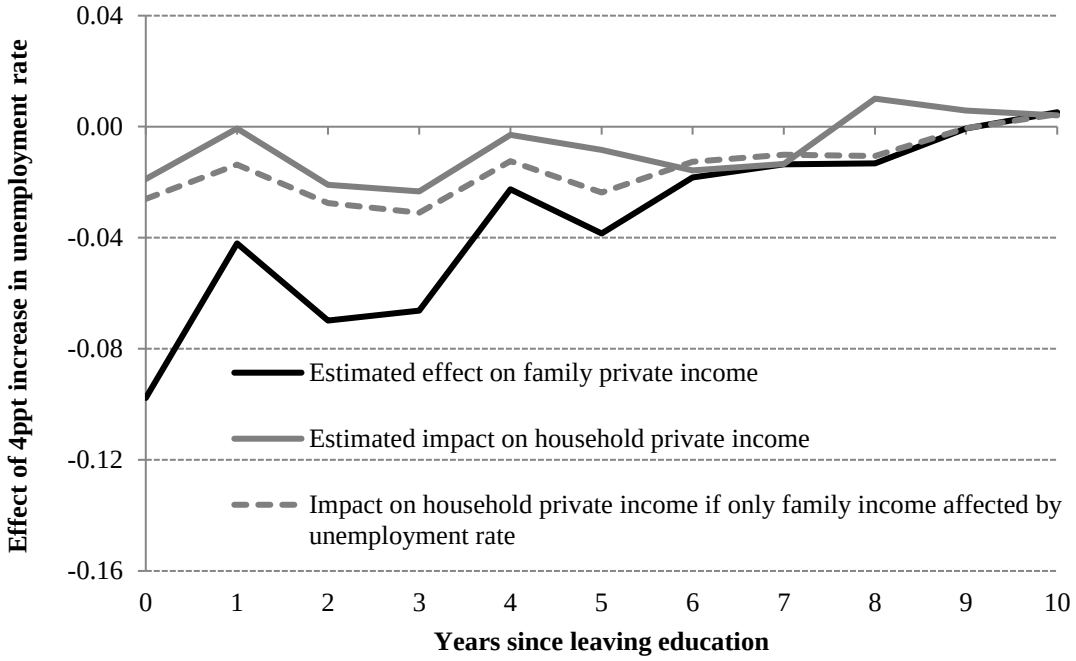
In this subsection, we explore how income from other household members plays such an important insurance role. Of those young adults in our data who do live in a household with people other than their partner (and their dependent children), the large majority (82%) live with their parents. This proportion is even higher (87%) for those who left education within the last three years. There are three potential ways in which this insurance from other household members might work.

First, there is a mechanical, non-behavioural 'dilution' effect: where a substantial amount of income in a young adult's household comes from other household members, proportional impacts on total household income of falls in their own income will be attenuated. Second, leaving education during a recession may encourage more young adults to live with their parents (by staying in the parental home longer, or even returning to their parents' home), thereby boosting the household incomes of some affected children. Third, even for those individuals who would have been living with their parents anyway, parents might increase their earnings to help make up for the lower amount earned by their child. Of course, these three mechanisms are not mutually exclusive.

Figure 6.5 shows that the first, mechanical "dilution" explanation is the most important. The figure presents the estimated impact on (log) family private income and (log) household private income as shown in Table 6.2. In addition, the dashed grey line shows what the mechanical impact on household private income would be if only family private income were affected by the initial unemployment

rate. This is calculated by multiplying the share of household private income coming from family private income at each experience year (see Table 6.1) by the estimated effect of the initial unemployment rate on family private income at each experience year shown in Table 6.2. The share of household income coming from outside the 'family' is high, making up 61% of household income for individuals two years after leaving education. As a result, the mechanical 'diluting' effect of the income of other household members explains almost all of the difference between the proportional effects on family incomes and household incomes.

Figure 6.5: Mechanical impact on household private income for working families if only family income is affected by unemployment rate at point of entering labour market



Note: Effects on (log) family and (log) household private income are obtained by estimating equation 6.1 by OLS with the specified dependent variables. See Table 6.2 for standard errors around the estimated effects. The dashed line is calculated by multiplying the share of household private income coming from family private income at each experience year (see Table 6.1) by the effect of the initial unemployment rate on family private income at each experience year shown in Table 6.2.
Source: Authors' calculations using Family Resources Survey and Family Expenditure Survey from 1978 to 2016.

Given that not everyone does (or has the ability to) live with their parents after leaving education, how is the insurance provided by the rest of the household so complete, even *on average*? This is where the interaction between heterogeneity in the labour market impacts and heterogeneity in insurance becomes important. Table 6.3 shows that the low-educated see the largest negative effects on family private income. Appendix Table E.2 confirms that the relatively large effects on family incomes are indeed driven by larger negative effects on employment and earnings effects being larger for low-educated individuals than for more educated people. This mirrors the findings of Burgess et al. (2003) for employment and Haaland (2017) for wages, and is not surprising less educated people have been found to be more negatively impacted by recessions in general (see Hoynes, Miller and Schaller 2012).

However, it is precisely these low-educated individuals who are the most likely to live with their parents in the years after leaving education (as shown in Figure 6.2). This means that the groups with the largest scarring effects on labour market outcomes (the low- and mid-educated) are the most likely to be insured by living with their parents.

This finding has three important implications. First, it helps to explain why the aggregate degree of insurance against scarring is so high even though a substantial share of the overall population of young adults do not live with their parents. Second, in terms of household private income, there are larger negative effects on the highly educated than there are on lower-educated individuals, despite the larger labour market impacts for the less educated, because many fewer high-educated individuals live with their parents. Third, living with parents would provide less insurance against scarring impacts on labour market outcomes if the scarring effects were more persistent. As shown in Figure 6.2, only 13% of people live with their parents 10 years after leaving education; so if labour market scarring were considerably more persistent, it would be more likely that leaving education in a recession would have long term negative impacts on household incomes and consumption expenditures. However, on average, the labour market scarring is not so persistent that it outlasts a typical period for which many people live with their parents.

Table 6.3: Effect of a 4 percentage point increase in unemployment rate upon entering labour market on family and household private incomes (in working families) by education group

Effect, by years since left education	Low education		Mid education		High education	
	Log family private income	Log household private income	Log family private income	Log household private income	Log family private income	Log household private income
0	–0.110***	–0.022	–0.091***	0.023	–0.063*	–0.043
	(0.028)	(0.041)	(0.029)	(0.028)	(0.035)	(0.037)
1	–0.014	0.016	–0.112***	0.014	0.009	–0.024
	(0.022)	(0.026)	(0.017)	(0.023)	(0.029)	(0.026)
2	–0.084***	–0.024	–0.079***	0.009	–0.030	–0.039*
	(0.020)	(0.024)	(0.023)	(0.023)	(0.027)	(0.022)
3	–0.063***	–0.009	–0.086***	–0.018	–0.036	–0.039*
	(0.015)	(0.024)	(0.019)	(0.020)	(0.027)	(0.022)
4	–0.039**	–0.003	–0.041	0.015	0.030	–0.011
	(0.016)	(0.020)	(0.025)	(0.016)	(0.026)	(0.017)
5	–0.036*	–0.010	–0.064**	0.009	–0.002	–0.014
	(0.018)	(0.019)	(0.027)	(0.019)	(0.023)	(0.022)
6	–0.017	–0.001	–0.059**	–0.031	0.036*	–0.007
	(0.016)	(0.016)	(0.022)	(0.022)	(0.021)	(0.017)
7	–0.005	0.014	–0.014	–0.022	0.012	–0.014
	(0.018)	(0.020)	(0.026)	(0.017)	(0.024)	(0.016)
8	–0.007	0.031*	–0.011	0.025	0.019	0.008
	(0.018)	(0.016)	(0.024)	(0.020)	(0.022)	(0.016)
9	0.012	0.028	0.009	0.017	0.023	0.010
	(0.019)	(0.019)	(0.021)	(0.016)	(0.021)	(0.015)
10	0.046**	0.036*	–0.011	0.024	0.011	–0.012
	(0.018)	(0.021)	(0.023)	(0.021)	(0.019)	(0.015)
Observations	54,433	54,213	42,940	42,648	65,577	65,118

Note: Effects are obtained by estimating equation 6.1 by OLS with the specified dependent variables on the subgroup specified. *** indicates that the effect is statistically different from zero at the 1% level, ** at the 5% level and * at the 10% level. Standard errors are clustered at the "year left education" level.
Source: Authors' calculations using Family Resources Survey and Family Expenditure Survey from 1978 to 2016.

Table 6.4 shows that there is no estimated effect on the probability that young adults live with their parents resulting from leaving education when unemployment is high, with the possible exception of 3 years of after education.[56] This implies that the reason for other household members insuring affected young adults is not due to an increased propensity to live with their parents. There is also no evidence

[56] Note that the 'scarring' impact of high unemployment at labour market entry is estimating an effect that is distinct from the work by Kaplan (2012), who looks at individuals returning home to live with their parents when they face a contemporaneous economic shock.

that there is any labour supply response of parents in reaction to their adult children starting their career when unemployment is high. We find no significant impacts on the earnings of parents who are in work (column 4) with the single exception of a negative effect 10 years after leaving education.

In addition, column 1 of Table 6.4 shows why spousal labour supply is not an important insurance mechanism for young adults who leave education at a bad time (contrary to the results for the population as a whole found by Blundell, Pistaferri and Saporta-Eksten (2016)). Not only do relatively few people live with a partner after leaving education, but facing a higher unemployment rate upon leaving education leads to a persistently lower probability of living with a partner – effects which are significant between two and nine years after leaving education.

Table 6.4: Effect of a 4 percentage point increase in unemployment rate upon entering labour market on living with partner, living with parents, and parents' employment and earnings

Effect, by years since left education	Pr(Live with a partner)	Pr(Live with parents)	Number of working parents	Log parents' weekly earnings
	(1)	(2)	(3)	(4)
0	0.018	–0.008	0.005	0.002
	(0.013)	(0.011)	(0.006)	(0.006)
1	0.000	0.002	0.008	0.004
	(0.010)	(0.008)	(0.006)	(0.005)
2	–0.024***	0.008	0.007	0.002
	(0.009)	(0.007)	(0.005)	(0.005)
3	–0.041***	0.017***	0.005	–0.003
	(0.008)	(0.007)	(0.004)	(0.004)
4	–0.012	0.012*	0.002	–0.001
	(0.008)	(0.007)	(0.005)	(0.005)
5	–0.023***	0.007	0.006	–0.001
	(0.007)	(0.007)	(0.006)	(0.005)
6	–0.008	–0.003	–0.002	–0.001
	(0.007)	(0.007)	(0.006)	(0.006)
7	–0.015**	–0.007	–0.002	0.006
	(0.007)	(0.007)	(0.007)	(0.005)
8	–0.017**	0.008	0.004	–0.005
	(0.007)	(0.007)	(0.007)	(0.007)
9	–0.012*	0.001	0.003	–0.004
	(0.007)	(0.007)	(0.010)	(0.007)
10	–0.001	–0.009	–0.008	–0.017*
	(0.007)	(0.007)	(0.009)	(0.009)
Sample restrictions:	All	All	All living with parents	All living with a working parent
Observations	205,349	205,349	81,842	65,229

Note: Effects are obtained by estimating equation 6.1 by OLS with the specified dependent variables on the samples shown. *** indicates that the effect is statistically different from zero at the 1% level, ** at the 5% level and * at the 10% level.
Source: Authors' calculations using Family Resources Survey and Family Expenditure Survey from 1978 to 2016.

Of course, there are individuals who are scarred in the labour market and who cannot or do not live with their parents. Our results suggest that, in fact, that may be the group of most concern. We have shown how important the incomes of co-resident parents are for insuring the household incomes and consumption expenditures of young adults. In comparison, spousal (or partner's) labour supply plays very little role for people in the years immediately after leaving education, and the tax and state transfer system only provides a limited degree of insurance against lower earnings and reduced employment probabilities.

This suggests that those leaving education when unemployment is high, but who do not live with their parents, may see significant reductions in their standard of living for a number of years after entering the labour market, particularly as they are very unlikely to have significant levels of assets or savings to self-insure against such shocks. Indeed, we can test this directly by examining the effect of entering the labour market when unemployment is high on "single family households" (those made up only of a single person or a couple plus any dependent children).[57] The results of this exercise are shown in Appendix Table E.4. We find that there are significant (albeit temporary) negative effects on the incomes and consumption of single-family households, and they are substantially larger than the results for all families. For example, three years after leaving education, we estimate that a 4 ppt increase in the unemployment rate upon entering the labour market, reduces the equivalised household income of single family households by 7.5% (compared to 5.3% for all families) and reduces the equivalised household expenditure by 9.3% (compared to 2.9% for all families).

6.5 Conclusion

In this chapter, we have estimated the causal impact of entering the labour market when the economy is weak, not only on the individual labour market outcomes focused on in previous research, but also on standard measures of material living standards – net household income and household expenditures. We have also studied a number of intermediate outcomes in order to isolate the key insurance mechanisms standing between labour market effects and impacts on living standards. For identification, we have exploited the economic cycle in the United Kingdom since the 1970s, which means that cohorts entering the labour market very close together can nevertheless face dramatically different initial economic conditions.

[57] Table 6.4 shows that this probability is itself not affected by the unemployment rate upon entering the labour market

We concur with previous research in finding substantial impacts on the individual earnings and employment rates of young adults who leave education when unemployment is high. However, we find little or no impact on their net household incomes and household expenditures. There are two key reasons for this. First, the tax and state transfer system helps to partially cushion the impact of lower earned income. Second, and more importantly on average, many young adults live with their parents for some years after they enter the labour force (75% one year after leaving education and 39% five years after). Parental incomes tend to be far higher than those of their adult children, meaning that the proportional shock to household incomes caused by lower earnings of the young adult is typically very small where they live together. To understand why the degree of insurance provided by parental incomes is quite so large, the relationship between heterogeneity in labour market scarring and in insurance is key: labour market scarring tends to be bigger for the lowest-educated, but that is also the group that is most likely to live with their parents in the years after leaving education, and so are cushioned from this shock by the incomes of their parents

There are several reasons why policymakers should still be concerned about the impacts on young adults of leaving education during a recession. First, reduced earnings and employment are important outcomes in their own right. They lead to lower tax revenues and higher government spending on means-tested transfer payments. National income will be lower if young adults are persistently out of work or less well matched with employers. The wider well-being of young adults may also be harmed simply by being out of work, or by being more dependent on their parents, irrespective of their household incomes or expenditures. Second, those who do not live with their parents do not benefit from the insurance provided by parental incomes. Indeed, we find that those young adults who do live on their own (or only with their partner) in the years immediately after entering the labour do see significantly lower income and expenditure as the result of a higher unemployment rate upon leaving education. Third, to be definitive about the impacts on living standards, we would need to know the degree of intra-household resource sharing where young adults live with their parents. The living standards of the young adults in these households may not always be the same as those of their parents.

Our results therefore lead us to suggest two fruitful areas of focus for further research in this area: the negative effect of entering the labour market during a recession on those young adults who do not live with their parents, and the degree to which resources are shared within households containing parents and their co-resident children.

7. Conclusions

Chapters 2 to 6 have each contained their own separate conclusions. In this final chapter, I briefly set some areas for future research that draw upon the papers in this thesis. Chapters 2 and 3 examined how labour supply increases in response to a higher state pension age, and subsequently used that reform as an instrument to examine the effect of a longer working life on two aspects of health. With ageing populations placing increasing strain on public finances, it will be important to both understand the effects of economic behavior, living standards and inequalities of reforms that seek to encourage a longer working life.

However, for around 45% of the female population that was already out of the labour force by age 59, a higher pension age does not increase labour supply (Amin-Smith and Crawford 2018), it simply leads to a period in which they must fund their own retirement. A pressing question is therefore: how do people late in working-age life who have already left the labour force respond to reforms which delay their ability to claim state pensions? Amin-Smith and Crawford (2018) previously found that it is not by returning to the labour force. A future project interested in this could exploit panel data from the UK, such as the English Longitudinal Study of Ageing and the UK Household Longitudinal Study, to examine how measures of individual welfare change in response to the higher pension age for those already retired before age 60. In particular it would be interesting to look at household spending patterns to see if affected families cut back on their spending in response, and also to measures of stress and mental health, as they may be exposed for much longer to work-search requirements of working-age benefit systems.

Understanding the potential for these effects is particularly important as pension ages across the world continue to rise. Although there is considerable evidence that current reforms have been effective at increasing labour supply, there is no reason to believe that the effect of raising from 60 to 62 or 63, as studied in this thesis, will be the same as from 65 to 66 (as is currently occurring for both men and women in the UK). More generally, though, the full consequences of increased labour force participation at older ages on wellbeing, health and time use at older ages are not fully understood.

Future work should consider the impact of longer working lives on – amongst other outcomes – the amount of unpaid care undertaken for both the very old and for children by their grandparents; the interaction with the healthcare system and use of healthcare; and how firms might change in response to a larger pool of older workers amongst their employees.

There are also a number of important open questions that are raised by the results in chapters 4 and 5. Automatic enrolment as a tool to boost workplace pension saving has been shown to be immensely effective. But questions remain about whether other actions by individuals will offset the extent to which this boosts retirement resources. The higher proportion of people that are "passive savers" (which is shown in Chetty et al (2014) to be high in Denmark), the more likely that other savings will not be reduced in response to higher workplace pension saving. But if individuals are not saving less in other forms, they must be consuming less. For many that may not cause hardship, but for some this may cause financial difficulties.

Therefore, one future strand of research could be to examine whether automatic enrolment leads to higher levels of debt, or falling into financial difficulties, such as falling behind on rent or mortgage payments, or other bills. If policymakers are automatically enrolling individuals, but allowing opt-out and not making such contributions compulsory, then are those who are most at risk of falling into financial difficulties from saving more for retirement actually those who take up the opportunity to opt out?

Finally, there are a number of important questions that are raised by how important different forms of insurance are – including the tax and benefit system – in responding to different kinds of economic shocks. While Chapter 6 studied a particular shock – leaving education during a recession – other groups may also be disproportionately affected by the prevailing macroeconomic conditions. For example, Coile and Levine (2011) argue that elevated rates of unemployment in the United States lead to early retirement and the early claiming of Social Security benefits. Moreover, given the generosity of benefit (transfer) systems have been pared back in many countries since the 2008 financial crisis, (see Cribb, Hood and Joyce 2017 for changes in the UK), forms of self-insurance may become more

important. Therefore future work understanding spousal labour supply, or parental income, or accumulated savings, as forms of (at least partial) insurance may be increasingly important in understanding in how individuals respond to future economic shocks.

Appendices

A: Appendix to Chapter 2

Table A.1: Effect of the State Pension Age on female employment: OLS regression results

	Effect on female employment	*Standard error*
Under State Pension Age	0.063***	[0.015]
Cohabiting	0.055***	[0.020]
Single	-0.020	[0.025]
Widowed	-0.021	[0.023]
Divorced/Separated	0.058***	[0.020]
Other HE	-0.063***	[0.015]
A level or equivalent	-0.021	[0.015]
O level or equivalent	-0.050***	[0.014]
Other	-0.078***	[0.015]
No qualifications	-0.215***	[0.014]
Not white	-0.062***	[0.020]
Rents house	-0.161***	[0.011]
Partner's age (years and quarters)	-0.026**	[0.011]
Partner's age squared	0.000**	[0.000]
Partner's age: 60–64	-0.032**	[0.015]
Partner's age: 65–69	-0.090***	[0.023]
Partner's age: 70+	-0.079*	[0.043]
Partner's education: other HE	0.036*	[0.019]
Partner's education: A level	0.064***	[0.015]
Partner's education: O level	0.064***	[0.018]
Partner's education: other	0.096***	[0.018]
Partner's education: no qualifications	0.061***	[0.017]

Notes: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level, * at the 10% level. Estimated by OLS with standard errors clustered at the individual level. This regression model uses women born in 1949–50 to 1953–54 from 2009Q2 to 2014Q2. Nineteen geographical area dummy variables, twenty year and quarter dummy variables, twenty-nine dummies for age in years and quarters, three dummies for financial year of birth, and a constant also included in the model. Effects estimated relative to baseline of cohort 1949–50, age 60Q1, married, white, owns house, with a degree, and with a partner with a degree. Number of observations: 60,173.

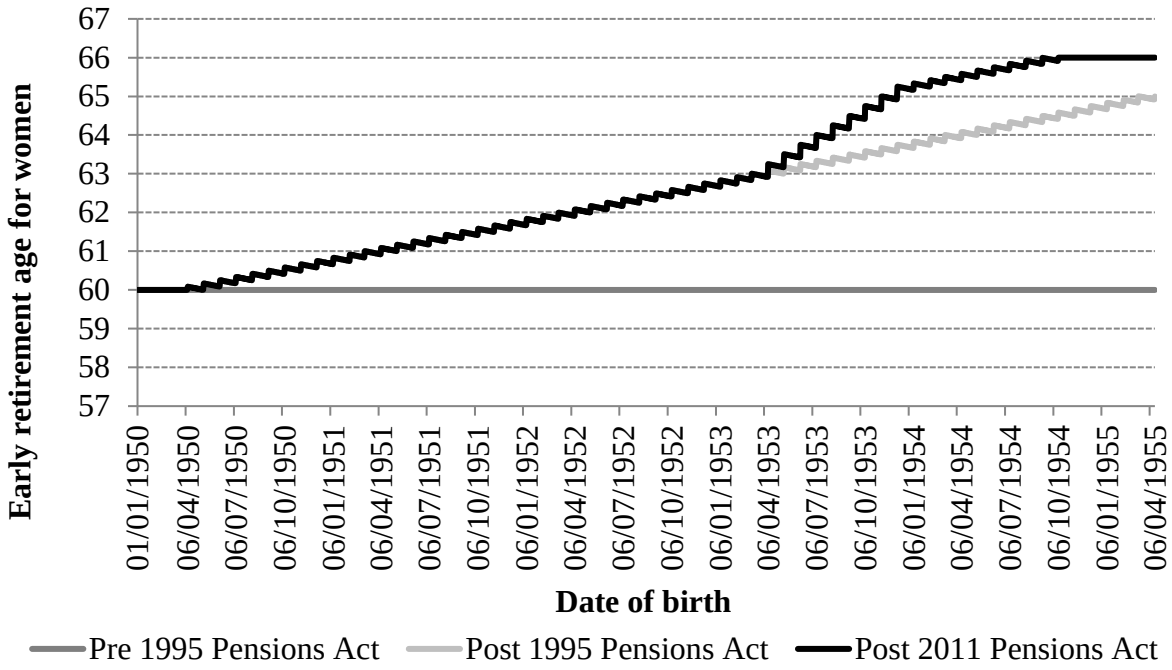
Table A.2: Number of women observed above and below the State Pension Age (aged 59 to 62)

Birth cohort	*Age in years and quarters*															
	59 Q1	59 Q2	59 Q3	59 Q4	60 Q1	60 Q2	60 Q3	60 Q4	61 Q1	61 Q2	61 Q3	61 Q4	62 Q1	62 Q2	62 Q3	62 Q4
1949Q2				73	165	159	158	155	168	166	164	137	159	163	163	146
1949Q3			73	154	149	140	135	155	137	129	126	148	147	157	153	154
1949Q4		76	153	157	172	157	162	150	144	141	134	161	133	144	137	139
1950Q1	92	171	186	174	159	169	154	151	129	138	129	147	133	147	140	146
1950Q2	181	179	175	178	171	169	158	155	146	163	151	136	147	148	126	149
1950Q3	170	159	148	142	121	128	119	138	147	163	146	150	155	145	149	153
1950Q4	149	137	134	120	120	115	131	140	157	135	133	129	134	134	141	134
1951Q1	139	138	121	123	124	126	150	152	153	156	156	139	155	141	135	139
1951Q2	177	157	155	132	133	138	149	148	143	142	140	134	142	138	132	144
1951Q3	122	125	110	112	128	144	141	132	148	153	154	136	131	140	140	59
1951Q4	117	125	130	134	137	128	126	109	119	131	139	126	129	120	56	
1952Q1	129	122	142	150	145	161	145	157	136	126	123	135	138	70		
1952Q2	137	143	171	142	156	175	155	174	159	165	144	142	83			
1952Q3	137	142	126	114	123	123	117	112	118	120	120	67				
1952Q4	132	134	137	133	119	124	130	121	136	146	64					
1953Q1	132	153	132	141	154	134	118	133	131	56						
1953Q2	150	149	147	161	167	163	159	166	83							
1953Q3	121	118	124	125	134	139	152	79								
1953Q4	124	131	135	107	139	135	72									
1954Q1	132	126	123	143	151	68										

Notes: Dark shaded cells indicate women who are all over their ERA. Light shaded cells indicate combinations of age and cohort where some women are above and some women are below the ERA. Number of women refers to number of observations in the LFS without data problems, and which are therefore used in estimation of impact of being under the ERA.

B: Appendix to Chapter 3

Figure B.1: UK State Pension Age for women under different legislation



Note: The reason the State Pension Age increases in a 'sawtooth' pattern, rather than a smooth line or a 'step' pattern, is that women born on any day in a given month are allocated the same 'State Pension Date' at which they are eligible for a state pension. Therefore women born later in the month have a slightly lower State Pension Age (measured in days) than those born earlier in the month.

Source: Pensions Act 1995, schedule 4 (http://www.legislation.gov.uk/ukpga/1995/26/schedule/4/enacted); Pensions Act 2007, schedule 3 (http://www.legislation.gov.uk/ukpga/2007/22/schedule/3); Pensions Act 2011, schedule 1 (http://www.legislation.gov.uk/ukpga/2011/19/schedule/1/enacted).

Table B.1: OLS estimates of the relationship being in paid work and measures of cognition and physical disability

Outcome	Effect of being in paid work	Number of observations
Verbal fluency test score	1.09***	8,490
	[0.22]	
Memory test score – immediate	0.19***	10,296
	[0.05]	
Memory test score – delayed	0.23***	10,309
	[0.06]	
Cognitive index score	0.85***	8,480
	[0.17]	
Walking speed (m/s)	0.05***	3,992
	[0.01]	
Any moderate mobility problems	–0.16***	10,628
	[0.02]	

Notes: Reported effects are the coefficients on a dummy for being in paid work in the regression where the dependent variable is the outcome listed, and the control variables as listed in section 3.2, including linear control for financial year of birth. Standard errors, reported in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level.

Source: Authors' calculations using the English Longitudinal Study of Ageing

Table B.2: Average (mean) levels of cognition and physical disability, by decile of net equivalised family income, for 60-63 year old women

Decile of equivalised net income	Verbal fluency	Memory test-immediate	Memory test - delayed	Cognitive index	Walking speed	Any moderate mobility problems	Family income (£ per week)
1 (lowest)	20.8	6.2	5.0	16.1	0.87	0.559	97
2	22.0	6.3	5.2	17.0	0.88	0.525	173
3	22.0	6.3	5.2	17.1	0.92	0.497	227
4	22.6	6.5	5.4	17.9	0.93	0.456	273
5	23.0	6.5	5.4	18.1	0.98	0.416	325
6	22.6	6.7	5.6	18.0	0.98	0.456	377
7	23.6	6.6	5.6	18.7	1.00	0.424	444
8	24.1	7.0	5.8	19.1	1.00	0.394	523
9	24.6	7.1	6.0	19.8	1.03	0.329	641
10 (highest)	25.2	6.9	6.1	19.9	1.02	0.295	1,151
Differences:							
Decile 10 – Decile 1	4.4	0.8	1.1	3.8	0.2	–0.26	1,055
Quintile 5 – Quintile 1	3.5	0.8	1.0	3.3	0.2	–0.23	761

Source: Authors' calculations using the English Longitudinal Study of Ageing.

Table B.3: Average (mean) levels of cognition and physical disability, by decile of family non-pension wealth, for 60-63 year old women

Decile of non-pension wealth	Verbal fluency	Memory test-immediate	Memory test - delayed	Cognitive index	Walking speed	Any moderate mobility problems	Family non-pension wealth (£)
1 (lowest)	20.3	5.9	4.5	15.2	0.74	0.71	–1,000
2	20.6	6.2	5.0	16.2	0.85	0.54	49,000
3	21.6	6.3	5.1	16.8	0.93	0.50	133,000
4	22.8	6.5	5.3	17.7	0.97	0.47	196,000
5	23.2	6.6	5.5	18.3	0.96	0.45	253,000
6	24.1	6.8	5.8	18.9	0.99	0.41	317,000
7	22.7	6.8	5.8	18.6	1.02	0.35	391,000
8	24.7	6.9	6.0	19.8	1.01	0.34	498,000
9	24.8	7.1	6.2	20.1	1.05	0.34	701,000
10 (highest)	25.6	7.0	6.1	20.4	1.08	0.24	1,710,000
Differences:							
Decile 10 – Decile 1	5.3	1.1	1.6	5.2	0.3	–0.5	1,711,000
Quintile 5 – Quintile 1	4.5	0.8	1.1	3.9	0.2	–0.3	1,619,000

Source: Authors' calculations using the English Longitudinal Study of Ageing.

Table B.4: Results of placebo tests: testing for whether there are effects of being under State Pension Age had reforms been implemented 4, 6 or 8 years earlier

	"Effect" of being under State Pension Age on outcome			
Timing of reform:	Actual timing	4 years early	6 years early	8 years early
In paid work	0.095***	–0.009	0.002	–0.003
	[0.024]	[0.039]	[0.030]	[0.026]
Verbal fluency	0.69*	0.14	–0.32	–0.21
	[0.38]	[0.53]	[0.38]	[0.34]
Memory: immediate	0.08	0.14	–0.03	–0.04
	[0.07]	[0.13]	[0.10]	[0.09]
Memory: delayed	0.19**	–0.04	–0.09	0.00
	[0.09]	[0.15]	[0.12]	[0.10]
Cognitive index	0.65**	0.17	–0.19	–0.18
	[0.27]	[0.38]	[0.29]	[0.27]
Walking speed	0.023	0.029	–0.030	–0.011
	[0.017]	[0.038]	[0.029]	[0.025]
Any moderate mobility problems	–0.055**	0.006	–0.002	0.030
	[0.023]	[0.039]	[0.031]	[0.027]

Note: Effects for "actual timing" are results of estimating equation 3.1 but with the outcomes listed as the dependent variables instead of a dummy for being in paid work. The effects for the "placebo" reforms e.g. "4 years earlier" use data from before the increase, estimating the same model but coding the variable "under State Pension Age" as if reform had been implemented 4, 6 or 8 years previously. Controls included as set out in section 3.3 and include a linear control for financial year of birth. Estimated by OLS. Standard errors, reported in brackets, are clustered at the individual level. *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. ELSA data only.
Source: Authors' calculations using the English Longitudinal Study of Ageing.

Table B.5: Construction of variable summarising amount of exercise undertaken

Panel A: Show Card provided to ELSA respondent when asked about physical activities, split by vigorous, moderate and mildly energetic

Vigorous	**Moderately energetic**	**Mildly energetic**
For example:	*For example:*	*For example:*
Running or jogging	Gardening	Vacuuming
Swimming	Cleaning the car	Laundry
Cycling	Walking at a moderate pace	Home repairs
Aerobics or gym workout	Dancing	
Tennis	Floor or stretching exercises	
Digging with a spade or shovel		

Source: ELSA Documentation Main Showcard Wave 8 (https://www.elsa-project.ac.uk/uploads/elsa/docs_w8/W8_MS_Showcards_Interviewer.pdf)

Panel B: Coding of information on intensity and frequency of exercise into summary variable on amount of exercise undertaken

For each intensity, individuals state the frequency they undertake exercise: 1) Hardly ever/never ; 2) 1-3 times per month; 3) Once a week; 4) More than once per week	
These data are combined to create a mutually exclusive and exhaustive categorisation of exercise:	
None	Vigorous: Hardly ever/never **AND** Moderate: Hardly ever/never **AND** Mild: Hardly ever/never
Low	Vigorous: Hardly ever/never **AND** (Moderate: Hardly ever/never **OR** 1-3 times per month **OR** Once a week) **AND** Exercise not already coded as "None"
Medium	(Vigorous: Once per week **AND** (Moderate: Hardly ever/never **OR** 1-3 times per month)) **OR** Vigorous: 1-3 times per month **OR** (Vigorous: Hardly ever/never **AND** Moderate: more than once per week)
High	Vigorous: More than once per week **OR** (Vigorous: Once per week **AND** (Moderate: Once a week **OR** More than once a week))
None and Low categories are subsequently combined into one category	

Table B.6: Definition of physicality of occupation

Panel A: ELSA question WPJACT on physicality of job

Q: Which of these best describes the work that you do in your main job? [Code One Only]

1. Sedentary occupation: You spend most of your time sitting (such as in an office)
2. Standing occupation: You spend most of your time standing or walking, however the way you spend your time does not require intense physical effort (e.g. Shop assistant, hairdresser, security guard etc.).
3. Physical work: This involves some physical effort including handling of heavy objects and use of tools (e.g. plumber, cleaner, nurse, sports instructor, electrician, carpenter etc.).
4. Heavy manual work: This involves very vigorous physical activity including handling of very heavy objects e.g. docker, miner, bricklayer, construction worker).

Source: ELSA Main Questionnaire Wave 8 https://www.elsa-project.ac.uk/uploads/elsa/docs_w8/W8_Interviewer_Questionnaire_FINAL_v02.pdf

Panel B: Proportion of women in each occupational class that are in jobs that are sedentary, standing or physical/heavy manual. Women born April 1948 to March 1957

	Percentage that are in:		
	Sedentary jobs	Standing jobs	Physical /heavy manual jobs
All	44.8%	36.2%	19.0%
Sedentary occupations			
SOC Group 4: Administrative	85.4%	11.9%	2.7%
Partly sedentary occupations			
SOC Group 1: Managerial	55.3%	30.7%	14.0%
SOC Group 3: Associate Professional	47.4%	33.6%	19.0%
SOC Group 2: Professional	46.1%	50.7%	3.2%
Average	*48.7%*	*39.2%*	*12.1%*
Non-sedentary occupations			
SOC Group 8: Process, plant and machinery	35.2%	34.2%	30.6%
SOC Group 7: Sales and Customer Service	23.8%	56.6%	19.6%
SOC Group 6: Caring & Leisure services	12.8%	54.0%	33.2%
SOC Group 5: Skilled trades	10.0%	46.9%	43.1%
SOC Group 9: Elementary occupations	3.4%	41.5%	55.1%
Average	*14.0%*	*49.4%*	*36.6%*

Notes: Number of observations: 8,370. Sample is women in ELSA waves 3-8 (2006/7 to 2016/17) born between April 1948 and March 1957, who were in work at time of interview. "Sedentary occupations" are those where 60% or more of the workers are in a sedentary job. "Partly sedentary occupations" are where 40% to 60% are in sedentary jobs, and "Non-sedentary occupations" are where less than 40% are in sedentary jobs.

C: Appendix to Chapter 4

Table C.1: Staging dates for introduction of automatic enrolment for employers

PAYE scheme size in April 2012	*Staging date*
120,000 or more	1 October 2012
50,000–119,999	1 November 2012
30,000–49,999	1 January 2013
20,000–29,999	1 February 2013
10,000–19,999	1 March 2013
6,000–9,999	1 April 2013
4,100–5,999	1 May 2013
4,000–4,099	1 June 2013
3,000–3,999	1 July 2013
2,000–2,999	1 August 2013
1,250–1,999	1 September 2013
800–1,249	1 October 2013
500–799	1 November 2013
350–499	1 January 2014
250–349	1 February 2014
160–249	1 April 2014
90–159	1 May 2014
62–89	1 July 2014
61	1 August 2014
60	1 October 2014
59	1 November 2014
58	1 January 2015
54–57	1 March 2015
50–53	1 April 2015
40–49	1 August 2015
30–39	1 October 2015
Fewer than 30	1 June 2015 to 1 April 2017
New employers (no PAYE income payable by April 2012)	1 May 2017 to 1 February 2018

Note: The staging dates for employers with fewer than 30 employees in April 2012 are based on the last digits of their PAYE reference number. The staging dates of 'new employers' are based on when they first had PAYE income payable.
Source: http://www.nowpensions.com/auto-enrolment-staging-dates.

Table C.2: Characteristics of targeted private sector employees working for employers with 58 or more employees in 2012

Characteristic	*Percentage with each characteristic*
Male	60.7%
Works for non-profit institution	11.5%
Works full-time	89.1%
Job is second job	0.4%
Temporary job	4.3%
Industry (based on SIC2007)	
Manufacturing	15.7%
Retail & wholesale	20.4%
Transport & storage	7.3%
Accommodation & food services	4.3%
Information & communications	5.7%
Finance & insurance	6.9%
Mining, electricity & gas	1.9%
Professional, science & technology	7.0%
Administrative & support	7.1%
Education	7.4%
Health	7.0%
Other	9.4%
Age (banded)	
22 to 29	20.9%
30 to 39	25.8%
40 to 49	27.9%
50 to state pension age	25.4%
Years working for employer	
<1 year	9.9%
1 to 2 years	11.4%
2 to 5 years	24.2%
≥5 years	54.4%

Source: Authors' calculations using the Annual Survey of Hours and Earnings.

Table C.3: Further characteristics of targeted private sector employees working for employers with 58 or more employees in 2012

Characteristic	*Mean*	*Standard deviation*	*Median*	*N*
Gross weekly pay (£)	560	346	460	68,970
Member of workplace pension	0.49	0.50	0	68,970
Member of DB pension	0.18	0.38	0	68,970
Employee contribution rate to pension (% of earnings)	2.06	2.95	0	68,285
Employer contribution rate to pension (% of earnings)	4.94	7.09	0	68,285
Total contribution rate to pension (% of earnings)	7.00	9.20	0	68,285
Age (years)	40.71	11.14	41	68,970
Job tenure (months)	97.72	97.69	66	68,970

Source: Authors' calculations using the Annual Survey of Hours and Earnings. Gross weekly pay is rounded to the nearest £ per week.

Table C.4: Effect of automatic enrolment on pension participation: OLS regression results

Independent variable	*Effect*	*Standard error*
Auto-enrolment (AE) in place	0.361***	[0.016]
AE partially introduced (2013)	0.067***	[0.020]
AE partially introduced (2014)	0.170***	[0.010]
AE partially introduced (2015)	0.204***	[0.015]
Year is 2012	–0.007**	[0.003]
Year is 2013	–0.004	[0.007]
Year is 2014	0.002	[0.005]
Year is 2015	0.032***	[0.006]
Employer size: 6,000 to 29,999	–0.043	[0.043]
Employer size: 350 to 5,999	–0.037	[0.042]
Employer size: 160 to 349	–0.075*	[0.043]
Employer size: 58 to 159	–0.137***	[0.043]
Employer size: 50 to 57	–0.200***	[0.045]
Employer size: 5 to 49	–0.280***	[0.044]
Male	0.009***	[0.003]
Age	–0.008	[0.005]
Age squared	0.000***	[0.000]
Age cubed	0.000***	[0.000]
Non-profit institution	0.091***	[0.008]
Full-time job	0.034***	[0.006]
Non-main job	–0.146***	[0.013]
Temporary job	–0.079***	[0.010]
North West	0.01	[0.009]
Yorkshire and the Humber	0.003	[0.010]
East Midlands	0.017*	[0.010]
West Midlands	0.008	[0.010]
South West	0.032***	[0.009]
East	0.034***	[0.009]
London	0.033***	[0.009]
South East	0.048***	[0.009]
Wales	0.024**	[0.012]
Scotland	0.040***	[0.009]
Industry: retail & wholesale	–0.134***	[0.013]
Industry: transport & storage	–0.035*	[0.019]

Industry: accommodation & food services	–0.206***	[0.012]
Industry: information & communications	–0.030**	[0.014]
Industry: finance & insurance	0.070***	[0.016]
Industry: mining, electricity & gas	0.103***	[0.021]
Industry: professional, science & technology	–0.029***	[0.010]
Industry: administrative & support	–0.167***	[0.014]
Industry: education	–0.028**	[0.011]
Industry: health	–0.113***	[0.013]
Industry: other services	–0.119***	[0.011]
Industry: other – not services	–0.110***	[0.009]
Occupational group: professionals	0.061***	[0.005]
Occupational group: associated professionals	0.012**	[0.005]
Occupational group: administrative/secretarial	–0.017***	[0.005]
Occupational group: skilled trades	–0.093***	[0.006]
Occupational group: caring/leisure	–0.171***	[0.012]
Occupational group: sales/customer service	–0.115***	[0.008]
Occupational group: plant and machinery	–0.145***	[0.008]
Occupational group: elementary occupations	–0.143***	[0.008]
Job tenure: 1 to 2 years	0.036***	[0.003]
Job tenure: 2 to 5 years	0.072***	[0.003]
Job tenure: 5 years or more	0.181***	[0.004]
Constant	0.399***	[0.063]

Note: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. Estimated by OLS with standard errors clustered at the employer level. This regression model uses private sector employees targeted for automatic enrolment, excluding those working for employers with 1 to 4 employees in 2012. Years included: 2011 to 2015. Number of observations: 457,443. Omitted categorical variables: year = 2011, employer size = 30,000+, region = North East, industry = manufacturing, occupation group = managerial, job tenure = less than 1 year. Industry is measured using Standard Industrial Classification 2007 (main letter). Occupational group is measured using Standard Occupational Classification 2010 (one-digit).
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

Table C.5: Placebo test: testing for any evidence of an effect had automatic enrolment been introduced 3 years earlier

	Specification	
	(1)	*(2)*
Effect of placebo 'automatic enrolment'	0.002	0.002
Standard error	[0.005]	[0.005]
Number of observations	350,848	350,848
Number of clusters	56,308	56,308
Estimated by:	OLS	Probit
Control variables (*X*) included?	Yes	Yes

Note: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. Probit models are estimated using maximum likelihood. Standard errors are clustered at the employer level and for specification 2 are estimated by bootstrapping the average marginal effect of the (placebo) policy dummy on pension par 250 times. Control variables (X) are listed in Appendix Table C.4. Sample includes all targeted private sector employees from April 2009 to April 2012.
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

Table C.6: Effect of automatic enrolment on pension participation of non-targeted employees, based on pre-reform pension participation in the industry of work

Pension membership in industry (pre-reform)	Effect of auto enrolment	Std error	Number of employers	Sample size	Participation in 2012	Participation in 2015
Low	0.184***	[0.030]	13,734	58,236	1.9%	19.0%
Medium	0.131***	[0.011]	15,205	76,838	10.5%	27.2%
High	0.106***	[0.011]	11,247	34,772	28.2%	42.4%

Notes: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level, * at the 10% level. Estimated by OLS including control variables listed in Appendix Table C.4. *Source*: Authors' calculations using the Annual Survey of Hours and Earnings.

D: Appendix to Chapter 5

Table D.1: Staging dates for employers who at April 2012 had 2 to 29 employees

Final two digits of employers' PAYE reference number	Staging date
92, A1-A9, B1-B9, AA-AZ, BA-BW, M1-M9, MA-MZ, Z1-Z9, ZA-ZZ, 0A-0Z, 1A-1Z or 2A-2Z	1st June 2015
BX	1st July 2015
BY	1st September 2015
BZ	1st November 2015
02-04, C1-C9, D1-D9, CA-CZ or DA-DZ	1st January 2016
00 05-07, E1-E9 or EA-EZ	1st February 2016
01, 08-11, F1-F9, G1-G9, FA-FZ or GA-GZ	1st March 2016
12-16, 3A-3Z, H1-H9 or HA-HZ	1st April 2016
I1-I9 or IA-IZ	1st May 2016
17-22, 4A-4Z, J1-J9 or JA-JZ	1st June 2016
23-29, 5A-5Z, K1-K9 or KA-KZ	1st July 2016
30-37, 6A-6Z, L1-L9 or LA-LZ	1st August 2016
N1-N9 or NA-NZ	1st September 2016
38-46, 7A-7Z, O1-O9 or OA-OZ	1st October 2016
47-57, 8A-8Z, Q1-Q9, R1-R9, S1-S9, T1-T9, QA-QZ, RA-RZ, SA-SZ or TA-TZ	1st November 2016
58-69, 9A-9Z, U1-U9, V1-V9, W1-W9, UA-UZ, VA-VZ or WA-WZ	1st January 2017
70-83, X1-X9, Y1-Y9, XA-XZ or YA-YZ	1st February 2017
P1-P9 or PA-PZ	1st March 2017
84-91, 93-99	1st April 2017

Source: http://www.nowpensions.com/auto-enrolment-staging-dates.

Table D.2: Effect of automatic enrolment on workplace pension participation among targeted employees of small employers: OLS regression results

Dependent variable: pension participation	Coefficient	Standard error
Automatic enrolment introduced by employer	0.440***	[0.027]
Male	–0.001	[0.009]
Gross earnings (£ per week)	0.001***	[0.000]
Gross earnings squared	0.000***	[0.000]
Hour worked: 16-29	0.059***	[0.014]
Hours worked: 30-39	0.031***	[0.011]
Hours worked: 40-49	–0.050***	[0.011]
Hours worked: 50+	–0.114***	[0.017]
Years with current employer	0.008***	[0.001]
Less than 1 year with current employer	–0.010	[0.010]
1-2 years with current employer	0.004	[0.010]
No information on job tenure	–0.035*	[0.018]
Employer size in 2012	0.004***	[0.001]
Employer size in 2016	0.005***	[0.001]
Age	0.007***	[0.002]
Age squared	0.000***	[0.000]
Industry: Agriculture	–0.050*	[0.026]
Industry: Manufacturing	–0.060***	[0.019]
Industry: Construction	–0.123***	[0.019]
Industry: Retail and Wholesale	–0.072***	[0.018]
Industry: Transport and Storage	–0.087***	[0.024]
Industry: Accommodation and Food Services	–0.095***	[0.020]
Industry: Information and Communications	–0.083***	[0.023]
Industry: Finance or Insurance	0.075**	[0.033]
Industry: Real Estate	–0.083***	[0.028]
Industry: Scientific or Tech professions	–0.067***	[0.019]
Industry: Administration or support	–0.046**	[0.020]
Industry: Education	–0.002	[0.028]
Industry: Health	0.135***	[0.019]
Industry: Arts and Recreation	–0.011	[0.032]
Occupation: Professional	0.041***	[0.016]
Occupation: Associate Professional	0.043***	[0.015]
Occupation: Admin/Secretarial	0.071***	[0.014]
Occupation: Skilled Trades	–0.031**	[0.013]
Occupation: Caring	–0.140***	[0.018]
Occupation: Sales/Customer Service	–0.017	[0.017]
Occupation: Plant and Processing	–0.038**	[0.016]
Occupation: Elementary occupations	–0.011	[0.015]
Constant	–0.257***	[0.049]

Notes: *** denotes that the effect is significantly different from zero at the 1% level, ** at the 5% level and * at the 10% level. Standard errors are clustered at the employer level. Results are from estimating equation 5.1 by OLS. Sample size: 14,756.

Source: Authors' calculations using the Annual Survey of Hours and Earnings.

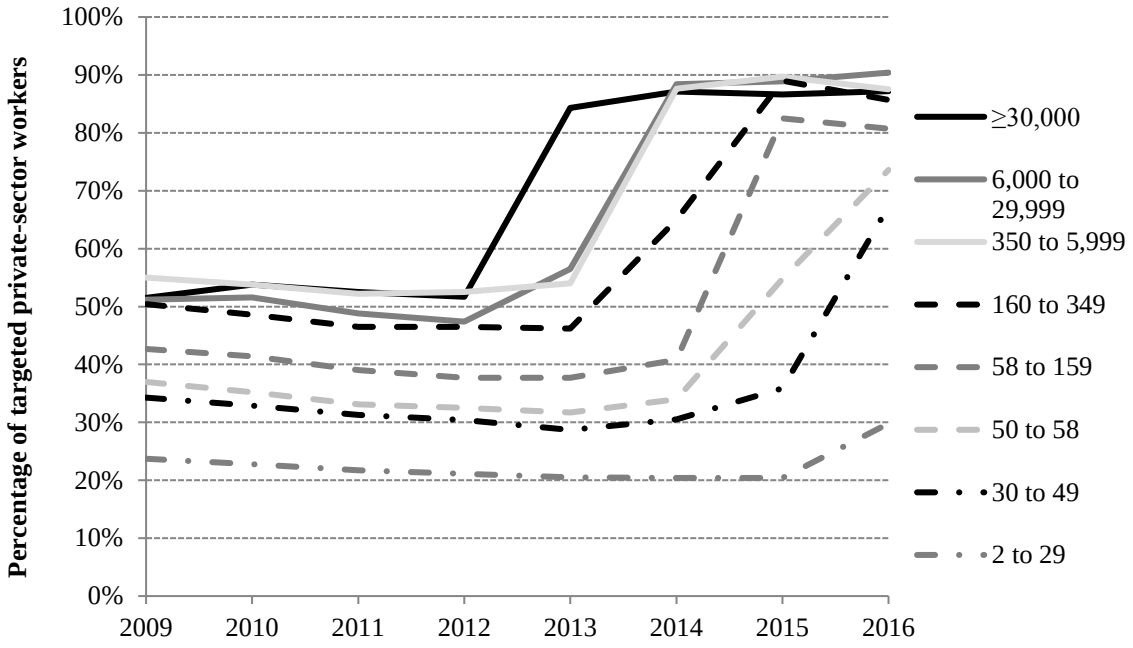
Table D.3: Effect of automatic enrolment on pension participation of subgroups working for small employers (with between 2 and 29 employees in April 2012)

	Effect of automatic enrolment	Standard Error	Sample size	Unadjusted pension participation rates in:	
				Control group	Treatment group
All	0.440***	[0.027]	14,756	23.2%	70.1%
Sex					
Male	0.444***	[0.036]	8,201	21.2%	68.2%
Female	0.433***	[0.039]	6,032	25.9%	72.7%
Earnings					
<£350 per week	0.457***	[0.047]	4,988	15.8%	65.0%
£350 to £500 per week	0.437***	[0.049]	4,285	20.8%	67.4%
£500+ per week	0.420***	[0.043]	4,960	32.6%	77.8%
Age group					
Age 22-39	0.543***	[0.039] †††	6,712	19.3%	74.6%
Age 40-state pension age	0.358***	[0.036] †††	7,521	26.6%	66.7%
Years with employer					
Fewer than four years	0.491***	[0.040] †††	6,903	18.6%	68.4%
Four years or more	0.396***	[0.036] †††	7,330	27.5%	71.5%
Not targeted for automatic enrolment					
All	0.139***	[0.029]	8,400	6.8%	20.9%

Notes: Effects are from estimating equation 5.1 by OLS with control variables as set out in Table D.2. ***, ** and * denote that the effect is significantly different from zero at the 1%, 5% and 10% levels, respectively. Standard errors are clustered at the employer level. †††, †† and † signify that the effect of automatic enrolment on one subgroup is statistically different to the effect for the other group at the 1%, 5% and 10% levels, respectively.

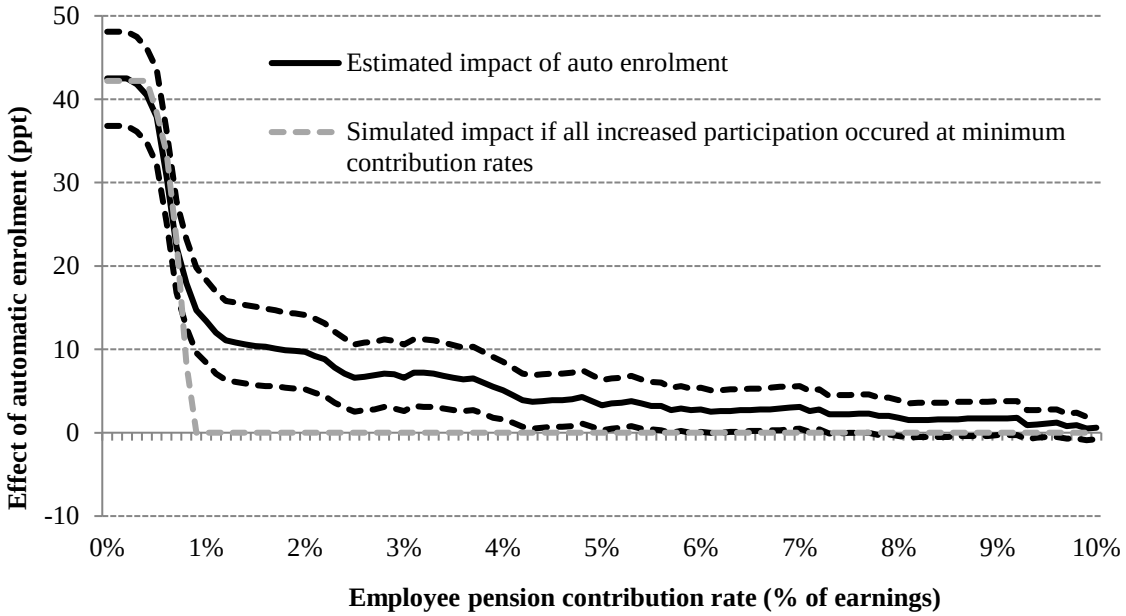
Source: Authors' calculations using the ASHE.

Figure D.1. Workplace pension participation rates among private-sector employees targeted for automatic enrolment, by employer size in 2012



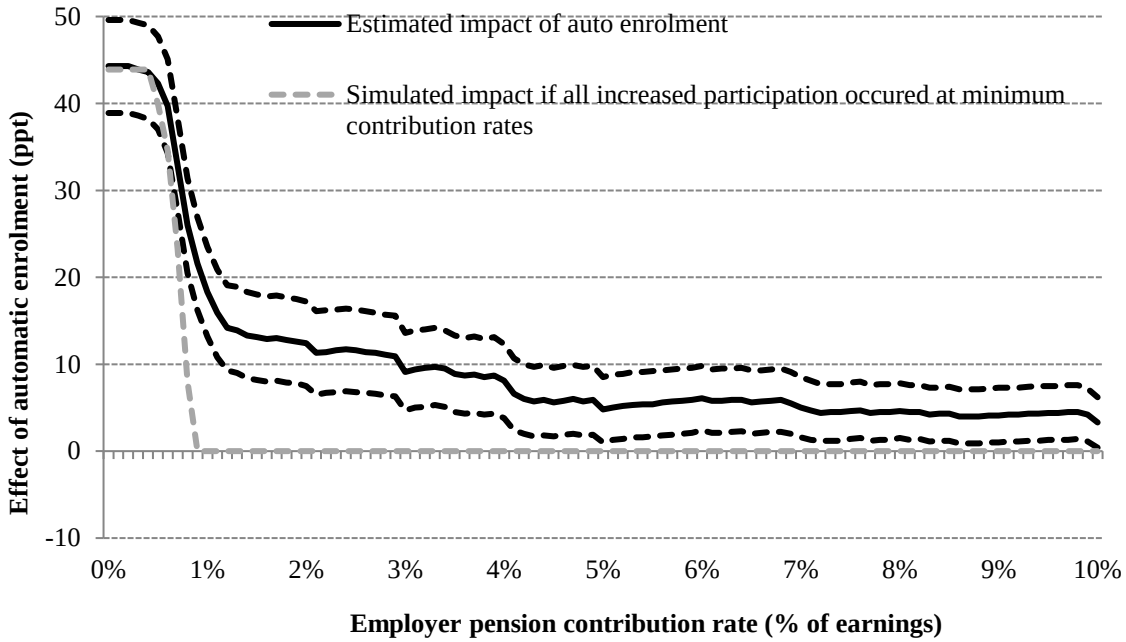
Source: Authors' calculations using data from the ASHE.

Figure D.2: Effect of automatic enrolment on probability of employee contribution rate being greater than certain percentages of earnings, for targeted employees of small employers



Note: Each of the data points (at each 0.1% of earnings) is the coefficient on auto-enrolment from estimating equation 5.1 where the dependent variable a dummy indicating if the contribution rate is in excess of the specified amount. Each regression is estimated by OLS including control variables (*X*, listed in Table D.2). Standard errors are clustered at the employer level and used to construct the 95% confidence intervals shown in dotted lines. The grey dashed line shows the simulated impact on contribution rates had the entire increase in pension participation occurred at the minimum default contribution rates and had no-one else changed their pension saving in response to the reform.
Source: Authors' calculations using the Annual Survey of Hours and Earnings.

Figure D.3: Effect of automatic enrolment on probability of employer contribution rate being greater than certain percentages of earnings, for targeted employees of small employers



Notes and sources: See notes and sources to Figure D.1.

E: Appendix to Chapter 6

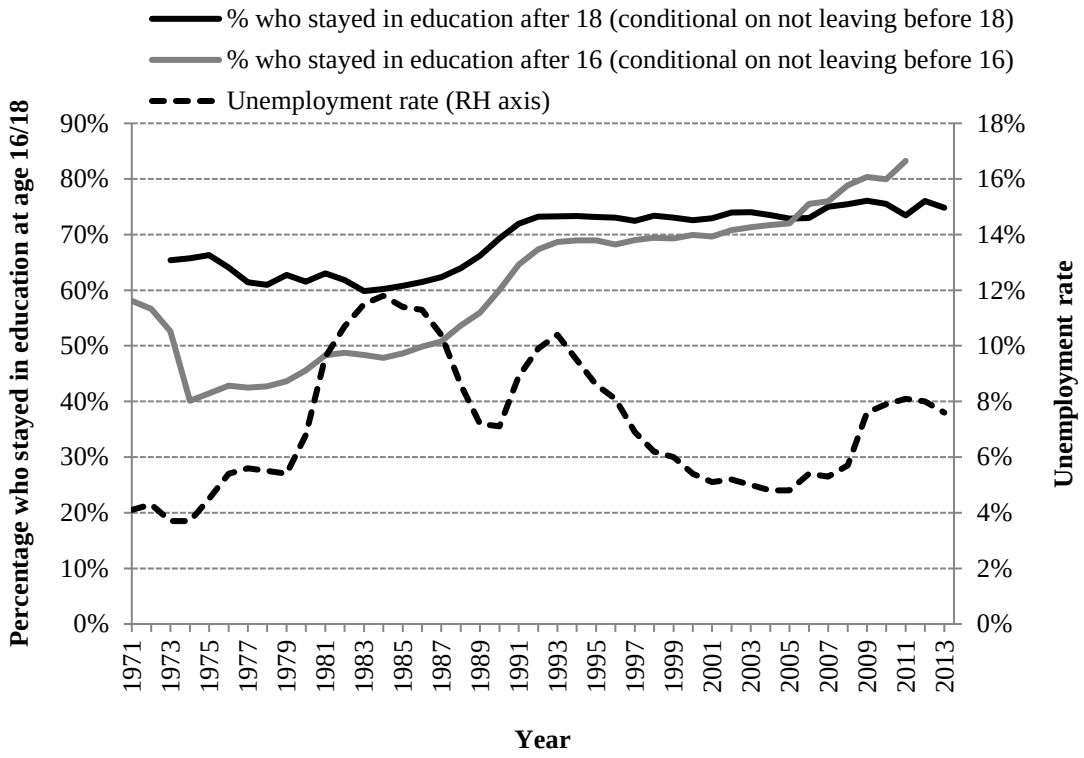
Appendix E.1: Effect of unemployment rate on probability of remaining in education

As discussed in sect ion 6.3, one possible challenge to our empirical strategy is that individuals may change the age at which they leave education in response to the economic conditions at the point they are making that decision. Following Altonji et al. (2016), who look at the probability of remaining in university education after age 22, we estimate what impact the unemployment rate at age 16 and age 18 has on the probability of remaining in education after those ages. Figure E.1 shows that it is important to account for the underlying trends of increasing participation in education. Two key determinants of the probability of staying on after age 16 have been policy related. First, the increase in the minimum school-leaving age to 16 in the early 1970s meant that, conditional on staying on until age 16, the probability of remaining in education afterwards fell. Second, the introduction of GCSEs (and replacement of O levels and CSEs) in 1988 meant that exam performance increased and the proportion who got the grades to stay on in education increased (see McVicar and Rice (2001) and Machin and Vignoles (2006) for more details). Given these policy-driven trends, it is important to control carefully for the cohort trends. Using a simple polynomial, or even the five-year cohort trends as used in the rest of the chapter, would not be appropriate in this case.

We therefore regress the probability of remaining in education on the unemployment rate at the relevant age, controlling for underlying cohort trends using a piecewise linear spline in year of birth that has four nodes, one of which (importantly) is in 1988 – as the reform to GCSEs were introduced. We also include dummies for sex, the data set used (FRS/FES) and whether the individual had a minimum school-leaving age of 15 or 16. The results of this analysis, shown in Table E.1, show that a 4ppt increase in the unemployment rate has a small and not statistically significant, impact on remaining on in education, increasing the probability of staying on after age 16 by 1.1ppts and the probability of staying on after age 18 by 1.6ppts.[58]

[58] While these effects are smaller than those estimated in some studies – such as Rice (1987) and Clark (2011) – our effects are estimated using the national unemployment rate, not local unemployment rates. These are slightly different conceptually. For example, in most of the period we examine, there were limited numbers of

Figure E.1: Probabilities of individuals staying on in education at ages 16 and 18, by year, and unemployment rate



Note: Sample is all 20- to 24-year-olds observed in the FRS and FES between 1978 and 2016.

Table E.1: Effect of a 4 percentage point increase in unemployment rate at age 16/18 on probability of staying on in education past those ages (conditional on not having already left education)

	Pr(Stay in education beyond age 16)	Pr(Stay in education beyond age 18)
Effect of 4ppt increase in unemployment rate at age 16	0.011 (0.011)	
Effect of 4ppt increase in unemployment rate at age 18		0.016 (0.011)
Sample size	99,195	51,976

Note: Sample is based on all 20- to 24-year-olds observed in FES and FRS data between 1978 and 2016. Standard errors clustered at the year of birth level. *** indicates that the effect is statistically different from zero at the 1% level, ** at the 5% level and * at the 10% level.

places in higher education for school leavers to take up. Therefore while higher unemployment in one area might encourage more people from that area to apply to higher education, it does not necessarily mean that more people nationally will undertake higher education.

Appendix E.2: Supplementary tables to Chapter 6

Table E.2: Effect of a 4 percentage point increase in unemployment rate upon entering labour market on labour market outcomes, by education group

Effect, by years since left education	Low education		Mid education		High education	
	In paid work	Log individual earnings of workers	In paid work	Log individual earnings of workers	In paid work	Log individual earnings of workers
0	–0.128***	–0.143***	–0.052**	–0.113***	–0.029	–0.019
	(0.017)	(0.034)	(0.022)	(0.027)	(0.016)	(0.030)
1	–0.070***	–0.030	–0.055***	–0.118***	–0.038***	–0.000
	(0.018)	(0.022)	(0.016)	(0.019)	(0.011)	(0.024)
2	–0.010	–0.059***	–0.038**	–0.073***	–0.037***	–0.016
	(0.014)	(0.020)	(0.019)	(0.020)	(0.009)	(0.019)
3	–0.027	–0.055***	–0.015	–0.082***	–0.018	–0.003
	(0.017)	(0.014)	(0.016)	(0.014)	(0.012)	(0.020)
4	0.000	–0.023*	–0.044**	–0.053***	–0.005	0.010
	(0.014)	(0.013)	(0.018)	(0.018)	(0.013)	(0.020)
5	0.008	–0.031*	–0.033**	–0.030	–0.024**	0.013
	(0.013)	(0.018)	(0.016)	(0.024)	(0.011)	(0.018)
6	–0.012	–0.005	–0.038**	–0.036**	–0.010	0.016
	(0.015)	(0.017)	(0.015)	(0.015)	(0.011)	(0.018)
7	–0.028**	–0.002	–0.039**	0.006	–0.007	–0.014
	(0.012)	(0.015)	(0.016)	(0.021)	(0.011)	(0.023)
8	–0.004	–0.002	–0.011	–0.001	–0.015	0.018
	(0.013)	(0.014)	(0.019)	(0.019)	(0.012)	(0.016)
9	–0.007	0.014	–0.026	0.009	–0.014	0.016
	(0.012)	(0.019)	(0.016)	(0.018)	(0.012)	(0.015)
10	–0.003	0.014	–0.032*	–0.035	–0.007	–0.011
	(0.011)	(0.016)	(0.017)	(0.026)	(0.011)	(0.016)
Observations	76,337	49,471	52,635	40,007	75,230	61,136

Note: Effects are obtained by estimating equation 6.1 by probit (for in paid work) or OLS (for log earnings of workers). Standard errors clustered at the year left education level. *** indicates that the effect is statistically different from zero at the 1% level, ** at the 5% level and * at the 10% level. *Source*: Authors' calculations using Family Resources Survey and Family Expenditure Survey, 1978 to 2016.

Table E.3: Effect of a 1 percentage point increase in the output gap (positive is above trend) upon entering labour market on measures of employment, earnings, income and expenditure

Effect, by years since left education			For working families only					For all families		
	In paid work	Log individual earnings of workers	Log family earnings	Log family private income	Log family net income	Log household private income	Log equivalised net household income	Log equivalised net family income	Log equivalised net household income	Log equivalised total expenditure
0	0.0060	0.0171***	0.0180***	0.0192***	0.0159***	0.0094**	0.0048	-0.0000	0.0075***	-0.0003
	(0.0039)	(0.0036)	(0.0047)	(0.0046)	(0.0036)	(0.0044)	(0.0038)	(0.0065)	(0.0024)	(0.0042)
1	0.0075***	0.0113***	0.0095**	0.0091**	0.0057	0.0020	-0.0005	0.0140***	0.0021	-0.0003
	(0.0024)	(0.0038)	(0.0039)	(0.0040)	(0.0035)	(0.0028)	(0.0022)	(0.0043)	(0.0024)	(0.0025)
2	0.0078***	0.0063**	0.0094***	0.0094***	0.0070***	0.0024	-0.0013	0.0095***	0.0045*	0.0012
	(0.0021)	(0.0029)	(0.0028)	(0.0031)	(0.0025)	(0.0035)	(0.0024)	(0.0032)	(0.0024)	(0.0031)
3	0.0011	0.0070***	0.0103***	0.0102***	0.0060***	0.0050	0.0015	0.0100***	0.0053*	0.0046
	(0.0021)	(0.0018)	(0.0027)	(0.0026)	(0.0018)	(0.0032)	(0.0023)	(0.0025)	(0.0028)	(0.0035)
4	0.0002	0.0034	0.0047	0.0040	0.0019	-0.0035	-0.0060***	0.0061*	-0.0016	0.0030
	(0.0024)	(0.0029)	(0.0032)	(0.0033)	(0.0028)	(0.0029)	(0.0022)	(0.0035)	(0.0024)	(0.0029)
5	0.0003	0.0042	0.0056*	0.0052*	0.0026	0.0057*	0.0010	0.0039	0.0040**	0.0031
	(0.0019)	(0.0027)	(0.0031)	(0.0030)	(0.0023)	(0.0029)	(0.0019)	(0.0029)	(0.0018)	(0.0021)
6	0.0015	0.0016	0.0005	0.0005	0.0002	0.0006	0.0012	0.0017	0.0021	-0.0002
	(0.0021)	(0.0031)	(0.0027)	(0.0026)	(0.0018)	(0.0024)	(0.0020)	(0.0025)	(0.0021)	(0.0024)
7	0.0023*	0.0031	0.0035	0.0030	-0.0002	0.0059**	0.0032*	0.0039	0.0059***	0.0055***
	(0.0013)	(0.0022)	(0.0023)	(0.0024)	(0.0020)	(0.0023)	(0.0018)	(0.0024)	(0.0020)	(0.0019)
8	0.0045**	0.0024	0.0049*	0.0054*	0.0036	0.0024	0.0009	0.0048	0.0020	0.0004
	(0.0019)	(0.0038)	(0.0029)	(0.0032)	(0.0027)	(0.0031)	(0.0024)	(0.0032)	(0.0025)	(0.0026)
9	0.0060***	0.0044	0.0029	0.0027	0.0012	0.0032	0.0009	0.0080***	0.0056***	0.0052*
	(0.0015)	(0.0032)	(0.0042)	(0.0042)	(0.0034)	(0.0029)	(0.0018)	(0.0025)	(0.0020)	(0.0028)
10	0.0031*	0.0118***	0.0038	0.0033	0.0011	0.0036	0.0011	0.0061**	0.0062***	0.0095***
	(0.0016)	(0.0031)	(0.0031)	(0.0030)	(0.0020)	(0.0027)	(0.0018)	(0.0024)	(0.0023)	(0.0023)
Observations	197,751	149,870	162,227	162,046	161,216	161,076	160,029	192,891	198,839	72,734

Note: Effects are obtained by estimating equation 6.1 by OLS with the specified dependent variables (except for the results for in paid work, which are estimated by a probit). Standard errors clustered at the year left education level. *** indicates that the effect is statistically different from zero at the 1% level, ** at the 5% level and * at the 10% level.

Source: Authors' calculations using Family Resources Survey and Family Expenditure Survey, 1978 to 2016.

Table E.4: Effect of a 4 percentage point increase in unemployment rate upon entering labour market on measures of employment, earnings, income and expenditure, for single-family households

Effect, by years since left education	In paid work	Log individual earnings of workers	For working families only: Log family earnings	Log family private income	Log family net income	Log household private income	Log equivalised net household income	For all families: Log equivalised net family income	Log equivalised net household income	Log equivalised total expenditure
0	–0.110***	–0.126	–0.175**	–0.151**	–0.116**	–0.151**	–0.038	–0.124**	–0.099**	–0.128**
	(0.040)	(0.085)	(0.069)	(0.068)	(0.053)	(0.068)	(0.042)	(0.047)	(0.042)	(0.058)
1	–0.051***	–0.011	–0.016	–0.019	–0.004	–0.020	0.004	–0.027	–0.027	–0.034
	(0.019)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.018)	(0.022)	(0.018)	(0.023)
2	–0.047***	–0.049**	–0.087***	–0.091***	–0.061***	–0.085***	–0.033*	–0.083***	–0.082***	–0.046*
	(0.015)	(0.021)	(0.023)	(0.024)	(0.020)	(0.023)	(0.016)	(0.016)	(0.019)	(0.026)
3	–0.036***	–0.080***	–0.091***	–0.090***	–0.053***	–0.090***	–0.046***	–0.075***	–0.073***	–0.093***
	(0.013)	(0.019)	(0.016)	(0.017)	(0.015)	(0.017)	(0.014)	(0.019)	(0.019)	(0.025)
4	–0.014	–0.038**	–0.018	–0.015	–0.002	–0.014	–0.001	–0.026	–0.029*	–0.068***
	(0.012)	(0.017)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)	(0.016)	(0.013)	(0.014)
5	–0.016	–0.014	–0.027	–0.025	–0.014	–0.025	–0.002	–0.034*	–0.024	–0.020
	(0.011)	(0.019)	(0.019)	(0.019)	(0.014)	(0.019)	(0.013)	(0.018)	(0.016)	(0.014)
6	–0.022**	–0.018	–0.020	–0.019	–0.008	–0.019	–0.008	–0.028**	–0.034***	–0.036**
	(0.008)	(0.014)	(0.015)	(0.015)	(0.013)	(0.015)	(0.010)	(0.012)	(0.011)	(0.011)
7	–0.020**	–0.030**	–0.027**	–0.027*	–0.016	–0.026*	0.004	–0.024*	–0.023*	–0.021
	(0.008)	(0.014)	(0.013)	(0.014)	(0.011)	(0.013)	(0.011)	(0.014)	(0.014)	(0.016)
8	–0.008	–0.012	–0.018*	–0.015	–0.008	–0.015	–0.003	–0.007	–0.012	–0.006
	(0.009)	(0.012)	(0.015)	(0.015)	(0.011)	(0.015)	(0.011)	(0.013)	(0.012)	(0.013)
9	–0.012	–0.010	–0.012	–0.010	–0.007	–0.010	0.006	–0.010	–0.010	–0.019
	(0.009)	(0.013)	(0.015)	(0.015)	(0.011)	(0.015)	(0.012)	(0.015)	(0.012)	(0.016)
10	–0.009	–0.037***	–0.012	–0.007	–0.003	–0.007	0.006	–0.006	–0.006	–0.010
	(0.009)	(0.012)	(0.014)	(0.013)	(0.010)	(0.013)	(0.011)	(0.013)	(0.012)	(0.014)
Observations	104,683	74,678	86,294	86,125	85,474	86,275	85,302	101,849	101,135	34,434

Note: Effects are obtained by estimating equation 6.1 by OLS with the specified dependent variables (except for the results for in paid work, which are estimated by a probit). Standard errors clustered at the year left education level. *** indicates that the effect is statistically different from zero at the 1% level, ** at the 5% level and * at the 10% level.

Source: Authors' calculations using Family Resources Survey and Family Expenditure Survey, 1978 to 2016.

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