Final Report

Activation and Welfare Dependency

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Executive Summary

The Welfare to Work reforms of 2006 introduced new measures and tightened eligibility requirements for various groups of Income Support (IS) recipients, including new claimant principal carer parents, with the aim of increasing workforce participation and reducing welfare dependence amongst these groups. But principal carer parents already in receipt of Parenting Payment (PP) at 30 June 2006 – around 600,000 individuals – were given a one year exemption from the new participation requirements in order to avoid too abrupt a change to the system. These 'grandfathered' PP recipients were not covered by the new eligibility requirements until at least one year later (from 1st July 2007). Since then parents from this grandfathered group with a youngest child aged seven years or older have been required to meet the new participation conditions – primarily a requirement to engage in 15 hours per week of paid work or work-related activity – in order to remain eligible for PP.

This report uses detailed administrative and survey data on tens of thousands of grandfathered low income parents to assess the impact of these new participation requirements on the probability of exiting PP; on the probability of exiting PP to other IS payments; on the probability of exiting IS altogether; on the probability of participation in paid employment; on hours worked for those participating in paid employment; and on the probability of participation in job search.

The evidence presented here shows that this second round of Welfare to Work reforms in 2007 led to an increase in exits from Parenting Payment and a further reduction in Parenting Payment caseload on top of that caused by the first round of reforms in 2006.

Following activation, a typical grandfathered low income parent was:

- more likely to exit Parenting Payment;
- more likely to switch from Parenting Payment to another Income Support payment;
- more likely to exit Income Support altogether;
- more likely to enter paid employment if remaining on Parenting Payment;
- more likely to increase working hours to 15 hours per week or more if already working and if remaining on Parenting Payment;
- perhaps more likely to engage in job-search or search-related activities (the evidence is somewhat mixed on job search impacts).

For those outcomes that can be separately estimated by payment type, the impact of activation has generally been larger for those in receipt of Parenting Payment Partnered (PPP) than for those in receipt of Parenting Payment Single (PPS). For exits from IS, this may reflect tighter income restrictions for PPP eligibility and the opportunity, not open to PPS recipients, of responding to activation requirements by exiting IS and compensating for the loss in household income by increasing partner earnings. For employment and job search, it may reflect the greater potential for PPP recipients to call on a partner to provide childcare while they engage in labour market activity.

These impacts are broadly in line with those estimated for the earlier round of Welfare to Work reforms for new entrant low income parents presented in DEEWR's 2008 in-house evaluation report. They also compare favourably in terms of magnitude with estimates of the impacts of recent Welfare to Work reforms for low income parents in some other OECD countries. This is likely to reflect both the nature of the reforms – they represent a substantial increase in conditionality – and the strong labour market context in which they have been introduced.

Welfare to Work appears to be working for low income parents, at least in terms of its stated aims of reducing welfare dependence and increasing workforce participation. More restrictive PP entry requirements for new claimants following the 2006 reforms and higher exit rates for existing claimants following the 2007 round of reforms have both contributed to the fall in the PP caseload.

There are of course various caveats to this positive conclusion. Firstly, the scope of this evaluation has been limited to examining impacts on benefit claiming and workforce participation; ultimately Welfare to Work needs to be judged by its impact on the welfare of low income families, whether measured by income, wellbeing or some other construct, and on its benefits net of its costs. Second, and more specifically, part of the impact on the grandfathered group of low income parents – some estimates suggest around one half – has been driven by benefit shift from PP to other IS payments. Although this sorting effect might serve to help some low income parents onto more suitable benefits, there is a cost in terms of increased caseloads on other IS payments, including less 'active' IS payments such as Disability Support Pension.

1. Introduction

One concern with means-tested social welfare payments for low income families with young children is that they can reduce incentives for low income parents to participate in the labour market, potentially leading to long episodes of welfare dependence, depreciation of human capital, and exacerbating rather than alleviating poverty. Policy makers across much of the OECD have responded to this concern by reforming programs to encourage or compel welfare recipient parents of all but the youngest children to either re-enter the labour market or to engage in activities aimed at maintaining or improving their 'employability' (e.g. Carcillo and Grubb, 2006). Australia is no exception.

In Australia the main payment of this kind – paid under the umbrella of Income Support (IS) – is known as Parenting Payment (PP), either Parenting Payment Single (PPS) for lone parents, or Parenting Payment Partnered (PPP) for partnered parents where the combined household income falls below a given threshold. Prior to 2006, receipt of PP for those with a youngest child under 13 years of age was conditioned only on attendance at an annual interview with a Centrelink advisor. From 1st July 2006, however, the Welfare to Work (WtW) reform package introduced more substantial part-time participation requirements – primarily a requirement to engage in 15 hours per week of paid work or work-related activity – for low income parents of school-aged children making new applications for PP (new entrants). Low income parents of school-aged children already in receipt of PP on 30 June 2006 – the stock of existing recipients – were not subject to the new participation requirements until after 30 June 2007. This report examines the impact of this 'second round' of WtW reforms on exit rates from PP for this grandfathered cohort. It is the first study to do so, and as such it makes a significant contribution to the body of evidence on the impacts of welfare reform in Australia.

The report also draws on the wider international evaluation literature on reforms introducing activation requirements for low income welfare-recipient (generally lone) parents (see e.g. Blank, 2002; Carcillo and Grubb, 2006; Finn and Gloster, 2010; Immervoll, 2010). This wider literature suggests that activation measures for low income parents have met with mixed success, partly depending on differences in the nature of the reforms, the target group, and in institutional and labour market contexts. For example, a 1996 package of welfare reforms for lone parents in the US led to a significant reduction in caseload, whereas contemporaneous lone parent welfare reforms in the Netherlands, and reforms a few years later in the UK, appear to have had (at best) only small impacts on caseloads (see Finn and Gloster, 2010). The Australian WtW reforms examined here represent a substantial tightening of participation requirements for the target group of low income parents, affecting only those with school age children (likely to face less substantial barriers to work than those with younger children)[1], and were introduced in a comparatively tight labour market.[2] *Ex ante* we might therefore expect reform impacts more in line with those found for the US than for the Netherlands or the UK. On the other hand the target group we focus on here is the existing stock of PP recipients as of 30 June 2006, and some studies suggest it can be harder to achieve positive

[1] Participation requirements start at an earlier age in most OECD countries (see Immervoll, 2010, Table 7). Australia has comparatively low labour force participation rates for mothers, but this is driven by lower rates for those with younger children. Participation rates for those with school aged children are in line with the OECD average (see Whitehouse and Hosking, 2005; www.oecd.org/dataoecd/29/61/38752721.pdf).

[2] In July 2007 the unemployment rate in Australia was 4.3%, and it subsequently peaked at just 5.8% in the aftermath of the Global Financial Crisis.

outcomes from activation for longer term recipients than for new entrants (see Dockery and Stromberg, 2004; Finn and Gloster, 2010).

Specifically, the report sets out a descriptive analysis of the following research questions:

- *Prior to activation, how many parents volunteered to participate in job activities? What were their training and employment outcomes? Did the introduction of the participation requirements affect these outcomes?*

- *Were there any adverse changes in behaviour as a result of this introduction of participation requirements (e.g. attempting to retain grandfathering status through having more children, transferring to non-activity tested payments etc.)?*

- *Did the introduction of the requirements when the youngest child turns seven affect the behaviour of parents before the youngest child reaches that age (in terms of education, employment or preparation activities such as volunteering for employment services and studying/training)?*

The primary focus of the report, however, is a statistical analysis of the following research questions:

- *What impact has activation had on claim duration and on exit rates from PPP/PPS for those covered by the new participation requirements?*

- *What impact has activation had on participation in paid work and on hours worked for those covered by the new participation requirements?*

- *What impact has activation had on participation in other activation activities, e.g. job search, for those covered by the new participation requirements?*

The remainder of this report is set out as follows. Section 2 provides brief details on PP and the activation reforms of 2006 and 2007. Section 3 reviews evaluations of earlier activation measures for low income parents in Australia and elsewhere. Section 4 summarises the data used for the analyses, which are taken from an administrative database longitudinally tracking IS recipients over time (the Research Evaluation Dataset, or RED) and also from a longitudinal survey tracking IS recipients just prior to the 2006 WtW reforms for a further two years (the Longitudinal Pathways Survey, or LPS). Section 5 briefly discusses the evaluation methodology which exploits differences in the coverage of the requirements based on age of youngest child to identify treatment effects. Section 6 presents a descriptive analysis of the first set of research questions listed above. Section 7 presents the statistical analysis of the impact of the reforms on the duration of PP episodes and on exit rates from PP. Section 8 presents the statistical analysis of the impact of the reforms on participation in paid work, hours worked and in work-related activities. Section 9 concludes. The Appendix contains further supporting evidence and sensitivity analyses. Analysis is conducted separately for PPS and PPP recipients where possible.

2. Parenting Payment and Activation

Prior to 2003, receipt of PP was not conditioned on any form of participation for those with a child under 16 years, although voluntary programs were available, including the Jobs, Education and Training (JET) Program, which combined an initial interview with a Centrelink advisor with other measures including career counselling, job search assistance and short training courses (for details see Banks, 2005). Limited conditionality – compulsory attendance at an annual interview with a Centrelink advisor – was introduced in 2003 for those whose youngest child was aged six years or older. More demanding participation conditions for those with a youngest child aged 13-15 years – compulsory Mutual Obligation participation in 150 hours of approved activities such as Work for the Dole, job search or training every 26 weeks of PP receipt, and a requirement to report activity every three months – were also introduced in 2003 (Banks, 2005). This was the regime in place until the 2006 WtW reforms.

Since 1st July 2006 new welfare claimants only qualify for PP if their youngest child is aged under eight years (PPS) or under six years (PPP).[3] Low income parents whose youngest child is older, or turns six/eight during a welfare episode, are no longer eligible for PP but may be eligible for less generous unemployment benefits (New Start Allowance (NSA)). Receipt of NSA for this group is conditional on meeting part-time participation requirements of 15 hours per week in paid employment, training or employment-related activities such as job search, in addition to NSA Mutual Obligation requirements after 26 weeks. New entrant PP recipients are only required to meet the part-time participation requirements (and PP Mutual Obligations) if or when their youngest child is aged six years or older.

Within the overall guidelines the precise nature of the requirements can be tailored to the particular PP recipient and are set out in a semi-contractual form known as an Activity Agreement, drawn up between the individual and the Centrelink advisor.[4] Failure to comply with these requirements, in the absence of any temporary exemption which may be granted for reasons such as ill health or disability of a child, triggers a series of warnings and ultimately, suspension of payments.

This report focuses not on new entrants to PP, however, but on the cohort of low income parents already in receipt of PP as of 30th June 2006, i.e. the stock of existing PP recipients at the time of the WtW reforms. This group – consisting of around 600,000 individuals – were 'grandfathered' and, provided they continued to meet the means-testing requirements for PP, and provided they didn't lose their grandfathered status by exiting IS for more than twelve weeks or by changing their partnered status, remained eligible for PP until their youngest child turned 16.

In order to avoid too abrupt a change to the system, this grandfathered cohort of parents were granted a grace period before being required to meet the new part-time participation requirements. The original intention was that this grace period would be for one year, with the new part-time participation requirements introduced for those whose youngest child was aged seven years or older, on 1st July 2007.[5] In practice, however, participation requirements were phased in, for those with a youngest child already aged seven or older, over a period of around

[3] There is some variation across states in the school entry age, but all six year olds in all states are required to be in school.
[4] Activity Agreements are similar in nature to UK Jobseeker's Agreements (for more details see Manning, 2009).
[5] The existing requirement to attend an annual interview remained for those with a youngest child aged six.

12 months, with those deemed furthest from the labour market activated first.[6] For this group, activation involved a call to interview with a Centrelink advisor during which the participation requirements were explained and, in most cases, an Activity Agreement setting out how the individual would meet the requirements drawn up and signed there and then. The rest of this group either signed an Activity Agreement at a later date (e.g. because of a temporary exemption)[7], exited PP following the interview but before signing an Activity Agreement, or were yet to sign an Activity Agreement by the end of our sample period (30th June 2009).[8] A parent whose youngest child was under seven as of 1st July 2007, but then subsequently turned seven, was called to interview within two weeks of the child's birthday, with a similar proportion signing Activity Agreements and with similar timing. Grandfathered PP recipients whose youngest child is aged under seven have no participation requirements but may *voluntarily* access employment services.

Before moving on, it is worth briefly setting out some additional details on payment rates and taper rates for those with other income, and, in particular, differences in these characteristics between PPS and PPP. These were not changed as part of the 2007 reforms (although they are periodically updated), but they help to place the reforms in context and, further, to understand why the impact of the reforms may have differed for PPS recipients compared to PPP recipients. The figures given refer to fortnightly periods and are those currently in place as of March 2011. The maximum PPS payment, for those earning no more than $170.60 (plus $24.60 for each additional child), is $611.90. Payments are reduced by 40 cents in the dollar for those earning above this threshold, with parents no longer eligible for part payments once their income exceeds $1,673.85 (again plus $24.60 for each additional child). In contrast, the maximum PPP payment is $424.00. Eligibility for the maximum payment is dependent on whether the individual's partner also receives a pension (e.g. PPP or other IS pension, Age Pension). If this is the case, then combined income must be less than $124 for maximum payment, with taper rates for combined income above this threshold initially 25 cents in the dollar (up to $500) and then 30 cents in the dollar, up to a maximum combined income of $1,579. If the partner does not receive a pension, then own income must be less than $62 and partner's income less than $790 for maximum payment. Taper rates are 60 cents in the dollar for own income (50 cents below $250) and partner's income, up to a maximum of $789.50 (own income), and $1,486.17 (partner's income) and $1,589.50 (combined income). In summary, PPS payments are higher than PPP payments at all eligible income levels for those whose partners receive a pension, although PPP taper rates are lower. For those whose partners do not receive a pension, PPP can be more generous than PPS for partners earning less than $1,100, but taper rates are higher.

3. Existing Evaluations of Related Reforms

There are no existing studies of the impact of this particular set of reforms, for this particular grandfathered group, in the public domain. But we can draw on the publicly available, in-house evaluation study of the 2006 reforms for the non-grandfathered group conducted by

[6] 75% of this group had been interviewed by the end of December 2007 and 99% by the end of June 2008. The first group of interviewees included those not engaged in any paid work and not registered with Job Network. The second group included those in working less than 15 hours per week but not registered with Job Network or those registered with Job Network but not in paid work. Those in the third group – activated last – were already working 15 or more hours per week.

[7] 81.5% of those that signed an Activity Agreement did so on the interview date. For the remainder, the mean gap between interview and signing an Activity Agreement was 93 days.

[8] 16% attended an interview but did not subsequently sign an Activity Agreement in the same PP episode.

DEEWR (DEEWR, 2008), together with two studies of an earlier pilot of mandatory interview attendance examined by Barrett and Cobb-Clark (2000) and Dockery and Stromback (2004). We can also draw on evaluations of recent activation reforms for lone parents in other countries. Here we focus on the UK and US, partly because reforms in these countries have been widely evaluated, but also because they provide a useful contrast in terms of the 'toughness' of participation requirements. For wider reviews see Carcillo and Grubb (2006), Finn and Gloster (2010) and Immervoll (2010).

3.1. Earlier Australian Welfare Reforms for Low Income Parents

In 1999 the Department of Family and Community Services piloted a reform making attendance at a JET interview compulsory, for the first time. The pilot was conducted as a random experiment with a sample of 5,000 PP recipients divided into one group that received no treatment (the control group), one that was asked to attend a face to face interview with a JET Advisor but not required to do so, and one that was required to attend such an interview. Barrett and Cobb-Clark (2000) exploit this to examine the preliminary impacts of making interview attendance compulsory on interview attendance and on participation *plans*. Unsurprisingly, they find much higher interview take-up rates for the compulsory interview group relative to the voluntary interview group. More interesting is that compulsion has a larger impact on some groups of low income parents, e.g. long term sole parents, than others. Evidence is also presented suggesting that participation in an interview changed the future employment and training plans of around thirty percent of attendees, although the evaluation came too early to examine impacts on employment and training *outcomes*. Dockery and Stromback's later study does examine differences in outcomes for those from the different treatment groups relative to the control group, finding evidence of a positive and statistically significant impact of treatment on benefit exit for recent claimants, but little evidence of positive impacts for existing longer term claimants. Their interpretation is that this group likely faces more substantial barriers to participation. The argument that mandatory activation measures may have more positive outcomes for those already closest to the labour market can be found more widely in the literature (see e.g. Finn and Gloster, 2010).

In the absence of random experimental evidence, the DEEWR (2008) evaluation of the 2006 WtW reforms adopts a combination of before and after comparisons and unconditional difference-in-differences to estimate the impacts of introducing the package of activation measures for new claimant low income parents on a variety of outcomes. The resulting evidence suggests that for those low income parents whose youngest child was school aged, the WtW reforms reduced inflows to IS, increased transfers from PP to other non-activity tested IS payments such as Disability Support Pension (DSP), increased participation in employment services, decreased the duration on IS, and, at least for partnered parents or single parents with a youngest child aged six or seven years, increased participation in paid employment for those still on IS. With the possible exception of transfers to DSP, these can be interpreted as positive outcomes given the reform objectives.

3.2. Welfare Reforms for Low Income Parents in Other Countries

First consider welfare reform in the US, in particular the major package of reforms introducing participation requirements and time-limiting welfare payments for low income parents in 1996 with the introduction of Temporary Assistance for Needy Families (TANF). TANF is a federal program, which among other things, mandates participation in work or work-related activities of at least 30 hours per week for single parents or longer for couples –

note this is ‘tougher’ than the Australian part-time participation requirements – whose youngest child is six years or older, although with some exemptions. Individual states have had considerable discretion in its implementation and the resulting variation in the program has encouraged extensive evaluation. The weight of evidence from this body of evaluation studies, using a variety of methods and data sources, points to significant impacts including reduced caseload, increased job entry for those exiting welfare, and increased participation in paid work for those remaining on welfare. These reforms were introduced at a time when the labour market was strong, and their impacts lessened after 2001 with a weaker labour market. There is also some evidence suggesting greater impacts for those already closer to the labour market, in line with Dockery and Stromberg’s (2004) interpretation of the impacts of the JET pilot reforms in Australia. For more detail on these US reforms see the reviews of Blank (2002) and Finn and Gloster (2010).

Second, consider recent reforms in the UK. Mandatory participation requirements for lone parents – conditioning benefits on attendance at increasingly regular Work Focused Interviews (WFIs) – were introduced in 2001, initially for new entrants with youngest children aged five or older and existing recipients with children aged 13 or over, but later extended to lower ages. These were intended to complement the existing and voluntary New Deal for Lone Parents (NDLP) program, itself broadly similar to the Australian JET program, for which positive impacts on welfare exit and employment entry for participants have been demonstrated using matching methods, albeit with low take-up rates (see Dolton et al., 2006). Perhaps unsurprisingly, given the much lighter participation requirements introduced in the UK relative to those introduced in the US, difference-in-differences estimates suggest that compulsory WFIs appear to have led to only a small decrease in welfare caseloads for the target group. Further, in contrast to Dockery and Stromberg (2004), impacts appear to have been stronger for existing recipients than for new claimants. For more detail see Cebulla et al. (2008) and Finn and Gloster (2010).

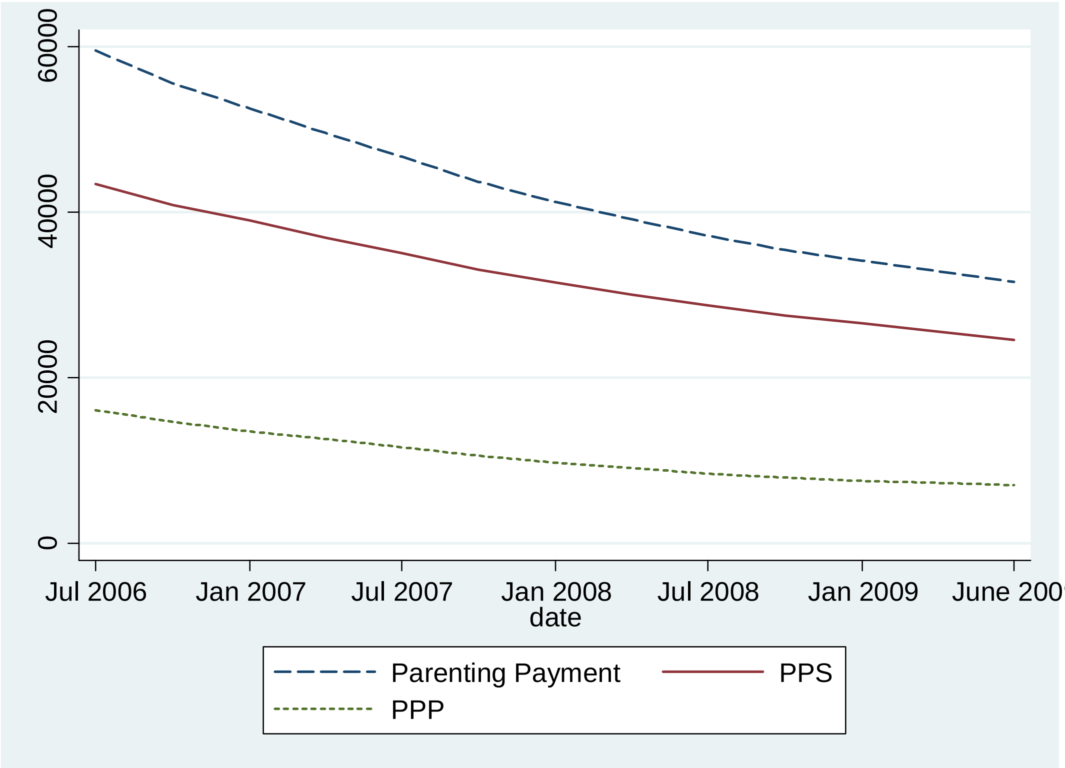
4. Data

This project uses a combination of administrative RED data and LPS survey data, although we primarily focus on the RED. Both data sets are in unit record form and contain very rich information, but each has its own specific advantages and disadvantages.

The RED records all episodes of IS receipt, along with details required to administer payment (e.g. earnings from paid employment) and some others, from the late 1990s onwards. It is longitudinal in the sense that individuals are tracked across multiple episodes, although it contains no information on individuals for periods outside of IS. Its main advantages are that it contains information on the full population of PP recipients and is continuous in time (IS ‘events’ are recorded to the day). Its main disadvantages are the lack of information outside of IS episodes and lack of detail and potential unreliability of information that is not required to administer IS payment (e.g. hours worked prior to WtW). Because of its size and complexity we take a ten percent random sample of the relevant RED population – all grandfathered PP recipients as of 30th June 2006 – and track them for all IS episodes from the beginning of the episode which was ongoing on 30th June 2006 until 30th June 2009. This gives us information on 90,664 IS episodes covering 59,490 individuals. We use these data in their episodic format (see Section 4.1) and also, for a subsample, in a six-monthly panel format (see Section 4.2).

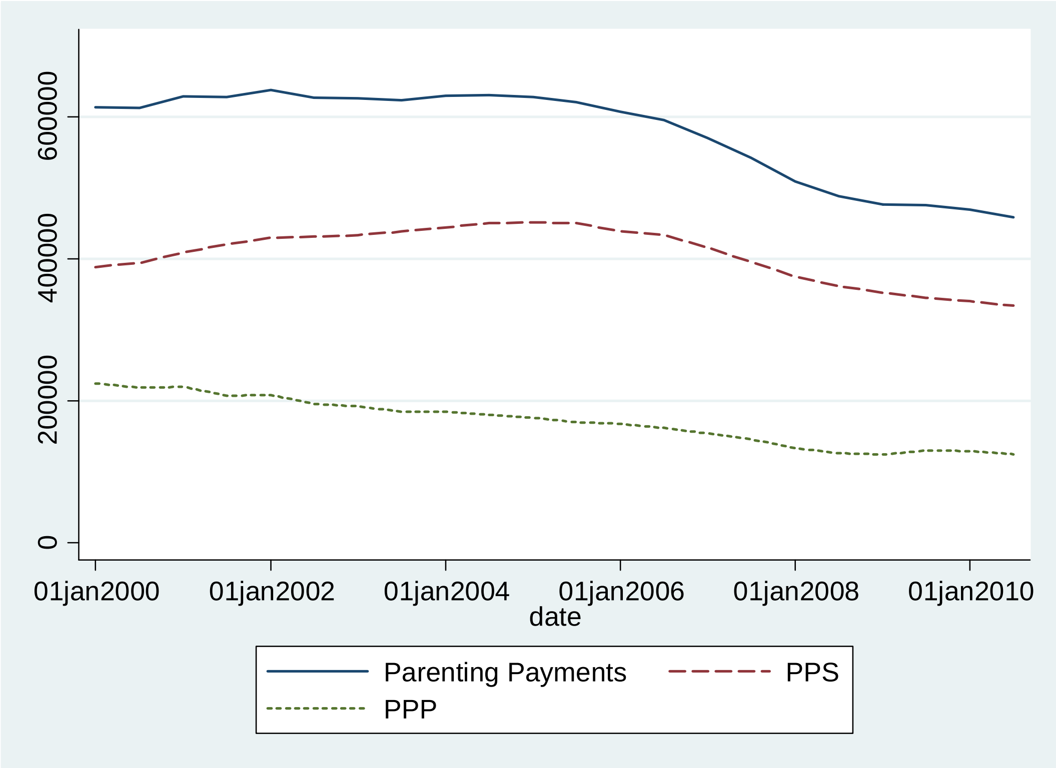
By 30th June 2009 the stock of recipients from the overall ten percent sample that were still receiving PP was down to just over 30,000 individuals – there are no inflows to the grandfathered cohort beyond 30 June 2006, only outflows – with some no longer receiving IS and others receiving other IS payments (see Figure 1).

Figure 1: Number of PP Recipients in the Ten Percent RED Grandfathered Sample, July 2006 – July 2009



Note that over the same period the total number of PP recipients – including new entrants to PP outside of the grandfathered group – fell by around twenty percent, most likely reflecting a combination of WtW impacts for those without grandfathered status together with a strong labour market, at least prior to the Global Financial Crisis (GFC) (see Figure 2).

Figure 2: Overall Number of PP Recipients, Jan 2000 - July 2010



The LPS tracks the experiences of a sample of IS recipients over time, including recipients who left IS. The LPS is designed to evaluate the effects of the WtW reform policies introduced in early 2006. As such it consists of three cohorts of IS recipients from the pre-reform, post-reform and transitional period of the reform. This report draws on data for Cohort 1 which tracks a sample of working age IS recipients drawn between 1st September 2005 and 28th February 2006, the period before WtW reform was implemented. Here we focus on all grandfathered PP recipients in wave 1 of cohort 1 (data collected May/June 2006), and we follow them for the five waves of the LPS, each approximately six months apart. This gives us information, initially, on 1806 individuals. The main advantages of the LPS are that outcomes (e.g. employment) are observable in between or after IS episodes, in contrast to RED, and that it contains more detailed information on parents' characteristics and behaviour (e.g. education level). The main disadvantages are the small sample size, attrition between waves[9], limited time span, and (for our purposes) the nature of the sampling frame which means the LPS Cohort 1 is not entirely representative of the wider grandfathered PP recipient population, even in wave 1. Summary information is presented in Table 1.

[9] Just under half of the original 1806 grandfathered PP recipients in wave 1 are still in the survey at wave 5 (see Table 1). The attrition is also non-random, e.g. with young sample members and those with lower education levels more likely to drop out of the survey.

Table 1: LPS Sample Size, Attrition, and the Characteristics of Grandfathered PP Recipients

	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
Male	9.36	9.70	9.62	8.79	8.18
Country of birth					
Australia born, non ATSI	69.32	71.75	72.64	72.85	73.32
NESB	15.84	13.92	13.27	13.43	12.67
ESB	7.70	8.10	8.79	9.58	10.20
ATSI	7.14	6.23	5.31	4.15	3.81
Age group					
Under 25	16.56	13.43	11.28	8.98	7.29
25-49	79.13	81.23	82.01	83.42	84.30
50-60	4.21	5.12	6.55	7.31	7.85
60+	0.11	0.21	0.17	0.30	0.56
Education					
Unknown	0.00	0.00	0.08	0.39	0.00
Primary school or lower	5.15	4.50	2.82	2.96	2.13
Year 10	41.14	37.95	35.74	33.66	34.87
Year 12	19.93	18.70	19.65	20.83	19.62
Trade qualification	25.75	29.29	30.35	29.42	30.72
Degree or above	8.03	8.93	10.61	11.55	12.22
Others	0.00	0.62	0.75	1.18	0.45
Remoteness					
Major cities	43.36	42.73	41.21	41.66	41.14
Inner regional	36.99	37.4	39.64	39.88	39.8
All outer regional	19.66	19.88	19.15	18.46	19.06
State					
NSW	32.78	30.82	31.01	30.21	28.70
VIC	20.27	20.91	20.23	21.22	21.08
QLD	24.81	25.48	25.29	25.57	26.35
SA	7.92	8.03	8.29	8.00	8.30
WA	9.25	9.21	9.20	8.49	8.52
TAS	3.54	3.88	4.06	4.54	5.04
NT	0.55	0.62	0.75	0.69	0.67
ACT	0.89	1.04	1.16	1.28	1.35
Living with partner	32.00	35.94	37.56	40.38	42.26
Partner currently working	18.16	24.31	25.54	29.52	31.39
Owns/buying house	28.02	29.71	32.17	34.35	36.10
Number of dependent children (mean)	2.05	2.09	2.11	2.17	2.18
Age of youngest					
Unknown	2.71	2.77	3.07	4.64	5.38
0-6	60.52	59.42	53.48	52.32	49.55
7-16	36.77	37.33	43.45	43.04	45.07
16+	0.00	0.48	0.00	0.00	0.00
Number of individuals	1806	1444	1206	1013	892

4.1. RED Data in PP Episodic Format

For each IS episode we have (time-varying) data on type of benefit claimed, episode start and end dates (right-censored if ongoing), benefit history of the individual, number of children and age of youngest child, age of the recipient parent, country of birth of the recipient parent, the Labour Force Statistical Region (LFSR) unemployment rate for each individual's local area, along with information on the date of the initial activation interview with the Centrelink advisor (for those activated) and the signing date for any subsequent (compulsory) Activity Agreement. We treat these episodic data as continuous in time and use them to analyse the duration of PP episodes and to estimate hazard functions for the daily probabilities of exiting PP, exiting PP to other IS payments and exiting IS.

Although the ten percent RED sample contains information on 90,664 IS episodes covering 59,490 individuals, most of the analysis that follows focuses only on the PP episodes 'live' at the grandfathering date for these 59,540 individuals (we call this the *full sample*), or on a smaller subsample of those episodes 'live' at either 30th June 2006 or 30th June 2007 for those with a youngest child aged six years at either date (we call this the *restricted sample*). This helps with identification of the impacts of activation (see Section 5). In the former case, by focusing only on 'first' episodes, analysis based on the full sample avoids complications arising from the fact that only those with younger children can subsequently re-enter PP once they have exited. In the latter case, by focusing only on those with a youngest child aged 6 and following them for only one year, analysis based on the restricted sample avoids complications arising from the phasing of activation and better allows us to separate the impact of activation from the impact of having a youngest child turn 7 years. Note that not all information on all variables of interest is observed for all individuals, so analysis is based on a slightly reduced sample in each case (see Section 7).

Tables 2 and 3 present summary information for the full and restricted samples respectively. The average duration of a completed PP episode in the full (restricted) sample is 1716 (1446) days, i.e. between four and five years, but many episodes – around 40% in the full sample and around 75% in the restricted sample – are still ongoing at the last point of observation (30th June 2009 for the full sample and 30th June 2007 or 30th June 2008 for the restricted sample)[10], so that these mean completed episode durations underestimate the mean duration of all PP episodes for the grandfathered cohort. In both samples PPS episodes are longer on average than PPP episodes, although the gap is small in the restricted sample. By definition no episodes can end before 30 June 2006 – either the individual concerned would not be in the grandfathered group or the episode ending prior to 30 June 2006 would be excluded from the sampling frame – but we have information on the elapsed duration of the current episode prior to this cut-off date, which again tends to be higher for PPS recipients compared to PPP recipients in both samples, although again with a larger gap in the full sample. We also have information on previous IS episodes, which on average sum to four years duration across both samples and both benefit types.

[10] We impose the earlier cut-off dates for the restricted sample for identification purposes (see Section 5).

Table 2: Durations and Covariate Sample Means (Standard Deviations), Full Sample

	All PP	PPS	PPP
Completed PP episode duration, days	1716 (1156)	1870 (1149)	1392 (1104)
Episode duration including right-censored episodes	2019 (1172)	2157 (1144)	1649 (1169)
Elapsed duration in current episode to 30 June 2006, days	1218 (1035)	1343 (1039)	954 (975)
Male	.101	.096	.110
Age of parent	36.7 (9.24)	36.9 (9.26)	36.0 (9.18)
Immigrant	.264	.215	.369
Number of children <16	1.67 (.927)	1.55 (.823)	1.94 (1.07)
LFSR Unemployment Rate, %	4.85 (1.31)	4.80 (1.31)	4.97 (1.31)
Previous IS episodes duration (prior to current episode), years	3.96 (3.56)	4.03 (3.58)	3.80 (3.52)
Proportion of episodes ending within window	62.9%	58.4%	74.8%

Note: covariates are measures at end of episode or right-censoring date.

As for the other observed covariates, around 90% of grandfathered PP recipients are women; the average age of grandfathered PP recipients is around 36 years; around one quarter of grandfathered PP recipients were born outside of Australia; grandfathered PP recipients have an average of two children under 16; and they face a local unemployment rate of almost 5% (full sample) or 4% (restricted sample). The difference in average unemployment rates reflects the temporary and marginal weakening of the labour market following the GFC in 2008/09. In both samples PPS recipients are more likely to be female, are less likely to be immigrants, have fewer children and episodes are more likely to be ongoing, relative to PPP recipients. Unemployment rates and previous IS episode durations are similar. PPS recipients are slightly older in the full sample but slightly younger in the restricted sample.

Table 3: Durations and Covariate Sample Means (Standard Deviations), Restricted Sample

	All PP	PPS	PPP
Completed PP episode duration	1446 (971)	1490 (904)	1418 (1084)
Episode duration including right-censored episodes	1929 (979)	1943 (937)	1936 (1086)
Elapsed duration in current episode to 30 June 2006, days	1267 (958)	1308 (895)	1243 (1064)
Male	.109	.090	.146
Age of parent	36.1 (6.60)	35.0 (6.57)	38.3 (6.13)
Immigrant	.264	.203	.385
Number of children <16	2.00 (1.03)	1.85 (.932)	2.39 (1.04)
LFSR unemployed rate, %	4.81 (1.34)	4.78 (1.32)	4.91 (1.39)
Previous IS episodes duration	4.13 (3.68)	4.05 (3.62)	4.12 (3.74)
Proportion of episodes ending within window	26.4%	22.9%	41.7%

Note: covariates are measured at end of episode or right-censoring date.

4.2. RED Data in Panel Format

For the restricted RED sample we also restructure the data into a pseudo-panel[11] format, with three waves, by recording the benefit status, work and work-related behaviours and other characteristics of the sample members at six monthly intervals over the period of interest. We construct this pseudo-panel as follows. For those PP recipients whose youngest child is aged six years on 30th June 2006 we treat 30th June 2006 as wave A, 31st December 2006 as wave B and 30th June 2007 as wave C. For those PP recipients whose youngest child is aged six years on 30th June 2007 we treat 30th June 2007 as wave A, 31st December 2007 as wave B and 30th June 2008 as wave C. For each wave we extract the relevant information from the RED episodic data on that date. We construct indicator variables for being on PP and being on IS at each wave, and for the age of youngest child, for all individuals. Note that all individuals in the restricted sample have a youngest child that turns seven years between waves A and C, but only for those in the latter group does this trigger activation. Also note that the two groups are mutually exclusive. For individuals that exit IS prior to wave C, information on participation in work and other activities, together with information on characteristics, is recorded as missing.

The purpose of constructing the RED pseudo-panel in addition to using the restricted sample data in episodic format is twofold. Firstly, although the RED episodic data are ideal for

[11] We use the term pseudo-panel because each wave combines individuals observed at different time periods.

analysing the duration of PP spells and exit probabilities, they are not well suited for analyzing participation in paid employment, hours worked or participation in other work-related activities because of the way this information is recorded: it is not recorded to the day, as is the case for IS payment entry or exit, but averaged over the relevant (fortnightly or longer) reporting period. So analysis of these behaviours requires some aggregation of the data along the time dimension. We adopt the six-monthly structure both for simplicity and for comparison purposes with the LPS. Secondly, the panel structure supports alternative estimation methods that we use to test the robustness of the main conclusions based on analyzing the data in their episodic format. Table 4 presents summary information by wave.

Table 4: RED Constructed Panel Summary Information

	Wave A	Wave B	Wave C
% with youngest child aged seven years	0	46.2%	75.0%
% activated	0	17.5%	33.2%
No. individuals in first group (on PP)	3665 (3665)	3665 (3269)	3665 (2918)
No. individuals in second group (on PP)	3007 (3007)	3007 (2643)	3007 (2327)
Total no. individuals (on PP)	6662 (6662)	6662 (5912)	6662 (5245)

Note: We treat those in the second group (those on PP with youngest child aged six years on 30th June 2007) as activated on and after the date their youngest child turns seven.

4.3. LPS Data

Because of its smaller sample size we are not able to split the LPS by age of youngest child to construct a similar panel for those with youngest child aged six years on either 30th June 2006 (this approximates to wave 1 in the LPS) or 30th June 2007 (this approximates to wave 3 in the LPS). But for similar reasons of identification (see Section 5), we construct a modified LPS pseudo-panel along broadly similar lines. In this case wave A of the constructed panel includes all those on PP at wave 1 of the LPS (May/June 2006) but also all those on PP at wave 3 of the LPS (May/June 2007). These groups are overlapping – by construction those on PP at wave 3 were also on PP at wave 1 – so that many individuals are recorded twice in wave A of the constructed panel.[12] Waves B and C of the constructed panel similarly merge waves 2 and 3 of the LPS proper with waves 4 and 5 for those on PP at wave 3. We also exclude those whose youngest child was aged six years at wave A. The result is a data set in which only some of those with youngest children aged seven plus – those drawn from the later waves of the LPS proper – are activated between wave A and wave C. Activation is assumed to occur prior to wave B for all those in this group. Table 5 presents summary information by wave. We also use the LPS in its raw form – as a five wave panel with the waves around six months apart – for descriptive analysis (see Table 1 for summary information).

[12] This is similar although not identical to pooling panel data (treating each wave as a separate cross-section).

Table 5: LPS Constructed Panel Summary Information

	Wave A	Wave B	Wave C
Mean age of youngest child, years	5.30	5.36	6.31
% activated	0	14.4%	16.0%
No. individuals in first group	1772	1412	1124
No. individuals in second group	957	781	675
Total no. individuals	2729	2193	1799

Note: We treat those in the second group (those in wave A drawn from wave 3 of the LPS) as activated in waves B and C if they have a youngest child aged seven plus at the time of wave A (see Section 5).

5. Methodological Approaches and Identification

In addition to descriptive analysis of the RED and LPS data as presented in Section 6, we also seek to identify the *causal* impact of activation on outcomes – average treatment effects on the treated (ATET) – by exploiting age-of-youngest-child based differences in the coverage of the participation requirements in a quasi-experimental, difference-in-differences type approach. This kind of age-based approach is common in the non-experimental evaluation literature on welfare reforms for low income parents (see e.g. Cebulla et al., 2008; DEEWR, 2008). The three generic identifying assumptions necessary for such estimates to provide unbiased estimates of the ATET are (i) that there are no contemporaneous shocks (other than activation) impacting on one group and not the other, (ii) that there are no underlying trends in outcomes that differ between the two groups, and (iii) that the probability of being treated is independent of outcomes, conditional on observed characteristics and other control variables.

Within this overall identification framework, we present a combination of simple unconditional estimates (essentially comparing the change in the means of certain outcomes for an age-based treatment group and an age-based comparison group), duration analysis akin to difference-in-differences regression analysis conditioning on observed characteristics, and a more standard difference-in-differences (probit) regression analysis. Because they allow us to control for differences in observed characteristics between those treated and those not treated, we place most weight on the regression analyses.

5.1. Duration Analysis

We present duration analyses of both the full and restricted variants of the ten percent RED sample, placing most weight on the estimates from the latter rather than the former for reasons explained below. In short, there are additional assumptions required for the first approach that are not required for the second approach, making it more difficult to rule out biases in the estimated ATETs from the full sample, although we can speculate as to the most likely direction of such biases. The second approach – using the restricted sample – is more likely to provide unbiased treatment effect estimates, but for a more narrowly drawn group of parents.

For duration analysis based on the full sample we use information on the date of each individual's activation interview (for those called to interview) to capture activation.[13] We specify a binary dummy equal to zero prior to interview and equal to one afterwards, and equal to zero throughout for the comparison group of those never called to interview because they have a youngest child aged under seven years.

Our outcomes of interest are the single risk hazard rate for exit from PP (including exit to other IS payments) and competing risk hazard rates for exits from PP to other IS benefits and exits from PP off IS altogether.[14] For examples of earlier evaluation studies adopting a similar duration analysis approach see Jensen et al. (2003), Fortin et al. (2004) and McVicar and Podivinsky (2010). We are interested in whether activation impacts on these hazards, and if so in what direction and with what magnitude.[15]

The hazard function for this approach is given below, where *postinterview* is defined as above:

$$h(t) = h_0(t)\exp(\alpha D + \beta_1 x_1 + ... + \beta_N x_N + \delta\, postinterview) \qquad (1)$$

In (1), $h(t)$ is the hazard rate for the relevant outcome, $h_0(t)$ is the baseline hazard, D is elapsed duration in the current episode prior to 30 June 2006, and $x_1 ... x_N$ are observed individual characteristics of the parent, e.g. gender, number of children, and whether born outside Australia. Our estimate of the ATET is given by $\hat{\delta}$. We estimate a single risk version of (1) for the hazard of exiting PP and an independent competing risks version of (1) for the hazard rate of exiting PP to another IS payment and exiting IS altogether.

(The following three paragraphs discuss technical issues concerning identification of the ATET and can be skipped for non-technical readers.)

Tables A1-A3 show that the treatment and comparison groups are somewhat different in terms of observed characteristics. For example, grandfathered PP recipients with older children are more likely to be male, older and born outside Australia, and on average have experienced a longer PP episode prior to the grandfathering date, than those with younger children. To the extent that these differences and their impacts on duration are stable over time, however, they are controlled for in (1).[16] We are also comparing those with school age

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[13] We use the date they sign a compulsory Activity Agreement (for those that do so) to explore robustness, although our preferred estimates use the interview date definition given that this is when PP recipients are first informed of the new requirements, advised of ways to meet them, and in most cases the date from which they are covered by a formal Activity Agreement. Where the dates do not coincide, using the Activity Agreement definition may introduce additional selection bias. Both events are measured with error, which may downwards bias estimated treatment effects using either variable, assuming the measurement error is random.

[14] Hazard rates give the probability of an event occurring in a particular period – in this case the daily probability of welfare exit – given that it hasn't yet occurred at the start of the period. Single risk hazard rates treat all events as the same – exiting PP – whereas competing risks hazard rates separately identify exit destinations (in this exiting PP to other IS payments or exiting PP and leaving IS altogether).

[15] In common with much of the empirical literature on welfare duration and unemployment duration, we take a reduced form Cox Proportional Hazards (CPH) approach to estimation to allow for flexibility in the shape of the baseline hazard (see van den Berg, 2001). For tractability we also assume independent competing risks, again in common with much of the literature.

[16] An examination of trends in durations for those with pre-school age children by gender, immigrant status and age, suggests the main concern is that males have been experiencing a downward trend in PP duration relative to females. This could impart an upwards bias on our estimated treatment effects. We therefore test sensitivity to excluding males from the sample.

children to those with pre-school age children, for whom we might expect large differences in participation, and therefore in PP claiming, unrelated to activation (e.g. Kalb and Thoresen, 2010). Again we control for this to some extent in (1) by the inclusion of a dummy for youngest child aged under six years, given that all children aged six are required to attend school throughout Australia. But differences in trends for the two groups over time, or asymmetric shocks, whether driven by observed or unobserved differences in their characteristics or the impact of these characteristics on durations, could still impart bias on our estimated treatment effects. We can get an indication of this by looking at trends in PP receipt before 1st July 2007, with Figure 7 (see page 31) suggesting that the number of PP recipients with younger children was falling slightly faster than the number of recipients with older children. A continuation of that trend beyond 1st July 2007, in the absence of activation, could impart a downwards bias on our estimated treatment effects.

Second, in part because of the phasing-in of the activation process for those already with a youngest child aged seven or older on 1st July 2007, the probability of being treated is unlikely to be independent, even conditional on the observed covariates, of the hazard for PP exit. Those with unobserved characteristics associated with higher hazard rates will have been less likely to survive until treatment than those with less favourable characteristics. This will have been reinforced by the nature of the phase-in, with those deemed furthest from the labour market treated up to one year earlier than those deemed closer to the labour market. In other words those that receive treatment are likely to be a select group, most likely with less favourable unobserved characteristics (in terms of exiting PP), than those that exit prior to treatment. If we assume homogeneity in treatment effects (i.e. that the impact of treatment is the same for everybody) the resulting bias is more likely to be negatively signed than positively signed.

Finally, we assume no anticipation effects – changes in behaviour in anticipation of being covered by participation requirements in the spirit of Black et al. (2003). Such anticipation effects could impart selection bias of uncertain sign on the estimated treatment effect even in the absence of phasing-in of the treatment. This is our main motivation for using the interview date rather than the Activity Agreement date as our preferred treatment variable: in the latter case anticipation effects are highly likely for those that have attended an interview but are yet to sign an activity agreement (we might also interpret these as initial treatment effects); in the former case we specify the treatment period from the earliest possible date, which reduces the scope for anticipation effects.

Our second duration analysis approach uses the restricted RED sample, focusing only on those parents on PP and with a child aged six years old on day one of the activation period, i.e. 1st July 2007 and their counterparts one year earlier. Because parents in the 2007 group are called to interview within two weeks of the child's seventh birthday we take the child's seventh birthday as the date on which the parent is subject to the new participation requirements, although we also test robustness by using the same interview dummy as in the full sample approach.[17] Our treatment variable is therefore equal to zero for the period prior to the child's seventh birthday and equal to one following the child's seventh birthday. For a comparison group we take the equivalent cohort one year earlier, i.e. those grandfathered parents with a child aged six years old on the 1st July 2006. The youngest children of the parents in this group will turn seven during the subsequent year running up to 30th June 2007, but this will not trigger activation because of the grace period for grandfathered parents.

[17] The data for age of youngest child, and the child's seventh birthday, are also more reliable than the data on interview and agreement dates.

Individuals are assumed to be at risk of exit from the 30th June 2006 (comparison group) or the 30th June 2007 (treatment group), with ongoing episodes treated as right-censored as of 30th June 2007 (comparison group) or 30th June 2008 (treatment group).

The hazard function for this second approach is given below:

$$h(t) = h_0(t)\exp(\alpha D + \beta_1 x_1 + ... + \beta_N x_N + \gamma_1 treatmentgroup + \gamma_2 turned7 + \delta turned7 * treatmentgroup) \tag{2}$$

In (2), *treatmentgroup* is a binary dummy indicating whether the individual is in the treatment group (those with a six year old child on 1st July 2007), *turned7* is a binary dummy equal to zero for parents with a youngest child aged six and equal to one with a youngest child aged seven, and we interpret their interaction as the treatment indicator. Under the above assumptions, our estimate of the ATET is again given by $\hat{\delta}$. As before, we estimate both single risk and competing risks versions of (2).

(As before the following paragraph discusses technical issues concerning identification of the ATET and can be skipped by non-technical readers.)

In contrast to the full sample approach, the treatment and comparison groups in this case are similar in terms of observed characteristics, also likely to signal greater similarity in terms of unobserved characteristics (see Tables A4-A6). We also have the treatment group dummy as an additional control. Further, because all six year olds are required to attend school throughout Australia any differences in PP claiming associated with the child turning seven that are not related to the activation, are likely to be small, and are in any case controlled for by the dummy for turning seven. Differential trends are also less likely, and because we look at a shorter window, are likely to impart a smaller bias if present than in the full sample approach. Although there is no phasing of treatment for this group – treatment occurs when the child turns seven – it is still possible for PP recipients in either the comparison or treatment groups to exit PP before their youngest child turns seven. So those still on PP when their youngest child turns seven may form a select group, but again because we are looking at a much shorter window (most individuals in both groups survive until this point) and because the groups are observationally similar, this is unlikely to impart a large bias. Taken together, these arguments suggest assignment of treatment is closer to being exogenous in the restricted sample case than in the full sample case. On the other hand, if there are anticipation effects for those whose youngest child is aged six after 1st July 2007, or for those whose youngest child turns seven prior to 1st July 2007, then this could impart a larger bias in this second approach compared to the first approach because the treatment and comparison groups are more similar and because the window of analysis is shorter.[18] Finally, to interpret the resulting estimate of the impact of youngest child turning seven as an ATET across all those grandfathered PP recipients with a youngest child aged seven *or older*, rather than treatment effect specific to those whose youngest child turns seven, we need to assume that activation has the same impact for those with a youngest child older than seven.

On balance then, although neither approach is ideal (given the lack of experimental data), our preferred estimates are from the second approach because there is less scope for selection and other biases. The first approach, however, serves as a useful sensitivity analysis. Further,

[18] We are able to test robustness to this in the restricted sample approach by including an anticipation dummy equal to one in the last three months prior to youngest child turning seven for the treatment group and zero otherwise. The results suggest no such anticipation effects.

although we stop short of claiming the first approach gives us lower bound estimates of treatment effects, the overall bias on the estimates does seem more likely to be downwards than upwards.

5.2. Probit Models

We also estimate probit models for the probability of (i) entering employment (for those not already in employment), (ii) increasing hours worked to 15+ hours per week for those previously working fewer than 15 hours per week, and (iii) probability of becoming job-search active for those not previously job search active. We use both the RED and the LPS pseudo-panels for this purpose (see Section 4), with the RED panel being the panel-format of the restricted RED sample discussed previously. The probit models require the same identification assumptions as discussed in Section 5.1, so we will not repeat the discussion here.[19]

The probit models take the following form:

$$\text{Prob}\,(y=1)=\Phi(\alpha+\beta_1 x_1+...+\beta_N x_N+\delta treatment+u) \tag{3}$$

Where y denotes the outcome of interest (e.g. job entry by wave C), $x_1...x_N$ are again observed individual characteristics of the parent (here defined at wave A), *treatment* denotes activation (given by a binary dummy equal to one for those parents with a youngest child aged seven plus post 1st July 2007 and zero otherwise), and Φ denotes the normal cumulative distribution function (we assume the error term is normally distributed). Rather than report the coefficients themselves, we follow standard practice and report *marginal effects*, i.e. the impact of being treated (or of a one unit change in the control variables) on the probability of the various outcomes of interest.

6. Descriptive Analysis

This section presents descriptive analysis of the first three research questions set out in the introduction. These are:

- *Prior to activation, how many parents volunteered to participate in job activities? What were their training and employment outcomes? Did the introduction of the participation requirements affect these outcomes?*

- *Were there any adverse changes in behaviour as a result of this introduction of participation requirements (e.g. attempting to retain grandfathering status through having more children, transferring to non-activity tested payments etc.)?*

- *Did the introduction of the requirements when the youngest child turns seven affect the behaviour of parents before the youngest child reaches that age (in terms of education, employment or preparation activities such as volunteering for employment services and studying/training)?*

[19] The equivalent information to that contained in Tables A1-A6 for the RED – setting out differences between age-of-youngest based groups in terms of observed characteristics – is presented in Tables A9 and A10.

The discussion is brief, given the primary aim of the report is to address the second set of (causal) research questions from the introduction. Further, none of the analysis in this descriptive section relies on the age-of-youngest based identification strategies discussed in Section 5; instead we discuss what we can learn from the raw data. We do return to the third question, however, in Section 7. We draw on both the RED and LPS data, depending on which is most suitable for the relevant question.

First consider participation in paid employment prior to activation. Figures 3 and 4 show the proportion of the ten percent grandfathered RED sample reported as being in paid employment, separately by weekly hours categories, at six monthly intervals from 1st July 2006 to 1st July 2009, separately by payment type. In each case the first three data points – 1st July 2006, 1st January 2007 and 1st July 2007 – can be viewed as prior to activation. Summing across the hours categories we can see that around 35 percent (15 percent) of PPS (PPP) recipients in the grandfathered group were in paid employment prior to activation. There is a slight increase in the proportion over the year to 1st July 2007 in both cases, but the main trend is an artificial one: improvement in data quality over time is reflected in a smaller proportion working with unknown hours and larger proportions in each of the known hours categories. Data quality caveats aside, there does appear to be a faster increase in the proportion working between 15 and 34 hours per week in both cases – this is where we might expect an impact of activation given that 15 hours per week is the post-activation participation requirement – than in the other hours categories. Overall, a similar proportion of PP recipients in LPS waves 1-3 participated in paid employment.

Figure 3: Proportion of Grandfathered PPS Recipients in Paid Employment, by Weekly Hours, Ten Percent RED Sample

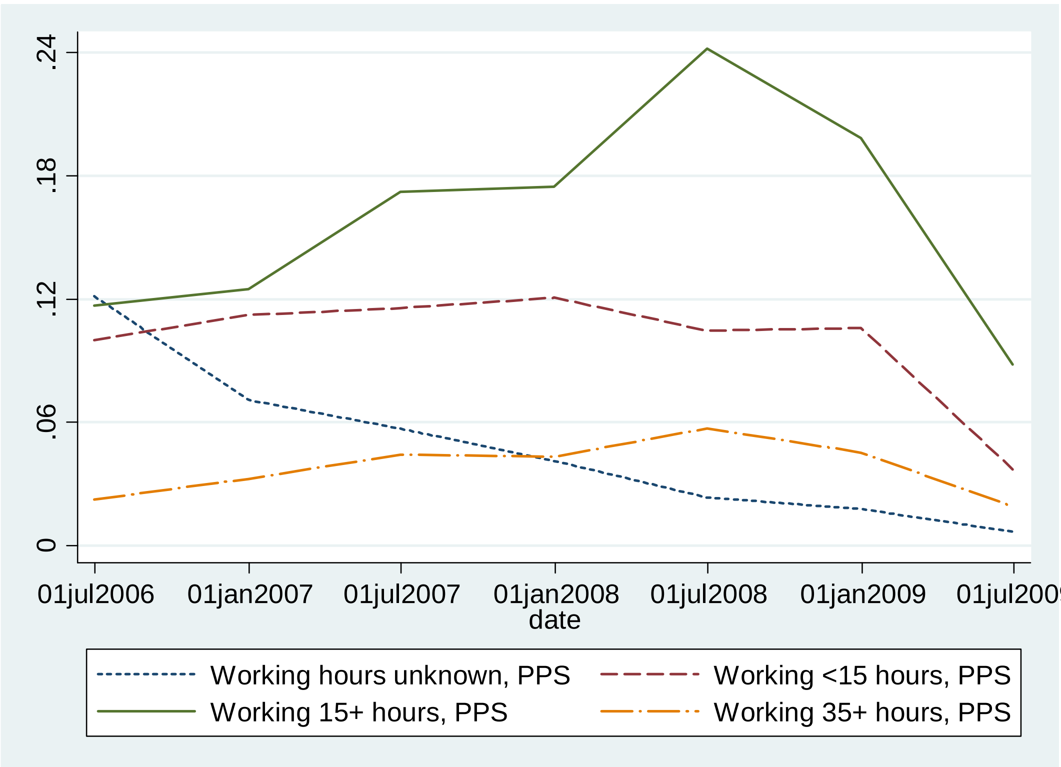
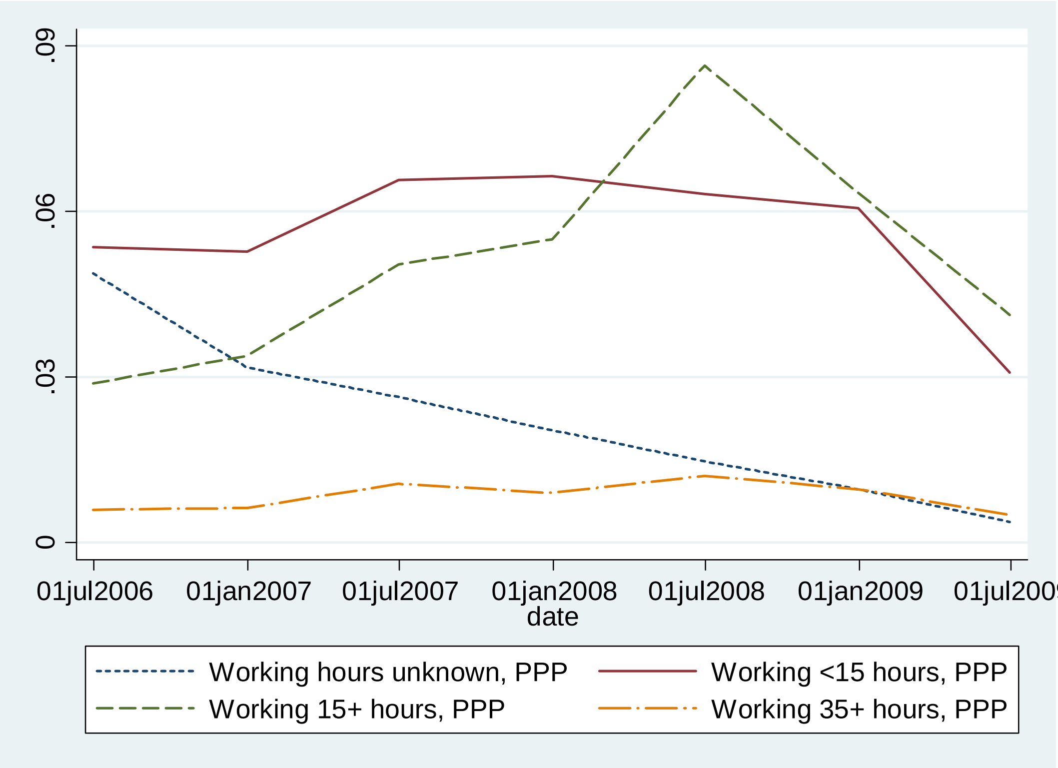


Figure 4: Proportion of Grandfathered PPP Recipients in Paid Employment, by Weekly Hours, Ten Percent RED Sample



In terms of work-related activities, just over half the ten percent grandfathered RED sample were reported on 30th June 2006 as having been actively job searching or engaged in related activities, for both payment types (see Tables 6 and 7).[20] In the LPS the proportion is much lower in each case, but with a narrower definition of job search. The LPS also includes information on those engaged in formal full-time or part-time study[21] and suggests that around 12 percent of those on PP in wave 1 do so.

[20] We use a broad definition here including job search, job search training, intensive support and other related activities.
[21] Education or training that leads to a qualification.

Table 6: Employment, Training and Related Outcomes for Those Participating in May/June 2006, Ten Percent RED Sample & LPS on PPP

	Sample Proportion	Proportion in Employment 30th June 2007/wave 3	Proportion Job searching (& related) 30th June 2007/wave 3	Proportion Increasing Education Level by wave 3
RED				
In employment 30th June 2006	13.7%	67.4%		
Not in employment 30th June 2006	86.3%	9.0%		
Job searching & related 30th June 2006	51.5%		95.7%	
Not job searching & related 30th June 2006	48.5%		9.9%	
LPS				
In employment wave 1	21.2%	89.1%		
Not in employment wave 1	78.8%	25.7%		
Job searching wave 1	16.7%		31.6%	
Not job searching wave 1	83.3%		13.2%	
Formal study wave 1	8.5%			27.8%
Not formal study wave 1	91.5%			18.7%

Table 7: Employment, Training and Related Outcomes for Those Participating in May/June 2006, Ten Percent RED Sample & LPS on PPS

	Sample Proportion	Proportion in Employment 30th June 2007/wave 3	Proportion Job searching (& related) 30th June 2007/wave 3	Proportion Increasing Education Level by wave 3
		RED		
In employment 30th June 2006	36.0%	80.7%		
Not in employment 30th June 2006	64.0%	16.9%		
Job searching & related 30th June 2006	55.1%		96.5%	
Not job searching & related 30th June 2006	44.9%		12.2%	
		LPS		
In employment wave 1	38.9%	88.9%		
Not in employment wave 1	61.1%	28.3%		
Job searching wave 1	21.9%		43.4%	
Not job searching wave 1	78.1%		16.7%	
Formal study wave 1	12.0%			28.0%
Not formal study wave 1	88.0%			19.6%

Tables 6 and 7 also summarise employment and other outcomes as of 30th June 2007 for the ten percent RED sample, separately by employment and job search status on 30 June 2006, and for those in the LPS sample on PP at wave 1, in both cases separately by payment type. As we would expect, those participating in paid employment in May/June 2006 are more likely to participate in paid employment one year later, for both payment types, and whether we use the RED data restricted to those still on IS or the LPS data that includes those outside of IS. The same goes for job search and related activities, again for both payment types and both data sets. These higher participation rates one year on for those in employment or job searching one year back are likely to reflect a combination of state dependence (other things being equal you are more likely to engage in an activity if you were engaged in the activity in the previous period) and observed and unobserved heterogeneity between individuals (some parents are more likely to work in both periods because of their characteristics). The LPS also includes information on participation in education and training and on highest education level, and a larger proportion of those in formal study in wave 1 of the LPS increase their education level by wave 3 than for those not in formal study at wave 1, for both payment types. The difference is small, however, and there are data quality issues to bear in mind with the education-related variables in the LPS.

The last part of the first research question concerns these behaviours following the introduction of participation requirements. We return to this issue in the discussion of Section 7 and Section 8, but we can see from Figures 3 and 4 that there is a jump in the proportion of grandfathered PP recipients that work between 15 and 34 hours per week in the first half of 2008, for both payment types, with a slight corresponding fall in the proportion working fewer than 15 hours per week. This is consistent with an impact of activation. Overall there is a slight increase in the proportion of PP recipients that participate in paid employment but only to 1st July 2008; after this the proportion working falls rapidly for both payment types, likely reflecting a combination of the GFC and a selection effect whereby those remaining on PP after this length of time are those less likely to work. There is no clear trend in the proportion of those on PP engaged in job search and related activities, although there is a slight increase in the second half of 2007 and the first half of 2008, but this is followed by a slight decrease. The LPS suggests a clear trend increase in the proportion participating in paid employment across all waves for both payment types, which is to be expected given the LPS sampling frame (parents remain in the sample once they leave IS). There is a declining trend in job search participation in the LPS, which again is likely to reflect the fact that those leaving IS for employment remain in the sample in subsequent waves.

Now consider the second of the descriptive research questions (concerning adverse changes in behaviour as a result of the introduction of participation requirements). In particular, we consider two such behaviours: attempting to retain grandfathering status through having more children, or transferring to non-activity tested payments.

Figure 5 shows the monthly birth rate for the RED ten percent grandfathered sample from July 2002 until June 2009, separately by payment type. Despite the noise in the data there does appear to be an increase in birth rates from around the middle of 2007 for PPS recipients, following a flat period between 2004 and 2006 and a downward trend prior to that. This is consistent with a change in behaviour aimed at retaining grandfathered status, but other explanations are also possible.[22] For PPP recipients birth rates have been rising more or less consistently since July 2002, with no clear jump from 2007 onwards.

[22] For example, the arrest of the decline in monthly birth rates for PPS recipients coincides with the introduction of the Baby Bonus in 2004 and the upward trend in monthly birth rates from 2007 coincides with substantial increases in the value of the Baby Bonus from that time. For further information on the fertility impact of the Baby Bonus see Drago et al. (2009).

Figure 5: Monthly Birth Rate for PP Recipients, Ten Percent RED Sample, July 2002 – July 2009

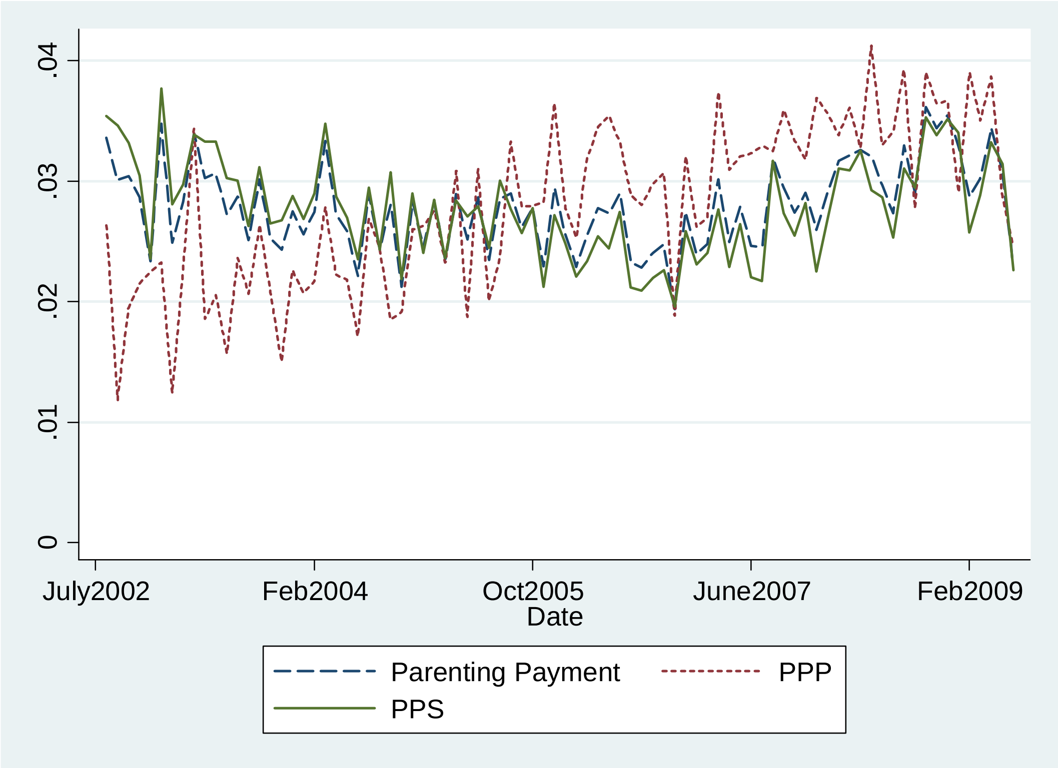
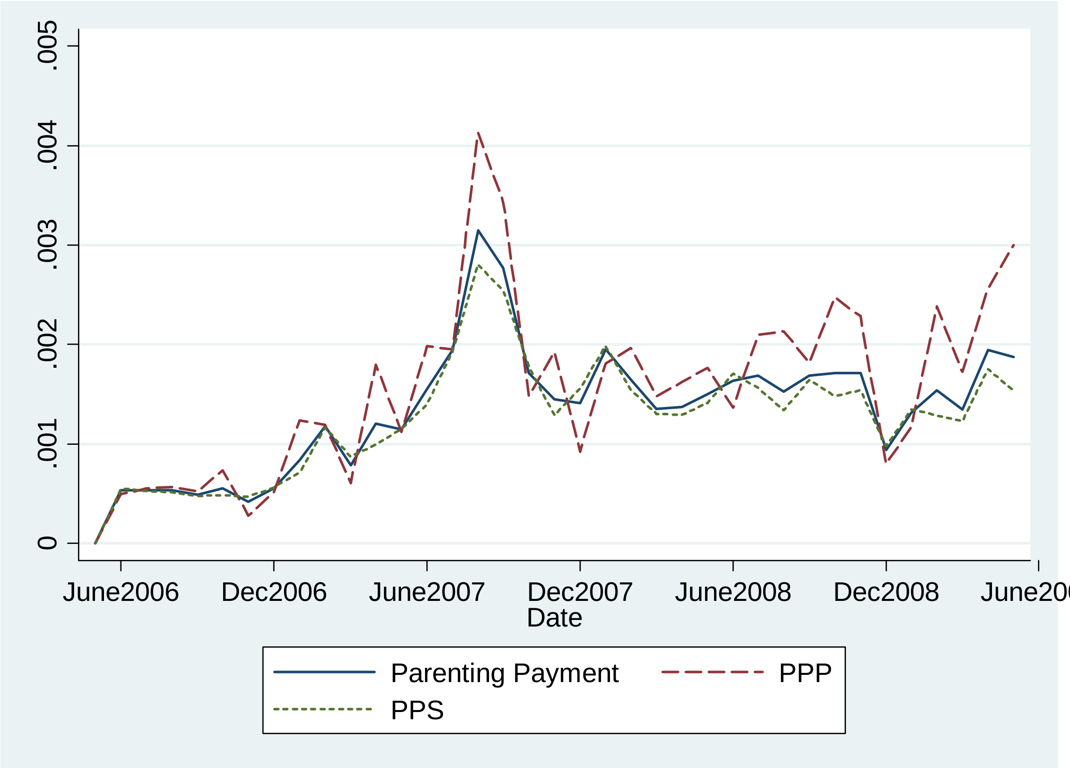


Figure 6 shows the monthly rate of transfer from PP to DSP for the RED ten percent grandfathered sample from June 2006 until June 2009, again separately by payment type. Once again the data are noisy, but there is a spike in the transfer rates for both PPS and PPP in the second half of 2007, after which they settle back at a higher level than that prior to 1st July 2007.[23] Again this is consistent with a change in behaviour that might be viewed as adverse. We return to this question in Section 7 where, among other things, we estimate a hazard model for exits from PP to other IS payments, controlling for other factors as far as possible. The suggestion there is also that activation led to an increase in transfers to other payments, including DSP.

[23] With an average stock of around 500,000 grandfathered PP recipients in the year prior to 30 June 2007 and of around 400,000 in the two years following 30 June 2007, Figure 6 implies an increase in the number of grandfathered PP recipients that flow on to DSP each month from approximately 300 in the earlier period to approximately 600 in the latter period.

Figure 6: Monthly Rate of Transfer from PP to DSP, Ten Percent RED Sample, July 2006 – July 2009



The final descriptive question concerns what in the literature is variously known as anticipation effects or threat effects, i.e. changes in behaviour that occur prior to treatment that might be driven by a desire to avoid treatment, or otherwise in anticipation of treatment (see Black et al., 2003). We can get some indication by examining trends in behaviour for PP recipients with a youngest child aged six years. The LPS – which has the better information on engagement in education and training – is too small a sample for analysis restricted to those with a youngest child aged six years. But we can use the ten percent RED sample to examine trends in employment, hours and job-search and related activity. Table 8 summarises these temporal patterns by payment type. There is no clear trend in either participation in paid employment or in the proportion of those working that work 15+ hours per week for either PPS or PPP recipients, and no clear evidence of an increase in the level of participation or hours following 30th June 2007. There is, perhaps, a small jump in the level of engagement in job search and related activity from the second half of 2007, at least for PPS recipients – plausibly consistent with an anticipation effect – although this falls back later. An important caveat here is that the RED data only include those on IS, and if parents shortly to be activated respond by exiting PP (or exiting IS altogether), we will not pick this up with this kind of simple descriptive analysis. We therefore return to this question in Section 7, but again we find no evidence of anticipation effects even with multivariate analysis.

Table 8: Trends in Behaviour for PP Recipients with Youngest Child Aged Six, Ten Percent RED Sample, by Payment Type, at 30/06/2006

	30/06/06	31/12/06	30/06/07	31/12/07	30/06/08	31/12/08
			PPP			
% in paid employment	16.8%	14.9%	17.2%	17.6%	24.7%	19.7%
% of those in employment working 15+ hours per week	59.4%	64.4%	72.3%	68.1%	71.1%	66.0%
% job searching or related	49.6%	55.1%	58.7%	59.3%	59.9%	50.8%
			PPS			
% in paid employment	41.3%	38.0%	40.7%	36.2%	42.0%	35.1%
% of those in employment working 15+ hours per week	78.5%	79.6%	84.6%	82.0%	85.2%	81.6%
% job searching or related	56.5%	60.0%	64.0%	65.9%	69.0%	62.8%

7. Impacts of Activation of Duration of PP Episodes and PP Exits

7.1. Simple Unconditional Estimates

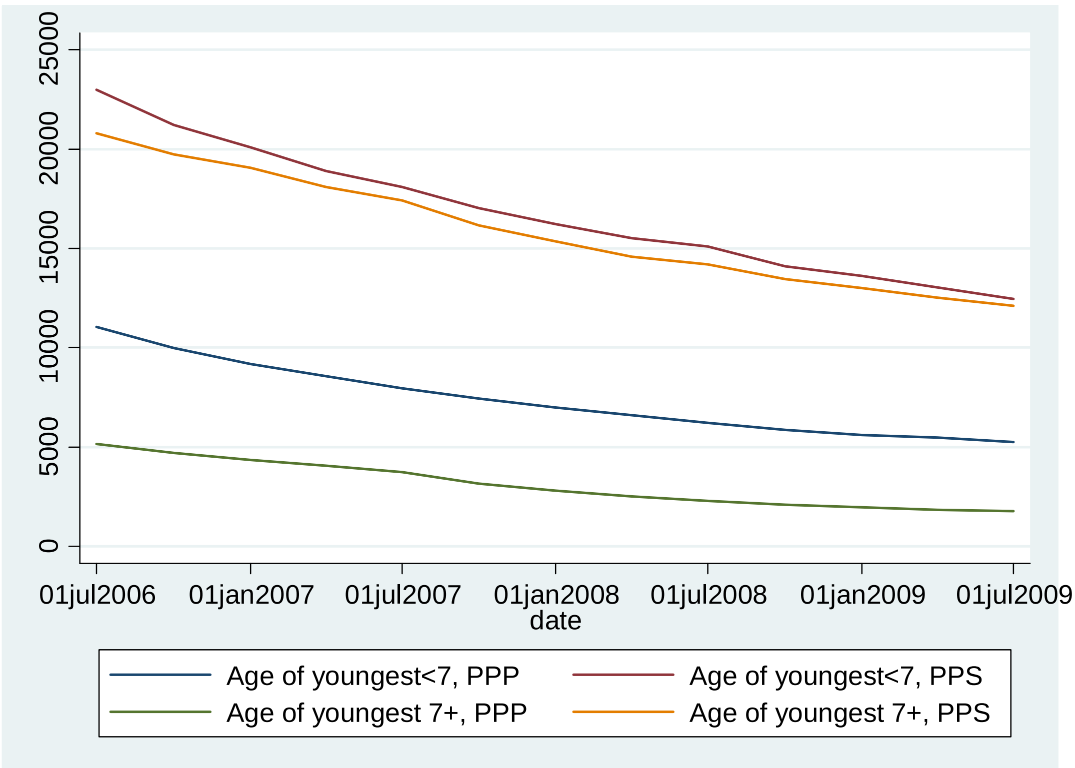
We have already seen in Figure 1 that the number of individuals from the (ten percent sample of the) grandfathered cohort on PP falls over time from 30 June 2006. At first glance there is no clear increase in the rate of decline of the stock of recipients, however, following 1st July 2007. On the other hand, following 1st July 2007, the number of PP recipients with a youngest child aged seven or older does appear to move more in parallel with the number of PP recipients with a youngest child aged under seven, whereas previously recipient numbers were falling faster for those with younger children (see Figure 7).

This relative increase in the pace of caseload decline for the target age group reflects an increase in the exit rates from PP for those covered by the new participation requirements relative to those not covered on age-of-youngest grounds. It is this impact on exit rates, and the resulting PP episode durations, that is the focus of this section. Specifically, we address the following research question:

- *What impact has activation had on claim duration and on exit rates from PPP/PPS for those covered by the new participation requirements?*

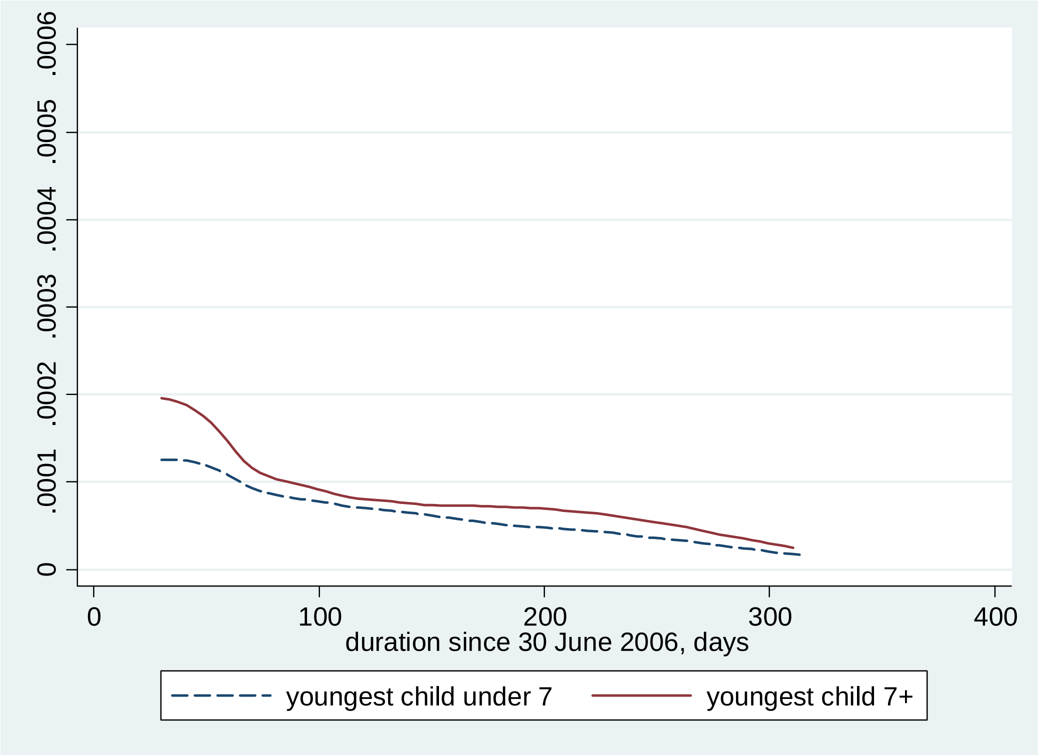
We begin with simple unconditional difference-in-differences estimates before moving on to more sophisticated estimates in Section 7.2 (the full sample) and 7.3 (the restricted sample).

Figure 7: Number of PP Recipients in the Ten Percent RED Grandfathered PP Sample, by Age of Youngest Child



Figures 8-11 suggest Kaplan-Meier (KM) hazard rates[24] for exit from both PPS and PPP to other IS payments have increased for the grandfathered cohort following 1st July 2007 whether the youngest child is aged seven or older or aged under seven, with the increase in the hazard for those with older children noticeably larger than the increase in the hazard for those with younger children.[25] For PPP recipients the increase in the KM hazard for those with youngest child aged seven or older is particularly pronounced. There is a similar picture for exits from IS shown in Figures 12-15, although such exits are more common both before and after activation, again with the impact of PPP recipients particularly pronounced. Note that

Figure 8: Kaplan-Meier Daily Hazard Rates for Exit to Other IS, by Age of Youngest Child on 30 June 2006, PPS Recipients, Duration since 30 June 2006 (Before Activation)

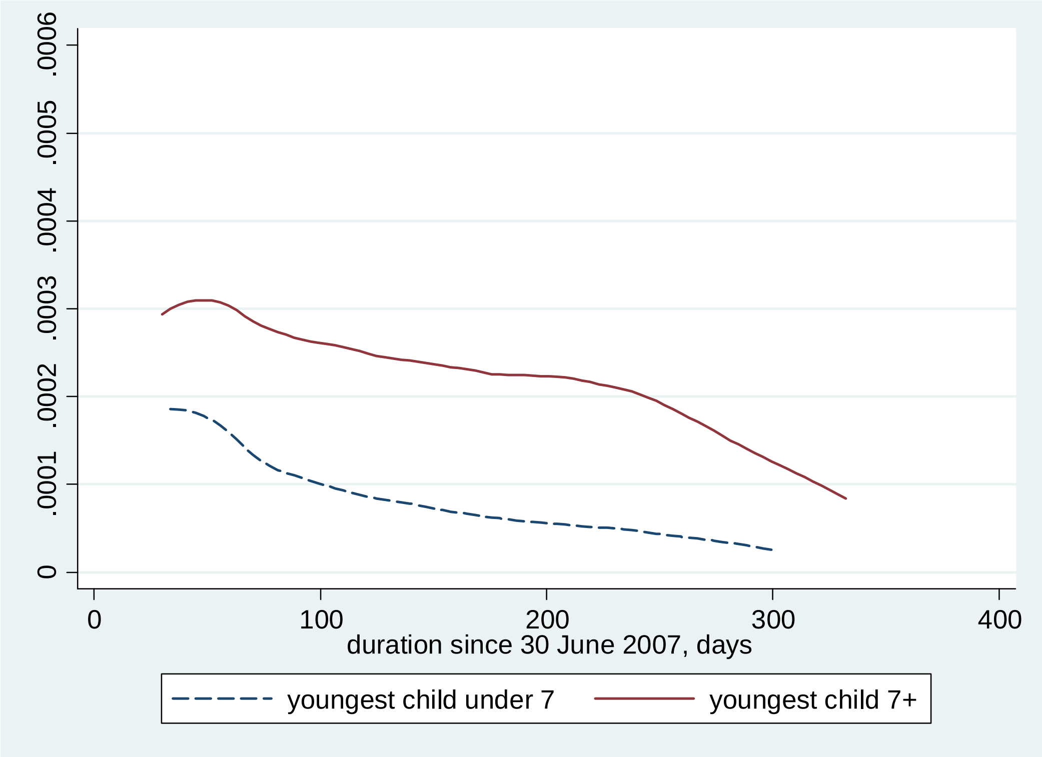


Note: Duration is measured from 30 June 2006 and episodes are treated as right-censored on 30 June 2007.

[24] KM hazard rates show the daily probability of exiting PP to a particular 'destination' given the parent has remained on PP until that day. Note that the daily hazards are very low – typically fewer than one in a thousand parents in receipt of PP on 30th June 2006 or 30th June 2007 exit on any given day subsequently – reflecting the long average duration of PP episodes (see Table 9).

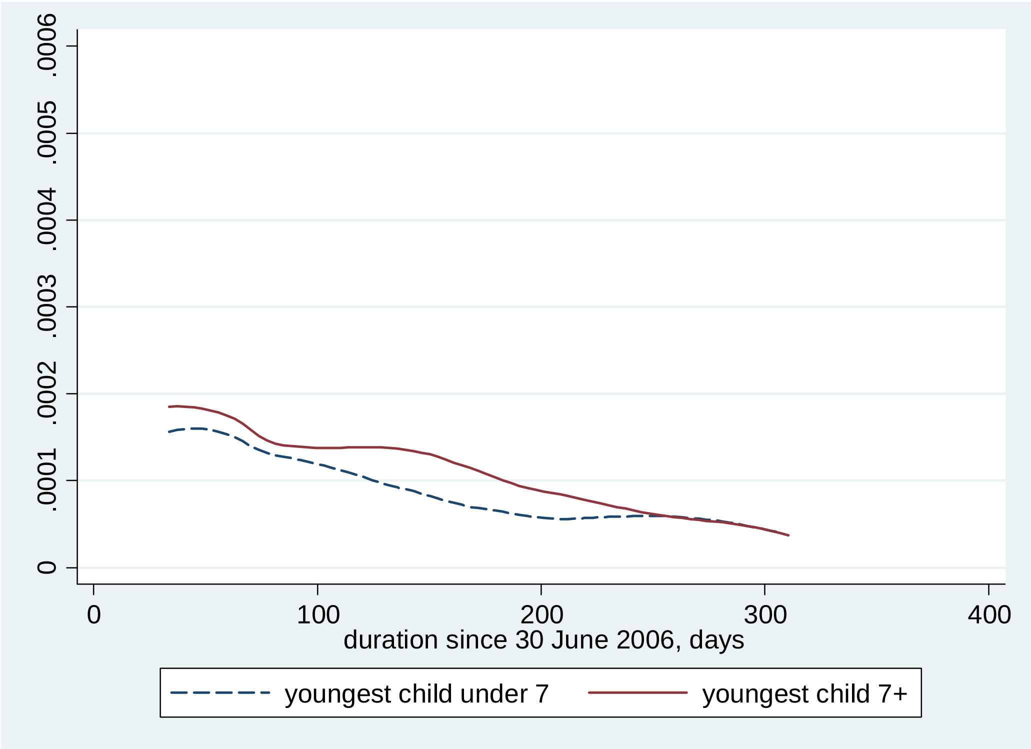
[25] The increase in the post-1st July 2007 hazards for those with youngest child aged under 7 on 30 June 2007 is consistent with some of this group being treated when their youngest child subsequently turns 7, but could also reflect an improving labour market and/or anticipation effects ahead of actual activation. These issues are explored further in Section 7.

Figure 9: Kaplan-Meier Daily Hazard Rates for Exit to Other IS, by Age of Youngest Child on 30 June 2007, PPS Recipients, Duration since 30 June 2007 (After Activation)



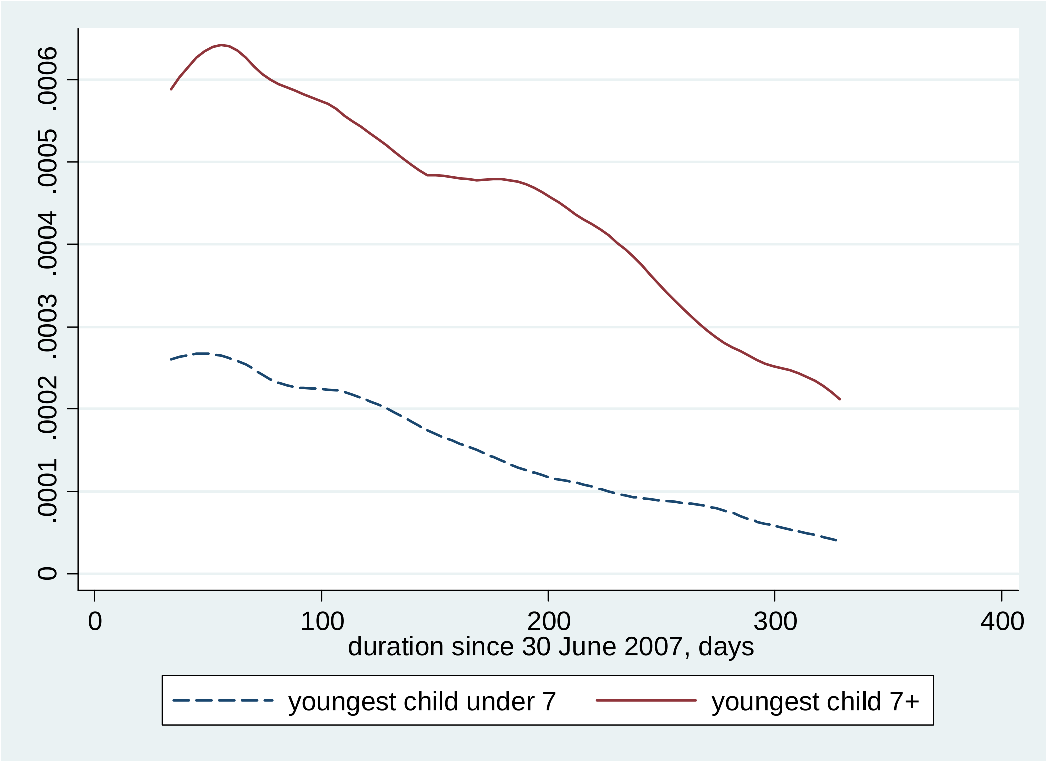
Note: Duration is measured from 30 June 2007 and episodes are treated as right-censored on 30 June 2008.

Figure 10: Kaplan-Meier Daily Hazard Rates for Exit to Other IS, by Age of Youngest Child on 30 June 2006, PPP Recipients, Duration since 30 June 2006 (Before Activation)



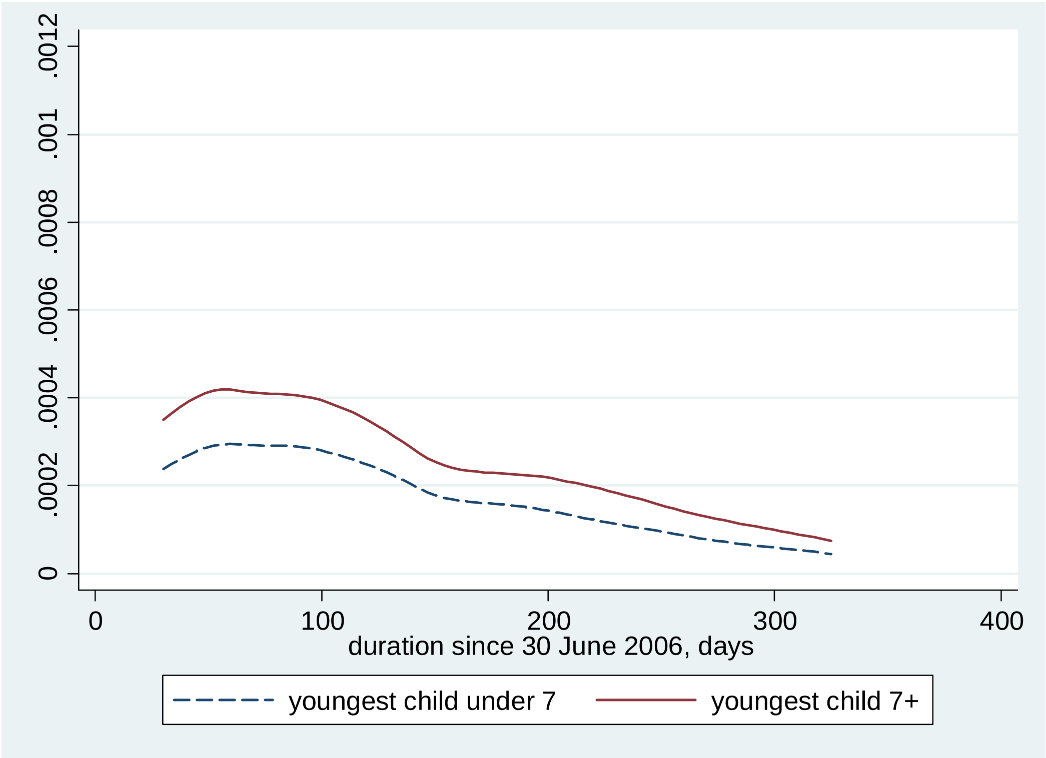
Note: Duration is measured from 30 June 2006 and episodes are treated as right-censored on 30 June 2007.

Figure 11: Kaplan-Meier Daily Hazard Rates for Exit to Other IS, by Age of Youngest Child on 30 June 2007, PPP Recipients, Duration since 30 June 2007 (After Activation)



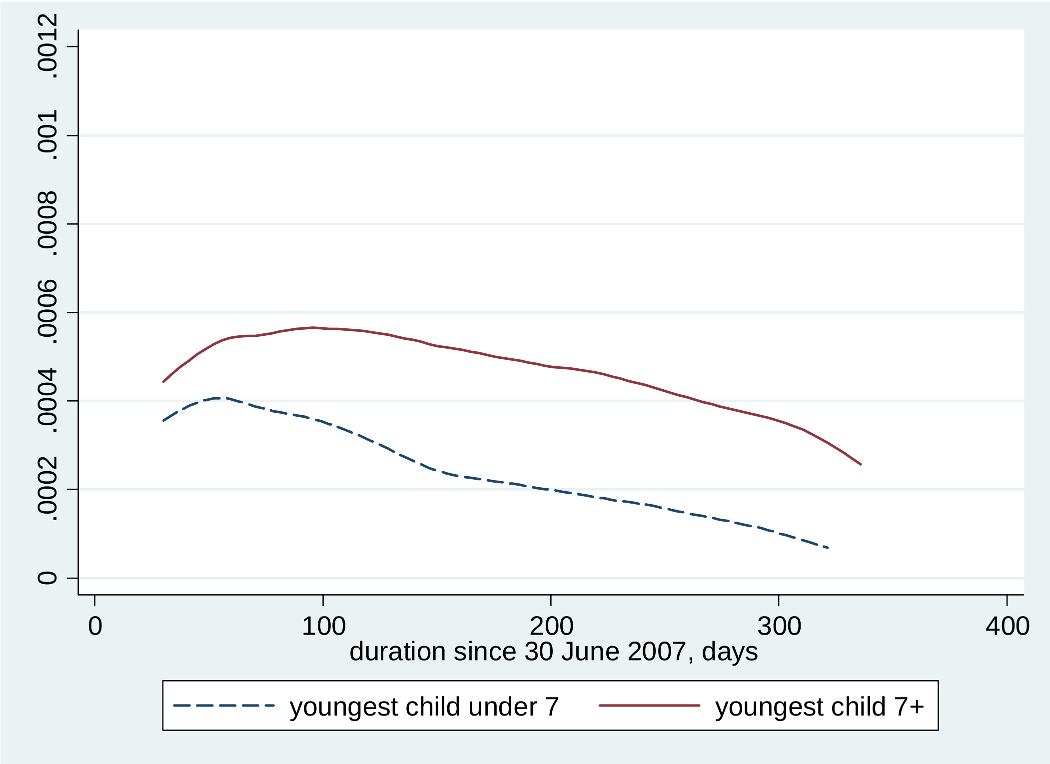
Note: Duration is measured from 30 June 2007 and episodes are treated as right-censored on 30 June 2008.

Figure 12: Kaplan-Meier Daily Hazard Rates for Exit from IS, by Age of Youngest Child on 30 June 2006, PPS Recipients, Duration since 30 June 2006 (Before Activation)



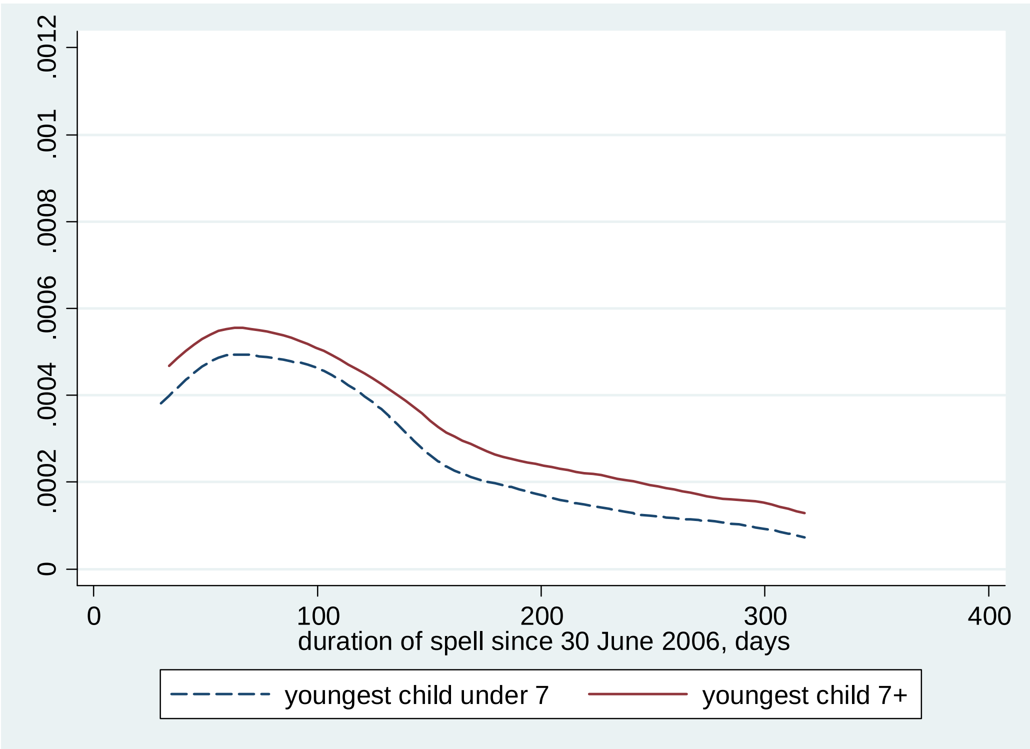
Note: Duration is measured from 30 June 2006 and episodes are treated as right-censored on 30 June 2007.

Figure 13: Kaplan-Meier Daily Hazard Rates for Exit from IS, by Age of Youngest Child on 30 June 2007, PPS Recipients, Duration since 30 June 2007 (After Activation)



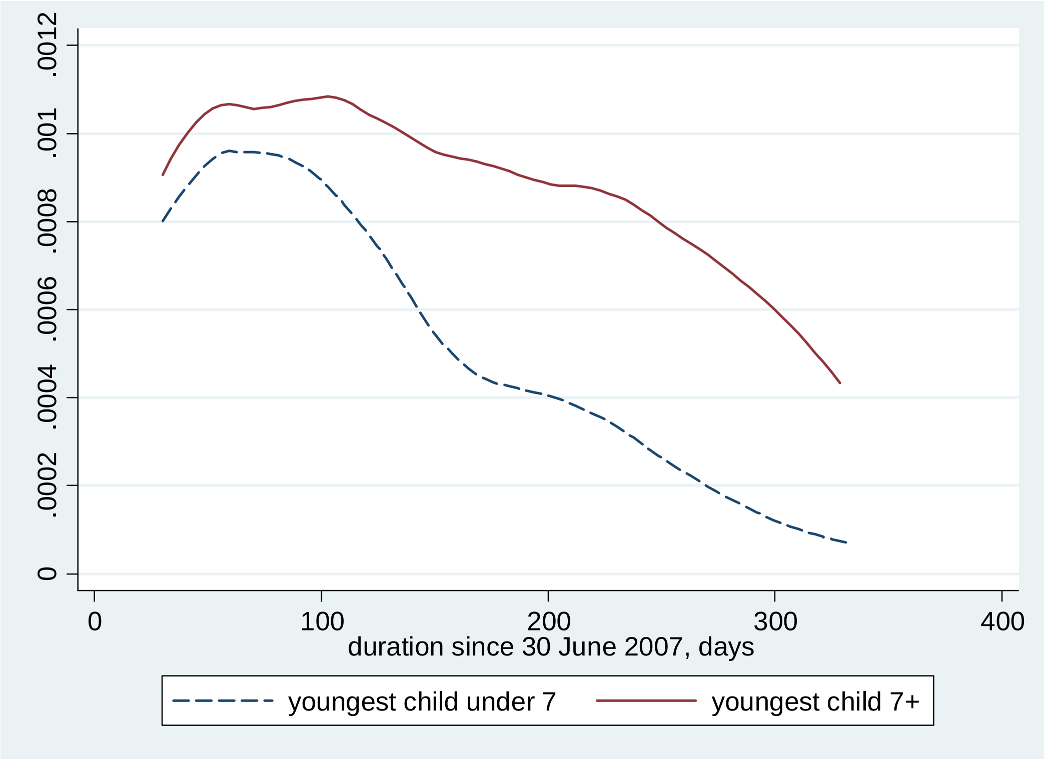
Note: Duration is measured from 30 June 2007 and episodes are treated as right-censored on 30 June 2008.

Figure 14: Kaplan-Meier Daily Hazard Rates for Exit from IS, by Age of Youngest Child on 30 June 2006, PPP Recipients, Duration since 30 June 2006 (Before Activation)



Note: Duration is measured from 30 June 2006 and episodes are treated as right-censored on 30 June 2007.

Figure 15: Kaplan-Meier Daily Hazard Rates for Exit from IS, by Age of Youngest Child on 30 June 2007, PPP Recipients, Duration since 30 June 2007 (After Activation)



Note: Duration is measured from 30 June 2007 and episodes are treated as right-censored on 30 June 2008.

Tables 9-11 give the average durations of completed episodes for the full sample, separately by age of youngest child at the end date of the episode and for episodes ending before and after 1st July 2007. Note that because of the way the sample is constructed, episodes ending after 1st July 2007 are, by definition, longer on average than those ending prior to 1st July 2007, both for those with a youngest child under seven and those with a youngest child aged seven or older. But by comparing the *change* in average durations of completed episodes, before and after 1st July 2007, for the two age groups, we can get a simple unconditional difference-in-differences estimate of the impact of activation – defined here simply as post-1st July 2007 for those with youngest child seven plus – on *completed* PP episode duration. From Table 9 we can see that the average duration of PP episodes completed after 1st July 2007 for those with youngest child under seven is 86% longer than those completed prior to 1st July 2007; whereas for those with youngest child aged seven or older it is 55%. The corresponding unconditional difference-in-differences estimate is therefore that activation has led to or has coincided with a reduction in mean duration of completed PP episodes, for those covered by the new requirements, of 31%. The corresponding unconditional difference-in-differences estimates for PPS and PPP recipients are a reduction of 15% in mean PPS episode duration and a reduction of 49% in mean PPP episode duration.

Table 9: Mean Durations (Standard Deviations) and Exit Rates, All PP, Full Sample, Before and After 1st July 2007 by Age of Youngest Child

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Completed PP episode duration, days	834 (723)	1568 (1118)	1554 (815)	2436 (1117)
Episode duration including right-censored episodes	722 (954)	1171 (1369)	1310 (1058)	1709 (1414)
Proportion of episodes ending within window	13.9%	13.3%	12.9%	22.7%

Notes: Episode durations refer to complete episodes only and are measured in days. 'Episode duration including right-censored episodes' for the period up to 1st July 2007 takes this date as the right-censoring date. The denominator for 'proportion of episodes ending within window' is the total number of episodes.

Table 10: Mean Durations (Standard Deviations) and Exit Rates, PPS Only, Full Sample, Before and After 1st July 2007 by Age of Youngest Child

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Completed PP episode duration, days	983 (760)	1665 (1102)	1610 (829)	2485 (1101)
Episode duration including right-censored episodes	901 (986)	1897 (1123)	1515 (1061)	2547 (1155)
Proportion of episodes ending within window	10.3%	13.3%	10.8%	23.9%

Notes: Episode durations refer to complete episodes only and are measured in days. 'Episode duration including right-censored episodes' for the period up to 1st July 2007 takes this date as the right-censoring date. The denominator for 'proportion of episodes ending within window' is the total number of episodes.

Table 11: Durations and Covariate Sample Means (Standard Deviations), Full Sample, PPP Only, Before and After 1st July 2007 by Age of Youngest Child

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Completed PP episode duration, days	658 (633)	1305 (1110)	1465 (786)	2274 (1155)
Episode duration including right-censored episodes	505 (852)	1706 (1190)	1032 (977)	2379 (1243)
Proportion of episodes ending within window	23.6%	13.3%	18.5%	19.4%

Notes: Episode durations refer to complete episodes only and are measured in days. 'Episode duration including right-censored episodes' for the period up to 1st July 2007 takes this date as the right-censoring date. The denominator for 'proportion of episodes ending within window' is the total number of episodes.

These rough estimates of course come with some caveats. First, we calculate average durations by ignoring all those episodes still ongoing (around half of all episodes), and the picture may look very different were we able to observe *all* episodes to their completion.[26] Second, there are differences between the 'treatment' and 'comparison' groups, not only in terms of episode duration, but also for most observed characteristics, which are here not controlled for (see Tables A1-A3). Although the compositional differences between the two groups appear reasonably stable over time, we cannot entirely rule out different trends over time in the durations of PP episodes between the two groups, which might confound these estimated impacts of activation on mean durations.

Tables 9-11 also report the fraction of episodes that end before and after 30 June 2007 for each of the age-of-youngest-child groups in the full sample. We can use this information in similar fashion to obtain rough, unconditional, difference-in-differences estimates of the impact of activation on the probability of completing a episode by a certain date, although again we must bear in mind that the treatment and comparison groups are different along various dimensions. In this case the suggestion is that activation led to or coincided with an increase in the proportion of episodes ending beyond 30 June 2007 but prior to 30 June 2009 of 9.4 percentage points for the treatment group with a corresponding fall of one percentage point for the comparison group, suggesting a difference-in-differences estimate of a 10.4 percentage point increase in the proportion of episodes ending within the period. The corresponding difference-in-differences estimates for PPS and PPP are 10.1 percentage points and 11.2 percentage points.

On balance the suggestion from both the KM hazard plots and these simple unconditional estimates is that activation has coincided with an increase in the hazard rate for exiting PP,

[26] A similar unconditional difference-in-differences estimate of the impact of activation on episode duration including right-censored episodes, where the right-censoring date is treated as the end date, suggests duration falls by 36% for those covered by the new participation requirements. The equivalent figures for PPS and PPP durations are falls of 34% and 65% respectively.

both to other IS payments and exiting IS altogether, for both PPS and PPP recipients, for those covered by the new requirements. The result is shorter episode PP durations and fewer ongoing episodes relative to those not covered by the new requirements. Activation also appears to have had a larger impact on PPP recipients compared to PPS recipients (we return to this point later). In the following sections we explore these issues further by estimating proportional hazard models for PP exit and PP exits of different types, using both the full and restricted samples, and controlling to a greater degree for observed differences between recipients that receive treatment and those that do not.

7.2. All Grandfathered PP Recipients (the Full Sample)

First consider the single risk hazard model for exits from PP presented in Table 12. Before discussing our estimated treatment effects, it is worth taking a moment to consider the impact of the control variables on the hazard for PP exit, most of which act in the expected directions (see e.g. van den Berg et al., 2004). For all PP exits (column 1), fathers have higher hazard rates than mothers; older parents have higher hazards than younger parents, perhaps because their children are themselves older; those with pre-school age children have lower hazards; immigrant parents have marginally lower hazards than native born parents; those with more children have lower hazards; and those in higher unemployment areas have lower hazards. Elapsed duration in the current episode prior to being at risk on the 30 June 2006 is positively related to the hazard, implying that those starting the current episode earlier have higher hazards following 30th June 2006 than those starting the current episode later. Our interpretation of this is that it is capturing the effect of the automatic cut-off for PP when the youngest child turns sixteen. These patterns are similar for PPP and PPS recipients estimated separately, although the gap in hazards between immigrant and native born parents is considerably wider for PPP recipients than for PPS recipients.

Now consider the estimated treatment effects in Table 12. When the model is estimated on all grandfathered PP recipients, the coefficient on the post-interview dummy is large, positive and highly statistically significant, with the hazard 20 percent higher following the interview than prior to the interview, other things being equal.[27] This suggests a larger impact in the Australian case than that found, for example, by Cebulla et al. (2008) for the introduction of WFIs in the UK. Our explanation is that the Australian WtW reforms for this group represent a more substantial increase in conditionality than was the case for the UK reforms studies by Cebulla et al. (2008), where there were no further requirements beyond interview attendance. As discussed in Section 5, we cannot rule out that these estimates suffer from selection and other biases, but if we accept that the most likely direction for such biases is downwards, then the implication is that activation increases the hazard rate for exit from PP by at least 20 percent. Note that this is somewhere in between the two unconditional difference-in-differences estimates – on mean duration and on exit rates – presented in Tables 9-11.

[27] This is robust to conditioning on reaching 30th June 2007 (rather than 30th June 2006) and to estimating on females only. When the post-interview dummy is replaced by the post-agreement dummy, the estimated treatment effect is small but still positive and statistically significant. The reduction in the size of the estimated treatment effect is likely to reflect a combination of measurement error in the Activity Agreement dummy and additional selection bias introduced by the impact of attending the interview for those don't immediately sign an Activity Agreement.

Table 12: Cox Proportional Hazard Model, Single Risk (All Exits from PP), Full Sample, Coefficients (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Post-interview	.198***	.320***	.164***
	(.019)	(.038)	(.022)
Male	.322***	.272***	.298***
	(.022)	(.040)	(.027)
Age of parent	.014***	.007***	.014***
	(.001)	(.002)	(.001)
Child under 6	-.171***	-.285***	-.256***
	(.020)	(.036)	(.024)
Immigrant parent	-.022	-.277***	-.056***
	(.015)	(.026)	(.018)
Number of children	-.139***	-.088***	-.245***
under 16 years	(.008)	(.012)	(.011)
LFSR unemployment	-.041***	-.082***	-.039***
rate, %	(.005)	(.010)	(.006)
Past IS duration	-.002	-.004	<-.000
	(.002)	(.004)	(.002)
Elapsed duration of	.147***	.144***	.146***
current episode prior to 30 June 2006, years	(.010)	(.019)	(.012)
No. Individuals	46947	11015	35932
No. Failures	25466	7228	18238
Log likelihood	-226495	-52952	-157837

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The full sample is all those in the grandfathered cohort currently in receipt of PP. Returners to PP after 30 June 2006 are omitted. The post-interview dummy takes the value 1 for all episodes or parts of episodes following an activation interview and 0 otherwise. The child under 6 dummy is equal to one for those with a youngest child aged 0-5 years and 0 otherwise. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) as is elapsed duration of current episode. Results are presented in coefficient form, i.e. the βs, α and δ from Equation (1), and are interpretable as semi-elasticities. Robust standard errors in parentheses.

Estimating the model separately on PPS and PPP recipients suggests the positive impact is common to both payment types, although the impact appears to be larger for PPP recipients compared to PPS recipients (columns 2 and 3 of Table 12).[28] This is consistent with the evidence presented in Section 7.1. PPP recipients may respond more strongly to activation than for PPS recipients for a number of reasons. First, working 15 hours per week in paid employment is more likely to render a PPP recipient ineligible for PPP on income grounds than is the case for PPS recipients. Second, although increased participation requirements may make PP less attractive for both PPS and PPP recipients, PPP recipients may be better able to compensate at a household level for lost PP income (if they exit) by increasing partner income, e.g. through increased earnings. There are also compositional differences between PPS and PPP recipients, e.g. in age and migrant status, which could drive differences between the groups in the average impact of activation, although this could work in either direction. It is unlikely that the stronger PPP impact is driven entirely by differential selection bias; if

[28] For PPP recipients the coefficient is smaller but still positive and statistically significant when the post-interview dummy is replaced by the post-agreement dummy. For PPS recipients it is zero.

anything, we would expect a larger downwards bias for PPP recipients given shorter average PP durations. Nevertheless, we cannot entirely rule out differential biases here.[29]

Tables 13 and 14 repeat the exercise for the competing risks hazards of leaving PP for another IS payment (this includes switching between PPP and PPS and vice-versa) and leaving IS altogether (leaving PP not to another IS payment).[30] As in the single risk case, controls influence the hazards largely in the expected directions: males have higher hazards for both types of exit; older parents have higher hazards for both types of exit; those with pre-school age children have higher hazards for exits to other IS payments but lower hazards for exit from IS; immigrants have lower hazards for exits from IS; those with more children have lower hazards for both types of exit; and higher unemployment rates are associated with higher hazards to other IS payments but lower hazards for exiting IS. Duration of previous IS episodes is positively related to the hazard for switches between IS payments but negatively related to exits from IS. Finally, elapsed duration in the current episode has a strong positive influence on the hazard for exit to other IS payments but no influence on the hazard for exit from IS. There are some differences in the magnitudes and in some cases the statistical significance of the estimated impacts of the controls between PPP and PPS recipients, but not in signs.

For both PPS and PPP recipients there is a large, positive and highly statistically significant impact from attending the interview on the hazard for exits to other IS payments, with respective increases of 64% and 67% (see Table 13).[31] Note the similarity of the estimated impacts for PPS and PPP in this case, in contrast to the single risk estimates discussed above. The implication is that the larger single risk impact for PPP recipients relative to PPS recipients is being driven by exits from IS rather than switches between IS payments. As in the single risk case we cannot rule out that these estimates suffer from selection and other biases. The direction of these biases is less clear, however, and unobserved characteristics that make single risk exit from PP less likely may make exit from PP to other IS payments more likely, so we place no lower bound interpretation on these estimates. Nevertheless, the suggestion is that the requirement to engage in 15 hours per week of work or work related activity has a significant 'benefit-shift' impact, including to other IS payments that are not conditioned on participation such as DSP. The finding that tightening conditionality of one part of the welfare system shifts some claimants to other parts of the welfare system has been found in other contexts (see e.g. McVicar, 2008) as well as in the earlier evaluation of the 2006 Australian WtW reforms (DEEWR, 2008). Of course those moving to NSA or other 'active' IS payments may subsequently be more likely to exit IS than would otherwise have been the case, but those moving to DSP may be less so.

For exits from IS the picture is more mixed. For PPP recipients but perhaps not for PPS recipients there is a positive and statistically significant impact post-interview on the hazard, although smaller than in the single risk case.[32] Again there are likely to be selection and other

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[29] For example, there is little difference between the PPP and PPS estimates when conditioning on reaching 30th June 2007 rather than 30th June 2006.

[30] The most common switch is between the two different PP payments (3066), closely followed by switches to NSA (2853). There are also 1304 switches from PP to DSP and 1355 switches from PP to other IS payments in the ten percent sample.

[31] These estimates are not robust to replacing the post-interview dummy with the post-agreement dummy, however, with the latter suggesting zero impact of activation. They are also smaller in magnitude, although still statistically significant, when the hazards are conditioned on reaching 30th June 2007.

[32] In this case the treatment effect appears slightly larger, and statistically significant for both PPP and PPS, when we replace the post-interview dummy with the post-agreement dummy. The estimated treatment effect is

biases as discussed in Section 5, most likely acting in a downwards direction as in the single risk case, in which case these will be lower bounds on the actual treatment effects. And again, the difference in the impact of activation for PPP and PPS recipients could reflect tighter income tests for PPP recipients, better 'outside options' for PPP recipients, compositional differences between the two groups or differential biases. Overall, Tables 12-14 suggest both benefit shift and exits from IS have contributed to the positive impact of activation on the single risk hazard, and therefore to falls in PP caseload over time.

Table 13: Cox Proportional Hazard Model, Exits to Other IS, Full Sample, Coefficients (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Post-interview	.646***	.635***	.669***
	(.039)	(.075)	(.046)
Male	.235***	-.016	.327***
	(.043)	(.083)	(.051)
Age of parent	.018***	.006*	.020***
	(.002)	(.004)	(.002)
Child under 6	.557***	.027	.684***
	(.036)	(.070)	(.042)
Immigrant parent	.057**	-.136***	-.001
	(.027)	(.047)	(.035)
Number of children	-.175***	-.197***	-.226***
under 16 years	(.015)	(.024)	(.020)
LFSR unemployment	.053***	.005	.057***
rate, %	(.009)	(.018)	(.011)
Past IS duration	.064***	.073***	.065***
	(.003)	(.006)	(.003)
Elapsed duration of	.552***	.405***	.607***
current episode prior to 30 June 2006, years	(.020)	(.037)	(.023)
No. Individuals	46947	11015	35932
No. Failures	7028	1997	5031
Log likelihood	-62189	-14513	-43300

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The full sample is all those in the grandfathered cohort currently in receipt of PP. Returners to PP after 30 June 2006 are omitted. The post-interview dummy takes the value 1 for all episodes or parts of episodes following an activation interview and 0 otherwise. The child under 6 dummy is equal to one for those with a youngest child aged 0-5 years and 0 otherwise. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) as is elapsed duration of current episode. Results are presented in coefficient form, i.e. the βs, α and δ from Equation (1), and are interpretable as semi-elasticities. Robust standard errors in parentheses.

of similar magnitude, positive and statistically significant for both PPP and PPS recipients when the hazards are conditioned on reaching 30th June 2007.

Table 14: Cox Proportional Hazard Model, Exits from IS, Full Sample, Coefficients (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Post-interview	.059***	.211***	.010
	(.022)	(.044)	(.025)
Male	.359***	.366***	.296***
	(.026)	(.045)	(.031)
Age of parent	.011***	.006***	.010***
	(.001)	(.002)	(.001)
Child under 6	-.446***	-.400***	-.631***
	(.023)	(.042)	(.029)
Immigrant parent	-.052***	-.334***	-.078***
	(.017)	(.030)	(.021)
Number of children under 16 years	-.125*** (.009)	-.048*** (.013)	-.257*** (.013)
LFSR unemployment rate, %	-.077*** (.006)	-.115*** (.011)	-.076*** (.007)
Past IS duration	-.031***	-.035***	-.029***
	(.002)	(.004)	(.003)
Elapsed duration of current episode prior to 30 June 2006, years	.015 (.012)	.056** (.023)	-.001 (.014)
No. Individuals	46947	11015	35932
No. Failures	18438	5231	13207
Log likelihood	-163494	-38277	-113782

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The full sample is all those in the grandfathered cohort currently in receipt of PP. Returners to PP after 30 June 2006 are omitted. The post-interview dummy takes the value 1 for all episodes or parts of episodes following an activation interview and 0 otherwise. The child under 6 dummy is equal to one for those with a youngest child aged 0-5 years and 0 otherwise. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) as is elapsed duration of current episode. Results are presented in coefficient form, i.e. the βs, α and δ from Equation (1), and are interpretable as semi-elasticities. Robust standard errors in parentheses.

7.3. Grandfathered Parents of 6/7 Year Olds (the Restricted Sample)

First consider the single risk hazard model for exits from PP presented in Table 15. Fewer controls are statistically significant in this case, given the smaller sample size, but again they largely take expected signs as in the full sample case: males have higher hazards; older parents have marginally lower hazards for PPS (note that this no longer captures age of youngest child effects given the restricted sample); immigrants have lower hazards but only for PPP recipients; number of children is marginally negatively related to the hazard for PPS recipients; and the unemployment rate is negatively related to the hazard. The main contrast with the full sample estimates is that elapsed duration in the current episode prior to being at risk is negatively related to the hazard. Our interpretation is that this now picks up the standard finding of a downward sloping hazard function for welfare exit, given that nobody in the restricted sample exits PP because their youngest child turns 16 unlike in the case of the full sample. Hazards are also lower for those in the treatment group, given activation status, perhaps also capturing a negative duration dependence or selection effect. The zero coefficients on the dummy for youngest child turning seven, both here and in the competing risks estimates, can be interpreted as placebo tests: for those in the comparison group this has no impact on either the single risk hazard or the competing risks hazards.

Table 15: Cox Proportional Hazard Model, Single Risk (All Exits from PP), Restricted Sample, Coefficients (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Activation	.639***	.882***	.511***
	(.104)	(.183)	(.127)
Treatment group	-.232***	-.195	-.204**
	(.075)	(.134)	(.091)
Youngest child 7	-.021	-.034	.003
years old	(.076)	(.131)	(.093)
Male	.234***	.135	.144
	(.083)	(.127)	(.111)
Age of parent	-.007*	.006	-.017***
	(.004)	(.007)	(.005)
Immigrant parent	-.020	-.390***	-.026
	(.060)	(.097)	(.079)
Number of children	.021	.003	-.066*
under 16 years	(.026)	(.040)	(.037)
LFSR unemployment	-.054***	-.079**	-.053**
rate, %	(.020)	(.036)	(.024)
Past IS duration,	-.007	-.007	-.006
years	(.007)	(.014)	(.009)
Elapsed duration of	-.145***	-.140***	-.142***
current episode prior to 30 June 2006 (control group) and 30 June 2007 (treatment group), years	(.011)	(.018)	(.014)
No. Individuals	6490	1486	5004
No. Failures	1552	517	1035
Log (pseudo)likelihood	-13284	-3603	-8616

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The restricted sample combines those with a youngest child aged 6 years on the 30th June 2006 (control group) and those with a youngest child aged 6 years on 30th June 2007 (treatment group). Returners to PP after 30 June 2006 are omitted. The treatment group dummy is equal to 1 for those in the latter group and 0 for those in the former group. The youngest child aged 7 dummy is equal to one for those with a youngest child aged 7 years and 0 otherwise. Activation is a binary dummy equal to the product of the treatment group and youngest child aged 7 dummies. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) as is elapsed duration of current episode. Results are presented in coefficient form, i.e. the βs, γs and δs from Equation (2), and are interpretable as semi-elasticities. Robust standard errors in parentheses.

Turning to the estimated treatment effects, the evidence suggests a large, positive and highly statistically significant impact on the hazard rate for exit from PP for both PPS and PPP recipients. These are roughly three times the magnitude of those estimated for the full sample. One explanation for the contrast in magnitudes is that possible biases due to differential trends, selection and/or measurement error are likely to be smaller in the restricted sample case, in which case the estimates in Table 15 are closer to capturing the actual treatment effect on the treated. On the other hand, it might be that treatment effects are larger for those with seven year old children compared to those with older children. Precise magnitudes and possible biases aside, however, for our purposes the bottom line is again that activation appears to increase the hazards for both PPS and PPP recipients, again with an apparently larger impact on PPP recipients compared to PPS recipients.

Now consider the competing risks estimates presented in Tables 16 and 17. As in the full sample case, the estimated treatment effect on exits to other IS payments is again positive, highly statistically significant and larger in magnitude that the estimated treatment effect on exits from IS.[33] As in the single risk case, the treatment effect appears stronger for PPP recipients compared to PPS recipients, at least for exits from IS. But unlike in the full sample approach, the evidence suggests a positive and statistically significant impact on activation on exits from IS for both PPP and PPS recipients. Again the likelihood is that we are picking up a positive treatment effect on the hazard that is either masked in the full sample case by larger selection and other biases, or that is stronger for those with seven year old children compared to those with older children.

These estimates are largely robust to including a dummy for anticipation effects – equal to one for the three months prior to the 7th birthday of the child for those in the treatment group and zero otherwise – although some fall slightly in magnitude. The anticipation dummy itself is insignificant in all cases, suggesting that parents are not exiting PP in anticipation of activation. They are also robust to estimating on females only and to extending the sample to include those with youngest child aged 6 on 30th June 2008, censored at 30th June 2009.

33 Although the *proportional* impact of activation on exits to other IS payments is larger than that on exits from IS, because the baseline hazard for such exits is lower than that for exits from IS (see Figures 8-15), the suggestion is that exits to other IS payments constitute no more than half the suggested *absolute* impact in terms of number of individuals exiting their current PP payment as a result of activation.

Table 16: Cox Proportional Hazard Model, Exit to Other IS, Restricted Sample, Coefficients (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Activation	1.14***	1.23***	1.07***
	(.223)	(.362)	(.282)
Treatment group	-.489***	-.473*	-.446**
	(.171)	(.285)	(.217)
Youngest child 7 years old	-.057	-.064	-.027
	(.163)	(.261)	(.210)
Male	.231	-.208	.386*
	(.174)	(.283)	(.218)
Age of parent	.004	.007	-.004
	(.010)	(.017)	(.013)
Immigrant parent	.252**	.049	.061
	(.120)	(.183)	(.175)
Number of children under 16 years	.098*	-.047	.083
	(.054)	(.083)	(.078)
LFSR unemployment rate, %	.050	.046	.027
	(.042)	(.071)	(.053)
Past IS duration	.101***	.114***	.107***
	(.012)	(.027)	(.014)
Elapsed duration of current episode prior to 30 June 2006 (control group) and 30 June 2007 (treatment group), years	-.039*	-.008	-.048*
	(.022)	(.034)	(.029)
No. Individuals	6490	1486	5004
No. Failures	358	136	222
Log (pseudo)likelihood	-3029	-944	-1821

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The restricted sample combines those with a youngest child aged 6 years on the 30th June 2006 (control group) and those with a youngest child aged 6 years on 30th June 2007 (treatment group). Returners to PP after 30 June 2006 are omitted. The treatment group dummy is equal to 1 for those in the latter group and 0 for those in the former group. The youngest child aged 7 dummy is equal to one for those with a youngest child aged 7 years and 0 otherwise. Activation is a binary dummy equal to the product of the treatment group and youngest child aged 7 dummies. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) as is elapsed duration of current episode. Results are presented in coefficient form, i.e. the βs, γs and δs from Equation (2), and are interpretable as semi-elasticities. Robust standard errors in parentheses.

Table 17: Cox Proportional Hazard Model, Exit from IS, Restricted Sample, Coefficients (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Activation	.482***	.758***	.346**
	(.118)	(.213)	(.143)
Treatment group	-.166**	-.095	-.151
	(.083)	(.152)	(.100)
Youngest child 7 years old	-.006	-.017	.015
	(-.086)	(.154)	(.103)
Male	.243***	.215	.081
	(.094)	(.145)	(.129)
Age of parent	-.012***	.004	-.023***
	(.005)	(.08)	(.006)
Immigrant parent	-.103	-.551***	-.052
	(.068)	(.115)	(.088)
Number of children under 16 years	-.001	.019	-.110***
	(.031)	(.045)	(.041)
LFSR unemployment rate, %	-.084***	-.130***	-.073***
	(.023)	(.043)	(.027)
Past IS duration, years	-.046***	-.049***	-.048***
	(.008)	(.016)	(.010)
Elapsed duration of current episode prior to 30 June 2006 (control group) and 30 June 2007 (treatment group), years	-.173***	-.188***	-.170***
	(.013)	(.022)	(.016)
No. Individuals	6490	1486	5004
No. Failures	1194	381	813
Log (pseudo)likelihood	-10186	-2633	-6742

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The restricted sample combines those with a youngest child aged 6 years on the 30th June 2006 (control group) and those with a youngest child aged 6 years on 30th June 2007 (treatment group). Returners to PP after 30 June 2006 are omitted. The treatment group dummy is equal to 1 for those in the latter group and 0 for those in the former group. The youngest child aged 7 dummy is equal to one for those with a youngest child aged 7 years and 0 otherwise. Activation is a binary dummy equal to the product of the treatment group and youngest child aged 7 dummies. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) as is elapsed duration of current episode. Results are presented in coefficient form, i.e. the βs, γs and δs from Equation (2), and are interpretable as semi-elasticities. Robust standard errors in parentheses.

Finally, we replace the treatment dummy based on the child's seventh birthday with the post-interview dummy used in the full sample approach. Results are presented in Table A7 (although not separately by payment type given sample size concerns for the PPP group when we split exit types). The evidence again suggests a large, positive and statistically significant effect of activation on the single risk hazard, but the magnitude of this effect is more in line with that of the equivalent estimate for the full sample, i.e. around a 20 percent increase in the single risk hazard post-interview. Further, this appears to be almost entirely driven by exits to other IS payments, rather than exits from IS, again more in line with the results for the full sample approach. The suggestion is that selection biases are not substantially worse in the full sample relative to the restricted sample, and that measurement error in the post-interview dummy may impart negative bias in both cases.[34]

8. Impacts of Activation on Participation in Paid Work and Work-related Activities

This section addresses the two remaining research questions:

- *What impact has activation had on participation in paid work and on hours worked for those covered by the new participation requirements?*

- *What impact has activation had on participation in other activation activities, e.g. job search, for those covered by the new participation requirements?*

The analysis begins with simple unconditional difference-in-differences estimates, as in Section 7, drawing on both RED and LPS data. We then move on to more sophisticated estimates, drawing primarily on RED data but also, to test robustness and widen the scope of the analysis, on LPS data.

First consider Table 18 which reports participation rates in paid employment, hours for those working, and participation in job search and related activities at six monthly intervals spanning activation, separately by age of youngest child grouping and by payment type. As we'd expect those with older children are more likely to be in paid employment and likely to work longer hours than those with younger children. For job search and related activities the pattern is the opposite, partly due to lower job search participation amongst those already working. Also note again the higher levels of paid employment among PPS recipients relative to PPP recipients.

We can use these data to derive simple unconditional difference-in-differences estimates of the impact of activation on behaviour, similar to the analysis in Section 7.1. Here we do so by averaging participation rates for each age-based group across the three pre-activation data points and the three post-activation data points (assuming activation takes place in the second half of 2007); taking the difference of the post activation and pre activation averages, and taking the difference of the differences between the seven plus group and the under seven group. The resulting estimates suggest that activation leads to or coincides with an increase in participation in paid employment of 11.1 percentage points for PPP recipients and 9.0 percentage points for PPS recipients, an increase in those working that work 15+ hours per week of 12.6 percentage points for PPP recipients and 3.9 percentage points for PPS recipients, and a decrease in job search and related activities of 6.6 percentage points for PPP recipients and 11.3 percentage points for PPS recipients. In other words, these simple estimates suggest a large positive impact on both employment and hours for PPP recipients and some increase for PPS recipients. The apparent negative impact on job search again might reflect lower levels of job search for those in employment.

[34] For example, ten percent of individuals in the treatment group in the restricted sample whose youngest child had turned seven have no recorded interview date.

Table 18: Trends in Behaviour for PP Recipients, by Age of Youngest Child and Payment Type, Ten Percent RED Sample

		30/06/06	31/12/06	30/06/07	31/12/07	30/06/08	31/12/08
				PPP			
% in paid employment	<7	10.9%	9.7%	11.4%	10.0%	11.1%	8.8%
	7+	19.8%	18.3%	23.6%	27.7%	35.0%	30.1%
% of those in employment working 15+ hours per week	>7	64.6%	67.3%	70.5%	66.0%	71.4%	65.3%
	7+	72.0%	70.2%	76.9%	80.0%	89.3%	87.7%
% job searching or related	<7	55.9%	57.1%	57.9%	58.1%	59.0%	59.5%
	7+	42.3%	46.9%	53.5%	54.1%	34.7%	39.8%
				PPS			
% in paid employment	<7	26.4%	24.7%	27.2%	24.7%	26.9%	21.7%
	7+	46.7%	44.0%	51.1%	52.0%	59.4%	52.5%
% of those in employment working 15+ hours per week	>7	78.4%	77.5%	81.0%	79.1%	82.8%	78.8%
	7+	82.3%	82.8%	87.8%	87.0%	92.1%	89.2%
% job searching or related	<7	64.3%	65.9%	66.6%	67.1%	67.5%	67.4%
	7+	44.9%	49.0%	55.0%	55.9%	28.4%	35.7%

Tables 19 and 20 present similar data for the LPS. As previously noted, in the case of the LPS the sample is not restricted to those remaining on PP, and only around half of those on PP in wave 1 remain on PP at wave 5. There is also additional information on participation in education and training which allows additional unconditional difference-in-differences estimates of the impact of activation on education and training participation for each payment type. The corresponding difference-in-differences estimates for PPP (PPS), in this case averaging over waves 1-3 and 4-5, are an increase in participation in employment of 7.5 (1.1) percentage points, an increase in the proportion of those working that work 15+ hours per week of 1.9 (3.1) percentage points, no change in the proportion of those job searching for PPP (but a fall of 1.9 percentage points for PPS), and a fall of 1.9 percentage points in participation in education and training for PPP (an increase of 0.3 percentage points for PPS). Note the apparent employment and hours effects are considerably smaller than those implied by the RED, which to some extent is likely to reflect the fact that over a third of those in the older age category will not be treated because they have already left PP. Also bear in mind the usual caveats with the LPS of small sample size, particularly when broken down by payment type, and high rates of attrition.

Table 19: Trends in Benefit Status and Behaviour by Wave, Grandfathered PPP Recipients in LPS Wave 1, by Age of Youngest Child

		Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
% on PP	<7	100.0	81.3	73.6	64.8	62.6
	7+	100.0	74.5	52.3	47.2	34.6
% on IS	<7	100.0	83.7	76.4	70.4	66.5
	7+	100.0	79.0	62.8	60.2	47.3
% Working	<7	14.2	25.0	29.5	35.2	40.1
	7+	31.4	45.2	53.6	65.0	66.4
% of those working	<7	45.8	60.2	56.6	64.5	65.8
on 15+ hours per week	7+	51.7	74.7	73.2	72.5	86.3
% job searching	<7	12.7	12.7	15.5	12.5	14.3
	7+	23.8	18.5	20.3	19.5	22.7
% in	<7	6.4	3.9	5.8	3.2	6.0
education/training	7+	11.5	3.2	5.2	4.1	3.6
Number individuals	<7	416	332	258	216	182
Number individuals	7+	185	157	153	123	110
Total number of individuals		601	489	411	339	292

Table 20: Trends in Benefit Status and Behaviour by Wave, Grandfathered PPS Recipients in LPS Wave 1, by Age of Youngest Child

		Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
% on PP	<7	100.0	93.3	88.1	82.8	80.4
	7+	100.0	84.8	72.7	65.1	54.6
% on IS	<7	100.0	93.9	88.9	83.8	81.5
	7+	100.0	89.2	78.9	74.0	66.3
% Working	<7	26.4	37.7	39.6	45.2	45.8
	7+	56.9	66.5	70.8	76.0	77.3
% of those working	<7	60.3	62.6	69.3	66.9	68.9
on 15+ hours per week	7+	75.4	75.2	78.3	81.4	84.9
% job searching	<7	17.4	17.9	19.7	14.7	16.5
	7+	26.8	23.7	23.8	21.8	18.6
% in	<7	12.6	6.1	7.8	5.7	9.6
education/training	7+	10.3	5.9	10.8	7.4	8.9
Number individuals	<7	677	525	386	314	260
Number individuals	7+	478	388	370	312	291
Total number of individuals		1155	913	756	626	551

Now we turn to the probit estimates (see Equation (3) in Section 5.2) on the constructed RED panel, controlling as far as possible for other influences on behaviour. Recall from Section 5 that the constructed RED panel is the panel-format equivalent of the restricted RED sample analysed in Section 7.3, i.e. it tracks the behaviour of parents of youngest children aged six, turning seven, over a period of one year, for a comparison group in the year from July 2006-June 2007 and a treatment group in the year July 2007-June 2008.

First consider the probit model for entering employment, estimated on the sample of those not in employment in wave A (prior to activation) (Table 21). As in the duration models presented in Section 7, observed characteristics impact on the probability of becoming employed largely in the expected directions. For example, immigrant parents have lower probabilities of taking up employment, as do those with more children, living in higher unemployment areas, and with a longer history of IS claiming. In contrast with the earlier analysis for PP exit, however, males and older parents are less likely to enter employment, although in the former case the coefficient is only statistically significant at the 90 percent level and in the latter case the effect is of very small magnitude.

Table 21: Probit Model for Having Entered Paid Employment One Year On for Those Still on PP, Restricted Sample, Marginal Effects (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Treatment Group	.024*	.045*	.015
	(.015)	(.027)	(.017)
Male	-.054*	-.065	-.044
	(.029)	(.055)	(.034)
Age of parent	-.004***	.002	-.005***
	(.001)	(.002)	(.001)
Immigrant parent	-.063***	-.117***	-.012
	(.018)	(.035)	(.021)
Number of children under 16 years	-.026*** (.007)	-.025* (.013)	-.019** (.009)
LFSR unemployment rate, %	-.016*** (.006)	-.019* (.012)	-.015** (.007)
Past IS duration (including current episode prior to wave A, years	-.007*** (.002)	.003 (.004)	-.010*** (.002)
No. Individuals	3411	859	2552
Pseudo R^2	.021	.037	.017
Log (pseudo)likelihood	-1775	-338	-1414

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The restricted sample combines those with a youngest child aged 6 years on the 30th June 2006 (control group) and those with a youngest child aged 6 years on 30th June 2007 (treatment group). Returners to PP after 30 June 2006 are omitted. The treatment dummy is equal to 1 for those in the latter group and 0 for those in the former group. Other covariates are lagged one year, i.e. wave 1 for the comparison group and wave 3 for the treatment group. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) and includes elapsed duration in current episode prior to wave 1 (control) or wave 3 (treatment) <double check this>. Results are presented as marginal effects, calculated at sample means or at zero as appropriate, and show the percentage increase in the probability of remaining on PP of a one-unit change in the relevant variable. Robust standard errors in parentheses.

Turning to the estimated treatment effects – here defined as having a child aged seven plus subsequent to 1st July 2007 – we see a small positive impact on the probability of entering employment for those still receiving PP of 2.4 percentage points. This is smaller than the unconditional difference-in-differences estimate discussed above, and is only statistically significant at the 90 percent level. The suggestion is that part of that earlier estimate may be explained by changes in the composition of the treatment and comparison groups which are here controlled for by inclusion of the observed parent characteristics, and the lack of robustness in terms of magnitude, if not direction, suggests a need for caution in drawing conclusions. The corresponding estimates for PPP and PPS recipients are an increase of 4.5 percentage points and zero increase, respectively. As for the analysis of exit rates and duration, activation appears to have had a larger impact on PPP recipients than on PPS recipients, although again the impact even for PPP recipients is only marginally significant. In this case, it is less intuitive that PPP recipients would react to activation more strongly than PPS recipients. On the one hand, PPP recipients may be able to rely on a partner to provide childcare if they take up part time work. On the other hand, tighter income eligibility conditions for PPP recipients could plausibly make part time work less attractive than for PPS recipients.

Table 22: Probit Model for Increased Hours Worked to 15+ Hours Per Week One Year On, for Those Working and Still on PP, Restricted Sample, Marginal Effects (Standard Errors)

	All Grandfathered PP Recipients
Treatment Group	.112**
	(.049)
Male	.078
	(.131)
Age of parent	-.001
	(.004)
Immigrant parent	-.028
	(.064)
Number of children under 16 years	-.022
	(.029)
LFSR unemployment rate, %	-.043**
	(.018)
Past IS duration (including current episode	-.007
prior to wave A, years	(.008)
No. Individuals	442
Pseudo R^2	.024
Log (pseudo)likelihood	-297

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The restricted sample combines those with a youngest child aged 6 years on the 30th June 2006 (control group) and those with a youngest child aged 6 years on 30th June 2007 (treatment group). Returners to PP after 30 June 2006 are omitted. The treatment dummy is equal to 1 for those in the latter group and 0 for those in the former group. Other covariates are lagged one year, i.e. wave 1 for the comparison group and wave 3 for the treatment group. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) and includes elapsed duration in current episode prior to wave 1 (control) or wave 3 (treatment) <double check this>. Results are presented as marginal effects, calculated at sample means or at zero as appropriate, and show the percentage increase in the probability of remaining on PP of a one-unit change in the relevant variable. Robust standard errors in parentheses.

Now consider the probit model for increasing hours worked to 15+ hours per week for those previously working fewer than 15 hours per week (Table 22). In this case, we do not estimate separately for PPP and PPS recipients given small sample size for the PPP group working fewer than 15 hours in wave A. We find a large, statistically significant, positive impact on the probability of increasing hours for those still receiving PP of 11.2 percentage points.

Again the lack of robustness in terms of magnitude, if not direction – this estimate is larger than the corresponding unconditional estimate discussed above – suggests a need for caution in drawing conclusions.

Table 23: Probit Model for Becoming a (Formal) Job Searcher One Year On for Those Still on PP, Restricted Sample, Marginal Effects (Standard Errors)

	All Grandfathered PP Recipients	Grandfathered PPP Recipients	Grandfathered PPS Recipients
Treatment Group	.120***	.159***	.110***
	(.011)	(.024)	(.013)
Male	-.009	034	-.038
	(.042)	(.059)	(.055)
Age of parent	.001	-<.001	.002
	(.001)	(.002)	(.001)
Immigrant parent	.041***	.052**	.028
	(.015)	(.022)	(.019)
Number of children	.003	-.013	.009
under 16 years	(.007)	(.012)	(.008)
LFSR unemployment	.017***	.032***	.013**
rate, %	(.005)	(.011)	(.006)
Past IS duration	.002	-<.001	.003
(including current episode prior to wave A, years	(.002)	(.003)	(.002)
No. Individuals	2046	433	1613
Pseudo R^2	.047	.106	.038
Log (pseudo)likelihood	-982	-204	-772

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The restricted sample combines those with a youngest child aged 6 years on the 30th June 2006 (control group) and those with a youngest child aged 6 years on 30th June 2007 (treatment group). Returners to PP after 30 June 2006 are omitted. The treatment dummy is equal to 1 for those in the latter group and 0 for those in the former group. Other covariates are lagged one year, i.e. wave 1 for the comparison group and wave 3 for the treatment group. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) and includes elapsed duration in current episode prior to wave 1 (control) or wave 3 (treatment) <double check this>. Results are presented as marginal effects, calculated at sample means or at zero as appropriate, and show the percentage increase in the probability of remaining on PP of a one-unit change in the relevant variable. Robust standard errors in parentheses.

The probit model for becoming job-search (and related) active for those previously not job searching (Table 23) also shows a large, positive and, in this case, highly statistically significant impact of activation. The probability of becoming job search active for those treated increases by 12 percentage points. In this case the probit estimate takes the opposite sign to that suggested by the simple unconditional difference-indifferences estimate for treatment effects of job search activity discussed above, again suggesting a need for caution in interpretation. The corresponding estimates for PPP and PPS recipients are an increase of 15.9 percentage points and 11 percentage points, respectively. Once again the impact of activation appears larger for PPP recipients than for PPS recipients. In this case, tighter income eligibility conditions for PPP recipients are likely to be less important than the potential for PPP recipients to call on a partner to provide childcare during job search.

Table 24: Probit Models for Outcomes One Year On, LPS Restricted Sample, All Grandfathered PP Recipients, Marginal Effects (Standard Errors)

	Become Worker	Increase to 15+ Hours per Week for Those Still Working	Become Job Searcher
Activation	.127**	.418**	.017
	(.056)	(.165)	(.032)
Treatment group	-.022	-.185	-.025
	(.032)	(.117)	(.022)
Youngest child 7+ years	.096***	.170	.035*
	(.034)	(.115)	(.020)
Male	.067	-.137	.016
	(.042)	(.140)	(.029)
Age of parent	-.001	-.001	-.002*
	(.002)	(.006)	(.001)
Immigrant parent	-.042	.068	-.007
	(.032)	(.100)	(.020)
Number of children under 16 years	-.019*	-.047	-.008
	(.010)	(.036)	(.007)
Home owner	.032	-.002	-.032*
	(.029)	(.088)	(.019)
Inner regional	-.007	.041	.056***
	(.028)	(.094)	(.015)
Outer regional	.023	.034	.050**
	(.033)	(.117)	(.020)
Poor/fair health	-106***	.041	.018
	(.034)	(.119)	(.019)
Year 12	<.001	-.143	.004
	(.034)	(.121)	(021)
TAFE	.096***	-.064	.018
	(.027)	(.109)	(.018)
Degree	.069	.012	.045*
	(.042)	(.133)	(.026)
Other education	-.107	-.348	.140*
	(.178)	(.338)	(.080)
Proportion of last 5 years (prior to wave 1/wave 3) on IS	-.137**	.069	-.001
	(.038)	(.127)	(.023)
No. Individuals	1086	186	1377
Pseudo R^2	.069	.129	.025
Log (pseudo)likelihood	-590	-111	-587

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The LPS restricted sample pools those with a youngest child aged <6 years or 7+ years at wave 1 (approximating to 30th June 2006) and those with a youngest child aged <6 years or 7+ years at wave 3 (approximating to 30th June 2007). Individuals can be in both groups. The treatment group dummy is equal to 1 for those in the latter group and 0 for those in the former group. The activation dummy is the product of the treatment group dummy and the dummy for having a child aged 7+ years at the start of the year. All other covariates are lagged one year, i.e. wave 1 for the comparison group and wave 3 for the treatment group. Results are presented as marginal effects, calculated at sample means or at zero as appropriate, and show the percentage increase in the probability of the relevant outcome of a one-unit change in the relevant variable. Robust standard errors in parentheses.

Finally, we estimate similar probit models on the pseudo-panel constructed from the LPS (see Table 24). The LPS contains richer data on characteristics of parents, allowing us to show, for example, a positive impact of education level and a negative impact of poor health on the probability of becoming a worker for those PP recipients previously not in employment. Because of differences in the way the RED and LPS pseudo-panels were constructed (see Section 4) we also control here for being in the treatment group (those still on PP at wave 3 of the LPS proper) and for having a child aged seven plus. The activation dummy is the interaction of these two variables. The estimated impact of activation on the probability of entering employment – in this case including those that have exited IS – is an increase of 12.7 percentage points. The equivalent estimate for increasing participation to 15+ hours per week for those previously working fewer hours is an increase of 41.8 percentage points, although this is estimated on a very small sample so should also be treated with caution. There is no apparent impact of activation on the probability of becoming a job searcher, which again leads us to question the robustness of the earlier RED-based estimate of the job search effect of activation reported above. (Note that we do not estimate separately for PPS and PPP recipients given sample size constraints with the LPS.)

Taken together, the weight of evidence from the RED and LPS analysis here, whether unconditional or conditional, suggests activation has led to increased job entry and increased hours for those remaining on PP. The LPS suggests a possible additional positive impact on job entry for those no longer on PP, i.e. a possible increase in exit from PP to employment. This is consistent with the earlier evidence presented in Section 7 on the positive impact of activation on the hazard rate for exiting IS, but given that the RED data do not include information on destinations on leaving IS, we treat this particular interpretation as tentative. Activation also appears to have had a larger impact on employment and job search for PPP recipients than for PPS recipients – consistent with the greater potential for PPP recipients to call on a partner to provide childcare while they work or job search – although the differences are more muted than in the case of exit rates as analysed in Section 7.

9. Conclusions

The evidence presented here shows that the second round of WtW reforms in 2007 led to an increase in exits from PP and a further reduction in PP caseload on top of that caused by the first round of reforms in 2006. Following activation, a typical grandfathered low income parent was more likely to exit PP; more likely to switch from PP to another IS payment; more likely to exit IS altogether; more likely to take up employment if remaining on PP or otherwise; and more likely to work 15+ hours per week if remaining on PP or otherwise. Estimates of impacts on job-search and related activity are not robust, but some suggest the possibility of additional positive activation impacts.

For those outcomes that can be separately estimated by payment type, the impact of activation has generally been larger for those in receipt of PPP than for those in receipt of PPS. For exits from PP, this may partly reflect tighter income restrictions for PPP eligibility and the opportunity, not open to PPS recipients, of responding to activation requirements by exiting PP and compensating for the loss in household income by increasing partner earnings. For employment and job search, it may reflect the greater potential for PPP recipients to call on a partner to provide childcare while they engage in labour market activity.

These impacts are broadly in line with those estimated for the earlier round of WtW reforms for new entrant low income parents presented in DEEWR's 2008 in-house evaluation report.

They also compare favourably in terms of magnitude with estimates of the impacts of recent WtW reforms for low income parents in some other OECD countries, e.g. the UK introduction of compulsory WFIs in 2001. This is likely to reflect both the nature of the reforms – they represent a substantial increase in conditionality – and the strong labour market context in which they have been introduced.

WtW appears to be working for low income parents, at least in terms of its stated aims of reducing welfare dependence and increasing workforce participation. More restrictive PP entry requirements for new claimants following the 2006 reforms and higher exit rates for existing claimants following the 2007 round of reforms have both contributed to the fall in the PP caseload.

There are of course various caveats to this positive conclusion. Firstly, the scope of this evaluation has been limited to examining impacts on benefit claiming and workforce participation; ultimately Welfare to Work needs to be judged by its impact on the welfare of low income families, whether measured by income, wellbeing or some other construct, and on its benefits net of its costs. Second, and more specifically, part of the impact on the grandfathered group of low income parents – some estimates suggest around one half – has been driven by benefit shift from PP to other IS payments. Although this sorting effect might serve to help some low income parents onto more suitable benefits, there is a cost in terms of increased caseloads on other IS payments, including less ‘active’ IS payments such as DSP.

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Appendix: Supporting Tables

Table A1: Covariate Sample Means (Standard Deviations), Full Sample, All PP, Before and After 1st July 2007 by Age of Youngest Child

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Elapsed duration in current episode to 30 June 2006, days	671 (706)	1390 (1101)	828 (765)	1672 (1058)
Male	.082	.141	.063	.109
Age of parent	29.8 (7.12)	40.7 (7.02)	30.0 (7.30)	42.3 (7.14)
Immigrant	.223	.286	.229	.297
Number of children <16	1.88 (1.07)	1.60 (.812)	1.87 (1.06)	1.48 (.750)
LFSR Unemployment Rate, %	5.13 (1.33)	5.08 (1.36)	4.71 (1.25)	4.63 (1.24)
Previous IS episodes duration (prior to current episode), years	3.76 (3.42)	4.34 (3.81)	3.60 (3.27)	4.06 (3.63)

Notes: Age of parent and duration of previous IS episodes are measured in years. Covariates are measured at the episode end date or right-censoring date. The denominator for 'proportion of episodes ending within window' is the total number of episodes.

Table A2: Covariate Sample Means (Standard Deviations), Full Sample, PPS Only, Before and After 1st July 2007 by Age of Youngest Child

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Elapsed duration in current episode to 30 June 2006, days	814 (748)	1486 (1090)	873 (780)	1704 (1045)
Male	.069	.139	.045	.107
Age of parent	29.2 (7.08)	40.4 (7.03)	29.1 (7.17)	41.9 (7.21)
Immigrant	.172	.239	.161	.243
Number of children <16	1.76 (.992	1.51 (.754)	1.73 (.942)	1.39 (.672)
LFSR Unemployment Rate, %	5.10 (1.34)	5.03 (1.35)	4.68 (1.25)	4.59 (1.24)
Previous IS episodes duration (prior to current episode), years	3.69 (3.33)	4.39 (3.82)	3.63 (3.19)	4.16 (3.68)

Notes: Age of parent and duration of previous IS episodes are measured in years. Covariates are measured at the episode end date or right-censoring date. The denominator for 'proportion of episodes ending within window' is the total number of episodes.

Table A3: Covariate Sample Means (Standard Deviations), Full Sample, PPP Only, Before and After 1st July 2007 by Age of Youngest Child

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Elapsed duration in current episode to 30 June 2006, days	503 (612)	1134 (1088)	757 (737)	1567 (1095)
Male	.098	.146	.092	.118
Age of parent	30.6 (7.08)	41.6 (6.91)	31.2 (7.33)	43.4 (6.80)
Immigrant	.282	.414	.336	.476
Number of children <16	2.02 (1.14)	1.84 (.910)	2.10 (1.20)	1.76 (.910)
LFSR Unemployment Rate, %	5.16 (1.33)	5.23 (1.35)	4.75 (1.25)	4.77 (1.24)
Previous IS episodes duration (prior to current episode), years	3.83 (3.51)	4.20 (3.80)	3.55 (3.39)	3.71 (3.43)

Notes: Age of parent and duration of previous IS episodes are measured in years. Covariates are measured at the episode end date or right-censoring date. The denominator for 'proportion of episodes ending within window' is the total number of episodes.

Table A4: Covariate Sample Means (Standard Deviations), Restricted Sample, All PP, Treatment & Control Groups

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Elapsed duration in current episode to 30 June 2006, days	1045 (902)	1110 (906)	1367 (947)	1572 (979)
Male	.116	.132	.083	.103
Age of parent	36.2 (6.47)	36.1 (6.49)	34.7 (6.57)	37.4 (6.45)
Immigrant	.283	.251	.218	.306
Number of children <16	2.13 (1.06)	1.95 (.952	1.93 (1.06)	1.97 (.996)
LFSR unemployed rate, %	5.08 (1.39)	4.98 (1.35)	4.52 (1.21)	4.58 (1.29)
Previous IS episodes duration	4.34 (3.80)	3.93 (3.86)	4.28 (3.63)	3.90 (3.58)

Notes: Age of parent and duration of previous IS episodes are measured in years. Covariates are measured at the episode end date or right-censoring date (30 June 2007 for the comparison group and 30 June 2008 for the treatment group). The denominators for 'proportion of episodes ending within window' are the number of comparison sample episodes and the number of treatment sample episodes respectively.

Table A5: Covariate Sample Means (Standard Deviations), Restricted Sample, PPS Only, Treatment & Control Groups

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Elapsed duration in current episode to 30 June 2006, days	1136 (853)	1206 (897)	1361 (866)	1530 (904)
Male	.105	.117	.072	.069
Age of parent	35.4 (6.66)	35.1 (6.39)	34.1 (6.60)	35.9 (6.29)
Immigrant	.231	.189	.167	.228
Number of children <16	1.95 (.964)	1.82 (.924)	1.88 (.983)	1.78 (.850)
LFSR unemployed rate, %	5.03 (1.37)	4.91 (1.32)	4.54 (1.19)	4.56 (1.28)
Previous IS episodes duration	4.22 (3.81)	3.74 (3.51)	4.30 (3.56)	3.88 (3.52)

Notes: Age of parent and duration of previous IS episodes are measured in years. Covariates are measured at the episode end date or right-censoring date (30 June 2007 for the comparison group and 30 June 2008 for the treatment group). The denominators for 'proportion of episodes ending within window' are the number of comparison sample episodes and the number of treatment sample episodes respectively.

Table A6: Covariate Sample Means (Standard Deviations), Restricted Sample, PPP Only, Treatment & Control Groups

	Child under 7 at end of episode, episode ends before 1st July 2007	Child 7+ at end of episode, episode ends before 1st July 2007	Child under 7 at end of episode, episode ends after 30th June 2007	Child 7+ at end of episode, episode ends after 30th June 2007
Elapsed duration in current episode to 30 June 2006, days	887 (965)	904 (896)	1584 (1053)	1791 (1035)
Male	.135	.163	.101	.169
Age of parent	37.6 (5.90)	38.2 (6.24)	36.5 (6.28)	40.5 (5.58)
Immigrant	.374	.382	.348	.449
Number of children <16	2.44 (1.15)	2.21 (.960)	2.35 (.978)	2.51 (.935)
LFSR unemployed rate, %	5.16 (1.42)	5.13 (1.42)	4.53 (1.22)	4.63 (1.32)
Previous IS episodes duration	4.55 (3.77)	4.33 (3.92)	3.79 (3.64)	3.52 (3.51)

Notes: Age of parent and duration of previous IS episodes are measured in years. Covariates are measured at the episode end date or right-censoring date (30 June 2007 for the comparison group and 30 June 2008 for the treatment group). The denominators for 'proportion of episodes ending within window' are the number of comparison sample episodes and the number of treatment sample episodes respectively.

Table A7: Cox Proportional Hazard Model, Using Interview Date, Single Risk (All Exits from PP) & Competing Risks (Exits from IS, Exits to Other IS), Restricted Sample, Coefficients (Standard Errors)

	All Exits	Exits from IS	Exits to Other IS
Post-interview	.217***	.073	.881***
	(.075)	(.091)	(.197)
Male	.204**	.250***	-.960**
	(.083)	(.094)	(.457)
Age of parent	-.006	-.011*	-.020
	(.004)	(.005)	(.015)
Immigrant parent	-.018	-.102	.624**
	(.060)	(.068)	(.171)
Number of children under 16 years	.018	-.004	.034
	(.026)	(.030)	(.088)
LFSR unemployment rate, %	-.055***	-.087***	.067
	(.020)	(.023)	(.068)
Past IS duration, years	-.006	-.046***	.113***
	(.007)	(.008)	(.019)
Elapsed duration of current episode prior to 30 June 2006 (control group) and 30 June 2007 (treatment group), years	-.140***	-.172***	-.043
	(.011)	(.013)	(.034)
No. Individuals	6490	6490	6490
No. Failures	1552	1194	148
Log (pseudo)likelihood	-13309	-10198	-2489

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. The restricted sample combines those with a youngest child aged 6 years on the 30th June 2006 (control group) and those with a youngest child aged 6 years on 30th June 2007 (treatment group). Returners to PP after 30 June 2006 are omitted. The post-interview dummy takes the value 1 for all episodes or parts of episodes following an activation interview and 0 otherwise. Age of parent is expressed in years. Past IS episode duration is expressed in years (since 1st January 1998) as is elapsed duration of current episode. Results are presented in coefficient form, i.e. the βs, γs and δs from Equation (2), and are interpretable as semi-elasticities. Robust standard errors in parentheses.

Table A8: Attrition and Characteristics of Grandfathered Parenting Payment Recipients in the LPS, Youngest Child Under Seven

	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
Male	6.86	6.77	6.21	6.23	5.43
Country of birth					
Australia born, non ATSI	70.81	73.63	75.00	74.53	74.89
NESB	14.91	13.19	13.20	13.96	13.12
ESB	5.95	6.07	6.52	7.17	7.69
ATSI	8.33	7.12	5.28	4.34	4.30
Age group					
Under 25	25.62	21.47	19.88	16.60	14.03
25-49	73.47	77.60	79.50	82.08	84.84
50-60	0.82	0.82	0.62	1.32	0.90
60+	0.09	0.12	0.00	0.00	0.23
Education					
Unknown	0.00	0.00	0.00	0.38	0.00
Primary school or lower	4.57	4.08	2.48	3.02	2.71
Year 10	40.62	36.52	32.14	27.55	28.73
Year 12	22.60	22.40	23.14	24.72	24.43
Trade qualification	24.43	28.12	30.90	32.45	32.81
Degree or above	7.78	8.63	10.87	10.94	10.63
Others	0.00	0.23	0.47	0.94	0.68
Remoteness					
Major cities	43.55	42.71	42.39	43.58	43.67
Inner regional	36.78	37.57	39.91	38.3	37.56
All outer regional	19.67	19.72	17.7	18.11	18.78
State					
NSW	35.04	33.37	33.70	33.02	33.26
VIC	19.76	20.54	19.88	20.57	20.14
QLD	24.43	24.85	24.22	24.53	24.21
SA	7.69	7.58	8.39	8.49	9.05
WA	8.97	9.10	8.54	7.17	7.01
TAS	2.93	3.03	3.11	4.15	4.30
NT	0.37	0.47	0.78	0.57	0.68
ACT	0.82	1.05	1.40	1.51	1.36
Living with partner	35.50	39.44	42.55	45.85	46.61
Partner currently working	20.40	27.42	28.88	33.58	34.84
Owns/buying house	21.50	22.76	23.14	24.15	26.47
Number of dependent children (mean)	2.14	2.23	2.31	2.37	2.41
Number of individuals	1093	857	644	530	442

Table A9: Attrition and Characteristics of Grandfathered Parenting Payment Recipients in the LPS, Youngest Child Seven Plus Years

	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
Male	13.23	13.99	13.39	11.67	10.91
Country of birth					
Australia born, non ATSI	66.86	68.88	69.82	71.04	71.71
NESB	17.35	15.21	13.39	12.92	12.25
ESB	10.38	11.19	11.43	12.08	12.69
ATSI	5.41	4.72	5.36	3.96	3.34
Age group					
Under 25	2.56	1.40	1.43	0.63	0.67
25-49	87.91	86.71	84.82	84.79	83.74
50-60	9.39	11.54	13.39	13.96	14.70
60+	0.14	0.35	0.36	0.63	0.89
Education					
Unknown	0.00	0.00	0.18	0.42	0.00
Primary school or lower	6.12	5.07	3.21	2.71	1.56
Year 10	42.11	40.21	40.00	40.42	40.98
Year 12	15.65	13.11	15.54	16.46	14.70
Trade qualification	27.60	30.77	29.64	26.25	28.73
Degree or above	8.53	9.62	10.36	12.29	13.81
Others	0.00	1.22	1.07	1.46	0.22
Remoteness					
Major cities	43.10	42.83	40.00	39.58	38.75
Inner regional	37.13	37.24	39.29	41.67	42.09
All outer regional	19.77	19.93	20.71	18.75	19.15
State					
NSW	29.02	26.92	27.86	27.29	24.28
VIC	21.34	21.85	20.71	22.08	22.05
QLD	25.18	26.22	26.43	26.25	28.29
SA	8.39	8.57	8.21	7.50	7.57
WA	9.67	9.27	10.00	10.00	10.02
TAS	4.55	5.24	5.18	5.00	5.79
NT	0.85	0.87	0.71	0.83	0.67
ACT	1.00	1.05	0.89	1.04	1.34
Living with partner	26.46	30.59	31.61	34.38	37.86
Partner currently working	14.79	19.93	21.61	25.00	27.84
Owns/buying house	38.40	40.73	42.68	45.84	45.65
Number of dependent children (mean)	1.92	1.90	1.89	1.95	1.96
Number of individuals	703	572	560	480	449