Australian Government
Productivity Commission

Exploring the effects of minimum wage increases on employment using a large administrative dataset

Workplace Relations Framework
Technical Supplement

September 2015

An appropriate reference for this publication is:

Productivity Commission 2015, *'Exploring the effects of minimum wage increases on employment using a large administrative dataset',* Workplace Relations Framework — Technical Supplement, Canberra.

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Acknowledgements

This technical supplement is primarily based on de-identified individual data from the Research and Evaluation Database (RED), supplied by the Australian Government Department of Employment (DOE). The Productivity Commission gratefully acknowledges the support and advice it has received from the Department. This supplement also uses unit-record data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. The HILDA Survey was initiated and is funded by the Australian Government Department of Social Services (DSS) and is managed by the Melbourne Institute of Applied Economic and Social Research (Melbourne Institute).

The Commission wishes to thank the following external academic referees — Richard Burkhauser (Melbourne Institute and Cornell University), Bob Gregory (Australian National University), and particularly Robert Breunig (Australian National University) — for their insightful and helpful feedback.

The findings and views based on RED or HILDA data are those of the Productivity Commission. They should not be attributed to either DOE, DSS or the Melbourne Institute. Neither should they be attributed to the external referees who provided feedback on the research.

Abbreviations

ABS	Australian Bureau of Statistics
DID	Difference-in-differences
FWC	Fair Work Commission
FWO	Fair Work Ombudsman
HILDA	Household, Income and Labour Dynamics in Australia
ISP	Income support payment
NPP	Non-payment partner
OECD	Organisation for Economic Cooperation and Development
OLS	Ordinary Least Squares
PC	Productivity Commission
RED	Research and Evaluation Database

1 Introduction

1.1 Background

A key issue in debates about minimum wages is the degree to which they affect employment and unemployment and the direction of any impacts. Were it found that a given level of the minimum wage had either no effect or a positive effect on employment, there would be a strong case for increasing it. If a given minimum wage adversely affects employment, on the other hand, this outcome would have to be weighed up against any beneficial impacts on income distribution. The larger the employment effect, the greater would be the grounds for constraining its growth or reducing it (albeit potentially in conjunction with the introduction of other measures for promoting equity).

However, there are several empirical obstacles that together can bedevil attempts to gauge the effects of minimum wages on employment:

- individual changes in minimum wages are often small and incremental, albeit with the potential to have more significant cumulative, long-term effects
- there is often incomplete or 'noisy' data on the incidence of minimum wages and on job loss, job entry and changes in hours worked
- the timing of business responses to a change in minimum wages can vary considerably, with some responses potentially quite lagged while others anticipate a foreshadowed increase
- it is not straightforward to disentangle the effects of changes in minimum wages from other factors, both in the labour market and the economy, with the potential to affect employment
- aggregate findings and trends may conceal offsetting effects, given that theory suggests that minimum wages can have positive or negative employment effects, depending in part on the market(s) under consideration.

There are some particular difficulties in estimating the effects of minimum wage changes in the Australian context. In contrast to the United States, for example, where minimum wages can vary significantly between or within different states, and where there have been some significant changes in minimum wages, there are few such 'natural experiments' in Australia. The Australian national minimum wage applies to all jurisdictions, with the exception of non-constitutional enterprises in Western Australia. It tends to be increased in regular and modest increments. Further, the Fair Work Commission (FWC) typically applies the same adjustment to the wages of award-reliant workers further up the wage distribution, and these can also flow into some above-award wages. As a result, a range of

wages move largely in unison with the national minimum wage. These features add to the difficulty of isolating the effects of a change in the 'floor' wage on those to whom it applies.

In its report on the Workplace Relations Framework (PC 2015, appendix C), the Productivity Commission surveyed a range of existing Australian minimum wage studies that have sought to overcome these difficulties in various ways. It additionally examined several studies that have looked at the employment effects of changes in Australian wage levels generally. While this survey found that, taken together, the studies suggest that minimum wages adversely affect employment, it noted that the number of minimum wage studies is small, they are often dated and their findings are subject to methodological and other caveats.

1.2 Purpose and approach of this study

To supplement these studies, the Productivity Commission has explored whether it is possible to gain additional, and more robust and up-to-date, empirical evidence on the employment effects of minimum wages in Australia by exploiting a newly-available administrative dataset: the Research and Evaluation Database (RED). RED is a confidentialised administrative dataset which captures all Australian federal income support recipients and their partners and children fortnightly, dating back to July 1998.[1] Over the period of interest, 2008 to 2013, a snapshot of RED on 1 June each year provides information on around 5 million individuals. This is the first time RED has been used to focus on minimum wage earners.

The RED sample is used to explore the impact of annual minimum wage increases ('upratings') between 2008 and 2013 on:

- the probability of job loss for minimum wage workers
- the number of hours worked by minimum wage workers
- the probability of being hired at the minimum wage.

To shed light on these issues quantitatively, the difference-in-differences (DID) approach is used. The simplest form of this econometric approach compares employment transitions of people directly affected by a minimum wage increase, the 'treatment' group, to those of comparable people who are not affected, the 'control' group. As the DID technique rests

[1] Labour earnings and hours worked data are only available from 2006. Income support recipients in RED are individuals receiving Newstart Allowance, Youth Allowance, Sickness Allowance, Mature Age Allowance, Partner Allowance, Disability Support Pension, Carer Payment, Widow Pension, Austudy Payment, Widow Allowance, Special Benefit, Parenting Payment Single, Parenting Payment Partnered, Age Pension, Wife Pension, Bereavement Allowance, Exceptional Circumstances Payment (living expenses component), Farm Help (income support component), or ABSTUDY (living allowance component). Partners and children of these recipients are also captured in RED. RED does not capture other government payments, concessions or rebates.

on a number of important assumptions, the validity of these assumptions in the present case is subjected to special scrutiny.

A wide range of theoretical constructs is proposed in the literature to explain aspects of the employment effects of minimum wages. They are covered in the Productivity Commission's inquiry report . This empirical investigation is not intended to be a test of any particular theory, but simply to ascertain what the evidence contained in RED can reveal.

The bottom line of this study is that the effects of minimum wage changes are ambiguous over the relevant period (chapter 7). However, there are many lessons learned from considering how minimum wages might affect employment or other aspects of labour markets (chapter 6), and in understanding promising new ways of diagnosing regulatory effects from administrative datasets (chapters 2 to 5).

2 Data

As with all quantitative exercises, awareness of the strengths and weaknesses of the data at hand is crucial. This is especially true when the data are administrative in nature. Unlike representative survey data produced by the Australian Bureau of Statistics (ABS), government data are designed to meet narrow regulatory or administrative objectives, not the needs of researchers. Thus, it is important to consider whether the variables collected are fit for purpose, including after data cleaning and adjustment in some cases. This chapter considers these issues, concluding that RED possesses a number of strengths for the intended task but also has some potentially important limitations.

2.1 Required data elements

To undertake the DID analysis, the following data elements are required:

- employment status over time, to evaluate people's transitions in and out of work
- the hourly wage, to identify minimum wage workers
- hours worked, to evaluate changes in hours and to calculate hourly wages
- some personal characteristics to account for sources of variation in the models that are unrelated to changes in the minimum wage.

Each of these elements is available or can be derived from RED, which contains longitudinal data on individuals' income, hours worked and income support receipt, together with some demographic details. Further details on how the required DID elements are drawn from the RED tables are provided in box 2.1.

2.2 Suitability of RED for this research

This section provides a critical evaluation of the RED dataset. It outlines the inherent strengths and weaknesses of RED, before turning to HILDA to examine the degree to which the RED population is representative of the wider population of minimum wage workers. Finally, the section explains why some subpopulations are excluded from the DID analysis.

Box 2.1 Extracting the required data elements from RED

In RED tables, durations for which individuals' characteristics remain unchanged are recorded as episodes. To enable the DID analysis, a number of data elements are extracted from RED and converted to monthly measures.

Hours worked and hourly wages

There are two sources of labour earnings data in RED. The Continuous Earnings table records the daily amount of an individual's income from regular, non-variable earnings for the duration of an episode, as well as the number of hours worked per fortnight for that episode. Therefore, the hourly wage equals $\frac{daily\ income*14}{fortnightly\ hours}$. The Variable Earnings table provides the daily amount of an individual's income from variable earnings for an episode, and the number of hours worked per fortnight for that episode. The formula for hourly wage using these variables is $\frac{daily\ amount*14}{fortnightly\ hours}$. It is possible for an individual to appear in both the Continuous and Variable Earnings tables at the same time, in either the same or different jobs. If the different earnings belong to the same job, a single observation is created for which the relevant wage is the weighted average hourly wage for the job over the month, and fortnightly hours worked are the weighted average fortnightly hours worked in the month (where weighting is based on the number of days in the month that each episode lasted). If earnings data are for two different jobs, each job is retained in the estimation sample as a separate observation, using the same weighting procedure as above. If an individual reports having more than two employers, those episodes are excluded, since the reported earnings and hours are summed over multiple jobs which cannot be distinguished. In general, jobs are excluded whenever hourly wages cannot be accurately calculated, such as when an earnings episode records positive earnings but zero hours.

Employment status

Because an individual only appears in the earnings tables when they have labour income, these tables need to be merged with the Benefit History table (which captures income support recipients) or the Both Partner table (to capture non-payment partners) to piece together the relevant periods of the individual's RED history. A person is deemed to be employed in a job if they worked in that job for at least one day in the calendar month.[2] Individuals who no longer report earnings, but remain in RED, can be identified as having left employment. On the other hand, there is no way of determining the labour force status of an individual who is no longer in RED, so these employment transitions cannot be captured in the sample.

Personal characteristics

A variety of personal characteristics are available in RED. For instance, the Customer table contains, among other things, gender, birth date and language, which are used in the analysis.

Source: Department of Education, Employment and Workplace Relations Social Policy and Economic Strategy Group Research Branch (2014).

2 The ABS defines a person as employed if they work at least one hour in a one week reference period (ABS 2001).

Inherent strengths and weaknesses

RED offers several advantages over alternative datasets. First, the variables available allow relatively accurate identification of minimum wage workers, which is crucial for the DID analysis envisaged. Second, in the event that a person holds two jobs simultaneously, RED captures the earnings and hours worked associated with each job separately, which permits the calculation of an hourly wage for each job, rather than an average across both jobs (box 2.1). Third, the episodic structure of RED allows the identification of the characteristics of individuals on at least a fortnightly basis, which means that employment transitions of any length can be studied. Within the literature, researchers often only have access to quarterly data (or even less frequent), which places restrictions on their choice of transitions.

A further advantage of RED is its size. From 2008 to 2013, on 1 June each year, RED captures an average of 52 000 workers aged between 21 and 64 years (excluding Age and Disability Support Pension recipients, and those receiving Supported Wage System or Disability Supported Employment wages) earning between 0 and 110 per cent of the adult minimum wage. No other Australian dataset comes close to capturing as many minimum wage earners. Moreover, RED is the only dataset to track an entire subgroup of minimum wage earners — those working while receiving income support — over time. Appendix A provides a breakdown of the labour force characteristics of recipients of different benefit types.

There are also weaknesses inherent in the RED data. All observations are conditional on the individual or their partner remaining on income support at the end of the transition. If individuals leave the database, their information is subject to 'right censoring'. Some spells will be right-censored because individuals or their partners begin to earn too much and lose eligibility for income support. As a consequence, censoring is non-random, and the results may be biased towards capturing people who lose their jobs, fail to enter jobs, or whose hours do not increase, since they are more likely to remain on income support and, therefore, remain in the sample. This issue is explored in more depth in section 6.1. RED also lacks information on casual loadings and penalty rates, so that workers receiving these supplements have their base hourly wages overestimated.[3] Aside from these concerns, RED shares with most surveys the potential for measurement error, recall error, and data entry error (box 2.2).

[3] Unfortunately, it is not possible to impute casual employee status in RED, for example on the basis of irregular hours and/or income. Even ongoing employees, either working part-time or full-time, can receive variable earnings if they occasionally work overtime hours or on weekends.

Box 2.2 Measurement error in RED and comparisons with HILDA

Because hourly wage is a derived variable, it will be inaccurate if earnings and/or hours data are not reported correctly. For most income support payments, eligibility depends solely on total income for the relevant fortnight. As well as weakening the incentive to accurately report hours, this may create incentives for underreporting of earnings. For some types of income support payment such as Carer Payments, there is a limit on the number of hours that recipients can work. This may create incentives to underreport also. It is difficult to determine how prevalent incorrect reporting is in the Centrelink income and hours data that underlie RED. Nevertheless, any misreporting of these variables would introduce measurement error.

One way of ascertaining the extent of measurement error is to benchmark against other datasets. In appendix B, the distributions of weekly hours, weekly income from wages and salary, and hourly wages of income support recipients in RED are compared with those in HILDA receiving the same benefit types (Bereavement, Newstart, Sickness, Widow, Partner, and Youth Allowances; Special Benefit; Austudy and ABSTUDY; Carer Payment; Wife Pension; and Parenting Payments).

- In terms of weekly hours, the RED distribution is slightly skewed towards lower values, indicating that individuals in RED are more likely to work part-time (figure B.1).
- The distributions of weekly incomes from wages and salary are similar. RED incomes have slightly more mass at the lower end of the distribution, perhaps because of incentives to underreport (figure B.2).
- Figure B.3 shows that, without any casual adjustment in HILDA, the shapes of the hourly wage distributions are similar. A greater number of very low hourly wage values are reported in HILDA, perhaps because there is no limit on reported hours. Once an adjustment is made for casual loading of 23 per cent, however, the HILDA distribution shifts left (figure B.4). In the HILDA sample, approximately 57 per cent of employed (excluding unpaid family workers) income support recipients are casuals. The potential influence of casuals in RED is explored further in section 4.4.

Sources: Productivity Commission estimates based on HILDA wave 12 and RED.

Representativeness of RED

The proportion of the RED workforce earning the minimum wage — that is, minimum wage coverage — is higher than comparable estimates for the whole Australian workforce. Defining minimum wage earners as those earning between 0 and 105 per cent of the minimum wage, it is estimated that, in September 2010, approximately 12.0 per cent of the RED sample aged 21 to 64 consisted of minimum wage workers.[4] Increasing the threshold to 110 per cent, the RED estimate is 15.5 per cent. These figures lie above the 4.1 to 9.1 per cent (105 per cent cut-off) and 6.0 to 11.6 per cent (110 per cent cut-off) ranges that Bray (2013) estimates for all workers aged 21 and over in 2010 and 2011.[5] More recently,

4 Excludes Age or and Disability Support Pension recipients, and individuals receiving Supported Wage System or Disability Supported Employment earnings.

5 The lower and upper bound estimates come from the ABS survey of Employee Earnings and Hours and HILDA, respectively (Bray 2013, p. 22).

the Productivity Commission (2015), defining minimum wage workers as those earning up to 110 per cent of the minimum wage, obtains a coverage estimate of 7.2 per cent for 2014.[6] The equivalent RED figure for 2014 is 15.7 per cent. The comparatively high proportion estimated from RED in all years might be expected, given that, by definition, RED captures low-income households.

Wave 12 of HILDA can be used to learn more about the subset of Australian minimum wage workers captured in RED. HILDA contains information on both income support receipt and labour earnings, and so can be used to estimate the proportion of minimum wage workers who receive income support or have a partner who does. Furthermore, HILDA can be used to determine the location of these minimum wage workers in the distribution of household incomes. This analysis may help to determine the extent to which the DID results can be generalised to the wider population of minimum wage workers.

First, minimum wage workers and minimum wage households are identified in HILDA. Minimum wage workers are defined narrowly as people aged 21 to 64 earning between $14 and $16.76 per hour in 2012. This range foreshadows the definition of a treatment group, and translates approximately to between 90 and 105 per cent of the July 2012 minimum wage of $15.96. A minimum wage household is one containing at least one of these minimum wage workers. For the purposes of this analysis, the household income measure used is household financial year disposable income, adjusted by the modified OECD equivalence scale (OECD n.d.).[7]

The HILDA analysis suggests that only a minority of minimum wage workers are likely to be captured in RED, and that those who are, are likely to belong to low-income households. The HILDA estimates indicate that only about 14 per cent of adult minimum wage workers or their partners receive federal government income support and, therefore, will be captured in RED. Fifty-two per cent of this subset, however, belong to households in the lowest three household income deciles.[8] In comparison, only 22 per cent of minimum wage earners not receiving income support belong to low-decile households. Furthermore, it is estimated from HILDA that, in about 64 per cent of minimum wage households receiving income support, the minimum wage worker is the primary income earner. The corresponding figure for minimum wage households not receiving income support is 35 per cent. Job losses by primary earners would have a larger negative impact on household income than job losses by other household members. These HILDA statistics suggest that, although representing only a fraction of all minimum wage workers, those captured in RED belong predominantly to a group of considerable policy interest.

6 This estimate comes from the ABS survey of Employee Earnings and Hours. It includes only federal system workers, and includes adjustments for junior rates of pay and casual loading.

7 The needs of a household increase with additional members, but not at a constant rate. Equivalence scales assign a value to each household in proportion to its needs. Household income is divided by this value to obtain equivalised household income. The modified OECD equivalence scale assigns a value of 1 to the household head, 0.5 to each additional adult, and 0.3 to each child (OECD n.d.).

8 While only people aged 21 to 64 can be minimum wage workers in this analysis, all household members remain in the sample for the purposes of determining household income deciles.

Nonetheless, the findings from the analysis should not be generalised unquestioningly to all minimum wage workers, since those living in higher-income households may exhibit different work preferences and incentives.

Exclusions from the analysis

The representativeness of the estimation sample, as opposed to that of RED itself, is further influenced by the exclusion of some groups from the analysis. Because of uncertainty surrounding their 'junior' (pro-rated) wage rate status, people aged below 21 are dropped from the estimation sample. Under the national minimum wage order and in awards, junior rates usually amount to a specified proportion of the adult rate of pay. Identification of a young worker's relevant wage is complicated because:

- not all 122 modern awards specify junior wages
- the structure of junior wages can vary with each award
- some junior employees are entitled to the adult rate while performing certain work or after a certain period of employment (PC 2015).

Thus, in RED, it is impossible to know, for example, whether a 20-year old worker earning the adult minimum wage is covered by an award containing no junior wages, or is in reality receiving a wage above the relevant junior minimum appearing in the award. The removal of young workers from the estimation sample, while undertaken for pragmatic reasons, is unfortunate. Many studies have suggested that minimum wages can have greater effects on this group (PC 2015).

People aged over 64, those receiving the Age or Disability Support Pension, and those receiving Disability Supported Employment or Supported Wage System earnings, are also removed from the estimation sample. These individuals may not be affected by minimum wage changes in the same way as other workers, perhaps because they have reduced work capacity, receive different wage entitlements, are covered by additional employment protections, or face different work incentives.

Ultimately, both RED and the estimation sample are affected by important limitations in terms of their accuracy, coverage and representativeness. However, RED also offers a number of countervailing advantages, such as its size, longitudinal nature and focus on a subgroup of considerable policy interest. Unfortunately, there is no dataset available in Australia at present that would represent an unalloyed improvement on the one chosen for this analysis. In the absence of a superior alternative, the Productivity Commission regards RED as broadly suited to a preliminary exploration of the association between minimum wage increases and employment outcomes. Nonetheless, the need for better data and further analysis remains.

2.3 Sub-minimum wage workers

The constructed RED dataset captures a number of individuals who report sub-minimum wages. While their precise percentage varies by month, it is estimated that, in September 2010, approximately 7.8 per cent of workers aged 21 to 64 had an average hourly wage for the month that was below the then legal minimum of $15.[9] Approximately 3.4 per cent earned less than 80 per cent of the minimum wage.

Sub-minimum wage workers are found in almost all datasets. These observations frequently arise due to measurement error in hourly wage, stemming from inconsistencies between an individual's reported values of income and hours. For example, a low-wage individual's reported number of hours worked may exceed the number of hours they were paid for, resulting in a calculated wage below the minimum. The sub-minimum wage estimates from RED are somewhat larger than Bray's estimate of 2.7 per cent for 2010 (2013). This may be expected, given that Bray's figures are drawn from the ABS Survey of Employee Earnings and Hours, which is based on employer payroll records and is, therefore, less likely to report sub-minimum wages.

The interpretation of below-minimum wage workers in the dataset is also related to the extent to which the Australian minimum wage actually binds. Sub-minimum wages may be identified because, for example, reported hourly wages do not take account of salary sacrificing or because employers have not complied with their minimum wage obligations (PC 2015) (Nelms et al 2011).[10] Non-compliance is more likely to affect vulnerable workers, including those with limited workforce experience and migrant workers (box 2.3).

9 Excludes Age or and Disability Support Pension recipients, and individuals receiving Supported Wage System or Disability Supported Employment earnings.

10 It is lawful for a salary sacrifice arrangement to reduce an employee's pay to below the minimum wage required under an applicable modern award (*Casey Grammar School v Independent Education Union of Australia* [2010] FWA 8218 (PwC 2011)). However, salary sacrifice is unlikely to be a problem within RED, because Centrelink requires income support recipients to provide their income before salary sacrifice is deducted (although reporting compliance may vary).

Box 2.3 Non-compliance with award wages

The Fair Work Ombudsman (FWO) is responsible for ensuring compliance with Australian workplace laws. According to the FWO's 2013-14 annual report, 75 per cent of the complaints it received related to underpayment, and more than $3.1 million was recovered. Underpayment may result from non-compliant rates of pay (not just below the absolute minimum, but wages below the relevant award rate) or unpaid hours of work. The latter could combine with a compliant weekly wage rate to produce a sub-minimum derived hourly wage in RED.

Violations of minimum employment standards are more common in some industries than in others. The FWO indicates that, in 2013-14, the most complaints originated from within accommodation and food services, construction, and retail. Non-compliance may be deliberate or it may occur when employers are unfamiliar with regulations. Smaller employers are more likely to be non-compliant, perhaps owing to cost pressures or a lack of compliance expertise.

Non-compliant pay and conditions are relatively more prevalent among migrant workers. This is due to a range of factors, including limited English skills, lack of awareness of workplace rights and reluctance to challenge employers. The latter reason is likely to feature even more prominently among those migrants working in breach of the Migration Act.

Sources: Fair Work Ombudsman (2014); Howe, Hardy and Cooney (2014); PC (2015).

3 Methodology

This chapter describes the DID modelling approaches used to estimate the effects of minimum wage increases on job loss, hours worked, and job entry. Two different versions of the conventional DID technique are implemented: a standard, two-group model; and a model with additional groups that allows for spillover effects of minimum wage increases. This chapter also explains how workers are allocated to various groups for the purposes of the analysis. Finally, it illustrates the employment transitions that are studied, and details the equations to be estimated.

The DID technique has been used extensively within the existing minimum wages literature.[11] Its attractiveness lies in its similarity with randomised controls trials used in scientific experiments to determine causality. While applying DID to economic data cannot replicate laboratory conditions, the analysis nevertheless can — under certain conditions — abstract from confounding influences on an outcome of interest, to focus on the impact of a single factor (box 3.1).

3.1 The standard difference-in-differences approach

The simple, two-group version of DID estimation (the 'standard' version) requires identification of a treatment group (or 'at risk' group) of observations, which is directly affected by the minimum wage increase, and a control group, which is unaffected by the change but otherwise possesses characteristics similar to the treatment group. The logic is that changes affecting the control group provide a counterfactual for the changes that would have affected the treatment group in the absence of treatment. This counterfactual provides the comparator against which the effect of the treatment can be measured. A simple comparison of 'before' and 'after' outcomes for the treatment group alone would not prove nearly as effective in establishing causality.

For the counterfactual to fulfil its role, two key assumptions must hold (Dickens, Riley and Wilkinson 2009). The first is that the control group is unaffected by the minimum wage increase. The second is the so-called 'parallel paths' assumption: that, in the absence of the minimum wage increase, the change in the average of the outcome variable for the treatment group would be the same as the change in the average of the outcome variable for the control group. Bias in the estimated treatment effect will be introduced by the violation of either assumption.

[11] Recent examples include: (i) in the United Kingdom: Bryan, Salvatori and Taylor, (2013); Dickens, Riley and Wilkinson (2009; 2012); and Dickens and Draca (2005); and (ii) in North America: Burkhauser, Hansen and Sabia (2012), and Campolieti, Fang and Gunderson (2005).

Box 3.1 Scientific experiments, natural experiments and difference-in-differences

The DID technique is often regarded as one of the closest alternatives economics has to scientific randomised controls experiments for program and policy evaluation (Borland, Tseng and Wilkins 2004). In randomised controls experiments, a sample of statistically identical individuals is chosen at random from a given population. Within that sample, one group is then randomly subjected to a treatment, while another group is not. The difference in some measurable outcome between the first and the second group is then noted. As the only significant development to have affected either of the groups, the treatment is the only causal explanation for any observed difference in outcome.

Because true scientific experiments are rare in economics, difference-in-differences instead takes advantage of natural experiments, whereby an unexpected change in some regulatory or legislative constraint affects one group and not the other. This creates the opportunity to measure the reaction of the 'treated' group, by reference to the 'untreated' group — a quasi-experiment. In other words, it is 'as if' allocation to the treatment and control groups was undertaken randomly.

The DID technique does not require perfect comparability between groups, but compositional changes over time can pose a problem. The initial requirement is that, in the absence of treatment, the two groups are influenced by the same factors and that they respond in the same way to those factors. For example, the treatment and control groups may have differing probabilities of job loss to start with, perhaps because of a difference in composition between groups, but these probabilities follow parallel paths in response to the business cycle (figure 3.1). A problem for DID arises if, over time, the composition of either group changes in a way that affects their respective outcomes. Then, the treatment can no longer be regarded as the only event to have affected outcomes.

One way to mitigate this problem is to add control variables to the DID analysis. As long as these variables are not affected by the minimum wage increase, tracking them over time will absorb part of the impact of changing characteristics. As mentioned by Stewart and Swaffield (2008, p. 152), 'in adding these control variables, the aim is to deal with any differences between the … "treatment" group … and the … "control" group … not controlled for with the additive group and time effect dummies'. Watson (2004, p. 168) notes that 'in a "natural" experiment the assumption that no further controls are needed is erroneous' and that 'in the absence of such controls, the burden falls even more heavily upon the choice of control group'. Hamermesh (1995, p. 837) concludes that '[a natural experiment] is more powerful when substantial effort is made to control for the changing determinants of the outcome'.

Even though the addition of control variables can alleviate the problem of non-parallel paths in DID, it may not completely negate it. Some relevant characteristics of either group may be unobserved (due to data constraints) or unobservable (by nature). As such, they cannot be added to the analysis, yet may influence the divergence in outcomes between the two groups.

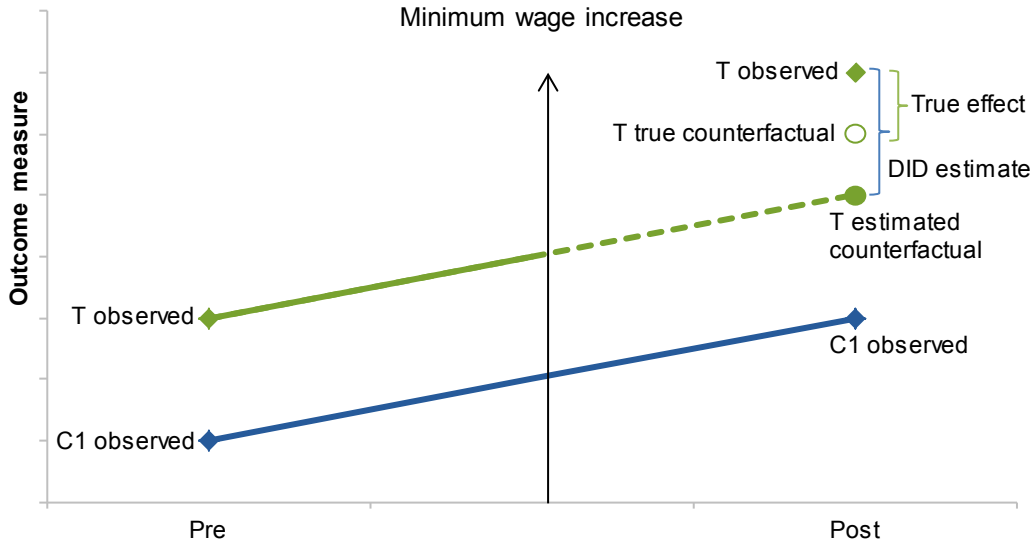
In this analysis, all models are estimated with and without control variables. Information on these variables is provided in appendix D.

Sources: Lechner (2011); Stewart and Swaffield (2008); Borland, Tseng and Wilkins (2004); Watson (2004); Hamermesh (1995).

The derivation of the DID estimate based on the parallel paths assumption is illustrated in figure 3.1. The estimate will be biased if there is a discrepancy between the unknown true

counterfactual ('T true counterfactual'), which is where the treatment group would have been, post-transition, in the absence of treatment, and the estimated counterfactual ('T estimated counterfactual'), which is determined based on the control group ('C1 observed') and the parallel paths assumption. The likelihood that the parallel paths assumption holds can be increased through the use of control variables (box 3.1).

Figure 3.1 **The parallel paths assumption and DID estimate[a]**



[a] In the legend, 'T' stands for Treatment, 'C1' stands for Control_1, and 'CF' stands for counterfactual.

Within the minimum wage literature, individuals are typically allocated to the treatment or control groups based on either geographic region or their wage. Geographical-based control groups are usually favoured by researchers but, due to the lack of geographical variation in Australia's minimum wage, the present analysis is conducted using higher wage groups as controls. This approach has been used extensively in the numerous minimum wage studies commissioned by the United Kingdom Low Pay Commission (for example, Bryan, Salvatori and Taylor (2013), and Dickens, Riley and Wilkinson (2009)). Wage-based control groups have been criticised on the grounds that higher-paid workers are imperfect comparators for minimum wage workers (Campolieti, Fang and Gunderson 2005). On the other hand, geographic group allocation creates the need to control for differences in local economic conditions over time, which has been a longstanding source of debate (for example, Dube, Lester and Reich (2010), and Sabia, Burkhauser and Nguyen (2015)).

In this technical supplement, workers are allocated to the following groups in the standard model:

- The Treatment group captures those individuals expected to be directly affected by the minimum wage increase.

- Control_1 is the wage group directly above the Treatment group.

Full details of the group definitions are provided in section 3.3 and appendix C.

3.2 The difference-in-differences approach with spillovers

An extension of the standard approach involves the inclusion of additional 'spillover' groups. This addresses the possibility that the group located directly above the Treatment group may also be affected by minimum wage increases.

Within the Australian context, the effects of the minimum wage increase will almost certainly spill over into higher wage groups to some extent, due to simultaneous adjustments made to award-based wages by the FWC and its predecessors. Employer-reported data presented in the FWC's Australian Workplace Relations Study First Findings report (2015) suggest that at least 18 per cent of employees have their wage set at exactly the award rate, and that the relevant award is used as a guide for setting the wages of up to a further 18 per cent of employees. This issue is explored in more depth in section 4.5.

Spillover effects may also arise outside of the award system. For example, employees may have their wages linked to the national minimum wage decisions though collective agreements or individual arrangements, or may be affected if employers pass on wage adjustments to higher earners to preserve wage relativities (sub. 158). Evidence of the latter is presented in Yuen, Rozenbes and Farmakis-Gamboni (2015). Also, employers may decide to substitute higher-paid workers (whether or not paid according to awards) for minimum wage workers, which would increase the wages of the former group through heightened labour market demand (Stewart 2009).

The existence of such spillover effects means that regarding workers in Control_1 as a genuine control group biases the standard analysis. To remedy this, it is instead defined as an additional treatment group within the spillover analysis. Furthermore, the following additional groups are included in the spillover model:

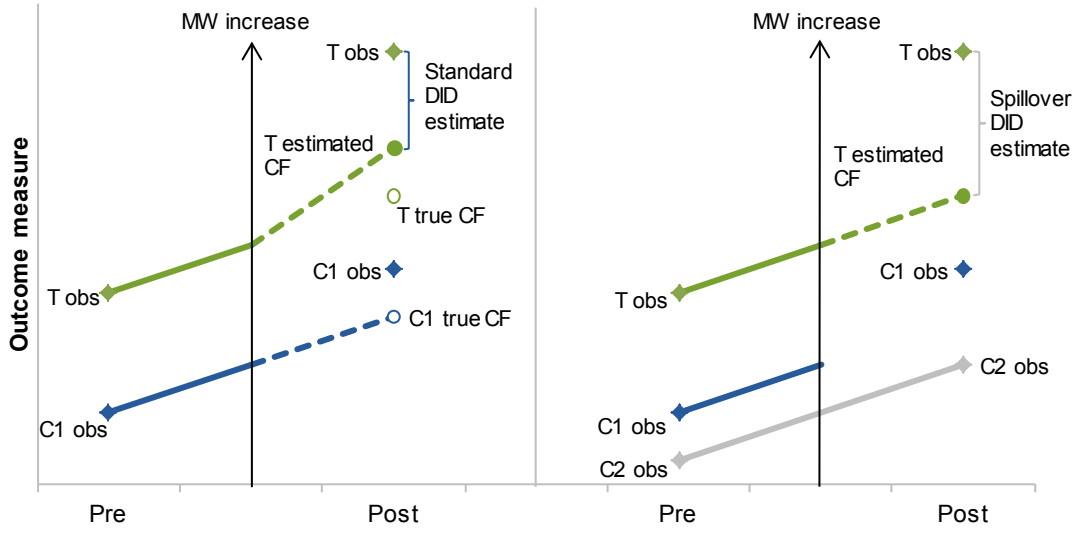
- the Control_2 group is the control group in the spillover model. It is adjacent to Control_1, higher up the wage distribution. This group is likely to better satisfy the DID assumption of being unaffected by the minimum wage change than Control_1.[12] However, the likelihood of parallel paths between the Treatment group and the Control_2 group may be weaker, since the groups are more likely to have different characteristics
- the Below group captures individuals earning a wage below the lower bound of the Treatment group

12 Noting that award coverage decreases as hourly wages rise (Buchanan and Wright 2013).

- the High group captures individuals earning a wage above the upper bound of the Control_2 group. In the literature, the High group has no theoretical upper limit. It seems reasonable, however, to set an upper bound for the High group, especially to avoid including wages that are obvious reporting errors.[13] (As previously foreshadowed, further discussion of group definitions is presented in section 3.3.)

The logic of DID estimation in the presence of potential spillover effects is illustrated in figure 3.2. (Below and High are omitted from the diagrams.)

Figure 3.2 **Standard versus spillover model DID estimates[a]**



[a] In the diagrams, 'T' stands for Treatment, 'C1' stands for Control_1, 'C2' stands for Control_2, 'CF' stands for counterfactual, and 'obs' is short for observed.

If the standard model is used (on the left-hand side of figure 3.2), the estimated counterfactual for Treatment ('T estimated CF') is constructed based on 'C1 obs', since Control_1 is assumed to be unaffected by the treatment. This results in the standard DID estimate. However, if both the Treatment and Control_1 groups experience minimum wage effects, 'C1 obs' *is* minimum wage-affected, and is different from the true Control_1 counterfactual. As a result, the standard DID estimate underestimates the true minimum wage effect, which should be based on 'T true CF'.

To remedy this problem, in the spillover model (on the right-hand side of figure 3.2), the assumed counterfactual for the Treatment group is instead constructed based on the observed path of Control_2. If the assumption of parallel paths between Treatment and Control_2 holds and Control_2 is unaffected by the minimum wage, then the assumed

[13] While they are not necessary for performing the DID analysis, the Low and High groups can assist in producing more accurate estimates, through greater degrees of freedom.

counterfactual for the Treatment group will equal the true counterfactual, and the spillover DID estimate will be accurate.

The DID estimates on their own do not reveal whether the standard or spillover model is more correct. Although some alternative DID approaches have been proposed to detect and remedy non-parallel paths, they are not able to be implemented here (box 3.2). However, in chapter 4, in an attempt to determine which model is more appropriate, descriptive analyses are used to compare the effects of the minimum wage on the wages of Control_1 and Control_2, as well as to compare the similarity of their characteristics with the Treatment group. From this, some preliminary conclusions are drawn regarding the extent of any spillovers affecting these groups, and the validity of the parallel paths assumptions.

3.3 Treatment and control group definitions

In the conventional DID approach, the Treatment group is made up of those with hourly wages between the old and new minimum wages at the beginning of the transition, who will necessarily require a wage increase when an uprating occurs. Such narrowly defined groups may not, however, capture all minimum wage workers. This could be due to some of the factors already described, including failure to account for casual loading, or perhaps measurement, recall or rounding error. In particular, bunching at round values of the hourly wage is prevalent in RED, as it is in many other datasets relying partly on recall. To deal with sources of error that potentially distort the calculation of hourly wages, each model is estimated using three different definitions of the Treatment and control groups — narrow, medium and wide.

Before examining these definitions in more detail, they are put in context in table 3.1, which sets out the minimum wage increases that occurred between 2008 and 2013. For comparison purposes, changes in the wage price index (WPI) and average weekly ordinary time earnings (AWOTE) are also presented. As the table shows, there was no minimum increase in 2009. The largest increase of the period was granted in 2010, and the smallest in 2013. In 2008 and 2010, all award rates were increased by an equal dollar amount (resulting in a proportionally higher increase for individuals earning lower wages).[14] From 2011 to 2013, equal percentage increases to all award rates were handed down (Workplace Info 2015).

[14] Notably, in 2010, the minimum wage was increased by $26.12 per week, while other adult award rates were only increased by $26 per week — a relatively smaller amount again in percentage terms.

Box 3.2 Alternative difference-in-differences approaches

Alternative approaches that do not hinge on the assumption of parallel paths between the treatment and control groups have been devised for estimating the unobserved counterfactual.

For example, to determine whether their standard DID results rest crucially on the parallel paths assumption, Bryan, Salvatori and Taylor (2013) implement two alternative versions of the DID approach, which they call vertical DID and triple DID. These approaches involve comparing the change in outcomes of their treatment and control groups with the change in outcomes of two additional groups higher up the wage distribution. Provided the two new groups are not affected by the minimum wage increase, they provide an independent benchmark. In the case of triple DID, for instance, the assumption is that the DID between the two higher-wage groups will be equal to the DID between the treatment and control groups in the absence of the minimum wage increase. Any divergence between the two DID estimates is attributed to the minimum wage effect.

These alternative approaches have not been trialled in here. In effect, they replace one parallel paths assumption with another. In the Australian context, it is not clear that using information from two higher-wage groups will result in an improved estimate of the counterfactual, where low-paid workers are concerned. Productivity Commission estimates based on the ABS Longitudinal Labour Force confidentialised unit record file indicate that employment is less stable at the lower end of the wage distribution. In particular, a greater share of employees who have been employed in their current job or business for less than 12 months are found at the bottom end of the weekly earnings distribution. In addition, the distribution of employees who expect to leave their job in the next 12 months because their employment is temporary or for business reasons is skewed towards lower hourly wage values. If there is more churn in employment at the lower end of the wage distribution, changes in the employment outcomes between two high-paid groups are unlikely to be a good counterfactual for the changes between two low-paid groups in the absence of a minimum wage increase.

Mora and Reggio (2012) show that the appropriateness of the parallel paths assumption can be tested by comparing treatment and control group trends over additional pre-treatment periods. Intuitively, the parallel paths assumption is more likely to hold if trends between the treatment and control groups do not differ over transitions that occur prior to treatment. Conversely, the existence of pre-treatment trend differentials can be accommodated through, for example, the addition of group-specific time invariant linear trends in the model (akin to a 'parallel growth' assumption).

Mora and Reggio's suggested models are not implemented in this technical supplement. In most cases, the timing of Australian minimum wage increases precludes the investigation of additional pre-treatment periods. (The employment transitions studied are detailed in section 3.4.) For example, an additional pre-treatment period for 2012 would overlap with the post-treatment period of the 2011 minimum wage increase, unless only short transitions were studied and their timing altered. Although Mora and Reggio's models are not formally tested, 2009 transitions are studied as a test of the parallel paths assumption, since no minimum wage increase occurred in that year (discussed further in section 3.4 and chapter 6).

Sources: ABS (2012); Bryan, Salvatori and Taylor (2013); Mora and Reggio (2012).

Table 3.1 **Minimum wage changes**
2008–2013

	Units	*Oct 2008*	*Oct 2009*	*Jul 2010*	*Jul 2011*	*Jul 2012*	*Jul 2013*
Minimum wage							
Old minimum wage	$	13.74	14.31	14.31	15.00	15.51	15.96
New minimum wage	$	14.31	14.31	15.00	15.51	15.96	16.37
Percentage increase	%	4.1	0.0	4.8	3.4	2.9	2.6
Wage growth							
Change in WPI[a]	%	4.2	4.0	3.1	3.8	3.6	3.3
Change in AWOTE[b]	%	4.0	6.1	5.2	4.4	3.4	5.3

[a] Annual change in wage price index to June, based on total hourly rates of pay excluding bonuses in private and public sectors, across all industries, seasonally adjusted. [b] Annual change in adult full-time average weekly ordinary time earnings to May quarter, seasonally adjusted.

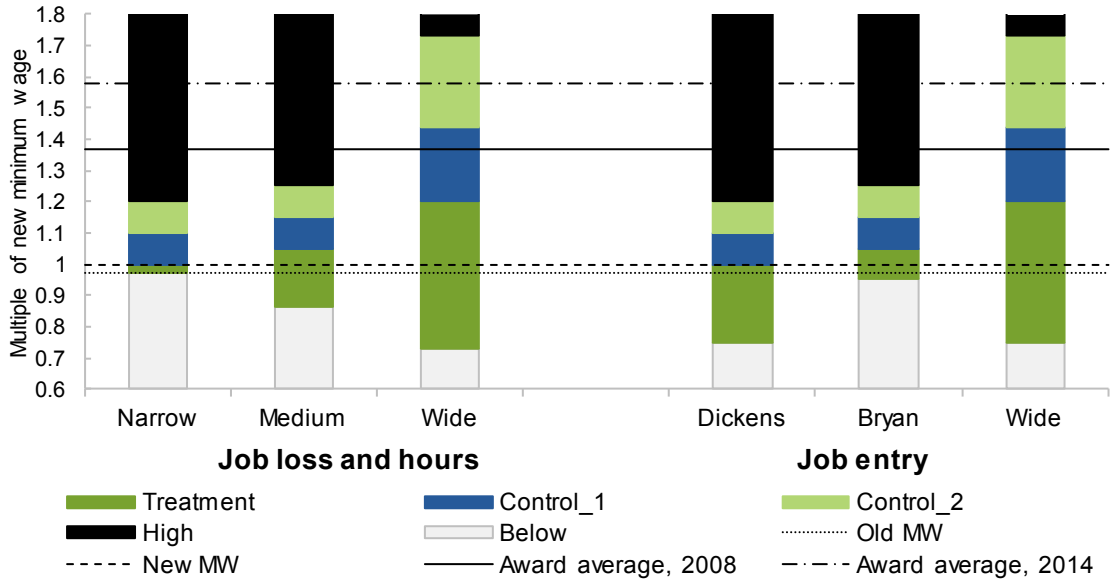
Sources: ABS (*Average Weekly Earnings, Australia*, Cat. no. 6302.0; *Wage Price Index, Australia*, Cat. no. 6345.0).

The three sets of group definitions used for the job loss and hours models are illustrated on the left-hand side of figure 3.3, and detailed in table C.1 in appendix C. The narrow definition is closest to the conventional DID approach, where Treatment is bounded by the old and new minimum wages. The upper bounds of the Control_1 and Control_2 groups are 110 per cent and 120 per cent of the new minimum wage, respectively. For the medium groups, the lower bound of the Treatment group is determined by looking at the data and identifying the wages of individuals who received pay rises consistent with the new minimum wage, while the upper bound is 5 per cent above the new minimum wage, to capture individuals who round up their earnings or round down their hours. The upper bounds of Control_1 and Control_2 are 115 per cent and 125 per cent of the new minimum wage, respectively. In the wide groups, the higher Treatment upper bound is designed to capture casual workers who receive casual loading, and workers earning penalty rates. The Control_1 upper bound is 1.2 times the Treatment upper bound, and the Control_2 upper bound is 1.2 times the Control_1 upper bound.

The groups are defined differently for the job entry model (displayed on the right-hand side of figure 3.3 and in table C.2 in appendix C). By definition, there is no group of *existing* workers whose wages put them 'at risk'. Rather, the groups are defined in terms of the wages that new hires receive, once they move into work, using criteria found in the literature. The Dickens groups are most similar to the definition used in Dickens, Riley and Wilkinson (2009), with the Treatment group defined as those earning up to and including the new minimum wage, but with the addition of a non-zero lower bound. Control_1 members earn between 100 and 110 per cent of the new minimum wage, and Control_2 members earn between 110 and 120 per cent of the new minimum wage. The Bryan groups employ Bryan, Salvatori and Taylor's (2013) approach, with Treatment group individuals earning 95 to 105 per cent of the new minimum wage. Control_1 and Control_2

individuals earn between 105 and 115 per cent, and 115 and 125 per cent of the new minimum wage, respectively. Finally, the wide job entry groups are defined in the same way as for job loss and hours, and are designed to account for casual loading and penalty rates.

Figure 3.3 **Job loss/hours and job entry group definitions[a,b,c]**



[a] These group definitions, when framed as multiples of the new minimum wage, are mostly consistent across all years. The exception is the lower bound of the medium Treatment group, which varies from year to year. Table C.1 in appendix C provides details. [b] The Department of Employment (sub. 158) reports that 102 of the 122 modern awards have minima that exceed the national minimum wage by less than 10 per cent. As such, the old award minima line is drawn at 1.1 times the old minimum wage, and the new award minima is 1.1 times the new minimum wage. [c] While not shown on the graph, the Below group extends to zero, while the High group extends to an hourly wage of $50.

Each of the sets of groups has the potential to capture different categories of wage earners (box 3.3). While each has theoretical and technical justification, descriptive analyses are used to shed further light on the appropriateness of each alternative. These analyses are presented in chapter 4.

Box 3.3 Who does the Treatment group capture?

The objective of this technical supplement is to identify the effect of minimum wage upratings on minimum wage-reliant individuals, but other wage earners may also be unavoidably captured in the Treatment group. Measurement error and the lack of adjustment for casual loadings and penalty rates provide a rationale for extending the Treatment group beyond a strict definition of minimum wage workers, but this leads to the inclusion of some individuals being paid under alternative arrangements.

Few individuals are 'true' minimum wage earners. In its submission to the Productivity Commission's Workplace Relations Framework inquiry, the Department of Employment estimates that, in 2014, only about 1.6 per cent of employees were paid *exactly* the then national minimum wage (rounded up from $16.37 to $16.50). This includes employees paid the national minimum wage under the national minimum wage order or an award (wages on par with the national minimum wage are found in 45 of 122 modern awards), as well as under individual arrangements or collective agreements. Regardless of their wage-setting arrangement, anyone earning exactly the minimum wage is entitled to receive a wage rise if the national minimum wage is increased.

Given that some of the Treatment group definitions extend below and above 100 per cent of the new minimum wage, who are the additional individuals likely to be captured?

First, many individuals earning the lowest wage in their award will be captured. More than 80 per cent of the modern awards have minima that are either equal to the national minimum or are within 10 per cent of it (the award minima lines in figure 3.3). Although individuals paid more than the national minimum are not pure minimum wage earners, some will be captured as soon as the Treatment group is extended above 100 per cent of the national minimum wage. That said, their inclusion in the Treatment group need not be problematic because their wages increase when the national minimum wage increases, and they are therefore subject to the 'treatment' at the centre of DID.

However, some low-wage individuals who are not directly affected by minimum wage rises may also be captured in the Treatment group. Some individuals paid more than the award minima are captured in the wide Treatment groups (figure 3.3). But, in addition to this, the wages of some low-paid workers who have their pay set through individual arrangements or collective agreements do not necessarily rise with the award, as long as they continue to be paid at or above the relevant award rate. These unaffected individuals may therefore contaminate the Treatment group.

All in all, using progressively wider Treatment groups captures employees paid under a more varied range of wage-setting arrangements. While some of these additional employees appropriately belong in the Treatment group, some others do not, but are unavoidably included.

Source: Department of Employment (sub. 158).

3.4 Employment transitions

The econometric analyses examine six-month employment transitions occurring from 2008 to 2013.[15] The use of six-month transitions is common within the minimum wage literature (box 3.4).

Box 3.4 Timing of minimum wage employment effects and DID

The time profile of any employment effects arising from a minimum wage increase is not known with precision. Neumark and Wascher (2010) contend that arguments can be mounted in favour of both rapid effects and lagged effects. Thus, they regard the question of timing as an empirical one. Their review of the international literature leads them to conclude that lagged effects do indeed occur, as well as quasi-instantaneous ones.

It should be emphasised that lagged responses are different from responses to *cumulative* minimum wage increases. The possibility that small successive wage increases can have a large overall effect *eventually* has been investigated in the literature (Campolieti, Fang and Gunderson 2005). However, this is conceptually different from a lagged, or delayed, response to a single minimum wage increase. Delayed employer responses may result from technical, regulatory or administrative factors that are internal to the firm, or because minimum wage increases eventually have consequences for demand for output (for example, through prices or incomes), and, therefore, employment.

Adding to the timing uncertainty is the possibility that some employers' responses to minimum wage increases are anticipatory (Dickens and Draca 2005). Because minimum wage decisions are announced in advance of their application, workforce adjustments that precede the rise in wages could potentially save the firm money during the minimum notice period for dismissal. Moreover, a firm expecting wage increases may defer or limit hires.

Timing issues complicate the choice of time period used in the DID analysis. This is especially true in the context of annual upratings. While it might be desirable to extend the post-increase window of observation beyond five months, this would overlap with the next uprating's pre-increase window. Disentangling short-run and long-run effects in this environment becomes fraught.

This technical supplement adopts international DID practice to define pre- and post-increase transitions of six months duration. Nonetheless, the possible existence of longer-term effects is acknowledged and discussed further in the Australian context in section 6.4.

Sources: Campolieti, Fang and Gunderson (2005); Dickens and Draca (2005); Neumark and Wascher (2010).

The models are estimated not just for years where the minimum wage increased (2008, 2010, 2011, 2012 and 2013), but also 2009, when the minimum wage was unchanged. Using 2009 as a 'placebo test' affords an opportunity to check the parallel paths assumption directly, something that is not often available to researchers dealing with annual upratings, and is, therefore, a strength of this technical supplement. Significant estimates for 2009 indicate changes in outcomes between the Treatment and control groups

[15] Although data are available for 2007, the choice of six-month transitions precludes the inclusion of the 2007 minimum wage uprating due to an overlap with the post-treatment period of the 2006 uprating.

that are not related to changes in the minimum wage, and may, therefore, be interpreted as parallel paths violations. The 2009 transitions cover the months that correspond with the 2008 transitions (based on an October uprating), to avoid an overlap with the post-treatment period in 2008, and use the same group definitions as 2010, since the same minimum wage ($14.31) applies at the time of group allocation in both years.

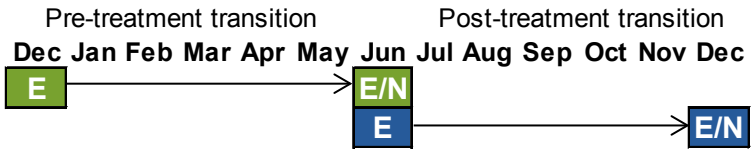
In the following sections, the transitions observed in each of the models are described.

The job loss model

The first analysis explores the effect of minimum wage increases on the probability of job loss for the Treatment group. This requires the observation of employment transitions, as shown in figure 3.4.

Figure 3.4 **Job loss transitions**[a]

July minimum wage increases[b]



[a] 'E' stands for employed, and 'N' represents inactivity (unemployed or not in the labour force). 'E/N' indicates that the job could be either active or inactive at the end of the transition. [b] The minimum wage increased in July in 2010, 2011, 2012 and 2013. In 2008, the minimum wage increased in October, and as such, the pre-treatment period begins in March and ends in September, and the post-treatment period begins in September and ends in December. Those months are also used for 2009.

- In the example in figure 3.4, pre-treatment transitions begin and end prior to the minimum wage increase in July, while post-treatment transitions span the minimum wage increase. For the October 2008 uprating (and for the 2009 freeze), the sequence of observations is shifted forward to hinge on September instead of June.
- In the analysis, each of an individual's jobs is observed separately. This allows for the possibility that the individual holds two jobs, perhaps paying quite different wages. (Episodes during which an individual holds more than two simultaneous jobs have been excluded (box 2.1).)
- To have a value for job loss, an individual must have been employed in the job at the beginning of the transition, and remain in RED at the end of the transition. At the end of the transition, job loss is equal to one if the job is no longer active and the person is not employed elsewhere. Job loss is equal to zero if the job is still active at the end the transition, or if the job has been lost but the individual remains employed in another

job. [16] As with most existing minimum wage studies, it is not possible to distinguish between voluntary and involuntary separations in RED.

- Allocation to the wage groups occurs at the beginning of the pre- and post-treatment transitions.

A 'treatment dummy model' is used to estimate the job loss effects of each minimum wage increase between 2008 and 2013 via a logit functional form. As explained previously, despite the absence of a minimum wage increase in 2009, the models are also estimated over 2009 transitions for the purposes of comparison. This model follows Bryan, Salvatori and Taylor's (2013) approach. It is estimated on stacked years data, and the DID coefficients (as well as the post-treatment and group dummies) are allowed to vary between years. In the equation below, *k* represents each year in the sample. The model is estimated with clustering over individuals, to correct for correlation in the unobservables for jobs that belong to the same person. Equation (1a) is the standard model, estimated with Treatment and Control_1 only. Equation (1b) is the spillover model, and includes all groups: Below, Treatment, Control_1, Control_2 and High.

$$P[e_{it+1} = 0|e_{it} = 1] = \Lambda[\beta_0 + X'_{it}\beta + \sum_k \beta_1^k Post_{t+1}^{\mathrm{k}} + \sum_k \beta_2^k Treatment_t^{\mathrm{k}} + \sum_k \beta_3^k (Post_{t+1}^{\mathrm{k}} * Treatment_t^{\mathrm{k}}) + \sum_k \beta_4^k Year^{\mathrm{k}}] \quad (1a)$$

$$P[e_{it+1} = 0|e_{it} = 1] = \Lambda[\beta_0 + X'_{it}\beta + \sum_k \beta_1^k Post_{t+1}^{\mathrm{k}} + \sum_k \beta_2^k Below_t^{\mathrm{k}} + \sum_k \beta_3^k Treatment_t^{\mathrm{k}} + \sum_k \beta_4^k Control\_1_t^{\mathrm{k}} + \sum_k \beta_5^k High_t^{\mathrm{k}} + \sum_k \beta_6^k (Post_{t+1}^{\mathrm{k}} * Below_t^{\mathrm{k}}) + \sum_k \beta_7^k (Post_{t+1}^{\mathrm{k}} * Treatment_t^{\mathrm{k}}) + \sum_k \beta_8^k (Post_{t+1}^{\mathrm{k}} * Control\_1_t^{\mathrm{k}}) + \sum_k \beta_9^k (Post_{t+1}^{\mathrm{k}} * High_t^{\mathrm{k}}) + \sum_k \beta_{10}^k Year^{\mathrm{k}}] \quad (1b)$$

- The dependent variable, $P[e_{it+1} = 0|e_{it} = 1]$, is the probability of being out of employment at the end of the transition (*t+1*), given that the individual (*i*) was employed in the job at the beginning of the transition (*t*).
- $Post_{t+1}^{\mathrm{k}}$ takes a value of one for transitions in which the new minimum wage was in force by the end of the transition. It is equal to zero for pre-treatment transitions.
- The dummy for each year, $Year^{\mathrm{k}}$, is included to allow the intercept to vary from year to year. The intercept is $\beta_0 + \beta_4^k$ in the standard model, and $\beta_0 + \beta_{10}^k$ in the spillover model.
- For the purposes of estimation, a dummy variable is created for each group — $Below_t^{\mathrm{k}}$, $Treatment_t^{\mathrm{k}}$, $Control\_1_t^{\mathrm{k}}$, $Control\_2_t^{\mathrm{k}}$ and $High_t^{\mathrm{k}}$ — taking a value of 1 if the individual is in the group at the beginning of the transition in year *k*, and 0 otherwise.

[16] This definition of job loss accords with that used in most overseas DID studies of the minimum wage, which only have access to information on an individual's employed/not employed status.

  - When estimating equation (1a), the Control_1 group is the omitted group, and is therefore the reference group against which the Treatment estimates are measured.
  - In equation (1b), the Control_2 group is the reference group against which the other groups' estimates are measured.
- X'_{it} is a vector of control variables which is included to account for changing characteristics over time within the sample of observations (box 3.1). While all other parameters are allowed to vary between years, the coefficient on each individual control is assumed to be constant across all years.[17] The controls used are dummy variables for age group, gender, income support payment type, current duration on income support (measured in months), whether the job is full-time, and whether the individual speaks English. The same controls are also used in the hours and job entry models. Further details on the control variables are presented in appendix D. Following standard practice, the model is also estimated without controls to examine the role of compositional differences between groups.
- The coefficient of interest for each year is the DID estimator.
  - In equation (1a), the DID estimator is β_3^k. As the coefficient on the interaction term between $Post_{t+1}^{\mathrm{k}}$ and $Treatment_t^{\mathrm{k}}$, it captures the effect of the treatment on the treated in year k.
  - In equation (1b), the DID estimator is β_7^k. Given the DID assumptions, a significant value for this coefficient would indicate that members of the Treatment group have been affected by the uprating. A significant coefficient on the interaction between $Post_{t+1}^{\mathrm{k}}$and $Control\_1_t^{\mathrm{k}}$, namely β_8^k, indicates that the Control_1 group experiences a change in its probability of job loss relative to the Control_2 group, after the increase in the minimum wage. Under the assumption that Control_2 is unaffected by the uprating, a significant β_8^k may be interpreted as evidence of spillover effects.

The treatment dummy model results in a unique DID estimate for each year but, alternatively, the upratings can be pooled to summarise the effects of the multiple increases. In the pooled variant, the DID coefficient is constrained to be equal across years. The advantage of pooled models is that they exploit changes in the outcomes across a number of years, thereby making better use of all available information in an attempt to capture the underlying minimum wage effect. On the other hand, the pooled estimate is a complicated weighted average of different minimum wage effects in different years (table 3.1), which makes its interpretation somewhat ambiguous if the direction of the effect varies by year.

All pooled models are estimated using a 'wage gap' measure in place of the treatment dummy, to account for the varying magnitudes of different minimum wage increases. The wage gap captures the intensity of treatment rather than just its occurrence. It is a measure of the new minimum wage relative to the individual's wage prior to the increase. In other

[17] In preliminary investigations, allowing the coefficients on the control variables to vary by year made little difference to the DID estimates.

words, it is related to the proportional increase required for the individual to earn the new minimum wage. The maximum value of the wage gap will increase with the size of the minimum wage increase, so it is useful in distinguishing large increases, such as 2010, from smaller increases (table 3.1). The preferred measure comes from Bryan, Salvatori and Taylor (2013) (which is the same as in Dickens, Riley and Wilkinson (2009)):

$$Gap_{it}^{k} = \begin{cases} \ln\left(\dfrac{NMW_t^{k*}}{w_{it}^{k}}\right), & Treatment_t^{k} = 1 \\ 0, & Treatment_t^{k} = 0 \end{cases}$$

In this expression, w_{it}^{k} denotes the individual's wage at the beginning of the transition, and NMW_t^{k*} denotes the new minimum wage. Under the medium and wide group definitions, where the upper bound of the Treatment group is not the new minimum wage, NMW_t^{k*} is replaced with the upper bound of the Treatment group. (Using the upper bound of the Treatment group ensures that the equation always gives a wage gap value bounded from below by zero.) Similarly, when a Treatment group member's original wage is below the old minimum, the relevant wage gap is calculated from that original wage. The wage gap measure is defined only for the Treatment group, and takes a value of zero for the comparison group. In the standard model, which has only two groups, the comparison group is Control_1. In the spillover model, the comparison group is the Control_2 group, and the Below, Control_1 and High groups are excluded. As a result, the spillover wage gap model is akin to a standard DID between the Treatment the Control_2 groups.

The implemented pooling approach enforces constant group differences, meaning that the coefficient on Gap_{it}, along with all other variables in the model, is constrained to be equal across years. The 2009 transitions are excluded from the pooled model (box 3.5).

The pooled job loss model is:

$$P[e_{it+1} = 0 | e_{it} = 1] = \Lambda\big[\beta_0 + X_{it}'\beta + \beta_1 Post_{t+1} + \beta_2 Gap_{it} + \beta_3 (Post_{t+1} * Gap_{it}) + \sum_k \beta_4^k Year^{k}\big] \quad (2)$$

In equation 2, the DID coefficient is β_3. A positive DID coefficient in the pooled standard model may be interpreted as evidence that upratings over the period have generally been associated with an increased probability of job loss for the Treatment group relative to the Control_1 group (the omitted group). In the pooled spillover model, the effect on the Treatment group is measured relative to the Control_2 group.

Box 3.5 Excluding 2009 from the pooled model

The possibility of including 2009 in the pooled models was investigated, but, ultimately, the 2009 transitions were excluded. To test the inclusion of 2009, pooled job loss models were estimated with (in combination):

- a dummy for 2009 included in the set of year dummies
- the wage gap set equal to zero for all 2009 observations, to reflect the absence of treatment
- two sets of control variables: one set interacted with a dummy for the 2009 transitions, and another set that was not interacted.

After estimating a model for each treatment group definition, a Chow test was performed to test the null hypothesis that the coefficients on the controls in 2009 were equal to the coefficients on the controls in the other years. In each case, the null hypothesis was rejected. Since the relationship with the covariates was different in 2009, it was determined that the inclusion of 2009 did not add to the accuracy of the estimates for years where the minimum wage increased. Therefore, 2009 was excluded from the pooled job loss and hours models.

Source: Woolridge (2009).

The change in hours worked model

The hours model is used to estimate whether changes in the minimum wage affect hours worked by minimum wage workers. Employers may have greater flexibility to make adjustments to hours, rather than instituting layoffs or changing hiring practices. In particular, the hours worked by part-time and casual employees may vary when the minimum wage rises, as may overtime for permanent employees. In this analysis, changes in hours worked by individuals in the Treatment group before and after the minimum wage increase are compared with the corresponding change in hours for Control_1 group workers in the standard model, and Control_2 group workers in the spillover model.

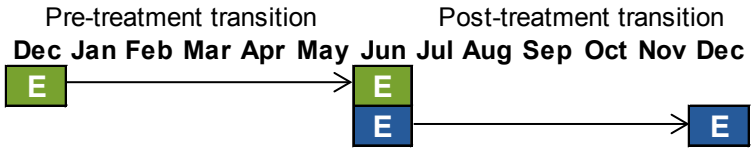
As figure 3.5 illustrates, the timing of transitions studied is the same as for the job loss models.

- To calculate the dependent variable, $Hourschange_{it+1}$, fortnightly hours worked (averaged over a month) at the beginning of the transition are subtracted from fortnightly hours worked at the end of the transition. This calculation requires that the individual is employed in the same job at the beginning and end of the six-month transition (and remains in RED).[18]
- Jobs are allocated to the wage groups based on their wage at the beginning of the pre- and post-treatment periods.

[18] Some versions of the hours model (for example, Dickens, Riley and Wilkinson (2009)) include individuals who have had their hours reduced to zero (that is, have been laid-off).

Figure 3.5 **Change in hours transitions**[a]

July minimum wage increases[b]



[a] 'E' stands for employed. [b]The minimum wage increased in July in 2010, 2011, 2012 and 2013. In 2008, the minimum wage increased in October, and as such, the pre-treatment period begins in March and ends in September, and the post-treatment period begins in September and ends in December. Those months are also used for 2009.

The hours models are estimated using ordinary least squares (OLS) on stacked years data with year-specific coefficients (apart from the coefficients on the control variables, which are again constant across years). Equation (3a) estimates the effect of each minimum wage increase on the Treatment relative to Control_1, and equation (3b) estimates the effect on Treatment relative to Control_2. As with job loss, the hours models are estimated with clustering over individuals.

$$Hourschange_{it+1} = \beta_0 + X'_{it}\beta + \sum_k \beta_1^k Post_{t+1}^{\mathrm{k}} + \sum_k \beta_2^k Treatment_t^{\mathrm{k}} + \sum_k \beta_3^k \left(Post_{t+1}^{\mathrm{k}} * Treatment_t^{\mathrm{k}}\right) + \sum_k \beta_4^k Year^{\mathrm{k}} \quad (3a)$$

$$Hourschange_{it+1} = \beta_0 + X'_{it}\beta + \sum_k \beta_1^k Post_{t+1}^{\mathrm{k}} + \sum_k \beta_2^k Below_t^{\mathrm{k}} + \sum_k \beta_3^k Treatment_t^{\mathrm{k}} + \sum_k \beta_4^k Control\_1_t^{\mathrm{k}} + \sum_k \beta_5^k High_t^{\mathrm{k}} + \sum_k \beta_6^k (Post_{t+1}^{\mathrm{k}} * Below_t^{\mathrm{k}}) + \sum_k \beta_7^k \left(Post_{t+1}^{\mathrm{k}} * Treatment_t^{\mathrm{k}}\right) + \sum_k \beta_8^k \left(Post_{t+1}^{\mathrm{k}} * Control\_1_t^{\mathrm{k}}\right) + \sum_k \beta_9^k \left(Post_{t+1}^{\mathrm{k}} * High_t^{\mathrm{k}}\right) + \sum_k \beta_{10}^k Year^{\mathrm{k}} \quad (3b)$$

- In the standard model (3a), the DID coefficient is β_3^k, which is the effect of the minimum wage on the Treatment group relative to Control_1.
- In the spillover model (3b), the DID coefficient is β_7^k, which is the effect of the minimum wage on the Treatment group measured relative to the Control_2 group. β_8^k captures the effect on Control_1 relative to the Control_2 group.

Pooled models are also estimated for the change in hours. The constant group differences model (equation (4)) is estimated using OLS. As in the job loss models, the parameter of interest in both the standard and spillover variants of the model is the coefficient on the interaction between Post and Gap for the Treatment group, and 2009 is excluded.

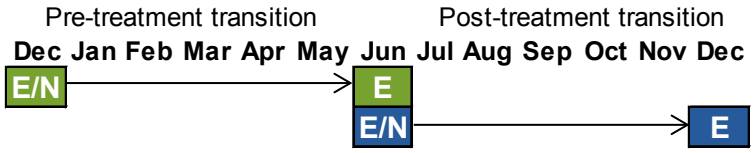
$$Hourschange_{it+1} = \beta_0 + X'_{it}\beta + \beta_1 Post_{t+1} + \beta_2 Gap_{it} + \beta_3(Post_{t+1} * Gap_{it}) + \sum_k \beta_4^k Year^k \quad (4)$$

In the hours models, negative DID coefficients in years of minimum wage increases or in pooled models suggest that upratings are associated with greater reductions in hours (or smaller increases) for Treatment group workers than comparison group workers, while positive DID coefficients indicate relative increases.

The job entry model

The possibility that the rate of hiring at the minimum wage is affected by an uprating is studied by estimating changes in the probability of having been outside employment in the previous period, given that the job is active in the present period (Dickens, Riley and Wilkinson 2009). The Treatment group contains people hired into jobs earning approximately the new minimum wage, while the control groups are those entering jobs paying just above the minimum wage or slightly higher. The differences in entry probabilities between the Treatment and control groups are compared before and after the minimum wage increase.

Figure 3.6 **Job entry transitions**[a]
July minimum wage increases[b]



[a] 'E' stands for employed, and 'N' represents inactivity (unemployed or not in the labour force). 'E/N' indicates that the job could be either active or inactive at the beginning of the transition. [b] The minimum wage increased in July in 2010, 2011, 2012 and 2013. In 2008, the minimum wage increased in October, and as such, the pre-treatment period begins in March and ends in September, and the post-treatment period begins in September and ends in December. Those months are also used for 2009.

- As depicted in figure 3.6, the subpopulation studied in the job entry model are individuals who are employed at the end of a transition, given that they were in RED at the beginning of the transition.
- Job entry will be equal to 0 if the individual was employed in the job at both the beginning and end of the transition (E to E in the diagram above).
- Job entry will be equal to 1 if the person was not employed in the job at the beginning of the transition, but was employed by the end of the transition (N to E in the diagram above). The probability of job entry is therefore expressed as $P[e_{it} = 0 | e_{it+1} = 1]$.

- Jobs are allocated to the wage groups based on the wage the individual was paid when they were first employed in that job across the course of the transition. The values of the control variables are also determined at the time of entry.

The job entry model permits the DID estimate to vary across years and is estimated via a logit functional form. There is no wage gap or pooled model version of job entry, as the wage gap has no interpretation in this context. Equations (5a) (standard model) and (5b) (spillover model) estimate a unique DID coefficient for each uprating, and are clustered over individuals.

$$P[e_{it} = 0|e_{it+1} = 1] = \Lambda[\beta_0 + X'_{it}\beta + \sum_k \beta_1^k Post_{t+1}^{\mathrm{k}} + \sum_k \beta_2^k Treatment_t^{\mathrm{k}} + \sum_k \beta_3^k (Post_{t+1}^{\mathrm{k}} * Treatment_t^{\mathrm{k}}) + \sum_k \beta_4^k Year^{\mathrm{k}}] \quad (5a)$$

$$P[e_{it} = 0|e_{it+1} = 1] = \Lambda[\beta_0 + X'_{it}\beta + \sum_k \beta_1^k Post_{t+1}^{\mathrm{k}} + \sum_k \beta_2^k Below_t^{\mathrm{k}} + \sum_k \beta_3^k Treatment_t^{\mathrm{k}} + \sum_k \beta_4^k Control\_1_t^{\mathrm{k}} + \sum_k \beta_5^k High_t^{\mathrm{k}} + \sum_k \beta_6^k (Post_{t+1}^{\mathrm{k}} * Below_t^{\mathrm{k}}) + \sum_k \beta_7^k (Post_{t+1}^{\mathrm{k}} * Treatment_t^{\mathrm{k}}) + \sum_k \beta_8^k (Post_{t+1}^{\mathrm{k}} * Control\_1_t^{\mathrm{k}}) + \sum_k \beta_9^k (Post_{t+1}^{\mathrm{k}} * High_t^{\mathrm{k}}) + \sum_k \beta_{10}^k Year^{\mathrm{k}}] \quad (5b)$$

- In the standard model (5a), a negative β_3^k in the year of a minimum wage increase would indicate that, in year k, the probability of being a new hire into the Treatment group falls relative to the probability of being a new hire into the Control_1 group in the post-treatment period.
- In the spillover model (5b), a negative β_7^k would indicate that the probability of being hired into the Treatment group falls relative to the probability of being hired into the Control_2 group. β_8^k will capture effects on Control_1 relative to the Control_2 group.

3.5 Estimation samples

The job loss, job entry and hours models are estimated over a number of different groups of individuals. In RED, earnings data are available for income support recipients and their non-payment partners (NPPs). To exploit these data, each of the models is estimated both for the full sample and for NPPs only. This allows the employment outcomes of income support recipients to be compared with those of other minimum wage workers. This set of findings makes up the primary results in chapter 5. Furthermore, as a check, three additional sets of models are estimated separately for income support recipients and non-payment partners belonging to the following groups:

- youths (aged 21 to 25)
- males (aged 21 to 64)
- females (aged 21 to 64).

The objective is to observe whether the employment effects of minimum wage increases vary for different groups of workers. A brief summary of these results is provided in section 5.3.

4 Descriptive statistics

Prior to presenting the results of the difference-in-differences analyses, this chapter sets the scene by comparing and contrasting various characteristics of the narrow, medium and wide definitions of the Treatment, Control_1 and Control_2 groups, used for the job loss and hours models.[19] Two key conditions, when carrying out DID, are:

1. to have the Treatment group affected by the minimum wage increase, and the control group unaffected, thereby isolating the minimum wage effect
2. to have the characteristics of the Treatment and control groups as similar as possible to improve the likelihood that the parallel paths assumption holds.

There is some tradeoff between these two objectives. A control group higher up the wage distribution is less likely to be affected by the minimum wage, but such a group is also less likely to satisfy the requirement of group comparability. Groups with common characteristics are more likely to be similarly affected by macroeconomic conditions and may also experience comparable seasonal employment patterns, which increases the likelihood that they follow parallel paths. The extent to which the two objectives are achieved by all or some of the current group allocations can be gauged through analysis of descriptive statistics.

4.1 Does the minimum wage bind?

For the minimum wage increase to affect the relative probability of job loss of minimum wage earners, that increase must be passed on to the targeted group. In addition, this increase must introduce a disturbance in relative wage levels or growth paths between minimum wage earners and higher-paid, control group workers.[20] If the latter group experiences a similar (rate of) wage increase as the Treatment group, the absence of disturbance will neuter differential employment effects. To establish whether any disturbances occurred, figures E.1, E.2, and E.3 in appendix E show movements in the median wages of the different groups in 2008, 2010, 2011, 2012 and 2013 (the 'uprating years'), and in 2009, when the minimum wage was frozen.

The median wage indexes for the narrow groups are illustrated in figure E.1. In all uprating years, the median wage of the Treatment group remains approximately constant in the

[19] No comparisons of the job entry groups have been performed. Broadly, the lessons from the narrow, medium and wide groups outcomes can be extended to the job entry groups.

[20] This requirement strictly applies in a short-run context which is that of DID. In the longer run, when the capital stock is variable, the price of capital inputs is also relevant.

pre-treatment period. In comparison, pre-treatment growth in the Control_1 and Control_2 indexes may suggest that wages in these groups increase independently of the minimum wage. Corroborating evidence that even very low wages trend upward over time at a rate unrelated to the minimum wage upratings is provided by Bray (sub. 32), who shows that the ratio of the minimum wage to the tenth percentile of full-time adult non-managerial employees has been largely declining since 1991.

In the post-treatment period, the median wage of the Treatment group invariably increases to an extent broadly consistent with the minimum wage upratings over the period. This suggests that this group consistently captures jobs with wages that are bound from below by the minimum wage. There is no such discernible step up in 2009, when there was a minimum wage freeze. Increases in the median wages of the Control_1 and Control_2 groups around the time of the minimum wage uprating may partly reflect the presence of award-based wages in those groups, however, those increases are often a continuation of the pre-treatment trend.

Figure E.2 presents the wage indexes for the medium groups. In 2008, 2011 and 2012, all groups display an upward trend prior to the uprating. Possible explanations for this include minimum wage anticipatory effects, as well as the greater variance in wages under the medium definition. Alternatively, a fraction of employees in all groups may earn wages that move independently of the minimum wage. In the post-treatment period, the Treatment group experiences the greatest wage increases in 2011 and 2013, while Control_2 tends to be the least affected. In 2010, there is very little movement in the Treatment group's median wage index in either the pre- or post-treatment period. Upon closer inspection, this appears to be driven by data bunching at a wage of $15 per hour. Nevertheless, in a majority of cases, all groups appear to receive wage increases that coincide with the minimum wage uprating.

Movements in the wide groups' median wages are displayed in figure E.3. The greater dispersion observed under the wide definitions, compared to the other definitions, likely results from increased variance in the median wage due to the width of the groups. In each year (including 2009), the wide Treatment group's median wage index displays an upward trend from the beginning of the pre-treatment period, with no noticeable upward kink at the time of the minimum wage increase. By contrast, the Control_1 group's wages tend to be more constant in the pre-treatment period, before increasing after the minimum wage upratings. The wages of Control_2 members dip down in the pre-treatment period, before recovering in the post-treatment period. The control group trends are less pronounced in 2009.

A few conclusions can be drawn from the graphs in appendix E. First, there is evidence of pre-treatment increases in median wages under all group definitions (narrow, medium and wide). This may be because some wages (even low ones) move independently of minimum wage increases, or may simply reflect the variation in reported hourly wages within the RED dataset. In addition, although the minimum wage appears to be binding for the Treatment groups, there is also evidence that minimum wage increases spillover onto

control groups. The spillover link seems likely to be causal, due to award wages rising for example, although to some extent it could also be a timing artefact. (Most annual pay rises occur at the beginning of the financial year which, for the 2010–13 period, coincides with minimum wage uprating months.) The wide Control_2 group is usually the group least affected by the minimum wage increase, which is favourable for the purposes of DID.

A more detailed analysis of the changes in the wages of different groups that occurred after the 2010 minimum wage increase is presented in appendix F. The findings can be summarised as follows:

- Under the narrow and medium group definitions in particular, the minimum wage appears to be binding for the Treatment group (left-hand side of figures F.1 and F.2). Specifically, in 2010, the wage distribution of Treatment group jobs shifted higher between the pre- and the post-treatment periods, in a way that was greater than in 2009 (when no minimum wage increase applied).
- However, under all group definitions, Control_1 and Control_2 also experience wage increases that coincide with the minimum wage uprating (middle and right-hand side of figures F.1, F.2 and F.3). As such, this analysis confirms that spillovers onto higher wage groups are present, which is important for the interpretation of the estimation results.
- Figure F.3 for the wide groups highlights the necessary compromise between avoiding spillover effects and having comparability between the groups. Although the effects on Control_2 wages are small, that group of workers is located much higher in the wage distribution, and is, therefore, less likely to share characteristics with the Treatment group, making the parallel paths assumption less plausible. Furthermore, the wage increases in the wide Treatment groups are more dispersed, meaning that the minimum wage effect will be less effectively isolated than under the narrow or medium definitions.

By and large, the analysis in appendix F corroborates that in appendix E.

4.2 How comparable are the groups?

To assess the comparability of the Treatment, Control_1 and Control_2 groups, a series of pairwise t-tests is performed to compare the equality of the means of different variables for each group (appendix G). Statistically significant differences between the Treatment and control groups are overwhelmingly observed, although these disparities may not always matter in practice.[21] However, for some variables, statistically significant differences may be of more material relevance for DID purposes. For example, the Treatment group stands

[21] For instance, using the medium groups, the average Treatment group age in 2008 is 40.2 and the average Control_2 age is 39.5 (table G.1). This difference is statistically significant at the 1 per cent level, but this is a consequence of the very large number of observations in the sample — the average ages are essentially the same and the small difference is unlikely to influence the results.

out as having the lowest proportion of females (medium and wide groups) and the lowest proportion of English-speakers (table G.1), the longest average duration on income support (table G.2), and the lowest proportion of full-time workers (under the narrow and medium definitions) (table G.5). However, each of these four variables (as well as age) is included in the regressions with controls. Effectively, therefore, these characteristics are held constant across groups when evaluating the minimum wage effect via DID, which means that they are not introducing bias.[22]

Moreover, if the effect of differences in the observable characteristics of the groups are accounted for, it is more likely that unobserved characteristics will be indirectly accounted for. Such characteristics as motivation and work ethic may influence employment outcomes, but are difficult to measure. However, the results of the pairwise t-tests in appendix G indicate that the groups share many similarities which, to some extent, allays concerns about the impact of unobserved differences.

4.3 Treatment group stability

The stability of Treatment group membership reflects the accuracy and appropriateness of group allocation, which is relevant to the precision of the DID technique. In this supplement's analysis, group allocation is solely based on a job's wage at the beginning of the period. Regardless of what happens subsequently to that wage, the job will remain a Treatment group job until the end of the transition, or as long as it remains active. Knowledge of the changes that occur between the beginning and the end of a transition may reveal whether this is an adequate allocation rule.

To that end, the following exercise investigates what happens to narrow, medium and wide Treatment group jobs between allocation and observation at the end of six months. As in the DID analysis:

- jobs are allocated to the Treatment group based on their wage at the beginning of the pre- and post-treatment periods
- the Treatment group definitions are those displayed in figure 3.3 and appendix C
- the upper and lower nominal wage bounds are the same for both the pre- and post-treatment periods in each year.

In this exercise, all that matters is the job's first movement out of the Treatment group at any point across the transition. Therefore, unlike in the DID analysis:

- if a pay rise takes the job out of the Treatment group, and *then* that job is lost, that first exit is recorded as a pay rise, and the subsequent job loss ignored

[22] The bias in question is confounding bias. Differing characteristics, such as gender, may still bias the analysis if they influence the impact of the minimum wage increase.

- if a person exits a Treatment group job and enters another job, this is still regarded as a loss of the original job.

Table H.1 in appendix H shows the proportion of jobs in the Treatment group at the beginning of the pre- or post-treatment period that continue to earn a Treatment group wage at the end of the period (six months later), and summarises the remainder of the original group's reasons for leaving.

The findings suggest that a large proportion of jobs do not maintain their Treatment group status for the duration of the pre- or post-treatment period. Group stability is weakest for the narrow definition, and strongest for the wide definition. A maximum of about 33 per cent of original Treatment group jobs remain in the group and employed after six months. There are a number of possible reasons why jobs lose Treatment group status:

- Increases and decreases in wage over time prevent workers from being consistently allocated to finitely defined wage groups. Not surprisingly, the narrowly-defined Treatment group jobs are most likely to receive pay rises or pay cuts that move them out of the group. Fluctuations may occur due to measurement error, or just variability in wages (for example, due to penalty rates). Pay rises are the most common reason for jobs leaving the Treatment group. In the majority of year/group combinations, the proportion receiving pay rises is higher in the post-treatment period, which is expected due to increases in the minimum wage and award-based wages (in all years except 2009, when no minimum wage increase occurred).
- Individuals leave the Treatment group if they lose their job. This is the second most common reason overall for exiting the group. Job losses are a DID-relevant outcome and, therefore, pose no problem. The prevalence of job losses tends to be higher for the medium and wide groups than for the narrow groups.
- Individuals who begin to earn higher wages may lose income support eligibility and leave the sample. This affects the wide Treatment groups the most, as they capture higher earners. The nature of RED, therefore, introduces the potential for selection bias, whereby the sample is biased towards capturing individuals who maintain or reduce their incomes over time. The implications of this type of bias for DID are discussed in section 6.1.

In sum, the results of this exercise suggest that, across the narrow, medium and wide definitions, a majority of jobs are no longer technically in the Treatment group at the end of the six-month transitions. The reasons for exiting the group are relevant to their impact on DID. Narrow Treatment group jobs are most likely to have lost their Treatment group status by the end of the period, and this is often because of wage fluctuations. The wide Treatment group extends to higher earners, and therefore, has the largest proportion of jobs that have left RED by the end of the period. Job losses and pay rises in the post-treatment period are not problematic, but if instability in Treatment group status reflects measurement error, or group exit occurs because income support eligibility is lost, there may be consequences for the precision of DID. Some of these factors are considered in more depth in chapter 6.

4.4 The influence of casual workers

The lack of adjustment for casual workers in RED means that there is likely to be some misallocation of individuals to the Treatment, Control_1 and Control_2 groups. Individuals employed on a casual basis receive casual loadings (usually between 20 and 25 per cent), which means that the hourly wage they report will be higher than their true base rate of pay. Thus, depending on the group definition used, some individuals reporting Control_2 or Control_1 group wages might actually belong in the Treatment group, for example. If these individuals are affected by the minimum wage increase, then the estimated counterfactuals will be erroneous and the DID estimates will be biased.

Wave 12 of HILDA is used to gain an indirect indication of the prevalence of casual workers in RED, and the possible measurement error introduced by casual loadings. Some general observations emerge from this exercise (results not shown):

- Assuming a casual loading of at least 20 per cent, the narrow groups cannot capture any casual workers unless they are paid below the adult minimum wage or are subject to measurement error. This means that, in the absence of casual adjustment, there is no misallocation between the narrow Treatment, Control_1 and Control_2 groups, which should mean that the narrow specification is less affected by casual bias. That said, sub-minimum wage earners who receive a casual loading could still be included in the groups of interest, with positive, negative or negligible impacts on the estimates.
- The wide groups are less sensitive to casual adjustment than the medium groups, due to their wage bandwidth (figure 3.3). In particular, inability to adjust for casual loading may be less of an issue for the wide spillover model than for some of the alternatives (such as the wide standard model and the medium spillover model).

In conclusion, casual bias means that a varying minority of the Control_1 and Control_2 groups is likely to be directly affected by the minimum wage increase. This will interfere with the identification of the minimum wage effect using DID. In some cases, such as the narrow groups and the wide spillover groups, misallocation due to casual status is unlikely to cause major problems for the analysis. In the remaining groups, it may be more damaging, although its effects are impossible to quantify.

4.5 The extent of award reliance

As mentioned in section 3.2, Australia's system of statutory award wages will result in the effects of minimum wage increases spilling over onto higher-paid workers. If a large share of the control group is award-reliant, an increase in the minimum wage will lead to a smaller disturbance in the wages of minimum wage workers relative to the comparator, and the differential employment effect may be reduced.

In RED, it is not possible to know whether an employed person's wage is award-reliant or not, but other data sources may be used to ascertain the possible extent of award reliance

within the Treatment, Control_1 and Control_2 groups. For example, Buchanan and Wright (2013) find that, in 2013, 25 per cent of award-reliant employees were paid more than $18.60 per hour, but only 5 per cent received a base rate of $22.00 or more per hour, and only 1 per cent earned more than $30.00 per hour. These values provide a preliminary indication of what proportion of the Treatment, Control_1 and Control_2 groups is likely to be award-reliant in RED. However, a person with a base wage of $18.60 receiving 24 per cent casual loading[23] would appear in RED as earning $23.06 per hour. Therefore, since casual loading is unadjusted for in RED, the proportion of award-reliant employees will be different in the RED groups to what Buchanan and Wright's cut-off wages suggest initially. To correct for this, appendix I presents analysis based on HILDA that reveals that a greater proportion of the RED groups is likely to be award-reliant than their non-adjusted wages would suggest (see appendix I for further details).

All in all, the presence of spillovers resulting from award-reliance and award-based arrangements appears likely, since a nontrivial proportion of higher-earning employees have their wages determined by awards. However, award-reliance decreases at higher wages, which suggests that, other things equal, members of higher control groups are progressively less likely to experience wage increases that coincide with minimum wage upratings. Nevertheless, this issue still has the potential to bias DID estimates of the employment effects of minimum wage increases, to an extent which is impossible to measure.

4.6 Evaluating alternative specifications

Based on the descriptive statistics presented, and as described in section 3.3, each of the possible specifications — standard and spillover models, and narrow, medium and wide groups — has advantages and disadvantages (table 4.1). For instance, the wide Control_2 group best avoids the issue of award reliance, and the majority of this group's members remain in the group after casual adjustment. However, the observable characteristics of the wide groups display the greatest divergence from the Treatment group. The narrow groups effectively isolate the treatment effect, and largely escape casual bias since they can only capture casuals with sub-minimum base wages. However, there are likely to be high rates of award-reliance in the narrow Control_1 and Control_2 groups. The medium groups strike a compromise between some of the weaknesses of the narrow and wide definitions. However, they remain subject to the effects of casual bias, award-reliance spillovers and differences in observed characteristics.

In the absence of the relevant information in RED, considering only a single set of estimates at the exclusion of others is not warranted. Instead, the results from all models — standard and spillover, and narrow, medium and wide — are presented. Similarly, for job entry, the Dickens, Bryan and wide group definitions and the standard and spillover models are all presented in the next chapter. This approach to the reporting of results

[23] The award/agreement free rate of casual loading at the time of Buchanan and Wright's survey.

reflects more faithfully the uncertainty surrounding minimum wage effects. When interpreting these results, the relative strengths and weaknesses of the underlying models need to be kept in mind. Nonetheless, the variation in estimates between model specifications may itself be instructive in revealing the way in which minimum wage effects 'ripple through' the wage distribution.

Table 4.1 **Advantages and disadvantages of alternative group definitions**

Group definition	*Advantages*	*Disadvantages*
Narrow	• Misallocation to groups resulting from lack of adjustment for casual loading is less likely to occur (section 4.4). • Treatment most effectively captures affected workers (figures E.1 and F.1)	• The Control_1 and Control_2 groups are likely to capture a large share of award-reliant workers (figure I.1). • Control_1 and Control_2 record wage increases that coincide with the upratings (figures E.1 and F.1).
Medium	• Treatment experiences the greatest wage increases in the post-treatment period, while Control_2 is least affected (figure E.2). • Medium Treatment group is more stable than the narrow Treatment group, and has a lower proportion of group members who exit by the end of the period than the wide group (table H.1).	• In HILDA, the medium groups are relatively more affected by lack of adjustment for casual loading (section 4.4). • Just under 20 per cent of award-reliant workers earn wages that would place them in the medium Control_2 group (figure I.1) which may lead to wage spillovers associated with uprating (figure E.1).
Wide	• There is the least movement between the wide groups when casual loading is adjusted for (section 4.4). • The wide Control_2 group is likely to have a low proportion of award-reliant workers (figure I.1), and appears to be largely unaffected by the upratings (figure E.3).	• More chance that individuals paid under alternative arrangements that are unaffected by the minimum wage are captured in Treatment group (box 3.3). • Less comparability between groups (tables G.1 to G.5) which has implications for the validity of the parallel paths assumption.

5 Results

This chapter presents the job loss, hours, and job entry model results. First, a brief discussion of goodness of fit is presented in section 5.1. The primary results are those obtained for the full sample and NPPs only. Full details of significant estimates from these models are presented in section 5.2. Additional regressions for youth, males and females are summarised in section 5.3.

5.1 Goodness of fit

The results obtained should be interpreted in light of each model's goodness of fit, a measure of their explanatory power. For job loss and job entry, the predictive accuracy of the year-by-year models with controls is evaluated using classification tables, while for hours, the goodness of fit is assessed using the adjusted R^2 (box 5.1).[24]

The findings may be summarised as follows:

- For job loss, the spillover model has a higher rate of correct classification than the standard model for the narrow and medium groups, while the reverse is true for the wide groups (left-hand side of figure 5.1). The narrow spillover model has the highest percentage correctly classified, although this is almost matched by the wide standard group result.
- The rate of correct classification is higher in the job entry model (middle of figure 5.1). Each of the job entry spillover models has a slightly higher correct classification percentage than the corresponding standard model. There is little difference between the Dickens, Bryan and wide definitions.
- The adjusted R^2 values in the hours model are low (right-hand side of figure 5.1), which reflects weak predictive accuracy of the models. However, explaining variations in human behaviours such as hours worked is notoriously difficult, and low R^2 values are common in comparable panel studies (for example, Gong and Breunig (2014)), and in cross-sectional analyses more generally. There is not much difference between the narrow, medium and wide group definitions. The adjusted R^2 values in the spillover models are marginally higher than those in the standard models.

[24] Only models with controls are considered in terms of their goodness of fit, because they invariably have greater explanatory power.

Box 5.1 **Measures of goodness of fit**

Job loss and job entry models

A classification table is a test of goodness of fit used for logistic regressions. After estimation, a predicted probability of a positive outcome (job loss or job entry) can be calculated for each observation. The classification table tabulates the observed outcomes against the predicted outcomes.

The calculation of predicted outcomes requires some cut-off probability, above which the outcome is predicted to be positive. The choice of cut-off probability involves a tradeoff between more false positives (as the cut-off falls) and more false negatives (as the cut-off increases). In the absence of a selected cut-off, the default value of 0.5 would bias correct predictions towards the more common outcome at the expense of the less common one. To remedy this tendency, it is recommended that cut-off probabilities be chosen to maximise the sum of sensitivity — the probability of a positive prediction, given that the outcome is actually positive — and specificity — the probability of a negative outcome, given that the outcome is actually negative (Fluss, Faraggi and Reiser 2005). Accordingly, cut-off probabilities are calculated for the full-sample job loss and job entry models, and are then used to calculate the percentage correctly classified (figure 5.1).[25]

Hours model

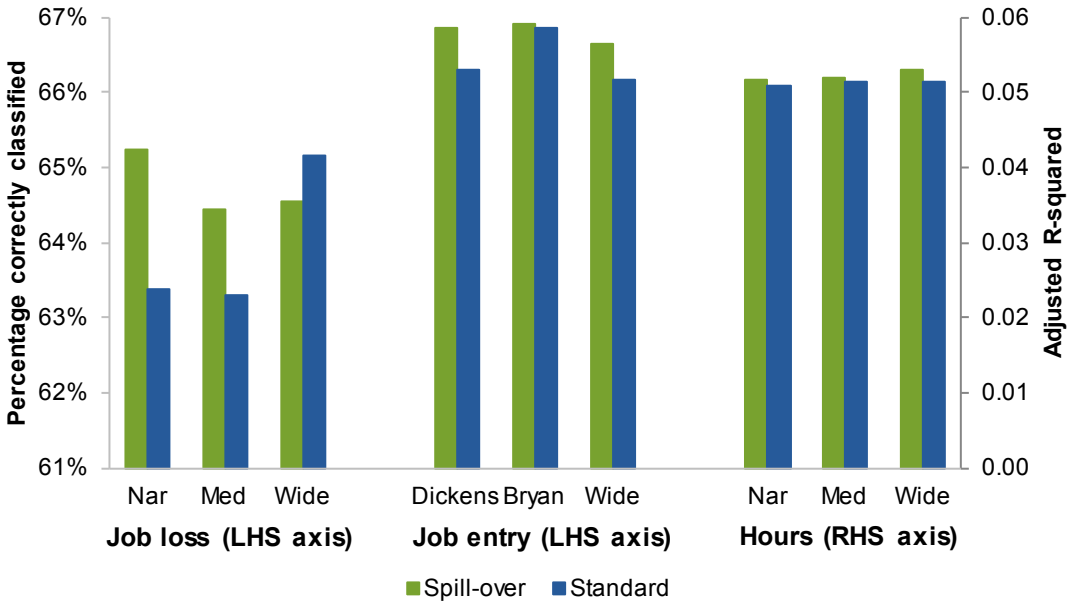
The adjusted R^2 can be used to assess the goodness of fit of a linear regression model. The standard R^2 measures the share of total variation in the dependent variable (change in hours) that is explained by the independent variables, and its value increases with every independent variable that is added to the model. On the other hand, the adjusted R^2 explicitly takes into account the number of independent variables (Woolridge 2009). A higher adjusted R^2 suggests greater predictive accuracy, but this value should not be considered in isolation. Low R^2 values do not prevent conclusions from being made about the effects of statistically significant independent variables on the outcome.

Sources: Fluss, Faraggi and Reiser (2005); Woolridge (2009).

[25] The cut-off probabilities are calculated using Stata's *cutpt* command with the option *youden*. The classification tables are then produced using *estat class*, inserting the cut-off point into the *cutoff* option.

Figure 5.1 **Goodness of fit**

Job loss, job entry and hours year-by-year models, with controls



Data source: Productivity Commission estimates based on RED.

5.2 Primary results

For each outcome — job loss, change in hours and job entry — the following model variants are presented:

- full sample[26] and non-payment partners only
- narrow, medium and wide group definitions (Dickens, Bryan and wide for job entry)
- standard and spillover models
- with and without controls
- year-by-year and pooled (there is no pooled variant for job entry).

The job loss and job entry estimates are reported as marginal effects, evaluated for each job and then averaged. They can be interpreted as the percentage point change in the average probability of, for example, job loss for a Treatment group worker, that is attributed to the minimum wage increase. In the pooled job loss models, the marginal effects of the DID coefficients are calculated at the mean value of the wage gap for the Treatment group.

[26] The full-sample results presented include NPPs, but the job loss, hours and job entry results are all qualitatively similar when NPPs are excluded, because NPPs make up a relatively small proportion of the full sample (on average, 16 per cent of the job loss sample, 18 per cent of the hours sample, and 14 per cent of the job entry sample).

Therefore, a pooled estimate is interpreted as the change in the average probability of job loss between the pre- and post-treatment at the average wage gap, less the change in probability of job loss between the pre- and post-treatment period at a wage gap of zero. Once again, the difference is attributed to the minimum wage increase.

Because the hours model is estimated using OLS, the marginal effect is given directly by the DID coefficient and is measured in hours. For reporting purposes, in the pooled wage gap variant, the DID coefficient is multiplied by the average (across years) wage gap of the Treatment group to make interpretation easier. Recall that the wage gap is related to the proportional increase required for a Treatment group individual to earn the new minimum wage. For example, a coefficient of -2.33 estimated using the medium groups pooled spillover model indicates that a wage gap of 1 is associated with a reduction of -2.33 hours in the post-treatment period, relative to someone with a wage gap of zero. However, in reality, the wage gap for the medium Treatment group in the 2008–2013 period only ranges from 0 to 0.23, with an average of 0.07. Therefore, the marginal effect is scaled by that average, and so becomes -0.16. This indicates that a person in the Treatment group with the average wage gap will experience a decrease of 0.16 hours relative to the Control_2 group.

For all year-by-year and pooled models with control variables, marginal effects are evaluated at the means of those variables.

Job loss results

Table 5.1 shows that both positive and negative marginal effects are estimated using the narrow groups. In 2010, significant positive effects (corresponding to adverse employment outcomes) of between 1 and 2 percentage points are estimated, which represent increases from a counterfactual probability of job loss of 18 per cent.[27] (The full set of job loss counterfactuals is available in table J.1 in appendix J.) Significant negative effects (favourable to employment) of between 1 and 3 percentage points are estimated in 2011 and 2013. In these years, the counterfactual job loss probabilities are about 24 per cent. The pooled spillover results are negative and significant, at approximately 1 percentage point, while the pooled standard estimate (no controls) is significant and negative, but close to zero.[28] For NPPs only, a single significant estimate is obtained, which is positive in 2012. The pooled standard model estimates are significant and positive, but close to zero.

[27] To illustrate in more detail, prior to the minimum wage increase, the Treatment group's average probability of job loss was 17 per cent. In the absence of the minimum wage increase, it would have increased to 18 per cent in the post-treatment period (the counterfactual, based on the parallel paths assumption). However, after the minimum wage uprating, it instead increased to 20 per cent, yielding a marginal effect of 2 percentage points.

[28] It should be recalled that marginal effects for the pooled model are based on the wage gap and, therefore, are not strictly comparable, in terms of their magnitude, to annual marginal effects (section 3.4).

Table 5.1 **Job loss model DID estimates**

Estimated average marginal effects of minimum wage increases on the Treatment group[a,b,c]

Sample	*Full sample*											
Groups	**Narrow**				**Medium**				**Wide**			
Spillovers	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
2008											-0.01**	-0.01**
2009					-0.01*						-0.01***	-0.01***
2010	0.02**	0.01*	0.02**		0.01**	0.01**	0.01***	0.01**				-0.01**
2011	-0.01*		-0.01*		0.01**	0.01**	0.01***	0.01**	0.01***	0.01***		
2012					0.01*	0.01*					-0.01***	-0.01***
2013	-0.03***	-0.03***	-0.03***	-0.03***							-0.01***	-0.01***
N	395 581		3 862 584		697 548		3 862 584		2 351 983		3 862 584	
Pooled	-0.001*		-0.011***	-0.009***	0.005***	0.005***	0.007***	0.006***	0.003***	0.002***	0.002***	
N	323 474		416 169		570 501		719 593		1 949 077		1 480 819	

Sample	*Non-payment partners only*[d]											
Groups	**Narrow**				**Medium**				**Wide**			
Spillovers	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
2008												
2009												
2010					0.03***	0.03***	0.02**	0.02***	0.01**	0.01**		
2011									0.01**	0.01**	0.01*	0.01*
2012		0.03*										
2013												
N	62 906		602 117		115 064		602 117		344 527		602 117	
Pooled	0.003*	0.003*			0.006***	0.006***	0.006**	0.005**	0.003***	0.003***	0.004***	0.003**
N	51 789		66 469		95 374		110 946		289 092		241 897	

[a] Marginal effects are interpreted as percentage points. For example, 0.02 is 2 percentage points. [b] The minimum wage increase is captured by the post-treatment period dummy (0/1). [c] * denotes significant at 10 per cent level; ** denotes significant at 5 per cent level; *** denotes significant at 1 per cent level. [d] Non-payment partners do not receive income support payments, but they appear in RED because their incomes affect the eligibility of their partners.

Source: Productivity Commission estimates based on RED.

Using the medium groups, almost all significant estimates are positive. One percentage point increases in the relative probability of job loss for the Treatment group are estimated in 2010 and 2011 using both the standard and spillover models, and in 2012 using the standard model. A single significant negative effect is estimated using the standard model without controls in 2009. The counterfactual probabilities of job loss are similar regardless of whether Control_1 or Control_2 is used to construct counterfactuals, suggesting that the two control groups follow similar paths. The counterfactual is approximately 21 per cent in 2009, 2010 and 2011, and 23 per cent in 2012. The pooled estimates are all positive and significant, with a magnitude of around half a percentage point. For NPPs only, significant

positive results are obtained for 2010. The year-by-year marginal effects for NPPs represent larger relative increases in the probability of job loss than for the full sample, since the counterfactual probabilities for NPPs are lower (appendix J). The pooled estimates for NPPs are also positive and significant, with a magnitude of approximately half a percentage point.

The wide groups spillover estimates are dominated by negative year-by-year results. One percentage point reductions in job loss probability are estimated using the spillover models in all years except 2011, indicating relative reductions in the probability of job loss for the Treatment group after the minimum wage increase. The counterfactuals hover between 20 and 23 per cent. Surprisingly, in contrast to the thrust of the year-by-year spillover estimates, the pooled spillover estimate in the model without controls is positive (albeit close to zero). Recall that the pooled spillover differs from the year-by-year model in two key ways. First, it is estimated using the wage gap, rather than the treatment dummy. Second, the spillover version includes only the Treatment and Control_2 groups, dropping the rest of the groups. Hence, the difference in treatment indicator and sample lies behind the sign reversal for the DID estimate. For NPPs only, positive estimates are obtained in 2011 using the spillover model, and the pooled estimates are positive.

Using the wide groups standard model, positive estimates are obtained for the full sample in 2011, and the pooled estimates are also positive and significant. For NPPs only, significant positive estimates are obtained in 2010 and 2011, and in the pooled models.

All in all, the job loss results for the full sample are somewhat mixed. There is consistent evidence across the medium and narrow group definitions that Treatment group jobs experienced a relative increase in their probability of job loss in 2010, while the medium and wide groups also record a relative increase in 2011. On the other hand, significant negative estimates, suggesting relative reductions in the probability of job loss for the Treatment group, are estimated under both the narrow and wide definitions in some uprating years. Furthermore, for the medium and wide groups, there are some significant 2009 estimates, which are not expected given there was no minimum wage increase in that year.

All of the significant estimates for NPPs only are positive, indicating a relative increase in the Treatment group's probability of job loss associated with the 2010 (medium and wide groups), 2011 (wide groups) and 2012 (narrow groups) minimum wage increases. There are no significant estimates in 2009, but there are also no effects detected in 2008 or 2013.

Some variation in results between the narrow, medium and wide groups is to be expected. As the left-hand side of figure 3.3 illustrates, the different group specifications capture different segments of the wage distribution, even before the potential for measurement error is taken into account. Appendix E also shows that the different groups record dissimilar wage rises, following the minimum wage upratings. But there may be other factors aside from wages, not controlled for in the estimation, that are contributing to the findings. The 2009 estimates provide some evidence of this, as significant effects are

observed in the absence of the minimum wage increase. These issues are discussed further in chapter 6.

Change in hours worked results

When the hours model is estimated using the narrow groups, significant negative effects (corresponding to adverse employment outcomes) are detected using the spillover model for all minimum wage upratings: 2008, 2010, 2011, 2012 and 2013 (table 5.2). In addition, significant negative estimates are obtained for the standard model in 2011. The counterfactuals constructed based on the assumption of parallel paths between Treatment and Control_2 (table J.2 in appendix J) suggest that, in the absence of the minimum wage increase, the Treatment group would have experienced a negative change in hours in all years except 2011. Therefore, the DID estimates indicate that these effects have been amplified by the minimum wage increase. The pooled estimates for both models are also negative and significant. All narrow group estimates point to additional reductions in hours worked of less than one hour being attributed to the rise in the minimum wage. For NPPs, a single negative estimated change of about one hour is obtained for 2012 using the spillover model. To put this result into context, in the absence of the minimum wage increase, it is estimated that NPPs would have decreased their hours worked by 0.88 hours per fortnight on average. However, after the uprating, the realised average reduction in hours was 1.87 (results not shown).

With the medium groups, significant negative estimates are obtained using both the standard and spillover models for 2010 and 2012. Significant negative estimates are additionally obtained using the standard model in 2009 (in the absence of the minimum wage increase), 2011 and 2013. All pooled estimates are negative and significant. All of the constructed counterfactuals (table J.2 in appendix J) indicate that negative changes in hours would have been experienced by the Treatment group, post-minimum wage increases. Thus, in some years, the uprating appears to have intensified this effect. For NPPs only, one significant negative spillover estimate is obtained, in 2010. On the other hand, positive spillover estimates are obtained in 2013, suggesting an increase in hours worked by the Treatment group, relative to the Control_2 group.

All wide groups estimates are significant and negative from 2010 to 2013 using both models, with the pooled estimates reflecting this. The estimates are more negative for the spillover model. Significant negative spillover estimates also emerge in 2009, when no increase occurred. Furthermore, significant positive estimates are obtained using the standard model in 2008. In this instance, the standard estimate conflicts with the spillover estimate. A closer look at the predicted changes in hours reveals that the Control_2 group experiences a small increase in their change in hours, while Control_1 experiences a steeper decline in their hours than the Treatment group (results not shown).

Table 5.2 **Change in hours model DID estimates**

Estimated marginal effects of minimum wage increases on the Treatment group[a,b,c]

Sample	*Full sample*											
Groups	**Narrow**				**Medium**				**Wide**			
Spillovers	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
2008				-0.54**					0.28**	0.23**	-0.26**	-0.29**
2009					-0.34**	-0.28*					-0.42***	-0.48***
2010			-0.87***	-0.70***	-0.43**	-0.42**	-0.59***	-0.58***	-0.46***	-0.48***	-0.64***	-0.69***
2011	-0.50**	-0.55**	-0.72***	-0.81***	-0.38**	-0.36*			-0.48***	-0.51***	-0.74***	-0.75***
2012			-0.67***	-0.65***	-0.42**	-0.41**	-0.45**	-0.38**	-0.40***	-0.34***	-0.71***	-0.70***
2013			-0.69***	-0.73***		-0.31*			-0.29***	-0.28***	-0.71***	-0.71***
N	244 728		2 412 574		430 942		2 412 574		1 470 734		2 412 574	
Pooled	-0.06**	-0.08***	-0.60***	-0.65***	-0.18***	-0.19***	-0.17***	-0.18***	-0.25***	-0.25***	-0.42***	-0.43***
N	199 837		264 693		351 834		445 016		1 216 423		931 956	

Sample	*Non-payment partners only*[d]											
Groups	**Narrow**				**Medium**				**Wide**			
Spillovers	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
2008												
2009												
2010								-0.73*				
2011									0.78***	0.73***	0.58**	0.55**
2012			-0.99*						0.66***	0.62***	0.76***	0.65***
2013							0.96***	0.77**	0.91***	0.87***	1.15***	1.06***
N	44 468		419 449		81 106		419 449		241 909		419 449	
Pooled									0.22***	0.19***	0.20***	0.14***
N	36 854		47 323		67 694		78 654		204 396		171 595	

[a] Marginal effects are measured in hours. [b] The minimum wage increase is captured by the post-treatment period dummy (0/1). [c] * * denotes significant at 10 per cent level; ** denotes significant at 5 per cent level; *** denotes significant at 1 per cent level. [d] Non-payment partners do not receive income support payments, but they appear in RED because their incomes affect the eligibility of their partners.

Source: Productivity Commission estimates based on RED.

For NPPs only, all significant estimates from 2011 to 2013 are positive, indicating a relative increase in hours for wide Treatment group individuals associated with those years' upratings. These year-by-year estimates contribute to positive pooled estimates. Invariably positive estimates for NPPs may be related to selection bias. If Control_1 and Control_2 members increase their hours and earnings, their partner may lose income support eligibility and exit RED. Then, the Treatment group outcomes will only be compared with those Control_1 and Control_2 members whose partner remains eligible for income support. This may contribute to the finding of an increase in hours worked by the

Treatment group, relative to the control groups. This issue is discussed in more detail in section 6.1.

Looking across all the hours results across all groups for the full sample, negative estimates largely outnumber positives, suggesting that minimum wage increases reduce the hours of minimum wage workers as they become relatively more costly for employers. All full-sample estimates are of a reasonably consistent magnitude of less than one hour. An explanation for this is that individuals in the estimation sample cannot have reduced their hours so much that they have ceased employment, but they also cannot have increased them so much that they lost income support eligibility.

In contrast to the full-sample estimates, the NPPs-only results are dominated by zero and positive estimates. This may suggest that the employment experiences of income support recipients are different from those of other workers, in which case the results obtained for those workers cannot necessarily be generalised to the broader population. Alternatively, it may reflect higher rates of attrition among wide NPP control group members.

A caveat to this interpretation of the hours results is the presence of some significant 2009 full-sample estimates, which brings into question the validity of the parallel paths assumption. As flagged earlier, possible explanations for the counterintuitive 2009 results are explored in chapter 6.

Job entry results

The job entry model results in table 5.3 show that, using the Dickens groups, all full-sample estimates are significant and negative (corresponding to adverse employment outcomes), including those for 2009 when no increase occurred. In 2008 and 2009 in particular, the standard and spillover estimates are similar, suggesting that entry into the Control_1 and Control_2 groups followed similar trajectories in those years. On the other hand, in 2010, for example, the standard model estimate is more negative than the spillover model estimate, because Control_2 experienced a greater reduction in job entry probability than Control_1 (not shown). To put the estimated marginal effects into perspective, the estimated counterfactual job entry probabilities for the Treatment group in the Dickens group definition range from 36 per cent to 43 per cent (table J.3 in appendix J). In the spillover model, an estimated marginal effect of -5 percentage points in 2013 represents a fall in job entry probability from 38 per cent to about 33 per cent. For NPPs only, significant negative estimates are obtained using the standard model in 2009 and 2013, and a negative spillover estimate is obtained in 2011.

When the job entry model is estimated using the Bryan groups, significant full-sample estimates are obtained in all years except 2010, including in the absence of a minimum wage increase in 2009. In 2008 and 2009, the estimates are of similar magnitudes between the standard and spillover models, indicating that the Control_1 and Control_2 job entry probabilities follow similar paths in those years. For NPPs only, significant negative

effects are obtained using the standard model in 2008 and 2011, while positive effects emerge from the spillover and standard models in 2010.

Table 5.3 **Job entry model DID estimates**

Estimated average marginal effects of minimum wage increases on the Treatment group[a,b,c]

Sample	*Full sample*											
Groups	**Dickens**				**Bryan**				**Wide**			
Spillovers	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
2008	-0.03***	-0.03***	-0.03***	-0.03***	-0.02***	-0.02***	-0.02***	-0.02***	-0.03***	-0.03***	-0.07***	-0.06***
2009	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.04***	-0.03***
2010	-0.04***	-0.03***	-0.02***	-0.02***					-0.04***	-0.04***	-0.05***	-0.05***
2011	-0.05***	-0.04***	-0.03***	-0.03***	-0.02***	-0.02***	-0.01*		-0.04***	-0.03***	-0.05***	-0.04***
2012	-0.06***	-0.05***	-0.04***	-0.04***	-0.02***	-0.01**	-0.01*		-0.03***	-0.02***	-0.05***	-0.04***
2013	-0.07***	-0.06***	-0.05***	-0.05***	-0.02***	-0.02***			-0.03***	-0.03***	-0.04***	-0.03***
N	624 468		4 159 153		634 594		4 159 153		2 470 087		4 159 153	

Sample	*Non-payment partners only*[d]											
Groups	**Dickens**				**Bryan**				**Wide**			
Spillovers	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
2008					-0.02*		-0.02*				-0.02***	-0.02***
2009		-0.02*									-0.01*	-0.01*
2010					0.02*		0.03**	0.03**				
2011			-0.02*		-0.03**	-0.02**			-0.01*	-0.01*	-0.02***	-0.02***
2012									-0.02***	-0.02***	-0.03***	-0.03***
2013	-0.02**	-0.02**									-0.01*	
N	88 599		566 341		89 109		566 341		313 553		566 341	

[a] Marginal effects are interpreted as percentage points. For example, 0.02 is 2 percentage points. [b] The minimum wage increase is captured by the post-treatment period dummy (0/1). [c] * denotes significant at 10 per cent level; ** denotes significant at 5 per cent level; *** denotes significant at 1 per cent level. [d] Non-payment partners do not receive income support payments, but they appear in RED because their incomes affect the eligibility of their partners.

Source: Productivity Commission estimates based on RED.

Estimating the wide groups models over the full sample produces estimates that are all negative and highly significant, including in 2009. The marginal effects estimated using the spillover model are often more negative than for the standard model, indicating some divergence in entry probability between the Control_1 and Control_2 groups. The marginal effects range from reductions of 2 to 7 percentage points, which can be considered relative to predicted job entry probabilities in the absence of treatment of around 40 per cent on average (table J.3). For NPPs only, significant negative effects are estimated using the wide groups in all years except 2010.

Across all specifications, the full-sample job entry results are dominated by negative estimates, which suggests that the probability of being a new hire into the Treatment group decreases, relative to the probability of being a new hire into Control_1 or Control_2 in the post-treatment period. Using the Bryan groups, the year 2010 stands out as somewhat different from the other years: no significant effects are estimated for the full sample in 2010 and, for NPPs only, significant *positive* effects are estimated for 2010.

Negative effects in 2009 are estimated across all groups, and are often of a similar magnitude to those observed in years of minimum wage increases. Once again, this is an issue for further investigation in chapter 6.

5.3 Results for specific groups

To identify the effects of minimum wage increases on specific groups, the spillover models are also estimated separately for three different groups of income support recipients and non-payment partners: younger workers (those aged between 21 and 25 inclusive), females (aged 21 to 64) and males (aged 21 to 64). Although detailed results of these regressions are not presented in this technical supplement, table 5.4 provides a brief summary, showing that:

- the estimated marginal effects for youths are not always of the same sign as those for the full sample. This is the case for the job loss model using the narrow groups in 2011 and the wide groups in 2008. Furthermore, in the wide groups job entry model, all full-sample estimates are negative, but the estimates for youths are positive in 2010, 2011, 2012 and 2013
- the estimates for females align with all significant full-sample estimates
- the estimates for males are mostly consistent with those for the full sample, apart from for the job entry model under the wide groups in 2009 and 2010.

This analysis demonstrates that a range of effects may be identified by focusing on different subgroups. Heterogeneity in sensitivity of employment to minimum wage increases is to be expected, given differences in productivity by age and gender. However, the objective of this technical supplement is to obtain the most generally applicable results. This was the motivation behind isolating NPPs. In any case, the effects on younger workers, males and females are, by and large, consistent with those for the broader population of all income support recipients and their partners.

Table 5.4 **Direction of estimated marginal effects[a]**

Job loss, change in hours and job entry spillover models

	Narrow/Dickens[b]				*Medium/Bryan*[c]				*Wide*			
	Youth	**Fem.**	**Male**	**All**	**Youth**	**Fem.**	**Male**	**All**	**Youth**	**Fem.**	**Male**	**All**
Loss												
2008	+		–		+		–		+		–	–
2009	–		–		–		–		–		–	–
2010	+		+	+	+	+	+	+			–	–
2011	+	–		–	+	+	+	+			+	
2012	–	–	–		+		+		–	–	–	–
2013	–	–	–	–	+				–	–		–
Hours												
2008				–								–
2009							–		–	–	–	–
2010	–	–	–	–	–	–	–	–	–	–	–	–
2011	–	–	–	–	–		–		–	–	–	–
2012		–		–	–		–	–	–	–	–	–
2013	–		–	–	–		–		–	–	–	–
Entry												
2008	–	–	–	–	–	–	–	–	–	–	–	–
2009	–	–	–	–	–	–	–	–		–	+	–
2010	–	–	–	–		–	–		+		+	–
2011	–	–	–	–		–	–	–	+	–	–	–
2012	–	–	–	–	–	–	–	–	+	–	–	–
2013	–	–	–	–	–	–	–		+	–	–	–

a Positive marginal effects are indicated by '+'; negative marginal effects are indicated by '–'; blank cells indicate that the marginal effect is approximately equal to zero. **b** Narrow group definitions for job loss and hours; Dickens group definitions for job entry. **c** Medium group definitions for job loss and hours; Bryan group definitions for job entry.

Source: Productivity Commission estimates based on RED.

6 Discussion

This chapter aims to provide context for the results. It investigates whether any biases have impaired the methodological approach, and considers institutional factors that may have interacted with minimum wage changes or influenced the estimated effects. It also explores complexities in employer responses that can alter the magnitude of minimum wage effects on employment. Finally, the chapter examines the possibility that differences exist between short-run and long-run employer responses. Some of the issues covered may be relevant in all years, while others arise from the economic climate and set of policies in place at a point in time.

6.1 Methodological factors

This section considers the validity of the DID approach, as well as ways in which data issues could potentially bias the results presented in chapter 5. Non-minimum wage changes to the labour market that affect the Treatment and control groups differently over a transition will entail violations of the parallel paths assumption. A specific example is provided by the Global Financial Crisis, which may have led to a divergence in outcomes between the groups in some years. This section also discusses how the nature of the RED dataset may create the potential for attrition bias, with implications for the analysis. Finally, errors in the measurement of hourly wages may introduce contamination between the Treatment and control groups, causing underestimation of the true minimum wage effect.

The Global Financial Crisis

The Australian Fair Pay Commission made no change to minimum wages in 2009, making that year a potential benchmark for evaluating parallel paths. If parallel paths were holding at the time, no significant DID effect should be observed for 2009. Yet, significant estimates are identified in some of the job loss, hours and job entry models. However, significant results obtained for one year do not necessarily imply that the assumption is violated in all years. It must first be established whether 2009 was a typical year in terms of non-parallel paths. A working hypothesis is that it was not, given that the onset of the Global Financial Crisis at the end of 2008 led to a set of unique economic circumstances that may have interfered with the DID estimates in 2009.

Effects on low-paid workers

Hiring and dismissal costs are likely to be lower for low-skilled than high-skilled employees, given that redundancy payouts reflect tenure and average wages, firm-specific human capital is low (and associated with this, so too training costs), and selection processes simpler. Low-skilled employees may also be less versatile, with fewer prospects for temporary re-deployment.

For these and other reasons, it might be expected that low-paid employees would be more severely affected by economic downturns than the higher-paid. There is some indirect evidence in support of this hypothesis. For example, Kuehnle and Scutella (2011) find that, during the economic downturn, low-paid workers experienced a higher rate of returning to income support than people earning higher wages, an effect that was not observed in earlier years. If a related trend were present within this supplement's RED sample — so that Treatment group workers became relatively less likely to work and more likely to rely on income support — it would lead to a divergence between the paths of the Treatment and control groups. This could explain the significant adverse employment effects on the Treatment group estimated for 2009, including relative decreases in hours worked (table 5.2), and decreases in the probability of job entry (table 5.3). However, these posited effects are inconsistent with the reduced probability of job loss estimated for the wide spillover groups (table 5.1).

Other circumstantial evidence, from Wilkins and Warren (2012), supports the contention that Treatment and control group outcomes may have diverged in 2009. Using the HILDA survey, they find that, between 2008 and 2009, there was a change in the composition of dismissed individuals that seems to have been driven by the increase in overall unemployment. Their findings suggest that lower-educated and casual workers became relatively *less* likely to be dismissed during the downturn. To the extent that Treatment group members are more likely to possess these characteristics, they may have seen their probability of job loss *fall* relative to the control groups. This is consistent with the medium and wide job loss results for 2009 (table 5.1). On the job entry front, Wilkins and Warren find that those who were successfully re-employed over the same period had higher wages in the job held prior to dismissal, and were more likely to have been employed full-time. Insofar as individuals enter new jobs that have similar characteristics to the jobs they held previously, Wilkins and Warren's findings may suggest a relative *rise* in the probability of being a new entrant into the control groups, and a relative *fall* in entry into the Treatment group, which is consistent with the 2009 job entry results (table 5.3).

Labour hoarding

In Australia and some other countries, the rise in unemployment during the Global Financial Crisis was less severe than it otherwise might have been as reductions in working hours prevented greater job losses. This phenomenon has been described as labour hoarding. Borland (2012) notes that between the third quarter of 2008 and the third quarter of 2009, employment remained stable while aggregate hours worked decreased by more

than 2 per cent. The OECD (2010) suggests that firms reduced hours rather than employment to prevent the re-emergence of the skills shortages that had occurred previously. Such an employer response may have been reinforced by the government stimulus package introduced at the time (box 6.1). This package may have given Australian employers confidence that business would improve, reducing pressures to make permanent changes to staffing levels. On the supply side, employees may have been more reluctant to leave their jobs amid the downturn due to concerns about being able to find new ones.

Box 6.1 The Australian fiscal stimulus package

Between December 2008 and April 2009, in response to the onset of the Global Financial Crisis, the Australian government rolled out a series of initiatives collectively referred to as the stimulus package. The $42 billion package adopted a dual-pronged approach to supporting economic activity, providing short-term transfers and tax cuts to households while also attempting to encourage longer-term development through investment in infrastructure. In addition, employment and training initiatives were subsidised, additional funding was allocated to case management for job seekers, and income tests for some benefits were relaxed (Starke 2013).

While contested by some, most analysts have concluded that the stimulus package had significant impacts on employment. The OECD (2009) estimated that, in Australia, the decline in employment was between 1.4 and 1.9 per cent less than it would have been in the absence of the stimulus package, which corresponds to the loss of 150 000 to 200 000 fewer jobs. Although the average annual unemployment rate did increase from 4.3 per cent in 2008 to 5.7 per cent in 2009, Australia avoided the worst of the downturn, experiencing only one quarter of negative growth at the end of 2008 and outperforming the rest of the OECD in terms of economic growth and unemployment (Starke 2013). Overall, Redmond, Patulny and Whiteford (2013) conclude that the major effect of the stimulus effect was to maintain the long-term labour market dynamic, rather than boost incomes.

Sources: Li and Spencer (2014); OECD (2009); Redmond, Patulny and Whiteford (2013); Starke (2013).

Labour hoarding occurring in 2009 would have implications for the DID analysis if it affected the Treatment and control groups differently, thereby leading to a divergence in paths between the groups for that year. For example, suppose that, based on the wage profiles of the groups, there are more casual workers in the Treatment groups than in the control groups (keeping in mind, however, that casuals cannot be identified in RED and, therefore, their wages cannot be adjusted for loadings). The hours of casual workers are easier to adjust than the hours of permanent workers and, therefore, Treatment group workers may have been more likely to experience reductions in hours associated with the economic downturn. Also, the greater flexibility of casual workers may make them relatively less susceptible to being laid-off. This scenario is consistent with the 2009 estimation results, where statistically significant reductions in changes in hours and statistically significant reductions in the relative probability of job loss are observed for the medium and wide Treatment groups.

The foregoing discussion illustrates how prevailing economic circumstances can interfere with the DID approach. DID is identified by assuming that the only thing that changes between the pre- and post-treatment periods is the minimum wage. In reality, this cannot always be the case, and much effort has been devoted, in the minimum wage DID literature, to controlling for macroeconomic trends and business cycles (Dube, Lester and Reich 2010; Sabia 2014)). Unfortunately, in Australia, the absence of geographic diversity in the application of the minimum wage rules out many of the techniques used overseas.

Nonetheless, the absence of a minimum wage increase in 2009 presents an opportunity to benchmark the ability of the DID approach to abstract from confounding factors. Significant DID estimates for 2009 expose the presence of forces other than the minimum wage that induced divergences in employment outcomes between the Treatment and control groups and, therefore, bring into question the validity of the parallel paths assumption in that and other years. However, the circumstances leading to the minimum wage freeze in that year (specifically, the consequences of the Global Financial Crisis) are also relevant to the interpretation of the estimates. Although there are many possible explanations for the observed 2009 results, the possibility that they reflect the exceptional features of that year cannot be discounted.

Attrition bias

The nature of RED creates the potential for attrition bias, which is a form of selection bias resulting from participants leaving the sample. In RED, all observations are conditional on the individual or their partner remaining on income support at the end of the transition. Since those who begin to earn more are more likely to lose their income support eligibility and leave RED, attrition is non-random.

Attrition bias can create at least two issues. First, it is a problem for DID estimation if the characteristics of individuals who leave the sample differ between the treatment and control groups, and if one or more characteristic is correlated with the outcome measure (in this case, job loss, change in hours or job entry) (Heckman, LaLonde and Smith 1999). Second, attrition bias may also mean that observed employment transitions will not be reflective of the broader population of income support recipients that existed before attrition. Heckman et al. (1999, p. 1913) explain that 'even if attrition affects both experimental and control groups in the same way, the experiment estimates the mean impact of the program only for those who remain in the sample.'

A comparison of exit rates from RED of the Treatment, Control_1 and Control_2 groups reveals evidence of non-random attrition. As expected, the probability of exiting RED increases as the individual begins to earn more: table K.1 in appendix K shows that Control_1 and Control_2 members always have higher probabilities of leaving RED by the end of the transition than Treatment group members. Although the differences between groups do not appear large in absolute terms, they are relatively large compared with the

estimated minimum wage effects, and are statistically significant. These differences indicate the potential for selection bias in the results.

Despite evidence of its existence in the RED sample, attrition bias has not been adjusted for here. The possibility of implementing the Heckman correction was explored, but the requirements and assumptions of the approach could not be satisfied in this instance.[29] In the absence of any correction, attrition bias may affect the results. The direction of such effects remains speculative, however. This is so even under the assumption that the majority of individuals who leave the sample do so because of improvements in earnings. In that event, the remaining individuals are relatively more likely to experience adverse employment outcomes, but the key issue is which of the groups is more affected. If the control group were to suffer from this depletion effect more, then employment outcomes may appear relatively more favourable to the Treatment group.

Allocation to the Treatment and control groups

Misallocation between groups occurs when some control group members are affected by the minimum wage and/or some Treatment group members are unaffected. Misallocation may occur because of incorrect calculation of hourly wages, possibly resulting from failure to adjust for penalty rates or casual loading (since this information is not available in RED), or from other sources of error. Regardless of its direction, this error will result in underestimation of the true minimum wage effect (Chuang, Hripscsak and Heitjan 2002). Intuitively, underestimation is caused by the blurring of employment outcome differences between the two groups. More specifically, the treatment effect is diluted by the presence of unaffected workers, and the counterfactual is inflated by the presence of directly-affected workers.

The presence of sub-minimum wage earners in the dataset could also have positive, negative or negligible impacts on the estimates. As discussed in section 2.4, sub-minimum wages may be recorded due to measurement error and data quality issues, in which case they could interfere with the estimates by inducing misallocation to groups. However, there are also some individuals who genuinely earn a sub-minimum wage, in which case the impact of their inclusion will depend on the explanation for that wage. Recall that some individuals in RED may earn sub-minimum wages due to non-compliance. An employer who is non-compliant prior to the increase is unlikely to become so after the increase. As such, employees who are unlawfully being paid below the minimum wage may not be subject to the same changes in job loss probability as other Treatment group members, which would blunt the overall minimum wage effect. Ultimately, since it is not possible to determine the reasons for sub-minimum wages in the estimation sample, it would be difficult to quantify their effects on the estimates.

[29] For example, the selection model must include at least one additional variable that does not appear in the regression equation. Such variables — exclusion restrictions — must determine sample selection, but not the outcome (Bushway, Johnson and Slocum 2007). Within the dataset, it was not possible to find suitable exclusion restrictions.

6.2 Institutional factors

Selected changes occurring within the regulatory and policy environment may have had a confounding or distorting influence on minimum wage effects. While the net effect of the factors canvassed below is not able to be calculated, this discussion highlights that minimum wage increases do not occur in a vacuum. For example, annual employment effects may be affected by changes to the industrial relation settings, and minimum wage increases granted by the FWC and its predecessors are themselves informed by the state of the economy and labour market. Also, since this technical supplement uses administrative RED data, changes to income support and related payments may also influence observed employment responses. Nevertheless, these contextual factors should not bias the results of the DID analyses as long as they: (i) affect the Treatment, Control_1 and Control_2 groups in similar fashion, resulting in parallel paths; and (ii) they allow the identification of a suitable control group.

The industrial relations environment

The response of employers to changes in the minimum wage is partly determined by the underlying institutions that affect their workforce decisions. The question is how much?

There is some evidence that employment protection measures can offset the negative employment effects of the minimum wage. This can happen because unfair dismissal laws reduce the average probability of job loss for all workers (Neumark and Wascher 2004). Boockman (2010) indicates this is likely a short-run effect. On the other hand, such laws could also reduce employers' propensity to hire, due to concerns about their ability to adjust their workforce in future. In a general sense, employment protection has the potential to shape the response of employers to changes in economic settings such as the minimum wage. Over the period studied, a major change to unfair dismissal laws was introduced by the *Fair Work Act 2009* (Cth), from 1 July 2009. Under that act, the coverage of unfair dismissal laws was extended through the removal of the previous exemption for businesses with fewer than 100 employees.

However, it is improbable that this one change in Australia's employment protection legislation provides a substantive explanation for the results obtained. First, there is a multitude of voluntary and involuntary exits from employment each year, but only a small number of unfair dismissal claims. Second, unfair dismissal laws do not protect employees from cases of genuine redundancy. This means that a situation where a minimum wage uprating results in labour cost increases that force an employer to lay off staff is unlikely to be upheld as unfair dismissal by the FWC. Further, from a technical standpoint, as long as the Treatment and control groups are similarly affected, changes to employment protection (and other) legislation should not bias the DID estimates.

The effects of the introduction of modern awards and of the FWC's pay equity decisions have also been considered, principally because they involved the need, in some industries,

for ‘catch up’ pay increases. It does not appear likely, however, that these changes interacted with this technical supplement’s estimates in a material way. Transitional arrangements under award modernisation were introduced to smooth the process of adjustment for employers, thereby moderating the impacts when wage increases were required.[30] In addition, although they have the potential to cause additional job losses, pay equity decisions that mandated wage increases have not yet been widespread enough to have a significant effect. (One example was a case relating to the Social and Community Services sector, which resulted in significant mandated wage increases for most employees covered by the Social, Community, Home Care and Disability Services Industry Award 2010, to be phased in over eight years (Layton, Smith and Stewart 2013).)

More broadly, to the extent that the FWC’s minimum wage decision rests on all available relevant information, any adverse effects of minimum wage increases should be mitigated. As illustrated by the minimum wage freeze of 2009, the minimum wage-setting body has to be mindful of the broader economic and social context when granting annual wage increases. Thus, it might usually set down a wage increase that most employers are able to absorb without making adjustments to their labour force. An exception to this cautious approach may arise when the FWC grants ‘catch up’ increases, as was the case in 2010. Following the wage freeze, an increase of 4.8 per cent was granted in that year — the largest across the 2007–2015 period (table 3.1). This unusually large uprating may partly explain the mainly adverse effects estimated by the models for that year.

Other institutional factors that can be loosely grouped with ‘industrial relations’ pertain to labour ‘on-costs’. Employers might respond to increases in, say, workers’ compensation premiums or payroll tax by laying-off workers, even if minimum wages did not change. The possible role of such confounding factors is not explored here.

Changes to income support and related payments

Given that the dataset captures income support recipients and their partners, changes to the welfare safety net system over time may also have influenced the results. In particular, it is likely that changes in eligibility settings for payments and entitlements have at times reshaped the work incentives of income support recipients. If a sufficiently strong labour supply response were to result from the new incentives, this could partly ‘muddy the waters’ in terms of measuring employment responses to a rising minimum wage.

Examples of relevant safety net settings changes include:

[30] If an employer was paying someone a wage below the agreed minimum pay point set out in the modern award, the required wage increase was to be phased in over a four-year period. For example, in 2010, the employer would pay the modern award wage less 80 per cent of the difference between the two pay points. In 2011, the percentage would be 60 per cent, and so on, until July 2014 when the employer was required to pay the full modern award rate.

- student start-up scholarships paid to Youth Allowance, Austudy and ABSTUDY recipients were introduced in 2010 that may have encouraged people to take up study, triggering a reduction in hours worked or an exit from employment
- the tightening of eligibility for parenting payments in January 2013 (which moved single parents receiving grandfathered Parenting Payments onto the lower Newstart Allowance payment when their youngest child turned eight) may have provided greater employment incentives, leading to a higher rate of job entry at low wage levels
- increases in the Child Care Tax Rebate in 2008-09 could also have resulted in a positive labour supply response. Gong and Breunig (2014) find that introduction of the Rebate in 2006-07, which improved work incentives and lowered the cost of working, increased workforce participation (employment and hours) of mothers. Connolly and Trott (2014) also estimate that decreases in child care fees are associated with increases in the female labour force participation rate. Extrapolating from these findings, it may be expected that the increase in the Rebate from 30 per cent to 50 per cent of a family's out-of-pocket childcare costs in 2008-09 could have induced a positive labour supply response.

The existence, let alone impact, of the labour supply responses hypothesised above on measured minimum wage employment effects is not able to be ascertained within the current modelling framework. Ultimately, the size and direction of these responses will be determined by the changes induced in people's 'reservation wage' — the minimum wage that a person will accept to do a particular job. This is not easily predicted, as two individuals may react quite differently to the same change in an income support payment.

6.3 Channels of adjustment

The foregoing analysis presupposes that an employer's response to an increase in the minimum wage is limited to adjusting the quantity of minimum wage labour used or not. This may be strictly true were non-wage determinants of labour costs constant. But, in reality, a range of possible channels of adjustment are available to employers to contain those costs. They include:

- reductions in non-wage benefits (for example, fewer employee discounts)
- reductions in on-the-job training
- adjustments to the proportion of full-time versus part-time workers
- efficiency improvements — minimum wage increases may provide incentives for employers to improve productivity, for example through higher performance standards or restructuring, and to consider potential cost savings
- wage compression, whereby employers delay raises to higher-paid employees in response to the minimum wage increase

- higher prices for goods or services — the extent to which this will reduce sales and therefore employment depends upon the elasticity of demand for the employer's goods or services (Card and Krueger (1994); Congressional Budget Office (US) (2014); Neumark and Wascher (2010); Schmitt (2013)).

The employment effects of a higher minimum wage could be mitigated by adjustments across these other channels. This heterogeneity in responses — itself a function of the institutional and regulatory environment — may help explain the historical difficulty in identifying consistent employment effects associated with minimum wage increases.

Research undertaken on behalf of the FWC into some employers' responses to the 2010 minimum wage increase found that 'the relative influence of the minimum wages increase in driving adjustments was minor when compared with other factors' (Evesson et al. 2011, p. 191). The authors found that employers operating in profitable environments were able to absorb the increase in wage costs. Many of the employers who made minor adjustments following the minimum wage increase indicated that these adjustments were the same as those ordinarily implemented in the ongoing management of their businesses, aimed at minimising costs and improving efficiency. Examples included workload intensification, increased prices, careful rostering, and reviewing relationships with suppliers. Employers who did respond directly to the minimum wage increase primarily did so through increased prices and reduced non-labour costs. Only one employer (out of the 18 in the study who complied with the minimum wage increase) implemented labour cost adjustment strategies, including thinking 'more closely' about hiring and reducing hours of work (Evesson et al. 2011, p. 190). Although based on a small-scale survey and largely anecdotal, such observations underscore the difficulty in identifying job loss effects attributable to specific minimum wage adjustments amongst the range of employer responses.

In addition to active responses from employers, there may be other offsetting forces that diminish the burden of a higher minimum wage on business costs and/or employment. For instance, in theory minimum wage increases might induce an efficiency wage response, whereby a higher wage motivates employees to exert more effort. Higher wages also make it more costly for individuals to leave their jobs, and, therefore, they may work harder or be less inclined to quit. Brochu and Green (2013) obtain some evidence that a higher minimum wage has a negative effect on quits in Canada. Additionally, minimum wage increases enhance the incomes of low-paid workers who maintain the same level of employment, and could, therefore, add to consumer spending. This increased demand may offset help wage cost increases for employers (Schmitt 2013), although the overall effects on the aggregate level of demand in an economy from marginal changes in the minimum wage are likely to be relatively limited.

6.4 Short- and long-run effects of minimum wage increases

Key requirements for a minimum wage increase to cause a change in minimum wage employment are that:

- the relativities between the price of minimum wage labour and that of other inputs into the production process must change, while their productivity ratios remain constant
- minimum wage labour and other labour and non-labour inputs must be substitutable, to a greater or lesser extent
- employers must be able to respond to changing relativities by adjusting their input mix. In a short-run timeframe, firms can only respond by varying their labour usage, in terms of quantity and/or quality. In the long run, capital stock adjustments offer another adjustment channel.

The first condition may not always be met within the context of Australia's annual minimum wage increases. Award wages paying above the minimum wage increase simultaneously with it, and usually to the same extent. Furthermore, other nominal labour earnings tend to increase over time throughout the wage distribution, with many pay rises effected yearly at the same time as the minimum wage uprating (in July). These factors may lead to the relativity between the minimum wage and other wages remaining constant (in the case of award wages) or increasing and then declining (in the case of steady economywide wage increases). The latter results in what Sorkin (2014) describes as a 'sawtooth' pattern, where, in the aftermath of a minimum wage increase, the relativity jumps up, only for the initial ratio to be restored after twelve months (figure 6.1). Under these circumstances, disturbances to minimum wage relativities with other wages will be either temporary or nonexistent.

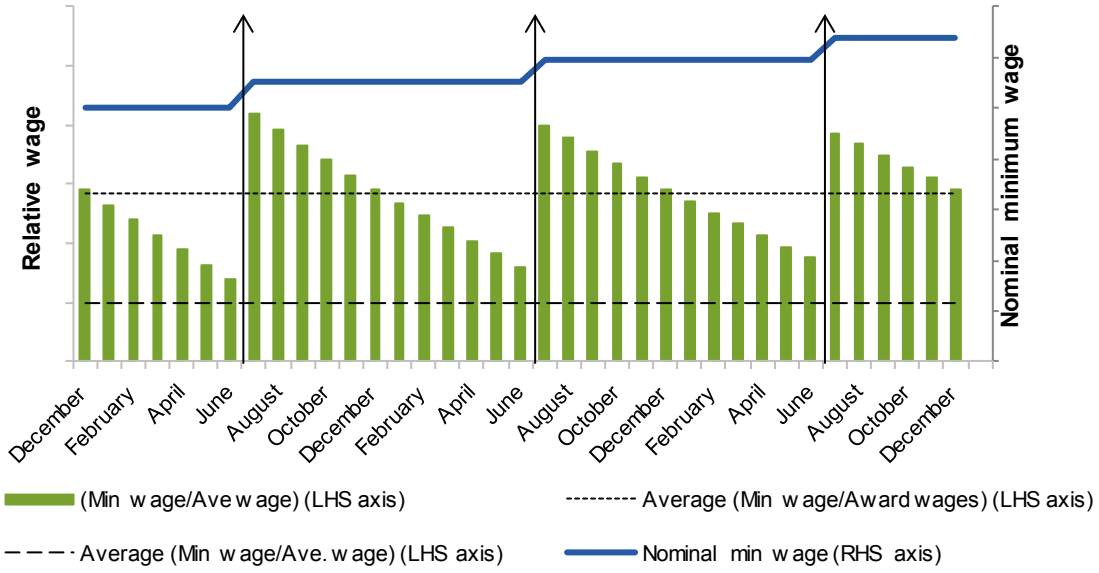
Unchanging wage relativities between minimum wage earners and substitute workers would not normally give rise to differential employment effects. Even temporary changes, if predicted, would not be expected to trigger substantial short-run adjustments to the minimum wage employee headcount. However, other transitory responses may be possible and desirable. For example, as investigated in this technical supplement, employers may make short-term adjustments to hours worked or their hiring policies.[31]

Even if short-run employment effects are small, the long-run employment effects of minimum wage increases may still be substantial. There are two potential explanations for this. First, it is only in the long run that firms are able to adjust their use of capital goods. Sorkin (2014) suggests that, in response to permanent changes in the real value of the minimum wage, firms adjust to the long-run average level of the minimum wage when choosing their capital intensity. Second, in an Australian context, longer-term employer responses may factor in repeated, annual increases in the minimum wage. While each

[31] This possibility may also explain the labour hoarding phenomenon. Since employers perceived the downturn to be temporary, they maintained employee headcount but reduced hours worked.

increase may be small, triggering only small short-run effects, the effects of multiple increases may be cumulative and/or lumpy. Neumark and Wascher (2006, p. 20) cite work by Baker et al (1999) indicating that the 'effects of minimum wages show up as longer-run responses to more evolutionary changes in the level of the minimum wage, rather than as a short-term response to a particular change in the minimum wage'.

Figure 6.1 **Sawtooth pattern of wage relativities**[a]



[a] Increases in the minimum wage are indicated by the black vertical arrows.

Thus, the full disemployment effect of minimum wage increases may only become apparent in the long run, after employers have experienced successive increases and had the opportunity to invest in labour-saving technology (Sorkin 2014). Investigating the potential for both short- and long-run impacts, Lee and Suardi (2008, p. 29) find no evidence that the seven minimum wage increases in Australia between 1997 and 2003 had any impact upon the employment dynamics of teenagers, and suggest that this may be because the increases were 'moderate and predictable'. This, of course, does not indicate what would have happened to employment had those wage increases not been made.

Moreover, it is an open question whether the more recent period under consideration here resembles that which Lee and Suardi examined. On the one hand, the ratio of the Australian minimum wage to the median wage has declined significantly over time, falling from 60 per cent to less than 55 per cent between 2004 and 2013 (PC 2015). Other things equal, this long–term trend would have reduced unit labour costs to employers, so that small annual minimum wage increases may not have mattered much — as Lee and Suardi concluded. On the other hand, the cost of many capital goods also fell significantly during the same period. For example, in the Retail and Accommodation and Food Services industries, the imputed capital price of Electrical and Electronic Equipment fell by between

one half and two thirds between 2004 and 2013 (Productivity Commission estimates based on ABS cat. no. 5260.0.55.0002 (2014)). This trend may have led to the automation of some tasks previously performed by minimum wage workers, in supermarkets for example. If so, a long-term impact may be expected from the substitution of capital for low-skilled labour.

Nonetheless, it is not possible to be definite regarding the net effect of the various trends described above on employment of minimum wage workers. This is true, not least because relative productivity levels and trends, not just costs, enter into the employer's input mix decision.

Ultimately, the DID methodology employed in this technical supplement can only capture the short-run effects of minimum wage increases. This characteristic may explain the range of the employment effects identified. Over a six-month period, when faced with small, periodic adjustments in wage relativities, it is plausible that employers tend to resort less to layoffs and more to hours and hiring adjustments, since initiating workforce reductions is a relatively costly and inflexible response. Longer-term, a more sizable response based on capital substitution cannot be ruled out.

7 Conclusion

This technical supplement has examined the effects of five annual minimum wage increases between 2008 and 2013 on job loss, hours worked, and job entry of minimum wage workers. Prior to attempting to summarise the results from this analysis, five key caveats are in order.

First, Australia's unique wage system means that minimum and award wage rates tend to move in unison, making the analyst's task difficult. The essence of DID — the identification of a 'pure' comparator with which to benchmark the effects of the minimum wage — is inevitably weakened in this complex environment. Some of the descriptive group statistics presented confirm that some higher-paid workers in the control groups also receive wage increases that coincide with minimum wage upratings. This means that spillovers are likely to affect the counterfactual. Nonetheless, the upshot of the group statistics is that not all model specifications are equally impaired by spillovers.

Second, quantitative estimates of minimum wage effects may be affected by institutional and economic confounding factors. These factors do not necessarily negate the possibility of minimum wage effects, but they can make their detection more difficult, particularly if they create non-parallel paths in the employment outcomes of the two DID groups. In the period examined by this technical supplement, events such as the Global Financial Crisis and the reform of the industrial relations system may have served to increase the 'background noise'.

Third, a salient example of the non-parallel paths risk is provided by the identification of significant effects in 2009 in all models. The detection of these effects when there should be none (due to a minimum wage freeze) is of concern. However, a partial response to this concern is the cumulative evidence that the Global Financial Crisis led to a one-off divergence in outcomes between minimum wage workers and others. This would imply that significant results obtained for 2009 do not reflect a violation of the parallel paths assumption in all years. Nonetheless, significant uncertainty remains in relation to this issue.

Fourth, the dataset has proven to have some important limitations for this exercise. For example, differential rates of attrition from RED for the Treatment, Control_1 and Control_2 groups may bias the estimates, but a solution to this issue is not forthcoming. Further, group construction is hindered by inaccuracies in the calculation of hourly wages, which may occur because penalty rates and casual loading cannot be adjusted for, or perhaps because of more fundamental data quality issues such as reporting errors. Also, the absence of information on junior rates means that all individuals aged below 21 have been omitted from the sample. The analysis would be improved by more precise wage data,

which are not available at this time. Owing to these issues, all estimates in this technical supplement should be considered as at best indicative, rather than precise, measures of the association between minimum wage increases and employment outcomes.

Fifth, even setting aside the caveats above, the analysis does not lend itself easily to generalisations. Crucially, most low-paid workers not receiving income support payments are excluded from the analysis. Extrapolating the results from the present, partial exercise to the entire population of minimum wage workers may not be valid, since those living in households that do not receive income support may exhibit different work preferences and incentives. Nevertheless, the analysis does capture a very large number of low-paid workers — the number of Treatment and Control_1 group workers reaches in the hundreds of thousands annually in some models. Moreover, a subset of this group is made up of non-payment partners. Because these individuals are not income support recipients, the specific analytical results obtained for this group may be regarded as more representative of the wider population of minimum wage workers.

Keeping these important caveats in mind, what is the thrust of the analytical results?

- In the job loss models, both adverse and favourable year-by-year effects on the probability of exiting employment are detected for the full sample, depending on the model variant and the year. Significant estimates are detected in all years, including 2009. The size of significant adverse effects ranges from 1 to 2 percentage points, while the size of favourable effects ranges from 1 to 3 percentage points. Reflecting the variability of the year-by-year results, the magnitudes of the pooled estimates are somewhat small, but suggest an adverse employment effect under the medium and wide group definitions, and a favourable effect under the narrow definition.
- The hours results for the full sample are almost always negative across all groups. This suggests that increases in the minimum wage are usually associated with reductions in hours worked for minimum wage workers relative to higher paid workers. Counterintuitively, some significant reductions are also estimated in 2009. The pooled models, which do not include 2009, are also all negative.
- The job entry results for the full sample are invariably negative, and thus the average estimated effect of the minimum wage across the period is a reduction in the prevalence of minimum wage earners amongst new hires. Effects in the same direction are observed, however, during the minimum wage freeze in 2009.
- For NPPs only, the job loss estimates point to adverse effects of minimum wage upratings in 2010, 2011 and 2012, and the pooled estimates also suggest adverse effects on average over all five upratings. There is evidence of adverse changes in hours for Treatment group NPPs in 2010 and 2012 obtained using the narrow and medium groups, but the estimates for 2011, 2012 and 2013 indicate favourable changes in hours worked for minimum wage workers after those upratings. Finally, the job entry results for NPPs only, as for the full sample, mostly point to adverse effects (including in 2009). However, some evidence of favourable effects is found using the Bryan definitions.

- The models are also estimated for youths, males and females separately. By and large, the estimates for these subgroups are of the same direction as those obtained for the full sample. However, in some of the job loss and job entry models, the direction of the effect on youths, and in some cases, males, runs counter to the effect for the full sample. This suggests some heterogeneity in the sensitivity of minimum wage workers to upratings.

Focusing on the summaries of the full-sample results, it appears that, in general, adverse employment effects from minimum wage increases were felt more by 'would-be employees' (that is, the unemployed and those outside the labour force). For those already in jobs, the main consequence appears to have been a reduction in hours worked rather than job loss.

There are a number of possible interpretations for these substantive findings.

First, the changing regulatory and economic environment inevitably shapes the nature and size of employers' responses to minimum wage increases over time. Employers operate within the confines of the workplace relations framework, which regulates their responses to changes in economic settings, and may prevent large employment effects. More broadly, as part of the workplace relations system, the level of the minimum wage is determined by the regulator in light of the prevailing economic and social conditions. To the extent that this decision considers all relevant factors, the impact of an uprating on the level of employment should be relatively muted.

Second, the relative attractiveness of alternative channels of adjustment to minimum wage increases will vary, based on labour market and product market conditions, and on employer heterogeneity. At any point in time, a range of cost-lowering strategies will coexist in the labour market. Some employers will be actively reducing the headcount of workers through layoffs and non-replaced quits. Others will 'labour hoard' by reducing the hours on offer. Others still will seek to lower training costs or non-wage benefits. If the balance of employers choosing different responses changes over time, quantitative estimates of average responses to minimum wage increases will vary accordingly.

Third, and related to the point above, the DID approach primarily detects short-run effects of minimum wage increases, which may differ from long-run effects. In the short term, employers may find it easier or more beneficial to respond to minimum wage increases by adjusting hours worked and/or refraining from recruiting. Layoffs may be a last resort due to the potential costs they entail and the lumpiness of their effects. In particular, if employers expect an increase in the real minimum wage to be temporary because of wage or product price inflation, small or zero employment effects may be observed. If, in time, these expectations are proven wrong, employers may take steps to permanently change their input mix away from minimum wage labour.

In the final analysis, however, it is not possible to disentangle fully the ambiguity of the results presented in this technical supplement nor, therefore, to reach definitive conclusions about the employment effects of the minimum wage changes analysed by the study.

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A Combining income support receipt and labour earnings

Table A.1 **Selected income support payment recipients at 1 June 2013**[a]
Individuals aged 21–64

	Total number	*Percentage earning non-zero labour income*[b]	*Of which, proportion employed full-time*[c]
	N	%	%
Bereavement Allowance	184	20.7	0.1
Carer Payment	191 862	11.5	0.8
Parenting Payment Single	246 960	27.1	16.0
Parenting Payment Partnered	100 427	9.6	11.6
Austudy	47 984	31.2	4.3
Newstart Allowance	693 833	25.1	12.0
Partner Allowance	9 872	6.7	2.7
Sickness Allowance	8 615	10.3	6.6
Special Benefit (Newstart)	663	5.6	16.7
Special Benefit (Other)	363	1.9	0.0
Widow Allowance	23 379	11.2	1.3
Youth Allowance (Apprentice)	2 594	91.9	69.9
Youth Allowance (Other)	36 603	18.6	8.7
Youth Allowance (Student)	130 904	42.5	1.5
ABSTUDY	3 974	17.5	9.3
Non-payment partner	248 965	48.5	26.5

a Individuals earning Supported Wage System or Disability Supported Employment wages are excluded. **b** Anyone who received any labour income in June 2013 qualifies as employed. **c** Full-time work is defined as more than 60 hours per fortnight. Individuals reporting positive labour income but zero hours are excluded from the calculation.

Source: Productivity Commission estimates based on RED.

Table A.1 shows that a moderate proportion of income support recipients works, and also that a non-negligible proportion of those who work do so full-time (ignoring apprentices and non-payment partners). Nonetheless, most income support recipients work part-time.

The extent of part-time work is also reflected in table A.2, where average earnings per person for selected benefit types are below the value of a full-time adult minimum wage in June 2012, namely $1 212.96 (people aged less than 21 or above 64 are excluded from the analysis). Table A.2 also shows, however, that for those benefit types that contain many

employed persons, it is possible for a single earner with dependents to work permanently and full-time at the minimum wage and remain eligible for part payments. The only payments for which this is not possible are Newstart and Parenting Payment Partnered. It is not possible for a single person or dual income couple without children to retain any income support payments when working full-time at the national minimum wage (Australian Government 2015).

Table A.2 **Income threshold and average earnings per fortnight for selected payment types at 1 June 2013**

Individuals aged 21–64

	Maximum income allowed for part payment to single person	*Max income for part payment higher or lower than full-time adult minimum wage of $1,212.96?*	*Average earned income per RED individual*[a]	*Average earnings as percentage of full-time adult minimum fortnightly wage*[b]
	$		$	%
Newstart Allowance[c]	1004.84	Lower	757.10	62.4
Youth Allowance (Student)[c]	1308.17	Higher	503.71	41.5
Parenting Payment Single	1914.35[d]	Higher	911.97	75.2
Parenting Payment Partnered	853.84[e]	Lower	696.06	57.4
Carer Payment	1768.80[e]	Higher	611.93	50.5
Austudy[c]	1308.17	Higher	570.78	47.1
ABSTUDY[c]	1308.17	Higher	594.59	49.0
Non-payment partner	-	-	1319.55	108.8

[a] Total earnings across all episodes and jobs are calculated for each individual for the month of June 2013, and then converted to a single fortnightly figure (= (total earnings/total days)*14). [b] Based on 72 hours per fortnight at $15.96 per hour, that wage is equal to $1,212.96. [c] Threshold is for single with dependents. [d] Plus $24.60 for each additional child. [e] If partner is not a pensioner. Also subject to income test for partner and for combined income of the couple. If partner is a pensioner, couple's combined income must be less than $1707.68 per fortnight.

Sources: Centrelink (2013); Productivity Commission estimates based on RED.

B Benchmarking RED against HILDA

The following series of graphs compares distributions of weekly hours worked (figure B.1), weekly income from wages and salary (figure B.2) and hourly wage (figures B.3 and B.4) from RED and HILDA 2012. First, table B.1 compares the samples used from each dataset.

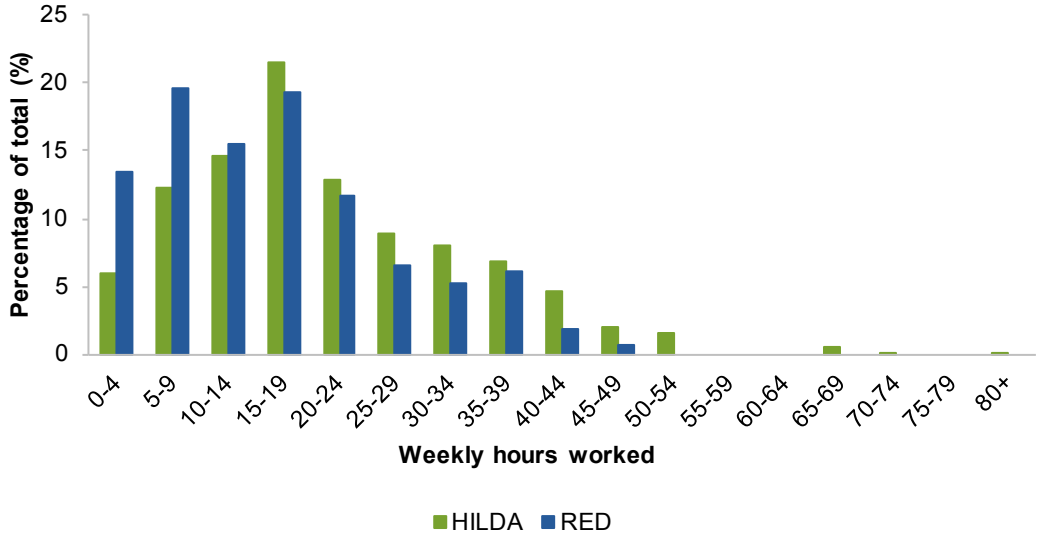
Table B.1 **RED and HILDA samples for benchmarking**

	RED	*HILDA*
Timing	Episodes active on 1 August 2012	HILDA wave 12, collected between 31 July 2012 and 10 February 2013 (only 2.9 per cent collected after December)
Income support recipients	Bereavement, Newstart, Sickness, Widow, Partner, and Youth Allowances; Special Benefit; Austudy and ABSTUDY; Carer Payment; Wife Pension; and Parenting Payment (single or partnered)[a]	Bereavement, Newstart, Sickness, Widow, Partner, and Youth Allowances; Special Benefit; Austudy and ABSTUDY; Carer Payment; Wife Pension; and Parenting Payment (single or partnered)
Ages	21 to 64	21 to 64
Included individuals/jobs	Highest-earning job; hourly wage greater than zero, and hours greater than zero.	Hours in main job greater than zero, and income in main job greater than zero, and employment status of employee, employee of own business, or employer/self-employed.

[a] This differs slightly from the sample used for the econometrics, which includes non-payment partners, and excludes Supported Wage System and Disability Supported Employment recipients.

Source: Summerfield et al (2013).

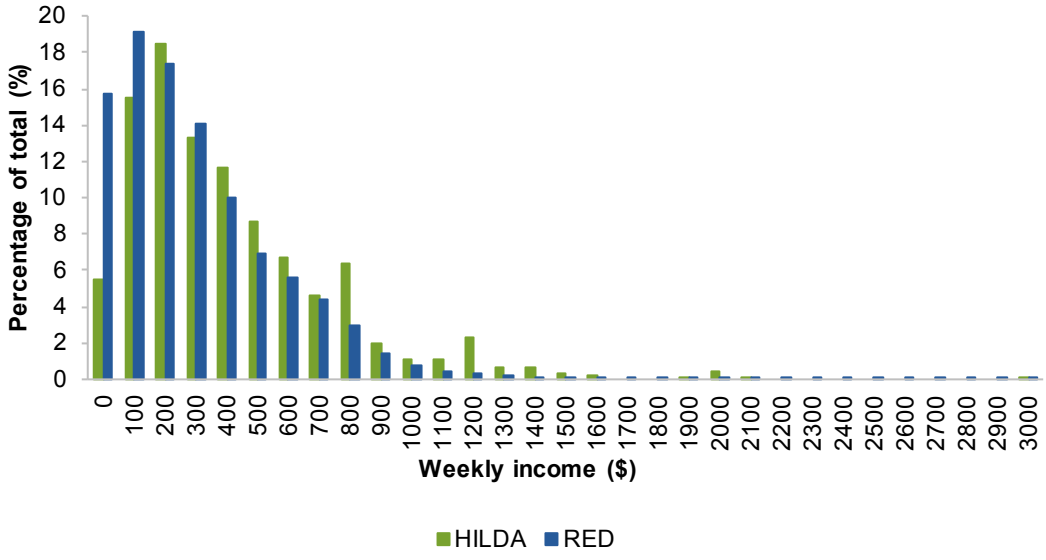
Figure B.1 **Distribution of weekly hours worked, 2012[a]**



[a] The maximum fortnightly hours that can be recorded in RED is 99.

Data source: Productivity Commission estimates based on RED and HILDA Wave 12.

Figure B.2 **Distribution of weekly labour income, 2012[a]**

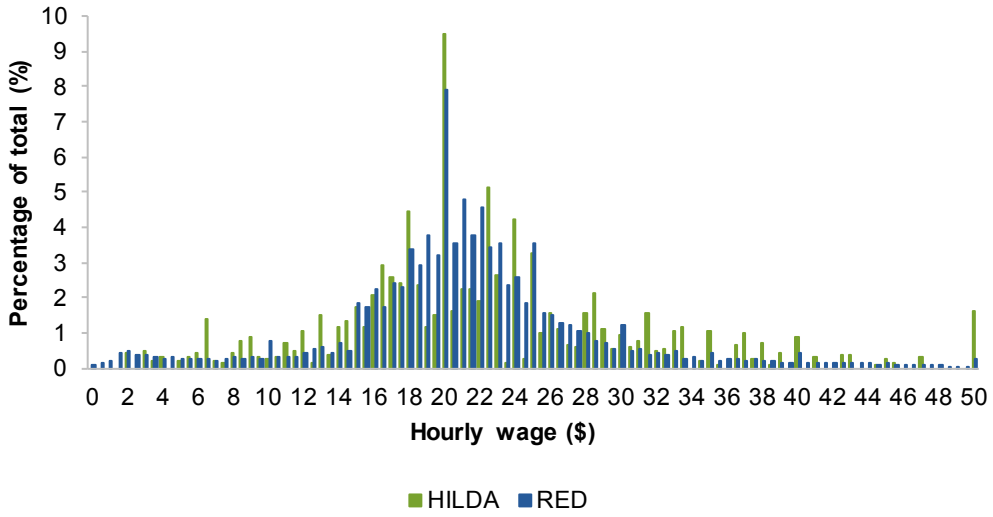


[a] Although only incomes between $0 and $3000 appear in the graph, the values in the chart are percentages of *all* income earners in the relevant group. The maximum in the HILDA sample is $3000. Approximately 0.04 per cent of RED observations are excluded.

Data source: Productivity Commission estimates based on RED and HILDA wave 12.

Figure B.3 **Distribution of hourly wages, 2012**

Without casual adjustment in HILDA

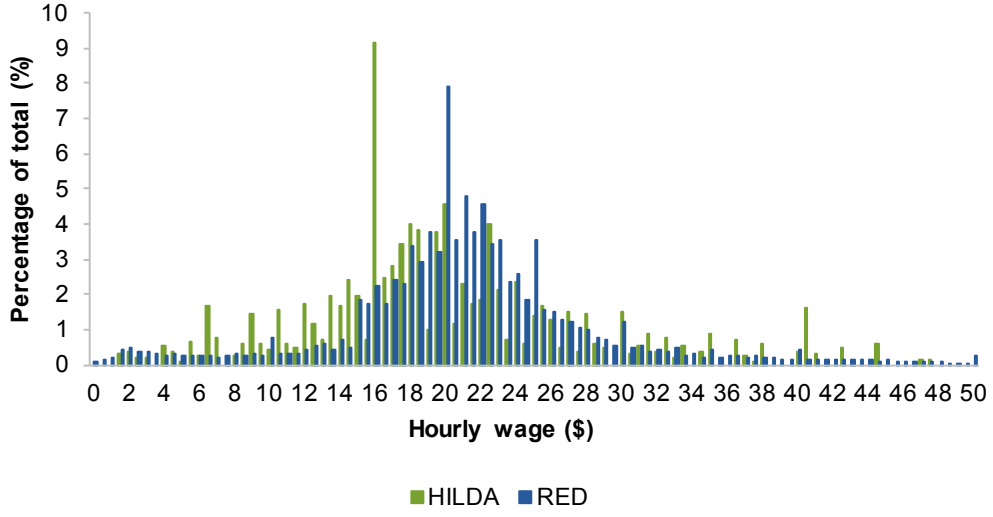


[a] Although only wages between $0 and $50 appear in the graph, the values in the chart are percentages of *all* wage earners in the relevant group. Approximately 3.9 per cent of HILDA observations and 2.3 per cent of RED observations are excluded.

Data source: Productivity Commission estimates based on RED and HILDA wave 12.

Figure B.4 **Distribution of hourly wages, 2012**

With casual adjustment in HILDA



[a] Although only wages between $0 and $50 appear in the graph, the values in the chart are percentages of *all* wage earners in the relevant group. Approximately 3.0 per cent of HILDA observations and 2.3 per cent of RED observations are excluded.

Data source: Productivity Commission estimates based on RED and HILDA wave 12.

C Group definitions

Table C.1 **Job loss and hours groups definitions**

Name	*Group*	*2008*	*2009 and 2010*	*2011*	*2012*	*2013*
Narrow	Below	(0,13.74)	(0, 14.31)	(0, 15)	(0, 15.50)	(0, 15.96)
	Treatment	[13.74, 14.31)[a]	[14.31, 15)	[15, 15.51)	[15.50, 15.96)	[15.96, 16.37)
	Control_1	[14.31, 15.74)	[15, 16.50)	[15.50, 17.06)	[15.96, 17.56)	[16.37, 18.01)
	Control_2	[15.74, 17.17)	[16.50, 18)	[17.06, 18.61)	[17.56, 19.15)	[18.01, 19.64)
	High	[17.17, 50)	[18, 50)	[18.61, 50)	[19.15, 50)	[19.64, 50)

- Below: $0< w_{it}< MW_t$: Treatment: $MW_t \leq w_{it}<MW_{t+1}$; Control_1: $MW_{t+1} \leq w_{it} <1.1* MW_{t+1}$; Control_2: $1.1* MW_{t+1}\leq w_{it} <1.2* MW_{t+1}$; High: $1.2* MW_{t+1}\leq w_{it} < 50$[b]

Pros

- Closest to conventional groups; little contamination of Treatment group

Cons

- Measurement error, bunching, casual loading unaccounted for.

Medium	Below	(0, 12)	(0, 13)	(0, 13.50)	(0, 14)	(0, 14.50)
	Treatment	[12, 15.03)	[13, 15.75)	[13.50, 16.29)	[14, 16.76)	[14.50, 17.19)
	Control_1	[15.03, 16.46)	[15.75, 17.25)	[16.29, 17.84)	[16.76, 18.35)	[17.19, 18.83)
	Control_2	[16.46, 17.89)	[17.25, 18.75)	[17.84, 19.39)	[18.35, 19.95)	[18.83, 20.46)
	High	[17.89, 50)	[18.75, 50)	[19.39, 50)	[19.95, 50)	[20.46, 50)

- Below: $0< w_{it} <$ Treatment lower bound (based on data); Treatment: (based on data) $\leq w_{it}<1.05*MW_{t+1}$; Control_1: $1.05* MW_{t+1} \leq w_{it} <1.15* MW_{t+1}$; Control_2: $1.15* MW_{t+1}\leq w_{it} <1.25* MW_{t+1}$; High: $1.25* MW_{t+1}\leq w_{it} <50$

Pros

- Treatment lower bound determined by wage earners receiving pay rises consistent with MW increase.

Cons

- Potential for group contamination.

Wide	Below	(0, 10.31)	(0, 10.73)	(0, 11.25)	(0, 11.63)	(0, 11.97)
	Treatment	[10.31, 17.17)	[10.73, 18)	[11.25, 18.61)	[11.63, 19.15)	[11.97, 19.64)
	Control_1	[17.17, 20.61)	[18, 21.60)	[18.61, 22.33)	[19.15, 22.98)	[19.64, 23.57)
	Control_2	[20.61, 24.73)	[21.60, 25.92)	[22.33, 26.80)	[22.98, 27.58)	[23.57, 28.29)
	High	[24.73, 50)	[25.92, 50)	[26.80, 50)	[27.58, 50)	[28.29, 50)

- Below: $0< w_{it} < 0.75* MW_t$; Treatment: $0.75*MW_t \leq w_{it} < 1.2*MW_{t+1}$; Control_1: $1.2*MW_{t+1} \leq w_{it} <$ 1.2*(Treatment upper bound); Control_2: 1.2*(Treatment upper bound) $\leq w_{it} <$1.2*(Control_1 upper bound); High: 1.2*(Control_1 upper bound) $\leq w_{it} <50$

Pros

- Captures casuals receiving up to 20 per cent loading. This is a conservative average to avoid too much contamination by going up to 25 per cent (also, not all casuals received 25 per cent loading in all years); accounts for measurement error and bunching.

Cons

- Treatment contamination by Control_1 members and vice versa.

[a] Interval notation is to be interpreted as follows: [or] denotes that the value is included in the interval, (or) denotes that the value is not included. [b] 'MW' stands for minimum wage.

Table C.2 **Job entry groups definitions**

Name	*Group*	*2008*	*2009 and 2010*	*2011*	*2012*	*2013*
Dickens	Below	(0, 10.73)	(0, 11.25)	(0, 11.63)	(0, 11.97)	(0, 12.28)
	Treatment	[10.73, 14.31][a]	[11.25, 15]	[11.63, 15.51]	[11.97, 15.96]	[12.28, 16.37]
	Control_1	(14.31, 15.74]	(15, 16.50]	(15.5, 17.06]	(15.96, 17.56]	(16.37, 18.01]
	Control_2	(15.74, 17.17]	(16.50, 18]	(17.06, 18.61]	(17.56, 19.15]	(18.01, 19.64]
	High	(17.17, 50)	(18, 50)	(18.61, 50)	(19.15, 50)	(19.64, 50)
Bryan	Below	(0, 13.59)	(0, 14.25)	(0, 14.73)	(0, 15.16)	(0, 15.55)
	Treatment	[13.59, 15.03]	[14.25, 15.75]	[14.73, 16.29]	[15.16, 16.76]	[15.55, 17.19]
	Control_1	(15.03, 16.46]	(15.75, 17.25]	(16.29, 17.84]	(16.76, 18.35]	(17.19, 18.83]
	Control_2	(16.46, 17.89]	(17.25, 18.75]	(17.84, 19.39]	(18.35, 19.95]	(18.83, 20.46]
	High	(17.89, 50)	(18.75, 50)	(19.39, 50)	(19.95, 50)	(20.46, 50)
Wide	Below	(0, 10.73)	(0, 11.25)	(0, 11.63)	(0, 11.97)	(0, 12.28)
	Treatment	[10.73, 17.17]	[10.25, 18]	[11.63, 18.61]	[11.97, 19.15]	[11.28, 19.64]
	Control_1	(17.17, 20.61]	(18, 21.60]	(18.61, 22.33]	(19.15, 22.98]	(19.64, 23.57]
	Control_2	(20.61, 24.73]	(21.60, 25.92]	(22.33, 26.80]	(22.98, 27.58]	(23.57, 28.29]
	High	(24.73, 50)	(25.92, 50)	(26.80, 50)	(27.58, 50)	(28.29, 50)

- Below: $0< w_{it}<0.75*MW_{t+1}$; Treatment: $0.75*MW_{t+1} \leq w_{it} \leq MW_{t+1}$; Control_1: $MW_{t+1} < w_{it} \leq 1.1* MW_{t+1}$; Control_2: $1.1*MW_{t+1}< w_{it} \leq 1.2* MW_{t+1}$; High: $1.2* MW_{t+1}< w_{it} < 50$[b]

Pros

- Closest to standard groups
- Similar to Dickens, Riley and Wilkinson's (2009) approach, although they had no lower bound for the Treatment group.

Cons

- Measurement error, bunching, casual loading unaccounted for.

- Below: $0< w_{it} < 0.95* MW_{t+1}$; Treatment: $0.95* MW_{t+1}< w_{it} \leq 1.05*MW_{t+1}$; Control_1: $1.05* MW_{t+1} < w_{it} \leq 1.15* MW_{t+1}$; Control_2: $1.15* MW_{t+1}< w_{it} \leq 1.25* MW_{t+1}$; High: $1.25* MW_{t+1}< w_{it} \leq 50$

Pros

- Bryan, Salvatori and Taylor (2013) use this approach.

Cons

- Measurement error, bunching, casual loading unaccounted for.

- Below: $0< w_{it}<0.75*MW_{t+1}$; Treatment: $0.75*MW_{t+1} < w_{it} \leq 1.2*MW_{t+1}$; Control_1: $1.2*MW_{t+1}< w_{it} \leq$ 1.2*(Treatment upper bound); Control_2: 1.2*(Treatment upper bound) $< w_{it} \leq$ 1.2*(Control_1 upper bound); High: 1.2*(Control_1 upper bound) $< w_{it} <50$

Pros

- Captures casuals receiving up to 20 per cent loading. This is a conservative average to avoid too much contamination by going up to 25 per cent. Also, not all casuals received 25 per cent loading in all years.
- Accounts for measurement error and bunching.

Cons

- Treatment contamination by Control_1 members and vice versa.

[a] Interval notation is to be interpreted as follows: [or] denotes that the value is included in the interval, (or) denotes that the value is not included. [b] 'MW' stands for minimum wage.

D Control variables

Table D.1 summarises the control variables included the DID regressions. A wide array of additional control variables could have been included, based on the literature. However, it does not appear likely that different controls would affect the findings of the DID analyses.

Table D.1 **Control variables used in regressions[a]**

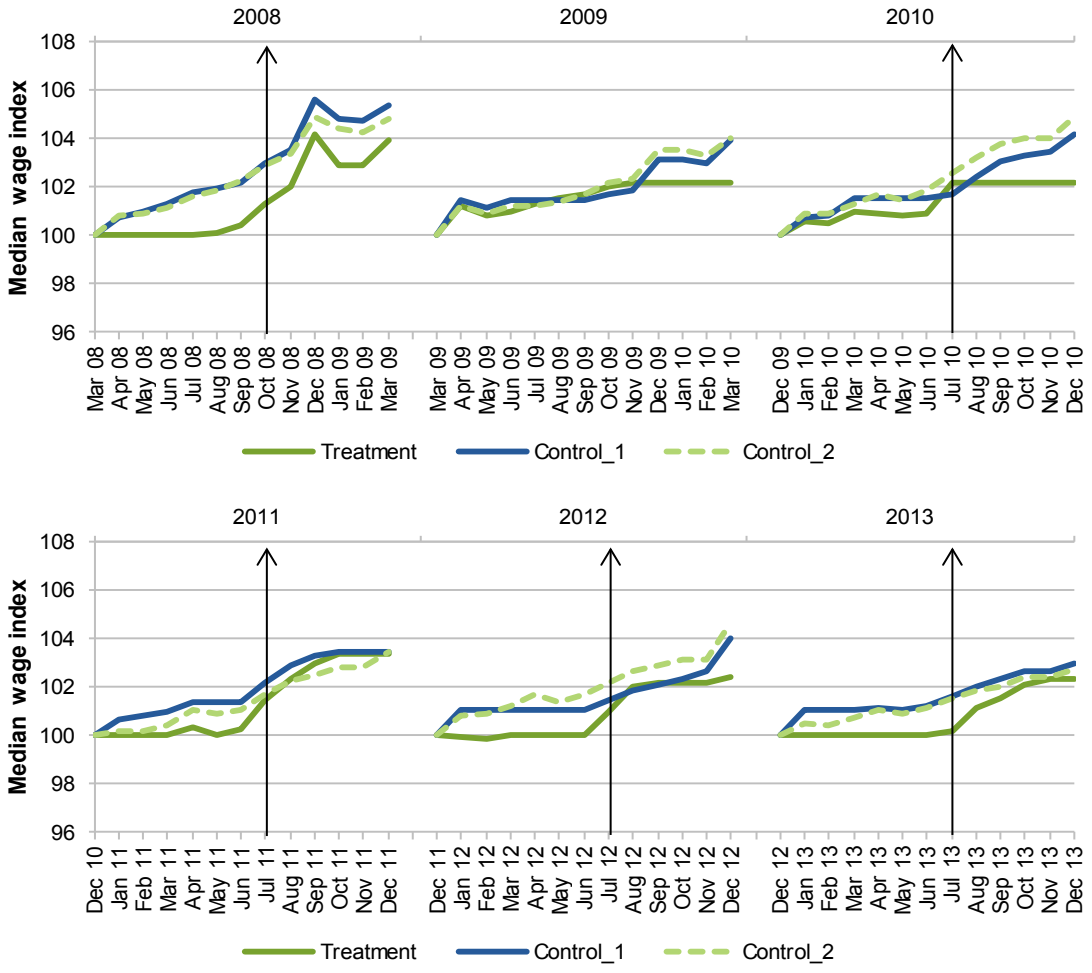
Variable name	*Description*
Age group	
Age1	Equals 1 if age ≤ 25, 0 otherwise
Age2	Equals 1 if 25 < age ≤ 35, 0 otherwise
Age3	Equals 1 if 35 < age ≤ 45, 0 otherwise
Age4	Equals 1 if 45 < age ≤ 55, 0 otherwise
Age5 (the omitted category in regressions)	Equals 1 if 55 < age ≤ 64, 0 otherwise
Benefit type	
Carer	Equals 1 if receiving Carer Payment, 0 otherwise
PPS	Equals 1 if receiving Parenting Payment Single, 0 otherwise
PPP	Equals 1 if receiving Parenting Payment Partnered, 0 otherwise
Austudy	Equals 1 if receiving Austudy, 0 otherwise
NSA	Equals 1 if receiving Newstart Allowance, 0 otherwise
YA	Equals 1 if receiving Youth Allowance (Student, Apprentice or Other), 0 otherwise
Misc.	Equals 1 if receiving some other benefit type, 0 otherwise
NPP (the omitted category in regressions)	Equals 1 if non-payment partner, 0 otherwise
English	Equals 1 if speaks English, 0 if interpreter required.
Female	Equals 1 if female, 0 if male.
Full-time	Equals 1 if employee worked more than 60 hours in the fortnight, 0 if 60 or fewer.
Months on ISP	The number of months that the individual has been on an income support payment in the current episode.

a All values are defined at the beginning of the transition for the job loss and hours models, and at the time of entry (in the relevant year) for the job entry model.

E Median wage indexes

Figures E.1, E.2, and E.3 display monthly movements in the median wage indexes of the Treatment, Control_1 and Control_2 groups between 2008 and 2013. To create the graphs, jobs are allocated to a group based on their wage at the beginning of the pre-treatment period in each year. They will remain in that group for the subsequent 12 months whenever the job is observed in RED, so the size of the group can vary over time. The median for the group is calculated from each job's average hourly wages for that month. Minimum wage increases are indicated by the vertical arrows. (There was no uprating in 2009.)

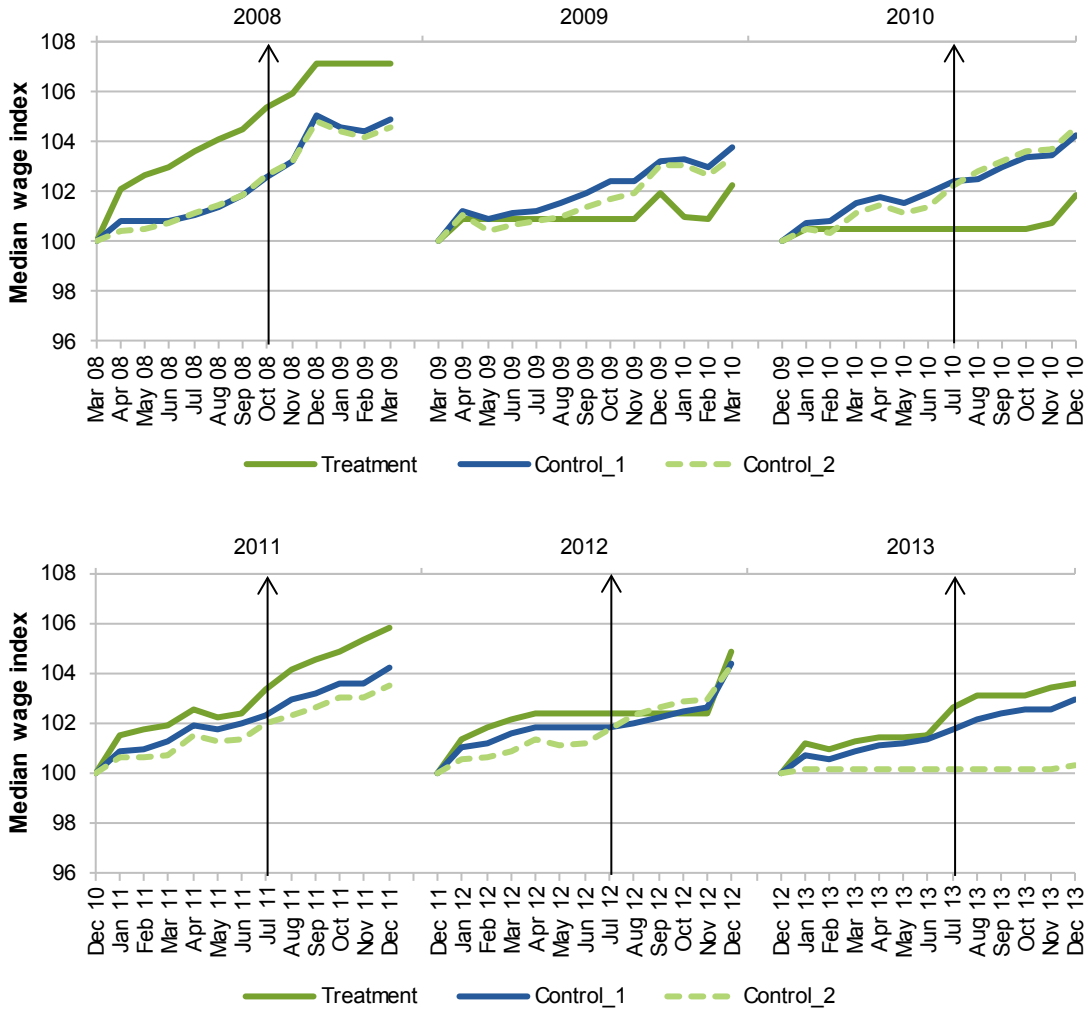
Figure E.1 **Narrow Treatment, Control_1 and Control_2 groups**
Median wage indexes, 2008–2013



Data source: Productivity Commission estimates based on RED.

Figure E.2 **Medium Treatment, Control_1 and Control_2 groups**

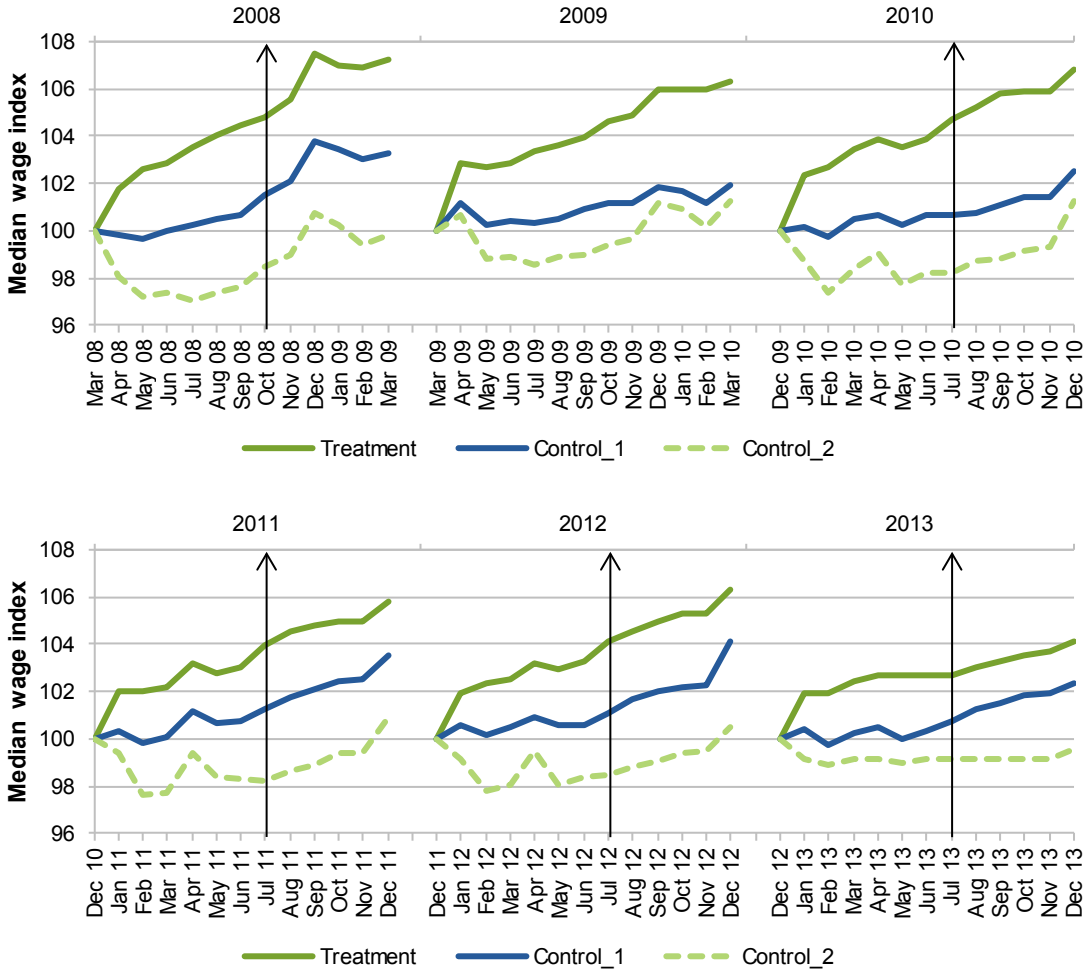
Median wage indexes, 2008–2013



Data source: Productivity Commission estimates based on RED.

Figure E.3 **Wide Treatment, Control_1 and Control_2 groups**

Median wage indexes, 2008–2013



Data source: Productivity Commission estimates based on RED.

F Changes in the groups' wage distributions

Figures F.1, F.2 and F.3 measure shifts in the wage distribution brought about by the 2010 minimum wage increase. The identification of the minimum wage effects uses the shifts that occurred during the 2009 minimum wage freeze as a comparator. This exercise is inspired by the analysis underlying Clemens and Wither's figure 7 (2014, p. 49). It consists of the following series of steps:

1. The periods of interest are April to September 2009 (the control period), and April to September 2010 (the treatment period).
2. Jobs are allocated to Treatment, Control_1 and Control_2 based on their average wage over the three months between April and June 2010 (the 'baseline' period, prior to the July 2010 minimum wage increase). The narrow, medium and group definitions (illustrated on the left-hand side of figure 3.3) are used, with the only difference being that under all definitions, the lower bound of the Treatment group becomes zero.
3. A wage category variable is generated which cuts monthly hourly wage into 50 cent intervals, and the frequency distribution of wage categories in the baseline period is graphed for each group. Months spent out of employment are not included.
4. Jobs from 2009 are then assigned to the three wage groups based on their average wage from April to June 2009.
5. A series of DID models are estimated, where the dependent variable ($Y_{i,t}$) for each model is an indicator variable for a wage category. For all months in the sample, that variable takes a value of one if the job's hourly wage for the month is in that category, and zero otherwise (including inactive jobs). The following model is estimated for each group (j = 1, 2, 3) separately:

$$Y_{i,t}^{j} = \beta_0 + \beta_1 Post_{it} + \beta_2 Treatment_{it} + \beta_3 (Post * Treatment)_{it} \quad (6)$$

   (a) The $Treatment$ group contains observations captured between April and September 2010. The control group ($Treatment$ = 0) contains observations captured between April and September 2009.

   (b) $Post$ is equal to 0 for April, May and June in 2009 and 2010, and 1 for July, August and September in 2009 and 2010.

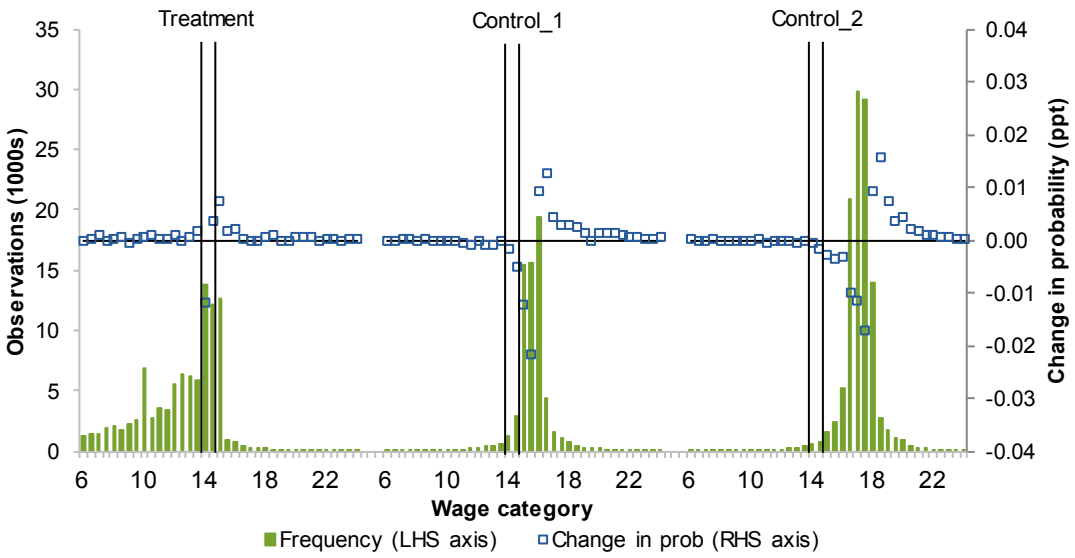
   (c) The DID coefficient is the coefficient on $Post * Treatment$, and therefore equals one in July, August and September 2010. The assumption behind each of these DID models is that, in the absence of the minimum wage increase, wage growth in each group would be the same in 2009 and 2010 (the parallel paths assumption).

Therefore, the DID coefficient (β_3) is taken to represent the change in probability of having a wage in that wage category resulting from the minimum wage increase in July 2010.

6. For each group, the DID estimates are graphed against the baseline wage distributions. In each graph, the values on the x-axis are the minimum values in each wage category (for example, wage category 14 includes jobs paying between \$14 and \$14.49 per hour). The black vertical lines indicate the wage categories that are directly affected by the minimum wage increasing from \$14.31 to \$15. The wage category frequencies (the histogram) are measured on the left-hand side y-axis, while the DID estimates (the dots) are measured on the right-hand side y-axis. Thus, the graphs should be read as showing that, for example, Treatment group members spent the most time in the \$14 an hour wage category during April–June 2010 (figure G.1). In July–September 2010, their probability of finding themselves in the same wage category had changed in a way that was more negative (less positive) than if the 2009 change had applied. The corresponding dot at the \$14 mark of figure G.1 is therefore below zero.

Figure F.1 **Narrow Treatment, Control_1 and Control_2 groups**

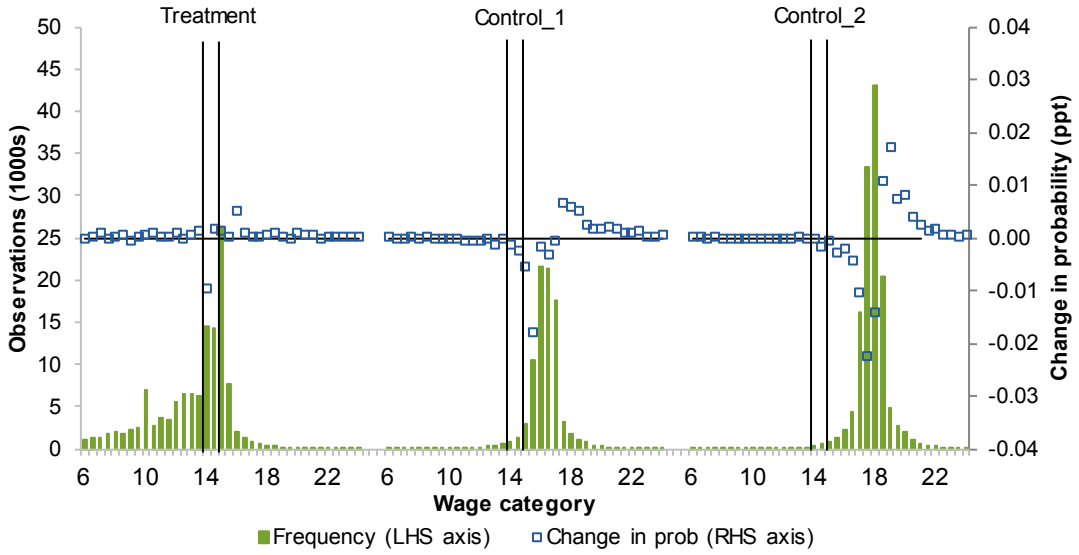
Changes in the wage distribution



Data source: Productivity Commission estimates based on RED.

Figure F.2 **Medium Treatment, Control_1 and Control_2 groups**

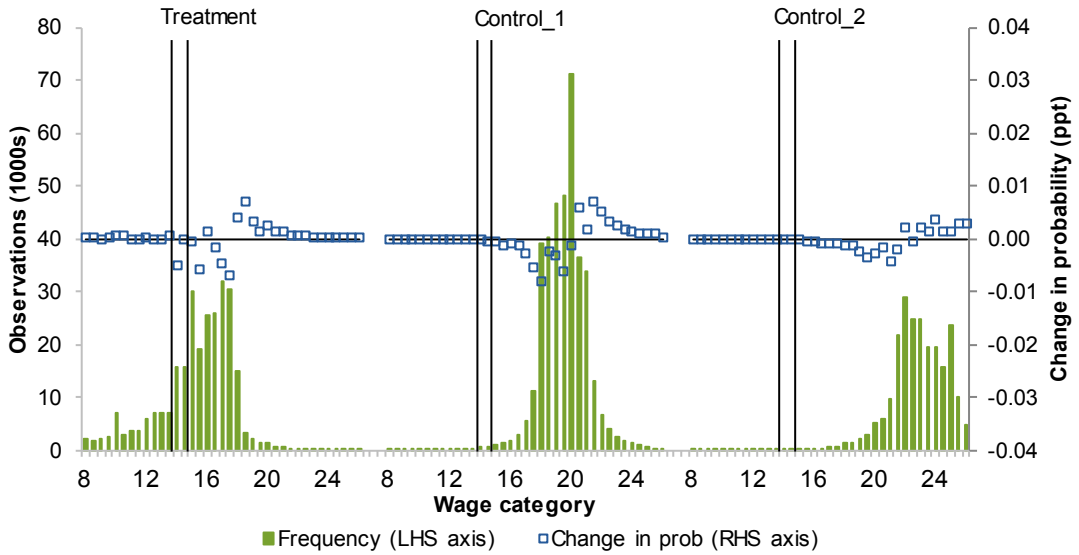
Changes in the wage distribution



Data source: Productivity Commission estimates based on RED.

Figure F.3 **Wide Treatment, Control_1 and Control_2 groups**

Changes in the wage distribution



Data source: Productivity Commission estimates based on RED.

G Pairwise comparisons of groups

Table G.1 **Pairwise comparisons: personal characteristics**
Treatment, Control_1 and Control_2 groups

	Narrow						*Medium*						*Wide*					
	T	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*
	Value			Significance[a]			Value			Significance			Value			Significance		
Age																		
2008	40.2	39.5	39.1	***	***	***	39.8	39.2	39.0	***	***	**	39.4	38.9	39.0	***	***	***
2009	40.1	39.3	39.0	***	***	***	39.7	39.0	38.9	***	***	*	39.3	38.5	38.8	***	***	***
2010	40.5	39.6	39.3	***	***	***	40.0	39.2	39.2	***	***	-	39.5	38.9	39.3	***	***	***
2011	40.8	39.0	39.1	***	***	-	39.9	39.0	39.1	***	***	-	39.3	38.8	39.4	***	*	***
2012	41.0	39.1	38.9	***	***	**	39.9	38.8	39.0	***	***	**	39.2	38.8	39.5	***	***	***
2013	41.6	38.8	38.8	***	***	-	39.8	38.7	38.6	***	***	-	39.1	38.7	39.5	***	***	***
Female																		
2008	0.69	0.69	0.73	-	***	***	0.65	0.72	0.74	***	***	***	0.68	0.76	0.75	***	***	***
2009	0.69	0.68	0.72	***	***	***	0.65	0.71	0.73	***	***	***	0.68	0.74	0.72	***	***	***
2010	0.68	0.66	0.71	***	***	***	0.63	0.69	0.72	***	***	***	0.66	0.73	0.72	***	***	***
2011	0.62	0.66	0.69	***	***	***	0.62	0.69	0.70	***	***	***	0.65	0.72	0.72	***	***	-
2012	0.66	0.65	0.69	***	***	***	0.61	0.67	0.70	***	***	***	0.64	0.71	0.71	***	***	-
2013	0.65	0.63	0.68	-	***	***	0.60	0.66	0.66	***	***	-	0.63	0.70	0.70	***	***	**
English-speaking[b]																		
2008	0.79	0.91	0.95	***	***	***	0.85	0.94	0.96	***	***	***	0.90	0.97	0.97	***	***	***
2009	0.80	0.90	0.95	***	***	***	0.84	0.94	0.96	***	***	***	0.90	0.97	0.97	***	***	***
2010	0.77	0.88	0.94	***	***	***	0.82	0.92	0.95	***	***	***	0.89	0.97	0.97	***	***	***
2011	0.75	0.89	0.94	***	***	***	0.80	0.92	0.95	***	***	***	0.88	0.96	0.97	***	***	***
2012	0.71	0.86	0.94	***	***	***	0.79	0.92	0.94	***	***	***	0.88	0.96	0.97	***	***	***
2013	0.69	0.86	0.93	***	***	***	0.78	0.91	0.94	***	***	***	0.87	0.96	0.97	***	***	***
ATSI[c]																		
2008	0.04	0.04	0.04	***	***	-	0.04	0.04	0.04	-	-	-	0.04	0.04	0.04	***	-	***
2009	0.03	0.04	0.04	***	***	-	0.04	0.04	0.04	-	*	-	0.04	0.04	0.04	***	-	***
2010	0.03	0.04	0.04	***	***	-	0.04	0.04	0.04	-	-	-	0.04	0.04	0.04	***	-	-
2011	0.03	0.04	0.04	***	***	-	0.04	0.04	0.04	**	*	-	0.04	0.04	0.04	***	-	***
2012	0.03	0.04	0.05	***	***	*	0.04	0.05	0.05	***	***	-	0.05	0.04	0.04	***	***	-
2013	0.03	0.04	0.05	***	***	***	0.04	0.04	0.04	***	***	-	0.05	0.04	0.04	***	***	-

[a] Significance of differences. *** denotes significant at 1 per cent, ** denotes significant at 5 per cent, * denotes significant at 10 per cent. [b] Based on value for spoken language code in RED. If value is not English, an interpreter is required for customer's interviews with Centrelink. [c] Aboriginal or Torres Strait Islander.

Sources: Productivity Commission estimates based on RED; Woolridge (2009).

Table G.2 **Pairwise comparisons: income and income support**

Treatment, Control_1 and Control_2 groups

	Narrow						*Medium*						*Wide*					
	T	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*
	Value			Significance[a]			Value			Significance			Value			Significance		
Unearned income[b]																		
2008	0.53	0.53	0.56	-	***	***	0.52	0.55	0.56	***	***	**	0.54	0.59	0.62	***	***	***
2009	0.52	0.52	0.55	-	***	***	0.50	0.54	0.55	***	***	***	0.52	0.57	0.61	***	***	***
2010	0.53	0.52	0.55	-	***	***	0.51	0.54	0.56	***	***	***	0.53	0.58	0.62	***	***	***
2011	0.52	0.53	0.56	-	***	***	0.52	0.54	0.56	***	***	***	0.53	0.58	0.63	***	***	***
2012	0.54	0.52	0.55	***	-	***	0.51	0.54	0.56	***	***	***	0.52	0.58	0.63	***	***	***
2013	0.53	0.52	0.55	***	***	***	0.51	0.53	0.54	***	***	***	0.52	0.57	0.63	***	***	***
Non-payment partner																		
2008	0.13	0.15	0.15	***	***	-	0.15	0.16	0.13	***	***	***	0.15	0.12	0.14	***	***	***
2009	0.13	0.16	0.15	***	***	***	0.15	0.16	0.14	***	***	***	0.16	0.12	0.14	***	***	***
2010	0.13	0.16	0.16	***	***	-	0.16	0.17	0.15	***	**	***	0.16	0.13	0.15	***	***	***
2011	0.13	0.17	0.16	***	***	***	0.16	0.17	0.15	***	***	***	0.16	0.13	0.16	***	***	***
2012	0.13	0.17	0.17	***	***	-	0.16	0.18	0.16	***	-	***	0.17	0.14	0.17	***	***	***
2013	0.13	0.19	0.19	***	***	-	0.17	0.20	0.16	***	***	***	0.18	0.15	0.17	***	***	***
Newstart recipient																		
2008	0.27	0.27	0.24	-	***	***	0.31	0.24	0.24	***	***	-	0.28	0.23	0.21	***	***	***
2009	0.31	0.32	0.28	-	***	***	0.34	0.29	0.28	***	***	-	0.32	0.28	0.25	***	***	***
2010	0.35	0.36	0.31	**	***	***	0.38	0.32	0.31	***	***	***	0.35	0.31	0.28	***	***	***
2011	0.44	0.36	0.33	***	***	***	0.41	0.34	0.34	***	***	-	0.37	0.33	0.28	***	***	***
2012	0.43	0.39	0.35	***	***	***	0.44	0.36	0.35	***	***	***	0.39	0.35	0.29	***	***	***
2013	0.61	0.47	0.41	***	***	***	0.53	0.43	0.44	***	***	***	0.46	0.42	0.34	***	***	***
Months on ISP[c]																		
2008	79.1	69.8	68.2	***	***	***	71.2	68.3	69.0	***	***	-	69.2	67.1	62.4	***	***	***
2009	77.2	66.3	65.0	***	***	***	70.4	65.0	65.0	***	***	-	66.3	62.2	56.7	***	***	***
2010	80.1	66.5	64.5	***	***	***	70.6	64.1	64.9	***	***	-	66.1	62.3	56.9	***	***	***
2011	76.8	64.9	64.1	***	***	-	70.7	64.3	64.1	***	***	-	65.8	61.9	57.1	***	***	***
2012	84.7	66.8	63.6	***	***	***	71.1	63.0	64.2	***	***	***	65.4	61.2	56.2	***	***	***
2013	82.9	62.2	60.0	***	***	***	68.8	59.4	58.1	***	***	***	62.4	57.4	52.6	***	***	***

a Significance of differences. *** denotes significant at 1 per cent, ** denotes significant at 5 per cent, * denotes significant at 10 per cent. **b** Unearned income is ordinary income that is not employment income. This includes income from all sources that is not an exempt lump sum or maintenance income, for example, profit from self-employment, a superannuation payment, or compensation (Department of Education, Employment and Workplace Relations Social Policy and Economic Strategy Group Research Branch 2014).**c** Months on income support in current episode. Takes a value of zero for non-payment partners.

Sources: Productivity Commission estimates based on RED; Woolridge (2009).

Table G.3 **Pairwise comparisons: family circumstances**

Treatment, Control_1 and Control_2 groups

	Narrow						*Medium*						*Wide*					
	T	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*
	Value			Significance[a]			Value			Significance			Value			Significance		
Has partner																		
2008	0.37	0.34	0.32	***	***	***	0.37	0.33	0.31	***	***	***	0.35	0.29	0.30	***	***	***
2009	0.36	0.35	0.32	***	***	***	0.37	0.33	0.31	***	***	***	0.35	0.29	0.31	***	***	***
2010	0.37	0.36	0.33	-	***	***	0.38	0.35	0.32	***	***	***	0.36	0.30	0.32	***	***	***
2011	0.38	0.35	0.34	***	***	***	0.38	0.34	0.33	***	***	***	0.36	0.30	0.32	***	***	***
2012	0.38	0.36	0.34	***	***	***	0.38	0.34	0.33	***	***	***	0.36	0.30	0.33	***	***	***
2013	0.38	0.37	0.35	***	***	***	0.39	0.36	0.33	***	***	***	0.37	0.31	0.34	***	***	***
Has partner on ISP[b]																		
2008	0.28	0.29	0.28	**	-	***	0.30	0.29	0.27	***	***	***	0.30	0.25	0.27	***	***	***
2009	0.28	0.30	0.29	***	*	***	0.30	0.29	0.28	***	***	***	0.30	0.26	0.28	***	***	***
2010	0.28	0.31	0.30	***	***	***	0.31	0.31	0.29	-	***	***	0.31	0.27	0.29	***	***	***
2011	0.30	0.31	0.30	***	-	***	0.32	0.31	0.29	***	***	***	0.31	0.27	0.29	***	***	***
2012	0.29	0.31	0.30	***	*	*	0.32	0.31	0.30	***	***	***	0.31	0.27	0.29	***	***	***
2013	0.30	0.32	0.32	***	***	-	0.33	0.32	0.29	-	***	***	0.33	0.28	0.30	***	***	***
No. dependent children																		
2008	1.20	1.13	1.13	***	***	-	1.12	1.14	1.13	***	-	*	1.12	1.11	1.10	-	***	***
2009	1.12	1.05	1.05	***	***	-	1.06	1.06	1.04	-	**	-	1.05	1.02	1.01	***	***	-
2010	1.09	1.04	1.04	***	***	-	1.04	1.05	1.02	-	***	-	1.03	0.99	0.99	***	***	-
2011	1.00	1.02	1.00	-	-	-	1.00	1.02	0.99	**	-	-	1.00	0.96	0.96	***	***	-
2012	1.05	0.99	0.99	***	***	-	0.97	1.00	0.97	***	-	-	0.97	0.93	0.94	***	***	-
2013	0.99	0.94	0.96	***	***	-	0.93	0.96	0.91	***	-	-	0.94	0.89	0.90	***	***	-
Age of youngest child[c]																		
2008	5.73	4.86	4.78	***	***	**	4.84	4.85	4.80	-	-	-	4.76	4.71	4.46	***	***	***
2009	5.47	4.47	4.48	***	***	-	4.74	4.52	4.43	***	***	***	4.47	4.23	3.94	***	***	***
2010	5.54	4.39	4.35	***	***	-	4.64	4.37	4.29	***	***	***	4.35	4.10	3.86	***	***	***
2011	4.73	4.13	4.04	***	***	***	4.39	4.12	3.99	***	***	***	4.08	3.86	3.66	***	***	***
2012	5.20	4.07	3.88	***	***	***	4.15	3.90	3.85	***	***	*	3.89	3.65	3.49	***	***	***
2013	4.70	3.64	3.52	***	***	***	3.77	3.55	3.34	***	***	***	3.54	3.33	3.22	***	***	***

a Significance of differences. *** denotes significant at 1 per cent, ** denotes significant at 5 per cent, * denotes significant at 10 per cent. **b** Proportion of all observations, including those without partners. **c** Takes a value of zero for observations with no children.

Sources: Productivity Commission estimates based on RED; Woolridge (2009).

Table G.4 **Pairwise comparisons: geographic details**

Treatment, Control_1 and Control_2 groups

	Narrow						*Medium*						*Wide*					
	T	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*
	Value			Significance[a]			Value			Significance			Value			Significance		
Lives in NSW																		
2008	0.33	0.30	0.27	***	***	***	0.32	0.27	0.27	***	***	-	0.30	0.28	0.32	***	***	***
2009	0.34	0.30	0.28	***	***	***	0.33	0.28	0.28	***	***	-	0.30	0.29	0.31	***	***	***
2010	0.35	0.32	0.28	***	***	***	0.34	0.29	0.28	***	***	***	0.31	0.29	0.31	***	*	***
2011	0.39	0.32	0.29	***	***	***	0.36	0.30	0.28	***	***	***	0.32	0.29	0.30	***	***	***
2012	0.40	0.34	0.29	***	***	***	0.37	0.31	0.28	***	***	***	0.33	0.29	0.30	***	***	***
2013	0.44	0.34	0.30	***	***	***	0.38	0.31	0.29	***	***	***	0.34	0.29	0.30	***	***	***
Outer regional/remote[b]																		
2008	0.09	0.12	0.13	***	***	***	0.10	0.13	0.13	***	***	-	0.12	0.13	0.13	***	***	**
2009	0.09	0.11	0.12	***	***	***	0.10	0.12	0.13	***	***	***	0.11	0.13	0.12	***	***	***
2010	0.08	0.11	0.12	***	***	***	0.10	0.12	0.13	***	***	***	0.11	0.13	0.12	***	***	***
2011	0.08	0.11	0.12	***	***	***	0.09	0.12	0.13	***	***	***	0.11	0.13	0.12	***	***	***
2012	0.07	0.10	0.12	***	***	***	0.09	0.11	0.13	***	***	***	0.11	0.13	0.12	***	***	***
2013	0.06	0.10	0.12	***	***	***	0.08	0.11	0.13	***	***	***	0.11	0.13	0.12	***	***	***

a Significance of differences. *** denotes significant at 1 per cent, ** denotes significant at 5 per cent, * denotes significant at 10 per cent. **b** Using ABS definitions of outer regional, remote and very remote.

Sources: Productivity Commission estimates based on RED; Woolridge (2009).

Table G.5 **Pairwise comparisons: full-time status**

Treatment, Control_1 and Control_2 groups

	Narrow						*Medium*						*Wide*					
	T	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*	*T*	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*
	Value			Significance[a]			Value			Significance			Value			Significance		
Works full-time[b]																		
2008	0.13	0.18	0.21	***	***	***	0.15	0.22	0.20	***	***	***	0.19	0.17	0.16	***	***	***
2009	0.15	0.18	0.20	***	***	***	0.15	0.20	0.19	***	***	***	0.18	0.16	0.15	***	***	***
2010	0.13	0.17	0.20	***	***	***	0.14	0.20	0.19	***	***	***	0.18	0.16	0.15	***	***	***
2011	0.10	0.18	0.19	***	***	***	0.14	0.20	0.18	***	***	***	0.17	0.16	0.16	***	***	**
2012	0.12	0.17	0.19	***	***	***	0.14	0.19	0.18	***	***	-	0.17	0.15	0.15	***	***	***
2013	0.09	0.16	0.18	***	***	***	0.13	0.18	0.14	***	-	***	0.17	0.13	0.14	***	***	***

a Significance of differences. *** denotes significant at 1 per cent, ** denotes significant at 5 per cent, * denotes significant at 10 per cent. **b** More than 60 hours per fortnight.

Sources: Productivity Commission estimates based on RED; Woolridge (2009).

H Treatment group stability

Table H.1 **Treatment group stability and reasons for leaving**

	Narrow		*Medium*		*Wide*	
	Pre	**Post**	**Pre**	**Post**	**Pre**	**Post**
2008						
Jobs in Treatment group at beginning and end[a,b]	17	13	20	20	24	23
Pay rise	37	48	38	40	37	39
Pay cut	16	12	7	6	3	2
Job loss	22	20	26	25	25	25
Left RED	8	7	10	8	11	10
2009						
Jobs in Treatment group at beginning and end	16	17	26	26	29	27
Pay rise	50	44	36	36	34	35
Pay cut	14	17	7	7	2	2
Job loss	15	16	23	23	24	25
Left RED	6	6	9	9	10	11
2010						
Jobs in Treatment group at beginning and end	16	11	25	24	27	26
Pay rise	46	53	35	35	35	35
Pay cut	16	13	7	6	3	2
Job loss	16	16	25	25	26	25
Left RED	5	7	8	10	10	12
2011						
Jobs in Treatment group at beginning and end	28	25	28	28	29	29
Pay rise	26	30	33	32	32	32
Pay cut	19	23	6	6	2	2
Job loss	22	17	25	25	26	25
Left RED	5	5	8	10	10	12
2012						
Jobs in Treatment group at beginning and end	27	13	30	26	31	29
Pay rise	33	50	31	34	31	32
Pay cut	15	13	6	6	2	2
Job loss	21	18	26	25	26	25
Left RED	5	6	7	9	9	11
2013						
Jobs in Treatment group at beginning and end	26	19	29	31	32	33
Pay rise	31	46	32	31	33	30
Pay cut	14	12	7	6	3	2
Job loss	23	18	25	24	25	25
Left RED	5	5	7	8	8	10

a The beginning is the first month of the pre- or post-treatment period; the end is 6 months later. **b** The Treatment group definitions are those from figure 3.3.

Source: Productivity Commission estimates based on RED.

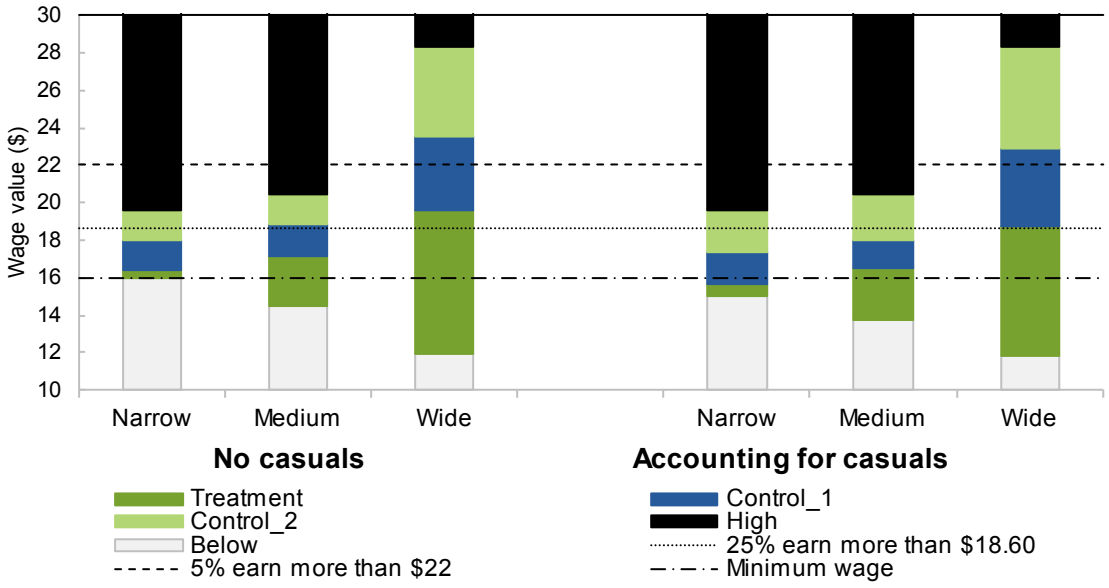
I Award reliance

On the left-hand side of figure I.1, two Buchanan and Wright hourly wage cut-offs for award-reliance ($22 and $18.60) are overlaid on the 2013 Treatment and control group definitions used in the analysis. For example, the 75 per cent cut-off line intersects the narrow Control_2, medium Control_1 and wide Treatment groups. However, the location of the groups relative to the cut-offs is biased by the lack of casual adjustment in RED.

To explore the possible effects of casual loading on measured award reliance, new, lower bounds for the Treatment and control groups are calculated using a weighted average of the adjusted wages of casual employees and the wages of non-casual employees, based on the analysis of the effects of casual adjustment in HILDA wave 12. That is, individuals remain in their initial group, but their wages are recalculated, which is reflected in the revised lower bounds. Revised bars are placed to the right of the original, unadjusted group definitions in figure I.1. For example, the $18.60 cut-off now takes in part of the medium Control_2 group. Although these calculations are an approximation, they demonstrate that a greater proportion of the RED groups is likely to be award-reliant than their non-adjusted wages would suggest.

Figure I.1 **Award reliance of Treatment and control groups with casual loading taken into account**

Prior to 2013 minimum wage increase



J Counterfactuals

Table J.1 **Job loss: estimated counterfactuals for the Treatment group[a,b]**

Narrow, medium and wide definitions

	Narrow	*Medium*	*Wide*
Full sample			
Standard			
2008	0.23	0.23	0.21
2009	0.19	0.22	0.20
2010	0.18	0.21	0.20
2011	0.24	0.22	0.20
2012	0.22	0.23	0.22
2013	0.24	0.23	0.21
Spillover			
2008	0.22	0.24	0.22
2009	0.19	0.21	0.20
2010	0.18	0.20	0.20
2011	0.24	0.21	0.21
2012	0.23	0.23	0.23
2013	0.25	0.23	0.23
NPPs only			
Standard			
2008	0.14	0.13	0.13
2009	0.12	0.13	0.12
2010	0.11	0.11	0.11
2011	0.13	0.12	0.11
2012	0.11	0.12	0.12
2013	0.13	0.13	0.12
Spillover			
2008	0.13	0.14	0.13
2009	0.11	0.12	0.12
2010	0.11	0.12	0.12
2011	0.15	0.13	0.11
2012	0.12	0.14	0.13
2013	0.14	0.13	0.11

a The estimated counterfactual is the predicted probability of job loss for the Treatment group in the absence of the minimum wage increase. It is equal to: $\text{Prob(job loss)}_{\text{Treatment, pre}}$.+ [$\text{Prob(job loss)}_{\text{Cont, post}}$ – $\text{Prob(job loss)}_{\text{Cont, pre}}$]. **b** These values are from models estimated without control variables.

Source: Productivity Commission estimates based on RED.

Table J.2 **Hours: estimated counterfactuals for the Treatment group[a,b]**

Narrow, medium and wide definitions

	Narrow	*Medium*	*Wide*
Full sample			
Standard			
2008	-0.25	-0.21	-0.67
2009	-0.30	0.09	-0.20
2010	-0.54	-0.53	-0.30
2011	-0.04	-0.42	-0.25
2012	-1.07	-0.78	-0.78
2013	-0.53	-0.63	-0.47
Spillover			
2008	-0.04	-0.59	-0.13
2009	-0.06	-0.06	0.19
2010	-0.02	-0.37	-0.12
2011	0.17	-0.65	0.01
2012	-0.69	-0.76	-0.47
2013	-0.03	-0.65	-0.04
NPPs only			
Standard			
2008	-1.53	-0.95	-1.70
2009	-1.23	-1.40	-1.35
2010	-1.42	-2.05	-2.27
2011	-1.09	-1.91	-2.35
2012	-1.33	-1.37	-2.42
2013	-1.69	-1.82	-2.43
Spillover			
2008	-1.32	-1.60	-1.53
2009	-1.26	-1.24	-1.19
2010	-1.33	-1.60	-2.12
2011	-0.98	-2.05	-2.14
2012	-0.88	-1.37	-2.52
2013	-1.77	-2.50	-2.67

a The estimated counterfactual is the predicted change in hours for the Treatment group in the absence of the minimum wage increase. It is equal to: $(\text{Change in hours})_{\text{Treatment, pre}}$.+ $[(\text{Change in hours})_{\text{Cont, post}} - (\text{Change in hours})_{\text{Cont, pre}}]$. **b** These values are from models estimated without control variables.

Source: Productivity Commission estimates based on RED.

Table I.3 **Job entry: estimated counterfactuals for the Treatment group[a,b]**

Dickens, Bryan and wide definitions

	Dickens	*Bryan*	*Wide*
Full sample			
Standard			
2008	0.36	0.34	0.34
2009	0.39	0.35	0.35
2010	0.43	0.37	0.40
2011	0.40	0.38	0.39
2012	0.39	0.35	0.36
2013	0.40	0.37	0.36
Spillover			
2008	0.36	0.34	0.37
2009	0.39	0.35	0.37
2010	0.41	0.36	0.41
2011	0.39	0.37	0.40
2012	0.37	0.34	0.38
2013	0.38	0.35	0.37
NPPs only			
Standard			
2008	0.14	0.15	0.13
2009	0.15	0.14	0.13
2010	0.20	0.17	0.16
2011	0.16	0.18	0.16
2012	0.15	0.15	0.16
2013	0.15	0.13	0.13
Spillover			
2008	0.15	0.15	0.15
2009	0.15	0.13	0.14
2010	0.18	0.16	0.17
2011	0.17	0.16	0.17
2012	0.15	0.14	0.17
2013	0.14	0.12	0.14

a The estimated counterfactual is the predicted probability of being a new hire into the Treatment group in the absence of the minimum wage increase. It is equal to: $\text{Prob(new hire)}_{\text{Treatment, pre}}$.+ [$\text{Prob(new hire)}_{\text{Cont, post}}$ – $\text{Prob(new hire)}_{\text{Cont, pre}}$]. **b** These values are from models estimated without control variables.

Source: Productivity Commission estimates based on RED.

K Exit rates

Table J.1 **Exit rates**

Treatment, Control_1 and Control_2 groups

	Exit rates[a]			*Significance of differences*[b]		
	T	*C1*	*C2*	*T vs C1*	*T vs C2*	*C1 vs C2*
Narrow						
2008	0.17	0.20	0.21	***	***	***
2009	0.16	0.19	0.20	***	***	***
2010	0.17	0.20	0.21	***	***	***
2011	0.17	0.21	0.21	***	***	***
2012	0.14	0.18	0.20	***	***	***
2013	0.13	0.17	0.18	***	***	***
Medium						
2008	0.19	0.20	0.21	***	***	**
2009	0.18	0.20	0.21	***	***	***
2010	0.19	0.21	0.22	***	***	***
2011	0.19	0.21	0.22	***	***	***
2012	0.17	0.19	0.20	***	***	***
2013	0.16	0.18	0.19	***	***	***
Wide						
2008	0.20	0.22	0.24	***	***	***
2009	0.19	0.22	0.25	***	***	***
2010	0.20	0.23	0.26	***	***	***
2011	0.20	0.23	0.25	***	***	***
2012	0.19	0.21	0.23	***	***	***
2013	0.17	0.20	0.23	***	***	***

[a] An exit occurs if a job appears in RED at the beginning of the transition, but has left RED by the end of the transition The pre- and post-treatment periods are aggregated. These values are higher than the exit rates in appendix H, because the appendix H values record the first movement out of the Treatment group, and, therefore, if a job experiences a pay cut or pay rise before exiting RED, it will not count as having left RED. [b] *** denotes significant at 1 per cent, ** denotes significant at 5 per cent, * denotes significant at 10 per cent.

Source: Productivity Commission estimates based on RED.