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Is Declining Union Membership Contributing to Low Wages Growth?

James Bishop and Iris Chan

RESERVE BANK
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Economic Research Department
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Abstract

The union membership rate has declined steadily in Australia since the 1950s. Some have suggested that this decline has caused a fall in the bargaining power of workers, which in turn has contributed to low wages growth in recent years. We test this hypothesis using a newly available source of micro data, covering all enterprise agreements federally registered between 1991 and 2017. We find that changing unionisation patterns are unlikely to account for much of the recent low wages growth. This conclusion reflects three key results. First, there has been no decline in the share of employees covered by enterprise agreements negotiated with union involvement even as union membership has declined. Second, the 'union wage growth premium' in the private sector has been stable over time. Third, spillover effects from union involvement in enterprise agreement negotiations onto wage outcomes in other enterprise agreements exist, but have not changed materially over time.

JEL Classification Numbers: E24, J31, J51, J52

Keywords: wages, trade unions, collective bargaining, wage differentials

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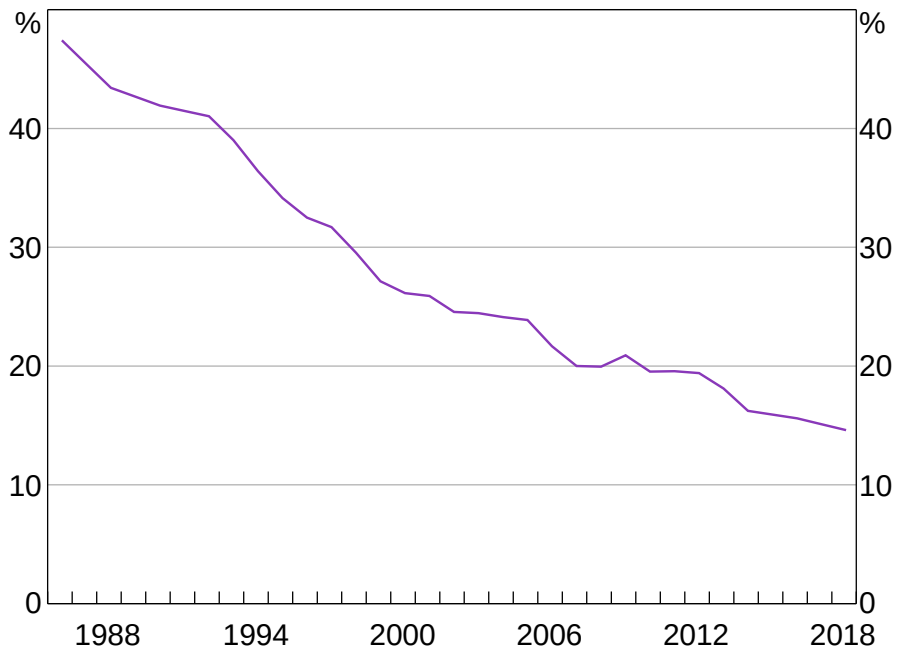
1. Introduction

The union membership rate in Australia has declined steadily since the middle of the 20th century.[1] As at 2018, around 15 per cent of wage earners were members of a union (Figure 1). Some have suggested that declining union membership has led to a fall in the bargaining power of workers, which in turn has contributed to low wages growth over recent years (for instance, Isaac (2018) and Leigh (2018)).[2] This argument is typically based on academic research that finds an hourly wage premium associated with trade union membership, even after taking account of differences in workers' skills, industry and location.

Figure 1: Trade Union Membership Rate

Share of all employees



Note: Excluding owner managers of incorporated enterprises

Sources: ABS; Authors' calculations

While focusing on the union membership rate makes sense in countries where there is a tight link between union membership and union involvement in wage bargaining (such as the United States), it makes less sense in Australia.[3] This is because a growing share of Australian employees choose not to be union members but continue to be covered by a union-negotiated enterprise agreement. Focusing on union membership – as much of the literature does – can therefore give a misleading impression about the contribution of unionisation to aggregate wages growth in Australia. We argue that a more appropriate measure of union influence on wages is the number of workers covered by a union-negotiated enterprise agreement, regardless of membership status. Similarly, the causal

1 The union membership rate peaked in 1948 (Bowden 2011). See Productivity Commission (2015, pp 105–106) for a brief discussion of the factors driving the more recent decline in the membership rate.

2 See Haldane (2017) for a discussion of the case in the United Kingdom.

3 Union membership is a good proxy for union coverage in the United States since once organised, most employees in a firm tend to join the union (Bryson 2007).

impact of unions on wage outcomes should be measured through their involvement at the enterprise agreement level instead of through individuals' union membership status.

To our knowledge, this is the first paper for Australia to estimate a union premium that both accounts for union involvement at the agreement level and unobserved firm heterogeneity. It is also the first time that administrative micro data on collective workplace agreements with union status information have been used to produce estimates of union wage effects for Australia.

We present three key findings based on a census of federally registered enterprise agreements between 1991 and 2017. First, despite declining union membership rates, the share of the workforce covered by enterprise agreements negotiated with union involvement has not changed materially over time. Second, the size of the private sector 'union wage growth premium' (the additional wages growth employees receive by having a union involved in wage negotiations with the firm) has remained stable at around ⅓ percentage point per year, despite changes to the industrial relations framework over our sample period. This premium is estimated using changes in union involvement in collective bargaining for the same firm over time. Our estimate is robust to a range of methodological choices and an alternative identification strategy using an exogenous change to union involvement. Third, we find little evidence that the spillover effects of union involvement onto wages in other enterprise agreements have changed in recent years.

Based on these results, we conclude that trends in unionisation rates are unlikely to have contributed materially to the decline in wages growth in recent years. It is important to note that this conclusion is limited to only the most direct channel in which unions influence wages in Australia – taking part in enterprise-level bargaining – and does not account for other aspects of influence. For instance, declining membership may have affected unions' ability to influence other wage outcomes in the economy, or diverted limited resources away from non-wage matters. These are avenues for further research.

2. What Can Previous Research Tell Us?

The literature on the link between union membership and wages is vast. This research generally finds that employees who are members of a union tend to earn more than those who are not.[4] However, to our knowledge the existing research has not directly examined the question of whether declining union membership has contributed to the slow wages growth in advanced economies in recent years; this is one of our paper's main contributions.[5] The closest related studies are those that examine the impact of unions on wage flexibility, the labour share of income, and inequality. The findings of these studies are mixed. Lower union membership has been found to be associated with lower downward nominal and real wage rigidity (Dickens *et al* 2007; Holden and Wulfsberg 2008), suggesting that declining unionisation may have reduced barriers to implementing smaller wage increases. On the other hand, Elsby, Hobijn and Şahin (2013) find that the decline in unionisation rates does not explain much of the decline in the labour share of income in the United States. Moreover, despite large changes in union density, the differential in wage levels

4 See Farber *et al* (2018) for a brief review.

5 Papers that have drawn such a link tend to have a narrative focus and rely on the correlation between union density and low wages growth (e.g. Mishel (2012); Shambaugh *et al* (2017, p 6); Bell and Blanchflower (2018, p 13); see Isaac (2018) for an Australian example).

between union and non-union workers has remained essentially unchanged over much of the last century in the United States (Farber *et al* 2018).[6]

Even if a stable wage *level* gap exists between union and non-union workers, as suggested by the literature, it does not necessarily follow that declining unionisation rates would contribute to slower wages *growth*. This could be for several reasons.

First, any observed wage differentials may reflect underlying differences between union and non-union workers rather than any *causal* effect of unions on wages.[7] If, say, union members are more productive than otherwise similar non-union members, the observed correlation between union membership and wages may overstate the causal effect of unions.[8] One approach to dealing with non-random selection has been to compare the wage rates for the same individual who switches between jobs, one of which is unionised and the other which is not (for instance, Lemieux (1998)). Other studies exploit discontinuities in unions' ability to bargain to identify a causal effect. A prominent example is DiNardo and Lee (2004), who use a regression discontinuity design around the threshold at which a unionisation vote is barely lost or won to conclude that unionisation has little effect on wages, business survival, employment, output and productivity in the United States.

The second important insight from the literature is that the size of this wage level premium depends crucially on the unit of observation of the data used to estimate it. Most studies examine employee-level survey data, rather than firm-level data as we do in this paper. This can have implications for the results since union membership is not necessarily the same as union coverage in wage setting – a point that we will return to later. In countries where union membership is considerably different from union coverage, estimates of the premium can differ substantially depending on whether employee-, firm- or industry-level data is used (Koevoets 2007; Fitzenberger, Kohn and Lembcke 2013).[9]

A third issue, which is addressed less frequently in the literature, is how quickly union wage effects emerge after workers at a firm first unionise. Most studies produce point-in-time estimates for a given cross-section of firms or employees. It is unclear from these studies whether the observed differences in wage levels between union and non-union members have emerged gradually over time through a slow accumulation of differences in annual wages growth rates, or whether they result from a 'first contract effect' – as Freeman and Kleiner (1990) argue – in which the immediate

6 Some studies have found a slight decline in the US union wage gap in recent decades (Blanchflower and Bryson 2004; Hirsch 2010).

7 Other factors that could account for the correlation between union membership and wages include the low mobility between union and non-union jobs, and mismeasurement of union status (Freeman 1984; Card 1996; Hirsch 2004).

8 There are similar omitted variable bias problems in firm-level studies: for example, unions may be more likely to organise at profitable firms that are more likely to grow and pay higher wages.

9 Even where there is a close link between union membership and union coverage, as in the United States, firm-level data may still be more appropriate for estimating the direct effects of a workplace becoming unionised (e.g. Freeman and Kleiner 1990). For instance, employee-level studies may find different union wage premiums than firm-level studies if the wage effects of a worker moving from the non-union to the union sector differs from any additional amount that a firm must offer its workforce when it becomes unionised, if there is unobserved firm-level heterogeneity, or if union status is measured more accurately for firms than for individuals (Freeman 1984; Freeman and Kleiner 1990; Card 1996; LaLonde, Marschke and Troske 1996; DiNardo and Lee 2004). Some studies using employee-level data attempt to limit the bias by including only those covered by union contracts in their sample (e.g. Budd and Na 2000; Booth and Bryan 2004). Conclusions from such studies are necessarily limited to the wage difference between union members and 'free riders' who nonetheless receive some of the union's services, and cannot tell us the wage difference caused by unions.

adjustment to wages on initial unionisation may differ from ongoing effects. This distinction is important for our research question, as we are ultimately interested in how changing union activity might cause the path of *wages growth* to evolve. Several papers distinguish between immediate and lagged impacts of unionisation on wage levels. Freeman and Kleiner (1990) and DiNardo and Lee (2004), for instance, attempt to reconcile estimates of the wage level gap between union and non-union employees at a given point in time (which tend to be large) with estimates of the immediate effect of union entry on wages (which tend to be small). These authors argue that such differences may reflect that point-in-time estimates of the union wage premium capture a combination of short- and long-run effects of union entry on wages (given that some firms have become unionised workplaces more recently than other firms), while studies on union entry identify only the effects from recent entry.[10] This distinction matters if union involvement has a persistent effect on wages growth over time, as we will argue in this paper.

Our study also distinguishes between long- and short-run wage effects as we believe this has important implications for the link between unions and aggregate wages growth. Our approach to doing this is more direct than in other studies in that our outcome variable of interest is the growth rate of wages rather than the level. That is, we examine how wage *increases* that workers receive each year differ by whether a union was involved in the negotiations. We also study whether this 'union wage growth premium' changes over time after a union first becomes involved, by making use of data on sequential wage negotiations for the same group of workers over several decades.

Studies for Australia have focused on the wage level premium and faced similar limitations and challenges to the overseas research discussed above. No Australian study to date has directly examined the link between unions and aggregate wages growth, nor distinguished between the short- and longer-term effects of union involvement in negotiations on wage outcomes. Most studies of the union wage premium for Australia use survey data on individual workers to compare wages of union members with wages of non-members. Most studies before the 1990s found evidence for a union wage premium, despite Australia at the time having a highly centralised system of wage setting in which a large number of workers were covered by the same awards regardless of their union membership. The estimates in these studies ranged from around 5 to 15 per cent (e.g. Christie (1992), Kornfeld (1993); see Miller and Mulvey (1993) for a comprehensive survey of early major studies). By contrast, Miller and Mulvey (1996) find the union wage effect to be negligible once firm size is accounted for.

A limited number of studies have re-examined this differential since Australia's industrial relations system shifted towards enterprise-level bargaining in the early 1990s, along with the ban on compulsory unionism. These studies again tend to use survey data on employees and rely on variation in union membership status. Most use individual-level panel data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey to find a small to negative union wage premium.[11] Cai and Liu (2008) find a union wage effect for men but not women, and a larger effect at the lower end of the wage distribution. Studies have also found that estimates of the union wage

10 The distinctions between immediate and longer-run union effects have also been found in non-wage outcomes. Lee and Mas (2012) is one such example. Using a regression discontinuity design on US data, the authors find almost no immediate impact from union entry on a firm's equity value, and that the full effect takes 15–18 months to appear.

11 Unlike these studies, Waddoups (2005) uses repeated cross-sectional data between 1993 and 2001 to conclude that declining union density and industrial relations reforms had widened the union wage differential in highly unionised industries.

premium for Australia are smaller once the potential endogeneity of union membership is taken into account. For instance, after accounting for workers' unobserved heterogeneity, Cai and Waddoups (2011) find that the union wage premium falls from close to 9 per cent to 5 per cent for men and from 4 per cent to 2 per cent for women, while Nahm, Dobbie and MacMillan (2017) find that union wage effects may be negative in Australia using a model with endogenous union membership.

A major limitation of these studies for Australia is their reliance on an individual's union membership status rather than union involvement in how that individual's wages are set. In Australia, many non-union members are affected by union involvement in collective bargaining to the extent that they are covered by a union-negotiated enterprise agreement. The studies discussed above effectively ignore that the bargaining unit is at the enterprise level, and that union membership of individuals may not have any effect on negotiations except to the extent that union involvement becomes more likely as the unionised share of a firm's workforce increases. Focusing on union membership, as the Australian literature overwhelmingly does, instead of the relevant bargaining unit can therefore give a misleading impression about the contribution of unionisation to aggregate wages growth in Australia. It can, for instance, lead to an attenuation bias in the estimated size of the union wage effect and to incorrect conclusions about the share of the workforce receiving a wage premium.

An exception is Wooden (2001), who argues that union wage effects in Australia likely stem from differences across workplaces rather than from the workers within them, especially when enterprise-level bargaining is pervasive. Using cross-sectional matched employer–employee data, he finds very small within-workplace union wage effects but a considerable effect (15–17 per cent) across different workplaces.[12] This estimate is much larger than those from studies relying on union membership status. Our paper also takes firms (or more precisely, families of collective agreements) as the relevant unit of observation for studying the effect of unions on wage outcomes.

No Australian study to date has made use of actual union involvement in wage negotiations. Wooden (2001), for instance, proxies for union involvement by using firm-level unionisation rates and a measure of whether the union with most members at each firm is active. Moreover, none of the studies control for unobserved firm-level heterogeneity. This is crucial for an unbiased estimate in situations where *inter*-firm variation is the main source of variation in identifying the union wage premium.

A union wage differential estimated using a representative panel of firm-level agreements with information about union involvement in negotiations is able overcome these issues; this is one of the main contributions of our paper. Using a census of federally registered enterprise agreements, we are able to construct a panel of sequentially negotiated agreements at the firm level where the same set of employees are covered in each linked agreement, along with information on wages growth outcomes and a direct indicator of union involvement for each agreement.[13]

12 The absence of a wage premium from union membership once workplace-level differences are accounted for is consistent with evidence from the United Kingdom (Booth and Bryan 2004; Koevoets 2007).

13 Since any within-firm union membership premium is small (Wooden 2001), and since the relevant bargaining unit is the firm, the lack of employee data should not present any major issues for our estimates.

The second contribution of our paper is to provide the first estimate of the share of Australia's workforce that is covered by an enterprise agreement negotiated with union involvement, and to consider whether this has changed over time.

Finally, our paper estimates the union wage *growth* premium – and how it has changed over time – for Australia using our enterprise-level dataset. This approach conveniently allows us to consider more directly whether unions are contributing to the low wages growth of recent years. Our main approach to estimating this premium is to track wages growth outcomes of firms over time as they transition from having union involvement in bargaining to having no union involvement, or vice versa. We are also able to use a series of legislative changes to provide useful exogenous variation for causal identification. Our results provide some insight into how seemingly negligible immediate effects from union entry could, over time, become the much larger union wage (level) premium seen elsewhere in the literature.

3. Has There Been a Decline in Union Involvement in Wage Setting?

There are currently three methods of setting wages in Australia: awards, collective/enterprise agreements, and individual arrangements. It is possible for unions to influence the wage outcomes of employees covered by any of these methods, albeit to different degrees. The most direct channel of union influence is via collective bargaining. As Figure 2 illustrates, the share of workers covered by collective agreements has remained steady at around 40 per cent since 2000, with most of these agreements registered under the federal system.[14]

Collective agreements may be negotiated with or without union involvement.[15] Under current legislation, unions are the default bargaining representatives of their members in enterprise-level wage negotiations provided that at least one of their members would be covered by the agreement (Stewart 2015, p 148). For most practical purposes, this means a union can get a seat at the bargaining table even if few employees to be covered by the agreement are a member of the union. A corollary is that an agreement negotiated with union involvement will cover both members and non-members alike, since a union cannot legally compel employees to become members.[16]

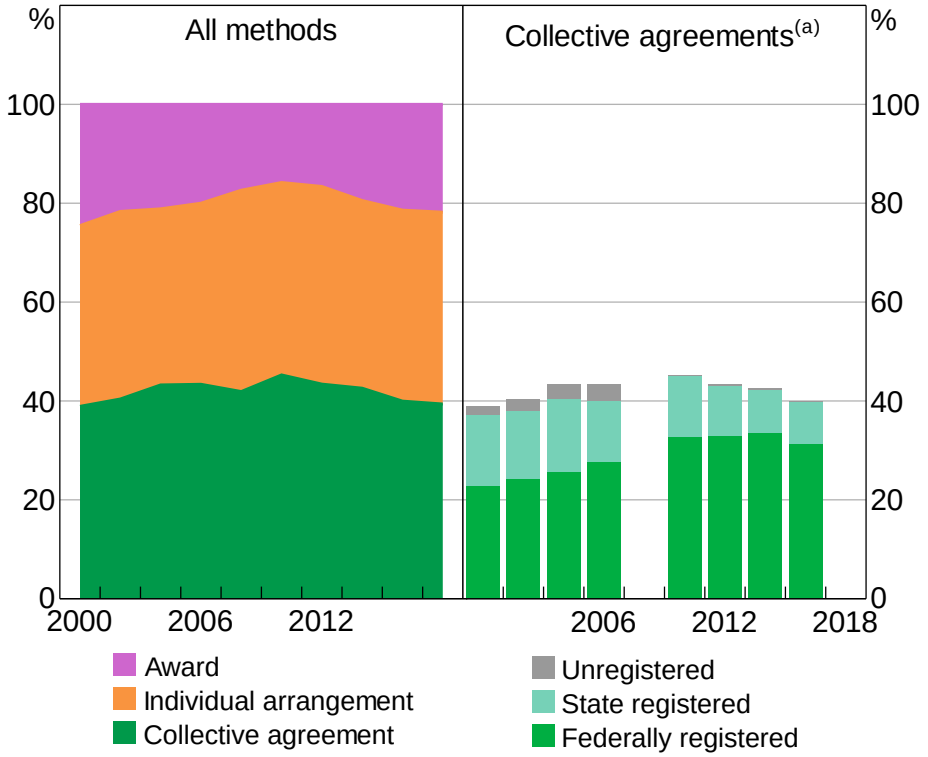
14 See Chapter 2 of Stewart (2015) for a discussion of employment laws in the state and federal systems.

15 In many other countries, such as the United States, collective agreements are synonymous with union involvement in wage bargaining. This is not the case in Australia.

16 See the Fair Work Ombudsman at <https://www.fairwork.gov.au/employee-entitlements/industrial-action-and-union-membership/union-membership>.

Figure 2: Methods of Setting Pay

Share of all employees



Notes: Excluding owner managers of incorporated enterprises
(a) Data are unavailable for 2008 and not yet available for 2018

Sources: ABS; Authors' calculations; Department of Jobs and Small Business; RBA

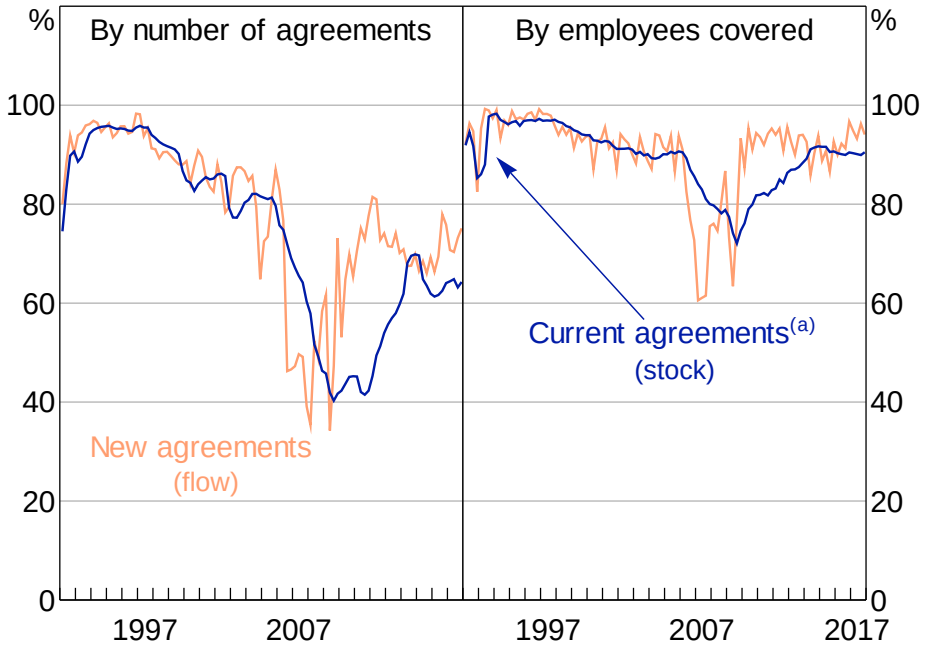
These features of collective bargaining in Australia have made it possible for the share of employees covered by union-negotiated agreements to remain little changed since the mid 1990s despite a large decline in the union membership rate. Within federally registered agreements, those agreements negotiated with union involvement make up around two-thirds of agreements (left panel of Figure 3). Union involvement is even higher, at around 90 per cent, by number of employees covered, since union involvement is more common in agreements negotiated with large firms than small firms (right panel of Figure 3). Most importantly, the share of employees on enterprise agreements covered by a union-negotiated agreement remains at around the same level as in the late 1990s.[17] Together with the unchanged share of workers covered by collective agreements (Figure 2), this implies that declining union membership has not translated into reduced union involvement in wage setting at the workplace level.[18] If anything, the share of the workforce on a union-negotiated agreement has *risen* in recent years. This is one of our key findings.

17 The largest shifts in Figure 3 have coincided with changes in the workplace relations system, since this can affect unions' scope to bargain. For instance, the share of agreements negotiated with union involvement declined under the 'Work Choices' period (2006–09) but has subsequently returned to its previous levels under the *Fair Work Act 2009*.

18 Our finding that the share of the workforce covered by a union-negotiated agreement has not declined in recent years assumes that the union/non-union split is similar between current agreements (those in Figure 3) and expired collective agreements that continue to operate, between federally and state-registered enterprise agreements, and between registered and unregistered agreements. We need to make this assumption because our agreement-level data do not capture state-certified or unregistered agreements, and do not allow us to precisely observe whether an agreement remains operational or not past its nominal expiry date.

Figure 3: Union Involvement in Federally Registered Enterprise Agreements

Share of enterprise agreements



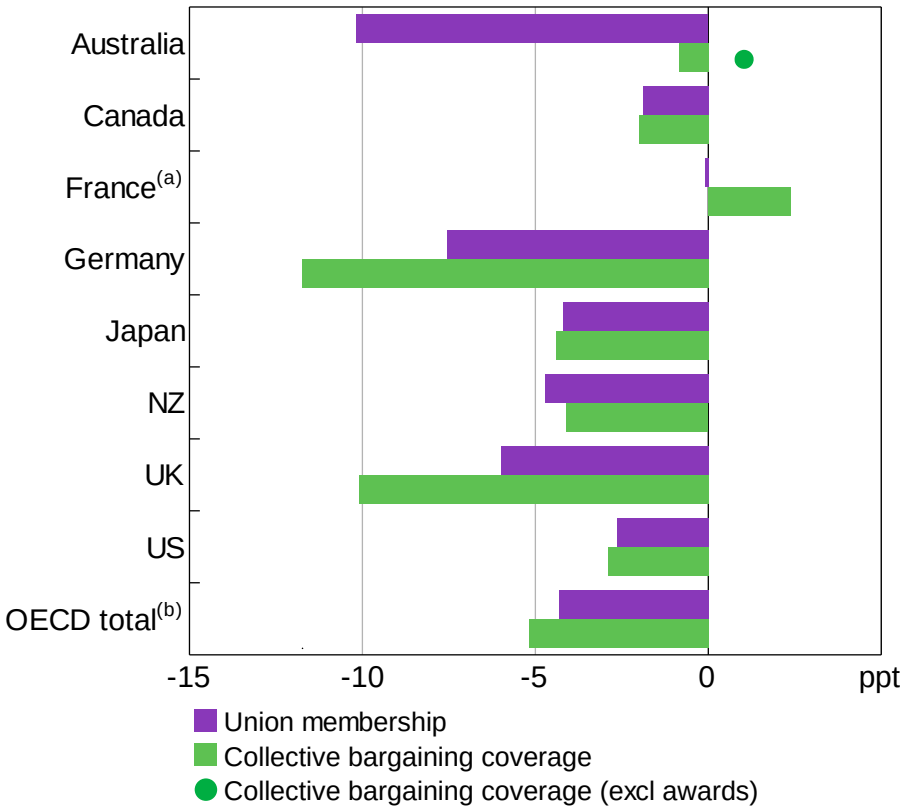
Note: (a) Agreements between their nominal commencement and expiry dates in a given quarter

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

These developments appear relatively unique to Australia. The magnitude of Australia's decline in the union membership rate is large relative to other countries in the Organisation for Economic Co-operation and Development (OECD). Notwithstanding this, Australia has experienced one of the smallest declines in collective bargaining coverage (along with the unchanged share of employees covered by union-negotiated collective agreements) among OECD countries (Figure 4). These developments provide further motivation to consider the Australian case in detail, even though union membership has also declined in many other countries facing similarly low wages growth.

Figure 4: Change in Union Membership and Collective Bargaining Coverage

Change in share of employees from 2000 to 2016



Notes: Collective bargaining coverage is based on an OECD measure which includes coverage by statutory wage regulations (e.g. awards)

(a) Change from 2004 to 2014

(b) Union membership estimated as an employee-weighted average across OECD countries with available data

Sources: ABS; Authors' calculations; OECD

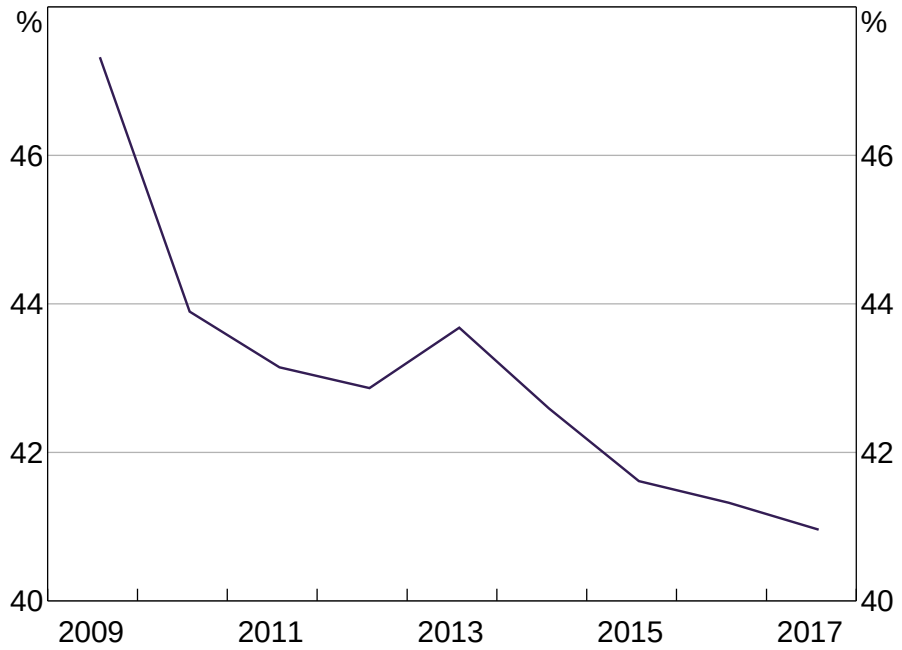
That the share of the Australian workforce covered by a union-negotiated agreement has remained little changed at the same time as the union membership rate has declined suggests that an increasing share of employees in Australia find it optimal to 'free ride' on the union membership of other employees at the firm.[19] An increase in free riding is also evident in individual-level data from the HILDA Survey, which suggests that only 41 per cent of employees on collective agreements were members of a union in 2017, down from around 47 per cent in 2009 (Figure 5).[20]

19 Peetz and Yu (2017) draw a similar conclusion after observing that collective bargaining coverage has declined by far less than union membership over the past decade. See Booth (1985) and Booth and Chatterji (1995) for examples of the free-rider problem and union membership.

20 Since HILDA does not distinguish whether individuals are covered by 'union' or 'non-union' enterprise agreements, the denominator used in Figure 5 is all enterprise agreements. Including non-union agreements in the denominator means that the union member share may understate the extent of free riding. However, this is unlikely to be an issue given that the vast majority of employees on enterprise agreements are covered by a union agreement. Using unpublished data from the Australian Bureau of Statistics, Peetz and Preston (2009) also found evidence of widespread free riding on union agreements.

Figure 5: Employees on Enterprise Agreements Who Are Union Members

Share of employees on enterprise agreements



Notes: Excludes employees of own businesses; includes both union and non-union enterprise agreements; population-weighted (excluding 2011 top-up sample)

Sources: Authors' calculations; HILDA Survey Release 17.0

Although the focus of our paper is collective agreements, collective bargaining is not the only means through which unions can influence wages. Unions can also influence award wages, albeit indirectly. Each year, unions propose a particular award wage outcome in their submissions to the Fair Work Commission's award wage decision. These Fair Work Commission decisions affect the wages of all employees on modern awards, again regardless of union membership status (17 per cent of employees on awards were union members in 2017 according to the HILDA Survey).[21] The influence of unions on these decisions is difficult to quantify, and is beyond the scope of this paper. Finally, in contrast to collective bargaining and awards, unions have minimal involvement in bargaining over individual agreements: less than 5 per cent of employees on individual agreements are members of a union. The impact of unions on these workers is limited mainly to indirect spillover effects, if any. Given the focus of our paper is on collective bargaining, any general conclusions we draw about aggregate wages growth will be valid to the extent that any effect of changes in union membership on the wages in awards and individual agreements has remained constant over time.

4. Is There a 'Union Wage Growth Premium' and Has This Shrunk over Time?

That the share of the workforce covered by enterprise agreements negotiated with union involvement has not changed materially over time suggests that the declining union membership rate has played only a limited role in the recent slowing in wages growth. Lower membership rates may nevertheless put downward pressure on wages growth insofar as it reduces the ability of unions

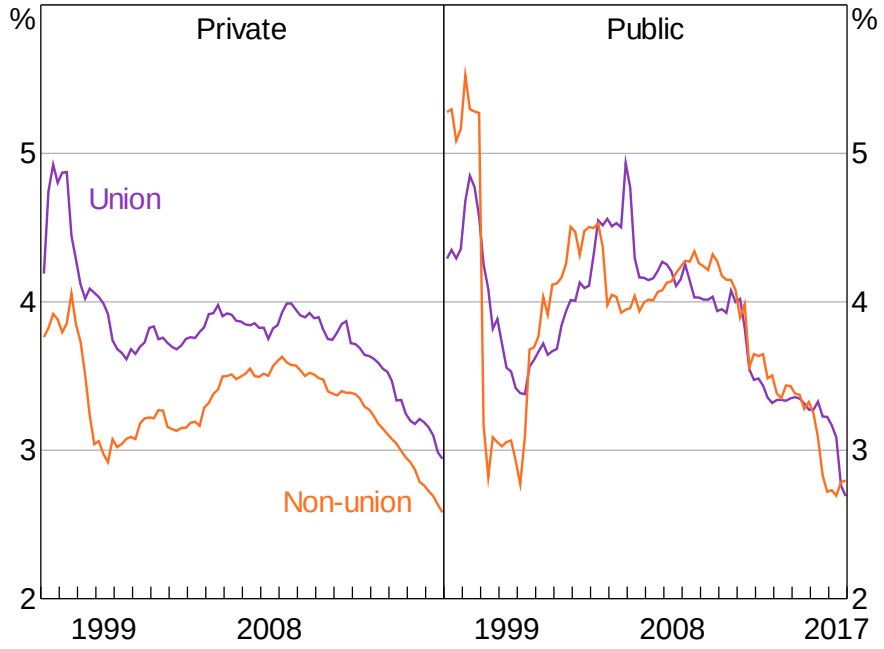
21 These union member shares among employees on awards and those on individual agreements (discussed below) were calculated from the HILDA Survey without the 2011 top-up sample for consistency with Figure 5. Including the top-up sample, 16 per cent of employees on awards, and 6 per cent of those on individual agreements, are union members.

to extract more favourable wage outcomes from firms. For example, fewer paying members may lead to fewer resources allocated to wage negotiations at the firm level. As such, this section of our paper examines whether the ability of unions to obtain higher wage outcomes changed over recent years.

We can examine this question empirically by looking at whether a union wage growth premium exists and, if so, whether this premium has declined over time.[22] Private sector enterprise agreements negotiated with union involvement tend to have higher wage increases than those negotiated without union involvement, while there is little evidence of a similar differential in the public sector (Figure 6).[23] This unconditional difference in wages growth in the private sector appears to have been relatively stable over recent years. Of course, this 'raw' difference could be due to inherent differences between union and non-union agreements, rather than a *causal* effect of union involvement per se. For instance, workplaces that are more profitable on average may also be more likely to have unions involved in their wage negotiations; these workplaces may offer workers larger wage increases even in the absence of union involvement. Our analysis in the following sections attempts to abstract from these inherent differences and, instead, estimate the causal effect of unions on wages growth in collective agreements.

Figure 6: Average Annualised Wage Increases in Federally Registered Enterprise Agreements

Current agreements



Notes: Agreements in force at quarter-end; weighted by number of employees

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

22 In this paper, any premium associated with union representation would be a 'pure' union premium within the subset of employees covered by collective agreements, over and above any (positive or negative) premium associated with collective bargaining. One limitation of our analysis is that we are unable to make more general conclusions about whether the existence and magnitude of this union premium has had wider effects on the likelihood of employers and employees choosing to set pay using enterprise agreements rather than awards or individual arrangements.

23 The wage growth data reported in Figure 6 – average annualised wage increases – are described in detail in Section 4.1.2.

4.1 Data

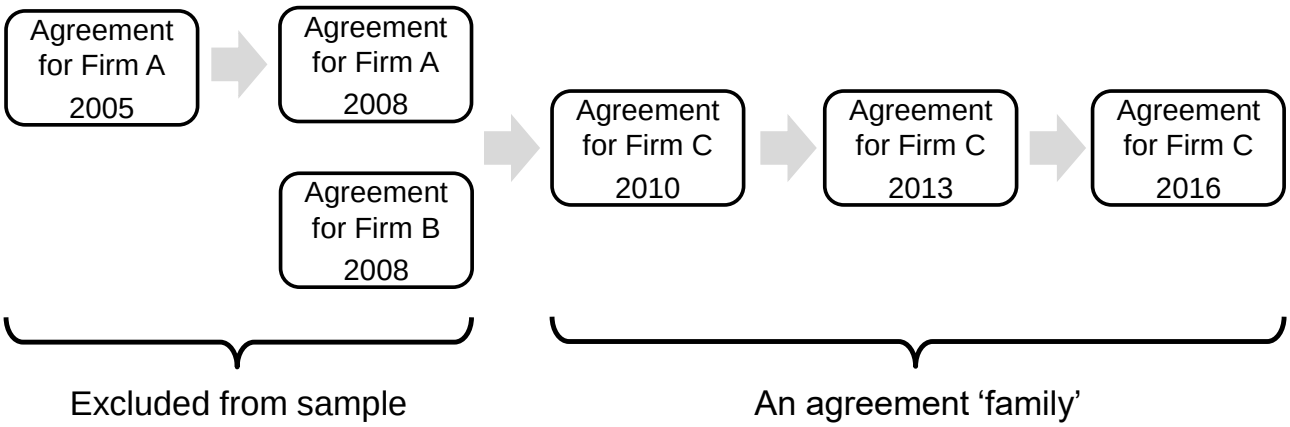
To estimate the union wage growth premium we use data from the Workplace Agreements Database (WAD), which is manually compiled by the Department of Jobs and Small Business from administrative data. This rich database includes information on every federally registered enterprise agreement since the early 1990s (subsequently referred to as 'enterprise agreements' or 'agreements'). There are more than 150,000 agreements in the WAD, with around 8,000 new agreements added every year. Data are available on the size and timing of wage increases, the number of employees covered, the industry and state of the firm, and the unions that were involved in wage negotiations (if any).

Given the nature of the WAD, our unit of analysis is the agreement. We also make the case that this is the most appropriate level of analysis for studying union wage effects in Australia. In most cases an agreement will correspond to a single firm, since the vast majority of agreements cover only one employer. But in some cases a single agreement may cover more than one firm (a 'multi-enterprise agreement'), or there may be multiple agreements covering different groups of workers within a single firm. For the remainder of our paper, we use the terms 'firm' and 'agreement' interchangeably.

4.1.1 Constructing agreement families

The WAD allows us to follow 'families' of agreements over time: that is, sequentially negotiated agreements that cover the same group of workers or positions, usually at a single firm. This means that our data have a panel dimension that enables us to control for any time-invariant characteristics of the firm, such as workplace culture. Our process for constructing agreement families in the WAD can be described using the stylised example in Figure 7.[24]

Figure 7: Stylised Example of Constructing an Agreement Family



Our first step is to identify the most recent agreement in any given family of agreements (e.g. the agreement for a firm in 2016). We then look at the most recent agreement that the 2016 agreement replaced (e.g. the agreement for the same firm in 2013) and check whether it covered exactly the same group of workers. If so, those two agreements are in the same family. We then check if the agreement that the 2013 agreement replaced (e.g. the agreement for the same firm in 2010) also

24 The WAD provides information on whether an agreement replaces a previous agreement (including its identifier), and whether the group of workers covered by the agreement is the same as the previous agreement.

covered exactly the same group of workers. If so, all three of these agreements are in the same family.

At some point we may find that an agreement covered a different group of workers to its predecessors. In the stylised example in Figure 7, the 2010 agreement replaces two different agreements. For example, Firm A might have merged with Firm B in 2010 and a new agreement was created that covered all employees in the combined Firm C. Because the 2010 agreement covered a different group of workers to earlier agreements, we do not include the 2008 agreements – nor any of the agreements they themselves replaced – in our matched sample. Only the 2010, 2013 and 2016 agreements will constitute a 'family' and be included in our matched sample.

Our approach means that more recent agreements are systematically more likely to be included in our matched sample. However, we are still capturing around 85 per cent of all agreements that we can possibly capture in our matched panel.

4.1.2 Measuring wage outcomes

Our measure of the wage outcome from negotiations is the average annualised wage increase (AAWI) over the life of the agreement. The AAWI captures any changes in base pay but not allowances or bonuses paid separately to the base wage. We use the following formula to calculate the AAWI of an agreement:

$$AAWI_t = \left[\left(\prod_{t=1}^{N} \left(1 + w_t\right) \right)^{\frac{1}{d}} - 1 \right] \times 100$$

where w_t is the percentage wage increase at time t, N is the number of increases over the life of the agreement, and d is the effective duration of the agreement in years.[25] Effective duration is defined as:

$$effective\,duration = \underbrace{\max\left(expidate, lastincr\right)}_{\text{latest end date}} - \underbrace{\min\left(certdate, commdate,\ firsincr\right)}_{\text{earliest start date}}$$

where $expidate$ is an agreement's nominal expiry date, $lastincr$ is the date of the final wage increase in the agreement, $certdate$ is the agreement's certification date, $commdate$ is its formal commencement date, and $firsincr$ is the date of the first wage increase in the agreement. This calculation of the effective duration recognises that certification dates often do not align with the first wage rise (certification sometimes happen after the first wage rise), and that contracts often overlap each other so that the first pay rise of a later contract is granted before the last pay rise of the earlier agreement.[26]

25 This method of averaging implicitly assumes that the increases are evenly spaced across the agreement's effective duration.

26 In the vast majority of cases, an agreement's nominal expiry date comes after the date of its final wage increase.

The AAWI calculation is similar to that used in Department of Jobs and Small Business (2018), with a small modification to ensure that the measure is appropriate for modelling. Unlike in the measure used by the Department, we do not use the actual termination date for an agreement as its end date if the agreement is terminated before its nominal expiry date. This is because our research question naturally focuses on the *negotiation* phase for each agreement: we want to include only information that was available to bargaining participants at the time an agreement was signed and not what happened subsequently. In Section 4.4.2, we consider whether our results are robust to using some alternative measures of wages growth that are adjusted for renegotiation delays.

The WAD does not include information on the wage *level* in each agreement. As such, we cannot directly estimate the union wage level premium commonly seen in the literature. We discuss the implications of this in Section 4.6.

4.1.3 Sample for estimation

Our baseline models are estimated using a sample of around 46,000 agreements, which is only a subset of the 150,000 or so agreements in the WAD. There are two reasons for this. First, some of the agreements in the dataset do not have a measure of AAWI that was quantifiable at the date the agreement was made. For example, we cannot construct an AAWI in cases where wages are indexed or linked to the consumer price index, to Fair Work Commission award decisions, or to the firm's performance.[27]

Second, in many cases we cannot match an agreement to another in the same agreement family.[28] We exclude these agreements as our baseline estimates of the union wage growth premium are necessarily based only on cases where the same group of workers switch from negotiating with union involvement to negotiating without unions (and vice versa). In Section 4.4.5, we examine if this creates a selection bias by comparing our baseline estimates (with fixed effects) to a simpler model (without fixed effects) estimated using the full sample of all agreements with a quantifiable AAWI.

Descriptive statistics on the estimation sample can be found in Tables B1 and B2.

4.2 Empirical Approach

We use the following model to estimate the effects of union involvement in enterprise bargaining on wages growth:

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27 Another common reason why an agreement's AAWI is not quantifiable is that it covers several groups of workers and each group receives a different percentage wage rise. More information about non-quantifiable wage increases in the WAD can be found in Department of Employment (2016).

28 There are several reasons why an agreement may not be matched. First, many agreements are never replaced (or are yet to be replaced). For example, a new business may start up, sign an agreement, and then cease operating prior to the agreement's expiration date. This is common in the construction industry. Second, there are also cases where the Department of Jobs and Small Business did not routinely check whether a new agreement replaced an existing agreement in its database. For example, prior to 2011 the Department did not check 'template or pattern' agreements to see if they replaced another agreement, which represented around 30 per cent of all agreements at the time. Finally, an agreement may not be matched due to our specific procedure for constructing the matched sample.

$$AAWI_{ijst} = \phi Union_{ijst} + X'_{ijst}\beta + \theta_i + \theta_{jst} + \varepsilon_{ijst} \quad (1)$$

where the dependent variable is the AAWI for firm i in industry j and state s in the quarter the agreement started, t.[29] The variable of interest ($Union_{ijst}$) is a dummy variable that equals one if a union was involved in bargaining, and zero otherwise. X_{ijst} is a vector of controls that vary by firm, industry, state and time.[30] The fixed effects (θ_i) control for any permanent differences in wages growth across firms. The model also controls for the effects of the economic cycle and industry- and location-specific shocks by including the three-way interaction between industry, state and time period (θ_{jst}). This absorbs the effects of any macro shocks that affect all firms in any given quarter, along with any time-varying shocks to specific industries or states (or industry–state combinations).

The coefficient of interest (ϕ) measures the union wage growth premium (if positive) or penalty (if negative). The inclusion of fixed effects in Equation (1) means that the union premium will only be identified by variation within firms over time. We will only be able to uncover a positive union wage growth premium if, on average, the size of wage increases shrink when an agreement negotiated with union involvement is replaced by one negotiated without union involvement, or vice versa. Our baseline estimates are not weighted by the number of employees covered by each agreement. We discuss some alternative weighting strategies in Section 4.4.1. Since unions in Australia are typically organised along industry lines and engage with multiple firms, we adjust the standard errors for two-way clustering at the firm and two-digit industry levels.

4.2.1 Potential omitted variable bias

For this approach to yield a causal estimate of the union premium, it is crucial that union involvement is 'as good as randomly assigned' given our controls. One concern is that, despite our extensive controls, union involvement could still be endogenous. An agreement negotiated without union involvement could be replaced by one negotiated with union involvement (and vice versa) for unobserved reasons that are also correlated with wages growth. The sign of any bias is unclear *ex ante*. For example, our estimates will be upwardly biased if employees are more likely to start involving a union in negotiations if they believe that they would receive a favourable outcome from doing so. On the other hand, our estimates will be downwardly biased if employees ask a union to step in to help offset the effect of a sudden reduction in their bargaining power in negotiations.

We address this concern by using a different identification strategy – one that relies on an exogenous change in union involvement introduced by a change in legislation – to estimate the premium. As will be discussed in Section 4.7, we find that the estimated premium using the alternative strategy is larger than the analogous estimate based on Equation (1), but has not changed materially over time.

4.2.2 Transition probabilities

It is also important to consider whether the within-firm variation is limited to only a few firms or confined to a specific period. Among agreement families in our estimation sample that negotiated a

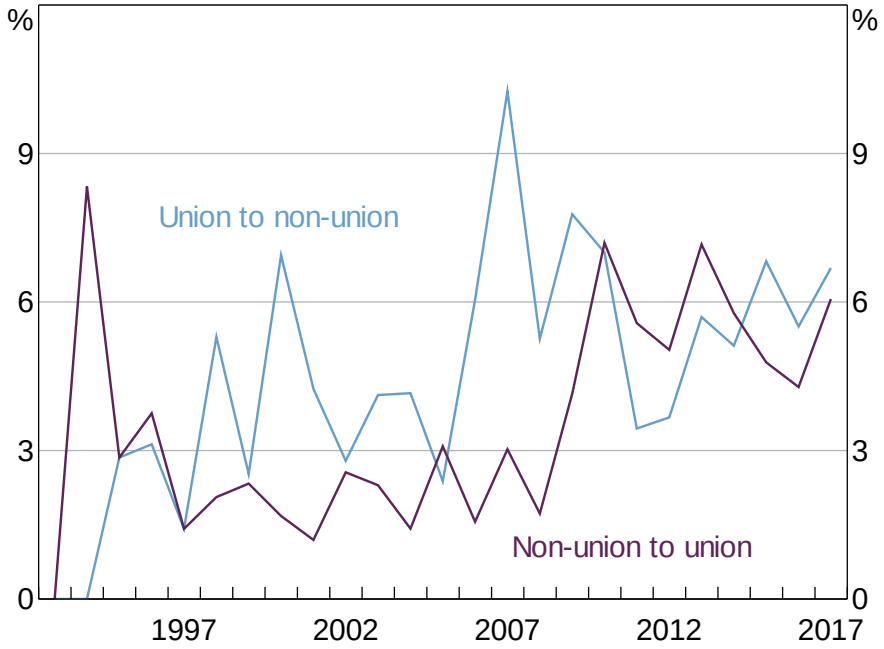
29 Industry is at the one-digit level under the 1993 or 2006 Australian and New Zealand Standard Industrial Classification (ANZSIC).

30 X_{ijst} includes the log of the number of employees covered by the agreement and a dummy for multi-enterprise agreements.

new agreement in a given year, the share of agreement families that switched from having union involvement to having no union involvement averaged 3.7 per cent per year in the period leading up to the Work Choices legislative regime (Figure 8). This transition probability rose sharply under Work Choices (2006–09), but declined after Work Choices was repealed and replaced by the *Fair Work Act 2009*. The share of agreements that switched in the other direction (i.e. no union to union) has averaged 4 per cent since the early 1990s, but has been at above average levels under the *Fair Work Act 2009*.[31]

Figure 8: Transitions in Union Involvement

Share of new enterprise agreements in a given year, private sector



Note: Agreements that have at least one other family member

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.3 Baseline Results

Our baseline estimates are shown in Table 1. We present results separately for the private and public sectors. For both sectors, we show results for three specifications, each based on Equation (1): specification (1) does not include the fixed effects or the three-way interaction between industry, state and time; specification (2) omits the fixed effects only; specification (3) is the full model. Comparing the results across the three specifications helps to isolate which controls are important.[32]

With a limited set of controls in the model, union involvement has a strong association with private sector wages growth (Table 1, column 1). This is in line with the persistent unconditional gap in wages growth between union and non-union agreements shown in Figure 6. As column 2 shows, only a small part of the unconditional difference between union and non-union agreements can be

31 Unfortunately, it is difficult to identify firm-level factors that may be associated with transitions in our data because there are few time-varying firm-level variables available.

32 In specification (1) we include the interaction between industry and state to account for permanent differences in wages growth across different industry–state combinations (unlike in the other specifications, we restrict these differences to be time-invariant).

accounted for by business cycle factors and idiosyncratic shocks to industries and states. Introducing firm fixed effects – our preferred specification – leads to a further reduction in the size of the union wage growth premium (column 3). The point estimate suggests that annual wages growth in private sector agreements is 0.34 percentage points higher when unions are involved in wage negotiations, relative to when they are not involved. This effect is estimated with a surprisingly small standard error given that we are focusing on variation within firms and within industry–state–time cells. By contrast, we find no evidence of a union wage growth premium in the public sector after accounting for permanent differences between agreement families (Table 1, column 6).[33] Given the absence of evidence for a union wage growth premium in the public sector, the rest of our paper focuses on the private sector union wage growth premium.

Table 1: Regression Results for Equation (1)

Dependent variable: *AAWI*

	Private sector			Public sector		
	(1)	(2)	(3)	(1)	(2)	(3)
Union	0.64***	0.52***	0.34***	–0.03	0.18**	0.04
	(0.15)	(0.11)	(0.09)	(0.12)	(0.07)	(0.04)
Industry–state–time effects?	No	Yes	Yes	No	Yes	Yes
Fixed effects?	No	No	Yes	No	No	Yes
Observations	41,611	41,611	41,611	4,472	4,472	4,472
Adjusted R^2	0.25	0.48	0.60	0.02	0.28	0.36
Adjusted R^2 (within)			0.01			0.00

Notes: ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively; standard errors (in parentheses) are clustered at the agreement-family and two-digit industry levels; all specifications control for industry and state

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.4 Robustness Checks

This section details a number of robustness tests we conduct on our baseline results for private-sector agreements. Readers wishing to continue to the next stage of our analysis may wish to skip to Section 4.5, where we discuss whether the union wage growth premium has changed over time.

The series of robustness tests we conduct, described in Sections 4.4.2–4.4.5 with the results presented in Table 2, often require adding variables that, in many cases, have missing data for some observations. In these cases, we compare the estimates from the robustness model of interest to those from the baseline model estimated over an identical sample. This allows us to abstract from the influence of sample composition between the baseline and robustness samples and isolate the importance of the particular robustness exercise being considered. As such, the 'baseline model'

33 The WAD captures only public sector agencies within the federal workplace relations system; conservative calculations suggest that is at least 37 per cent of all public sector employees. This includes all employees of the Commonwealth, along with public sector employees in Victoria, the ACT and the NT, and some state-owned trading entities. All other state and local government workers in the other states remain covered by state laws. For a discussion on features in public sector bargaining, see Productivity Commission (2015, Ch 24). Our regressions for public sector enterprise agreements replace the three-way industry–state–time interaction with two-way state–time interaction, given that these agreements are highly concentrated in only a handful of industries, such as public administration & safety.

estimate in Table 2 differs for each robustness test, but is based on Equation (1) unless otherwise indicated.

Table 2: Robustness Tests on Equation (1) – Private Sector

Dependent variable: *AAWI*

	Baseline model	Robust model	Common sample size	Notes
Weighting by employee numbers				
Union	0.34***	0.22***	41,611	
	(0.09)	(0.05)		
Accounting for renegotiation delays				
Union	0.28***	0.22***	19,645	Sample excludes first agreement in each family
	(0.08)	(0.06)		
Controlling for firm-specific shocks				
Union	0.14**	0.15**	7,153	Sample excludes agreements with missing firm data
	(0.09)	(0.09)		
Controlling for inertia in wage setting				
Union	0.23***	0.18***	18,795	Arellano-Bond estimator; includes aggregate time effects only
	(0.07)	(0.05)		
Including unmatched agreements in the sample				
Union		0.85***	94,906	Excludes agreement family fixed effects
		(0.12)		
Matched_sample		0.21***		
		(0.04)		
Union × Matched_sample		–0.31***		
		(0.04)		
Selection model				
Union	0.64***	0.65***	45,513	Heckman model; excludes agreement family fixed effects and time-varying industry and state effects
	(0.02)	(0.02)		

Notes: ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively; standard errors (in parentheses) are clustered at the agreement-family and two-digit industry levels

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database; Dun & Bradstreet

4.4.1 Weighting by employee numbers

All agreements are given equal weight in our baseline regressions. An alternative approach is to weight each observation by the number of employees under that agreement. There is little reason to do this on efficiency grounds – our unweighted OLS estimates with robust standard errors are already precise. Rather, weighting may, under some assumptions, get us closer to the population average causal effect of union involvement in the presence of unmodelled heterogeneity of effects (Solon, Haider and Wooldridge 2015). For example, if union involvement tends to have larger effects in the construction industry, then weighted least squares (WLS) estimates that place relatively less

weight on smaller agreements – which are more common in construction – will yield a smaller overall premium than OLS. In that case, it may seem that weighting should yield the weighted-average effect of union involvement across the population of employees on enterprise agreements (which is the quantity that we are ultimately interested in for our research question). However, as Solon *et al* (2015) argue, this is only the case if certain strong assumptions are satisfied; otherwise WLS and OLS are both inconsistent in the presence of unmodelled heterogeneous effects, and neither identifies the population average effect of union involvement.

We find that WLS estimates of the union wage growth premium are smaller than our baseline OLS estimates (0.22 versus 0.34; Table 2). That the differences between OLS and WLS estimates are not large gives us confidence that our baseline results are not being unduly influenced by misspecification bias due to a failure to model heterogeneous treatment effects (Solon *et al* 2015). We leave the study of heterogeneous effects (e.g. by industry, firm size or union) for future research.

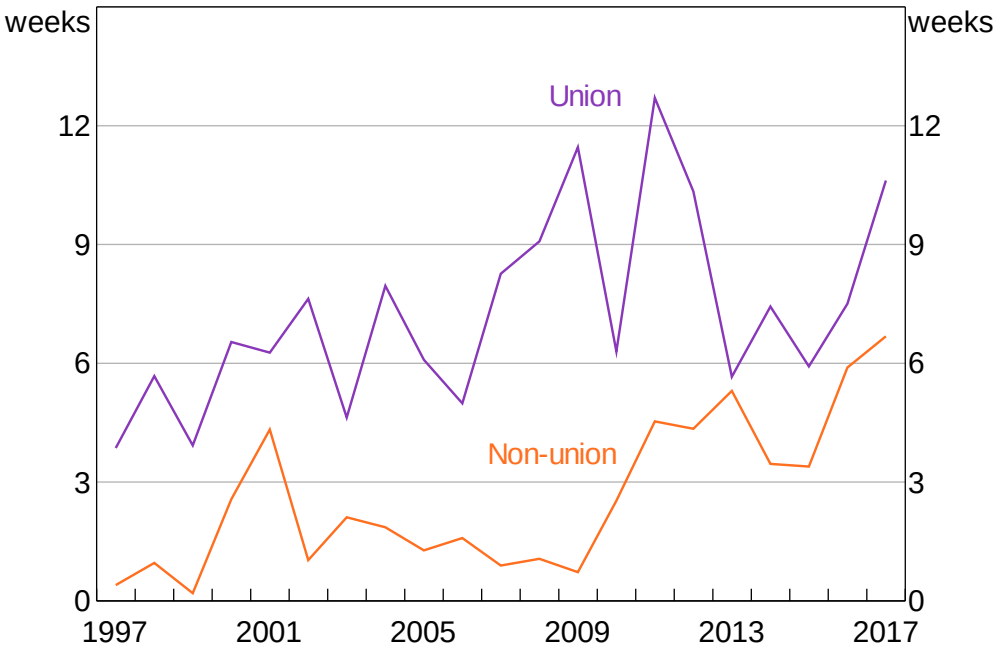
4.4.2 Accounting for renegotiation delays

Our baseline results are based on a measure of wages growth that is similar to the Department of Jobs and Small Business calculation of AAWI. As defined in Section 4.1.2, this measure assumes that the duration of an agreement is the length of time from the start date of the contract to the end date of the contract (the 'contractual duration'). Any length of time between the end of the firm's previous contract and the start of the current contract (the 'renegotiation delay') is ignored. All else being equal, a longer renegotiation delay will reduce wages growth for a group of workers, as wages are often frozen during the negotiation period. Failing to account for these delays could therefore put a bias into our estimates if the length of these delays is related to union involvement in bargaining.

On average, agreements negotiated with union involvement take longer to renegotiate than non-union agreements (Figure 9). After controlling for other factors (using a regression), we find that union involvement in bargaining adds around four weeks to renegotiation delays, compared to when unions are not involved.[34] Since union involvement leads to more protracted wage negotiations on average, our baseline estimates of the union wage growth premium (which do not adjust for these lags) are likely to be biased upwards. However, the size of the bias does not appear to be large: adjusting our measure of AAWI for renegotiation delays yields only a slightly smaller estimate of the union wage growth premium than in the baseline model (0.22 versus 0.28; Table 2).

34 We replaced the dependent variable in our baseline model with a direct measure of the renegotiation delay (in weeks).

Figure 9: Average Renegotiation Delay
By start date of the new agreement



Notes: Weighted by number of employees; excludes agreements that do not replace an existing agreement and those with implied renegotiation delays of more than 18 months

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.4.3 Controlling for firm-specific shocks

Our baseline model controls for firm-specific factors that do not vary over time, along with any industry- or state-specific shocks. It does not control for the effects of firm-specific shocks that vary over time. For example, while the model would control for the effects of an industry-wide decline in profits (e.g. due to a decline in industry export prices), it would not control for, say, a profit shock to a specific firm (e.g. due to the retirement of a CEO). If these idiosyncratic firm-level shocks are correlated with changes in union involvement and also with wages growth, we may have omitted variable bias.

We can partially control for firm-level shocks using measures of firm profitability and sales from Dun & Bradstreet. These data are available only for listed companies and can only be linked to the WAD if a firm's Australian Business Number (ABN) is available in both datasets. Unfortunately, ABNs were only routinely recorded in the WAD since 2009, and so we are unable to observe firm-level profit and sales for many firms prior to this.[35] Firm-level data is missing for around three-quarters of all agreements in our sample, which is the reason we did not include these variables in our baseline model. In a model that controls for firm-specific shocks, the union wage growth premium is estimated to be around 0.15 (Table 2). If we re-estimate our baseline model (excluding firm-level controls) using the sample of agreements with non-missing Dun & Bradstreet data, we find a very

35 We can infer the ABN of some firms in years prior to 2009.

similar estimate as in the regression with firm-level controls (Table 2). This provides some reassurance that our baseline results are not being driven by idiosyncratic shocks to firms.[36]

4.4.4 Controlling for inertia in wage setting

Our baseline model does not control for the wage outcome in the previous agreement from the same agreement family. Including this variable leads to a large reduction in our sample size: we lose observations both due to the lag itself and because deeper lags of wages growth are needed as instruments. However, excluding this variable may result in biased estimates of the union premium if the firm's previous wage outcome is correlated with workers' decision to negotiate with union involvement. And since wages growth is serially correlated due to inertia in wage setting, omitting lagged AAWI could put a bias into our estimate of the union premium.

To examine the importance of this bias, we re-estimate our baseline model including lagged wages growth as an additional control. We used an Arellano and Bond (1991) estimator to obtain a consistent estimator of the coefficient to lagged AAWI. In this specification the union wage growth premium is only slightly smaller than in the baseline model when estimated over the same sample (0.18 versus 0.23; Table 2).

4.4.5 Sample selection bias

Another potential concern around robustness is that the matched sample of agreements (i.e. those with at least one family member) used in our analysis may not be representative of the broader population of agreements in the WAD. For example, some firms do not have sufficient resources to regularly renegotiate agreements, and as a result are less likely to be included in our matched sample. If such firms also respond differently to union involvement in wage negotiations compared with firms that are matched, then our estimates of the union wage growth premium could be biased. We can examine this issue by estimating a pooled model using the full sample of all agreements – both matched and unmatched – but excluding the agreement-family fixed effects. The estimation sample in this model is more than twice the size of that in the baseline model. We also include a dummy variable that equals one if an agreement is in the matched sample and zero otherwise, and an interaction between this dummy and the dummy for union involvement.

The coefficient of interest is the coefficient on the interaction term, which tells us whether the union premium is different across matched and unmatched samples, conditional on the variables included in the baseline model. We find that this interaction is statistically significant (Table 2). Its sign suggests that the union premium in the unmatched sample is larger than the matched sample. This suggests that our baseline estimates of the union premium may be downwardly biased. However, we show in Section 4.5.1 that these differences between the matched and unmatched samples do not affect our conclusions regarding the role of unions in the decline in aggregate wages growth.

Another sample selection issue is that our baseline model is restricted to agreements whose wage outcomes can be expressed in terms of a 'quantifiable' AAWI. This could lead to a selection bias if agreements with a quantifiable AAWI are not a random sample of enterprise agreements (Heckman 1979). For example, workers with less bargaining power may be relatively more willing

36 That the estimated premium is smaller than our baseline estimate in Table 1 (specification (3)) mainly reflects that firms with non-missing Dun & Bradstreet data are not a random sample of enterprise agreements.

to agree to wage outcomes that are linked to the consumer price index or to the Fair Work Commission's award decisions (and hence the AAWI cannot be calculated *ex ante*; see Section 4.1.3 for further details). Union involvement in negotiations may have a different effect for such workers.

We can test for this type of bias using Heckman's two-step procedure to correct for selection bias. In the first stage we model the probability of having a quantifiable AAWI using a probit model. This step requires an 'excluded instrument' – that is, a variable that influences whether an agreement has a quantifiable AAWI but does not have a direct effect on the AAWI itself. We use the share of non-expired agreements in the firm's industry that had quantifiable AAWIs in the previous quarter (weighted by firm size) as the instrument. The idea is that a firm's wage-setting practices will be influenced by wage-setting trends in the firm's industry. In the second stage we correct for self-selection by incorporating a transformation of the predicted probabilities from the first stage as an additional explanatory variable in Equation (1).[37] We find no evidence of a selectivity effect (Table 2). This justifies our focus on the sample of agreements with non-missing AAWI in our baseline model.

4.5 Has the Union Wage Growth Premium Changed over Time?

In this section, we use our baseline model to examine whether the influence of unions on wages growth has changed over time. Factors such as the decline in membership rates and changes in the workplace relations system, for instance, may have affected union influence. Any decline in unions' ability to negotiate favourable wage outcomes should manifest itself in a smaller union premium over time.

We can examine the stability of the premium by interacting the $Union$ indicator variable in Equation (1) with dummies for the four major legislative regimes governing the agreements in our sample. These regimes are: the *Industrial Relations Act 1988* (1991 to 1996); the *Workplace Relations Act 1996* (1996 to 2006); the Work Choices period (2006 to 2009); and the *Fair Work Act 2009* (2009 onwards).[38]

The results in Figure 10 suggest there has been little change in the union wage growth premium over time. Indeed, the premium has *increased* since the *Workplace Relations Act 1996* despite the declining union membership rate. We find no evidence of a more recent shift in the union premium: a test for the equality of the coefficients to the interacted indicator variables for Work Choices and the *Fair Work Act 2009* cannot be rejected (p-value = 0.704). Indeed, the estimated premium under both of these periods is similar in size to the full-sample estimate.[39] This finding is consistent with studies in the United States and United Kingdom, which also find no evidence of a secular decline in the union wage premium over time (Farber *et al* 2018; Bryson 2007).

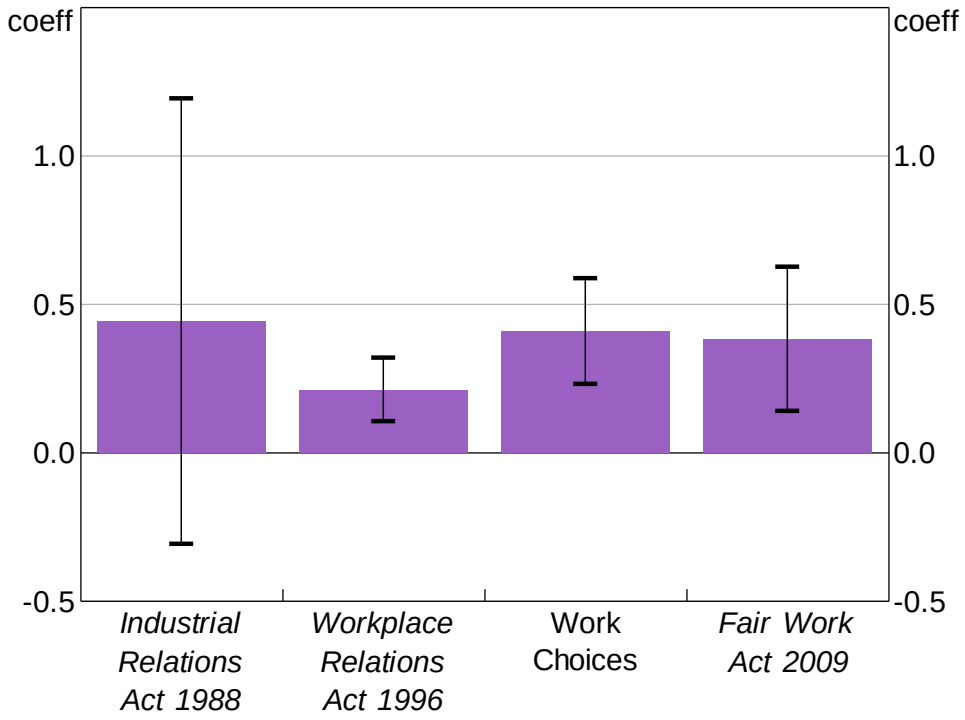
37 To isolate this type of bias, we estimate this equation using the matched sample only. We cannot control for agreement-family fixed effects in this specification.

38 See Chapter 2 of Stewart (2015) for a brief overview of these legislative regimes.

39 We show in Appendix A that this conclusion is not affected by changes to the definition of union coverage in the *Fair Work Act 2009*.

Figure 10: Union Wage Growth Premium over Time

By major workplace relations regimes



Note: Black lines denote 95 per cent confidence intervals

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.5.1 Trends in the unmatched sample

It is important to gauge whether the sample selection issue from our matched and unmatched agreements (discussed in Section 4.4.5) is relevant for estimating changes in the union wage growth premium over time. What ultimately matters for our conclusion that trends in unionisation have not contributed to low wages growth in recent years is whether the *difference* between the union premiums in the matched and unmatched samples has changed over time. Given our finding in Figure 10 that the union premium has been stable over time for agreements in the matched sample, this boils down to examining whether the premium for the unmatched sample has also been stable.

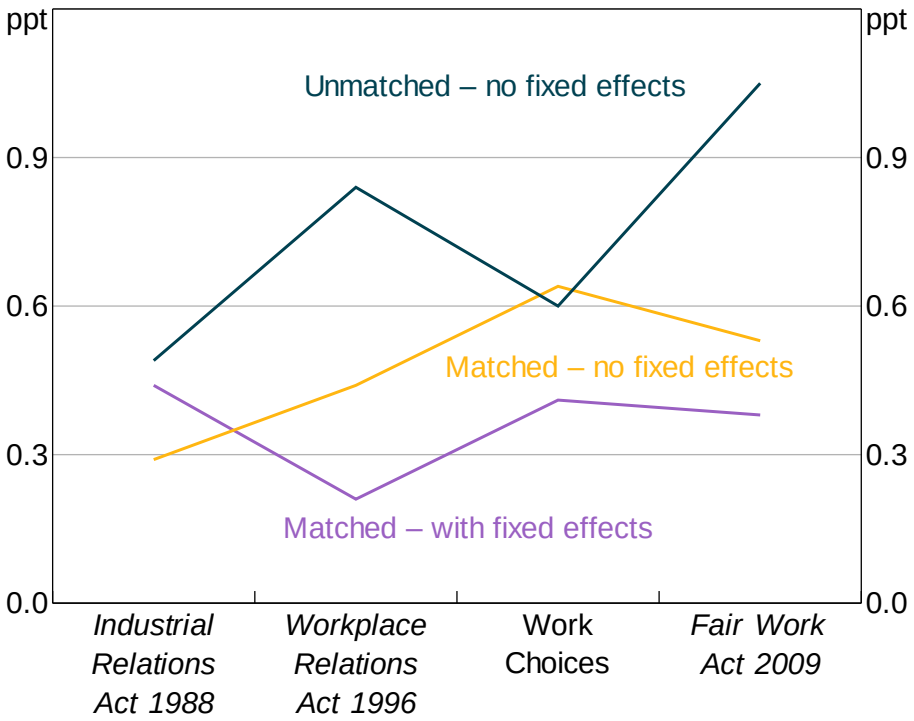
The union premium for the unmatched sample is more volatile than that for the matched sample (Figure 11). The union premium for the unmatched sample (dark aqua line) has been higher under the *Fair Work Act 2009* than under Work Choices. And since the union premium for the matched sample was broadly unchanged across these two legislative regimes (Figure 10; reproduced as the violet line in Figure 11), this implies that a combined sample – pooling both matched and unmatched agreements – would find that the union premium has *risen* over recent years. This reinforces our conclusion that unionisation has not contributed to low wages growth in recent years.

Note also that the union premium from the unmatched sample (necessarily without fixed effects) is larger than our preferred estimates from the matched sample (with fixed effects). This gap is partly explained by the inclusion of fixed effects for the matched sample: if we omit the fixed effects from our matched sample model, the estimates are closer to the unmatched sample estimates, signalling

the presence of unobserved heterogeneity that is positively correlated with wages growth and union involvement (yellow line in Figure 11). However, as discussed in Section 4.4.5 some of the gap is also unexplained.

Figure 11: Union Wage Growth Premium over Time

Matched versus unmatched sample



Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.6 Dynamics of the Union Wage Growth Premium

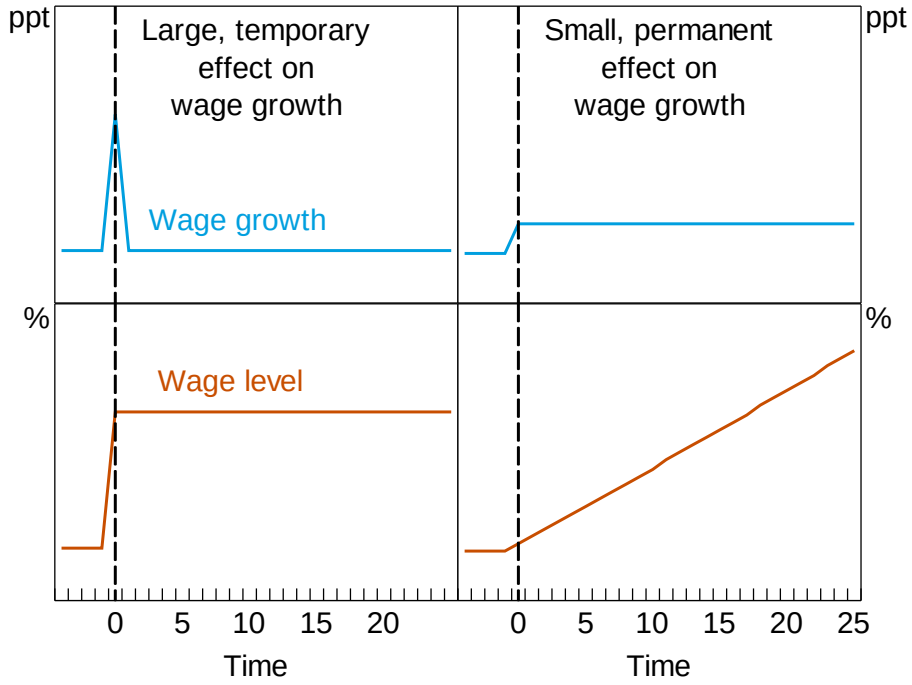
We have so far discussed the wage growth premium from union involvement in bargaining. However, our baseline results do not reveal how this union wage growth premium evolves over the duration of a union's involvement with a particular agreement family, and what this means for wage *levels*.

Although our estimates of the union wage growth premium are only identified if an agreement transitions in union status, all wage observations for a given agreement family before and after the transition contribute to the estimation.[40] This means our baseline estimate could be consistent with several possible dynamic paths for wages growth following a change in union status. One possibility is that the union wage growth premium reflects a large one-off spike in wages growth immediately after an agreement changes from no union involvement to union involvement or vice versa, with no further effect on wages growth (relative to the counterfactual of no union involvement) in subsequent negotiations. A stylised example of this case is depicted in the top left panel of Figure 12.

40 This is true in cases where union status changes once within an agreement family. In cases where union status changes more than once, the difference in wage growth outcomes between all observations with and all observations without union involvement in an agreement family contribute to the estimate.

Figure 12: Effect of Union Involvement on Wages

Relative to baseline, stylised examples



Note: Dashed vertical line denotes year of union entry, t = 0

Another possibility is that our baseline estimates reflect a union wage growth premium (e.g. of ⅓ percentage point per year) that is sustained over all contract negotiations in which a union is involved (top right panel of Figure 12). Although both of these cases can be consistent with our baseline fixed effects estimate of the union wage growth premium, they have different longer-run implications for the level of wages in union, relative to non-union, agreements. These differences are shown in the bottom panels of Figure 12. In the first case (left panels), the adjustment to wage levels from union involvement is immediate, while in the second case (right panels) the adjustment is gradual, with the gap in wage levels between union and non-union agreements widening over time.

We can test which of the two profiles in Figure 12 is most supported by the data by estimating the following equation,

$$AAWI_{ijst} = \phi_1 Union_{ijst} + \phi_2 CUnion_{ijst} + X'_{ijst}\beta + \theta_i + \theta_{jst} + \varepsilon_{ijst} \qquad (2)$$

where $CUnion_{ijst}$ is a count variable that equals the number of consecutive times an agreement has been renegotiated with union involvement up to time t, after a union first became involved in the negotiations. For instance, this variable would equal one if the agreement becomes the first one in its agreement family to involve a union, and two if the agreement is the second consecutive one in its family to be negotiated with union involvement.[41] If at any point a union is no longer involved in the negotiations, this count variable resets to zero. Given that our baseline union wage premium is identified from both switches from no union involvement to union involvement and switches in the

41 This count variable is always equal to zero for any agreement that does not change union status at any time in the sample period.

other direction, $CUnion_{ijst}$ also counts the number of consecutive times an agreement has been renegotiated without a union after a change in status from union involvement to no union involvement (in this case we multiply the count by minus one).

The immediate effect of union involvement on wages growth – that is, when an agreement negotiated with union involvement replaces one negotiated without a union – is given by $\phi_1 + \phi_2$. The extent to which this initial wage growth premium then changes over subsequent agreements (conditional on union status remaining the same) depends on ϕ_2: a negative estimate for ϕ_2 would indicate that the wage growth premium shrinks from the first to subsequent agreements despite ongoing union involvement. In terms of the stylised examples in Figure 12, the left panels would imply a negative value for ϕ_2, while the right panels imply ϕ_2 is equal to zero.

The results for this specification are presented in Table 3. The coefficient estimates suggest the immediate union wage growth premium is around ⅓ percentage point when union status switches within an agreement family, in line with our baseline results. There is no evidence that this premium changes for subsequent contract renegotiations: our estimate for ϕ_2 is close to zero and not statistically significant.[42] That is, ongoing union involvement leads to *permanently higher wage growth outcomes*. We discuss the implications of this finding below.

Table 3: Regression Results for Equation (2)

Dependent variable: *AAWI*

Union	0.38***
	(0.09)
CUnion	–0.02
	(0.02)
Industry–state–time effects?	Yes
Fixed effects?	Yes
Observations	41,611
Adjusted R^2	0.60
Adjusted R^2 (within)	0.01

Note: See notes to Table 1

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.6.1 Implications of the dynamics

The results above suggest that union involvement in bargaining confers a premium in terms of wages growth that is sustained for the duration of the union's involvement with an agreement family. This has implications for the evolution of wage *levels*. In cases where union involvement does not change over successive agreements, a premium in growth rates implies an increasing premium in levels over time, as wage rises compound (e.g. the lower-right panel of Figure 12). This is the case for most agreements in our sample, as once unions become involved they tend to remain involved. This can be seen in the sample Markov transition probabilities in Table 4.

42 Our specification constrains the effect from the presence/absence of a union in consecutive negotiations to be linear. We get similar results when we use a more flexible specification that treats each value the count variable takes as separate dummy variables.

Table 4: Markov Transition Probabilities

Weighted by employee counts, 1991–2017

$\mathrm{P}\left[Union_t = 0 \middle| Union_{t-1} = 0\right] = 0.50$	$\mathrm{P}\left[Union_t = 1 \middle| Union_{t-1} = 0\right] = 0.50$
$\mathrm{P}\left[Union_t = 0 \middle| Union_{t-1} = 1\right] = 0.02$	$\mathrm{P}\left[Union_t = 1 \middle| Union_{t-1} = 1\right] = 0.98$

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

Calculating the steady state distribution from these Markov probabilities implies that, in equilibrium, a given agreement family will spend around 96 per cent of the time negotiating with union involvement, and 4 per cent without.

An ever-increasing wage gap between union and non-union agreements is unlikely to be a long-run steady state in the labour market. However, given that our estimate of the union wage growth premium is fairly modest – at ⅓ percentage point per year – a deviation in growth rates could prevail for an extended period of time and still yield a difference in wage levels that is within a reasonable range. Collective bargaining has been a feature of Australia's wage-setting system only since the early 1990s. With this in mind, consider a hypothetical case in which two groups of employees started with the same level of wages in the early 1990s, with only one of those groups receiving a union wage growth premium each year. The difference in wage levels between these groups of workers in 2017 would be around 8 per cent (assuming average wages growth for the group not receiving a union wage growth premium), which is well within the range of estimates of the union premium from studies that focus on wage levels (see Section 2). Our estimates suggest that any current differentials in wage levels between union and non-union agreements reflects the slow accumulation of a modest differential in growth rates over a period of several decades.

Our results are more closely in line with Freeman and Kleiner (1990) and DiNardo and Lee (2004) – both of which consider the short-run effects of new unionisation – than studies using employee-level data, in that our estimates only pertain to recent union entry into negotiations within the past twenty years. Moreover, unlike studies that use a regression discontinuity design, our estimates are identified from a wider population of agreements than those in which the case for union involvement is necessarily only marginal. These reasons help to reconcile our relatively modest estimate of the union wage growth premium with the larger estimates of the union wage level premium from the vast US literature using employee-level data.

4.7 An Alternative Identification Strategy: A Natural Experiment

In this section we use an alternative identification strategy to address concerns about endogeneity in our baseline model. This identification strategy, which exploits a natural experiment that affected union involvement in bargaining for certain types of agreements, should be less susceptible to omitted variables bias (e.g. firm-level shocks that affect both union involvement and wages growth), and as such provides a crosscheck on the fixed effects estimates presented earlier.

4.7.1 The natural experiment

The Work Choices legislation came into effect on 27 March 2006 (amending the *Workplace Relations Act 1996*), and remained in force until 30 June 2009 when it was replaced by the *Fair Work Act 2009*. Both the introduction and repeal of Work Choices led to changes in whether a union must be involved

in the negotiations of greenfields enterprise agreements. A greenfields agreement is one made in relation to a new business or project, before any workers are employed. Under Work Choices, these agreements could be created without a union – the employer would essentially strike an 'agreement' with itself, and a union had no right to demand a seat at the bargaining table (Bray and Stewart 2013). An employer could also choose to make a greenfields agreement with a union. Under the preceding and subsequent legislation, however, greenfields agreements could only be made with a union. These legislative changes are summarised in Table 5, which also shows that there were no changes to rules on whether a union must be involved in non-greenfields agreements during this period.

Table 5: Union Involvement in Enterprise Agreements

	Greenfields agreements[(a)] ('treatment group')	Non-greenfields agreements ('control group')
Before Work Choices	Compulsory	Not compulsory
During Work Choices	Not compulsory	Not compulsory
After Work Choices	Compulsory	Not compulsory

Note: (a) An agreement made in relation to a new business or project, before any workers are engaged

These legislative changes can help us identify the union wage effect in two ways. First, we can study the initial introduction of Work Choices to see if the associated *decrease* in union involvement in greenfields agreements after March 2006 caused wage growth outcomes in those agreements to be *lower* (we refer to this as 'experiment 1'). We can also study the reverse experiment that occurred when Work Choices was repealed in 2009, to see if the associated *increase* in union involvement in greenfields agreements led to *higher* wage growth outcomes ('experiment 2'). In both cases, greenfields agreements are the 'treatment group' that is affected by the policy changes governing union involvement in bargaining. The 'control group' comprises non-greenfields agreements, which were unaffected by those policy changes on union involvement. For both experiments, we can estimate the causal impact of union involvement on wages growth by comparing the wage outcomes of both groups before and after the policy changes using a difference-in-differences model.

As far as we are aware, beyond the changes to the requirements for union involvement, the legislative changes had no other substantive *differential* effect on greenfields and non-greenfields agreements.

Prior to Work Choices, our data suggest that all greenfields agreements were made with union involvement, consistent with legislation in place at the time (Figure 13). During the Work Choices period, when union involvement was no longer compulsory for any agreement, the share of greenfields agreements involving a union fell sharply to reach a low of 45 per cent in late 2008. Following the repeal of Work Choices in 2009, union involvement returned to 100 per cent for all greenfields agreements as compulsory union involvement was reinstated for those agreements. By contrast, the rate of union involvement in non-greenfields agreements was little changed over the entire period.

Figure 13: Union Involvement in Enterprise Agreements

Share of agreements certified in each quarter

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.7.2 Difference-in-differences model

We use a difference-in-differences (DD) approach to study the effect of the two policy changes (in 2006 and 2009) to union involvement on wage outcomes in enterprise agreements. First, to study the effects of the introduction of Work Choices – our 'experiment 1' – we estimate the following model:

$$AAWI_{it} = \alpha + \gamma_1 Greenfields_i + \gamma_2 WC_t + \gamma_3 \left(Greenfields_i \times WC_t \right) + \varepsilon_{it} \qquad (3)$$

where the dependent variable is the average annualised wage increase in firm i in period t; the two time periods in t are 'before' and 'after' the introduction of Work Choices. WC_t is a dummy variable that equals one if the agreement was lodged under Work Choices and zero if it was lodged under the previous legislation (the *Workplace Relations Act 1996*), and $Greenfields_i$ is a dummy that equals one if the agreement is greenfields and zero otherwise. The coefficient of interest is γ_3. Differences in overall average wages growth between greenfields and non-greenfields agreements and changes over time that are common to both groups are captured by γ_1 and γ_2, respectively. We use data from a symmetric 3¼-year window around the policy change (21 December 2002 to 30 June 2009) for our estimation.[43] Unlike in Equation (1), this difference-in-differences approach does not require a matched sample.

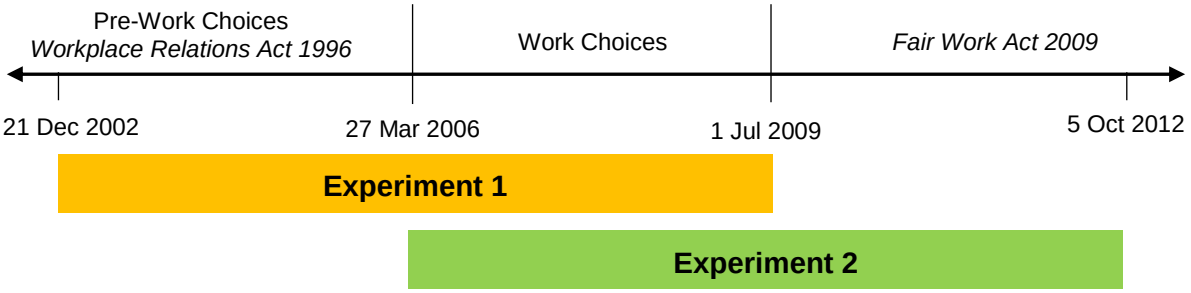
43 We use a 3¼-year window as this was the length of time Work Choices was in place.

To study the reverse experiment – our 'experiment 2' – we estimate the following model, again using a symmetric 3¼-year window around the date of the policy change (27 March 2006 to 5 October 2012):

$$AAWI_{it} = \delta + \phi_1 Greenfields_i + \phi_2 WCrepealed_t + \phi_3 \left(Greenfields_i \times WCrepealed_t \right) + \upsilon_{it} \qquad (4)$$

where the two time periods in t are 'before' and 'after' the repeal of Work Choices. $WCrepealed_t$ is a dummy that equals one if the agreement was lodged after Work Choices was repealed, and zero otherwise. Figure 14 clarifies how the timing of experiments 1 and 2 relates to the relevant legislative changes.

Figure 14: Time Line of Difference-in-difference 'Experiments' and Legislative Changes



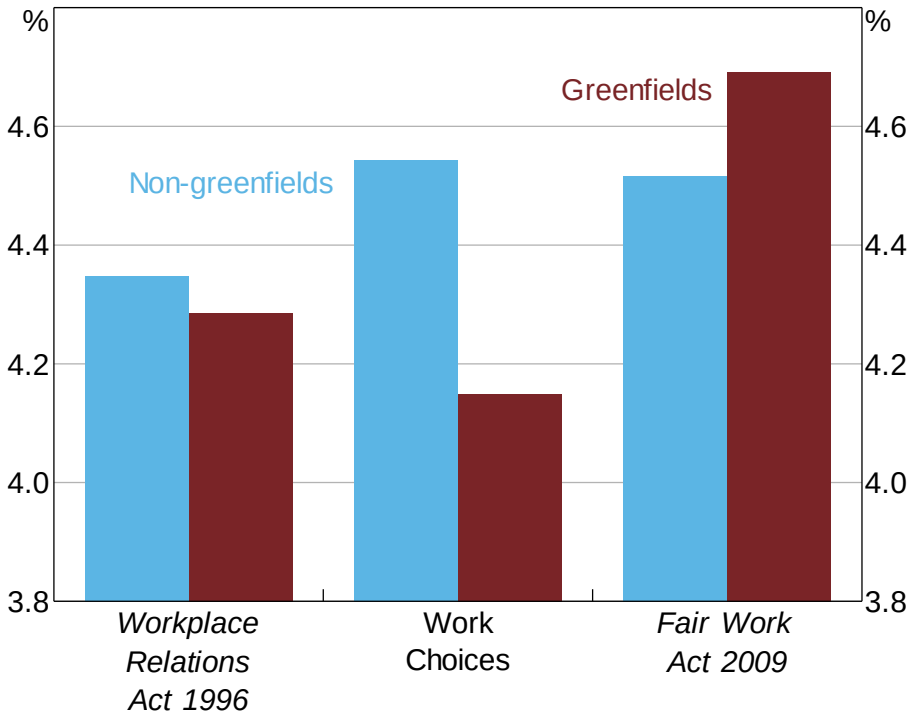
Our earlier finding of a positive union wage growth premium would imply that γ_3 is negative and ϕ_3 positive. That is, relative to non-greenfields agreements, the annualised wage increases in greenfields agreements should have decreased during Work Choices, and increased with its repeal, because of the associated changes to compulsory union involvement in greenfields agreements. Moreover, assuming that union entry and union exit have symmetric effects on wages growth, our earlier finding that the union wage growth premium has been little changed over time should manifest in estimates of γ_3 and ϕ_3 that are opposite in sign, but equal in magnitude. We test this below.

4.7.3 Graphical results

Before turning to the results, it is useful to first examine whether there is graphical evidence of an effect of union involvement on wages. Figure 15 compares average wages growth over the three legislative regimes for greenfields and non-greenfields agreements. Agreements with no change to their requirement for union involvement (non-greenfields) saw an increase in wage outcomes during Work Choices, compared to the previous period. In contrast, agreements that shifted from compulsory to voluntary union involvement (greenfields) experienced a decline in wages growth. This is consistent with a positive effect of union involvement on wages growth. Similarly, the gap in wages growth between greenfields and other agreements that emerged during Work Choices was reversed after Work Choices was repealed. Again, this is consistent with a positive union wage growth premium.

Figure 15: Wages Growth by Agreement Type

Average AAWI for agreements certified in each regime



Note: Agreements certified between 21 December 2002 and 5 October 2012

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.7.4 Difference-in-differences results

The difference-in-differences estimates for Equations (3) and (4) are shown in column 1 of Table 6.[44] Focusing first on the results for experiment 1, the DD estimate suggests that the abolition of compulsory union involvement after 2006 led to a 0.48 percentage point decrease in wages growth in greenfields agreements relative to non-greenfields agreements. As expected, the estimated effect is opposite in sign and only slightly smaller in magnitude (0.44) for the reverse experiment in 2009. A test that the estimates for experiments 1 and 2 are equal in magnitude (but opposite in sign) cannot be rejected (p-value = 0.90).

44 We also include controls for industry–state fixed effects to help control for any compositional changes over time.

Table 6: Difference-in-differences and Instrumental Variable Regression Results

	Effect of policy change on AAWI (1)	Effect of policy change on union involvement (2)	Effect of union involvement on AAWI (3) = (1)/(2)
Experiment 1[(a)]	–0.48***	–0.23***	2.14***
	(0.17)	(0.07)	(0.63)
Experiment 2[(b)]	0.44***	0.28***	1.59*
	(0.16)	(0.07)	(0.96)
Test of equal effect size (χ^2)[(c)]	0.01	4.13	0.23
Pr(*test statistic* > χ^2)	0.90	0.04	0.63

Notes: ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively; standard errors (in parentheses) are clustered at the two-digit industry level
(a) Equation (3); N = 33,894; adj R^2 = 0.20, 0.19 and 0.09 in columns (1), (2) and (3), respectively
(b) Equation (4); N = 29,385; adj R^2 = 0.36, 0.14 and 0.34 in columns (1), (2) and (3), respectively
(c) Cross-model hypothesis tests are performed using the `suest` command in Stata; for columns (1) and (2), this is a test that the sum of the DD coefficient in experiment 1, γ_3, and the DD coefficient in experiment 2, ϕ_3, equals zero; for column (3), it is a test that the difference between the Wald ratios in experiments 1 and 2 equals zero
Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

4.7.5 Backing out the union wage growth premium

The results in column 1 of Table 6 do not provide a direct estimate of the union wage growth premium. For example, in the case of experiment 1 the DD model yields the average effect of the abolition of compulsory union involvement on wages growth, which would only translate directly into a *union effect* on wages if the policy change caused the treatment group to go from having compulsory union involvement to having no union involvement at all. This was not the case because although union involvement was not compulsory under Work Choices, many of those agreements did have union involvement (Figure 13). Using experiment 2 as an example, this suggests that in a counterfactual world in which Work Choices had not been repealed, some greenfields agreements would still have been negotiated with a union between 2009 and 2012. That is, some of the agreements that were 'treated' after the policy change would still have been treated even if the policy change had not occurred.

To translate our DD estimates into estimates of the union wage growth premium we need to adjust them by an estimate of the share of greenfields agreements whose union involvement was actually affected by the policy changes.[45] We can estimate this share by replacing the dependent variable in Equations (3) and (4) with our dummy variable for union involvement. The results for this specification are shown in column (2) of Table 6. For experiment 1, the point estimates suggest that the abolition of compulsory union involvement due to the introduction of Work Choices induced 23 per cent of agreements to forgo union involvement in bargaining; for experiment 2, the

45 The intuition is that only a subset of the agreements are actually affected by the policy change because only that subset would choose to have no union involvement in the absence of any legislative requirement for involvement ('compliers'). The DD estimate captures the average effect on *all* greenfields agreements, whether or not they were actually affected by the policy change. As such, we can obtain an estimate of the union wage growth premium by dividing the DD estimate by the fraction of agreements actually affected by the policy change. This estimate could be interpreted as an overall union wage growth premium if other groups have similar responses as the compliers; we discuss this possibility in more detail later in the section.

reinstatement of compulsory union involvement (from the repeal of Work Choices) induced 28 per cent of agreements to take up union involvement.

The union wage growth premium can then be obtained by dividing the estimates in column (1) by those in column (2). This estimator is equivalent to an instrumental variable (IV) regression of AAWI on union involvement, where union involvement is instrumented using the DD interaction term.[46] The results, in column (3), imply a union wage growth premium of 2.14 percentage points for experiment 1, and 1.59 percentage points for experiment 2. The difference between these estimates is not statistically significant (p-value = 0.63). This provides us with further evidence that the union wage growth premium has not declined materially in recent years.

The union wage growth premium implied by the DD models is several times larger than the fixed effects estimates in Section 4.3. This could reflect several things. First, it may be that union wage effects are *heterogeneous by type of agreement*: unions may be able to extract larger wage outcomes from greenfields agreements than non-greenfields agreements. This seems plausible given that, in the absence of union involvement, the baseline level of worker bargaining power across these agreements was very different – under Work Choices an employer could simply strike a greenfields 'agreement' with itself, while a non-greenfields agreement still had to be agreed to by its employees. As such, the potential upside in wage outcomes for unions may have been larger for greenfields agreements.[47] The effects could also be *heterogeneous among greenfields agreements*. This matters because our IV estimator has a local average treatment effect interpretation: it yields the effect of union involvement on wages only for those firms that were induced by the legislative changes to change union involvement status. These firms may have had more bargaining power relative to other greenfields employers, making it possible for them to forgo negotiations with a union and instead set their own terms.

Another possible explanation for the discrepancy between the fixed effects and IV estimates is that one (or both) of these estimators are biased. For example, the fixed effects estimates may be downwardly biased due to omitted, time-varying, firm-level factors that are positively correlated with union involvement but negatively correlated with wages. The DD/IV estimates could be upwardly biased by a macro shock during the Work Choices period that reduced wage outcomes in greenfields relative to non-greenfields agreements (e.g. any shock that differentially affected new businesses, relative to existing businesses). It is nevertheless reassuring that, despite differences in the size of the premium, both identification strategies point to a relatively stable union premium over recent years.

46 This is an application of a Wald estimator (see Angrist and Pischke (2009, pp 127–133) for further details).

47 Until recently, firms also had limited avenues to resolve bargaining stalemates with unions in greenfields agreements. Unions could therefore potentially use negotiation delays to influence the starting date and viability of projects – a strategy that would likely have been less effective in non-greenfields agreements. For a discussion of union bargaining power in greenfields agreements after the repeal of Work Choices, see Productivity Commission (2015) and Department of Employment (2017). When the *Fair Work Amendment Act 2015* came into force on 27 November 2015, 'good faith' bargaining requirements were extended to greenfields agreements, and an avenue was created for employers to take the agreement to the Fair Work Commission for approval if agreement could be reached with a union after six months. See Productivity Commission (2015, p 712) and Department of Employment (2017) for further details.

5. Are There Spillover Effects onto Wage Outcomes in Other Enterprise Agreements?

Up to this point, we have considered only the direct effect of unions' bargaining on wages and not any broader spillover effects on the wages of other workers. There are a range of general equilibrium effects that may be relevant. For example, higher wages growth in firms that negotiated their enterprise agreements with union involvement could lead to less job creation in those firms relative to other firms. This could lead to an increase in relative labour supply to the 'non-union' firms and thus further dampen wages growth in those firms. It is also possible that unions have a 'threat effect' on wages, whereby non-union firms voluntarily decide to pay higher wages to reduce the threat of unions mobilising at the firm (Farber 2005; Bryson 2007; Rosenfeld, Denice and Laird 2016). These general equilibrium effects could raise or lower the level of aggregate wages in the economy (Farber 2001).

This section of our paper examines a narrower class of general equilibrium effects: whether union presence has spillover effects on the wages of enterprise agreements negotiated without union involvement. We test for this union 'threat effect' by considering whether wages growth is higher with the degree of union presence in a firm's industry. The threat of union entry, in the form of a union being present at the next round of wage negotiations, is likely to be higher in industries where unions already have a strong presence (e.g. manufacturing) compared to industries where unions have less presence (e.g. hospitality). Similarly, the threat level should increase for a firm as union presence in its industry increases. Among non-union firms, those subject to a higher threat level would be expected to offer higher wages growth to dissuade union entry. The union wage growth premium may also decrease with higher union presence, since firms in highly unionised industries would already be offering higher wage increases.

5.1 Spillover Effects Model

To test for spillover effects on the wages of agreements negotiated without union involvement we estimate the following model (based on Equation (1)):

$$AAWI_{ijst} = \tau_1 Union_{ijst} + \tau_2 Presence_{j,t-1} + \tau_3 Union_{ijst} \times Presence_{j,t-1} + X'_{ijst}\beta + \theta_i + \theta_{st} + \varepsilon_{ijst} \qquad (5)$$

where $Presence_{j,t-1}$ is the share (out of 1) of employees on enterprise agreements in industry *j* at quarter $t-1$ that are covered by an agreement negotiated with union involvement.[48] A positive coefficient (τ_2) on the main $Presence$ variable would suggest the existence of a threat effect. A negative coefficient (τ_3) on the interaction term would imply that the union wage growth premium decreases with union presence. That is, a threat effect should manifest in a positive estimate of τ_2 and potentially a negative estimate of τ_3.

5.2 Spillover Effects Results

The results are shown in Table 7. The coefficient on union presence (τ_2) is positive and statistically significant. It implies that among firms without a union-negotiated agreement, a firm would be willing to offer an additional 3.2 basis points in annualised wage increases for every one standard

48 We lag this variable by one quarter to avoid endogeneity.

deviation (approximately 10 percentage points) increase in union involvement in the firm's industry. The negative coefficient on the interaction term (τ_3) implies that the union wage growth premium shrinks by 6.7 basis points for every 10 percentage point increase in union presence in an industry. Both of these findings indicate a spillover effect of union presence onto the wage outcomes of agreements negotiated without union involvement. Given the average levels of union presence from the right panel of Figure 3, these estimates also appear to be consistent with our baseline estimate of an overall union wage growth premium of around ⅓ percentage point.[49]

Table 7: 'Threat Effect' Regression Results for Equation (5)

Dependent variable: *AAWI*

	Coefficient	All controls
Union	τ_1	0.84***
		(0.29)
Presence	τ_2	0.32*
		(0.17)
Union × Presence	τ_3	–0.67**
		(0.27)
State–time effects?		Yes
Fixed effects?		Yes
Observations		39,563
Adjusted R^2		0.53
Adjusted R^2 (within)		0.02

Notes: ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively; standard errors (in parentheses) are clustered at the agreement-family and industry levels

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

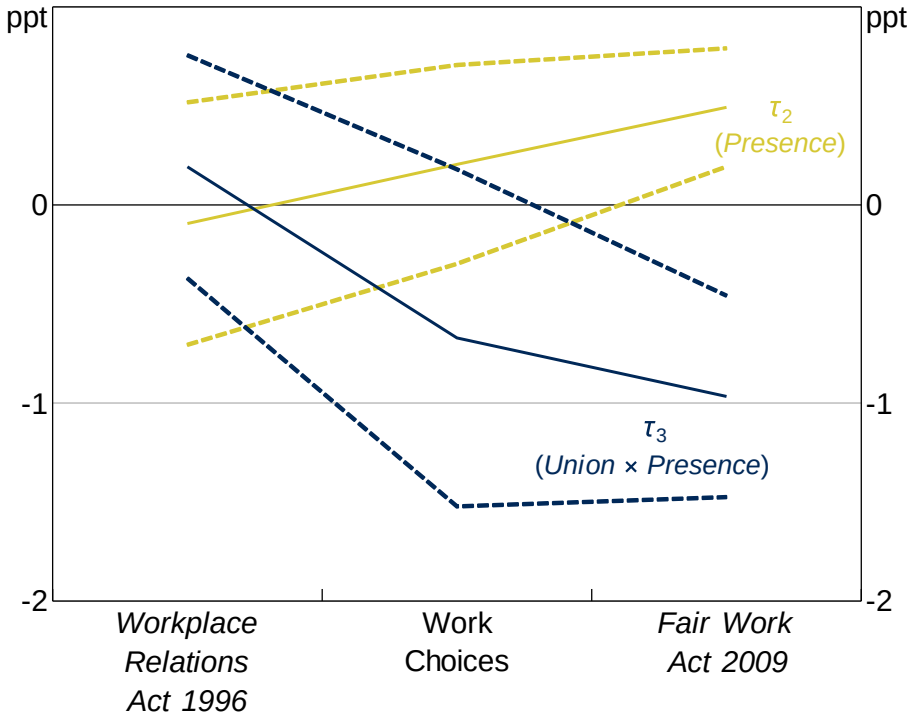
These spillover effects only have implications for aggregate wages growth insofar as they have changed over time. We can examine the stability of union threat effects by interacting the three key variables in Equation (3) (*Union*, *Presence* and *Union × Presence*) with dummies for each of the four legislative regimes spanning our sample. The estimates and their 95 per cent confidence intervals are shown in Figure 16.[50] The results suggest that there has been some increase in spillover effects on the wages of enterprise agreements negotiated without union involvement over time: the coefficient on *Presence* has become more positive, and the coefficient on *Union × Presence* has become more negative. However, the difference in the estimates between the Work Choices and the *Fair Work Act 2009* periods are not statistically significant at the 10 per cent level, nor are they economically significant. Moreover, the largely unchanged level of union coverage shown in Figure 3 suggests that the threat of union entry has not changed much in aggregate. As such, spillover effects to non-union agreements are unlikely to have contributed materially to the decline in aggregate wages growth.

49 For example, a firm in an industry where union presence is 80 per cent would offer a 30 basis point union wage growth premium (0.84 – 0.67 × 0.8), while a firm in an industry with 90 per cent union presence would offer a 24 basis point premium (0.84 – 0.67 × 0.9).

50 We omit estimates for the *Industrial Relations Act 1988* period because the standard errors are too large to draw any meaningful conclusions for that period.

Figure 16: Union Threat Effects over Time

By major workplace relations regimes



Note: Dashed lines denote 95 per cent confidence intervals

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

6. Conclusion: What Has Been the Overall Effect on Wages Growth?

We have shown that trends in unionisation are unlikely to have contributed to low wages growth in recent years, at least via their influence on collective agreements. Unlike previous research – which largely ignores the unique institutional aspects of wage setting in Australia – we account for these institutional features in our empirical strategy. We are also the first study to use a rich dataset of all federally registered enterprise agreements to provide three key insights on union wage effects.

Our first finding is that the share of the Australian workforce covered by enterprise agreements negotiated with union involvement has not fallen, even as membership rates have declined. This implies that the number of free riders who choose not to join a union has risen.

Second, we find that, despite declining membership, unions are just as effective in extracting larger wage increases from firms in wage negotiations as they were in the past. We estimate a 'union wage growth premium' of around ⅓ percentage point per year among private sector agreements (and no evidence of a wage growth premium for public sector wage agreements). We find no evidence that this premium has declined in the period since enterprise bargaining was introduced in Australia. This conclusion is also robust to an alternative identification strategy that exploits an exogenous policy change to the level of union involvement, and a range of other robustness checks.

Our final insight is that spillover effects of unions onto the wage outcome of other enterprise agreements are fairly modest. And crucially, the size of these spillover effects have remained broadly unchanged over time.

Our research is another step toward answering the question of why aggregate wages growth outcomes continue to be so low in Australia. The findings in our paper suggest that we can rule out one key channel through which a decline in employee bargaining power might be dragging on wages growth. However, our analysis does not rule out the possibility that other shifts in labour bargaining power – such as that due to rising competition from automation or a move towards global supply chains – are at work, or that the role of unions in other forms of wage setting (e.g. awards) has changed.

Appendix A: Definitional Changes under the *Fair Work Act 2009*

The classification of union and non-union enterprise agreements in the WAD changed following the introduction of the *Fair Work Act 2009*. Prior to the *Fair Work Act 2009*, the 'union' indicator variable in the WAD measured whether or not a union was involved in bargaining. Under the *Fair Work Act 2009* it instead measured whether a union is *covered* by the agreement. Being covered enhances the union's rights to enforce the terms of the agreement (Bray and Stewart 2013).

Under the *Fair Work Act 2009* unions need to apply to the Fair Work Commission to be covered by an agreement; that is, it is at a union's discretion as to whether to apply to be covered when they have been involved in the negotiations. A union may therefore choose to be covered only when the wage outcome is favourable, potentially biasing our estimate of the union wage growth premium upwards during the post-2009 sample period. Conversely, our estimates will be biased downwards if unions are more likely to apply for coverage in cases where employee bargaining power (and hence wages growth) is low, in order to ensure the firm upholds the terms of the agreement.

This definitional change does not appear to be driving our finding that the union wage growth premium has been little changed over time. Indeed, the difference-in-differences estimates in column (1) of Table 6 are unaffected by the recent definitional changes to union involvement. In the difference-in-differences model, the effect of unions on wages growth is *inferred* based on whether an agreement was greenfields or not, and hence any endogeneity in unions' decisions to apply for coverage would not affect these results. That these estimates also show no evidence of a decline in the effect of unions on wages growth in recent years gives us confidence that our baseline estimates from Sections 4.3 and 4.4 are not biased materially by these definitional changes.

Appendix B: Descriptive Statistics for Baseline Sample

Table B1: Private Sector Agreements

	Non-union agreements				Union agreements			
	Average AAWI (per cent)	Average effective duration (years)	Number of agreements	Average number of employees per agreement	Average AAWI (per cent)	Average effective duration (years)	Number of agreements	Average number of employees per agreement
By industry[(a)]								
Agriculture, forestry & fishing	2.7	3.0	66	40	3.3	2.7	209	101
Mining	3.5	3.0	145	36	3.7	2.7	584	122
Manufacturing	3.4	2.7	2,571	33	3.9	2.5	12,058	92
Electricity, gas & water supply	3.5	3.0	184	16	3.9	2.8	788	89
Construction	3.6	3.1	1,668	22	5.3	2.7	8,786	27
Wholesale trade	3.2	2.8	270	36	3.5	2.6	1,044	88
Retail trade	3.2	2.8	72	541	3.6	2.7	687	2,009
Accomm, cafes & restaurants	3.0	3.2	102	80	3.4	2.8	242	208
Transport & storage	3.4	2.8	463	37	3.8	2.6	2,953	128
Communication services	2.9	3.0	72	72	2.9	2.7	281	154
Finance & insurance	3.4	2.9	83	193	3.7	2.6	317	1,212
Property & business services	3.3	2.9	359	48	4.3	2.7	1,227	92
Government admin & defence	3.5	2.9	53	24	3.7	2.8	243	117
Education	3.5	2.8	156	94	3.7	2.9	900	334
Health & community services	3.5	2.9	498	80	3.8	2.8	3,248	204
Cultural & recreational services	3.3	2.8	60	208	3.6	2.6	397	418
Personal & other services	3.2	3.0	151	49	3.7	2.7	674	100
By state/territory in which agreement applies[(b)]								
New South Wales	3.4	2.8	1,585	31	3.9	2.6	6,883	91
Victoria	3.4	2.8	1,764	50	4.6	2.7	15,545	79
Queensland	3.5	2.9	1,325	38	3.9	2.7	3,892	154
South Australia	3.6	2.8	616	36	3.9	2.6	2,585	86
Western Australia	3.8	2.9	504	42	4.0	2.7	2,009	124
Tasmania	3.5	2.8	178	33	3.7	2.6	817	89
Northern Territory	3.4	2.8	115	25	4.0	2.7	252	84
Australian Capital Territory	3.5	2.8	118	21	4.3	2.6	459	76
Other territories	3.0	2.0	1	27	3.7	2.7	7	69
Multiple states	3.3	3.1	767	102	3.9	2.7	2,189	972
Total	**3.4**	**2.9**	**6,973**	**46**	**4.2**	**2.7**	**34,638**	**149**

Notes: Excluding agreements with non-quantifiable AAWI, those with effective duration of less than one year, and those with missing effective duration
(a) Matched to ANZSIC 1993 classifications by modal employees covered where an agreement covers multiple industries
(b) Agreement applying exclusively to that state/territory; 'multiple states' includes national agreements and where state is unknown

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

Table B2: Public Sector Agreements

	Non-union agreements				Union agreements			
	Average AAWI (per cent)	Average effective duration (years)	Number of agreements	Average number of employees per agreement	Average AAWI (per cent)	Average effective duration (years)	Number of agreements	Average number of employees per agreement
By industry [(a)]								
Agriculture, forestry & fishing	3.0	4.5	2	98	3.4	2.3	14	505
Mining	na	na	0	na	na	na	0	na
Manufacturing	na	na	0	na	3.6	2.4	19	252
Electricity, gas & water supply	4.4	2.4	3	177	3.7	2.6	145	598
Construction	na	na	0	na	3.6	2.4	3	2,080
Wholesale trade	na	na	0	na	2.0	3.0	3	192
Retail trade	na	na	0	na	1.7	3.0	1	24
Accomm, cafes & restaurants	na	na	0	na	4.9	2.2	2	217
Transport & storage	4.2	3.0	8	406	3.9	2.7	177	1,166
Communication services	na	na	0	na	3.7	2.8	43	6,289
Finance & insurance	3.4	3.0	3	34	3.8	2.4	46	742
Property & business services	2.8	2.8	6	131	3.5	2.6	57	1,156
Government admin & defence	3.8	2.7	257	243	3.7	2.5	2,748	611
Education	3.1	3.0	11	166	4.2	2.7	517	2,326
Health & community services	3.5	2.7	15	178	3.9	2.5	183	1,448
Cultural & recreational services	3.1	2.6	18	201	3.6	2.6	136	281
Personal & other services	4.7	2.9	3	3,536	4.2	2.5	52	477
By state/territory in which agreement applies[(b)]								
New South Wales	3.2	2.5	10	512	3.8	2.5	267	1,327
Victoria	3.4	2.7	62	259	3.8	2.7	1,321	801
Queensland	3.2	2.5	13	70	3.5	2.4	414	675
South Australia	4.2	2.9	26	77	3.9	2.3	564	339
Western Australia	3.9	3.0	80	79	3.7	2.7	424	514
Tasmania	3.3	2.8	12	90	3.4	2.6	189	267
Northern Territory	2.7	3.0	8	155	3.8	2.7	106	984
Australian Capital Territory	3.5	2.5	47	187	3.8	2.5	251	663
Other territories	na	na	0	na	3.7	3.1	6	48
Multiple states	4.0	2.5	68	655	3.6	2.5	604	2,433
Total	**3.7**	**2.7**	**326**	**264**	**3.7**	**2.5**	**4,146**	**939**

Notes: Excluding agreements with non-quantifiable AAWI, those with effective duration of less than one year, and those with missing effective duration

(a) Matched to ANZSIC 1993 classifications by modal employees covered where an agreement covers multiple industries

(b) Agreement applying exclusively to that state/territory; 'multiple states' includes national agreements and where state is unknown

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

Appendix C: Descriptive Statistics for Difference-in-differences Sample

Table C1: Difference-in-differences Estimation Sample

Private sector agreements certified between 21 December 2002 and 4 October 2012

	Greenfields (treatment)			Non-greenfields (control)		
	Workplace Relations Act 1996 (pre-Work Choices)	Work Choices	*Fair Work Act 2009*	*Workplace Relations Act 1996* (pre-Work Choices)	Work Choices	*Fair Work Act 2009*
Average AAWI (per cent)	**4.3**	**4.1**	**4.7**	**4.3**	**4.5**	**4.5**
Effective duration (years)	**2.8**	**2.4**	**3.1**	**2.7**	**2.9**	**2.8**
Union involvement (per cent of agreements)	**100.0**	**62.5**	**100.0**	**90.8**	**72.9**	**81.3**
Number of agreements	**1,036**	**1,319**	**1,486**	**19,561**	**11,983**	**14,602**
Average number of employees covered per agreement	**na**	**na**	**na**	**72**	**93**	**88**
Industry composition (per cent of agreements)[(a)]						
Agriculture, forestry & fishing	0.0	0.1	0.1	0.3	0.4	0.7
Mining	2.8	3.0	5.0	0.7	1.7	1.9
Manufacturing	8.1	2.7	4.8	21.7	20.6	22.4
Electricity, gas & water supply	0.6	0.8	1.1	0.7	2.0	2.3
Construction	79.1	77.6	68.0	55.9	44.6	37.2
Wholesale trade	0.5	0.2	0.8	0.8	1.4	3.1
Retail trade	0.4	0.6	0.5	1.9	1.6	1.2
Accommodation, cafes & restaurants	1.1	1.0	0.4	0.8	1.5	1.3
Transport & storage	2.6	3.0	3.9	5.5	6.8	6.7
Communication services	0.0	0.0	0.0	0.4	1.3	0.9
Finance & insurance	0.0	0.0	0.2	0.5	0.6	0.7
Property & business services	4.1	3.1	12.0	2.4	2.2	5.3
Government administration & defence	0.1	1.0	0.9	0.1	0.8	1.0
Education	0.0	0.5	0.1	0.9	4.1	3.3
Health & community services	0.4	5.0	0.5	5.5	8.4	9.2
Cultural & recreational services	0.1	1.4	0.2	0.7	0.9	0.8
Personal & other services	0.3	0.4	1.3	1.2	1.1	2.0

Notes: Excluding agreements with non-quantifiable AAWI, those with effective duration of less than one year, and those with missing effective duration

(a) Matched to ANZSIC 1993 classifications by modal employees covered where an agreement covers multiple industries

Sources: Authors' calculations; Department of Jobs and Small Business Workplace Agreements Database

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