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Union Wage Effects in Australia: Evidence from Panel Data

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Abstract

Using data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey, our research indicates that unobserved heterogeneity substantially biases cross-sectional estimates of union wage effects upward for both males and females. Estimates of the union wage premium for male workers between the ages of 25 and 64 fall from 8.7 percent to 5.2 percent after controlling for unobserved heterogeneity. For females aged 25 to 63 the estimated 4.0 percent cross-sectional union wage premium falls to 1.9 once unobserved heterogeneity is controlled for. Our results also indicate positive sorting by unobserved skills into union membership, especially among low skilled male and female workers. There is also evidence of negative sorting into unions among the most highly skilled.

JEL Codes: J31; J51

Keywords: union wage effects; fixed effects models; panel data

Introduction

Industrial relations reforms since the 1990s have significantly liberalized Australia's centralized system of wage setting, progressively moving it away from compulsory arbitration and conciliation toward a greater reliance on bargaining at the enterprise level. For example, Wooden (2005) cites recent evidence from Australian Bureau of Statistics (ABS) data showing that only 20 percent of employees rely on awards as a source of increasing pay, compared to an estimated 68 percent in 1990. As of 2004 about 38 percent of workers relied mainly on collective agreements to set their pay, and much, though not all, of these collective agreements involved unions negotiating agreements on behalf of workers in a manner reminiscent of collective bargaining as it occurs in the U.S., Canada, and the U.K.

It is reasonable to expect that the benefits of union bargaining have become more localized under the new decentralized structure. Before reforms, the benefits of union bargaining and advocacy were more widely dispersed among union and nonunion workers through the wide application of arbitration awards, which applied to covered workers regardless of union membership.[1] Under union-bargained enterprise agreements, the higher wages associated with union bargaining will not necessarily extend to nonunion workers outside the firm, which should put upward pressure on the union-nonunion wage differential. Thus, one purpose of this study is to update estimates of the union-nonunion wage differential using data generated after the reforms took effect.

Another reason to update estimates of the union wage effect is the availability of a new nationally representative longitudinal data set that allows the researcher to control

[1] An exception to this is the practice of bargaining for over-award payments, which gave union members a small pay premium over the award rate.

for endogeneity in union status. The Household, Income and Labour Dynamics in Australia (HILDA) Survey has been generating annual data on a group of respondents since 2001. The six waves of data currently available are sufficient to generate reliable fixed effects estimates of union wage effects, which allow one to better control for unobserved quality differences among workers.

Before the availability of the HILDA Survey data, researchers interested in union wage effects in Australia generally relied on cross-sectional data (e.g. Blanchflower and Freeman 1992; Miller and Mulvey 1996; Miller, Mulvey, and Neo 1999; Wooden 2001; Waddoups 2005; Cai and Liu 2008; Waddoups 2008). The standard empirical methodology for such research is to regress the natural log of the wage on variables that control for differences in human capital, demographic, employer, and other characteristics that may affect the wage. Also generally included in the vector of regressors is a dummy variable that controls for union membership, or some other indicator of union status. Most studies have found positive and statistically significant parameter estimates on union variables, thus indicating that unionism is associated with substantially higher wages after holding other observable characteristics constant.

A well-known criticism of this methodology for estimating union effects is that workers are not randomly assigned to union membership status. For example, if workers in unions are systematically more productive than other workers in ways that are unobservable to the researcher (e.g. unobserved ability), at least part of the union premium may be attributable to such productivity differences rather than to union status. The union effect as captured by the coefficient on the union variable in a wage equation is thus biased upwards. A common econometric solution to the problem is to observe respondents' wages at two points in time, at one point when they are union members and at another when they are not. Using longitudinal data to observe the same worker at two points in time, it becomes possible for the researcher to hold constant unobserved productivity differences that may bias coefficient estimates on the union variable.

After briefly discussing the nature and scope of industrial relations reforms in Australia, the previous literature on union wage effects, and the possibilities of endogeneity bias, we will describe the HILDA Survey. Then we will propose an empirical model, which will be estimated. The results will then be discussed and conclusions drawn.

Industrial Relations Reforms in Australia

The *Commonwealth Conciliation and Arbitration Act* of 1904 governed Australian industrial relations through a system of compulsory arbitration and conciliation for much of the 20th century. The arbitration system was designed to settle industrial disputes. For example, in federal cases, unions and employer associations brought their arguments before the Australian Industrial Relations Commission (AIRC), which heard cases and issued widely applicable, legally binding multi-employer awards. The awards set rates of pay and conditions of employment for workers in the relevant occupation or industry groups. Before the reforms of the 1980s and 1990s, awards were highly prescriptive, outlining detailed terms and conditions of employment for the majority of workers (Deery et al. 2001).

The Act gave officially recognized unions the ability to submit industrial disputes to a hearing before an arbitration tribunal. As unions advocated for their members, the outcomes embodied in arbitration awards – higher wages and improved working conditions – applied equally to members and nonmembers.[2] Such awards were applied widely across industry and more often occupational groups above the level of the individual enterprise (Wooden 2001). Thus under the arbitral system of wage determination, one would expect only a modest union-nonunion wage differential.[3]

Reforms to the award system started in the late 1980s and culminated in the passage of the *Workplace Relations Act (WRA)* in 1996. The reforms, although keeping some elements of the arbitration system, gave primacy to 'enterprise bargaining' as a mechanism for establishing wages and conditions of employment. An important step in the reform movement was the AIRC's adoption of the 'enterprise bargaining principle' in October 1991. 'Enterprise bargaining' refers to several varieties of employment relationships, whose common thread is the determination of wages and conditions at the level of the individual workplace rather than at the level of multi-employer awards. A common form of enterprise bargaining is similar to single employer-single union collective bargaining as it occurs in North America. Enterprise bargaining also includes a form of bargaining between an employer and group of workers *not* represented by a union, according to a provision in the *IRRA*. Also included is 'bargaining' between individuals and employers codified into Australian Workplace Agreements, which are registered *individual* agreements between individual workers and their employers provided for by the *WRA*. According to MacDonald, Campbell and Burgess (2001), many

[2] Miller and Mulvey (1993) offer a more thorough description how unions and employers interact in the arbitration process.

[3] Miller and Mulvey (1993) suggest several explanations for existence of a union-nonunion differential in spite of the arbitration system mandating that arbitration awards apply equally to union members and nonmembers, including, 1) over-award payments are distributed across the workforce in a manner favourable to union members; 2) union workers work more overtime hours at premium rates; 3) union members are distributed across awards in such a way that union membership is disproportionately associated with the highest wage awards; and 4) nonunion workers are more likely to be paid under-award wages—which is illegal under Australian labor law.

industrial relations scholars also list the relatively unregulated sphere of common law employment contracts between employers and individual workers (which have always co-existed with the arbitration system) as part of the enterprise bargaining framework.[4]

Although structural changes have increasingly emphasized enterprise bargaining, arbitration is still an important element in the structure of Australian industrial relations. Participation is still mandatory, rulings are still binding on the parties, and awards still apply to union and nonunion workers alike. The main difference is that under the new reforms, awards establish only *minimum* standards of employment. In contrast to the highly prescriptive, detailed awards of the pre-*WRA* era, awards under the *WRA* regulate only 20 allowable matters (Deery et al. 2001). The reforms prompted Wooden (2001: 245) to observe, "[I]t is very clear that the agreements [enterprise agreements] are gradually replacing awards as the principle mechanism for determining wages and conditions."

Thus a major thrust of Australian industrial relations reforms of the 1990s was to decentralize wage setting by devolving it toward the enterprise level, either through more collective bargaining between unions and individual enterprises or through an expansion of individual bargaining. Interestingly, Katz (1993) points out that such decentralization in collective bargaining and wage determination has also been observed in other countries such as the U.K. and the U.S.[5] Marchington, Goodman and Berridge (2004) and Katz (1993) document a reduced incidence of multi-employer bargaining in the U.K between 1979 into the early 1990s, which meant that wage setting through collective bargaining was located closer to the level of the individual enterprise. In addition to decentralized

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[4] Waddoups (2008) also discusses industrial relations reforms.
[5] Katz (1993) also reviews trends in decentralization of collective bargaining in Australia, Sweden, Germany, and Italy.

collective bargaining, fewer workers were actually covered by collectively bargained agreements as union density declined from 55 percent to 29 percent over the period.

Although collective bargaining has always been relatively decentralized in the U.S., a pattern of further decentralization occurred during the 1980s into the early 1990s. According to Katz (1993), the already fragmented system became further decentralized with a notable breakdown in multiemployer bargaining in the steel industry (1986) and a weakening of multiemployer bargaining in the mining and freight hauling industries. In addition, the relatively low and declining union density during the period implies a large nonunion sector characterized by a high degree of individualization in the employment relationship, and high levels of variation in pay and working conditions across employers and industries (Katz and Wheeler 2004).

Previous Estimates of the Union Wage Effect in Australia

Several studies have addressed the impact of unions on wages in Australia. In general they have found that union members earn significantly more than their nonunion counterparts. Blanchflower and Freeman (1992), using data collected during 1985-1987 (depending on the country) from the International Social Survey Programme, found that Australia's union wage premium of 8.3 percent for males and females combined was substantially smaller than the analogous estimate of 24.6 percent for the U.S and 10.5 percent for the U.K. Other studies have found a larger union-nonunion differential. Miller and Rummery (1989), using data from the Australian Longitudinal Survey, only examined data on male workers between the ages of 19 and 25. They found a mark-up of roughly 14.6 percent after controlling for variables indicating location, human capital, occupation, and industry. In a survey article, Miller and Mulvey (1993) found that most estimates of the union wage effect were between 9 and 15 percent when the researchers used data representative of the labor force.

There is some evidence, however, that estimates of the union wage premium shrink when more extensive controls are employed. Using data from the Australian Bureau of Statistics (ABS) gathered in the Survey of Education and Training (SET) in 1993, Miller and Mulvey (1996) show that controlling for establishment size along with other variables traditionally included in wage equations reduces the size of the union mark-up to only 2.6 percent for males and 1.6 percent for females, an effect which, although statistically significant for males, is arguably economically negligible. Also using the SET, but adding data gathered in 1997 and 2001, Waddoups (2005) showed that even after controlling for employer size the union wage premium appears to have grown somewhat during the 1990s as labor market reforms began to weaken the arbitral system of wage determination. The growth was mainly confined to high union density industries. For male workers the union wage premium was approximately 5.2 percent in 2001.

The findings of Miller and Mulvey (1996) indicated that the impact of unions on wages was quite small after controlling for firm size. Wooden (2001) argued that the result was driven by a lack of data on the level of union activity at the workplace level. Using data from the 1995 Australian Workplace Industrial Relations Survey (AWIRS), which combines observations at the individual and establishment levels, he estimated a union wage premium of roughly 10 percent in establishments with active unions. In industry specific regressions that do not directly control for the level of union activity in firms, Waddoups (2005) also shows higher union wage effects in "Mining, Utilities, Transportation, and Communication" and "Construction," with the former industry grouping growing from 5.3 percent in 1993 to 10.0 percent in 2001 and Construction increasing from 8.3 to 9.6 percent over the period. In anther study, Waddoups (2008) demonstrated that under the new industrial relations policies, union effects are still relatively small in large firms (100 or more workers), but appear to have increased somewhat among workers in smaller firms (10 to 99 workers). Such findings hold for males but not for females, who experience no appreciable differences in union effects based on firm size.

The union effect also differs along the wage distribution (Cai and Liu 2008). Using data from the HILDA Survey gathered between 2001 and 2004, they found that male union members in the 10th percentile of the wage distribution earned approximately 13.3 percent more then their nonunion counterparts, while for female union members, the premium was 5.1 percent. At the 90th percentile, the effect drops to 6.4 percent for males and 3.5 percent for females.

The preceding has documented multiple studies of the union-nonunion differential. When extensive controls are included, the estimated union mark-up tends to fall. The research also demonstrates that the union wage premium appears to have increased somewhat during the 1990s into the 2000s along with industrial relations reforms and declining union density. More recent research has found that the union wage premium tends to be relatively higher at the low end of the wage distribution, in firms with active unions, in the mining, utilities, transportation, communication and construction industries, and among workers in smaller firms. Changes in industrial relations policies and practices along with an increasingly globalized economy continues to alter the labor market, which suggests that additional research on union effects remains relevant to the Australian economy.

Endogeneity and the Union Wage Effect

One topic that appears to be under-researched in the Australian context is the impact that endogenously determined union status exerts on the union wage premium. Suppose unobserved factors affect both workers' wages and the probability of being a union member. Then the union variable in an OLS wage equation is correlated with the error term, which causes the parameter estimates used to compute the union wage effect to be biased and inconsistent. Because of this methodological problem, the actual impact of unions could either be overstated or understated. Intuitively, endogeneity could originate from workers at the lower end of the skill spectrum queuing for higher-wage jobs in which union membership is more likely. If a larger pool of workers is available to firms that are paying a union wage premium, they may theoretically be able to choose the most productive workers. If some of the indicators of productivity are visible to the firm, but not to the researcher, the higher wages going to union members may appear to be attributable to union membership, when in fact the higher wages are partly due to unobserved characteristics that lead to higher productivity. Thus, the union effect will be upwardly biased. If the unobserved productivity-related characteristic leads to lower productivity, say at the upper end of the distribution, because workers with high levels of observed skill select themselves out of the union queue, then the same logic applies, but the union effect will be biased downward.[6]

Researchers generally account for unobserved heterogeneity by controlling for individual fixed effects using longitudinal data. In essence, the researcher observes the same worker before and after a change in union status. Because unobservables describing

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[6] Hirsch and Schumacher (1998) and Card (1996) suggest that such a dual selection process is consistent with lower levels of wage dispersion among unionized workers.

the wage generating process presumably remain fixed over time, controlling for individual fixed effects is a way to control for individual heterogeneity, leaving the union effect unbiased. To the authors' knowledge the only research on the Australian labor market that has used the longitudinal nature of a data set to control for endogeneity is Kornfeld (1993). He found a union mark-up of between 7 and 10 percent using data from the Australian Longitudinal Survey (ALS) on workers aged 19-25 in 1984-1988. Notably, the estimates are for males and females combined and do not control for firm size. Unfortunately the study does not provide cross-sectional OLS estimates for comparison purposes.

Freeman (1984) notes that access to longitudinal data to control for unobserved heterogeneity, however, does not provide a research panacea. He demonstrates that measurement error, in which union status is wrongly recorded in just a modest number of observations, can substantially bias the union effect downwards. Intuitively, suppose a non-union worker in time t is wrongly observed as a union member in time t+1; absent extenuating circumstances, the wages will likely be similar in the two periods. Thus it will appear as if the change in status from non-union to union has very little effect. Freeman (1984) argues that the probable upward bias of the cross sectional estimates, along with the probable downward bias of the longitudinal fixed effects estimates provide reasonable upper and lower bounds for the true union effect.

Data from the HILDA Survey

The empirical analysis is based on an unbalanced panel originating from the first six waves (2001–2006) of the HILDA Survey. The survey is a national household panel study focused on families, income, employment and well-being. Wooden and Watson (2007) provide detailed information on the survey. The first wave was conducted between August and December 2001. A total of 7,683 households representing 66 percent of all in-scope households were interviewed, generating a sample of 15,127 persons 15 years or older and eligible for interview. Of them, 13,969 were successfully interviewed. Subsequent interviews for later waves were conducted about one year apart.

Attrition rates are a particular concern in panel studies. Wooden and Watson (2007) report that attrition rates have moderated over time, and are similar to those found in other major household panels surveys, such as the British Panel Household Survey (BHPS). During the second wave, surveyors successfully re-interviewed 86.4 percent of first wave households. However, 94.4 percent of households in the fourth wave were re-interviewed during the fifth wave.

The HILDA Survey contains detailed information on individuals' current labor market activity including labor force status, earnings and hours worked, and employment and unemployment history. For those employed, information on job characteristics, such as the size of the workplace and the industry to which the employee belongs is also collected. The wages used in this study refer to hourly wages in the main job, which is computed by dividing weekly earnings by average hours worked in the main job.[7,8] Wages are deflated to the first quarter of 2001 using quarterly wage growth rates for males and females separately. One advantage of the HILDA Survey data compared to other large nationally representative data sets in Australia is that the earnings data are not grouped, thus avoiding possible measurement error. We exclude workers who do not

[7] Using wages derived from earnings and working hours of all jobs produces virtually identical results.
[8] We use hourly wages to avoid complications arising from the potential effects of unions on hours worked (Andrews et al. 1998).

know their union status to reduce bias in the estimated effect arising from self-reporting error (e.g. Freeman 1984).[9]

Our analysis focuses on workers aged 25 or over, but under the Australian pension age. Our sample of males includes workers aged 25 to 64 years (inclusive), and the sample of females includes workers aged 25 to 62 years for waves 1 to 4, and aged 25 to 63 years for waves 5 and 6.[10] Full-time students are excluded even if they reported employment. Also excluded are the self-employed and workers in agriculture.

Appendix Table 1 presents summary statistics of the pooled data samples. In the sample of males there are 13,272 observations, representing 3,004 individuals; in the sample of females there are 13,251 observations, representing 3,096 individuals. Thirty-six percent of observations on males and 33 percent of observations on females report union membership. The wages of union workers are on average slightly higher than that of nonunion workers for both males and females, and the differences in average wages between union and nonunion workers are statistically significant. There are other observable differences between union and nonunion workers. For example, union workers tend to be older, have more work experience, and have longer tenure than nonunion workers. Relatively fewer male union workers have a degree, but a much larger proportion of male union workers have a non-degree post-school qualification, such as trade certificate or diploma. In contrast, a much larger proportion of female union workers have a degree, and a much smaller proportion of female union members report

[9] This definition of union status is different from that of the Australian Bureau Statistics (ABS). The ABS classifies those who do not know their union status as nonunion workers.

[10] Qualifications for women to receive Age Pension payments changed in July 1995. The minimum Age Pension age in 2001 and 2002 was 62 years, 62.5 years in 2003 and 2004, and 63 years in 2005 and 2006. To simplify the analysis, we counted 62 years as the female Age Pension age for waves 1-4 and 63 years for waves 5 and 6.

not finishing school. Union members are less likely to work part-time or as casual workers compared to nonunion workers. Among males, a larger proportion of nonunion workers are white-collar employees while a larger proportion of union workers are blue-collar employees. The opposite pattern holds for females. Workers reporting union membership tend to work in larger firms compared to nonmembers.

Fixed-effects models rely on those workers who change their union status (union switchers) to identity the unbiased union wage effect. For an indication of changes in union status, consider Table 1, which provides a summary of year-to-year changes. The transition matrix shows that 428 male nonunion workers switched from nonunion to union status, while 421 switched from union to nonunion status. The number of switchers among female workers is slightly higher, with 502 moving from nonunion to union status and 451 moving from union to nonunion.

Table 1 about here

Because we only use workers with positive earnings for our analysis, there could be a sample selection problem if the unobserved determinants of wages also affect individuals' labor force participation decisions. A commonly used method in a cross-sectional analysis for correcting sample selection bias is Heckman's two-step estimation. Wooldridge (1995) and Wooldridge (2002: 582) propose a similar correction procedure for models estimated using panel data. Essentially, it tests the significance of the inverse Mills ratio in the wage equation estimated by fixed effects. The inverse Mills ratio is calculated from a pooled-data probit model for the selection equation. Our results show that the parameter estimate on the inverse Mills ratio is statistically insignificant for both males and females, which suggests that sample selection bias with respect to participation is probably not be an issue.[11]

The Econometric Model

For the econometric model we use an augmented version of the standard wage equation. Letting w_{it} denote the natural log of hourly wages for an individual *i* at time *t*, the wage determination equation is specified as

$$w_{it} = X_{it}^{'}\beta + \psi U_{it} + \mu_i + \varepsilon_{it} . \qquad (1)$$

Where $i = 1,..., N$ represents the number of individuals at each wave and $t = 1,\ldots.6$ is the number of waves. The symbol *X* represents a vector of observed variables determining wages, and β is a vector of associated parameters, U_{it} denotes union status, ψ measures the effect of unions on wages (the parameter of interest), μ_i is an unobserved time invariant individual-specific effect, and ε_{it} is a random error term.

Cross-section estimation of the equation (1) is likely to produce biased estimates of ψ, because individuals' decisions to join unions are likely to be affected by unobservable factors. In particular, μ_i may reflect an individual's ability in market production, which is fixed over time. If unions attract more capable workers, then the estimate on the union variable will be biased upward in a cross-sectional regression. It may also be the case that less-capable workers join unions to seek union protection. In this case, the estimate on the union variable will be biased downward due to the lack of

[11] For males the estimate on the Mills ratio is 0.093 with a standard error of 0.069; for females the estimate and its standard error are -0.021 and 0.300.

control for ability in a cross sectional analysis. We estimate equation (1) using two estimation procedures. The first is a pooled OLS, which uses the panel as an extended cross-sectional data set and thus provides estimates that would arise from similar analyses on a cross-sectional survey. Estimates from this model should be comparable to other cross-sectional studies and form the basis from which to compare with the individual fixed effects models. The second is the fixed effects model, which uses the panel nature of the data to control for unobserved heterogeneity and provide consistent estimates when the unobserved individual fixed effect is correlated with observed variables.[12]

Estimation Results

We estimate a wage level (pooled OLS) equation and wage change (fixed effects) equation as specified in equation 1. The estimation results are located in Table 2. The first and third columns show the wage level models for males and females. Consistent with earlier research using cross-sectional data, the wage level results show a significant union wage effect. Male union members earn about 8.7 percent more than their non-member counterparts and female union members earn 4.0 percent more than non-members.[13] Both estimates reach statistical significance at conventional levels. The results are similar to those found in Cai and Liu (2008), who also use data from the HILDA Survey, and fall near the range estimated by previous studies of the Australian labor market.

Table 2 about here

[12]A Hausman test indicated that the fixed effect specification is preferred to the random effects model.
[13] The union wage effect is computed using the formula exp(β)-1.

The estimates from the wage change models are located in the second and fourth columns. The union wage effect, at 5.2 percent for males, is substantially smaller using the wage change approach, though at 2.0 percent, it was not statistically significant (at the 0.5 level) for females.[14] The findings that estimates decrease substantially from the pooled OLS model to the fixed effects model are consistent with the notion that unobserved productivity differences upwardly bias cross-sectional estimates of the union premium. It appears that unobserved factors that increase the probability of being in a union are also positively correlated with the wage.

Other interesting results in Table 2 are also worth mentioning. The results from pooled OLS equations show no difference in wages for part-time or casual male workers. However, when unobserved heterogeneity is controlled for with the fixed effect specification, the coefficients on part-time and casual work both become statistically significant and positive. These results suggest that unobserved characteristics associated with working in part-time and casual employment are also associated with a lower wage, which biases the OLS estimates downward. The result is a wage premium for both part-time and casual workers after unobserved characteristics are controlled. For female workers the results also reveal a wage premium for part-time work, but there appears to be no association between casual employment and wages. The OLS estimates are also smaller than the fixed effects estimates on both part-time and casual variables, but statistical significance is only found for the part-time estimate.[15]

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[14] Booth and Bryan (2004) suggest a number of explanations for workers joining unions even when there is no apparent wage advantage for doing so. Among the reasons include excludable goods and services offered, avoidance of reputational damage, and friendly society benefits.

[15] The results on part-time and casual for males differ somewhat from Booth and Wood's (2008) Table 2. In particular we find a larger part-time effect and a larger positive casual effect. We also find a larger negative bias on the OLS estimates from unobserved characteristics. For females we find a smaller part-time

Another interesting finding emerges on the workplace size variables. Empirical research using cross-sectional data generally shows that if all things are equal, larger firms pay higher wages. For example, Brown and Medoff (1989) document the pattern with estimates using data from the U.S., while Green, Machin, and Manning (1997) find a similar pattern in the U.K., and Miller and Mulvey (1996) and Waddoups (2008) confirm the pattern using data from Australia. Our results show the expected progression of larger wage premiums as workplace size increases on the pooled OLS estimates for both males and females. For males, the premium for workplace size ranges from 10 percent for firms with 20 to 99 workers relative to those with fewer than 20, to 22 percent in firms with 500 or more workers. For females the premiums range from zero to 11 percent. After unobserved heterogeneity is controlled, however, the estimates diminish to a small range between roughly 3 and 5 percent among males, and statistical significance completely vanishes for females. Our results suggest that the workplace size effect appears to be driven mostly by unobserved quality differences across workers.

Union Wage Effects and Observed Skill

Studies from the U.S. often find that the union wage effects are larger for lower-skilled workers than for higher skilled workers (Lewis 1986; Card 1996). The conventional explanation holds that unions standardize wages by reducing differentials across and within job positions so that lower-skilled workers receive a larger premium relative to their alternative nonunion wages (Freeman 1980). An alternative explanation

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premium and positive interaction between part-time and casual. The differences likely originate from differences in model specification and the addition of two waves of data. See also Rodgers (2004) who conducted earlier work on the full-time/part-time wage differential in Australia using a Heckman selection model.

discussed in detail by Hirsch and Schumacher (1998) holds that union workers are more homogenous in terms of unobserved ability than their nonunion counterparts because of dual selection process by employers and employees. Workers with low levels of observed skills queue for union jobs, and employers choose those with high levels of unobserved skills. Workers with high levels of observed skills select out of the queue for union jobs, leaving observably skilled workers with lower levels of unobserved skills. The resulting lower dispersion of unobserved ability among union workers implies that union workers with low levels of measured skills, such as education, have high unobserved ability, and union workers with high measured skills have low unobserved ability. As a result, without controlling for unobserved ability, union wage effects are overestimated for lower skilled workers and underestimated for higher skilled workers.

Although it may be argued that the ‘true’ relationship between union wage effects and observed skills can be better estimated by using panel data to control for unobserved heterogeneity, the evidence using panel data from the U.S. is mixed. While Card (1996) finds significantly higher union wage effects in the lowest quintile of the skill compared to the highest quintile (12 percent compared to 32 percent) after controlling for unobserved heterogeneity, Hirsch and Schumacher (1998) show that the longitudinal estimates controlling for unobserved worker-specific skills are relatively uniform across the distribution of observed skills, ranging from 11 to 15 percent.

To the authors' knowledge there are no studies examining how the union wage effect varies with observed skills in the Australian context. In this section we examine whether the inverse relationship between union wage effect and observed skills holds in

Australian data, and how the relationship varies between cross-sectional and panel data models.

Following Hirsh and Schumacher (1998), we split the samples into different skill groups using two observed skill measures, education and predicted wages.[16] The estimation results are located in the left panel in Table 3. The OLS estimates are qualitatively consistent with the U.S. findings that use cross-sectional data. That is, the lower the observed skills, the higher the estimated union wage effect. For example, male workers with a college or higher degree qualification are found not to benefit from unionism, while male workers who did not complete year 12 enjoy a union premium of about 15 percent. The results for females exhibit a qualitatively similar pattern for the "College Degree" and "Year 12" groups, but there is no significantly higher union effect for the "Less than Year 12" group, as was observed among males.

Table 3 about here

The estimates using predicted wages as observed skills show a generally similar pattern. However, the negative union wage effect for male workers in the top quarter of observed skills becomes significant when predicted wages are used to measure skill. The estimate for the top quartile of females is also negative, but statistically insignificant. The fixed effect estimates accounting for unobserved ability are about one half or less than one half the size of the OLS estimates for both males and females, except for the highest skilled workers. Our results indicate that only male workers with lower observed skills are found to clearly enjoy a union wage premium. The results for male workers are,

[16] The predicted wage is calculated from the coefficients of a wage equation estimated using nonunion workers. The wage equation is estimated separately for males and females. The variables included in the wage equation are the same as in the fixed effects model except for that the union variable is excluded. Union and nonunion workers are then divided into skill quartiles measured by their predicted log wages.

however, suggestive that unobserved heterogeneity biases the cross-sectional estimate of union wage effects upward for those with relatively high observed skills (though the t statistics on the OLS and FE models are quite small, registering at only 1.700 and 1.625 in the two models, respectively). For male workers with lower observed skills, OLS estimation biases the union effect downwards. In other words, the results suggest that there is positive selection into union membership among the lowest skilled workers and perhaps mild negative selection among the highest skilled workers. Positive selection among union workers into lower skilled jobs only seems to hold for females in the second quartile of the wage distribution. The other estimates in the fixed effect models do not approach statistical significance.

Unlike the findings in Hirsch and Schumacher (1998), but consistent with Card (1996), we find the union effect to be the largest among the lowest skilled males in both the OLS and fixed effects specifications, albeit the pattern is more pronounced in the OLS equations. For females none of the fixed effects estimates reach statistical significance, which suggests that the union wage effect found using OLS is likely to be more about unobserved heterogeneity than union bargaining power. Putting the results together, the panel data evidence suggests that in Australia only lower skilled male workers enjoy higher wages attributable solely to union status.

A possible objection to the specification estimated above is the inclusion of workplace size variables in the estimating equation. Wooden (2001) found that the level of union activity, not just union status per se, exerts a significant independent effect on the union-nonunion differential. The HILDA Survey does not observe union activity; however, workplace size is likely to be a proxy for it. For example, it is reasonable to expect that unions may find economies of scale in operation or more potential rents to extract at larger firms, and thus may be more active there. It thus follows that if we control for workplace size when it is correlated with union activity, we might underestimate the actual union effect by controlling away union activity through the workplace size effect.

We therefore re-estimated the model without workplace size controls. The results in the second panel on the right side of Table 3 show a systematic increase in the union wage effect among males in most skill groups. The overall union effect is increased by about 24 percent from 8.7 percent to 10.8 percent. The pattern is evident among most of the skill categories as well. Among females a similar qualitative pattern emerges but is much less pronounced. Interestingly, however, for both male and female respondents, the larger union wage effect that emerges when excluding workplace size controls mostly disappears when unobserved worker quality is controlled for in the fixed effects models. This pattern is especially evident among males, where the coefficient on union status grows from .0831 to .1027 (23.5 percent) in the OLS equation compared to the fixed effects model, where the union coefficient grows from just .0508 to .0529 (4.1 percent). The pattern is qualitatively similar but smaller for females, where the OLS coefficient grows from .0394 to .0463 (17.5 percent) and the fixed effect coefficient increases from .0193 to .0213 (10.4 percent).

Measurement Error. Both Card (1996) and Hirsch and Schumacher (1998) dealt with measurement error as presented in Freeman (1984). Recall that in fixed effects estimation, mis-measurement of the union status variable on even a small number of observations can significantly bias union wage effects downward. One strategy to deal with potential measurement error involves excluding observations that can plausibly be attributable to erroneous reporting of union status. A question in the HILDA Survey starting in Wave 2 asks respondents whether they have changed jobs during the survey year. If workers respond that they have not changed jobs, but they are observed to have changed union status, they are excluded from the sample.[17] Of the 9,302 male person/year observations from wave 2 through wave 6, only 104 male union switchers remained. The analogous numbers for females are 8,814 female person/year observations and 83 union switchers (see Table 4). The exclusion criteria likely increase the quality of the data, but at the cost of not leaving enough observations to subdivide the data into skill categories.

Table 4 about here

The results of estimations using data on union switchers who also changed jobs are located in Table 4. For males the magnitude of the parameter estimates in both the OLS and fixed effects estimates increase somewhat; however, the precision of the estimated union effect in the fixed effects equation drops along with the smaller sample size. Although still statistically significant, it is only marginally so. The pattern of results among females is similar before and after the exclusion of non-job changers. The OLS estimates are still relatively small and statistically significant, while the fixed effects estimates are still small and statistically insignificant.

Another strategy to deal with measurement error is to exclude those individuals with a pattern of union status changes that could plausibly be attributed to measurement error. For example suppose that an individual was in the sample for four years and had a

[17] Here we assume that those who changed both union status and employers are true union switchers, while those who changed union status without changing employers misreported their union status. This may be a plausible assumption given that union density varies across industries and thus employers. However, it should be noted that in Australia a worker can join or quit unions on the same job.

union status history of UNUU or UUNU. It is possible that such a worker was a union member the entire four-year period, but during the second or third erroneously reported his or her union status. It is less likely, however, that an individual with a union status history of UUNN or NNUU erroneously reported a union status change. To reduce the potential effects of measurement error on our results, we re-estimate equation 1, keeping only workers with at least two years of continuous union or nonunion status. The results are reported in Table 4.

The results for males in Table 4 show that although the size of the OLS and fixed effect coefficients both increase, the relative increase is larger for the fixed effect coefficient. Such a result is consistent with the idea that measurement error attenuates the fixed effect estimates of the union wage premium. For females the results are very similar to those found using the unrestricted sample. That is, a relatively small union wage premium estimated using OLS that disappears when controlling for unobserved heterogeneity using the fixed effects model.

Discussion and Conclusions

The results suggest that even after controlling for unobserved heterogeneity using fixed effects estimation, a modest union wage effect remains for male workers. If, as Freeman (1984) suggests, the cross-sectional OLS and fixed effects models provide a reasonable upper and lower bound, the union-nonunion differential for male workers in Australia during the 2001-2006 period lies between 5.2 (fixed effects estimate) and 8.7 percent (OLS estimate). For males it appears as if there may have been a mild uptake in the union-nonunion differential since the reforms of 1997. Using data from the Survey of

Education and Training gathered in 1993, Miller and Mulvey (1996) found a wage differential of only 2.7 percent. Waddoups (2005) found a differential of 3.0 percent, which increased to between 5 and 6 percent by 1997 and continued at that level through 2001, which is about two or three percentage points lower than the estimates reported here. However, given the substantial changes in the structure of collective bargaining, the union- nonunion differential has remained relatively stable. For females, the union wage effect ranges between 0 percent and 4 percent, which is similar to what other researchers have found for female workers (e.g. Waddoups 2008).

The longitudinal data in the HILDA Survey allow us to generate new insights into the nature of the union wage effect in Australia. In particular, the substantially lower estimates of the union wage effect in the fixed effects models suggest that workers in unions have unobserved positive productivity-related characteristics. This indicates that union members tend to have unobserved characteristics that are positively correlated with both wages and the probability of union membership, thus biasing the cross-sectional estimates of the union wage effect upwards. The findings also indicate that the union wage effect varies over the observed skill distribution. Male workers with lower observed skills, whether measured by education or predicted wage, enjoy the largest union premium. The top of the observed skill distribution exhibits either a union wage penalty (in the OLS specification) or no premium (in the fixed effects model). We also find that the Australian data are suggestive of negative sorting into union status at high levels of observed skill; however, the estimates to do not reach conventional levels of significance. The results pointing to positive sorting at low levels of observed skill are stronger. Among females, however, we do not find negative sorting at high levels of observed skill, but there is evidence of positive sorting at lower levels.

Finally, after controlling for unobserved quality differences among male workers, the union effect is significantly diminished in the lower two education categories and the lower three quartiles of the predicted wage distribution. In fact, focusing on the predicted wage distribution of males, we find the only statistically significant union coefficient in the lowest quartile, with no statistically significant estimates in the predicted wage distribution among females. It appears that a significant part of what we have interpreted as union wage effects using cross-sectional data may be just unobserved productivity differences.

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Table 1

Transitions Between Union and Nonunion Status

	Non-union at time t+1	Union at time t+1
Males		
Non-union at time t	6,107	428
Union at time t	421	3,312
Females		
Non-union at time t	6,204	502
Union at time t	451	2,998

Source: Authors computations from the Household, Income and Labour Dynamics in Australia (HILDA) Survey.

Table 2

OLS and Fixed Effects Models of lnWages (Standard Errors in Parentheses)

	Males				Females			
	OLS		Fixed Eff.		OLS		Fixed Eff.	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Union Member	0.0831	0.0135	0.0508	0.0107	0.0394	0.0113	0.0193	0.0106
Age	0.0051	0.0091	0.0294	0.0218	-0.0072	0.0059	-0.0161	0.0185
Age squared	-0.0001	0.0001	-0.0004	0.0002	0.0001	0.0001	0.0001	0.0001
Work Experience	0.0158	0.0041	0.0310	0.0168	0.0158	0.0034	0.0514	0.0127
Work experience sq.	-0.0002	0.0001	-0.0001	0.0002	-0.0003	0.0001	-0.0003	0.0002
Tenure	0.0056	0.0020	0.0028	0.0017	0.0107	0.0018	0.0036	0.0018
Tenure sq.	-0.0001	0.0001	-0.0001	0.0001	-0.0003	0.0001	-0.0001	0.0001
Degree or Post Grad.	0.2835	0.0226	-0.0223	0.0703	0.1844	0.0173	0.0573	0.0569
Diploma/certificate	0.1226	0.0148	0.0182	0.0440	0.0579	0.0134	0.0125	0.0288
Year 12	0.1001	0.0232	0.0070	0.0607	0.0552	0.0165	0.0249	0.0485
Married	0.1008	0.0136	0.0241	0.0131	0.0375	0.0110	0.0005	0.0138
Born Eng.-Speaking Ctry	0.0258	0.0193	---	---	0.0292	0.0154	---	---
Born Non-Eng.-Speaking Ctry	-0.0537	0.0196	---	---	0.0051	0.0160	---	---
Part time	-0.0011	0.0464	0.2077	0.0225	0.0492	0.0109	0.1190	0.0102
Casual	0.0426	0.0182	0.0494	0.0134	-0.0122	0.0238	0.0116	0.0175
Part time*casual	0.0522	0.0552	-0.0839	0.0280	0.0381	0.0265	0.0049	0.0198
Work-limiting health cond.	-0.0701	0.0149	-0.0069	0.0090	-0.0491	0.0135	-0.0116	0.0099
Other white collar	-0.1576	0.0171	-0.0304	0.0105	-0.2365	0.0137	-0.0622	0.0115
Blue collar	-0.2595	0.0187	-0.0229	0.0140	-0.3677	0.0234	-0.0603	0.0204
Workplace size 20-99	0.0951	0.0126	0.0362	0.0089	0.0069	0.0106	0.0044	0.0097
Workplace size 100-199	0.1135	0.0166	0.0508	0.0119	0.0195	0.0159	0.0182	0.0134
Workplace size 200-499	0.1647	0.0158	0.0516	0.0127	0.0715	0.0154	0.0483	0.0143
Workplace size 500+	0.2214	0.0190	0.0575	0.0131	0.1053	0.0152	0.0456	0.0142
Workplace size unknown	0.0633	0.0507	0.0636	0.0333	0.0153	0.0351	0.0162	0.0276
Wave 2	-0.0035	0.0084	-0.0161	0.0189	-0.0065	0.0089	-0.0331	0.0189
Wave 3	-0.0045	0.0084	-0.0345	0.0350	-0.0100	0.0092	-0.0554	0.0351
Wave 4	0.0103	0.0090	-0.0305	0.0516	-0.0254	0.0093	-0.0838	0.0517
Wave 5	-0.0041	0.0098	-0.0580	0.0688	-0.0225	0.0106	-0.1096	0.0690
Wave 6	-0.0017	0.0105	-0.0690	0.0856	-0.0030	0.0098	-0.1063	0.0858
Constant	2.5353	0.1497	1.8193	0.6058	2.8806	0.1016	2.4939	0.6228

Source: Estimates computed from the Household Income and Labour Dynamics of Australia (HILDA) Survey.

Note: A series of industry, occupation, and regional control variables were included. The full results can be obtained from the authors. Hausman test statistic suggests that the fixed effects model is preferred to the random effects model.

Table 3

Estimates of Union Wage Effects by Skill Level and Gender: Unrestricted Sample With and Without Employer Size Controls

	With Employer Size Controls				Without Employer Size Controls					
	OLS		Fixed Eff.		OLS		Fixed Eff.		Number of	Number of
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Observations	Un. Switchers
Males										
Entire Sample	0.0831	0.0135	0.0508	0.0107	0.1027	0.0137	0.0529	0.0107	13272	849
By observed skills										
A. Education										
College Degree or Post Grad.	0.0121	0.0240	0.0366	0.0213	0.0090	0.0253	0.0382	0.0212	3,655	271
Year 12 or Non-degree Qual.	0.0951	0.0180	0.0559	0.0152	0.1219	0.0181	0.0591	0.0152	6,853	408
Less than Year 12	0.1384	0.0283	0.0483	0.0213	0.1609	0.0271	0.0509	0.0212	2,764	170
B. Predicted wages										
4th quartile	-0.0433	0.0262	0.0376	0.0248	-0.0467	0.0267	0.0375	0.0248	3,338	201
3rd quartile	0.0432	0.0222	0.0235	0.0202	0.0591	0.0226	0.0264	0.0202	3,370	226
2nd quartile	0.1484	0.0297	0.0600	0.0216	0.1715	0.0293	0.0646	0.0216	3,343	216
1st quartile	0.1877	0.0256	0.0757	0.0191	0.2001	0.0257	0.0781	0.0190	3,221	206
Females										
Entire Sample	0.0394	0.0113	0.0193	0.0106	0.0463	0.0111	0.0213	0.0106	13251	953
By observed skills										
A. Education										
College Degree or Post Grad.	0.0235	0.0166	0.0189	0.0178	0.0265	0.0167	0.0216	0.0178	4,353	376
Year 12 or Non-degree Qual.	0.0479	0.0161	0.0251	0.0171	0.0573	0.0158	0.0255	0.0171	5,181	354
Less than Year 12	0.0452	0.0256	0.0026	0.0214	0.0537	0.0245	0.0074	0.0213	3,717	223
B. Predicted wages										
4th quartile	0.0070	0.0172	0.0147	0.0204	0.0062	0.0176	0.0158	0.0204	3,456	282
3rd quartile	0.0162	0.0212	-0.0040	0.0218	0.0208	0.0212	-0.0029	0.0217	3,267	257
2nd quartile	0.0686	0.0194	0.0408	0.0208	0.0722	0.0191	0.0427	0.0207	3,307	194
1st quartile	0.0751	0.0292	0.0279	0.0230	0.0920	0.0272	0.0341	0.0230	3,221	222

Note: the estimates are coefficients on the union status variable from equation 1 in the text. The models include a full set of controls as described in Table 2.

Table 4

Estimates of Union Wage Effects Controlling for Mis-measurement of Union Status

	OLS		Fixed Eff.		Number of	Number of
	Coef.	S.E.	Coef.	S.E.	Observations	Un. switchers
Males						
Unrestricted Sample: Waves 2-6[1]	0.0835	0.0144	0.0447	0.0122	10,882	680
Restricted Sample: Keep Job Changers[2]	0.0982	0.0179	0.0473	0.0282	9,203	104
Restricted Sample: Continuous Union/Nonunion Status[3]	0.0954	0.0185	0.0658	0.0169	9,139	178
Females						
Unrestricted Sample: Waves 2-6[1]	0.0347	0.0118	0.0016	0.0118	10,866	756
Restricted Sample: Keep Movers[2]	0.0448	0.0160	0.0386	0.0335	8,814	83
Restricted Sample: Continuous Union/Nonunion Status[3]	0.0407	0.0158	0.0009	0.0166	8,780	204

[1]Note that wave 1 is dropped for these results since the job change variable is not defined in wave 1. Unrestricted sample includes union switchers who did not change jobs; restricted sample exclude union switchers who did not change jobs. A job change va

[2]Waves 2-6 with all union status switchers who did not change employers excluded.

[3]Excludes union status switchers with less than two years in either union or nonunion status. Thus a pattern of NNUU or UUNN would be included, but NUUU, UUUN, or UNUU would be examples of switching patterns that would be excluded. Note that data from Wav

Appendix Table 1

Descriptive Statistics of HILDA Survey Data Used in the Analysis

	Males			Females		
	Non-union	Union	All	Non-union	Union	All
Hourly Wages	21.7	24.5	22.7	18.1	20.8	19.0
s.d.	10.7	10.0	10.5	10.2	10.1	10.3
Age	40.7	42.9	41.5	40.9	43.6	41.8
s.d.	9.9	9.3	9.7	9.5	9.1	9.4
Work experience	21.7	24.5	22.7	18.7	21.6	19.7
s.d.	10.7	10.0	10.5	9.2	9.0	9.2
Tenure	6.0	11.4	8.0	5.1	10.0	6.7
s.d.	7.1	9.5	8.4	5.9	8.1	7.1
Year 11 or below	20.4	19.9	20.2	29.9	19.6	26.4
Year 12	11.6	9.8	11.0	16.6	8.9	14.0
Other Post Sec.Qual.	39.0	44.0	40.8	27.0	24.3	26.1
Degree	28.9	26.3	28.0	26.5	47.3	33.4
Married	75.8	79.1	77.0	72.0	71.0	71.7
Australia born	75.4	79.7	76.9	77.8	78.8	78.1
Born Eng. Speaking Ctry	12.6	10.3	11.8	10.4	10.8	10.5
Born Non-Eng-Speaking	12.1	9.9	11.3	11.8	10.4	11.4
Part-time	8.8	5.2	7.5	45.7	33.6	41.6
Casual	15.6	5.5	12.0	28.5	8.7	21.9
Work-Limiting Health Cond.	15.5	15.7	15.6	13.6	15.7	14.3
Blue collar	33.4	42.7	36.7	10.0	7.9	9.3
Other White Collar	31.4	28.3	30.3	61.6	37.0	53.4
White collar	35.3	29.0	33.0	28.4	55.1	37.3
Workplace size less 20	37.0	20.9	31.3	42.9	18.9	34.9
Workplace size 20-99	31.1	31.3	31.2	29.2	36.3	31.5
Workplace size 100-199	10.1	15.2	11.9	8.2	13.8	10.0
Workplace size 200-499	9.2	15.1	11.3	7.9	13.4	9.7
Workplace size 500+	11.9	17.0	13.7	10.8	16.6	12.7
Workplace size unknown	0.6	0.6	0.6	1.2	1.0	1.1
Mining	2.9	4.5	3.5	0.4	0.2	0.3
Manufacture	19.3	17.5	18.7	7.3	4.0	6.2
Electricity/gas	1.2	2.9	1.8	0.4	0.2	0.4
Construction	7.3	7.6	7.4	1.7	0.3	1.2
Whole sale	7.6	2.0	5.6	4.1	0.8	3.0
Retail	10.5	3.3	7.9	11.5	6.9	10.0
Accom./Restaurant	3.7	1.8	3.0	5.2	1.3	3.9
Transport	5.9	8.4	6.8	2.3	1.4	2.0
Community Services	20.2	10.6	16.8	20.3	8.5	16.4
Government	6.8	11.2	8.4	5.9	7.2	6.3

Source: Computed by the authors from the Household Income and Labour Dynamics of Australia (HILDA) Survey.

Appendix Table 1 (cont.)

Descriptive Statistics of HILDA Survey Data Used in the Analysis

	Males			Females		
	Non-union	Union	All	Non-union	Union	All
Education	5.1	13.8	8.2	13.3	32.9	19.8
Health	4.0	6.9	5.0	22.2	31.4	25.3
Culture	3.2	2.3	2.9	2.6	1.4	2.2
Other Industries	2.4	7.4	4.2	3.0	3.4	3.1
NSW/ACT	30.8	34.4	32.1	30.2	37.1	32.5
VIC	25.6	25.5	25.6	27.4	21.7	25.5
QLD	20.5	20.5	20.5	20.8	20.5	20.7
SA	9.2	7.4	8.5	8.7	8.2	8.5
WA/NT	11.7	9.2	10.8	10.0	8.3	9.4
TAS	2.2	3.0	2.5	2.9	4.2	3.3
Capital city	66.2	59.0	63.6	62.7	60.7	62.1
Wave 1	15.9	16.9	16.2	15.9	16.3	16.1
Wave 2	17.2	17.4	17.3	17.1	17.0	17.1
Wave 3	17.2	17.3	17.2	17.1	17.2	17.2
Wave 4	16.7	16.5	16.6	16.6	16.5	16.5
Wave 5	17.1	16.4	16.8	17.1	16.7	17.0
Wave 6	16.0	15.6	15.9	16.2	16.3	16.2
No. of obs.	8,529	4,743	13,272	8,833	4,418	13,251

Source: Computed by the authors from the Household Income and Labour Dynamics of Australia (HILDA) Survey.