The earnings of casual employees: The problem of unobservables

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2005 HILDA Survey Research Conference
The University of Melbourne
29 September 2005

Abstract

The findings of this paper suggest that female part-time *casuals* earn about 10 percent less than female part-time *permanents*. Three different estimation approaches are used to deal with the problems of bias resulting from unobservable effects: fixed effects estimation, endogenous switching regression, and propensity score matching. All three approaches lead to similar conclusions, suggesting that unobserved ability or motivation are unlikely to account for this wages outcome. The substantive findings show that, compared to permanents, casuals are well rewarded for their educational qualifications, but poorly rewarded for their occupational status. The paper concludes that it is more likely that factors in the workplace, such as the devaluation of casual jobs, contribute more to the lower wages of casuals than do the characteristics which casuals bring to the workplace.

1 Introduction

The word 'casual' has become synonymous with everything that is bad in the labour market. In the same breath, one hears phrases like 'casual, low paid, low skilled' used to describe many of the jobs which have proliferated during the last two decades. Such jobs appear to be 'low quality' jobs:

> contingent work (particularly casual part-time work) continues to be characterised by low pay, limited control and discretion, relative exclusion from workplace decision-making, a lack of task diversity and a high level of dissatisfaction with the amount of work provided by employers (Hall et al., 1998, p. 77).

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Thanks to Jan O'Leary and John Buchanan for valuable feedback on earlier drafts of this paper.

and

> low-paid casual workers may be the least likely to make a lasting transition out of low-paid employment and also be at a higher risk of being caught in a cycle of low pay and joblessness (Dunlop, 2001, p. 109).

From another perspective casual jobs are seen as flexible jobs. They often meets employer needs for 'just-in-time labour': for having the right quantity of labour available when most needed. Employers are seen to benefit by gaining flexibility in their deployment of labour, while employees are seen to benefit by gaining greater choice in balancing work and non-work activities. From within this perspective, casual jobs—particularly if they are part-time jobs—can be seen as desirable jobs:

> ...the persons who are most content with their jobs are those in part-time jobs, and it appears to matter little whether these workers were hired on a permanent, casual or fixed-term basis (Wooden, 2001*a*, p. 65).

and

> ...the evidence presented in this analysis suggest that it is extremely misleading to characterise non-standard jobs as substandard jobs, and that initiatives intended to inhibit the diversity of employment options that are available to employers will in most instances not result in changes in working arrangements that will be unambiguously preferred by employees (Wooden and Warren, 2003, p. 26).

In an earlier analysis (Watson, 2005), I argued that the important issue is not the psychological well-being of the job incumbents, but the integrity of the job structure. I was concerned with whether the wages which casuals earned were fair wages, since fairness is basic to the integrity of the job structure. That analysis showed that casuals fared worse than permanents in the labour market, after controlling for their compositional differences. In this paper I continue this line of inquiry by investigating the wage differential between casuals and permanents within the female part-time workforce. Restricting the population of interest in this way not only makes for a sharper focus on the casual / permanent distinction, but it also locates this study within an important policy setting: the question of whether many part-time jobs are invariably poor-quality jobs?

The international literature on wages suggests that in many countries part-time workers fare poorly compared to their full-time counterparts (Tilly, 1996; Levitan and Conway, 1988; Joshi et al., 1999). In the United States, for example, they earned only 62 per cent of the hourly rate of a full-timer in 1993. Research on the United Kingdom also found part-timers falling behind full-timers. In her contribution to the research on whether women pay a penalty for motherhood, Whitehouse (2002, p. 388) compared Australia and the UK and found that the part-time to full-time ratio for average hourly earnings in the UK was around 80 per cent, a wages gap which has worsened over time (Joshi et al., 1999, p. 561).[1] It has mainly been this decline in the value of part-time work that has contributed to the deterioration of the wages earned by mothers in the UK (Joshi et al., 1999).

Another comparative study which looked at the US, the UK, Canada and Australia found poor results for part-timers in all countries (Gornick and Jacobs, 1996). Among women, part-time workers in the US and UK fared worst, earning just under 80 per cent of a full-timer's wage. In Canada and Australia there was still a wages gap in which part-timers earned around 90 per cent of a full-timer's wage(1996, p. 12). However, more recent research in Australia using the HILDA data found no statistically significant differences between the hourly wages of part-time and full-time female employees (Rodgers, 2002, p. 252).

In his important study, Tilly (1996) draws a distinction between 'retention' part-time workers and 'secondary' part-time workers. The former are generally those more highly skilled employees who convert to part-time work through their own volition, and whose employers facilitate this in order not to lose them altogether. These workers often retain many of the benefits of their full-time counterparts. On the other hand, the many adverse features of part-time work found in the literature are largely confined to Tilly's 'secondary' part-time workers, a group who occupy the familiar 'secondary labour market' identified in the labour market segmentation literature (see, for example, Edwards, 1979). In Australia, the employment category of *part-time casual* comes close to Tilly's notion of secondary part-time workers, whilst the category of *part-time permanent* resembles Tilly's retention workers.

The notion that 'part-time work equals low pay' leads some writers to regard part-time work as a 'trap' which marginalises women in the labour market. Yet part-time jobs are obviously a desirable destination for many workers who wish to balance paid work with other aspect of their lives. In Sweden, for example, part-time work has 'not marginalized women but, on the contrary, has increased the continuity of their labor force attachment, strengthened their position in the labor market, and reduced their economic dependency' (Sundström, 1991, p. 167). But Australia is not Sweden and many of the advantages which part-timers encounter in the Swedish labour market are absent here.[2] What characterises Australia's female part-time

[1] For Australia, the ratio slightly favoured part-time workers.

[2] Marianne Sundström notes the following: highly progressive tax rates, extensive provision

workforce is its high incidence of casualisation: more than 50 per cent of the jobs held by adult workers are casual.

The issue of whether casual jobs are poorly paid jobs matters for at least two important reasons. On the one hand, the level of wages paid to part-time casuals shapes the long-term debate about whether part-time work in Australia should be viewed as a 'trap', or whether it can become an economically viable destination within the labour market. On the other hand, the wages paid to casuals also matter in the short-term. Recent government policy initiatives have been aimed at encouraging women in receipt of welfare payments, particularly those on parenting payments, to enter the labour market. Many of these women will find themselves in casual employment. Indeed, reviewing the first three waves of the HILDA data is quite revealing. Of the 3.2 million adult women who were outside the workforce in 2001, nearly half a million had moved into employment by 2003, the vast majority as employees. Of these, 54 per cent were working as casuals, and 48 per cent were employed as part-time casuals. As Table 1 shows, this is the destination for nearly 220,000 women. By way of comparison, only 90,000 end up working as full-time permanents and 80,000 as part-time permanents. Whether these casual part-time jobs are poorly paid or not has considerable importance for large numbers of women who will enter the labour market in coming years.

Table 1: Destinations in 2003 of those outside the workforce in 2001 (%)

Status of employment	Number	Per cent
Full-time permanent	89,788	19.9
Full-time casual	25,574	5.7
Part-time permanent	79,984	17.7
Part-time casual	217,460	48.1
Fixed term	38,847	8.6
Total	451,654	100.0

Note: Weighted by Wave 3 longitudinal weights.
Source: HILDA Release 3. *Population:* Adult females outside the workforce in 2001 (unemployed or outside the labour market)who became employees in 2003.

There is, of course, some debate about what is 'poor' pay and what constitutes economic viability. In a useful exposition, Jerold Waltman outlines a number of terms which have been used to define earnings in this context: minimum wages, fair wages, just wages and living wages (Waltman, 2004, p. 9). While the *living wage* is the focus for Waltman, and a key aspect

of childcare, generous parental leave and a 'diminishing net-wage differential between full-time working men and part-time working women' (1991, p. 172).

to the Award Safety Net Adjustment process in Australia, for my purposes *fair wages* is more appropriate. To fully assess whether casuals earn a living wage entails considerable research into issues like budget standards (see, for example Saunders, 1998), whereas to assess whether they earn a fair wage is a more modest task. It becomes an issue of pay equity, and of whether any wage gap between casuals and permanents can be adequately explained. This task belongs within a long research tradition which has scrutinised wage gaps, whether these be negative—such as the gender wage gap—or positive—such as the trade union wage premium. These kinds of studies are very useful methodolologically, and I draw upon a number of them in the following analysis.

1.1 Descriptive overview

At first glance, casual jobs do appear to be low paid jobs. In terms of hourly rates of pay one third of part-time casuals are in the bottom quintile (Table 2). By way of comparison, only 16 per cent of part-time permanents are found there. Figure 1 shows the distribution of hourly rates of pay for each category of employment status. Full-time casuals have the most compressed earnings, while part-time casuals have a dispersed earnings pattern. Some of this takes place at the top of the distribution (around $30 per hour and upward) but most takes place at the bottom (below $10 an hour). Part-time casuals differ considerably from part-time permanents: they fall well behind at the top of the distribution (from $20 per hour and upward) and they concentrate at the bottom (below about $12 per hour). In terms of hourly rates of pay, part-time permanents earn about $2.00 an hour more than part-time casuals. On an annual basis, part-time permanents earn about $10,000 more than part-time casuals.

Labour supply explains most of the annual differences. Table 2 shows that part-time casuals are far more likely to be working shorter hours than part-time permanents: nearly sixty per cent of them are working 16 hours or less per week, compared with a figure of 25 per cent among the permanents. Similarly, the proportion of the year spent working differs considerably between part-time casuals and part-time permanents. Nearly 90 per cent of permanents work full-year (much the same as full-time permanents) whereas only 70 per cent of part-time casuals do. Table 2 also shows that fixed term employees have far more in common with full-time permanents than they do with either full-time or part-time casuals when it comes to earnings. (This parallel is weaker when it comes to labour supply).

While the differences in annual earnings can be explained by labour supply, explaining the gap in hourly rates of pay requires investigation of the different characteristics of permanents and casuals. While Table 3 shows all five categories of employment status, in the following discussion—and from now on in this paper—I will concentrate on the differences between

Table 2: Earnings and hours

	Status of employment					
	FT Perm	FT Cas	PT Perm	PT Cas	Fixed term	All
Quintiles hourly rates (%)						
Bottom	16.0	21.7	16.1	32.2	17.3	20.1
Second	19.2	31.7	23.6	19.9	17.9	20.6
Third	20.5	26.1	18.4	19.8	21.4	20.3
Fourth	23.5	13.8	21.0	14.8	26.1	20.9
Top	20.8	6.7	20.9	13.3	17.4	18.2
Usual weekly hours (%)						
16 hrs or less	0.2	0.5	25.0	58.2	10.4	18.3
17 - 24 hrs	0.4	2.2	32.6	23.2	8.7	12.1
25 - 34 hrs	1.9	3.0	42.4	18.6	15.3	14.3
35 - 40 hrs	59.2	69.7	0.0	0.0	36.8	33.7
41 - 48 hrs	20.9	9.9	0.0	0.0	14.4	11.5
49 plus	17.4	14.8	0.0	0.0	14.4	10.0
Period working (%)						
Under 6 mths	2.7	16.4	4.6	13.7	6.5	6.4
6 mths to under 1 year	6.2	21.6	6.2	16.7	13.3	9.8
Full year	91.1	62.0	89.2	69.6	80.2	83.9
Total	100.0	100.0	100.0	100.0	100.0	100.0
Earnings ($)						
Mean hourly rates of pay	19.89	16.52	19.98	17.66	19.20	19.19
Median hourly rates of pay	17.87	15.50	17.33	15.30	18.00	17.00
SD hourly rates of pay	8.60	6.29	10.46	11.13	8.96	9.60
Mean annual earnings	43,047	26,957	23,417	13,107	33,706	31,855
Median annual earnings	40,000	26,000	21,800	10,562	31,500	29,143
SD annual earnings	19,861	14,730	12,481	10,442	19,987	20,784
n	1,556	138	659	738	348	3,439

Note: Weighted by Wave 3 cross-sectional weights.
Source: HILDA Release 3. *Population:* Adult female employees in Wave 3.

Table 3: Demographic characteristics (%)

	Status of employment					
	FT Perm	FT Cas	PT Perm	PT Cas	Fixed	All
Age group						
18 to 25	16.6	33.2	10.9	37.0	21.9	21.1
25 to 34	30.4	22.2	18.8	18.8	25.8	25.0
35 to 44	22.5	19.8	31.9	17.9	23.2	23.2
45 to 54	22.7	20.6	25.6	16.6	25.6	22.1
55 and up	7.8	4.2	12.9	9.8	3.5	8.6
Highest education						
Degree or above	32.1	17.7	25.2	15.9	38.9	27.4
Diploma	11.9	7.6	8.2	8.3	8.3	9.9
Certificate	24.1	27.1	29.1	25.5	24.8	25.5
Yr 12	13.9	18.3	11.6	23.9	14.7	15.9
Yr 11 below	18.0	29.2	25.9	26.4	13.4	21.3
Currently studying						
No	87.2	90.7	85.9	67.4	80.8	82.2
Yes	12.8	9.3	14.1	32.6	19.2	17.8
Marital status						
Legally married	44.1	30.9	66.4	38.9	40.3	46.2
De facto	18.1	13.5	7.5	13.9	22.6	15.5
Separated	3.4	3.0	4.5	1.7	2.8	3.2
Divorced	7.8	10.2	4.9	4.6	9.3	6.8
Widowed	1.4	0.0	2.0	2.5	0.5	1.6
Single †	25.1	42.3	14.7	38.4	24.5	26.7
Children under 5						
No	94.3	96.9	83.3	88.0	94.8	91.1
Yes	5.7	3.1	16.7	12.0	5.2	8.9
Housework per wk						
None	12.8	26.7	6.6	14.7	11.1	12.4
5 hrs or less	32.5	29.0	15.3	28.6	29.9	28.1
6 to 10 hrs	29.7	21.8	23.5	18.2	26.1	25.4
11 to 15 hrs	11.0	9.0	15.5	9.5	11.9	11.5
16 to 20 hrs	7.6	10.1	16.9	8.6	9.4	9.8
More than 20 hrs	6.5	3.4	22.2	20.4	11.6	12.7
Total	100.0	100.0	100.0	100.0	100.0	100.0
n	1,556	138	659	738	348	3,439

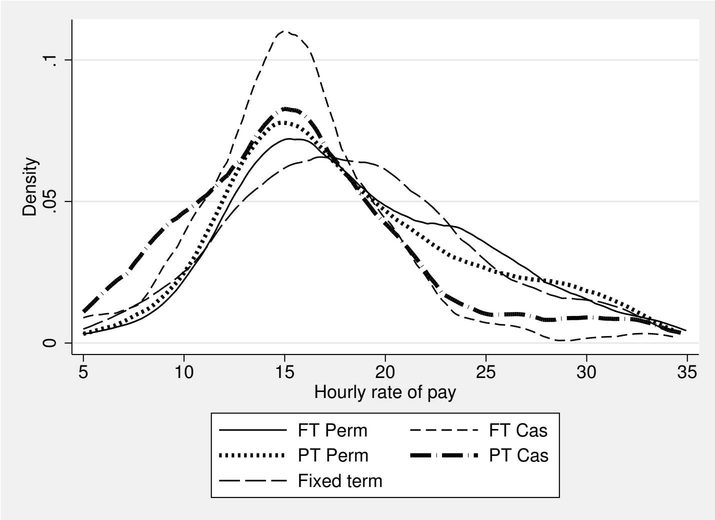
Note: Weighted by Wave 3 cross-sectional weights. † Never married and not de facto.
Source: HILDA Release 3. *Population:* Adult female employees in Wave 3.

Table 4: Workplace characteristics (%)

	Status of employment					
	FT Perm	FT Cas	PT Perm	PT Cas	Fixed term	All
Industry group						
Agric & mining	1.4	5.4	1.1	2.4	1.2	1.7
Manufacturing	10.3	18.3	3.3	3.1	3.5	7.1
Infrastructure	9.7	10.6	3.6	3.8	5.1	6.9
Wholesale & retail	12.7	18.7	16.6	30.2	8.9	17.1
Services	9.8	16.9	7.8	21.3	14.6	12.7
Finance & business	19.0	9.4	12.1	12.1	16.2	15.6
Health	16.4	12.8	35.9	16.4	18.1	20.0
Govt & education	20.6	7.8	19.7	10.7	32.6	18.9
Occupation						
Managers	8.0	1.1	1.5	0.6	4.4	4.6
Professionals	29.5	12.4	26.4	12.5	43.0	25.9
Ass Professionals	15.2	9.6	10.4	6.7	11.7	11.9
Tradespersons	4.2	4.9	1.1	2.6	3.4	3.2
Adv Clerical & Service	6.3	1.3	5.5	4.5	7.0	5.6
Inter Cler, Sales & Serv	23.1	19.0	34.1	28.6	20.9	25.9
Inter Prod & Transport	3.3	11.8	1.4	3.0	2.0	3.1
Elem Cler, Sales & Serv	5.2	15.2	13.7	29.4	5.8	12.5
Labourers	5.1	24.7	5.9	12.0	1.9	7.2
Firm size						
Less than 20 employees	16.5	30.5	24.1	33.6	16.4	22.1
20-99 employees	14.0	20.9	13.4	21.0	16.3	15.9
100-499 employees	11.5	8.1	8.5	9.7	14.6	10.8
500-999 employees	7.9	7.4	5.9	5.2	11.3	7.2
1000-4999 employees	15.0	7.6	7.8	5.2	9.4	10.7
5000 employees or more	29.1	11.0	27.5	14.4	22.6	24.3
Unknown	6.0	14.5	12.8	11.0	9.4	9.0
Union member						
No	65.6	86.3	66.4	88.1	71.9	72.1
Yes	34.4	13.7	33.6	11.9	28.1	27.9
Accessed training						
No	48.2	75.6	54.9	76.0	50.8	56.8
Yes	51.8	24.4	45.1	24.0	49.2	43.2
Job tenure						
Less than 1 yr	16.0	58.0	15.9	36.6	27.4	23.2
1 yr to less than 2 yrs	9.1	12.4	7.7	16.5	12.9	11.0
2 yrs to less than 5 yrs	30.9	18.1	27.6	25.4	32.6	28.8
5 yrs to less than 10 yrs	20.9	6.6	22.4	12.2	14.7	18.1
10 yrs or more	23.1	4.8	26.5	9.2	12.4	19.0
Total	100.0	100.0	100.0	100.0	100.0	100.0
n	1,556	138	659	738	348	3,439

Note: Weighted by Wave 3 cross-sectional weights.
Source: HILDA Release 3. *Population:* Adult female employees in Wave 3.

Figure 1: Distribution of hourly rates of pay



Source: HILDA Release 3.
Population: Adult female employees in Wave 3.

casuals and permanents within the *part-time* workforce. (My reasons for this focus were outlined earlier.) It is evident that casuals are more likely to be younger, to be single, and less likely to have young children. Consistent with their age, their hours of housework are also considerably less. The educational profile of casuals is also quite different. Nearly one third of casuals are currently studying, compared with a figure of about 14 per cent for permanents. Because many are still studying, casuals are less likely to have completed degrees than permanents. However, it is important to note that their ranks are no more likely to be filled with early school leavers than are the ranks of permanents.

The workplace situation also differs considerably between part-time permanents and part-time casuals. Casuals are more likely to be in smaller workplaces, less likely to be trade union members, and more likely to be working in lower skilled occupations (Table 4). Their industry profile is also distinctive: the are concentrated in retail and in various service sectors (hospitality, recreation, personal services). By contrast, permanents are concentrated in the health industry and in government and education. Casuals have only about half the length of job tenure of permanents, and they are only about half as likely to have accessed training in their workplaces.

Regression modelling is used to control for these compositional differences and to calculate the *net effect* of casual status on earnings. The details of this regression modelling will be discussed in section 3.2 below. While these results are largely consistent, and quite robust to different estimation methods, their interpretation is complicated.

Casual status is a product of Australia's award system, a unique employee classification which is often empty in its definition—"a casual is someone defined as such"—but profound in its consequences. In particular, the vast majority of casuals receive an hourly loading on top of their ordinary time rates. This loading varies between 15 per cent to over 30 per cent (Watson, 2005) and is partly paid in lieu of annual holiday leave and sick leave, which casuals do not receive. The existence of such a loading makes interpretation of the differences in hourly wages between casuals and permanents complicated. Wage 'parity' between casuals and permanents would not translate into a wages gap close to zero because of the existence of these loadings. Indeed true parity would be evident if the wages gap on hourly wages favoured casuals by about 10 to 15 per cent. Anything less would constitute a wages deficit. However, being more precise than this is difficult.

The size and prevalence of these loadings are unknown for the sample in HILDA because this data item is not available. Not all casuals receive it, but the vast majority do (Wooden, 2001*b*). As well as compensation for lost leave, casual loadings are also inserted into awards to curb the growth of casualisation. They are thus a penalty on employers, as well as a compensatory payment for casual employees. The actual 'lost earnings' through non-payment of holiday and sick leave, is difficult to calculate because the loading is a composite figure. Shifting focus from hourly earnings to annual earnings does not solve the problem. Thirty per cent of casuals report that their most recent pay was not their usual pay, so summing their usual hours across the proportion of the year for which they were employed will not produce accurate annual hours totals. Consequently, one cannot deduce the true value of their loadings by working backwards from annual earnings.

In summary, problems of measurement error make quantifying the wages gap difficult because one cannot be certain what constitutes parity. Two strategies are available for dealing with this. The first makes use of the hourly rate of pay as reported, and judges the results in knowledge of the existence of loadings. The second follows a strategy used by Dunlop (2000), and followed by Watson (2005), of 'discounting' the hourly rate of casuals by a fixed amount to take account of the payment of loadings (Dunlop used a figure of 18 per cent; in this paper I use a more conservative figure of 15 per cent). While for most purposes there is little difference between correcting the final output by 15 per cent, and modifying some of the input by 15 per cent, there are times when interpretation of the results is clearer using one method rather than the other. The key point is recognising that wages parity between casuals and permanents should be defined as a wages gap which *favours* casuals by about 10 to 15 per cent. Anything considerably less than this constitutes unfair pay.

2 Estimation: the problem of unobservables

Earlier research suggested the wages gap—taking into account the loadings—favoured permanents by over 10 per cent (Watson, 2005). The research for this paper largely confirms that finding, and the details will be discussed at length in section 3.2. Regression modelling takes account of the observable characteristics of individuals and their workplaces, and to that extent its results can indicate the net effect of casual status on wages. However, as with all wages modelling, unobservable effects which may be correlated with casual status can induce bias in the results. Dealing with omitted variable bias has been one of the prime motivators for developments in panel data analysis (Wooldridge, 2002, p. 247), though a range of strategies are also available using cross-sectional data. My approach in this paper spans both.

In seeking to explain the wages gap, neo-classical economic theory might suggest that casuals earn less because they are less competent. From this perspective, the unobserved characteristics represent unobserved ability or motivation. The logic behind this view flows from the assumption that employers are often risk averse in their engagement of labour, and may therefore chose to employ on a casual basis someone whom they regard as an unknown quantity. After a period of probation, which provides an opportunity for assessing the employee's ability, the employer may offer that person permanency. Over time, this leads to a non-random sorting of employees into casual and permanent status on the basis of ability, something evident to the employer but unobserved by the researcher. Unobserved factors, like ability, are also likely to be correlated with wage outcomes, but will not be evident in the regression modelling which only controls for observable characteristics.

An alternative perspective would see the wage outcomes reflecting less the characteristics which casuals bring to the workforce, and more the conditions of their employment. These include a lack of training and opportunities for skills progression, as well as limited career paths in internal labour markets. For many casuals, their work experience consists of a succession of short-term dead-end jobs, with little opportunity to enhance their wages except through working unsociable shift or rosters. While some of these factors can be incorporated into regression modelling—such as training and tenure—the problem of unobservables remains, particularly for those elements of discrimination or unfair treatment within the workplace which go unmeasured. Lack of access to more responsible classifications, such as supervisory positions, is not captured well in the occupational data. Nor is the content of work readily observable. From within this perspective, casuals earn less because they are devalued, but capturing this dimension of their work is elusive.

These competing hypotheses are mirrored in the marriage premium debate. As Chun and Lee (2001, p. 307) put it: do married men earn more because they become more productive after marriage, or because they were already more productive before marriage and this made them more attractive in the marriage market? Translating this into casuals and permanents: do the former earn less because of the conditions under which they work, or do they earn less because their lower ability selects them into casual employment. The strongest parallel for this problem is the tradition of research into the trade union premium (Lee, 1978; Duncan and Leigh, 1980; Mellow, 1981; Freeman, 1984; Jakubson, 1991).

I pursue three strategies for dealing with this problem of unobservables: fixed effects estimation; endogenous switching regressions; and propensity score matching estimation. In the case of the first approach I use HILDA's panel data, and for the second and third I use HILDA's Wage 3 cross-sectional data. Before discussing my use of these data, I outline these three estimation methods in sections 2.1 to 2.3. All have particular weaknesses, but it is my contention that if they all point in the same direction, then the conclusions in this paper should be regarded as robust. While these approaches use different terminology, they all confront the same problem of establishing the net effect of some variable, free of omitted variable bias. Fixed effects estimation deals with the potential problem that 'unobservable effects' may be correlated with one or more of the regressors, leading to biased estimates of their effects on the outcome. Endogenous switching regression belongs within the family of sample selection models, which attempt to model whether unobservables which influence selection into a subsample are correlated with unobservables which influence the 'behavioural' outcome.[3] Finally, propensity score matching estimation tries to take account of 'hidden bias', a potential confounder which influences both selection and 'treatment' outcomes. Thus while all three approaches use different terms, and focus on the problem of potential bias through different prisms, their goals are essentially the same.[4]

My main goal is also dealing with omitted variable bias, and I use the shorthand phrase, 'the problem of unobservables' to encapsulate this task. However, the substantive findings on the determinants of casual earnings are also of great interest, and these are discussed in section 3.2 below. Indeed, linking the two areas forms the basis for my conclusion and allows me to return to the key themes raised in the introduction.

[3] Self-selection bias is succinctly defined by Fishe et al. (1981, p. 172) as follows: 'The problem of selectivity bias arises when, say, a specific feature of an individual is to be examined and we ignore the decision process involved that generated the observed data.'

[4] Instrumental variables estimation can also be used for this goal. See Heckman (1990) for an overview of selection bias and instrumental variables and DiPrete and Gangl (2004) for propensity score matching and instrumental variables.

2.1 Fixed effects estimation

If one assumes that casual status only enters the determination of wages as an intercept effect, then a regression model with casual status as a dummy is appropriate. The wages equation takes the form:

$$y_i = \alpha + \beta \mathrm{x}_i + \delta c_i + \epsilon_i \tag{1}$$

where the subscript i represents the ith individual, y_i is the log of hourly wages, α is the y-intercept, x_i is a vector of demographic and workforce characteristics, c is a dummy for casual status and ϵ_i is the usual disturbance term. Interest centres on the sign and magnitude of δ. A negative coefficient indicates that, relative to permanents, casuals are paid less after controlling for a range of observable characteristics. The results for this model are shown in Tables 5 and 9 and indicate that the coefficient on the casual dummy is 0.02 and is not statistically significant.

The key problem with the dummy intercept approach is the possibility of endogeneity, that an unobservable effect which is correlated with casual status is also influencing the wages outcome. This problem has been extensively studied in the literature on union wage effects and has been one of the main reasons for the enthusiastic adoption of panel data models. As Hsiao (2003, p. 4) argues, cross-sectional estimates 'are likely to reflect interindividual differences inherent in comparisons of *different* people' whereas estimates based on panel data can examine the wage differential as the same worker moves from unionised to non-unionised jobs and vice versa.[5]

The most useful panel data model for my concerns is fixed effects estimation, which deals with the problem of unobservables by 'sweeping' them away. Panel data, such as the three waves of HILDA, provide repeated observations on the same individual, and this allows one to include in a wages model an additional person-specific error term (v_i) which does not vary over time:

$$y_{it} = \alpha + \beta \mathrm{x}_{it} + v_i + \epsilon_{it} \tag{2}$$

where the subscript it represents the observation for the ith individual in period t. While ϵ_{it} is the usual error term (which varies across both the individual and the period), the v_i term only varies between individuals. It represents the unobserved effect. It is assumed that ϵ_{it} is uncorrelated with the regressors, thereby satisfying the requirement for exogeneity in the model. Eliminating the person-specific time-invariant variables from the model also

[5] Similarly, movement from casual to permanent provides an opportunity, at least in theory, for isolating the 'pure' wage differential. The 'wage change' approach explicitly models such movements (see, for example Mellow, 1981) but its usefulness for my purposes is limited. Examining the HILDA data shows that any movement by the same person from casual to permanent status is accompanied by a drop in hourly wages, invariably because the (unmeasured) casual loading is lost from the hourly rate.

eliminates unobservables. This follows from the underlying strategy of fixed effects estimation, which deals with the unobserved effect (v_i) by allowing it to be arbitrarily related to the observed variables in x_{it} (Wooldridge, 2002, p.266). It is then swept away when the other time-invariant variables are removed from the equation during the time-demeaning transformation:[6]

$$(y_{it} - \bar{y}_i) = \beta(\mathrm{x}_{it} - \bar{\mathrm{x}}_i) + (\epsilon_{it} - \bar{\epsilon}_i) \tag{3}$$

In the same way that time-invariant regressors in x_{it} are removed by this transformation, so too are the unobservables ($v_i - v_i$). Using this approach, equation 3 is fitted to pooled data from all three waves of HILDA, employing the same specification as used in the pooled cross-sectional wages equation. The results of this modelling are discussed in section 3.2 below. In summary, this modelling results in a coefficient of 0.03 for casual status, a result which is not statistically significant (see Tables 5 and 9).

The major shortcomings with fixed effects estimation is that it relies on considerable within-individual variation to produce interesting results. Moreover, individuals with no variation on some variables do not contribute to the analysis for those variables (Petersen, 1993, p. 448). When the data only span three time periods, as is the case with the HILDA data, this problem is quite acute. It is notable in the comparison of the OLS and fixed effects estimates (Table 9) that many of the results are comparable, but most of the estimates in the fixed effects model are not statistically significant. Despite the potential advantages of panel data, estimation using cross-sectional data remains essential for dealing with the problem of unobservables.[7]

2.2 Endogenous switching regression

It is also possible to directly adjudicate between the two rival perspectives: the view that casuals earn less by virtue of pre-existing characteristics versus the view that the work they do is devalued. This strategy involves directly modelling the selection effects on the wages of casuals and permanents. Of course, one would expect to find non-random sorting into these two categories of employment because each group is quite distinctive in terms of their

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[6] In this procedure each variable's observations have subtracted from them the mean value for all other observations for that individual. It is called the *within transformation* because it averages the values across the time periods within each individual. See Wooldridge (2002, pp.266-267).First differencing can also be employed to model fixed effects. It is important to note that other variables may be time-constant for some individuals, but not for others. These variables are retained in fixed effects estimation because they have varied for some individuals (Wooldridge, 2002, p.266).

[7] Indeed, Mroz (1999, p. 257) argues: 'Cross-sectional data with precise endogeneity controls might yield better estimators than longitudinal data with fixed effects approaches.'

demographic background. *The critical issue, however, is whether the unobservable factors behind selection are correlated with the unobservable factors which influence wages outcomes.* This can be modelled using an endogenous switching regression approach, a two-stage estimation technique which models the selection process (using probit regression) and then incorporates these results into modelling the wages outcomes (using OLS regression). Specifically, if the error component of the selection equation is correlated with the error component of the wages equation, then one can argue that selection into casual employment is endogenous to the wages paid to casuals. This constitutes evidence for the notion that the abilities brought to the workplace are influential in wage outcomes. On the other hand, if there is no correlation, then one must look elsewhere than the selection process. One possibility, explored in the conclusion of the paper, is that the answer lies in the nature of the work itself, and its devaluation within the workplace.

The literature on union effects and the gender wage gap both highlight problems of sample selectivity. The possibility of sample selection bias arises because the unobservables which influence selection into one subsample may be correlated with the unobservables which influence the wage outcomes. Specifically, if some kind of non-random sorting based on unobservable characteristics influences the composition of one group of workers (such as women, unionists, or casuals), then OLS estimates of their earnings will be biased by this selection effect. Such non-random sorting may arise through self-selection, selection by employers, or both. By explicitly taking account of such selection processes, one can correct this bias in the estimates of wage differentials. In addition, as suggested above, testing for this selection effect is one strategy for directly confronting the problem of unobservables.

Before implementing an endogenous switching model, any prior selection effects must also be accommodated. As is well known, OLS regression estimates of the wages of women who undertake paid work are likely to be biased by their non-random selection into the workforce (Gronau, 1974; Heckman, 1979; Vella, 1998). The usual estimation method entails fitting a selection equation, calculating the inverse mills ratio, and incorporating this ratio as a correction factor into the OLS regression on wages. In this paper I employ a double-selection approach (Krishnan, 1990) in which the first stage entails modelling the participation decision and the second entails modelling the 'regime' or sector in which the worker finds employment as either a casual or permanent (this is the switching regression selection component). Because the truncation of the sample for the second selection model is restricted to part-time workers, the initial selection model involves more than one outcome: no employment, self-employment, part-time employment as an employee or full-time employment as an employee. These outcomes are assumed to be contemporaneous, rather than sequential, and are therefore modelled using a multinomial probit model (see Lee (1995), Tansel (2004) and Rodgers (2002) for examples of polychotomous selection models). The correction factor from this model is used in the subsequent probit model for selection into casual employment, and the correction factor from this model is used in the wages equations. It is this last stage in the process which employs the switching regression model.

The switching regression approach assumes that the wages of individuals are determined by two regression equations, with a 'criterion function' determining which of the equations is appropriate (Maddala, 1983, pp. 283ff). The model assumes two regimes:

$$\text{Regime 1:} \quad y_{1i} = X_{1i}\beta_1 \; + \; u_{1i} \tag{4}$$

$$\text{Regime 2:} \quad y_{2i} = X_{2i}\beta_2 \; + \; u_{2i} \tag{5}$$

with a criterion function based on the latent variable:

$$I{*}_i = Z_i\gamma \; - \; \epsilon_i \tag{6}$$

The observed y_i is defined as:

$$y_i = y_{1i} \quad \text{iff } I_i = 1 \tag{7}$$

$$y_i = y_{2i} \quad \text{iff } I_i = 0 \tag{8}$$

with $(u_1, u_2, \epsilon)' \sim N(0, \sigma)$. Let ρ_1 be the correlation between u_1 and ϵ and ρ_2 the correlation between u_2 and ϵ. If either $\rho_1 \neq 0$ or $\rho_2 \neq 0$ then the error component of the selection equation is correlated with the error component of the wages equation. In other words, selection into a particular regime—such as casual status—is endogenous to wages. This implies that the unobserved characteristics which influence the probability of being in the casual regime are also likely to influence the wages an individual receives once she becomes employed (Lokshin and Sajaia, 2004, p. 287). In the modelling reported in section 3.2 the 'selection effect' for both casuals and permanents is not statistically significant, suggesting that selection is not endogenous to wages.

Analysis of sub-group wage outcomes have used switching regressions since the late 1970s and have become popular again recently. They have been used extensively in studies of the wage differential between public sector and private sector employees (Ophem, 1993; Heitmueller, 2004; Tansel, 2004; Lokshin and Jovanovic, 2003; Adamchik and Bedi, 2000); union and non-union sectors (Lee, 1978; Hartog et al., 2000); part-time and full-time workers (Hotchkiss, 1991); the impact of employer size on the wages gap (Main and Reilly, 1993); and in studies on the impact of marriage on men's wages (Chun and Lee, 2001).

The endogenous switching approach has faced criticism. As early as 1981, for example, Poirier and Rudd (1981) raised doubts as to the appropriateness of endogenous switching in some applications. In his exposition of the approach, Maddala showed that these reservations were unfounded (Maddala, 1983, pp. 283–289). More generally, the overall two-step sample selection approach—in which a selection correction effect is calculated and then introduced into a second equation—has been compared unfavourably with two-part models—where there is no selection effect introduced (Manning et al., 1987). These criticisms—based on Monte Carlo simulations—were subsequently answered by Leung and Yu (1996), who argued that the poor results for sample selection models were due to design flaws in the simulations. When these flaws were removed, the poor performance of the sample selection models disappeared and Leung and Yu (1996) concluded that sample selection models do perform well under the appropriate conditions. Their vulnerability to collinearity problems, however, is something which must be addressed. They argue for testing whether there is collinearity between the inverse Mills ratio and the regressors using condition numbers. The selection model results, presented in section 3.2 below, were tested in this way and no evidence was found to suggest problems of collinearity.

2.3 Matching estimators

Another approach to modelling the influence of selection on outcomes involves matching estimators. While they have been used to explore wage gaps (Bryson, 2002), they are more often found in the program evaluation literature, particularly assessing whether training or labour market programmes improve employment or wage outcomes (see, for example, Aakvik (2001); DiPrete and Gangl (2004); Imbens (2003); Dehejia and Wahba (2002)). This approach maps easily onto differences in wage outcomes between permanents and casuals, with casual status viewed as a form of program participation or 'treatment'.

The basic idea behind matching estimators is that one group of people participate in a program—undergo treatment—while another group does not, and one assesses the effectiveness of the treatment by comparing average outcomes. Because the data is observational, rather than experimental, there is no random allocation of individuals to the program. Instead, a matching process—based on observed attributes—is used to compare participants and non-participants (or casuals and permanents). In the language of treatment effects: 'For each i, matching estimators impute the missing outcome by finding other individuals in the data whose covariates are similar but who were exposed to the other treatment' (Abadie et al., 2004, p. 292). Matching on covariate patterns is not always practical, particularly if there are a large number of covariates, because some patterns may lack matches between participants and non-participants. Consequently propensity scores—the probability of receiving treatment conditional on covariates—are favoured as one way to reduce this problem of dimensionality (Dehejia and Wahba, 2002, p. 153).

Once sample members are matched on their propensity scores, the effect of their participation in a program—or in this case, their employment as casuals—can be assessed by way of 'average treatment effects on the treated' (ATT). Bootstrapping can be used to estimate standard errors for the ATT estimates.

Using propensity score matching relies on two key assumptions. The first, termed 'selection on observables', requires that the assignment to treatment is independent of the outcomes, conditional on the covariates. (This is also termed the CIA, conditional independence assumption). The second assumption is the 'common support' or overlap condition. People with the same covariate values cannot all fall into one category (perfect predictability): they must have a positive probability of being either participants or non-participants (Abadie et al. (2004, p. 292) and Caliendo and Kopeinig (2005, p. 4)).

The CIA assumption is central to my concerns in this paper. It assumes that the effects of casual employment are not influenced by any correlation between unobserved factors and a person's selection into casual employment. The potential violation of this assumption is essentially the same problem which switching regression confronts: the potential endogeneity arising from selection bias. As discussed in the last section, switching regression deal with this problem by way of a two-step estimator, which incorporates a selection effect into the wages equation. If that selection effect is not statistically significant, one can conclude that self-selection bias is not present.

In the case of propensity score matching, there is no equivalent statistical test. The CIA assumption cannot be directly tested, because the overall approach relies on non-experimental data. One way to address the issue of whether treatment effects are influenced by unobservables makes use of 'Rosenbaum bounds' (Rosenbaum, 2002; DiPrete and Gangl, 2004). This approach begins by accepting the CIA assumption, but then postulates the existence of unobservables which influence assignment to the treatment. These can be viewed as a form of 'hidden bias'. One then conducts sensitivity analysis to assess how strong the influence of these postulated unobservables would have to be in order to influence the treatment outcomes.

As outlined by Caliendo and Kopeinig (2005, p. 19–20) sensitivity analysis makes use of the odds ratios of participating in a program between two matched individuals, i and j. Let the probability of participation be expressed as:

$$P(x_i) = P(D_i = 1|x_i) = F(\beta x_i + \gamma v_i) \tag{9}$$

where D_i equals 1 if the individual receives treatment, 0 otherwise; x_i are the *observed* characteristics of the individual i, v_i is the *unobserved* variable, and γ is the effect of v_i on the participation decision. If there is no hidden bias, then γ will be zero and the participation probability will be based entirely on the effects of x_i. However, in the presence of hidden bias, the two matched individuals, i and j, will have different chances of participation. The odds ratio for participation is given by $exp[\gamma(v_i - v_j)]$. Now if the individuals have identical observed covariates, which is implied by the matching, then the x vector of observed characteristics is cancelled out. It follows from this that if their odds of participation differ—that is, if the odds ratio departs from a value of 1—this can only be due to hidden bias. Either, it is due to differences in unobserved covariates ($v_i \neq v_j$) or the effect of these unobservables being nonzero ($\gamma \neq 0$). Sensitivity analysis evaluates how much the programme's effects (the effects on wages of casual status) are altered by changing the values of γ and $(v_i - v_j)$. In practice, this means examining the bounds on the odds ratio for participation that lie between $1/e^\gamma$ and e^γ. As Caliendo and Kopeinig (2005, p. 20) conclude:

> Both matched individuals have the same probability of participating only if $e^\gamma = 1$. If $e^\gamma = 2$, then individuals who appear to be similar (in terms of x) could differ in their odds of receiving the treatment by as much as a factor of 2. In this sense, e^γ is a measure of the degree of departure from a study that is free of hidden bias.

Sensitivity analysis is used in section 3.2 below and gives some insight into how much the wages outcome of 'treatment' as a casual would change as the values of e^γ are varied. While this approach does not formally test the (untestable) CIA assumption, it does provide a way of judging how great the influence of unobservables would need to be for the substantive results of treatment to change (Rosenbaum, 2002, p. 106).

The results from conducting propensity score matching are reported in section 3.2 below. They confirm the results of the OLS pooled data, that casuals fare worse than permanents. The results of the sensitivity analysis also confirm the switching endogenous findings. Before turning to look more closely at all these results, I outline the data and modelling steps undertaken.

3 Analysis and findings

3.1 Data and modelling

For the analysis in this paper I draw upon the unit record files from the HILDA Survey, a national survey carried out by the Melbourne Institute on behalf of the Federal Department of Family and Community Services.[8] I make use of the panel component of HILDA, as well as cross-sectional

[8] For details, see www.melbourneinstitute.com/hilda, and Watson and Wooden (2002).

data from Wave 3 (2003).[9] The population of interest in this analysis is female part-time employees, for the reasons outlined in the introduction.[10] About half of these workers are employed as casuals, and half as permanents, and the sample size is just over a thousand observations (depending on the model).

Ideally, the dependent variable for this modelling should be annual earnings. This measure would take account of the casual loadings paid in lieu of unpaid holiday and sick leave. Unfortunately, as outlined in section 1, annual earnings are difficult to derive with any accuracy because the weekly wages of a large proportion of casuals are irregular. Consequently, hourly rates of pay are used, and these are calculated by dividing usual weekly wages by usual weekly hours.[11] The natural log of hourly wages is used in the regression modelling as the dependent variable. In addition, for comparison purposes a 'discounted' hourly rate is also used in some of the modelling. This rate reduces the hourly rate of casuals by 15 per cent to take account of the payment of a loading. As noted earlier, not all casuals receive a loading, so this procedure introduces some degree of measurement error. Finally, the wages variable used in the fixed effects estimation is adjusted for the CPI.

The definition of a 'casual' has spawned a considerable literature, much of its focused on job security and on anomalies like the 'permanent casual' (see, for example, Murtough and Waite (2000); Campbell and Burgess (2001); Owens (2001)). The proportion of employees who are deemed casual can vary considerably. Using a 'leave entitlements' definition, the figure is as high as 27 per cent. Using a 'self-identification' definition, the figure drops to 18 per cent. Finally, using a definition which focuses on variable earnings and no ongoing employment prospects, the figure drops to 11 per cent. In this paper I follow Wooden and Warren (2003) and Watson (2005) and use a 'contract of employment' definition.[12] This has the considerable advantage

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[9] The households for this survey were selected using a complex sampling design, involving both stratification and clustering. These sample design aspects of HILDA need to be taken into account when analysing the data since they impact on the size of the standard errors. Fortunately, the HILDA dataset provides identifiers for this sample design allowing the design effect to be corrected for. The descriptive statistics incorporate the appropriate weights, and the cross-sectional regression models were estimated using Stata's survey regression estimators, procedures which take account of the design effect. See Stata (2005). The fixed effects estimation does not incorporate either weighting or other aspects of the survey design since selectivity effects are 'swept' away with the unobservables in the time-demeaning transformation.

[10] Further restrictions include omission of those under 18, since junior rates will distort the earnings estimates. Those on fixed-term contracts are also excluded, since their characteristics suggest they have far more in common with permanents than casuals (yet combining them with permanents is also unwarranted.)

[11] Outliers are dealt with by recoding the lowest to \$5 an hour, and the highest to \$100 an hour.

[12] As Wooden and Warren (2003, p. 8) showed, 'access to leave entitlements is highly

of distinguishing permanent and casual employees from fixed term employees. The latter are clearly an anomaly: their profile is much closer to that of permanents, but their job insecurity parallels that of casuals. In this paper, they are omitted from the analysis because the casual / permanent contrast is the key focus in this paper and issues of precarious employment—while of considerable importance—are not part of my object of inquiry. The numbers of female part-time fixed term employees is very small (107 observations), and like the exclusion of junior workers, their omission serves to remove another unnecessary confounder from the analysis.

The controls used in the wages models consist of demographic, workplace and geographical variables. Given the nature of women's broken workforce participation, age is entered as a categorical variable. Occupational tenure and its square are entered in the usual fashion (since these are less subject to broken work patterns) and job tenure is in linear form. Education is also entered as categorical, in order to capture the non-linearity in its effects. One novel aspect to the modelling is the use of an occupational status score (entered in log form). The reason for this is that the finer gradations of occupational classification can make a some difference when comparing earnings between subgroups who do not differ much on other characteristics. While HILDA only provides ASCO 2 digit occupational codes, it does provide the ANU occupational status codes, which are themselves derived from 4 digit occupational codes. Finally, geography is included to capture labour demand aspects, both at the state-wide level and at the local labour market level. While access to training clearly differs in the bivariate results reported earlier, in preliminary multivariate analysis it did not prove to be statistically significant. It has been omitted from the modelling because the data item is only available for Wave 3, and I have sought to make the model specifications comparable across all estimation approaches (and thus variables must be available in all waves).

The participation selection model uses a number of controls which have been commonly employed in studies on female labour supply (Nakamura et al., 1979; Mroz, 1987). As well as education, studying, marital status and geographical variables, the controls also include two sets of childcare variables (children under 5 and children 6 to 18); receipt of government income (in thousands of dollars); hours of housework per week and renting in public housing. These last set of controls reflect some of the constraints known to influence female labour force participation. The means and standard deviations for all the control variables are shown in Table 13.

Identification of the switching regression model is achieved by including some variables in the selection model which are absent from the wages model.[13] In this particular case, two variables were used: working shifts and

correlated with self-reported casual employment status'.

[13] The three equations (one selection and two wages) are identified even if the same vari-

working very short hours (10 or less per week).

3.2 Results

As mentioned in section 2, the results appear to reject the notion that unobservables associated with selection into casual status are correlated with unobservables which influence wage outcomes (the endogenous switching regression results). They also suggest that unobservables play a minor role in the influence of casual status on wage outcomes (the fixed effects results). Finally, while hidden bias cannot be completely ruled out, the 'treatment' effect of casual status on wages seems likely to be free of unobservable confounders (the propensity score matching results). In this section, I present the details for each of these findings. While the focus of this paper has been on observables, there are some interesting substantive results from the modelling which are worth commenting on. They provide insights into why casuals earn less than permanents and their implications will be pursued in the conclusion to this paper.

The OLS results for the pooled model shows that the earnings differential between casuals and permanents is 0.02 and is not statistically significant (see Table 9).[14] If the dependent variable is discounted by 15 per cent for casuals—to take account of the casual loading—the coefficient becomes –0.16 and statistically significant.[15] This result for casual status is consistent with other research using HILDA. Analysing Wave 1 data, Rodgers found a statistically non-significant coefficient of -0.002 for casual status among female part-time employees (Rodgers, 2002, p. 249).

The other results also provide useful insights into how part-time workers fare in the labour market. Growing older increases wages up to a certain stage, and then its effects decline. Educational outcomes are what one might expect, with degrees and diplomas highly rewarded. Working in a small workplace incurs a wages penalty, while union membership proves to be an asset. The occupational variable is significant and indicates that a one unit increase on this scale is associated with a 19 per cent increase in

ables feature in both, because of the non-linearity of the functional form. However, relying only on the functional form for identification produces unstable estimators and does not link strongly to the structural equations. Stronger identification is preferable, and can be achieved by including some variables which influence selection but do not influence individual wages. See Lokshin and Jovanovic (2003, p. 13), Adamchik and Bedi (2000, p. 209) and Vella (1998, p. 135) for further discussion of this issue.

[14] Because the dependent variable is the natural log of hourly wages, the coefficients can be interpreted as percentages when the independent variables is continuous. When it is categorical, small values are relatively close to percentages. Otherwise, the formula $100(e^{\beta} - 1)$ can be used to convert coefficients into percentages.

[15] Naturally, the other coefficients in the model do not change (and the results of this modelling are not shown because only the casual status variable differs).

Table 5: Summary of coefficients for casual status (hourly wage rates)

Estimation method	Normal rates	Discounted rates
Pooled sample		
OLS	0.02	–0.16
Fixed effects	0.03	–0.13
Propensity matching	0.05	–0.12

Note: All results with normal rates are not statistically significant. All discounted rates statistically significant at $p < 0.001$. *Population:* Adult female part-time employees (excluding those on fixed-term contracts).

the hourly rate of pay. Living outside cities incurs an earnings penalty, as does living outside New South Wales.

The fixed effects estimation results show very few statistically significant results (Table 9). This is to be expected, given the small amount of variation captured by the within estimator and the small number of panels used for this analysis. The magnitude of the coefficients are smaller, in general, than the OLS results. The coefficient on casual status is 0.03 and is not statistically significant. When the model is estimated using discounted wages as the dependent variable, the coefficient becomes –0.13. Thus the fixed effects estimation gives similar results for casual status to the OLS estimation on the pooled sample (0.02 and –0.16). In other words, using panel data to 'sweep' away the time-invariant unobserved effects leads to essentially the same substantive findings as modelling the data in cross-sectional form. This suggests that unobservables which are correlated with casual status do not appear to be influencing the wages gap.

As discussed earlier, the switching regression approach involves two selection models (Tables 10 and 11) and a wages equation (on a split sample). For comparison purposes an OLS wages equation (on the same split sample) is also shown (Table 12). The model of selection into working compared three outcomes (full-time employee, part-time employee and self-employed) against a base outcome (not in paid work). This model was estimated using multinomial probit. The results are illuminating and suggest that age is an important factor for full-time employment and self-employment, but not for part-time employment. Among the two former groups, the probability of working full-time increases steadily until mature age, and then drops off. Education is a major contributor to labour force participation, but this is stronger among full-timers than part-timers. As one might expect, studying inhibits full-time work, but it encourages part-time work. Having children under 5 constrains full-time work, but has no effect on part-time work or self-employment, whilst having children aged 6 to 18 encourages all three forms of employment. The more hours of housework, the less likely an individual will work, and this is more pronounced for full-timers than for part-timers and the self-employed. Tenancy in public housing is also associated with a reduced likelihood of employment. Living in inner regional areas is associated with an increased likelihood of paid work for all three forms of employment, but only self-employment benefits from the other geographical areas (and even the remote category is associated with an increased probability of employment for the self-employed).

Selection into casual status is modelled using binomial probit and incorporates the selectivity effect constructed from the multinomial probit model. This selectivity effect is not statistically significant, suggesting that the unobservables which influence part-time employment are not correlated with the unobservables which influence selection into casual status. Among the observable factors, the results are largely what one would expect. Union membership is negatively correlated with casual status, as is length of job tenure. As discussed earlier, identification in the switching regression approach relies on the inclusion of some variables which are correlated with selection, but not with wages. Working very short hours (10 or less per week) and working shifts were included for this reason, and the probit results confirm their inclusion: both are highly statistically significant.

The estimation results for the wages models are shown in Table 12, which presents separate casual and permanent findings, and compares OLS results with those from the switching regression approach. In terms of the main theme in this paper—the influence of unobservables—these results indicate that selectivity effects for both casuals and permanents are not statistically significant. This suggests that unobservables which influence selection into casual status are not correlated with unobservables which influence wage outcomes.

The results also justify the split sample, since the impact of several key variables differs between casuals and permanents. In particular, casuals are rewarded for their educational qualifications more strongly than are permanents, particularly if they hold diplomas. Nor are casuals penalised by virtue of studying, whereas for permanents, this constitutes a liability. Union membership confers on casuals a higher premium than it does for permanents. Finally, occupational status rewards permanents more than it does casuals. For a casual, a unit increase on the occupational status scale leads to a 15 per cent increase in the hourly rate; for a permanent it leads to a 35 per cent increase.[16] These results are important, and I will return to them in the conclusion.

To calculate the wage gap with a split sample one needs to calculate adjusted wages. These are the wages predicted by the modelling, with the net effect of status calculated by applying the casual coefficients to the characteristics of the permanents. This answers the question: what would casuals

[16] The formula $100(e^{\beta} - 1)$ has been used to convert these coefficients into percentages.

be paid if they had the same attributes as permanents? The difference between this adjusted wage and the unadjusted wage provides one measure of the effect of casual status.[17] Comparing the adjusted wage gap with the original also allows one to conduct a simple decomposition of the gap into that proportion accounted for by characteristics, and that proportion due to other factors. These include both unobservable factors, as well as the different valuation which the same attributes receive, depending on whether they are held by casuals or permanents. The results of these calculations are shown in Table 6 and show that an original wage gap of about 11 per cent shrinks to about 5 per cent once the characteristics of casuals are taken into account. The decomposition shows that these characteristics account for nearly 50 per cent of the original difference in wages between casuals and permanents.

Table 6: Unadjusted and adjusted wages and decomposition

Unadjusted wages		
Permanent	\$19.61	
Casual	\$17.52	
Gap	\$2.09	10.7%
Adjusted wage		
Casual (with characteristics of a permanent)	\$18.60	
Gap	\$1.01	5.2%
Decomposition		
Amount characteristics account for in original gap		48.3%

Note: Gap percentages express the difference as a percentage of the higher wage. Adjusted wage calculated by applying the casual coefficients to permanents and averaging across the sample (that is, 'mean predictions' rather than 'predictions at the mean'.) *Population:* Adult female part-time employees (excluding those on fixed-term contracts).

The propensity score matching results are largely in line with the switching regression results. After matching on propensity scores, an average treatment effect on the treated (ATT) was estimated, and standard errors calculated using bootstrapping. These results (Table 7) show that casuals have an ATT of about .05, a result which is not statistically significant. Using the discounted hourly earnings approach results in an ATT of –0.12 (which is statistically significant). As one would expect, factoring in the casual load-

[17] There are a number of ways of calculating net effects. As the literature on Blinder-Oaxaca decompositions shows, one can weight the predicted wages according to the characteristics of the high wage group, the low wage group, or an average. It depends on whether one views the gap as a premium or a penalty. Blinder-Oaxaca decompositions can also be used to further divide the 'unexplained' component of the wage gap (that part not due to attributes) into a part due to differential effects and a part due to differential constants (Blinder, 1973; Oaxaca, 1973). However, as Jones and Kelley (1984) show, the results of this further stage in the decomposition process are subject to how the dummy variables are coded, and are therefore somewhat arbitrary.

ing using the discounting approach simply shifts the final result from not significantly different, to considerably negative and statistically significant (as it does with the dummy variable approach in the pooled OLS regression).

Table 7: Results of propensity score matching: ATT for casuals

	ATT	SE	P value	LCI	UCI
Normal rates	.047	.029	0.113	-.011	.106
Discounted rates	-.115	.030	0.000	-.174	-.057

Note: ATT = average treatment effect on the treated. Standard errors produced by bootstrapping. Matching carried out with psmatch2 (Leuven and Sianesi, 2003). LCI = lower confidence interval, UCI = upper confidence interval (both at 95 per cent.) *Population:* Adult female part-time employees (excluding those on fixed-term contracts).

In the case of propensity score matching, working with the discounted hourly rate has an advantage when it comes to sensitivity analysis. As mentioned earlier, this is carried out using Rosenbaum bounds, by selecting a number of values for e^γ, and seeing at what point the treatment effect is no longer statistically significant. By modelling discounted hourly rates, one can assess the point at which the negative treatment outcome of -0.12 becomes statistically non significant. In a sense, one is estimating the magnitude of the hidden bias needed to reverse the findings. The results of this analysis are shown in Table 8 and suggest that a e^γ value of about 1.6 provides such a cut-off point.

This finding can be interpreted as follows. The unobserved effect (v_i) would have to increase the odds of becoming a casual by more than 60 per cent, before it would alter the conclusion that casual 'treament' leads to a drop in earnings of about 12 per cent. Another way of interpreting Table 8 makes use of the Hodges-Lehmann point estimates (Rosenbaum, 2002, p. 116-117). When $e^\gamma = 1$, and there is no hidden bias, these estimates are equal ($\hat{t}_{max} = \hat{t}_{min} = -0.12$). At $e^\gamma = 1.6$, the estimated treatment effect may be as high as –0.21 or as low as –0.04. At the lower bound, this is not significantly different from 0.

In terms of sensitivity to hidden bias, is an odds ratio of 1.6 large, moderate or small? As Rosenbaum notes, a study is sensitive to hidden bias when values close to one 'could lead to inferences that are very different from those obtained assuming the study is free of hidden bias' (Rosenbaum, 2002, p. 107). But how close to one is close? While observational studies in the health sciences typically find their results may not be subject to hidden bias until the odds ratios are quite large (as high as 6 in smoking and lung cancer studies), studies in the social sciences find much lower figures. Sensitivity analysis for the well-known Card and Krueger minimum wage studies found figures between 1.34 and 1.5 (Rosenbaum, 2002, p. 188), while DiPrete and Gangl (2004, p. 36) found their results sensitive to values ranging from 1.1 to 2.2. In his sensitivity analysis, Aakvik (2001), for example, consid-

Table 8: Rosenbaum bounds for casual status 'treatment' effects

e^γ values	p-critical	Hodges-Lehmann point estimate			
		$\hat{t}_{max}$	$\hat{t}_{min}$	CI_{max}	CI_{min}
1.0	.000000	-.124632	-.124632	-.156447	-.092543
1.2	.000000	-.157414	-.091643	-.189659	-.059077
1.4	.000091	-.185051	-.063599	-.218412	-.031575
1.6	.009846	-.209265	-.040277	-.243500	-.006705
1.8	.136393	-.230887	-.019247	-.265541	.015407
2.0	.502067	-.250168	.000075	-.286133	.034591

Note: Rosenbaum bounds calculated using rbounds (Gangl, 2003). *Population:* Adult female part-time employees (excluding those on fixed-term contracts).

ered the range from 1.25 to 2 and argued that a factor of 2 (or 100 per cent) should be considered 'a very large number given that we have adjusted for many important observed background characteristics.'(2001, pp. 132–33).

One way of placing the figure of 1.6 into context, is to compare this odds ratio with other factors which influence casual selection. Union status and job tenure, for example, have a considerable impact on selection, with odds ratios of about 2.2 and 2.1.[18] In this light, the impact of the postulated unobservable effects would have to be considerably large—but not as decisive as union status or job tenure— to throw into doubt the treatment results. Moreover, as DiPrete and Gangl (2004, p. 15) suggests, Rosenbaum bounds are 'a "worse-case" scenario ... [which show] just how large the influence of a confounding variable must be to undermine the conclusions of a matching analysis'.

Ultimately, with sensitivity analysis there are no definitive answers, just matters for judgment. As Aakvik (2001, p. 133) puts it: 'A sensitivity analysis shows how biases might alter inferences ... it does not indicate whether biases are present or what magnitudes are plausible.'

3.3 Conclusion

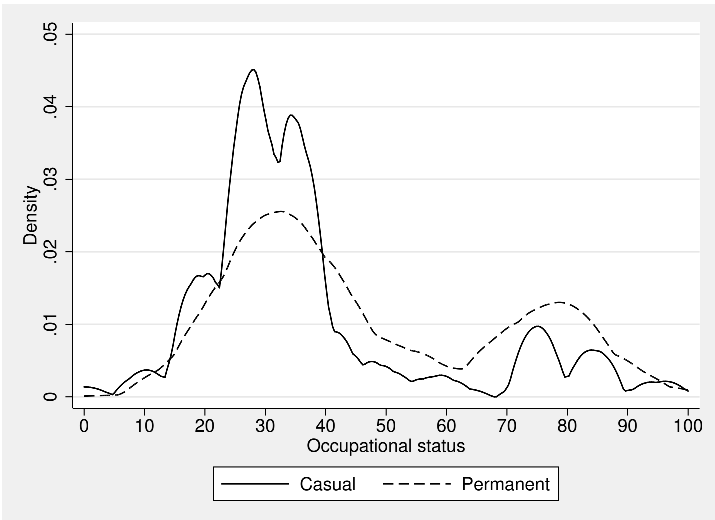
The results for the various models in this paper suggest that the wages gap between permanents and casuals is about –10 per cent. That is, in terms of their observed characteristics, and taking into account their loadings, casuals are underpaid by about 10 percent. Moreover, unobserved factors which select part-timers into casual jobs do not appear to be correlated with unobserved factors which influence their wage outcomes. This would seem to rule out unobserved ability or motivation as reasons for the wages gap.

[18] Of course, these figures express the odds ratios for selection into *permanent* status, since they are negatively associated with casual status. However, reversing their sign (or taking the reciprocal of their odds ratio as done here) is warranted for illustrative purposes. These figures are also calculated using logit, rather than probit, modelling.

All three estimation approaches in this paper produce similar results in dealing with the problem of unobservables. Admittedly, these models all employ the same data, and similar specifications, so these results may still be overturned by subsequent research. Nevertheless, the convergence between these different estimation techniques suggests the findings are reasonably robust.

If it is not the characteristics which casuals bring to the workplace which account for the wages gap, what factors do? I would argue that it is more likely that these factors are located *within* the workplace, and relate to the devaluation of casual jobs which takes place there. To explore this issue, it is useful to distinguish between the engagement of labour, its deployment, and its development (for this distinction, see Watson et al. (2003, ch. 10)). At first glance, casual status is simply a mode of engagement—a short-term contract of employment with additional compensation paid in lieu of lost entitlements. Yet casual employment is far more than just this: it is also a strategy for the deployment of labour: a way of maintaining a just-in-time workforce and a way of shifting the risks of the employment relationship onto the employee. As Buchanan puts it, casualisation represents 'a new approach to managing labour that boosts labour productivity by pushing many of the costs and risks of employment onto workers.' (Buchanan, 2004, p. 4). This shifting of risk is part of a strategy which studies of contingent employment in the United States have described as 'access to labour without obligation' (Gonos, 1997).

Figure 2: Distribution of occupational status



Source: HILDA Release 3.
Population: Adult female part-time employees in Wave 3.

Consequently, once engaged, casuals are deployed in a different way in the workplace compared with permanents. Their reduced entitlements reflect the reduced obligations which employers feel toward them. In a sense, their casual status makes them 'disposable' workers. Consequently, their deployment is marked by a lack of development. On a day-to-day basis, the skills content of their work is not deepened, whether this be technical skills, cognitive skills or behavioural skills (for the importance of these distinctions, see Mounier (2001).) Other research suggests casuals face only limited career options in the workplace (Pocock et al., 2004; Hall et al., 1998), and their lack of access to training is also evident in the HILDA data.

The wage equation results are consistent with this notion. Compared with permanents, casuals are rewarded well for their pre-existing attributes, such as their educational qualifications, but they are not rewarded well in occupational terms. Occupational status—a way of measuring occupational grades more finely—is simply not rewarded in the same way for casuals as it is for permanents. The system of over-award payments may be one of the mechanisms for this process. Traditionally, such payments were often seen by employers as 'merit' payments, a reward for longevity in the workplace or an enticement not to leave. They were invariably paid to permanents, not casuals. In a sense, over-awards were one form of flexibility—upward flexibility—within the award system which made its wages system more responsive to market pressures.[19] On the other hand, those workers paid at the award were more insulated from market pressures, and the lack of rewards for occupational status suggests the extent to which a backwater for casuals within the occupational classification structure has emerged over time. In terms of wages, the main thing to be said in favour of casual status was the loading, and this no doubt reconciled many casuals to their more marginalised workplace existence.

This backwater phenomenon is evident in the occupational distributions of the two groups. Figure 2 illustrates this distribution using the ANU occupational status score. While this distribution is polarised for both groups, the degree of polarisation is much sharper among casuals. Their overall concentration at the bottom of the distribution is not surprising, given the occupational profile discussed earlier, but their specific concentration in particular peaks, rather than being more evenly dispersed, is noteworthy. It suggests a kind of ghettoisation in the labour market that is absent among permanents.

Taking this argument further is limited by the lack of more detailed occupational data (for which ASCO 6 digit codes would be needed), but the overall results of this research do suggest that the award system brings a certain uniformity to its outcomes, and that this matters more for casuals than for permanents since this is their primary domain. This is consistent

[19] Thanks to John Buchanan for this insight.

with another of the key regression results: the higher union wage premium for casuals, which reflects the tight fit between the award system and the role of trade unions in the labour market. Without an effective award system, and without trade union involvement, this uniformity would dissipate over time. Unlike the situation for permanents, the market-responsive flexibility likely to ensue would almost certainly be downward flexibility. The impending loss of casual loadings, and other penalty rates, looms as the first instalment of this process.

Appendix

Table 9: Pooled sample wages models: estimation by OLS and fixed effects

	OLS		Fixed Effects	
Variables	β	SE	β	SE
Age (control=18 to 24)				
25 to 34	0.20***	(0.033)	-0.10	(0.096)
35 to 44	0.27***	(0.042)	-0.15	(0.112)
45 to 54	0.22***	(0.041)	-0.06	(0.123)
55 and up	0.21***	(0.057)	-0.24	(0.143)
Occupational tenure (in 10 yrs)	0.09*	(0.039)	0.02	(0.036)
Occupational tenure squared	-0.02	(0.012)	-0.01	(0.010)
Job tenure (in 10 yrs)	0.02	(0.022)	0.01	(0.030)
Education (control=Year 11 or below)				
Degree or above	0.22***	(0.039)	0.19	(0.157)
Diploma	0.09*	(0.037)	0.14	(0.200)
Certificate	0.05	(0.032)	-0.04	(0.103)
Completed Year 12	0.04	(0.038)	-0.02	(0.133)
Studying at school or higher	-0.01	(0.029)	0.02	(0.029)
Marital status (control=Single)				
Legally married	-0.03	(0.028)	0.17	(0.098)
De facto	0.01	(0.036)	0.09	(0.063)
Separated	-0.09	(0.049)	0.13	(0.105)
Divorced	0.00	(0.042)	-0.00	(0.108)
Widowed	-0.07	(0.076)	0.35	(0.182)
Children under 5	-0.09*	(0.039)	0.01	(0.059)
Less than 20 employees	-0.14***	(0.030)	-0.03	(0.027)
Union member	0.00	(0.027)	0.04	(0.031)
Log of occupational status	0.19***	(0.027)	0.11***	(0.032)
Industry (control=Education & government)				
Agric & mining	0.26*	(0.130)	-0.24*	(0.107)
Manufacturing	0.06	(0.048)	0.08	(0.073)
Infrastructure	0.09	(0.047)	-0.07	(0.082)
Wholesale & retail	-0.02	(0.037)	-0.00	(0.058)
Services	0.03	(0.042)	0.03	(0.056)
Finance & business	0.07	(0.042)	0.03	(0.057)
Health	0.01	(0.037)	0.04	(0.051)
Remoteness (control=City)				
Inner regional	-0.00	(0.026)	-0.01	(0.041)
Outer regional	-0.08*	(0.038)	0.03	(0.063)
Remote	0.06	(0.050)	0.23*	(0.103)
States (control=NSW)				
Vic	-0.07*	(0.031)	0.09	(0.154)
Qld	-0.14***	(0.031)	0.03	(0.174)
SA	-0.08*	(0.034)	0.28	(0.377)
WA	-0.04	(0.037)	(dropped)	
Casual contract	0.02	(0.039)	0.03	(0.025)
Constant	1.88***	(0.123)	2.27***	(0.211)
R-squared	0.39		0.04	
Number of Cases	1,070		3,765	

Note: Robust standard errors in brackets. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. For OLS estimation uses Stata's survey regression command (standard errors take account of the design effect). *Source:* HILDA Release 3. *Population:* Adult female part-time employees (excluding those on fixed-term contracts).

Table 10: Selection model for working: multinomial probit

	Outcomes					
	Full-time		Part-time		Self-emp	
Variables	β	SE	β	SE	β	SE
Age (control=Aged under 25)						
Aged 25 to 34	0.492***	(0.149)	-0.171	(0.141)	0.689***	(0.192)
Aged 35 to 44	0.621***	(0.152)	0.122	(0.148)	1.091***	(0.194)
Aged 54 and older	0.510**	(0.155)	0.099	(0.150)	0.985***	(0.196)
55 and up	-1.108***	(0.169)	-1.029***	(0.154)	0.161	(0.201)
Education (control=Yr 11 or less)						
Degree or above	0.919***	(0.106)	0.506***	(0.100)	0.650***	(0.114)
Diploma	0.534***	(0.126)	0.169	(0.125)	0.359**	(0.139)
Certificate	0.442***	(0.100)	0.346***	(0.091)	0.447***	(0.104)
Completed Year 12	0.270*	(0.126)	0.127	(0.113)	0.169	(0.142)
Studying at school or higher	-0.538***	(0.117)	0.291**	(0.109)	-0.225	(0.136)
Marital status (control=Single)						
Legally married	-0.628***	(0.139)	-0.606***	(0.130)	-0.160	(0.154)
De facto	-0.021	(0.141)	-0.310*	(0.143)	0.060	(0.175)
Separated	0.708**	(0.228)	0.215	(0.216)	0.216	(0.276)
Divorced	0.656***	(0.199)	-0.053	(0.187)	0.336	(0.231)
Widowed	-0.584*	(0.229)	-0.654**	(0.202)	-0.564*	(0.242)
Number children under 5	-0.303***	(0.085)	0.090	(0.065)	0.045	(0.083)
Number of children 6 to 18	0.161***	(0.048)	0.264***	(0.042)	0.166***	(0.047)
Hours of housework	-0.060***	(0.004)	-0.021***	(0.004)	-0.023***	(0.004)
Living in public housing	-0.846***	(0.217)	-0.870***	(0.213)	-1.183***	(0.303)
Ann govt transfer income ('000s)	-0.229***	(0.013)	-0.124***	(0.008)	-0.148***	(0.013)
Remoteness (control=City)						
Inner regional	0.221*	(0.086)	0.372***	(0.079)	0.558***	(0.093)
Outer regional	0.068	(0.125)	0.014	(0.118)	0.475***	(0.124)
Remote	0.461	(0.242)	0.373	(0.227)	0.869***	(0.239)
States (control=NSW)						
Vic	0.048	(0.097)	0.151	(0.089)	-0.012	(0.107)
Qld	0.223*	(0.103)	0.163	(0.098)	0.111	(0.115)
SA	0.066	(0.126)	0.375**	(0.117)	0.335*	(0.144)
WA	-0.094	(0.125)	0.114	(0.124)	0.340*	(0.133)
Tas NT ACT	0.016	(0.170)	0.182	(0.158)	-0.238	(0.209)
Constant	0.950***	(0.155)	0.394**	(0.148)	-1.387***	(0.205)
Log pseudolikelihood	-5331.5					
Model chi-square	1794.2					
Number of Cases	6,133					

Note: Significance levels: * p< 0.05, ** p< 0.01, *** p< 0.001. Estimation using HILDA Wave 3 cross-sectional weights. Outcome baseline category: not in the paid workforce. *Source:* HILDA Release 3. *Population:* Adult women.

Table 11: Selection model for casual status: binomial probit

Variables	β	SE
Age (control=Aged under 25)		
Aged 25 to 34	-0.157	(0.200)
Aged 35 to 44	-0.224	(0.208)
Aged 54 and older	-0.021	(0.210)
55 and up	-0.389	(0.287)
Occupational tenure (in 10 yrs)	-0.016	(0.159)
Occupational tenure squared	0.009	(0.043)
Job tenure (in 10 yrs)	-0.339**	(0.106)
Education (control=Year 11 or below)		
Degree or above	0.341*	(0.165)
Diploma	0.266	(0.184)
Certificate	0.016	(0.136)
Completed Year 12	0.131	(0.173)
Studying at school or higher	0.207	(0.151)
Marital status (control=Never married & not defacto)		
Legally married	-0.186	(0.172)
De facto	0.174	(0.196)
Separated	-0.811**	(0.282)
Divorced	0.011	(0.268)
Widowed	-0.106	(0.342)
Children under 5	-0.260	(0.155)
Less than 20 employees	0.032	(0.111)
Union member	-0.630***	(0.124)
Log of occupational status	-0.133	(0.118)
Industry (control=Education & government)		
Agric & mining	0.314	(0.413)
Manufacturing	0.135	(0.334)
Infrastructure	-0.136	(0.261)
Wholesale & retail	0.286	(0.170)
Services	0.307	(0.201)
Finance & business	-0.002	(0.189)
Health	-0.421**	(0.152)
Geographical remoteness (control=city)		
Inner regional	0.214*	(0.108)
Outer regional	-0.197	(0.162)
Remote	-0.207	(0.332)
States (control=NSW)		
Vic	-0.043	(0.132)
Qld	0.105	(0.151)
SA	0.282	(0.156)
WA	-0.182	(0.179)
Tas NT ACT	0.106	(0.174)
Works shifts	0.477***	(0.109)
Very short hours (10 or less)	0.715***	(0.133)
Selectivity effect	0.251	(0.129)
Constant	0.391	(0.507)

Note: N=1,103. Robust standard errors in brackets. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimation using Stata's survey probit command. Standard errors take account of the design effect. *Source:* HILDA Release 3. *Population:* Adult female part-time employees (excluding those on fixed-term contracts).

Table 12: Split sample wages models: OLS and switching regression

	Casuals		Permanents	
Variables	**OLS**	**Switch**	**OLS**	**Switch**
Age (control=Aged under 25)				
Aged 25 to 34	0.04	0.04	0.01	0.02
	(0.072)	(0.074)	(0.054)	(0.053)
Aged 35 to 44	-0.03	-0.01	0.03	0.04
	(0.078)	(0.080)	(0.063)	(0.063)
Aged 54 and older	-0.06	-0.06	0.03	0.03
	(0.082)	(0.082)	(0.067)	(0.066)
55 and up	-0.11	-0.12	0.02	0.03
	(0.106)	(0.105)	(0.085)	(0.085)
Occupational tenure (in 10 yrs)	0.06	0.06	0.01	0.01
	(0.080)	(0.080)	(0.046)	(0.046)
Occupational tenure squared	-0.00	-0.01	-0.00	-0.00
	(0.023)	(0.024)	(0.011)	(0.011)
Job tenure (in 10 yrs)	0.08	0.11	0.05	0.08
	(0.054)	(0.062)	(0.036)	(0.052)
Education (control=Year 11 or below)				
Degree or above	0.21**	0.20**	0.18**	0.15*
	(0.067)	(0.067)	(0.054)	(0.058)
Diploma	0.24**	0.23**	0.01	-0.00
	(0.075)	(0.074)	(0.054)	(0.055)
Certificate	0.07	0.08	-0.05	-0.05
	(0.053)	(0.052)	(0.037)	(0.037)
Completed Year 12	0.09	0.08	0.01	-0.00
	(0.058)	(0.058)	(0.058)	(0.054)
Studying at school or higher	-0.03	-0.05	-0.08	-0.10*
	(0.053)	(0.055)	(0.042)	(0.045)
Marital status (control=Single)				
Legally married	0.20*	0.22**	0.10	0.13
	(0.076)	(0.076)	(0.065)	(0.068)
De facto	0.23***	0.22**	0.04	0.03
	(0.069)	(0.070)	(0.068)	(0.068)
Separated	0.21	0.28	0.11	0.18
	(0.142)	(0.144)	(0.089)	(0.110)
Divorced	0.13	0.14	0.11	0.12
	(0.111)	(0.111)	(0.087)	(0.087)
Widowed	0.28*	0.27	0.08	0.08
	(0.136)	(0.139)	(0.088)	(0.088)
Children under 5	-0.08	-0.07	0.07	0.08
	(0.068)	(0.068)	(0.040)	(0.040)
Less than 20 employees	-0.03	-0.04	-0.03	-0.03
	(0.042)	(0.042)	(0.033)	(0.033)
Union member	0.17**	0.23**	0.04	0.09*
	(0.064)	(0.077)	(0.032)	(0.045)
Log of occupational status	0.13**	0.14**	0.28***	0.30***
	(0.049)	(0.051)	(0.036)	(0.040)
Industry (control=Education & government)				
Agric & mining	-0.03	-0.04	0.16	0.15
	(0.133)	(0.135)	(0.092)	(0.095)
Manufacturing	-0.04	-0.06	0.14	0.13
	(0.156)	(0.153)	(0.071)	(0.072)
Infrastructure	-0.11	-0.09	0.04	0.04
	(0.110)	(0.110)	(0.083)	(0.081)
Wholesale & retail	-0.05	-0.07	0.03	0.00
	(0.074)	(0.076)	(0.043)	(0.049)
Services	-0.10	-0.12	-0.04	-0.08

Variables	Casuals OLS	Casuals Switch	Permanents OLS	Permanents Switch
	(0.079)	(0.084)	(0.071)	(0.078)
Finance & business	0.08	0.08	0.07	0.07
	(0.085)	(0.086)	(0.044)	(0.044)
Health	-0.08	-0.04	0.13***	0.16***
	(0.075)	(0.073)	(0.039)	(0.044)
Remoteness (control=City)				
Inner regional	-0.12*	-0.14**	-0.09**	-0.10**
	(0.049)	(0.051)	(0.032)	(0.036)
Outer regional	-0.11	-0.10	-0.07*	-0.06
	(0.064)	(0.062)	(0.034)	(0.033)
Remote	-0.09	-0.08	-0.10	-0.10
	(0.085)	(0.086)	(0.079)	(0.076)
States (control=NSW)				
Vic	-0.12*	-0.12*	-0.07	-0.06
	(0.049)	(0.049)	(0.036)	(0.035)
Qld	-0.01	-0.01	-0.00	-0.01
	(0.054)	(0.054)	(0.040)	(0.040)
SA	-0.19**	-0.21**	-0.06	-0.08
	(0.068)	(0.068)	(0.046)	(0.051)
WA	-0.15	-0.14	-0.03	-0.01
	(0.083)	(0.084)	(0.045)	(0.048)
Selectivity effect		-0.15		-0.13
		(0.101)		(0.105)
Constant	2.19***	2.21***	1.67***	1.69***
	(0.209)	(0.212)	(0.137)	(0.139)
R-squared	0.28	0.28	0.46	0.46
Number of Cases	527	527	519	519

Note: Robust standard errors in brackets. Significance levels: * p< 0.05, ** p< 0.01, *** p< 0.001. Estimation using Stata's survey regression command. Standard errors take account of the design effect. *Source:* HILDA Release 3. *Population:* Adult female part-time employees (excluding those on fixed-term contracts).

Table 13: Variables used in modelling: means and standard deviations

Variables	Means	SD
Selection into work		
Age		
25 to 34	0.20	0.401
35 to 44	0.20	0.401
45 to 54	0.18	0.386
55 and up	0.28	0.447
Education		
Degree or above	0.20	0.402
Diploma	0.09	0.284
Certificate	0.24	0.430
Completed Year 12	0.14	0.342
Studying at school or higher	0.13	0.337
Marital status		
Legally married	0.54	0.498
De facto	0.11	0.312
Separated	0.03	0.176
Divorced	0.07	0.247
Widowed	0.07	0.251
Number children under 5	0.17	0.530
Number of children 6 to 18	0.62	1.009
Hours of housework	16.33	14.447
Living in public housing	0.04	0.204
Ann govt tranfer income ('000s)	3.87	5.507
Remoteness		
Inner regional	0.22	0.412
Outer regional	0.10	0.306
Remote	0.02	0.131
States		
Vic	0.25	0.434
Qld	0.19	0.396
SA	0.08	0.265
WA	0.10	0.294
Tas NT ACT	0.05	0.217
Selection into casual and wages models		
Age		
Aged 25 to 34	0.19	0.391
Aged 35 to 44	0.26	0.439
Aged 54 and older	0.23	0.418
55 and up	0.11	0.310
Occupational tenure (in 10 yrs)	0.77	0.867
Occupational tenure squared	1.34	2.765
Job tenure (in 10 yrs)	0.50	0.606
Education		
Degree or above	0.21	0.406
Diploma	0.09	0.285
Certificate	0.27	0.446
Completed Year 12	0.17	0.375
Studying at school or higher	0.23	0.422
Marital status		
Legally married	0.55	0.498
De facto	0.10	0.305
Separated	0.03	0.166
Divorced	0.05	0.223
Widowed	0.02	0.153

Variables	Means	SD
Children under 5	0.16	0.362
Less than 20 employees	0.31	0.463
Union member	0.21	0.411
Log of occupational status	3.60	0.554
Industry		
Agric & mining	0.02	0.137
Manufacturing	0.03	0.167
Infrastructure	0.03	0.182
Wholesale & retail	0.25	0.434
Services	0.14	0.348
Finance & business	0.13	0.336
Health	0.25	0.434
Geographical remoteness		
Inner regional	0.24	0.425
Outer regional	0.09	0.284
Remote	0.02	0.144
States		
Vic	0.26	0.441
Qld	0.18	0.383
SA	0.08	0.277
WA	0.10	0.304
Tas NT ACT	0.05	0.225
Works shifts	0.35	0.477
Very short hours (10 or less)	0.24	0.427

Source: HILDA Release 3. *Population:* For selection into work: adult females; for other models: adult female part-time employees (excluding those on fixed-term contracts).

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