Men at Work in a Land Down-under: Testing Some Predictions of Human Capital Theory *

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Abstract

We use new training data from waves 3-6 of the Household, Income and Labour Dynamics in Australia Survey to investigate training and wages of full-time men. We explore the extent to which the data are consistent with the predictions of human capital theory or with recent alternative theories based on imperfectly competitive labour markets. According to the raw data, most work-related training received by full-time private sector men is general but it is also paid for by employers. Our fixed effects estimates reveal that this training is associated with higher wages in current and in future firms, and that the effect in future firms is larger and more precisely determined. These results are more consistent with the predictions of human capital theory based on imperfectly competitive labour markets than with the alternative of perfect competition.

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1. INTRODUCTION

For many years it was believed that orthodox human capital theory – as formalized by Becker (1964) and Oi (1962) and based on the assumption of a perfectly competitive labour market - fully explained who would pay for general training. The consensus was that stylized facts diverging from the predictions of this model could be explained by imperfections in the capital market such as credit constraints. However, from the mid-1990s, this orthodoxy was challenged by Stevens, (1994, 1996) and Acemoglu and Piscke (1999a, 1999b).[1] These papers relaxed the assumption of perfectly competitive labour markets. They argued instead that the labour market is imperfectly competitive, resulting in a wedge between wages and productivity. By showing conditions under which wage returns to general training are less than productivity returns, they also demonstrated that firms may find it profitable to pay for general training, provided that post-training productivity is increasing in training intensity at a faster rate than are wages. In this context, general training is defined as training that can be used at more than one employer and is therefore transferable across firms. In the perfectly competitive case, there are countless other firms where the training can be utilised.

In this paper we use data from the Household, Income and Labour Dynamics in Australia Survey to confront the main predictions of the various human capital theories for wages and cost-sharing. Surprisingly few papers have addressed this to date, perhaps because of the difficulties of finding appropriate data that provides information not only on work-related training but also who pays for it[2]. Notable exceptions are the studies by Loewenstein and Spletzer (1998) for the US, and Booth and Bryan (2005, 2007) for the Britain. Using data from the 1988 to 1991 waves of the National Longitudinal Survey of Youth that provides rich training

[1] See also the papers in Green (2007).
[2] Acemoglu and Pischke (1999a) survey research on training practices and firms' willingness to contribute to the costs of general training investments, and suggest that labour market imperfections play an important role.

information, Loewenstein and Spletzer (1998) show that completed spells of general training paid for by previous employers have a larger wage effect than completed spells of general training paid for by the current employer. They argue that their findings are consistent with their model in which contract enforcement considerations cause employers to share the costs and returns to purely general training. They do not, however, explore whether or not their results are consistent with other theories based on imperfect competition in the labour market. Booth and Bryan (2005, 2007) build on this approach by exploiting information from the new training questions introduced into the British Household Panel Survey (BHPS) from 1998. They find that employer-financed training is associated with significantly higher wages at current and 'future' firms, with a larger impact in future firms. They demonstrate that this is consistent with human capital theory with credit constraints as well as with the new training literature assuming imperfectly competitive labor markets.

It is important to know if these results can be generalized to other countries, or if instead they are are specific to the two countries already analysed – the US and Britain. In the present paper we take a step in this direction by testing the predictions of the various models against data from another 'Anglo-Saxon' country - Australia. We build on the small literature described above, using data from waves 3 to 6 of the Household, Income and Labour Dynamics in Australia (HILDA) Survey. This spans the period 2003-6. The HILDA Survey is particularly appropriate for studying work-related training and wages following the addition, from wave 3, of new questions on training that will be detailed below. The dataset also provides a very rich set of controls and its panel nature – where the same individuals are surveyed over time - facilitates estimation controlling for unobserved heterogeneity. To our knowledge, ours is the first study using representative Australian panel data to estimate the impact of work-related training on

wages, probably because a suitable panel dataset has only very recently become available with the HILDA Survey.

Our main results can be summarised as follows: First, a large proportion of work-related training received by full-time private sector men is general. Second, this general training is not paid for by the men receiving it. Third, our fixed effects estimates reveal that this employer-financed general skills training paid for by the previous employer has a larger wage effect than completed spells paid for by the current employer. We will show that these results are more consistent with alternative hypotheses put forward by recent theoretical developments in the training literature than with the predictions of human capital theory.

The remainder of our paper is set out as follows. In Section 2, we briefly outline the theoretical hypotheses and their predictions as to who pays for general training and the returns to training (at both the training firms and at subsequent firms). In the following section we describe the data source and the novel features of the training questions. In Section 4 we present and interpret our estimates of the impact of the various forms of training on wages. The concluding section compares our main results with those of Loewenstein and Spletzer (1998) and Booth and Bryan (2005, 2007) who, as described above, used a broadly similar approach on US and British data respectively.

2. BACKGROUND

The main predictions of the various human capital theories for wages and cost-sharing are summarized in Table 1, reproduced from Booth and Bryan (2005). According to human capital theory – Row [1] of Table 1 - workers in competitive labour markets invest in *general* work-related training by receiving low training wages and reap the returns by receiving higher wages

afterwards (Becker, 1964). This is because general training represents skills that can be used at numerous other firms, and hence the training is general or portable across companies as individuals change jobs. According to human capital theory, general training would therefore be financed by the worker through the receipt of lowered wages during training. This is because training is embodied in the worker, who could leave at any time to another job where she would be equally as productive, and therefore no firm would ever finance such training. (We use the terms *firm* and *employer* interchangeably and reserve the term *job* for a particular function or set of duties within a firm. The theories being tested concern employers not jobs.)

Workers who cannot afford to accept low wages during general training will be adversely affected by credit market constraints disbarring them from borrowing to finance their investment. But if the firm were willing to act as lender, it could pay workers more than their marginal product (net of training costs) during training and less afterwards – see Row [2]. The firm would agree to such a contract only if it can devise a mechanism (like an apprenticeship contract or a minimum employment guarantee) to bind workers to the firm until repayment of the loan. The wedge between wages and productivity reflects the degree of cost-sharing. Since general training is transferable across firms by definition, trained workers who change employers should get a greater return than they received in the firm providing the training and the loan. This greater return at future employers is expected to fall as the period between the reception of the training spell and the job change lengthens.

Insert Table 1 near here

In the *specific* training model - see Row [3] of Table 1 - it is well-known that it is efficient for the firm and worker to share both the costs and the net returns of the training

investment (Hashimoto, 1981).[3] Workers have an incentive to invest in training that will not help then receive higher wages elsewhere by being paid above marginal productivity during training. Employers receive a return on this investment by paying wages below marginal productivity after training. The magnitude of this wedge will reflect the degree of cost-sharing. Specific training will not be transferable across firms since, by definition, it is of value only to the firm providing the training. Hence, to reduce potential hold-up problems, both parties would contribute to the financing of training. This sharing mechanism ensures that both firm and worker have the incentive to maintain the relationship after training and thereby to reap the returns.

If training comprises a mix of general and specific components, workers will finance the general component and firms will share the costs of the specific training component. Since there will be some cost-sharing, wages at the training firm will be greater than productivity during training and less than productivity after training – see Row [4]. Wages at subsequent firms will reflect returns *only* to the general component of training, and will consequently be less than wages at the training firm (in which there is some return to the worker to the shared investment in specific training).

However in a labour market characterized by imperfectly competitive wage-setting, market imperfections lead to an associated wage 'compression' where the wage function is increasing in the level of training less rapidly than productivity and so the gap between productivity and the wage is higher at greater levels of skills. Since the return to skills for a worker is less than the one prevailing in a competitive labor market, the incentive for firms to invest in general training is higher, provided that post-training productivity net of training costs

[3] This model can be illustrated with a simple example. Suppose the firm pays for all the costs of specific training. If the worker then leaves, the firm will lose the investment. Analogously, if the worker were to pay all the costs of specific training, he too would lose the investment – that is specific to the firm - if the firm terminates the job. By sharing in the costs, these holdup problems are overcome.

is increasing in training at a faster rate than wages. (For examples of this new training literature, see Katz and Ziderman (1990), Stevens (1994), Chang and Wang (1996), Loewenstein and Spetzer (1998), Booth and Chatterji (1998) and Acemoglu and Pischke (1999a, 1999b), Booth and Zoega (2004), and Green (2007).) Moreover the equilibrium amount of training provided may be sub-optimal from society's viewpoint. The predictions are that the firm may finance general training and that the training firm's wages will be less than net marginal product. According to the contracting model of Loewenstein and Spletzer (1998), if a minimum wage guarantee binds in the current job, the current employer can extract rents from providing general training and the wage-return to training may be greater in future firms than in the current firm. According to the model of Acemoglu and Pischke (1999a, 1999b) – based on mobility costs – although all workers receive a positive return to their training, the current employer has monopsony power over the worker because of the mobility costs. Consequently wages will increase more with the future employer than the current employer. These predictions are summarized in Row [5].

The predictions of some hypotheses are observationally equivalent. For example, two models predict that transferable training might have bigger returns to subsequent firms than to the firms at which training actually takes place - see Rows [2] and [5]. However, some predictions are quite distinct. For example, the models in Rows [1] and [5] predict that training is transferable, but the first predicts workers pay for it while the fifth predicts that firms do.

In the next section we describe the data source that will be used to discriminate between some of the hypotheses outlined above. The econometric model and the estimates will be reported in the following section.

3. THE DATA AND VARIABLES

3.1 The Data

The Household, Income and Labour Dynamics in Australia (HILDA) survey is a nationally representative random-sample panel survey of private households in Australia. All members of the households providing at least one interview in wave 1 formed the basis of the panel followed in each subsequent wave. The sample has been gradually extended to include new household members resulting from changes in the composition of the original households. While the HILDA survey data have been collected annually in a standardised format since 2001, the relevant questions on work-related training were not asked until wave 3. However, once asked, they were repeated through subsequent waves, and they covered training incidence, training type and training financing. We therefore use data from waves 3 to 6, spanning the period 2003-6. Further details of the survey are provided in Watson (2008) and Wooden and Watson (2007). [4]

The HILDA Survey data offer a number of advantages for our purposes. First, the HILDA Survey is a remarkably rich source of information on education and other relevant attributes. . Second, they are micro-data that trace the same individuals over time, allowing us to control for changes in circumstances, and for unobserved individual effects. Clearly the richer the set of controls, the lower is unobserved heterogeneity. Finally, HILDA is particularly appropriate for studying training and wages because of its new questions on training added from wave 3 onwards.

A central component of our study was the differential impact on wages of training received with the current employer and training received at any previous employers during the sample period. We therefore dropped from the analysis cases where it was not possible to determine whether training was in the current or previous jobs.

[4] Wave 1 included 13,969 respondents aged 15 and older distributed across 7682 households.

Preliminary testing of our data indicated that it is inappropriate to pool private and public sector workers when estimating our wage equations.[5] The coefficients across the sub-groups differed significantly. This is as might be expected, given that public and private sector employers typically have different objective functions, and in addition the theory summarised in the previous section is applicable to the private sector. We therefore only focus on the private sector in this study. Moreover, we consider only men, since preliminary testing also indicated that it is inappropriate to pool male and female workers. Finally, we tested the hypothesis that male full-time and part-time workers could be pooled in the wages equation and again this hypothesis was rejected by the data.

Our estimating subsample therefore covers full-time male private-sector employees.[6] We also restricted the subsample to include workers aged between 18 and 60 years in Wave 3, who are not in the armed forces, farming or fisheries, and with valid information on our main variables (hours of work, salary, and whether or not training was received). We exclude the self-employed and owner-managers drawing a salary from their own businesses. Men reporting over 100 working hours per week (hours are used to derive hourly wages) were dropped, as were full-time students. Our measure of full-time work is based on individuals' usual hours of work in their main job (including any paid or unpaid overtime for work done at the workplace or at home). We dropped from the estimating sub-sample men reporting fewer than 35 hours per week

[5] We estimated two specifications over private and public sector workers. The first included only the explanatory variables, while the second included the explanatory variables alone as well as interacted with a dummy variable for the public sector. Our null hypothesis was that none of the coefficients to the extra variables in the second specification differed from zero. This was rejected by the data. The likelihood ratio test also rejected pooling.

[6] The reader may be interested in how these restrictions progressively reduce our sample. Waves 3 to 6 provide 50,800 observations, of whom 24,016 are male. We first dropped 1,166 men with less than one year in their current job who had received training in the past year. By restricting the sample to private sector employees we are left with 11,985 men. Restricting further to the age group 18-60, we retain 7,610observations. We then dropped 271 workers in agriculture and afterwards 65 workers studying fulltime, leaving 7,274observations. Restricting the sample to cases with valid information on wages and training leaves us with 5,671. Further restricting to fulltimers who are working fewer than 100 hours per week leaves 5,058observations. In order to undertake fixed effects estimation, we needed to restrict to having at least two consecutive waves, thereby keeping 4,335 observations. For some of our analyses we utilize the balanced panel (present in all waves) which leaves 2,823.

in their main job, as also did Rodgers (2004) and Booth and Wood (2008) using the HILDA Survey data. This differs slightly from the Australian Bureau of Statistics measure, where part-time workers are defined as those reporting that they worked fewer than 35 hours per week in *all* of their jobs in the survey week.

Where there were many missing observations for control variables, we created dummy variables indicating their missing status, to maintain reasonable sample sizes. Our estimating subsample represents an unbalanced panel of men who are present - and satisfy the selection criteria - in at least two adjacent waves. It comprises 1430 men, representing 4335 person-year observations. These 1430 men with usable responses and in full-time employment are distributed across waves as follows. There are 582 men (2823 person-year observations) present in all four waves; 111 men in waves 4, 5 and 6 only; 188 men in waves 3, 4 and 5 only; 243 men in waves 5 and 6 only; 45 men present in waves 4 and 5 only; and 261 men in waves 3 and 4 only.

3.2. The Wages and Training Variables

The dependent variable in equation (1) is the *hourly wage rate* in the main job. To calculate this, we used the HILDA derived variables for the current weekly gross wages and salary for the main job, and for hours worked per week in the main job during the survey week (for more details see Watson, 2008). We deflated wages to 2003 (wave 3) levels using the headline Consumer Price Index (CPI) from the Australian Bureau of Statistics. Respondents earning an hourly wage of less than A$1 or more than A$100 in 2003 values were omitted from the analysis.

The precise form of the first training question in the HILDA Survey questionnaire is as follows:

> "During the last 12 months, have you taken part in any education or training schemes or courses, as part of your employment? (We are only interested in structured training courses the respondent has received. Do not include training they may have participated in as a trainer.)"

The framing of this question suggests that training responses should be interpreted more as formal courses of instruction. As is usual in the literature, we proxy informal on-the-job training by job tenure.

Insert Table 2 near here

The means and standard deviations of training incidence, financing and types are presented in Table 2 in both weighted and unweighted form. Table 2 also gives means for training of various types to be described below. The population estimates in Table 2 were weighted using the longitudinal weight provided in the HILDA dataset for the sub-sample of respondents in wave 3 through 6. Of course this weighting could only be used on the *balanced* panel subsample, where the number of cases for each variable type is smaller as indicated in Section 3.1. For example, using the unweighted statistics, there were 1487 person-year observations receiving employer-financed training in our unbalanced panel, as compared with 836 from the balanced panel. Of these, for the unbalanced panel 157 person-year observations received employer-financed training at their last employer and for the balanced panel the number was 68. Similarly, again using the unweighted data, there were 246 person-year observations *self-financing* their own training in our unbalanced panel, as compared with 128 from the balanced panel. Of these, for the unbalanced panel 29 person-year observations did this while at their last employer and for the balanced panel this number was only 7.

Discussion of raw training statistics and international comparisons below will proceed using the data in weighted form for the balanced panel subsample. However, as is usual practice, the econometric estimates - to be reported in Section 4 - will be unweighted.

Column [4] of Table 2 indicates mean training incidence across all waves, and shows that around 37% of full-time men received training over the period. Data from comparable Australian

surveys gives the same level of on-the-job training incidence. The Australian Bureau of Statistics found that in 2001, 37% of 15-64 year olds (or 4,760,000 individuals) took at least one work-related training course in the twelve months prior to the Survey of Education, Training and IT (OECD, 2003). How does this figure compare with other countries? In a cross-country comparison of training incidence using harmonised data from the European Community Household Panel, Arulampalam, Booth and Bryan (2004) identified three other high training incidence countries—Britain, Denmark, and Finland. They found that the proportion of individuals in work receiving training is 42% in Denmark, 41% in Britain, and 34% in Finland.[7] (For British studies of training incidence see also Felstead and Green, 1994 and Green, Machin and Wilkinson, 1999; and for other cross-country comparisons see OECD, 1999; and Bassanini et al., 2007.) In contrast, training incidence in the US is lower; see for example Lynch (1992) and Loewenstein and Spletzer (1998). For Australia see *inter alia* Tan et al. (1992), Miller (1994), and Wooden and van den Heuvel (1997), who found that Australian training incidence in the 1980s and 1990s was approximately one third of all wage and salary earners *including* the part-time workers whom we exclude from our analysis. In summary, the training figures from the HILDA Survey data do not appear out of line.

The second training question in the HILDA Survey asked respondents who had received training about its purpose. They were shown a card on which there were seven potential responses. These were: induction ("to help you get started in the current job"); improving skills in the current job; maintaining professional status or meeting occupational standards; preparation for future job; developing skills generally; health and safety; other. Respondents were allowed to pick multiple responses. Panel C of Table 2 illustrates the frequency of these responses. The most common response was that the training was to improve skills in the current job, followed

[7] Arulampalam et al. (2004) found that the proportion receiving training is between 10% to 16% in Austria, Belgium, France, and Spain, while in Ireland, Italy, and the Netherlands the incidence of training is below 10%.

by training to maintain professional or occupational standards and training to develop skills generally. Perhaps unsurprisingly, the least common form of training was for induction purposes. The highest correlation between pairs of training types is 0.495, between training to improve skills in the current job and training to develop skills generally. The lowest correlation was 0.066, and this was between induction training and training to maintain professional status or occupational standards.

We now consider the most appropriate proxy for general training. Training to "improve skills in the current job" might be either general or specific, or might comprise elements of each. This therefore does not seem the most suitable measure for our purposes. The most appropriate proxy for general training would seem to be training to develop skills generally. However, training to maintain professional or occupational standards could also be regarded as general, in that by definition it is transferable to other firms in that profession or occupation. For this reason, we construct separate variables for "training to maintain professional or occupational standards" and "training to develop skills generally" and we use these in our empirical analysis, as will be explained further below.

The third question on training was directed to its *financing*. Individuals were asked if they contributed towards the cost of any training they had received. They were prompted with a showcard listing the following ways of contributing towards training costs: pay course fees; purchase materials, books, etc.; pay for travel, accommodation, while attending the course; or take unpaid time off to attend training course. While this is a comprehensive set of prompts, respondents were only given the opportunity to answer 'yes' or 'no' and were not required to specify in precisely which way they may have contributed. However, as we shall see, the vast majority of men being trained paid for none of these training costs listed above.

We now define *employer-financed training* as courses to which the individual makes no financial contribution, that is, where respondents did not reply in the affirmative to any of these prompts above. Table 2 shows that 87% of training of any type is employer-financed. Further disaggregation showed that 86% of training "to improve skills in the current job" is employer-financed, while 84% of "training to develop skills generally" or "to maintain professional or occupational standards" is employer-financed. Around 71% of the least common form of training, induction training, is employer-financed.

Some men did, however, contribute to the costs of their training. Where respondents replied in the affirmative to one or more of these prompts, we defined their training as "other-financed" or "shared financing" (we use these two terms interchangeably in what follows). Notice that around 14% of training to improve skills is "other-financed".

What can we infer from these raw data? Like Booth and Bryan who used British data, we find that in Australia a large proportion of work-related training is viewed by its recipients as general. We also find that private sector men in Australia are largely not paying any of the explicit costs of general training. The obvious inference is that this private-sector general training is financed by employers. Yet this is not what orthodox human capital theory predicts, as we saw in Section 2. In Section 4 we will present estimates of the impact of the various forms of training on wages, but first we briefly consider some of the other variables.

3.3. The Other Explanatory Variables

Table A.1 in the Appendix gives the weighted means and standard deviations of the other explanatory variables. Full-time male employees work on average 46 hours per week and earn an average hourly wage of A$23.97. Their mean age is 40.84 years and 61% of them are married while 10% are cohabiting. On average, their job tenure is 8.9 years and their labour market

experience is 22.8 years. Around 90% of men have permanent employment and some 20% have a degree or above, while 24% have only Year 11 qualifications[8] or less. Only 5% of men in the sample change job one or more times. Of those who did change job, just over 83% did so only once.

4. THE FIXED EFFECTS WAGE GAP ESTIMATES

4.1. The Econometric Model

Our basic estimating equation, incorporating the influence of observed and unobserved characteristics on hourly wages, is given by:

$$\ln w_{ijt} = \boldsymbol{X}'_{ijt}\beta + T_{it}'\alpha + D'_t\gamma + \mu_i + \upsilon_{ij} + \varepsilon_{ijt} \qquad (1)$$

where w_{ijt} is the natural logarithm of the real (2003 prices) hourly wage of individual i in job j at time t; $\boldsymbol{X}_{ijt}$ is a vector of individual and firm characteristics that influence the outcome variable w_{ijt}; the associated parameter vector is β; T_{it} is a vector containing various measures of training and is associated with parameter vector α; and D_t comprises year-specific dummy variables with associated parameter vector γ. Unobservable characteristics affecting individual's wages are decomposed into a permanent effect μ_i, an employer match-specific component $\upsilon_{ij,}$ and a transitory effect ε_{ijt} (the random error term). The parameter vector of interest is α. Equation (1) is estimated as a fixed effects (FE) model, in which μ_i is treated as the fixed effect that can be differenced out. We approximate υ_{ij} by dummy variables. Thus we have a dummy variable taking the value one throughout the duration of a new job (if an individual changes jobs), and zero otherwise; and another similar dummy capturing a second or third new job (a maximum of three

[8] In most Australian states and territories students complete their compulsory schooling at age 15, and at age 16 in some others. For most students this corresponds with Year 10.

job changes can be observed within our sample period). The base case is the first job observed in the panel. This is similar to the approach of Loewenstein and Spletzer (1998) in their analysis of the returns to training across jobs.

The *training incidence* variables take the value one if the individual reported taking part in any employment-related training courses of that particular type during the last 12 months, and zero otherwise. Thus in the specifications of equation (1) reported in Table 3, we include the following separate types of training incidence: (i) employer-financed professional status training; (ii) employer-financed general skills training; (iii) other-financed professional status training; (iv) other-financed general skills training; (v) all other types of training not included in the first four categories[9]. (Recall that in Section 3 we defined employer-financed training to be all training for which the individual has not made any financial contribution.) We further divide the variables into training undertaken with the *current* employer (including training received in the past year) and training undertaken with *previous* employers. If training is general, it should be transferable across jobs and therefore we would expect it to have positive returns after job changes. However, general training may depreciate, so that the effect might for this reason be smaller in future than in current jobs.

4.2. The Estimates

The specifications in Table 3 estimate the impact of training incidence on the natural logarithm of hourly wages. The table reports the coefficients to the training variables obtained from fixed effects (FE) estimation of equation (1). Ordinary least squares (OLS) estimates are also

[9] General training spells were identified for types (i)-(iv) whenever the two categories (general skills and professional status) were used regardless of whether the respondent also coded other aims of training on the multiple-response. We tested the sensitivity of our results by identifying general training using these two categories only when they were the single response to the question. Although the estimates from this second procedure were of same sign and similar size, the cell sizes were very small indeed. Thus, we chose to keep and report our original specification.

presented and we shall briefly discuss these at the end of this section. The FE estimates are the preferred specification.

The second column of Table 3 reports FE estimates from a *balanced* panel of men present in all four waves, while the last column presents estimates from the *unbalanced* panel described above. Table A.2 of the Appendix reports the remaining coefficients, including year dummies for waves 4, 5 and 6. The number of person-year observations is given in the last row of Table 3. The unbalanced panel does, of course, contain more observations, since it includes not only men present in all four waves but also some men present in just two or three adjacent waves.

Insert Table 3 near here

First, consider the estimates from the balanced panel in the second column. We constructed a number of training variables, as shown in Table 3. The first panel reports the wage effects of employer-financed professional status training. This is training that respondents have defined as being to maintain professional or occupational standards, and which they have *not* paid for themselves. We label this "employer-financed professional status training". We further stratify this into training received with the current employer and training received at any previous employers during the sample period. The top panel of coefficients shows that *employer-financed professional* training has no statistically significant effect on wages, regardless of whether the training was received in the present or the previous job.

The second panel presents the estimated coefficients to the *employer-financed general* training variables. (We tested to see if the coefficients to each set of training type variables were equal and this hypothesis was rejected. Thus it is inappropriate to pool these forms of training.) These variables are of most interest to us in the present context. The estimates show that employer-financed general training received with previous employers raises present hourly

wages more than similar training undertaken with the current employer. Indeed, employer-financed general training appears to have a statistically significant impact on hourly wages only if the individual subsequently changed employer. Receipt of employer-financed training at previous employers is associated with 13.4% higher expected wages subsequently, whereas training at the current employer has no statistically significant effect on wages. Moreover, this difference in impact is significant.

These results are interesting because they show that general training that is employer-financed is clearly portable across employers and the impact is large. Hence workers' assessment of the training type seems to have been correct – it is genuinely transferable. Indeed, workers only get a return to this form of training if they change employers. But the puzzle for theory is that, if the labour market is perfectly competitive, employers should not be providing transferable training because of the potential hold-up problem. And yet they do, and the following might explain why. According to imperfectly competitive models of the labour market, firms might be willing to finance transferable training if the wage returns to training are increasing at a slower rate than the productivity returns (see Row [5] of Table 1).

Next we consider the impact of general training to which the individual has made some financial contribution. As noted in Section 2, this could be the individual paying course fees; purchasing materials or books, paying for travel or accommodation while attending the course; or taking unpaid time off to attend training courses. This represents only 16% of all training events of this type, and more closely approximates the orthodox human capital theory of general training – that it should be worker-financed. The third panel of Table 3 displays the coefficients to other-financed professional status training, while the fourth panel shows the coefficients to other-financed general skills training. Notice that the returns to other-financed professional status training are large and statistically significant for training in the current job and become even

larger if the person changes job subsequently. This is as one might expect. Individuals who self-finance professional status training are rewarded for it. The larger return after a job change may be because self-financed professional training can open up better job opportunities elsewhere, *ceteris paribus.*

From the fourth panel of estimates we see that other-financed general skills training has a negative effect on wages. The negative effect in the *current* job is consistent with several hypotheses. First individuals may be self-financing this type of training through lower wages, and that is why the estimated coefficient is negative. Second, such individuals may be providing less effort on-the-job, or may be working in lower status jobs in order to free their effort for the training they are financing themselves. Note that, while there is an even more negative effect for other-financed general skills training at previous employers, the cell size for this variable if very small. Therefore caution should be exercised in any interpretation of that coefficient.

The fifth panel of Table 3 includes "Other types of training". This is the residual category comprising all other forms of training apart from those described above. The impact of these variables is typically statistically insignificant. They are not of interest to our hypotheses and were included only as controls.

Finally the FE results using the unbalanced panel – reported in the last pair of columns in Table 3 - support the findings from the balanced panel, although the effects are smaller. For example, employer-financed general skills training is associated with 8.2% higher wages (t-statistic 2.9) only if the person changes jobs.

In the bottom panel of Table 3 we have the employer match-specific component, which is approximated by an employer-specific effect that is constant across individuals. This is modelled by including a dummy variable taking the value one throughout the first change of jobs and another one capturing any subsequent job change. The base case is the first job observed in the

panel. Note that none of the estimated coefficients on the "employer match" variables are statistically significant. [10]

While cross-sectional estimation of equation (1) is likely to produce biased estimates of α, since no account is taken of unobservable heterogeneity, it is nonetheless interesting to compare the FE estimates with the estimates obtained from ordinary least squares (OLS) using pooled person-year observations, as suggested by a referee. Our standard errors for the latter models are robust to heteroskedasticity, clustering by respondents' cross-wave identifier. The two sets of training variables in which we are particularly interested are employer-financed professional status training and general skills training. None of the OLS estimated coefficients are statistically significant, in sharp contrast to the FE estimates that difference out unobserved heterogeneity that is time-invariant. However in both cases the magnitude of the OLS coefficients is slightly larger after a job change than before.

4.2. Other Variables

The estimated coefficients of the extra explanatory variables not reported in Table 3 are given in Table A.2 of the Appendix. The most interesting additional findings are as follows. Using the FE estimates, we find that union membership is associated with an increase in the hourly wage of full-time men of just over 3%, although this is statistically significant only at the 10 percent level. (Union status is a dummy variable obtained from the question "Do you belong to a trade union or employee association?"). The effect is smaller but of the same sign as that found by

[10] A referee suggested that training returns for stayers might take some time to materialize, whereas training returns for movers might be more immediate. While there is no obvious theoretical reason why this should be the case, this is an interesting hypothesis to investigate in future work using a longer panel than we currently have available. The same referee also suggested that work experience, job tenure, and union membership might be potentially endogenous in equation (1). We therefore also re-estimated a reduced form model, in which we dropped those variables. All of our results reported above were found to hold in this new specification, though the magnitudes altered very slightly.

Rodgers (2004) for wave one, and Cai and Waddoups (2008) and Mavromaras et al. (2009) for waves one through six of the HILDA Survey.

Job tenure (proxying informal on-the-job training) has a statistically insignificant effect, as was also found by Booth and Bryan (2007) using comparable data from the British Household Panel Survey and Booth and Wood (2008) for waves one through four of HILDA. However, while Australian men do not benefit from an extra year of job tenure, they do benefit from labour market experience (total time in years in paid work). This has a statistically significant positive effect of between 1% and 2% for each extra year spent working, and is slightly larger than the comparable analysis for Britain. This effect was estimated to be non-linear, as also found by Watson (2005) for the first wave of HILDA and Booth and Wood (2008) for waves one through four.

A *ceteris paribus* change from a small establishment or workplace to a larger one brings a significant earnings boost of between 5% (for a shift into a workplace of 100-499 employees) and 7% (for a shift into a workplace of more than 500 employees). This a similar effect to that found for Britain by Booth and Bryan (2007) and by Rodgers (2004) and Watson (2005) for the first Australian wave.

Men over 40 years old see their earnings increased by between 13% and 21% with respect to otherwise equal workers in the 18-24 years age group. Effects of the same sign and similar size were found by Mavromaras et al. (2009) for the higher age groups. Finally, living in an urban area has a statistically insignificant effect on earnings, as was also found by Booth and Wood (2008) and Mavromaras et al. (2009).

4.3. ARobustness Check

The HILDA Survey did not ask respondents to indicate specifically whether or not their wages were reduced during training. It is possible that the wage returns at the current employer are measured too soon after training ended and reflect a salary reduction during the training period. We cannot test this precisely because we are not able to date training courses within that 12-month period between interviews. Following Booth and Bryan (2007), what we can do is to see if there is a negative correlation between pretraining wages and training received some time in the 12 month period after those wages were measured. Suppose that the pretraining wage is the wage the individual receives all through the subsequent year, including over the training period. Then if wages are actually lower during a training year, the coefficient on an additional regressor – next period's training - should be negative and statistically significant.

Insert Table 4 near here

We therefore ran additional specifications in which we included training to be received in the *next* year in our *current* wage equations. While the fixed-effect framework solves the problem of time-invariant unobserved ability, our test does assume - as noted above - that the current wage is the same as that to be paid during the following year's training spell, which could take place any time between one and twelve months after the current wage is observed. Our test also assumes that next year's training is not a response to a current unobserved productivity shock (which would not be removed by the estimation procedure). Nonetheless, if wages are lowered during training, the coefficient on the additional regressor – next period's training - should be negative and statistically significant. In fact, while the estimates reported in Table 4 are negative, they are typically statistically insignificant, suggesting that wages are not lowered during training. Thus, for our sample, employees do not appear to be paying for training indirectly through reduced earnings.

5. CONCLUSIONS

Our main results can be summarised as follows: First, a large proportion of work-related training received by full-time private sector men is general. Second, this general training is typically not paid for by the men receiving it. Third, our fixed effects estimates reveal that this employer-financed general skills training is associated with higher wages in future firms than in current firms. Our findings are similar to those of Booth and Bryan (2005, 2007). Those authors used British data to establish that employers do indeed pay the explicit costs of training that is general, and that employer-financed general training has a statistically significant positive impact on wages in the subsequent firm conditional on changing firm.

The fact that employers pay the direct costs of training that is transferable across employers is inconsistent with orthodox human capital theory without credit constraints. It is, however, consistent with some of the relatively recent training literature that assumes imperfectly competitive labour markets (see Row [5] of Table 1). It is also consistent with the hypothesis that firms offer credit-constrained workers binding training contracts whereby firms pay for general training and workers repay this 'loan' by receiving a post-training wage below their marginal product - see Loewenstein and Spletzer (1998), whose model is also based on imperfectly competitive labour markets. Our results from the Australian labour market corroborate theirs. In an empirical study using US data on wage and productivity growth, Barron, Black and Berger (1999) also show that employees pay only a small fraction of the training costs even though this training is reported by employers as being general.

In summary these separate studies corroborate our conclusion that the available stylised facts for Australia, Britain and the US are consistent with the predictions of human capital theory

based on imperfectly competitive labour markets rather than the alternative of perfect competition.

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Table 1: Predictions of Human Capital Theory

Row No.	Model	Who Pays	Divergence between Wages (w) and Net Marginal Productivity (MP) at Training Firm	Transferability of Training
[1]	Perfect competition, general training	Worker	None	Fully transferable
[2]	As above but with credit constraints	Sharing	w>MP during training and w<MP after training	Transferable but wage returns elsewhere greater than returns at firm providing training
[3]	Perfect competition, specific training	Sharing	w>MP during training and w<MP after training	Non-transferable
[4]	Perfect competition, mix of general and specific training	Sharing	w>MP during training and w<MP after training	Partially transferable; wage returns elsewhere less than returns at firm providing training
[5]	Oligopsonistic labor market, general training	Firm	w<MP during and after training, implying rents for the firm	Fully transferable, wage returns elsewhere greater than returns at firm providing training

Source: Booth and Bryan (2005).

Table 2. Means of training variables (Balanced sample).

	Current employer					**Previous employers**					**All employers**				
	Unweighted			*Weighted*		*Unweighted*			*Weighted*		*Unweighted*			*Weighted*	
	N	Mean	St.D.	Mean	St.D.	N	Mean	St.D.	Mean	St.D.	N	Mean	St.D.	Mean	St.D.
A. Training events															
2003	232	0.40	0.49	0.40	0.49	0	0	0	0	0	232	0.40	0.49	0.40	0.49
2004	220	0.38	0.49	0.36	0.48	7	0.01	0.11	0.01	0.10	227	0.39	0.49	0.37	0.49
2005	237	0.41	0.49	0.39	0.49	24	0.04	0.20	0.04	0.19	261	0.44	0.49	0.42	0.49
2006	200	0.34	0.48	0.32	0.47	44	0.08	0.26	0.07	0.26	244	0.40	0.49	0.38	0.49
All waves	889	0.38	0.49	0.37	0.48	75	0.03	0.18	0.03	0.17	964	0.41	0.49	0.39	0.49
B. Financing method															
Employer	768	0.33	0.47	0.32	0.46	68	0.03	0.17	0.03	0.17	836	0.35	0.48	0.34	0.48
Other	121	0.05	0.22	0.05	0.22	7	0.00	0.05	0.00	0.06	128	0.05	0.22	0.05	0.22
C. Training types															
Induction	14	0.00	0.07	0.00	0.07	0	0	0	0	0	14	0.00	0.07	0.00	0.07
Current skills	594	0.26	0.43	0.24	0.43	55	0.02	0.15	0.02	0.15	637	0.27	0.45	0.26	0.44
Future skills	232	0.10	0.29	0.10	0.30	27	0.01	0.11	0.01	0.10	257	0.11	0.31	0.11	0.31
General skills	419	0.18	0.38	0.17	0.38	53	0.02	0.15	0.02	0.15	465	0.20	0.40	0.19	0.39
Professional standards	461	0.20	0.40	0.19	0.40	42	0.02	0.13	0.02	0.13	499	0.21	0.41	0.21	0.41
Other skills	269	0.12	0.32	0.11	0.31	23	0.01	0.10	0.00	0.10	290	0.12	0.33	0.12	0.32
D. Job changes															
At least one job change											101	0.04	0.20	0.05	0.21
More than one job change											17	0.00	0.08	0.00	0.08
Number of observations											**2,328**				

Table 3: Effect of Training on Wages, OLS and FE Estimates

	BALANCED PANEL		UNBALANCED PANEL	
	OLS	FE	OLS	FE
Employer-financed professional status training				
Current employer	-0.015	(0.19)	0.01	0.004
	(0.64)	0.003	(0.53)	(0.22)
Previous employers	-0.009	-0.039	-0.000	-0.017
	(0.12)	(0.79)	(0.01)	(0.41)
Employer-financed general skills training				
Current employer	0.01	-0.013	0.000	-0.015
	(0.40)	(0.72)	(0.02)	(1.04)
Previous employers	0.081	0.134***	0.042	0.082**
	(1.03)	(2.82)	(0.87)	(2.09)
Other-financed professional status training				
Current employer	0.026	0.121***	-0.034	0.053
	(0.34)	(2.68)	(0.62)	(1.39)
Previous employers	-0.008	0.365***	-0.174	0.293***
	(0.10)	(2.64)	(1.36)	(2.88)
Other-financed general skills training				
Current employer	-0.113	-0.133***	-0.063	-0.062
	(1.56)	(2.82)	(1.23)	(1.58)
Previous employers	-0.174	-0.508*	0.211	-0.288**
	(1.49)	(2.68)	(1.16)	(2.48)
Other types of training				
Current employer	0.009	-0.037*	0.014	-0.022
	(0.37)	(1.88)	(0.78)	(1.24)
Previous employers	0.050	-0.117*	0.044	0.001
	(0.38)	(1.78)	(0.84)	(0.03)
Job changes				
First new job	-0.01	-0.016	-0.004	-0.006
	(0.26)	(0.67)	(0.12)	(0.37)
Second or third new job	0.016	-0.03	-0.003	-0.018
	(0.19)	(0.56)	(0.04)	(0.60)
Intercept	1.859***	1.01	2.181***	2.05***
	(13.13)	(0.71)	(16.87)	(3.47)
Log-likelihood	-	1193.30	-	2187.09
Rho	-	0.978	-	0.92
R-squared	0.485	-	0.465	-
Observations	2328		4335	

Notes: (1) Asterisks denote level of significance: *10%, **5%, ***1%. (2)Absolute z-statistics in parenthesis. (3) Other controls included but not reported are age bands, tenure, tenure squared, experience, experience squared and dummies for multiple jobs, casual contracts, union member, marital status, highest educational qualification, occupation, charity sector, establishment size, industry, region and year. See Appendix Table A.2 for full set of FE estimates.

Table 4: Professional status and general skills training disaggregated *(Adding next period's training spell)*

	Effect of training on wages	
	BALANCED PANEL	**UNBALANCED PANEL**
Employer-financed professional status training		
Current employer	-0.01	-0.002
	(0.48)	(0.11)
Previous employers	-0.054	-0.019
	(1.06)	(0.45)
Next period	**-0.02**	**-0.006**
	(1.17)	**(0.39)**
Employer-financed general skills training		
Current employer	-0.022	-0.024
	(1.05)	(1.34)
Previous employers	0.129***	0.074*
	(2.64)	(1.83)
Next period	**-0.017**	**-0.018**
	(1.00)	**(1.16)**
Other-financed professional status training		
Current employer	0.13**	0.063
	(2.56)	(1.48)
Previous employers	0.367**	0.305***
	(2.62)	(2.94)
Next period	**0.018**	**0.024**
	(0.39)	**(10.62)**
Other-financed general skills training		
Current employer	-0.157***	-0.09*
	(2.95)	(1.97)
Previous employers	-0.528***	-0.316***
	(2.66)	(2.68)

Next period	**-0.043**	**-0.048**
	(0.91)	**(1.15)**
Other types of training		
Current employer	-0.026	-0.003
	(1.17)	(0.16)
Previous employers	-0.106	0.019
	(1.58)	(0.4)
Next period	**0.013**	**0.026**
	(0.65)	**(1.46)**
Job changes		
First job change	-0.016	-0.032
	(0.7)	(1.45)
Second/Third job change	-0.0028	-0.037
	(0.51)	(0.72)
Intercept	1.11	2.04***
	(0.78)	(3.45)
Log-likelihood	1196.802	2193.27
Rho	0.976	0.902
Observations	2328	4335

Notes: (1) Asterisks denote level of significance: *10%, **5%, ***1%. (2)Absolute z-statistics in parenthesis. (3) Other controls included but not reported are age, tenure, tenure squared, experience, experience squared and dummies for multiple jobs, casual contracts, union member, marital status, highest educational qualification, occupation, charity sector, establishment size, industry, region and year.

APPENDIX: Table A.1: Means and Standard Deviations of Other Explanatory Variables (Balanced sample)

	Unweighted		*Weighted*	
	Mean	St. D.	Mean	St. D.
Hourly wage	24.17	11.03	23.97	10.89
Hours of work per week	46.05	8.09	45.91	8.17
A. Industry				
Mining	0.047	0.212	0.043	0.203
Manufacturing	0.306	0.461	0.31	0.460
Utilities	0.012	0.107	0.012	0.004
Construction	0.081	0.273	0.077	0.107
Wholesale trade	0.079	0.271	0.078	0.267
Retail trade	0.119	0.324	0.11	0.326
Accommodation, cafes and restaurants	0.032	0.177	0.033	0.181
Transport and storage	0.056	0.23	0.059	0.237
Communication services	0.023	0.149	0.025	0.155
Finance, insurance, property and business services	0.15	0.357	0.164	0.371
Education	0.02	0.141	0.016	0.127
Health and community services	0.026	0.16	0.026	0.16
Other services	0.045	0.208	0.043	0.201
Unknown	0.004	0.046	0.004	0.041
B. Occupation				
Managerial	0.11	0.313	0.108	0.311
Professional	0.178	0.383	0.177	0.382
Non-manual	0.239	0.427	0.234	0.424
Skilled manual	0.398	0.49	0.398	0.490
Unskilled	0.075	0.263	0.083	0.276
C. Multiple jobs	0.042	0.2	0.039	0.194
D. Day worker	0.791	0.407	0.781	0.415
E. Trade union member	0.313	0.464	0.317	0.466
F. Charity worker	0.057	0.231	0.052	0.222
G. Contract				
Fixed-term	0.036	0.185	0.038	0.226
Casual	0.056	0.23	0.054	0.295
Permanent	0.904	0.01	0.904	0.191
Other	0.004	0.02	0.004	0.02
H. Tenure	8.92	7.95	8.94	8.11
I. Experience	22.86	10.35	22.84	10.90
J. Workplace size				
1-19	0.309	0.462	0.317	0.466
20-49	0.316	0.465	0.316	0.466
100-499	0.261	0.439	0.251	0.435
500-.	0.112	0.316	0.114	0.318
Unknown	0.002	0.046	0.002	0.045

K. Highest education level				
Year 11 and below	0.234	0.423	0.24	0.428
Year 12	0.117	0.321	0.135	0.342
Tertiary (certificate/advanced diploma)	0.442	0.497	0.424	0.495
Bachelor	0.136	0.343	0.138	0.345
Postgraduate	0.071	0.257	0.063	0.245
L. Marital status				
Married	0.638	0.481	0.61	0.489
Cohabiting	0.123	0.329	0.105	0.307
Divorced / Widowed / Separated	0.067	0.25	0.075	0.264
Single	0.172	0.377	0.21	0.408
M. Age	40.81	9.75	40.84	10.21
N. Region of residence				
ACT	0.005	0.068	0.003	0.056
NT	0.009	0.094	0.014	0.119
QLD	0.233	0.423	0.241	0.429
NSW	0.269	0.443	0.285	0.452
VIC	0.239	0.427	0.231	0.422
TAS	0.034	0.182	0.028	0.165
SA	0.115	0.32	0.105	0.308
WA	0.096	0.294	0.093	0.289
Urban	0.701	0.458	0.731	0.444
O. Job changes				
At least one job change	0.04	0.20	0.05	0.21
More than one job change	0.00	0.08	0.00	0.08
N		**2,823**		

APPENDIX: Table A.2: Control variables coefficients (Table 3 estimations)

FE- Wage Equation – Coefficient estimates

	BALANCED PANEL	UNBALANCED PANEL
Employment attributes		
Multiple jobs	0.000	0.014
	(0.01)	(0.06)
Casual contract	0.017	0.063***
	(0.47)	(2.8)
Day worker	-0.045*	-0.036
	(2.25)	(2.28)
Union member	0.034*	0.022
	(1.63)	(1.37)
Tenure	-0.001	-0.000
	(0.36)	(0.11)
Tenure squared	0.000	-0.000
	(0.22)	(0.46)
Age(compared with 18-24 y.o.)		
25-29	0.022	0.007
	(0.56)	(0.23)
30-34	0.024	-0.021
	(0.45)	(0.5)
35-39	0.096	-0.037
	(1.46)	(0.71)
40-44	0.131*	-0.02
	(1.79)	(0.34)
45-49	0.158**	-0.012
	(1.97)	(0.19)
50-54	0.167**	0.007
	(2.13)	(0.09)
55-.	0.21**	0.011
	(2.17)	(0.14)
Marital status (compared with single)		
Married	-0.038	-0.025
	(0.96)	(0.81)
Cohabiting	0.039	-0.002
	(1.21)	(0.08)
Widowed/Divorced/	0.008	0.028
Separated	(0.16)	(0.66)
Occupation (compared with unskilled)		
Manager	-0.026	-0.001
	(0.76)	(0.04)
Professional	0.017	0.022
	(0.51)	(0.82)
Non-manual	-0.007	0.008
	(0.23)	(0.38)
Skilled manual	-0.028	0.015
	(1.09)	(0.77)
Experience	0.137**	0.095***

	(2.07)	(3.08)
Experience squared	-0.001***	-0.001***
	(4.12)	(6.54)
Highest education level (lagged 1 year and compared with Year 11 and below)		
Postgrad/Graddip	-0.082	-0.061
	(0.61)	(0.95)
Bachelor	-0.14	-0.058
	(1.21)	(1.02)
Cert/AdvDiploma	-0.045	0.036
	(0.70)	(0.91)
Year12	-0.073	-0.019
	(0.98)	(0.38)
Type of business (compared with private for profit)		
Charity worker	-0.012	-0.011
	(0.24)	(0.29)
Workplace size (compared with less than 20 employees)		
20_99 employees	-0.005	0.007
	(0.31)	(0.56)
100_499 employees	0.036*	0.042**
	(1.73)	(2.52)
500+ employees	0.077***	0.075***
	(2.97)	(3.57)
Industry (compared with other services)		
Mining	-0.065	0.006
	(0.93)	(0.11)
Manufacture	-0.097*	-0.047
	(1.85)	(1.29)
Utilities	0.055	0.124*
	(0.66)	(1.79)
Construction	-0.072	-0.008
	(1.27)	(0.21)
Wholesale trade	-0.111**	-0.057
	(2.03)	(1.51)
Retail trade	-0.098*	-0.042
	(1.73)	(1.08)
Hospitality	-0.027	-0.099**
	(0.42)	(2.01)
Transport	-0.162***	-0.056
	(2.68)	(1.3)
Community services	-0.058	0.009
	(0.69)	(0.16)
Finance	-0.088	-0.062*
	(1.62)	(1.67)
Education	-0.326***	-0.302***
	(3.23)	(3.43)
Health	-0.172*	-0.03
	(1.77)	(0.43)

Region of residence (compared with ACT)		
New South Wales	-0.309	-0.126
	(1.36)	(0.74)
Victoria	-0.219	-0.087
	(1.08)	(0.52)
Queensland	-0.519*	-0.314*
	(2.16)	(1.75)
South Australia	-0.253	-0.123
	(1.03)	(0.58)
Western Australia	-0.284	-0.088
	(1.12)	(0.44)
Tasmania	0.000	-0.502*
	.	(1.73)
Northern Territory	0.167	-0.129
	(0.56)	(0.62)
Urban	-0.037	-0.019
	(0.91)	(0.56)
Wave (compared with wave 3)		
wave4	-0.062	-0.002
	(0.97)	(0.07)
wave5	-0.129	-0.011
	(1.0)	(0.19)
wave6	-0.197	-0.014
	(1.03)	(0.17)

Notes: (1) Asterisks denote level of significance: *10%, **5%, ***1%. (2)Absolute z-statistics in parenthesis.