Lone Parent Obligations Impact Assessment

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Executive Summary

In a series of four successive reforms, the UK government tightened the age eligibility requirement for lone parents to claim Income Support (IS). Prior to 2008, lone mothers were eligible to claim IS provided their youngest child was under the age of sixteen. Between 2008 and 2012, the child age eligibility criterion was changed as follows:

- November 2008: IS restricted to LP with children under twelve
- October 2009: IS restricted to LP with children under ten
- October 2010: IS restricted to LP with children under seven
- May 2012: IS restricted to LP with children under five

Economic theory predicts that the employment rate of affected lone mothers should not decrease in response to these changes. Whether employment increases or remains unaffected in response to the change in utility of non-employment depends upon the preferences of lone mothers.

This study takes advantage of the staggered nature of the changes and uses a difference-in-differences approach to test whether the reforms have had a statistically significant impact on the employment rates of affected lone mothers relative to lone mothers with slightly younger children unaffected by the reforms. As a result of the close timing among the reforms, these estimates reflect the short-term impacts of these policy changes on lone mother employment.

Restricting IS eligibility to lone parents with children under twelve raised the employment rate of lone mothers who lost eligibility by 3 percentage points relative to lone mothers whose youngest child was 10–11 years old and still eligible to claim IS. Repeating the analysis for these lone mothers by qualification level, the impact is substantially larger for lone mothers with high qualifications. For lone mothers with low qualifications, all policy impact estimates were positive but not statistically significant, at even the 10% level.

The estimated impact on employment rates of the three subsequent policy changes are not statistically significant at the 10% level. The eligibility restriction from ten to seven increased employment by around 3 percentage points as well, though the standard errors on this estimate are large. While not significant at the 10% level, lone mothers with high qualifications see a larger 3.8% increase in employment in response to the change, mirroring the pattern seen in the under twelve eligibility restriction.

On the basis of these results, the changes to the IS child age eligibility rules have probably been successful in raising employment rates for lone mothers. The response in employment rate is larger for those with high qualifications than low qualifications and the magnitude of the change in employment is likely smaller for lone mothers with younger children. This result suggests that lone mothers

with greater barriers, either in terms of the costs of working (e.g. childcare for younger children) or ability to secure employment (e.g. low skills), may require additional support before they can respond to changes in incentives by returning to work.

1 Introduction

Since April 1988, workless lone parents with children under the age of sixteen had been able to claim Income Support (IS), a transfer payment also intended to support carers and sick or disabled individuals. Over the course of four years, the UK government instituted a number of incremental reductions to the age of youngest child eligibility requirement for Lone Parents (LP) to qualify for IS.[1]

- November 2008: IS restricted to LP with children under twelve
- October 2009: IS restricted to LP with children under ten
- October 2010: IS restricted to LP with children under seven
- May 2012: IS restricted to LP with children under five

These reforms represent a substantial change in the government-claimant contract among lone parents. Lone parents who no longer qualify for IS transition onto Jobseeker's Allowance (JSA) instead. Unlike IS, JSA places a strong conditionality on its claimants. To be eligible, JSA recipients must, among other requirements, be able and available to work and actively seeking work. Claiming JSA also requires visits every other week to a Jobcentre Plus (JCP) office where recipients must demonstrate how they have been searching for work.[2]

In preparation for the transition from IS to JSA, lone parent IS claimants must attend Work Focussed Interviews (WFI) which begin the process of examining employment and training options. These mandatory interviews are every six months for two years before being conducted quarterly in the final year before a lone parent IS claimant moves onto JSA.[3] The purpose of these interviews is to provide information about the upcoming change as well as the differences between benefit regimes.

This report seeks to assess whether these reforms were successful in raising the levels of employment for the lone mothers affected by leveraging sequential nature of the changes to child age eligibility. While the study will not examine the impact of these policy changes on claims for other benefits, it will produce an estimate of the short-run impact on lone mother employment rates.

The paper will proceed as follows. The next section reviews the economic theory around unemployment and inactivity before reviewing the empirical literature. In the following section, the methodology and the data are discussed. Section 5 proceeds with the analysis and a series of robustness checks. A discussion of the findings concludes.

1 The first three changes were first proposed in the DWP Green Paper "In work, better off: Next steps to full employment" published in 2007. The final change was introduced as part of the Welfare Reform Act 2012

2 www.gov.uk/jobseekers-allowance/eligibility

3 ssac.independent.gov.uk/pdf/equality-analysis.pdf, www.dwp.gov.uk/docs/dmgch05.pdf

2
Literature Review

2.1 Theoretical predictions

According to theoretical labour economics, job search can be modelled within a search optimization framework. In essence, an unemployed individual attempts to maximise his utility by choosing the intensity at which he searches for work and the wage at which he is willing to accept an offer of employment. Mathematically, this may be represented in discrete time without loss of generality as a Bellman equation:

$$\max U_t = \max\left\{b_0 + (b_1 - a) + \frac{1}{1+r}\int \max\{W, U_{t+1}\}\, dF(W)\right\}, t = 1,2,\ldots \quad (1)$$

where b_0 includes government transfers and other benefits of inactivity, b_1 represents the additional benefit received provided the individual is searching for work, a is the cost of job search, W is the present value of the income stream of an accepted job offer, U_{t+1} is the value of job search next period, $F(W)$ represents the cumulative distribution function of wage offers, and r is the discount rate.[4]

The right hand side can be divided into three elements. The leftmost portion is the benefit from being non-employed this period. The middle parentheses enclose the net cost of searching for work and the rightmost expression encapsulates whether the jobseeker accepts the wage offered next period or prefers the discounted stream of utility from continuing to search for employment in future periods. The unemployed or inactive individual attempts to maximise her utility by deciding whether to search and, if searching, whether to continue searching after each received wage offer. As the discounted net benefit of non-employment rises (e.g., higher benefit levels), the wage level that the individual is willing to accept (i.e., reservation wage) rises. If the effort or cost of job search (a) is high enough, then the claimant maximises utility by not searching at all and relying simply on the discounted stream of benefits.

Changes to IS eligibility may be represented by setting b_0 to zero for the affected lone parents and increasing b_1 to account for the JSA if the individual decides to search for employment. If the net value of job search is positive, we would expect this to lead to job search behaviour and a lower reservation wage; however, if the cost is large enough or the expected future wage offers low enough, it may be optimal to remain inactive even after this loss of benefit. This means that at worst, the job search decision is unaffected (but the claimant is substantially worse off) and at best, the age eligibility reforms increase movement off of IS onto JSA. The dominant effect will depend on the preferences of the lone parent population.[5]

4 Adapted from Mortensen, D. T., & Pissarides, C. A. 1999. "New developments in models of search in the labor market." Handbook of Labor Economics, 3: 2571–2573

5 A graphical depiction of these two outcomes may be found in: Petrongolo, Barbara. 2009. "The Long-term Effects of Job Search Requirements: Evidence from the UK JSA Reform." Journal of Public Economics 93, no. 11: pp 1234–1253

2.2 Previous evidence

Government support for lone parents has undergone several changes over the past two decades and the research that has evaluated these reforms has found support for both theoretical outcomes discussed above.

Studies of a major reform to the US welfare system in 1996 encompassed by the Personal Responsibility and Work Opportunity Act (PRWOA) found evidence of both higher exits to work and higher rates of off-benefit inactivity. Hofferth, Stanhope and Harris (2001) provided some evidence that stricter work requirements and a reduction in exemptions for mothers with young children was associated with a higher rate of exit from benefits to work.[6] In contrast, Blank and Kovac (2008) found that the reforms increased the number of inactive families that were not claiming benefits, suggesting that many families left benefits to unsupported inactivity and became 'disconnected families'.[7] Within the context of the predictions put forward by the theoretical model above, the results of both studies are consistent with an increase in the cost of non-employment a. For some lone parents, the increased cost of remaining on benefits outweighed the combination of government transfers and expected returns of job search for all reservation wages. For others, the job search was the rational response.

The UK also made some fundamental changes to its unemployment support regime in 1996 by replacing Unemployment Benefit with the more rigorous Jobseeker's Allowance (JSA). Once again, the impact of the changes was mixed. Careful analysis in Manning (2005) suggests that much of the impact of JSA on unemployed claimants was not into employment, at least in the short-term.[8] Petrongolo (2009) examined the longer-term effects and found that the reform increased off-flow from JSA into disability benefits. Furthermore, she found that the reform reduced the weeks worked and earnings of claimants at one year after unemployment exit.[9]

A broader review of the evidence on lone parent benefit requirements from research on subsequent reforms indicates that these changes have generally increased exits to employment. Gregg and Harkness (2003) reported that while the initial JSA reforms had little impact on lone parent employment, reforms after 1997 resulted in an estimated 5% increase in employment.[10] The impact on the employment rate of lone parents working over sixteen hours was even larger, at roughly 7.2%.[11] A range of DWP studies have found a positive impact due to the introduction of Work Focused Interviews (WFI) in 2001.[12] The introduction of Jobcentre Plus (JCP), in combination with these changes, appears to have also increased exits into employment for all claimant groups.[13] Increasing requirements on claimants out of the labour force more generally also appears to increase employment. For instance, Brewer (2008) found support for a short-run positive impact of increasing conditionality on Incapacity Benefit (IB) claimants in the Pathways to Work reforms.[14]

A pair of studies by Zeenat Soobedar illustrates the real possibility that changes to the child age eligibility requirements to qualify for IS for lone parents could both increase lone parent employment and raise the number of claims for other benefits. Soobedar (2009a) found that lone mothers with no qualifications just above the age eligibility cut-off had a 3 percentage point higher probability of finding a job than those just below.[15] In contrast to this finding, Soobedar (2009b) estimated that lone mothers without qualifications crossing the IS age

6 Hofferth, Sandra L., Stephen Stanhope, and Kathleen Mullan Harris. 2002. "Exiting Welfare in the 1990s: Did Public Policy Influence Recipients' Behavior?" Population Research and Policy Review 21, no. 5: 433–472

7 Blank, Rebecca M., and Brian K. Kovak. 2009. "The Growing Problem of Disconnected Single Mothers," *Making the Work-Based Safety Net Work Better*: pp 227–58

8 Manning, Alan. 2009. "You Can't Always Get What You Want: The Impact of the UK Jobseeker's Allowance." Labour Economics 16, no. 3: pp 239–250

9 Petrongolo (2009), op. cit

10 Gregg, Paul, and Susan Harkness. 2003. "Welfare Reform and Lone Parents Employment in the UK." Leverhulme Centre for Market and Public Organisation, University of Bristol, Department of Economics. www.bris.ac.uk/cmpo/publications/papers/2003/wp72.pdf

11 Gregg, Paul, Susan Harkness, and Sarah Smith. 2009. "Welfare Reform and Lone Parents in the UK." The Economic Journal 119, no. 535: pp 38–65

12 See for instance: Hosain, Mehreen and Eleanor Breen. 2007. *New Deal Plus for Lone Parents Qualitative Evaluation*

13 Karagiannaki, Eleni. 2007. "Exploring the Effects of Integrated Benefit Systems and Active Labour Market Policies: Evidence from Jobcentre Plus in the UK." Journal of Social Policy 36, no. 2

14 Brewer, Mike. 2008. Welfare Reform in the UK: 1997–2007. Working paper//IFAU-Institute for Labour Market Policy Evaluation. www.econstor.eu/handle/10419/45770

15 Soobedar, Zeenat. 2009. "Labour Supply Disincentives of Income Support: An Analysis of Single Mothers with No Qualifications in the UK." Working Paper, School of Economics and Finance, Queen Mary, University of London. www.econstor.eu/handle/10419/55192

eligibility threshold are 4.2% more likely to claim disability benefits, while over a quarter apply for sickness and disability benefits from IS.[16] These results suggest that lowering the age eligibility of youngest child may increase employment and health benefit claims for lone mothers of younger children.

In support of the theoretical model, the evidence illustrates that inactive or unemployed lone parents respond to higher benefit conditionality either by engaging in job-seeking behaviour, moving to other benefits, or leaving benefits to inactivity. Whether employment rates of lone mothers has increased in response to the recent changes to IS age eligibility will be explored in greater detail below.

16 Soobedar, Zeenat. 2009. "Disability Benefits: A Substitute for Income Support for Single Mothers with No Qualifications in the UK." Working Paper, School of Economics and Finance, Queen Mary, University of London. www.econstor.eu/handle/10419/55183

3
Methodology

3.1 Difference-in-Differences

The following analysis uses a Difference-in-Differences (DD) approach frequently used in the impact evaluation literature. Measuring the effects of a policy is inherently difficult since the ideal case, simultaneously comparing the same individual affected by the policy to the same individual not subject to the change, is clearly impossible.

To clarify, suppose Y_t^d represents the outcome of interest in time t and d indicates whether this individual has been affected by the policy change. For instance, Y_t^1 indicates the outcome in time t of an individual who has received 'treatment' whereas Y_t^0 is the outcome for someone who is unaffected by the policy change in time t. Suppose there are only two periods, 0 and 1, and the policy change happens between these two periods. Mathematically, the effect of a policy change is represented as:

$$Avg.\,Treatment\ Effect\ on\ Treated = ATE^1 = E(Y_1^1 - Y_1^0 | D = 1) \quad (2)$$

This equation may be interpreted as the difference in the means of the outcome variable (e.g., whether employed or not) for the group subject to the policy change (e.g., lone parents with children 12–15, represented by $D = 1$) when this group is subject to the policy change and when it isn't. As mentioned earlier, however, the same individuals cannot be observed under the new policy and at the same time be observed under the old policy. Hence, we can never observe both Y_1^1 (i.e. outcome under policy) and Y_1^0 (i.e. outcome without policy).

To address this problem, DD compares a treatment group against a control group both before and after a policy change. Provided several identifying assumptions are met, this creates valid counterfactual groups that allow the average effect of the policy change to be estimated. Returning to the notation used above:

$$ATE^1 = [E(Y_1 | D = 1) - E(Y_0 | D = 1)] - [E(Y_1 | D = 0) - E(Y_0 | D = 0)] \quad (3)$$

Intuitively, we difference the mean outcome for the treatment group pre- and post-change to isolate any group-specific factors influencing the outcome, leaving the change arising from the policy and from other time effects. By repeating this process for the control group and subtracting the resulting figure from the treatment group amount, we should eliminate any time-related change and isolate the policy impact.

The most basic formulation of the DD model is one with two periods: a pre-change and a post-change period. Within a regression framework, this specification is:

$$Y_{i,t} = \alpha_1 + \alpha_2 \cdot g_i + \alpha_3 \cdot t + \alpha_4 \cdot g_i \cdot t + \varepsilon_{i,t} \quad t = 0,1 \qquad (4)$$

In this case, $Y_{i,t} = 1$ if individual i is employed at time t and zero otherwise. t is a dummy variable equal to zero before the policy change and equal to one after the change. Similarly, g_i is captures the group membership of individual i, and is equal to one if person i is in the treatment group. The term $\varepsilon_{i,t}$ is an unobservable term comprised of any random variation or omitted factors affecting employment. Additional variables may be added to this simple model to control for compositional differences of the treatment and control groups. If the identifying assumptions to be discussed below are met, then the coefficient on the group-time interaction, α_4, will reflect the impact of the policy change.

The advantage of this model is its simplicity, but it fails to incorporate the variation across the multiple time periods available in the data. To do so, the model can be extended:

$$Y_{i,t} = ß_1 + ß_2 \cdot g_i + \sum_{j=0}^{T} ß_{3,j} \cdot t_j + ß_4 \cdot I(t \geq s) + ß_5 \cdot g_i \cdot I(t \geq s) + \varepsilon_{i,t} \quad t = 0,\ldots,T \qquad (5)$$

After adding period-specific dummy variables, t_j for each time period and an indicator function $I(t \geq s) = 1$ for all periods post-intervention and zero otherwise, we can interpret $ß_5$ as the policy impact within a more flexible framework.[17]

Both DD models above rely critically upon several assumptions.[18] First, neither the treatment nor the control group should alter their behaviour in response to the policy *before* the policy becomes active, or the resulting estimate will fail to take the impact of this behaviour into account. Similarly, the control group after the policy is in place must not be affected by the policy directly or indirectly (e.g., a policy change raises employment for the treatment group, which reduces the number of vacancies available for members of the control group). Second, the policy change should not directly affect the additional control variables included in the model.[19] Third, both groups must experience similar time trends for the differencing strategy to properly isolate the policy impact. In the simple case with two time periods, this means that we expect the average change in the outcome variable experienced by the control group would have been the change experienced by the treatment group had the policy intervention not occurred. In the time dummy model, this assumption implies that the sum of the time dummy coefficients for the treatment group is equal to that of the control group. We attempt to check these assumptions in section 4.3.

3.2 Data

This analysis uses a constructed panel of Quarterly Labour Force Survey (LFS) data from the first quarter of 2006 through the first quarter of 2013.[20] The LFS surveys five cohorts to build its representative sample. Each cohort is included in the LFS for five quarters on a rolling basis and is no longer followed after the fifth quarter of inclusion. These datasets sample over 100,000 observations on a quarterly basis and, from 2006 to 2012, include approximately 120,000 lone parents in

17 The policy change takes place in period *s*

18 For a thorough overview of the methodological assumptions and additional assumptions needed for common extensions to this framework, see Lechner, Michael. 2010. "The Estimation of Causal Effects by Difference-in-Difference Methods," *Foundations and Trends in Econometrics*, Vol. 4, No. 3

19 Whether this assumption should hold depends on how 'impact' is defined in any particular case. If a policy change causes relocation by the affected groups, then the estimate of the policy change will not capture this effect (it will be reflected in the coefficients of any geographical control variables). Such a scenario would be a violation of the assumption and yet it may be perfectly reasonable to want to include such strategic behaviour

20 LFS data prior to 2006 was collected on a seasonal, rather than a calendar quarter' basis. For the sake of data consistency, we use only data from 2006 onward

total.[21] Factors covered by the survey include a broad range of personal and family characteristics that will allow us to control for many potentially important factors.

As lone fathers likely differ systematically from lone mothers, constitute only around 10% of lone parents, and represent a relatively small number of observations, the analysis below will focus on lone mothers. We restrict the age of lone mothers used to estimate the policy impact to 16–59 to reflect the definitions of working age at the time of the initial reform. In addition to controlling for region, quarter, LFS wave, and personal characteristics, we have also included local unemployment and inactivity rates.

Depending on the policy change being investigated, the time period restrictions, and the treatment and control group definitions, our sample size ranges between 5,000 and 20,000 observations.

21 For the purposes of this analysis, individuals surveyed during different quarters are treated as separate observations

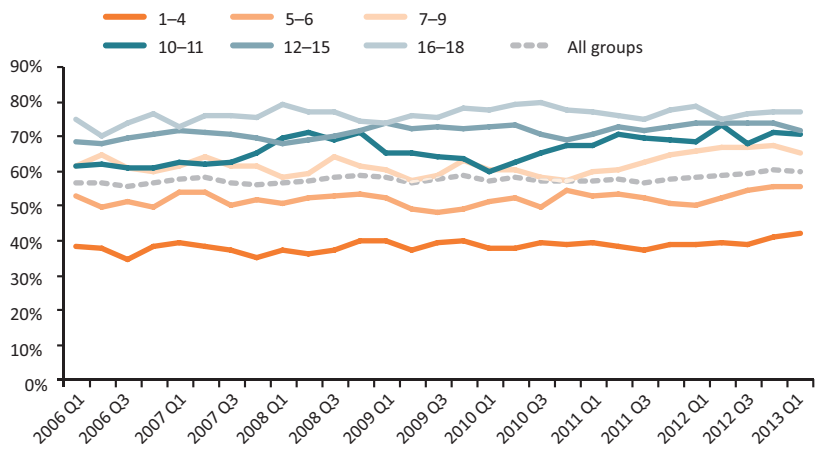
4
Analysis

4.1 Descriptive statistics

The size and quality of the LFS dataset will allow for an extensive set of parameters to be fit. One important concern arising from the use of repeated cross-sectional data is the potential for the group composition to change over time. If lone mothers of a ten year old children have different levels of education or are of different ages in 2006 and in 2013, this shift in characteristics of the group will undermine the empirical approach used below and make any meaningful interpretation of estimates recovered biased. We check along a couple important dimensions to see whether group compositions change over time. Graphs of these characteristics may be found in the section 6.1.

When group compositions are not stable over time, we expect to see changes in the heights of the bars within each group. There does not appear to be much shift in group composition over time in terms of the lone mother age, number of dependent children, or age of the youngest child.

Figure 4.1: Employment rate by lone mother group



Along another important dimension, education, there is more cause for concern. The percentage of lone mothers with degrees seems to vary in a similar fashion across time for all groups, although the standard deviation on this measure is quite large and the twofold increase in degree earners in the 10–11

group over the space of two years does not seem tenable. This instability may be an artefact of the change to the Qualification and Credits Framework (QCF) in 2008, a revision to the way foreign qualifications are treated, or the unintentional omission of 65–69 year old survey participants in the January–March 2011 dataset.[22] It is hoped that any changes in group composition over time may be at least partially absorbed by the period dummies included in the DD specifications discussed in section 3.1.

Another requirement for the DD methodology to provide robust results is choosing a good counterfactual for the treatment group. The employment rates of the various lone mother groups are graphed over time in Figure 4.1. For the most part, the trends in employment rates appear to track relatively closely in adjacent lone parent groups. Further tests will be conducted in section 4.3 to examine the extent to which this is true.

4.2 Regression analysis

As our base case, we define the treatment and control groups for each of the reductions in child age eligibility as displayed in Table 4.1. Note once again that this dataset is comprised of repeated cross-sections, and as such, the members of the treatment and control groups are generally different each period.

Table 4.1

Policy change	Treatment (age of youngest)	Control (age of youngest)	Quarters included
Under 12 (2008 Q4)	12–15	10–11	2006 Q1–2009 Q3
Under 10 (2009 Q4)	10–11	7–9	2008 Q4–2010 Q3
Under 7 (2010 Q4)	7–9	5–6	2009 Q4–2012 Q1
Under 5 (2012 Q2)	5–6	3–4	2010 Q4–2013 Q1

The sample consists of lone mothers of working age. To avoid overlap among the various reforms, each regression is limited to time periods that do not include any of the other age eligibility changes. For instance, the regression examining the IS eligibility restriction to LP with children under 10 limits the sample to observations from 2008 Q4 to 2010 Q3. In each case, we regress an employment variable taking the value of one if the individual is employed and zero otherwise within a basic difference-in-differences specification (see equation 5). This basic specification is subsequently augmented with a full set of control variables. Results are shown in Table 4.2.

None of the coefficients for the interaction variables, represented in Equation 5 by $ß_5$, for any of the reforms are significant at the 5% level in the basic specifications. In the full regressions, we generally observe the expected signs on the various controls. Health problems, having more dependent children, and higher local unemployment or inactivity rates all reduce the employment of lone mothers. Age is quadratic in employment, with employment increasing at younger ages and decreasing closer to pensioner age. Once the full set of individual characteristics and dummies are included, we see that the age 12 reform interaction coefficient is significant at the 5% level. This estimate suggests

22 Labour Force Survey User Guide, Volume 3 – Details of LFS Variables 2012, Version 4 – December 2012, pp. 183–184

that this reform raised the employment rate of the treatment group by around 3.4 percentage points relative to the control group. We see an average effect of around 3 percentage points from the under 7 reform with a p-value almost significant at the 10% level. For the under 5 and under 10 reforms, the estimates are far noisier as the large p-values associated with these values demonstrate.

Table 4.2: Impact of policy changes on lone mother employment rate

DV: employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	**5 Basic**	**5 Full**	**7 Basic**	**7 Full**	**10 Basic**	**10 Full**	**12 Basic**	**12 Full**
Treatment interaction	0.003	0.006	0.024	0.030	-0.028	-0.011	0.030	0.034**
	(0.890)	(0.766)	(0.259)	(0.115)	(0.242)	(0.617)	(0.109)	(0.032)
Post-reform dummy	0.030	0.000	0.001	0.000	0.003	0.000	0.026	-0.053*
	(0.259)	(.)	(0.980)	(.)	(0.900)	(.)	(0.266)	(0.090)
Treatment group	0.080***	0.042***	0.085***	0.022	0.057***	0.038***	0.050***	0.031***
	(0.000)	(0.001)	(0.000)	(0.103)	(0.000)	(0.003)	(0.000)	(0.000)
Linear time trend		-0.009		-0.002		-0.004		0.005**
		(0.113)		(0.574)		(0.390)		(0.015)
Has health problem		-0.213***		-0.272***		-0.368***		-0.389***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.078***		-0.087***		-0.059***		-0.049***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.913***		-0.781***		-0.833***		-0.909***
		(0.001)		(0.004)		(0.006)		(0.000)
Local inactivity rate		-0.396***		-0.338***		-0.379***		-0.220***
		(0.004)		(0.007)		(0.004)		(0.010)
Age of respondent		0.060***		0.080***		0.064***		0.069***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	8,553	8,545	8,887	8,878	7,158	7,158	17,506	17,506
R-squared	0.007	0.189	0.010	0.227	0.004	0.252	0.005	0.297

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
Region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

One potential reason for the lack of significance may be related to the fact that the estimated marginal effects are decreasing in size as the youngest child age cut-off falls. If we expect mothers with younger children to be less likely to respond to these reforms by seeking work, then the true policy effect size for lone mothers with younger children will be smaller. As effect size shrinks, larger sample sizes are needed to distinguish these effects from the noise at a statistically significant level. Unfortunately, the number of lone mothers in the LFS survey generally falls with the age of youngest child precisely when we would

hope to have larger sample sizes to work with. Even in the absence of statistical significance, the estimates below still provide weak evidence that the reforms may have raised employment rates for lone mothers with younger children.

While the analysis above represents the overall average effect on the employment rate of lone mothers, the effect may vary across subpopulations of lone mothers within each group. To get an insight into how these policy changes may have affected lone mothers at different levels of education, the above analysis is applied to lone mothers with high and low qualifications separately. For the purposes of this exercise, high qualifications is defined as equivalent to or greater than GCE, A level, five or more GSCE, or higher education. Conversely, low qualifications represent less than five GCSE A*–C, other qualifications, no qualifications, does not know or didn't answer.

Table 4.3: Impact of policy changes on employment rate of lone mothers with high qualifications

DV: employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	**5 Basic**	**5 Full**	**7 Basic**	**7 Full**	**10 Basic**	**10 Full**	**12 Basic**	**12 Full**
Treatment interaction	-0.040	-0.028	0.030	0.038	-0.023	-0.019	0.052**	0.050**
	(0.202)	(0.339)	(0.261)	(0.131)	(0.437)	(0.487)	(0.018)	(0.013)
Post-reform dummy	0.052	0.000	-0.015	0.000	0.004	0.000	-0.029	-0.063
	(0.147)	(.)	(0.641)	(.)	(0.899)	(.)	(0.288)	(0.122)
Treatment group	0.097***	0.052***	0.080***	0.037**	0.033*	0.023	0.051***	0.029***
	(0.000)	(0.002)	(0.000)	(0.040)	(0.072)	(0.193)	(0.000)	(0.002)
Linear time trend		0.003		-0.002		0.008		0.005*
		(0.751)		(0.621)		(0.249)		(0.089)
Has health problem		-0.199***		-0.245***		-0.406***		-0.370***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.104***		-0.097***		-0.069***		-0.045***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.868**		-0.265		-1.156***		-1.977***
		(0.030)		(0.458)		(0.005)		(0.000)
Local inactivity rate		-0.399**		-0.378**		-0.588***		-0.081
		(0.039)		(0.031)		(0.001)		(0.480)
Age of respondent		0.064***		0.091***		0.066***		0.068***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	4,453	4,453	4,848	4,848	3,800	3,800	8,411	8,411
R-squared	0.010	0.136	0.012	0.126	0.003	0.158	0.009	0.165

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
Region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

The results for lone mothers with high qualifications are included in Table 4.3. Despite the much smaller sample size, the effect of the 12–16 eligibility change appears to have had a particularly strong impact on the employment rate of lone mothers with higher qualifications, raising employment by approximately 5 percentage points. Once more, the estimates for the impact of the other reforms are not significantly different from zero. It is worth noting that the estimate for the 7–10 eligibility change is almost significant at the 10% level and also appears to reflect a larger impact on the employment rate of the affected lone mothers with high qualifications.

For lone mothers with low qualifications, the reforms to IS eligibility do not appear to have had much of an impact on employment. As Table 4.4 illustrates, none of the coefficients on the interaction term for the full regressions are

Table 4.4: Impact of policy changes on employment rate of lone mothers with low qualifications

DV: employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	**5 Basic**	**5 Full**	**7 Basic**	**7 Full**	**10 Basic**	**10 Full**	**12 Basic**	**12 Full**
Treatment interaction	0.060*	0.046	0.018	0.017	0.016	0.000	0.030	0.021
	(0.071)	(0.146)	(0.564)	(0.559)	(0.655)	(0.992)	(0.292)	(0.400)
Post-reform dummy	-0.017	0.000	-0.014	0.000	-0.036	0.000	0.042	-0.041
	(0.650)	(.)	(0.712)	(.)	(0.364)	(.)	(0.221)	(0.380)
Treatment group	0.056***	0.037**	0.043**	0.011	0.074***	0.054***	0.049***	0.032***
	(0.001)	(0.029)	(0.044)	(0.598)	(0.001)	(0.006)	(0.000)	(0.003)
Linear time trend		-0.003		-0.001		-0.010		0.005*
		(0.540)		(0.784)		(0.238)		(0.097)
Has health problem		-0.220***		-0.293***		-0.338***		-0.397***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.060***		-0.076***		-0.051***		-0.052***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.968**		-1.346***		-0.503		-0.024
		(0.013)		(0.001)		(0.261)		(0.940)
Local inactivity rate		-0.473**		-0.219		-0.197		-0.356***
		(0.014)		(0.227)		(0.310)		(0.005)
Age of respondent		0.051***		0.067***		0.058***		0.069***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	4,100	4,092	4,039	4,030	3,358	3,358	9,095	9,095
R-squared	0.007	0.113	0.003	0.172	0.010	0.241	0.005	0.284

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%

Region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

statistically significant, suggesting that once other factors are controlled for, the employment rates of lone mothers with low qualifications did not change as compared to those unaffected by the policy changes.

It appears that lone mothers with high qualifications are more likely to respond to the additional cost of maintaining eligibility by seeking and obtaining employment. While unobserved heterogeneity, such as average differences in ability or motivation across groups, is potentially affecting the results, it is also likely that the lack of response to the eligibility changes for lone mothers with low qualifications arises from the challenges this group faces in securing employment. In other words, these women may well be seeking employment but having difficulty securing it.

In summary, there is evidence that the eligibility changes have increased the employment rate of lone mothers whose youngest child is 12–16 by over 3 percentage points and weaker evidence in favour of a similarly sized effect for lone mothers whose youngest child is 7–10. These average effects appear to be driven by the increases in employment by lone mothers with high qualifications and there is no evidence that the employment rate of lone mothers with low qualifications changed in response to the policy changes relative to control groups once a full set of individual characteristics are taken into account.

4.3 Robustness checks

While the results presented in the last section are encouraging, the validity of the results rest upon the identifying assumptions discussed at the end of section 3.1. These three assumptions are:

1. Treatment should not affect either group pre-treatment, nor the control group post-treatment
2. In the absence of treatment, the treatment group should experience the same trend as the control group
3. Non-treatment covariates should be uncorrelated with treatment

Some evidence suggests that one common violation of assumption 1, anticipation of upcoming policy changes, may lead to substantial downward bias in impact estimates for specifications that do not model anticipation effects directly. Blundell, Francesconi, & van der Klaauw (2011) reported a downward bias in the range of 15–35% arising from anticipation of the Working Families' Tax Credit reform.[23]

4.3.1 Anticipation effects

There is no evidence of endogeneity between the policy changes and the pre-reform groups or post-reform control groups. Specifically, we find that the results in section 4.2 are robust to anticipation effects arising from the quarterly WFIs preceding loss of IS eligibility, and we find no evidence of anticipation effects in the pre-reform treatment or post-reform control groups.

WFI

There are sensible reasons to expect anticipation effects on a subset of the control group. The increased frequency of mandatory quarterly interviews lone

23 Blundell, Richard, Marco Francesconi, and Wilbert van der Klaauw. 2011. "Anatomy of Welfare Reform Evaluation: Announcement and Implementation Effects." Discussion Paper series, Forschungsinstitut zur Zukunft der Arbeit. www.econstor.eu/handle/10419/58817

parents attend the year up to the loss of IS eligibility may affect lone parents in the control group by encouraging greater job search behaviour and increasing employment. If such an effect exists, we would expect the estimates in Table 4.2 to underestimate the effect of the change to IS eligibility. To check whether our results are robust to the potentially biasing effects of quarterly WFIs, we run a difference-in-differences specification with the full set of control variables on employment using modified control groups that omit lone mothers subject to quarterly WFIs, i.e. with youngest child a year below the age eligibility threshold. The treatment and control groups are displayed in Table 4.5.[24]

While all coefficients on the impact variable are positive, only the coefficient for the under 12 reform is statistically significant (1% level). This is consistent with the findings in Table 4.2. Given the larger magnitude of the under 12 reform interaction variable coefficient in the present regression, there is some evidence that the earlier estimate may suffer from a downward bias due to increased employment in the control group as a result of the increased frequency of WFI in the year before JSA.

Table 4.5

Policy change	Treatment (age of youngest)	Control (age of youngest)	Quarters included
Under 12 (2008 Q4)	12–15	9–10	2006 Q1–2009 Q3
Under 10 (2009 Q4)	10–11	7–8	2008 Q4–2010 Q3
Under 7 (2010 Q4)	7–9	4–5	2009 Q4–2012 Q1
Under 5 (2012 Q2)	5–6	2–3	2010 Q4–2013 Q1

Pre-reform treatment group

Pre-reform behavioural change in the lone mother treatment groups would also bias the policy impact estimates. To examine whether the lone mother treatment groups exhibit anticipation effects, the treatment and control groups are designated in the same way as in Table 4.1 but with treatment periods defined to include only the year preceding the age eligibility change in question. If the interaction variable is significantly different from zero, then this will indicate that the treatment group increased their employment relative to the control group in the year preceding the implementation of the reform and point to anticipatory behaviour in the treatment group. Group definitions and time periods included are displayed in Table 4.6.[25]

Table 4.6

Policy change	Treatment (age of youngest)	Control (age of youngest)	Quarters included
WFI 12 (2007 Q4)	12–15	10–11	2006 Q1–2008 Q3
WFI 10 (2008 Q4)	10–11	7–9	2007 Q4–2009 Q3
WFI 7 (2009 Q4)	7–9	5–6	2008 Q4–2010 Q3
WFI 5 (2011 Q2)	5–6	3–4	2009 Q4–2012 Q1

24 Output from this regression may be found on page 25

25 Results available on page 26

The only notable result is a DD estimate of -5.5% significant at the 1% level for the Under 12 reform in a regression with a full set of controls. Caution is warranted before concluding that this reflects a negative anticipation effect in the treatment group, however. Visual inspection of the group employment rates over this period (i.e., 2006 Q1–2008 Q3) reveals a surge in the employment rate of lone mothers belonging to the 10–11 group relative to the levels of any of the other lone mother groups. In fact, the employment rate for this group rises almost nine percentage points over the course of a year, which points to a potential issue around the data that may be driving the strong negative DD estimate.

If this estimate is truly driven by anticipation in the 12–16 lone mother group, we would expect to retain this effect even for other choices of controls. We re-run the above regression adjusting only the control groups, defining them as per Table 5 to include only mothers whose youngest child is over a year younger than the age eligibility threshold.[26] None of the DD estimates are statistically significant at the 10% level, suggesting that the previous result is not an anticipation effect of the treatment group but rather an artefact of the data.

Post-reform control group

The assumption of no anticipation effects also requires that the control group be unaffected directly by the intervention affecting the treatment group in the post-treatment period. We check whether these policy changes impact the control groups by designating the control groups in Table 1 as the treatment group and using lone mothers with younger children as the new control groups. Table 4.7 displays how each group has been defined.

Table 4.7

Policy change	Treatment (age of youngest)	Control (age of youngest)	Quarters included
Under 12 (2008 Q4)	10–11	7–9	2006 Q1–2009 Q3
Under 10 (2009 Q4)	7–9	5–6	2008 Q4–2010 Q3
Under 7 (2010 Q4)	5–6	3–4	2009 Q4–2012 Q1
Under 5 (2012 Q2)	3–4	1–2	2010 Q4–2013 Q1

None of the interaction terms are significantly different from zero and thus there is no evidence of anticipation in the post-reform control groups.[27]

4.3.2 Common trends

While the common trends assumption is critical for identification using the difference-in-differences methodology, it is impossible to test directly in practice. To do so would require that the treatment group trends and control group trends be compared in the absence of any policy changes, and such counterfactuals do not exist. Examining the trends for treatment and control groups over an interval of time preceding the policy change, however, can provide some indication of whether the assumption is likely to be met in practice. The validity of this exercise

26 Results available on page 27

27 Results available on page 27

requires, of course, that trends in this earlier period may be reasonably expected to be similar to those around the time of the policy change in question.

The common trends assumption is tested using data from 2006 calendar year. We find no evidence of differential time trends during this period, suggesting that the common trends assumption holds that year and provides some suggestive evidence that the common trends assumption may be a reasonable assumption.[28]

4.3.3 Exogeneity

The analysis also rests upon the assumption that none of the control variables introduced in the full specification are correlated with the treatment variable. Such endogeneity would cause a portion of the variation accounted for by the treatment to be incorporated into the coefficient of the endogenous variable and would bias the estimate of the policy impact.

For most of the variables, exogeneity is not likely to be an issue. Age, sex, region, survey wave, ethnicity, and the majority of the other controls either cannot or are unlikely to be affected by behavioural change in response to the policy changes. One variable that could potentially be problematic is the variable accounting for self-reported health problems, particularly as lone parents might respond to the policy changes by reporting poorer health in order to move onto disability benefits. We regress this health problem dummy on period dummies, treatment groups, post-treatment indicator dummy, policy interaction variable and control variables separately for each reform to see whether this difference-in-difference specification picks up any conditional correlation between health problems and the policy change in the treatment group. We do not observe any increase in self-reported health problems in response to any of the reforms.[29]

4.3.4 Probit specification

The dependent variables used in all cases are binary dummy variables and such an outcome variable may create problems for linear probability models. For instance, a linear model may produce coefficients that lead to impossible outcome variable values (e.g., below zero or above one). A commonly used alternative model that does not suffer from these issues is the probit model.

To check whether the results above are sensitive to the model employed, we rerun the first regression in section 4.2 using a probit model with and without controls, and find that the estimates are similar, if generally larger in magnitude.[30] These results suggest that the use of the linear probability model in the analyses of the previous section yield relatively conservative policy impact estimates. The linear model is retained in consideration of the issues around the use of probit models within a difference-in-differences framework and the difficulty of interpretation of interacted variable coefficients.[31,32]

4.3.5 Serial correlation

The largest limitation to the empirical approach utilised in this paper is the likely presence of serial correlation in the dependent variable, employment. Many labour market variables, employment included, display persistence over time and while such positive serial correlation does not bias the point estimates, it does render the standard errors associated with the estimates invalid. Bertrand, Duflo, and Mullainathan (2004) have documented the extent to which positive serial

28 Results available on page 28

29 Results on page 29

30 Results on page 30

31 For a discussion of limitations of probit-based difference-in-differences models, see Lechner (2010), op. cit

32 Norton, Edward C., Hua Wang, and Chunrong Ai. 2004. "Computing Interaction Effects and Standard Errors in Logit and Probit Models." Stata Journal 4: 154–167

correlation can raise the frequency of Type I error and likelihood of arriving at a spurious statistically significant result.[33]

One relatively simple solution Bertrand et al. (2004) found to perform well in the presence of serial correlation is to omit the time dimension of the data altogether, effectively eliminating the source of serial correlation. However, since the data used here is a repeated cross-section rather than a panel dataset, the employment dummy variable cannot be averaged into pre- and post-treatment figures within individuals. As an alternative, we remove the time dimension by aggregating periods into two pre- and post-change periods. Using this formulation, we run the DD specification according to Equation 4 to arrive at unbiased standard errors on the DD estimate.

The policy impact coefficients and standard errors are nearly identical to the coefficients found in section 4.2.[34] Of the four policy changes, only the age 12 reform had a statistically significant increase in employment. It appears that any serial correlation in the dependent variable does not appear to affect the standard errors and, in light of this, the statistical significance of the under 12 impact estimate appears valid.

4.3.6 Spurious policy periods

An additional check on the methodological approach repeats the analysis of section 4.2 using arbitrarily chosen policy change periods. Since these periods do not include the policy reforms directly affecting the treatment and control groups, the interaction term coefficients are not expected to be significantly different from zero. A significant interaction coefficient would raise questions about the methodology or indicate the presence of other time-related factors (e.g., other policy enactments). Treatment and control groups are once again defined according to Table 4.1. Spurious policy periods are chosen such that the periods used do not overlap with the actual reform affecting that lone mother group. Each regression includes the four quarters preceding and following each spurious policy change.[35]

The interaction terms are not significantly different from zero except for the 10–12 lone mother group in Q3 2007 and 5–7 group in Q1 2007. The former once again reflects the spike in the employment rate documented in 4.3.1 and, while still concerning, may have more to do with data problems than providing evidence against the methodology. The significant coefficient for the 5–7 group is more worrying. It is possible that this result reflects some other change, policy or otherwise, affecting this lone mother group or the 3–5 group (i.e., the control group). It appears that the methodological approach used in this paper may not be appropriate for these particular lone mother groups and that the policy impacts estimated earlier may not reflect the true effects of the reforms.

33 Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-differences Estimates?" The Quarterly Journal of Economics 119, no. 1: 249–275

34 Results on page 31

35 Results are available on page 32

5 Discussion

The analyses conducted here estimate a roughly 3 percentage point increase in employment in response to the Income Support age eligibility reduction for lone mothers with youngest child aged 12–16, with much of the change in employment driven by lone mothers with high qualifications. There is weak evidence pointing to a similarly sized effect for lone mothers with youngest child aged 7–10 less, likewise driven by lone mothers with high qualifications, although this result is not statistically significant at the 10% level.

It should be noted that it may not be surprising that statistically significant results were not found in the case of the 5–7 and 10–12 groups. The spurious policy period robustness check found potential problems with the methodology when applied to these groups and hence the policy impact estimates may not reflect the actual impacts. Even if this were not the case, the impact analysis of the 10–12 policy change suffered from substantially smaller sample sizes than for the other reforms. Two factors contributed to this. First, this specification had the fewest periods to work with since it took place exactly one year between the 12–16 and 7–10 eligibility changes. Second, the policy affected mothers with children aged either ten or eleven. Hence the combination of fewer included periods and more narrowly defined child ages resulted in a small sample size.

There is good reason to believe that mothers caring for younger children are less responsive to marginal changes in work incentives and as a result, it is reasonable to expect that the effect size of the eligibility reforms falls as the age of youngest child decreases. This means that to recover a statistically significant estimate of the policy effect requires greater statistical power, e.g. a larger sample size.

We also do not observe statistically significant impacts of the reform among lone mothers with low levels of qualifications. However, whilst the coefficients tend to be smaller they are still positive, and it is possible that with a greater sample size, we might have the precision needed to identify these small effects. What is clear, however, is that the discrepancy in the responses of the low skill and high skill subgroups underscores the importance of supporting lone mothers with skill-related barriers to work.

Finally, we must recognise that the approach used in this paper can only identify short-term changes in employment rates resulting from the changes in policy. There will likely be a greater effect on employment over a longer time horizon as the lone mothers and the labour market adjust to these new incentives.

On the basis of the evidence presented here, we believe that the changes to child age eligibility for IS have probably had a positive impact on employment

for lone mothers with a magnitude of around 3 percentage points, with larger employment gains among high skilled lone mothers and potentially smaller gains for mothers of young children and lone mothers with lower levels of qualifications.

6
Appendix

6.1 Stability in group characteristics

Figure 6.1: Age of mother by group over time

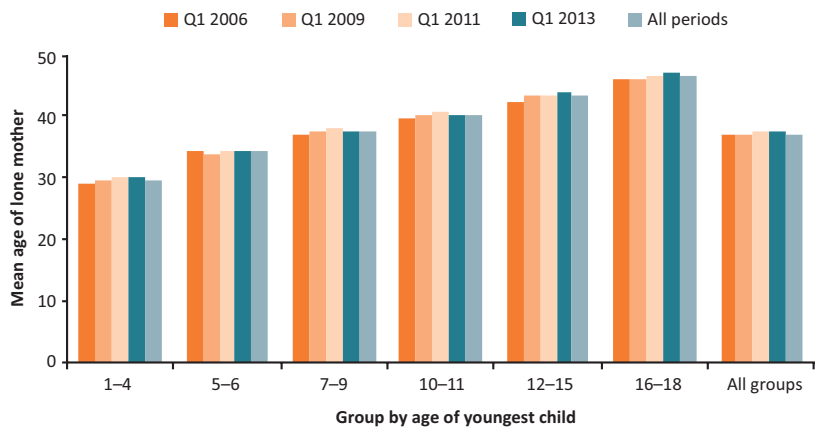


Figure 6.2: Age of youngest child by group over time

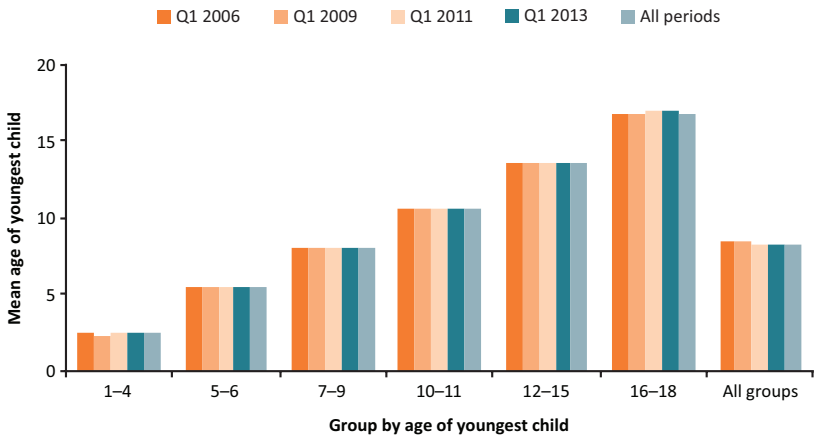


Figure 6.3: Number of dependent children by group over time

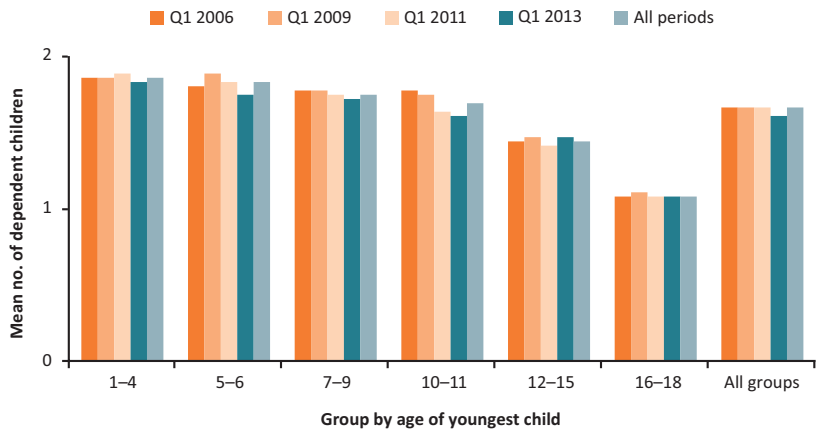
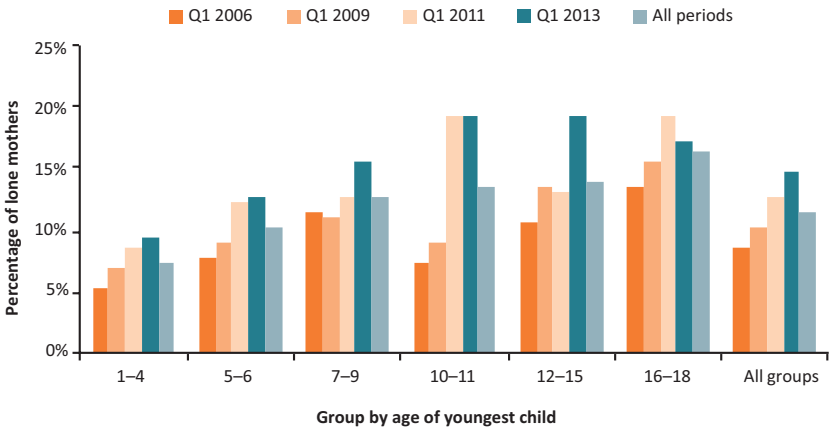


Figure 6.4: Percentage of mothers with a degree by group over time



6.2 Anticipation effects

6.2.1 Robustness to WFI effects in control group

DV: Employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Basic 5	Full 5	Basic 7	Full 7	Basic 10	Full 10	Basic 12	Full 12
Treatment interaction	0.011	0.016	0.010	0.020	-0.012	0.001	0.058***	0.061***
	(0.623)	(0.440)	(0.631)	(0.275)	(0.636)	(0.961)	(0.002)	(0.000)
Post-reform dummy	0.019	0.000	0.006	0.000	-0.016	0.000	0.008	-0.056***
	(0.463)	(.)	(0.809)	(.)	(0.579)	(.)	(0.738)	(0.010)
Treatment group	0.129***	0.059***	0.140***	0.051***	0.061***	0.035**	0.062***	0.032***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.017)	(0.000)	(0.000)
Linear time trend		-0.013**		-0.001		-0.007		0.003
		(0.027)		(0.733)		(0.184)		(0.103)
Has health problem		-0.216***		-0.267***		-0.379***		-0.378***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.085***		-0.093***		-0.056***		-0.049***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.576**		-0.407		-0.968***		-0.965***
		(0.032)		(0.123)		(0.004)		(0.000)
Local inactivity rate		-0.311**		-0.359***		-0.358**		-0.293***
		(0.019)		(0.003)		(0.015)		(0.001)
Age of respondent		0.062***		0.078***		0.056***		0.066***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	8,897	8,886	9,313	9,304	5,773	5,773	17,446	17,446
R-squared	0.018	0.200	0.023	0.232	0.005	0.258	0.007	0.289

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
A linear time trend and region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

6.2.2 Anticipation effects in pre-reform treatment group

DV: Employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Basic 5	Full 5	Basic 7	Full 7	Basic 10	Full 10	Basic 12	Full 12
Treatment interaction	-0.022	-0.022	-0.028	-0.019	-0.023	-0.018	-0.086***	-0.055***
	(0.354)	(0.298)	(0.187)	(0.312)	(0.332)	(0.383)	(0.000)	(0.001)
Post-reform dummy	0.013	0.000	0.015	0.000	-0.040*	0.000	0.093***	0.000
	(0.619)	(.)	(0.567)	(.)	(0.099)	(.)	(0.000)	(.)
Treatment group	0.085***	0.045***	0.099***	0.039***	0.082***	0.058***	0.077***	0.044***
	(0.000)	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)
Linear time trend		0.001		0.020***		-0.007		0.005**
		(0.747)		(0.000)		(0.219)		(0.042)
Has health problem		-0.216***		-0.285***		-0.362***		-0.391***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.084***		-0.077***		-0.049***		-0.058***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.676**		-1.198***		-0.745**		-0.787***
		(0.015)		(0.000)		(0.026)		(0.004)
Local inactivity rate		-0.321**		-0.451***		-0.445***		-0.269***
		(0.013)		(0.000)		(0.001)		(0.009)
Age of respondent		0.064***		0.072***		0.054***		0.073***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	8,475	8,471	9,163	9,163	7,726	7,726	13,001	13,001
R-squared	0.006	0.192	0.008	0.232	0.007	0.238	0.005	0.300

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%

A linear time trend and region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

6.2.3 Anticipation effects in pre-reform treatment group with adjusted control groups

DV: Employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	basic5	full5	basic7	full7	basic10	full10	basic12	full12
Treatment interaction	-0.003	-0.012	-0.024	-0.014	-0.019	-0.018	-0.039**	-0.022
	(0.897)	(0.571)	(0.248)	(0.430)	(0.456)	(0.430)	(0.048)	(0.185)
Post-reform dummy	0.014	0.000	0.009	0.000	-0.047*	0.000	0.059**	0.000
	(0.578)	(.)	(0.712)	(.)	(0.096)	(.)	(0.012)	(.)
Treatment group	0.119***	0.052***	0.157***	0.072***	0.089***	0.059***	0.072***	0.031***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Linear time trend		-0.001		0.022***		0.011**		0.005*
		(0.790)		(0.000)		(0.041)		(0.069)
Has health problem		-0.205***		-0.280***		-0.363***		-0.378***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.088***		-0.082***		-0.051***		-0.055***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.667**		-0.882***		-0.706*		-0.742***
		(0.013)		(0.001)		(0.057)		(0.007)
Local inactivity rate		-0.369***		-0.395***		-0.377**		-0.345***
		(0.003)		(0.001)		(0.014)		(0.001)
Age of respondent		0.064***		0.075***		0.056***		0.071***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	8,877	8,873	9,511	9,511	6,208	6,208	12,929	12,929
R-squared	0.014	0.197	0.022	0.233	0.010	0.243	0.005	0.290

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
A linear time trend and region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

6.2.4 Anticipation effects in post-reform control group

DV: Employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Basic 5	Full 5	Basic 7	Full 7	Basic 10	Full 10	Basic 12	Full 12
Treatment interaction	0.019	0.029	-0.003	-0.012	-0.013	-0.017	0.027	0.020
	(0.376)	(0.130)	(0.883)	(0.538)	(0.574)	(0.414)	(0.194)	(0.281)
Post-reform dummy	0.008	0.000	0.017	0.000	0.008	-0.001	-0.016	-0.055
	(0.750)	(.)	(0.525)	(.)	(0.763)	(0.965)	(0.508)	(0.113)
Treatment group	0.103***	0.030***	0.080***	0.044***	0.099***	0.039***	0.032***	0.011
	(0.000)	(0.005)	(0.000)	(0.002)	(0.000)	(0.003)	(0.000)	(0.184)
Linear time trend		-0.010*		0.000		0.000		0.004*
		(0.077)		(0.891)		(0.927)		(0.072)
Has health problem		-0.168***		-0.216***		-0.293***		-0.356***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.091***		-0.084***		-0.074***		-0.059***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.338		-0.678**		-1.128***		-0.667***
		(0.170)		(0.014)		(0.000)		(0.008)
Local inactivity rate		-0.463***		-0.320**		-0.369***		-0.496***
		(0.000)		(0.013)		(0.006)		(0.000)
Age of respondent		0.063***		0.064***		0.069***		0.063***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	10,363	10,343	8,475	8,471	7,409	7,409	14,930	14,930
R-squared	0.013	0.188	0.006	0.192	0.010	0.238	0.003	0.253

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%

A linear time trend and region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

6.3 Common trends

DV: Employment	(1)	(2)	(3)	(4)
VARIABLES	Full 5	Full 7	Full 10	Full 12
Difference in time trends	0.010	-0.003	0.000	0.001
	(0.458)	(0.834)	(0.992)	(0.946)
Linear time trend	-0.010	-0.001	-0.003	-0.001
	(0.288)	(0.885)	(0.711)	(0.890)
Treatment (5–7)	-0.029			
	(0.418)			
Treatment (7–10)		0.075**		
		(0.023)		
Treatment (10–12)			-0.027	
			(0.410)	
Treatment (12–16)				0.050*
				(0.092)
Has health problem	-0.255***	-0.263***	-0.341***	-0.397***
	(0.000)	(0.000)	(0.000)	(0.000)
No. dep. children < 19	-0.093***	-0.087***	-0.069***	-0.056***
	(0.000)	(0.000)	(0.000)	(0.000)
Local unemployment rate	-0.959	-0.296	-0.842	-1.100**
	(0.106)	(0.581)	(0.124)	(0.017)
Local inactivity rate	-0.710***	-0.789***	-0.693***	-0.269
	(0.001)	(0.000)	(0.000)	(0.102)
Age of respondent	0.083***	0.086***	0.077***	0.069***
	(0.000)	(0.000)	(0.000)	(0.000)
Age squared	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	3,578	4,267	4,112	4,747
R-squared	0.232	0.249	0.279	0.330

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
An intercept term, a linear time trend, and dummies for period, region, wave, seasonal, ethnicity, and education are included in all regressions

6.4 Exogeneity of self-reported health problems

DV: Health Problem	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Basic 5	Full 5	Basic 7	Full 7	Basic 10	Full 10	Basic 12	Full 12
Treatment interaction	0.026	0.022	0.003	-0.008	0.009	-0.007	0.014	0.008
	(0.135)	(0.185)	(0.853)	(0.633)	(0.643)	(0.710)	(0.377)	(0.610)
Post-reform dummy	-0.008	0.000	-0.026	0.000	0.026	0.000	-0.009	-0.004
	(0.679)	(.)	(0.206)	(.)	(0.188)	(.)	(0.634)	(0.900)
Treatment group	0.026***	0.014	0.020*	0.007	0.003	-0.012	0.029***	0.015**
	(0.005)	(0.148)	(0.083)	(0.540)	(0.770)	(0.309)	(0.000)	(0.032)
Linear time trend		0.003		0.004		0.005		0.001
		(0.579)		(0.147)		(0.245)		(0.761)
No. dep. children < 19		0.011***		-0.001		-0.006		-0.009*
		(0.009)		(0.778)		(0.289)		(0.051)
Local unemployment rate		-0.328		0.192		-0.171		-0.047
		(0.130)		(0.408)		(0.528)		(0.828)
Local inactivity rate		-0.141		0.165		0.228*		0.283***
		(0.182)		(0.127)		(0.056)		(0.001)
Age of respondent		0.001		0.019***		-0.002		0.001
		(0.767)		(0.000)		(0.762)		(0.891)
Age squared		0.000		-0.000**		0.000		0.000
		(0.270)		(0.025)		(0.188)		(0.438)
Observations	8,553	8,545	8,887	8,878	7,158	7,158	17,506	17,506
R-squared	0.003	0.045	0.002	0.058	0.002	0.047	0.002	0.057

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
A linear time trend and region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8). A constant term and period dummies are included in all regressions

6.5 Probit

DV: Employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Linear 5	Probit 5	Linear 7	Probit 7	Linear 10	Probit 10	Linear 12	Probit 12
Treatment interaction	0.006	0.004	0.030	0.033	-0.011	-0.013	0.034**	0.044**
	(0.766)	(0.884)	(0.115)	(0.151)	(0.617)	(0.625)	(0.032)	(0.023)
Post-reform dummy	0.000		0.000		0.000		-0.053*	-0.030
	(.)		(.)		(.)		(0.090)	(0.413)
Treatment group	0.042***	0.052***	0.022	0.027	0.038***	0.047***	0.031***	0.037***
	(0.001)	(0.000)	(0.103)	(0.104)	(0.003)	(0.004)	(0.000)	(0.000)
Linear time trend	-0.009	-0.013*	-0.002	-0.001	-0.004	-0.005	0.005**	0.003
	(0.113)	(0.082)	(0.574)	(0.748)	(0.390)	(0.432)	(0.015)	(0.392)
Has health problem	-0.213***	-0.240***	-0.272***	-0.319***	-0.368***	-0.420***	-0.389***	-0.442***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. dep. children < 19	-0.078***	-0.094***	-0.087***	-0.106***	-0.059***	-0.073***	-0.049***	-0.061***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Local unemployment rate	-0.913***	-1.074***	-0.781***	-0.924***	-0.833***	-1.075***	-0.909***	-1.260***
	(0.001)	(0.001)	(0.004)	(0.005)	(0.006)	(0.004)	(0.000)	(0.000)
Local inactivity rate	-0.396***	-0.467***	-0.338***	-0.392**	-0.379***	-0.476***	-0.220***	-0.265**
	(0.004)	(0.004)	(0.007)	(0.011)	(0.004)	(0.004)	(0.010)	(0.015)
Age of respondent	0.060***	0.068***	0.080***	0.096***	0.064***	0.078***	0.069***	0.081***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age squared	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	8,545	8,545	8,878	8,878	7,158	7,158	17,506	17,506
R-squared	0.189		0.227		0.252		0.297	

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
An intercept term, a linear time trend, and dummies for period, region, wave, seasonal, ethnicity, and education are included in all regressions

6.6 Serial correlation robustness check

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	**Basic 5**	**Full 5**	**Basic 7**	**Full 7**	**Basic 10**	**Full 10**	**Basic 12**	**Full 12**
Treatment interaction	0.004	0.008	0.024	0.031	-0.029	-0.011	0.031	0.034**
	(0.872)	(0.727)	(0.258)	(0.104)	(0.236)	(0.616)	(0.106)	(0.031)
Post-reform dummy	0.026*	0.011	0.006	-0.005	-0.006	0.001	0.000	-0.004
	(0.097)	(0.452)	(0.712)	(0.742)	(0.702)	(0.965)	(0.987)	(0.796)
Treatment group	0.080***	0.041***	0.085***	0.022*	0.057***	0.038***	0.050***	0.030***
	(0.000)	(0.001)	(0.000)	(0.099)	(0.000)	(0.003)	(0.000)	(0.000)
Has health problem		-0.214***		-0.272***		-0.368***		-0.389***
		(0.000)		(0.000)		(0.000)		(0.000)
No. dep. children < 19		-0.078***		-0.087***		-0.059***		-0.049***
		(0.000)		(0.000)		(0.000)		(0.000)
Local unemployment rate		-0.936***		-0.768***		-0.883***		-0.825***
		(0.001)		(0.004)		(0.003)		(0.000)
Local inactivity rate		-0.378***		-0.323***		-0.393***		-0.229***
		(0.004)		(0.009)		(0.003)		(0.007)
Age of respondent		0.059***		0.080***		0.064***		0.069***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared		-0.001***		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	8,553	8,545	8,887	8,878	7,158	7,158	17,506	17,506
R-squared	0.007	0.188	0.010	0.225	0.003	0.252	0.004	0.296

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
A linear time trend and region, wave, seasonal, ethnicity, and education dummies are included in (2), (4), (6), and (8).A constant term and period dummies are included in all regressions

6.7 Spurious policy change periods

DV: Employment	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Reform	**5**	**5**	**5**	**7**	**7**	**7**	**10**	**10**	**12**	**12**
Spurious period	**jm07**	**aj08**	**js09**	**jm07**	**aj08**	**od08**	**js07**	**jm12**	**jm07**	**js11**
Treatment interaction	0.049**	-0.001	-0.012	-0.006	-0.009	-0.019	0.059***	-0.023	-0.016	-0.003
	(0.025)	(0.972)	(0.601)	(0.771)	(0.646)	(0.360)	(0.002)	(0.293)	(0.349)	(0.866)
Observations	7,207	7,121	6,875	8,399	7,995	7,832	8,270	6,524	9,523	8,039
R-squared	0.216	0.211	0.222	0.248	0.231	0.234	0.267	0.248	0.312	0.267

Note: p-values included in parentheses; *: p < 10%, **: p < 5%, ***: p < 1%
An intercept term, a linear time trend, number of dependent children under 19, local unemployment rate, local inactivity rate, age, age squared, and dummies for period, post-reform period, treatment, health problem, region, wave, seasonal, ethnicity, and education are included in all regressions