CREDENTIALS AND LEARNING IN THE LABOUR MARKET FOR YOUNG AUSTRALIANS

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DECLARATION OF ORIGINALITY

I hereby declare that this thesis is my own work, and that, to the best of my knowledge and belief, it contains no material previously published or written by another person, nor any material that has been accepted for the award of any other degree or diploma of a university or other institute of higher learning, except where due acknowledgement is made in the text.

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ABSTRACT

This thesis reports two tests of information-based theories of the returns to education, in the labour market for young Australians. The first is a test of whether these returns increase discontinuously with credentials such as high school graduation and university degrees. The second is a test of employer learning based upon how the returns to education, and to measures of ability not initially observed by employers, evolve with experience. These tests are conducted using a new data source which tracks individuals during the years in which they are entering and establishing themselves in the labour market, the period during which such credential and learning effects are most likely to be important.

It is found that there are large and highly significant credential returns to completion of bachelor’s degrees, of 14% for males and 10% for females. For males, around 39% of the returns to 15 years of education (relative to 9 or fewer years) are attributable to credential effects, while the corresponding figure for females is 36%. These effects are stronger among workers who were recruited through hiring channels that convey less initial information to employers. There is also evidence that post-secondary admission or attendance without completion of a credential may itself have a sorting effect in the labour market.

In the employer learning estimates, when parental education is used as a measure of ability observed by the researcher but not initially by employers, it is found to become increasingly correlated with wages as experience accumulates. However, no such result is found when a standardised test score is used as the ability variable – apparently because the information captured by this score is already observed by employers at the time of labour market entry. When the model is estimated separately by occupational class, the finding of employer learning holds only among white-collar workers. This may be due to the types of attributes that are reflected in parental education as a measure of initially unobserved ability.

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CHAPTER 1

INTRODUCTION

The positive correlation of wages with years of education is one of the most well-known empirical results in labour economics. For the past thirty years, this result has also been the subject of a vigorous contest between proponents of traditional human capital theory, and those of the alternative educational 'sorting' hypothesis. As part of this debate, an extensive – and at times highly creative – empirical literature has developed that seeks to discriminate between the implications of these two competing views.

The work reported in this thesis uses data from a longitudinal survey of young Australians to apply two tests of the educational sorting hypothesis. Both of these may be seen as modern versions of tests that can be traced back to a seminal early paper by Layard and Psacharopoulos (1974). The first is a test of whether the returns to education increase discontinuously upon completion of credentials such as high school graduation or bachelor's degrees. The second is a test of whether the return to education declines with experience – and conversely, of whether there are increasing returns to measures of ability that are observed by the researcher but not initially by employers. This second test is based upon the model of employer learning with statistical discrimination proposed by Altonji and Pierret (2001). It is believed that this is the first time either of these tests has been conducted in an Australian setting.

The purpose of the present chapter is to outline the structure and organisation of the chapters that follow. Chapter 2 describes the educational sorting hypothesis in greater detail, and provides a broad survey of the full range of empirical strategies that economists have devised to test its implications. The final section of chapter 2 undertakes a detailed review of both the empirical specification and results from previous research on the first of the two tests, which is based upon the estimation of credential, or ‘sheepskin’, effects in the returns to education. The second test, which seeks evidence of employer learning with labour market experience, is similarly reviewed at length in chapter 3. This chapter develops a simplified two-period version of the model in Altonji and Pierret (2001), and considers a number of extensions, before summarising empirical results from the existing literature on this type of test.

Descriptions of the data source and the construction of the dataset to be analysed in the subsequent chapters – as well as definitions of each of the variables to be considered – are provided in chapter 4, while the results are reported in chapters 5 through to 8. In particular, chapter 5 establishes the basic pattern of credential effects, while chapter 6 explores a number of extensions. These extensions take the form of an application of quantile regression analysis, an investigation of whether there is any sorting effect of post-secondary admission (without completion of any credential), and estimates of credential effects separately for workers who were recruited though different hiring channels, which may convey differing amounts of information to employers.

Similarly, chapter 7 is concerned with the basic pattern of employer learning effects, while chapter 8 develops a number of extensions. These take the form of separate estimates by occupational status and hiring channels, as well as a preliminary investigation of whether learning effects are stronger for incumbent employers than they are for outside firms. Finally, chapter 9 briefly concludes.

CHAPTER 2

TESTING THE EDUCATIONAL SORTING HYPOTHESIS

The correlation of higher levels of education with higher wages is one of the most robust and extensively documented empirical findings in applied economics. Traditional human capital theory explains this relationship by emphasising the causal role of years of education in enhancing workers' productivity. However since the early 1970s, an alternative 'sorting' hypothesis has emerged (Arrow 1973, Spence 1973). According to this view, higher levels of education may also be correlated with other attributes that make workers more attractive to employers, such as superior cognitive ability, greater perseverance, lower absenteeism, lower propensity to quit, and better health. Instead of these being inculcated through education, workers may innately possess different endowments of such characteristics, which may in turn be correlated with the costs or benefits of undertaking education. Moreover, these characteristics may not be observable by employers, or if they are observed it may be illegal to use them explicitly as criteria for hiring and remuneration. Given that employers observe education, and are free to take it into account in rewarding workers, they may thus favour better educated workers for reasons that go beyond the direct effect of education on workers' productivity. Recognising this, workers who know they possess more desirable attributes will face a heightened incentive to invest in education, because of the information this conveys to employers about these characteristics.

'Signalling' occurs when workers choose their level of education at least in part to communicate information to employers regarding their characteristics. 'Screening' occurs when employers design minimum education requirements and wage offers with a view to identifying workers' unobservable characteristics through a process of self-selection. Succinct modern accounts of the theory of signalling and screening may be found in Brown and Sessions (2004), Gibbons (1992, chapter 4), Hirshleifer and Riley (1992, chapter 11) and Riley (2001), among others. Formally, in signalling models it is the informed party (workers) who make the first move through their choice of education. Given a worker's observable characteristics such as education, and the conditional probabilistic beliefs of the employer regarding the relationship between these characteristics and productivity, the employer responds by making a wage offer equal to expected productivity. In screening models it is the uninformed party (employers) who move first, by defining a 'menu of contracts' that sets out the relationship between wage offers and levels of education. Given their own characteristics, workers respond by choosing the level of education that maximises their utility.

Signalling models may suffer from a multiplicity of equilibria when they permit too much freedom in the permissible beliefs of employers (Spence 1973). Conversely, when there is a preponderance of higher ability types, screening models may suffer the opposite problem of non-existence of equilibrium (Rothschild and Stiglitz 1976). However, with appropriate refinements to the equilibrium concepts, each of the classes of models can be shown to share the same unique efficient separating equilibrium. This equilibrium is efficient in the sense that each ability type needs only invest in the minimum amount of education required to credibly differentiate it from the one below. Following Weiss (1995) and Brown and Sessions (2004), the term 'sorting' can thus be used to refer to both signalling and screening.

The debate over human capital versus sorting is sometimes framed as a dichotomous choice between mutually-exclusive alternatives. This arises in part because the simplest expositions of sorting theory make the simplifying assumption that education has no effect on workers' productivity. In reality, proponents of sorting do not deny the productivity-enhancing effect of education. Instead, sorting models are best seen as an extension of human capital theory, in that the latter can only accommodate unobservable attributes that affect productivity where these are *uncorrelated* with education (Weiss 1995). However, to the extent that sorting is important, the private rate of return to a year of education no longer reflects just the effect of that year on productivity, but also the value of the information communicated by that year regarding a worker's characteristics. Because of this, there will be a discrepancy between the private and social returns to education, and workers' decisions over their investments in education will thus be distorted. It is for this reason that empirical research to establish the magnitude of sorting effects continues to be of ongoing research and policy interest.

The research reported herein uses data from a longitudinal study of young Australians to apply two modern tests of the sorting hypothesis and its implications. The first of these is a test of whether the returns to education increase discontinuously upon completion of credentials such as high school graduation or bachelor's degrees. The second test is concerned with how the returns to education and measured ability evolve with labour market experience, and is based upon a model of employer learning with statistical discrimination, as proposed by Altonji and Pierret (2001).

The purpose of this chapter is to survey the broad range of empirical strategies that economists have devised to test the educational sorting hypothesis. The research literature on the first of the two tests will be reviewed in detail in the final section of this chapter. The second test is discussed at length in chapter 3.

Early research strategies

Prior to the formalisation of the sorting hypothesis in the early-1970s, an earlier body of empirical research was concerned with whether estimates of the return to education in earnings equations were biased upward as a result of the researcher lacking adequate measures of workers' ability. If there were attributes that were observed by employers but not the researcher, and these were positively correlated with education, then the effect of these characteristics on productivity would be picked up by the coefficient on education. This early research proceeded by assessing the extent of change in the coefficient on education when additional explanatory variables, such as IQ or other ability test scores, were added to the model. Results of this early literature are summarised in Appendix 1 of Layard and Psacharopoulos (1974). The generally-accepted conclusion from this type of work is that the inclusion of such variables had little effect on the estimated return to education.

Weiss (1995, p. 135) notes that such findings are sometimes mistakenly taken as evidence against the sorting hypothesis – on the grounds that unobserved ability apparently does not account for a very large part of the return to education – although it should be acknowledged that Layard and Psacharopoulos do not themselves fall into this trap. The critical distinction is that whereas this early work was concerned with the effect of ability observed by the market but not the researcher, in sorting theories the 'omitted' ability variables are assumed to also not be observed by employers. Thus in sorting theories, employers must form expectations over these variables conditional on the information available to them, most notably education, and take into account the correlation of these variables with education in making wage offers. Therefore sorting theories predict that the coefficient on education will reflect the correlation of education with unobserved ability, but that *this arises through the inference process of employers, and not as a result of an erroneous inference on the part of the researcher*. Accordingly, if the researcher has access to an ability variable, but this is not observed by the firm, then adding that variable to the wage regression should have little effect on the education coefficient as it plays no part in wage determination within the firm. Nonetheless, as will be seen in the next chapter, this conclusion is modified to the extent that the ability variable observed by the researcher becomes increasingly correlated with information that is observed by the employer with experience.

A second early strand of research is typified by the paper by Psacharopoulos (1979), which in turn is closely related to a section of the earlier paper by Layard and Psacharopoulos (1974). However it is possible that the earlier paper avoids the most serious conceptual error made in the later one. The reason is that Psacharopoulos (1979) explicitly interprets the sorting hypothesis as entailing ‘irrational’ behaviour on the part of employers, in that they systematically overestimate the productivity of more highly-educated workers, and underestimate that of less-educated ones, at the time of hiring. According to his ‘test’ of the sorting hypothesis, upon recognising the error of their ways, employers will revise the wages of more highly-educated workers downward, and those of the less educated upward, as they learn workers’ true capabilities through experience. This leads to the implication that under the sorting hypothesis, the return to higher levels of education will diminish with experience.

However as pointed out by Riley (1979), in equilibrium models of sorting starting wages should equal expected productivity conditional on the information available to employers at the time of hiring – including education. That is, although an employer’s initial assessment of a worker’s productivity may be imprecise, it should at least be unbiased. With experience, firms indeed learn that some highly-educated workers are less productive than first expected – and their wages will be adjusted accordingly – but this should be offset on average by other highly-educated workers who prove to be even more productive than first predicted (and similarly among the less-educated). According to this argument, the estimated return to education should be independent of experience – notwithstanding that learning indeed takes place – although the fraction of the variance in wages that can be explained by education will decline. Riley’s insight was subsequently formalised by Farber and Gibbons (1996), and is stated as proposition 3.1 in chapter 3.

Using data on males from the 1975 UK General Household Survey, Psacharopoulos (1979) drew a distinction between a non-competitive sector of the economy (public administration) and a competitive one (distributive trades), where sorting is presumed not to be as widespread in the latter, and presented two pieces of evidence that were said to contradict the sorting hypothesis. Firstly, the ratio of mid- to early-career earnings increases with education, and this is more pronounced in the competitive sector. However as we have seen, there is no reason to expect this ratio to decline as hypothesised by Psacharopoulos, while Riley (1979, p. S231) suggests that the finding of an increase may reflect the fact that more educated workers are selected into positions in which they receive more on-the-job training.

Secondly, Psacharopoulos estimates standard earnings equations separately for the two sectors. Given that these equations do not include any interaction of years of education with experience, it is not clear how this strategy sheds any light on his stated hypothesis. Nonetheless, he points to a higher estimated return to education in the competitive sector, and a superior goodness-of-fit for his model in this (presumptively *unscreened*) sector, as evidence against sorting. While he does not provide a formal justification for why these results are not consistent with sorting, a suitable framework for interpretation may be found in the model of Riley (1979) which is discussed below. It turns out that according to that model, the finding of a higher return to education in the competitive sector is indeed consistent with Psacharopoulos' conclusion that sorting is less important in that sector.

However, Riley (1979) also argues that a standard earnings function should have greater explanatory power in the *screened* sector. The reason is that it is here that the information sets of the employer and researcher are most closely aligned. Workers who look alike to the researcher will also look alike to the firm, because both rely upon easy-to-observe variables such as education, and are equally ignorant of the harder-to-observe determinants of productivity. Wages within the firm are determined primarily on the basis of education, and that is precisely the model posited by the researcher. By contrast, where sorting on the basis of education is *not* important, this will be because the employer has access to superior information; if that information is not available to the researcher then the result is a *poorer* fit of the earnings equation in the unscreened sector. Thus, Psacharopoulos' finding of superior fit in the competitive sector runs counter to his interpretation of it as the unscreened sector.

Notwithstanding the flaws in this paper, Psacharopoulos' idea of comparing unscreened and screened sectors has been taken up by a number of other authors. The seminal papers in this strand of research will be discussed below, while a more exhaustive survey is provided by Brown and Sessions (2004).

Layard and Psacharopoulos (1974)

The paper by Layard and Psacharopoulos (1974) was one of the first to assess the empirical significance of the sorting hypothesis, after it had been formalised by Arrow (1973) and Spence (1973). While the evidence they presented is now quite dated, their paper nonetheless remains significant as a direct antecedent of large parts of the more recent research in this field – including that which will be reported herein. Taking a stance in defence of human capital theory against the challenge posed by sorting, Layard and Psacharopoulos highlighted three testable implications of sorting, each of which they claimed was not supported by the evidence. Notwithstanding this conclusion, the first two of their hypotheses in particular have spawned separate ongoing research literatures, toward each of which the present research aims to make a contribution. These more recent bodies of research will be detailed in the final section of this chapter, and in chapter 3.

The first hypothesis is that if education plays a sorting role, then the fact that a worker successfully completes a course should convey more information to an employer regarding their ability than the mere fact that they attended it. If this is the case, the returns to education will accrue disproportionately to completed qualifications as opposed to years of education, and the highest marginal returns should be observed in years in which a credential is attained.

To investigate this first hypothesis, Layard and Psacharopoulos compiled rates of return for dropouts and course completers from a number of pre-existing research studies, and concluded from this exercise that the dropout rates of return were generally not much lower than those of completers. However they also acknowledged some limitations to the data that was available to them at the time of their paper. In particular, they conceded that their data typically compared returns to completion of one course to completion of the one below it – as opposed to non-completion of the first course – and did not permit a decomposition of the return to each individual year of a course. More modern research investigating so-called 'sheepskin' or credential effects – which takes advantage of more ideal data than that which was available to Layard and Psacharopoulos – is outlined in the final section of this chapter.

Layard and Psacharopoulos secondly asserted that under the sorting hypothesis, the effect of education on earnings should diminish with experience, holding constant the effect of ability as observed by the researcher, *while the effect of an ability variable observed by the researcher should increase*. The idea behind the latter claim is simply that the ability variable becomes increasingly correlated with the information learned by employers with experience regarding workers' true productivity. Because this hypothesis is stated conditional on ability observed by the researcher (A^R in the notation of Layard and Psacharopoulos), it is more precise than that of Psacharopoulos (1979) and may even be interpreted as anticipating the result formalised by Altonji and Pierret (1998, 2001) as detailed in the next chapter. This is not how the hypothesis has customarily been interpreted in the literature, for instance by Wolpin (1977, p. 955). However if the interpretation here is correct, Layard and Psacharopoulos may have avoided the error in the later paper by Psacharopoulos (1979), which explicitly presupposed irrational behaviour on the part of employers.

To examine their second hypothesis, Layard and Psacharopoulos presented results from an earlier study by Taubman and Wales (1973) from which they conclude that the effect of higher levels of education on earnings increase with age, as does the effect of measured ability. They argue that this is incompatible with sorting, unless the declining effect of education is being offset by a sufficiently large training effect. A modern statement of the employer learning hypothesis, due to Altonji and Pierret (1998, 2001) is given in the next chapter, along with a discussion of more recent research investigating this hypothesis.

Finally, Layard and Psacharopoulos point out that employers will only be willing to use education as a sorting device – and pay a higher wage to graduates – if the cost of identifying high ability non-graduates is at least as great as the wage differential between graduates and non-graduates. If there were cheaper alternative methods of sorting – for example, if a private institution could test and certify the ability of non-graduates – then education would no longer be demanded for this purpose. Since the earnings differential is large, yet no such institutions exist, Layard and Psacharopoulos conclude that sorting must not be important.

However, this proposition is only strictly true if education does not enhance productivity, such that its only value to employers is in sorting. Otherwise, a test would only be profitable if its cost were less than the wage difference between graduates and non-graduates *adjusted for differences in their productivity*. Stiglitz (1975) also objects that the first firm to adopt testing runs the risk that other employers may poach its high ability non-graduates, and so there will be insufficient private incentive to invest in testing. Somewhat contradictorily, Weiss (1995) suggests that the first firm to adopt testing will have monopsony power over its non-graduate employees, because other employers may not recognise the test. The more general observation here, also articulated by Weiss, is that starting from a position of separating equilibrium without testing, the first player to adopt testing is making an out-of-equilibrium move, and so it is not at all clear how the rest of the market would respond to such an action.

Screened and unscreened sectors

Riley (1979) argues that directly observing a worker's productivity is more difficult in some jobs than in others, and that it is in the harder-to-observe sectors or occupations that sorting will be most important. On this basis, he uses sorting theory to derive implications that can be tested by comparing 'screened' and 'unscreened' groups of workers.

The basic insight of Riley's model is that where sorting is important, workers at every ability level other than the lowest one will choose more education than they would if their ability were readily observable, such that they may separate themselves out from less able workers. That is, holding ability constant, years of education will be higher in the screened sector than in the unscreened one. Conversely, it follows that holding education constant, ability and hence also productivity will on average be higher in the unscreened sector (see figure 2.1). Finally, since ability increases more rapidly with education in the unscreened sector, the rate of return to education should be greater when sorting is *less* important.

Furthermore, if sorting is important, a model that only includes variables that are easy for both employers and the researcher to observe (such as education) should fit the data well early in workers' careers. In particular, a standard earnings function should fit better in the screened sector than in the unscreened one. Over time, as a result of learning by employers, the portion of the variation in earnings that can be explained by education will decline (although, as has been emphasised, this need not imply that the estimated coefficient on education will decline). Thus the ratio of unexplained variance in earnings in the screened group, relative to the unscreened group, should increase over time. Riley tests these implications of his model using US data from the Current Population Survey (CPS) for 1971 to 1975. He interprets his results as strongly supporting the sorting view.

In an earlier but closely related paper, Wolpin (1977) identifies the self-employed as an unscreened group, arguing that they have less need to acquire education for signalling purposes compared to salaried workers. Using data from the NBER-Thorndike sample of World War II veterans, he finds that the self-employed have only slightly lower mean levels of education and ability, and concludes from this that education has only a minor sorting function. However, he also finds that mean earnings are substantially higher among the self-employed. While part of this finding no doubt reflects non-labour earnings, Riley (1979) emphasises this particular result and interprets it as evidence in support of sorting.

Building upon the contributions of Riley (1979) as well as Psacharopoulos (1979), a large body of research has sought to test the sorting hypothesis through comparisons of screened and unscreened sectors or occupations. Much of this work is reviewed in the survey by Brown and Sessions (2004).

The hiring process

Albrecht (1981) focuses on the hiring process of the employer, and in particular on whether the availability of alternative sources of information concerning a job applicant has any effect on the weight given to education in the hiring decision. If part of the value of education to employers is due to its sorting function, then employers will rely more heavily on education when they have less *a priori* information about job applicants. Albrecht tests this hypothesis using data on applicants for blue-collar positions at a Volvo factory in June 1978, assigning applicants to the high information category if they were recommended by an existing employee. This information variable is interacted with education in a probit model of the hiring decision. If education has a sorting function then the interaction term is predicted to be negative, in other words the impact of greater education on the probability of being hired should be smaller for those applicants about whom the firm has better information. It turns out that the interaction term takes the predicted sign but it is quite small, such that the null hypothesis of no sorting cannot be rejected.

In a more recent paper, Albrecht and van Ours (2005) use data from a Dutch vacancy survey to test whether employers are more likely to relax the educational requirements for a position when workers are recruited through informal channels. They find that workers who were hired through informal channels were more likely to have a lower level of education than that which was originally sought by their employer when the position was advertised, and conclude that this provides evidence in favour of sorting.

Natural experiments

A number of recent papers propose strategies to test the educational sorting hypothesis by taking advantage of exogenous differences or changes in policies and institutions to conduct natural experiments. These experiments have the effect of randomly assigning individuals into comparison and treatment groups, whose educational choices or earnings can be compared without fear of contamination by selection effects.

Lang and Kropp (1986) exploit variations in compulsory minimum schooling laws across US states and over time to examine whether these laws have ‘ripple’ effects on the choices of those not directly affected by the laws. According to human capital theory, there should be no effect on those whose choices are not directly constrained by the laws. On the other hand, the sorting hypothesis predicts that there will be an indirect effect on those above the minimum schooling age, who will need to further increase their schooling in order to separate themselves out from those whose schooling is directly increased by the laws. Lang and Kropp test the effect of compulsory attendance laws at the ages of 14 and 15 upon rates of enrolment at ages 16 and 17, and find evidence of a positive effect – contrary to human capital theory and consistent with the sorting hypothesis.

Chevalier, Harmon, Walker and Zhu (2003) apply Lang and Kropp's insight to study the effect of the 1973 increase in the school leaving age from 15 to 16 in England and Wales. They compare the schooling choices of the cohorts immediately before and after this reform took effect, and find that its effect was simply to increase the schooling of those who would previously have left school at age 15 by one year – with no discernable effect upon the choice of higher levels of schooling. They conclude that this supports the human capital interpretation, and that there is little evidence of sorting.

Bedard (2001) employs a related strategy, exploiting regional variations in access to college education. To illustrate the idea behind this test, consider three levels of education – high school dropout, high school graduate, and college enrolee – and suppose some individuals are constrained by lack of access to a college in their local region. Then in regions where access is constrained, the average ability of high school graduates will be greater than where higher-ability individuals are able to advance to college. If the sorting hypothesis holds, then lower-ability individuals in constrained regions will have an incentive to attain high school graduation, to take advantage of pooling with these constrained higher-ability individuals. By contrast, human capital theory predicts that college access should only affect choices at the margin between high school graduation and college enrolment, and not at the margin between dropping out and graduating from high school. Bedard finds that high school dropout rates are higher where there is access to a post-secondary education institution in the local labour market, and concludes that this finding supports the educational sorting hypothesis.

Whereas the preceding papers used natural experiments to study the choice of years of education, Tyler, Murnane and Willett (2000) consider the effect of a natural experiment upon earnings. Their paper is concerned with the signalling value of the General Educational Development (GED) credential, which serves as a pathway to high school certification for dropouts in the US. The quasi-experimental variation they exploit arises from the fact that passing standards on the GED differ across states, such that individuals having the same test score may be awarded a GED in some states but not in others. Assuming that individuals with similar test scores have similar levels of human capital, the authors focus on individuals whose scores place them at the threshold of passing the exam, and use the variation between states in their GED status to extract the signalling value of the credential net of human capital effects. To control for differences in state labour markets, these individuals are compared to others whose score was high enough to meet the higher passing standard (and thus receive a GED regardless of state) to construct a difference-in-differences estimator. Using this procedure, the authors estimate a signalling value of the GED upon earnings in the range of 10% to 19%.

The final section of this chapter surveys existing research on the first of two tests that will be implemented in later chapters.

‘Sheepskin’ or credential effects

As noted above, Layard and Psacharopoulos (1974) argued that a corollary of the sorting hypothesis was that the increases in earnings due to higher levels of education should occur discontinuously upon completion of a credential, rather than as a smooth function of the length of time spent in study. More generally, a weaker statement of this ‘sheepskin hypothesis’ holds that even where education has a direct productivity-enhancing effect, there will be additional returns associated with the completion of a credential, over and above those that can be explained as a function of years of study alone. More concretely, those who complete a credential will earn more than those who do not, even when the latter undertake the same number of years of study. A ‘sheepskin’, or credential, effect is thus the difference in earnings between those who hold a credential and those who do not, over and above the return that can be explained by the differences in their respective years of education. In the literature on sheepskin effects, the return to years of education is typically interpreted as being due to human capital effects, while the additional return to credentials is identified with the sorting role of education.

Spline and step-function estimates of credential-year effects

The classic human capital earnings function models an individual’s earnings as a log-linear function of their number of years of education. That is, each additional year of education increases earnings by the same percentage amount, irrespective of the total number of years. For this reason, researchers in the early literature on sheepskin effects – who only had access to data on years of education, and could not directly observe whether individuals held any completed qualifications – sought evidence on deviations from log-linearity as support for the sheepskin hypothesis. In particular, these researchers sought to identify discontinuous jumps in earnings occurring after the years of education normally associated with the completion of a credential. Since they inferred the presence of a credential from the completion of a given number of years of education, these researchers in effect reported estimates of *credential-year* effects, as distinct from the direct effect of a credential *per se*.

In their original paper, Layard and Psacharopoulos (1974) were unable to differentiate between the earnings of dropouts according to the number of years of a course they had completed. They acknowledged that it would have been preferable to show the gains in earnings associated with each individual year, culminating in the year in which a credential is successfully attained. Such an analysis was first reported by Hungerford and Solon (1987), who used US data on white males from the May 1978 CPS to report estimates of credential-year effects for several different specifications of the returns to years of education.

The first specification contains a *spline function*, which models log wages as a *piecewise linear* function allowing for both discontinuous increases and slope changes in the returns to education at 8, 12, and 16 years. Credential-year effects are inferred from discontinuous jumps in any of these years. Since the exact spline function specification estimated by Hungerford and Solon reappears in a number of subsequent papers, it is set out in full below:

$$\begin{aligned}\ln W = \alpha_0 + \alpha_1 x + \alpha_2 x^2 + \beta_1 s + \beta_2 D(s \geq 8) + \beta_3 D(s \geq 8) \cdot (s-8) + \beta_4 D(s \geq 12) \\ + \beta_5 D(s \geq 12) \cdot (s-12) + \beta_6 D(s \geq 16) + \beta_7 D(s = 17) + \beta_8 D(s = 18)\end{aligned}$$

where x denotes experience, s denotes years of education, and $D(\ )$ is a dummy variable that takes the value 1 whenever the condition within parentheses holds, or zero otherwise. This model is also depicted in figure 2.2, which is based upon figure 1 in Belman and Heywood (1997). In this specification, the coefficients β_2, β_4 and β_6 capture credential-year effects at 8, 12 and 16 years of education respectively, while β_3 and β_5 are included to allow slope changes after 8 and 12 years. Since by definition $D(s \geq 8) = 1$ whenever $D(s \geq 12) = 1$, and

$D(s \geq 12) = 1$ when $D(s \geq 16) = 1$, β_4 and β_6 are defined as the *marginal* returns to the credentials presumed to be received after 12 and 16 years of education, over that of the next credential below. In their estimate of this specification, Hungerford and Solon find a significant credential-year effect only at 16 years, which they interpret as a sheepskin return to graduation from college.

In another specification, Hungerford and Solon relax the assumption of log-linearity by replacing the linear years of education term with a string of dummy variables for 1 through to 18 years of education. This treats the profile of wages as a *step function* of years of education, with no restrictions imposed upon its shape:

$$\ln W = \alpha_0 + \alpha_1 x + \alpha_2 x^2 + \sum_{i=1}^{18} \beta_i D(s = i) \qquad 2.1$$

In this specification, any large jumps in the coefficients for individual years are interpreted as evidence of sheepskin effects. Hungerford and Solon find particularly large steps after credential years, as well after the first year of college. They interpret the latter result as indicating that college admission may itself have a sorting function, as first suggested by Arrow (1973).

Whereas Hungerford and Solon focused on white males, Belman and Heywood (1991) used the same data source to report separate estimates for minorities and females. Their model is a replication of the spline function specification of Hungerford and Solon. The strongest finding is that for both black males and black females, there are stronger credential-year effects at 16 years of education than is the case for white males. For white females, there is a credential-year effect at 12 years, but not at 16 years. Belman and Heywood also note that the variance explained by their model is greater for minorities and females than for white males, consistent with education being a better quality signal of productivity in these groups.

Heywood (1994) used 1989 data from the CPS to replicate the spline function specification of the earlier papers for four separate sub samples, partitioned according to union / non-union and public / private sector status. For the full sample of white males, significant credential-year effects are found at 8, 12 and 16 years of education. This pattern is carried over into the private non-union subgroup. However results for the remaining sub samples are weaker, with no significant positive effect at 16 years in any of these groups. Indeed for unionised public-sector workers, none of the credential-year effects are positive and significant. Similar results are obtained when the step function specification is estimated for each of the subgroups.

In contrast to authors such as Riley (1979) and Farber and Gibbons (1996), Belman and Heywood (1997) draw on a model of job matching to argue that the return to educational signals will *decline* with labour market experience. This claim rests on the argument that over time, workers are placed into better-quality job matches so that on average their productivity and wages increase. Moreover, since their model implies that more highly-educated workers are less likely to initially find themselves in jobs in which their productivity is constrained by an inappropriate match, the increase in average productivity is smaller for those with higher educational signals, resulting in a decline in the return to higher signals with experience.

To test their hypothesis, Belman and Heywood (1997) used 1991 CPS data on non-black males, which they partition into five age-based cohorts. They estimate the spline function specification, allowing the coefficient estimates to vary across cohorts. Consistent with their model, they find significant credential-year effects at 12 and 16 years of education for the two youngest cohorts (up to 40 years of age), but not in the older ones. Moreover, they find that the size of the estimated effects after 12 and 16 years of education decline monotonically with age through to age 60. Finally, comparing returns in 1991 with returns to a matched cohort in 1979, they also find a significant credential-year effect at 16 years of education in 1979 for two of the older cohorts for whom no such effect is observed in 1991.

These early studies did not directly observe the completion of a credential, but rather inferred it from the standard length of time taken to complete one. They therefore implicitly assumed that everyone who completes a credential does so in the standard number of years, and that everyone who studies for the standard number of years will attain the credential. To the extent that these assumptions are violated, estimates of sheepskin effects based such models are likely to be biased downward due to measurement error (Jaeger and Page 1996).

Even leaving aside the fact that the researcher does not directly observe an individual's actual credentials, a further drawback of the spline function approach is that the selection of the years of education at which discontinuities are permitted is determined by the prior expectations of the researcher. By contrast, a step function imposes no restrictions on the shape of the wage-education profile, leaving open the possibility of discontinuous jumps in the returns to education in years other than those conventionally associated with the completion of a credential. For example as noted above, Hungerford and Solon (1987) observe a sizable step at 13 years of education, which they interpret as suggesting that admission into college may itself have a sorting function in the labour market.

Patrinos (1996) obtains another such result using a step function model in the context of a developing country (Guatemala). Most notably, he observes a very large increase in earnings associated with the completion of three years of education, yet not after six years, which is associated with the completion of primary schooling. Such a result confirms that the returns to education are indeed non-linear, yet it does not support an interpretation in terms of credential-year effects. Patrinos offers an alternative explanation in terms of a threshold level of human capital, arguing that three to four years of education corresponds to the minimum required for workers to attain a functional level of literacy. The possibility of such thresholds casts doubt more generally on the interpretation of studies that seek to infer credential effects from data on years of education alone, since they suggest a human capital explanation for discontinuities in the log wage profile, which might not be due to credential effects after all.

Direct estimates of credential effects

Accordingly, a priority of more recent research has been to establish more robust confirmation of the existence of sheepskin effects, using datasets that contain information on both individuals' years of education and a record of the credentials they complete. Such a data source makes it possible to specify a more flexible specification in which, for example, the coefficient on the 12th year of education captures the potentially larger amount of human capital embodied within that year, while the coefficient on a high school diploma captures the marginal return to attaining that credential, holding years of education constant.

Unfortunately, progress in this research has been constrained by the fact that standard labour and demographic surveys do not typically record data on both dimensions of educational attainment. As a result, researchers have often had to resort to one-off and specialised data sources. For example, the papers by Jaeger and Page (1996) and Park (1999) both exploit the same one-off change in the structure of the US CPS.

In 1992, the CPS changed its question on educational attainment from one based on years of education to one focusing on credentials. Jaeger and Page (1996) matched individuals from 1991 and 1992 to construct a dataset containing responses to both questions. They use this data firstly to replicate the spline and step function specifications, and then to estimate a model in which the step function is augmented with dummies for each type of credential:

$$\ln W = \alpha_0 + \alpha_1 x + \alpha_2 x^2 + \sum_i \beta_i D(s=i) + \sum_j \gamma_j D(q=j) \qquad 2.2$$

where q indexes completed credentials. Since this specification incorporates a step function, it maximises the amount of wage variation that can be explained by years of education. Even so, Jaeger and Page report direct estimates of the credential effects γ_j for high school and college graduation that are more than twice as large as the corresponding credential-year effects obtained using a spline function. Further, by comparing the step function estimates before and after the inclusion of credential variables, they find that a substantial portion of the total return to education is attributable to sheepskin effects. Finally, in contrast to Belman and Heywood (1991), using the more flexible specification they find no significant differences in the returns to high school and college graduation between demographic groups.

Park (1999) used CPS data from February 1990 (when both educational attainment questions were asked as a pilot for the subsequent change) to estimate a model in which the dummy variables for years of education are fully interacted with dummies for credentials:

$$\ln W = \alpha_0 + \alpha_1 x + \alpha_2 x^2 + \sum_i \sum_j \beta_{ij} D(s=i) \cdot D(q=j)$$

This specification allows, for example, the credential effect of a bachelor's degree to differ between those who have 15 and 16 years of education. Once again, this specification results in much larger estimates of the sheepskin effects of high school and college graduation, when compared to the corresponding estimates obtained from a spline function.

Following the initial work on direct estimates of sheepskin effects in the US, a number of authors have sought to replicate these results using data from other developed countries. Denny and Harmon (2001) used data on men from the International Adult Literacy Survey (IALS) to estimate the following specification for five countries:

$$\ln W = \alpha_0 + \alpha_1 x + \alpha_2 x^2 + \beta s + \sum_j \gamma_j D(q = j) \qquad 2.3$$

That is, the effect of education is modelled by a log-linear specification, with discontinuous increases upon completion of a credential. Since this does not permit as much freedom for earnings to be explained by years of education, it is a more restrictive specification than the step function employed by Jaeger and Page. As a result, it is possible that it may overstate the contribution of credentials relative to years of education. Indeed, the point estimates of credential effects obtained by Denny and Harmon are generally larger than those estimated by previous authors; however they are also not estimated with great precision due to relatively small sample sizes. Denny and Harmon find significant credential effects for university degrees in the United States, Sweden, Britain and Ireland, but not for Canada. They do not find a statistically significant credential effect for high school graduation in any of the countries although, as noted above, their point estimates are in some cases quite large.

In general, most of the recent papers reporting direct estimates of sheepskin effects have broadly followed the step-function-plus-credentials specification introduced by Jaeger and Page (equation 2.2 above). These studies include Gibson (2000) for New Zealand, using data from a one-off Education and Training Survey (ETS) conducted in 1996; Ferrer and Riddell (2002) for Canada, using 1996 census data; and Bauer, Dross and Haisken-DeNew (2005) for Japan, using data from the Japanese Panel Survey on Consumers (JPSC) from 1993 to 1997.

Table 2.1 summarises the implied percentage increases in earnings, calculated from the reported estimates of credential-year effects at 12 and 16 years, or the direct estimates of credential effects for high school and college / university graduation, for each of the papers discussed above. A subtle technical point concerns the baseline comparison group relative to whom credential effects are defined. In spline function specifications, the credential-year effect for 'college' is always defined relative to that for 'high school', since $D(s \geq 12)$ is always equal to one by definition whenever $D(s \geq 16)$ is also set to one. However in direct estimates of sheepskin effects, the credential effect for college / university graduation is variously defined relative to no credentials, relative to high school graduation, or relative to some college with no degree. Where possible, these reference groups have been noted in the table; however this inconsistency in variable definitions between different sets of authors unfortunately complicates cross-country comparisons of estimated credential effects.

Conclusion

This chapter has provided a broad overview of the range of strategies that economists have adopted to test the educational sorting hypothesis. In chapters 5 to 8, two tests – both originally deriving from insights in the paper by Layard and Psacharopoulos (1974) – will be applied to a new dataset on young Australians. The first of these tests, reported in chapters 5 and 6, is based on the estimation of 'sheepskin' or credential effects, as discussed in the previous section of this chapter. The second test, reported in chapters 7 and 8, seeks evidence of employer learning with labour market experience, as described in the following chapter.

Figure 2.1. Education-ability profiles in screened and unscreened sectors

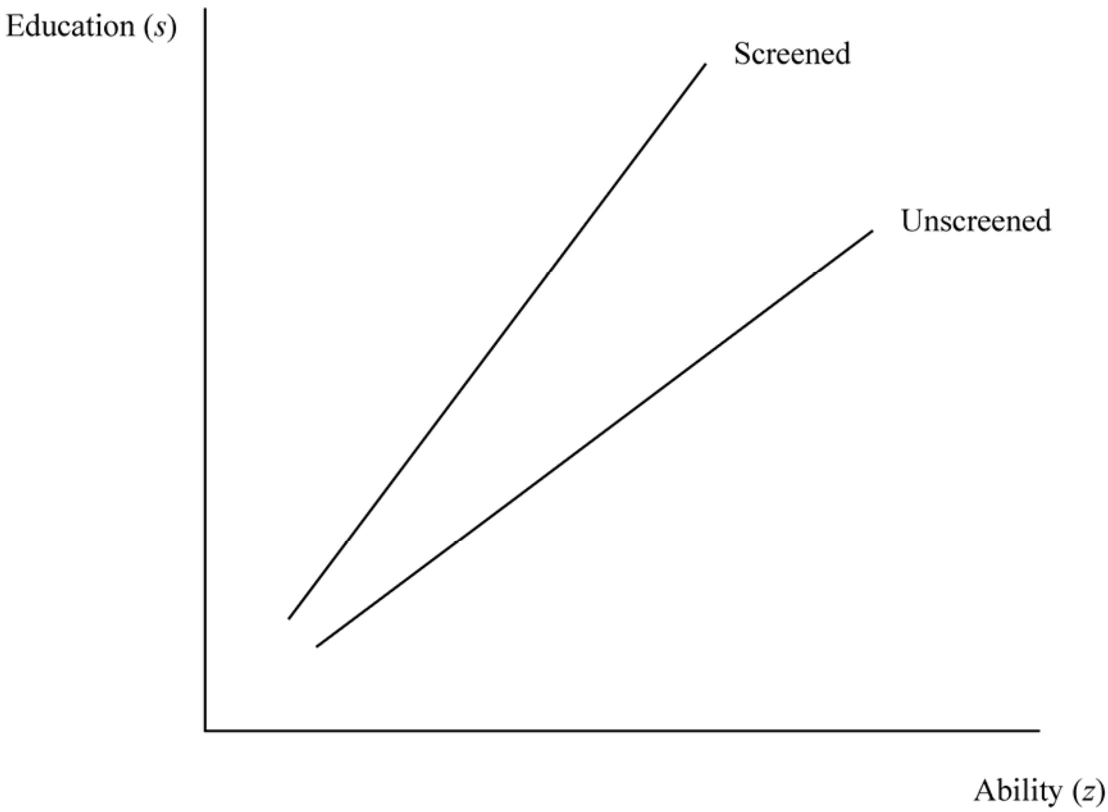


Figure 2.2. Spline function model of returns to education

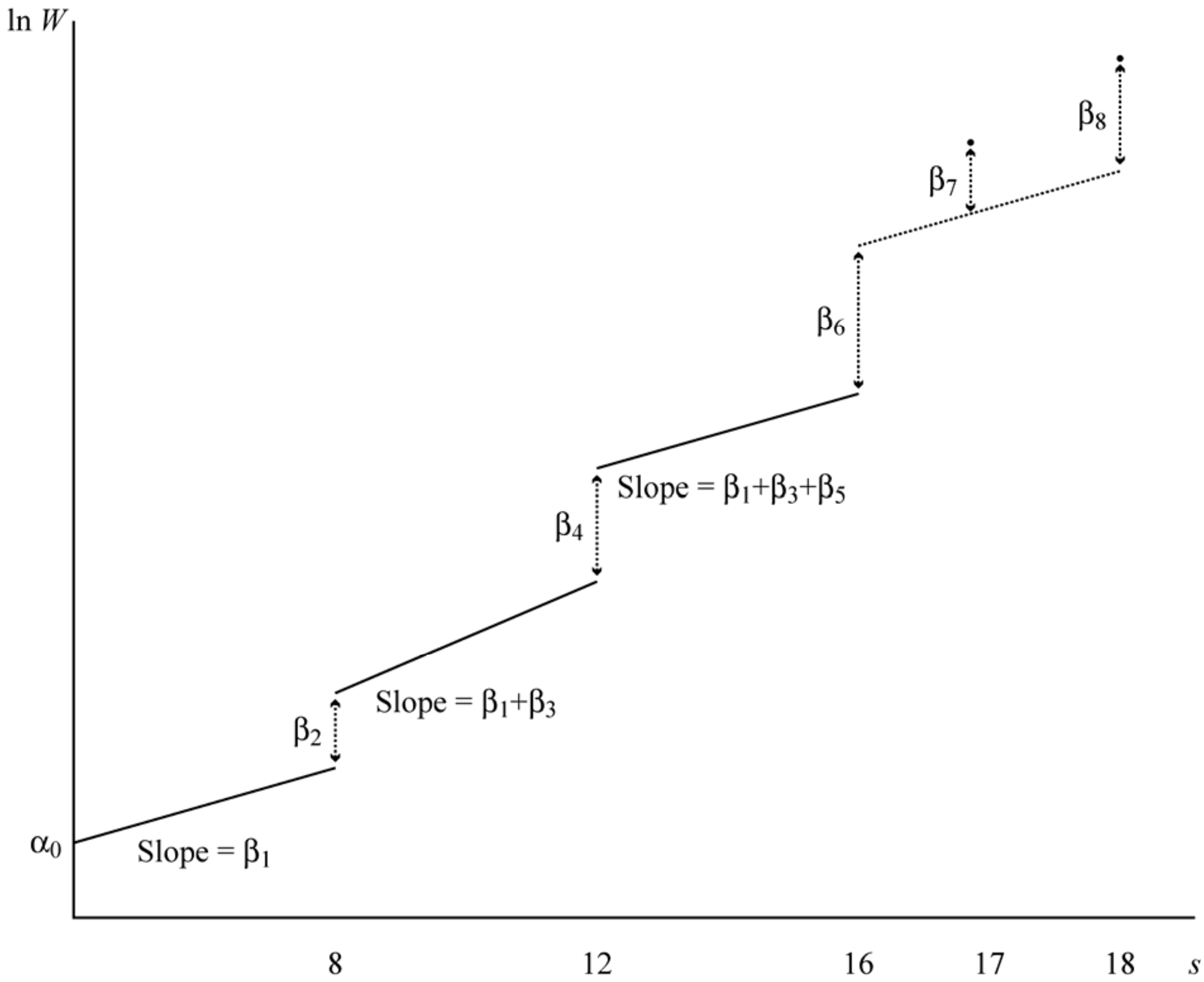


Table 2.1. Reported estimates of credential effects

Authors	Country / Dataset	Model / Credentials?	Subgroup	Implied increase in earnings: High school / 12 years	Implied increase in earnings: College / 16 years
Hungerford / Solon (1987)	USA, CPS 1978	Spline, No	White males	3.6%	9.4%***
Belman / Heywood (1991)	USA, CPS 1978	Spline, No	White males	4.7%*	10.5%***
			Minority males	6.8%	23.2%***
			White females	7.8%***	-2.6%
			Minority females	7.8%	28.3%***
Heywood (1994)	USA, CPS 1989	Spline, No	Private non-union	13.2%***	9.8%***
			Private union	5.1%**	0.4%
			Public non-union	12.8%**	2.3%
			Public union	-1.9%	-13.2%***
Belman / Heywood (1997)	USA, CPS 1991	Spline, No	Age 24-30	7.8%***	12.0%***
			Age 31-40	6.5%***	5.7%***
			Age 41-50	4.5%	2.7%
			Age 51-60	1.9%	1.8%
			Age 61-65	2.4%	9.2%
Jaeger / Page (1996)	USA, CPS 1992	Step, Yes	White males	13.3%***	27.8%***[a]
			Black males	12.6%	35.7%**[a]
			White females	6.4%	25.0%***[a]
			Black females	11.1%	48.3%***[a]
Park (1999)	USA, CPS 1990	Fully-interacted, Yes	All individuals	9.3%**	23.5%***[b]
Denny / Harmon (2001)	Cross-country, IALS 1997	Log-linear, Yes	USA	31.0%*	79.7%***[c]
			Canada	-12.0%	3.3%[c]
			Sweden	15.1%	36.2%**[c]
			Britain	21.2%	82.9%***[c]
			Ireland	11.5%	46.7%*[c]
Gibson (2000)	New Zealand, ETS 1996	Step, Yes	Males	24.0%***	43.0%****[d]
			Females	26.1%***	64.4%***[d]
			European	24.7%***	56.7%***[d]
			Maori / Islander	15.7%***	34.4%**[d]
Ferrer / Riddell (2002)	Canada, Census 1996	Step, Yes	Males	4.1%***	22.9%***[a]
			Females	5.5%***	23.5%***[a]
Bauer / Dross / Haisken-DeNew (2005)	Japan, JPSC 1997	Step, Yes	Small firm	10.0%	21.2%*[a]
			Large firm	26.5%*	2.7%[a]

Notes: ***, ** and * denote significance at the 1%, 5% and 10% levels respectively.
[a] Relative to high school graduation. [b] Conditional on 16 years of education, and relative to 'some college'.
[c] Reference group not specified. [d] Relative to no educational qualifications.

CHAPTER 3

TESTING FOR EMPLOYER LEARNING: A SYNTHESIS AND REVIEW

As discussed in chapter 2, one of the earliest debates in the empirical literature on the educational sorting hypothesis concerned the question of whether the return to education may be expected to decline with experience. This debate can be traced back to Layard and Psacharopoulos (1974), who argued that the importance of education would diminish as employers learned more accurate information about workers' productivities. This view was also expressed by Psacharopoulos (1979); however other authors such as Riley (1979) and Farber and Gibbons (1996) have argued that the return to education should remain constant with experience, even in the presence of this type of learning.

Farber and Gibbons went on to propose a test of employer learning which uses a measure of ability that is observed by the researcher, but which has been purged of its correlation with information initially observed by employers. They argued that employer learning implies that the return to this (residualised) ability variable should increase with experience, but that the return to education remains unchanged. Finally, Altonji and Pierret (1998, 2001) considered the case in which the ability variable is *not* first purged of its correlation with information observed by employers. In their framework, employer learning implies that the return to measured ability should increase with experience, while the return to education declines.

The purpose of this chapter is to review both the theoretical and empirical literature on tests of employer learning, with a focus on how the returns to both education and measured ability vary with experience. A simplified two-period version of the model of Altonji and Pierret is developed, to show how some of the seemingly conflicting claims in the literature may be reconciled. Some important extensions to this framework are then considered. Finally, the empirical results reported in the existing literature will be summarised.

Framework

The framework for this chapter is a simplified version of the model in Altonji and Pierret (2001). This model is concerned with how the coefficients on education (*s*, which is observed by employers) and measured ability (*z*, which is not initially observed by employers) evolve with experience as employers become better informed. Let y_x, the log of labour market productivity of a worker with experience *x*, be given by:

$$y_x = rs + \alpha_1 q + \lambda z + \eta + H(x)$$

Both *s* and *z* are observed by the researcher. Clearly, *s* is permitted to have a direct effect on productivity in addition to any purely informational value it may have for employers. The variable *z*, which is observed by the researcher but not initially by the employer, may take the form of a proxy for cognitive ability – such as a test score or parental education. The variable *q* denotes information that is observed by employers but not by the researcher (for example high school and university grades, or information obtained from references and at interview). The variable η denotes information that is not observed either by employers or the researcher, and $H(x)$ is the experience profile of productivity. For the purpose of exposition, all variables are taken to be scalars, expressed in deviations from means, and with *s*, *q* and *z* taken to be orthogonal to $H(x)$. Also, $H(0)$ is normalised to be zero at the time of labour market entry.

For the purpose of highlighting the key empirical implications of this model, it is sufficient to consider two limiting time periods. At the start of a worker's career ($x = 0$), an employer only observes *s* and *q*. Because employers do not initially observe *z* and η, they form expectations on these variables conditional upon the information that is available to them. Assume that these expectations are linear:

$$\begin{aligned} z &= E(z \mid s,q) + v = \gamma_1 q + \gamma_2 s + v \\ \eta &= E(\eta \mid s,q) + e = \alpha_2 s + e \end{aligned}$$

(where η and α_1 are defined such that the mean of η does not depend upon q). These conditional expectations may reflect a causal relationship between s and z and η, for instance because more able individuals complete more education, or because individuals who possess other unobservable traits that contribute to productivity (such as perseverance) also complete more education. By construction, v and e have mean zero and are uncorrelated with s and q.

The log of initial productivity can thus be expressed in terms of the information initially observed by an employer (s and q) as follows:

$$\begin{aligned} y_0 &= (r + \lambda\gamma_2 + \alpha_2)s + (\alpha_1 + \lambda\gamma_1)q + (\lambda v + e) \\ &= E(y_0 \mid s,q) + (\lambda v + e) \end{aligned}$$

Thus the weight given to education in an employer's initial assessment of productivity takes into account both its direct effect, r, and the fact that s is correlated with variables z and η that are not initially observed by the employer. Similarly, in forming expectations conditional upon q, an employer considers both its direct effect and the fact that it is correlated with z. After taking all this into account, ($\lambda v + e$) is the remaining error in an employer's initial assessment of log productivity at the time a worker enters the labour market. If all potential employers observe the same information, then competition ensures that the worker is paid their expected productivity. Thus log wages (w) at the time of labour market entry are:

$$w_0 = (r + \lambda\gamma_2 + \alpha_2)s + (\alpha_1 + \lambda\gamma_1)q$$

Next, suppose that after some limiting amount of experience (denoted $x = 1$), the value of ($\lambda v + e$) – and thus a worker's true productivity – is eventually fully revealed to employers, and that this is priced into wages, such that:

$$\begin{aligned} w_1 &= \left(r + \lambda\gamma_2 + \alpha_2\right)s + \left(\alpha_1 + \lambda\gamma_1\right)q + H\left(1\right) + \left(\lambda v + e\right) \\ &= rs + \alpha_1 q + \lambda z + \eta + H\left(1\right) \end{aligned}$$

Thus wages evolve with experience both because productivity itself grows, $H(x)$, and because employers receive new information that enables them to update the error in their initial assessment of productivity, ($\lambda v + e$).

This simple two-period representation can now be used to obtain the key results of the employer learning model. The first proposition concerns how the coefficient on education varies with experience when measured ability is *not* included in the model.

Proposition 3.1 (Farber and Gibbons 1996)

Suppose that we estimate regressions of w_x on s, separately by experience level for $x = \{0, 1\}$:

$$w_x = E\left(w_x \middle| s, x\right) + \xi_{1x} = a''_x + b''_{sx}s + \xi_{1x}$$

Then the coefficients b''_{sx} on s should be independent of experience, x.

In these models, the separate intercept terms a''_x by experience level pick up the effect of the experience profile $H(x)$. Comparing the equations estimated by the researcher to the underlying log wage process, we see that q is an omitted variable. Further, at $x = 1$, ($\lambda v + e$) is also an omitted variable. To see why proposition 3.1 holds, define the auxiliary regressions:

$$\begin{aligned} q &= \kappa_1 s + \zeta_1 \\ \lambda v + e &= \kappa_2 s + \zeta_2 \end{aligned}$$

Then by the omitted variable bias formula for least squares regression, we have:

$$\begin{aligned} b''_{s0} &= \left(r + \lambda\gamma_2 + \alpha_2\right) + \left(\alpha_1 + \lambda\gamma_1\right)\kappa_1 \\ b''_{s1} &= b''_{s0} + \kappa_2 \end{aligned}$$

The second term in the first expression picks up the effect of omitting q, which is taken into account by employers in forecasting productivity, but which is unobserved by the researcher, and is correlated with s through κ_1. The second term in the second expression picks up the effect of also omitting $(\lambda v + e)$ at $x = 1$. However, recall that $(\lambda v + e)$ is the component of productivity that employers *cannot* predict from their initial information, and that an employer's initial information set includes s. Thus by construction $(\lambda v + e)$ is uncorrelated with s, such that $\kappa_2 = 0$ and $b''_{s0} = b''_{s1}$. Intuitively, all productivity-relevant information captured by s should be priced into wages at $x = 0$, so no part of the change in wages from $x = 0$ to $x = 1$ should be able to be explained by information available at $x = 0$. It follows that the coefficient on s is independent of experience.

Proposition 3.1 formalises the insight first expressed by Riley (1979). Note that Farber and Gibbons (1996) derived their original version of this result in terms of wage *levels*. The next proposition is the test of employer learning proposed by Farber and Gibbons.

Proposition 3.2 (Farber and Gibbons 1996)

Let z' be the component of z that is uncorrelated with an employer's initial information set $\{s, q\}$. Now suppose that we estimate regressions of w_x on s and z', separately for $x = \{0, 1\}$:

$$w_x = E\left(w_x \,|\, s, z', x\right) + \xi_{2x} = a'_x + b'_{sx}s + b'_{zx}z' + \xi_{2x}$$

Then the coefficients b'_{sx} on s should be independent of experience, while the coefficients b'_{zx} on z' should increase with experience.

The first part of this result follows from proposition 3.1 and the fact that adding a variable to a regression has no effect on the coefficient of an uncorrelated variable. To show the second part, define the auxiliary regressions:

$$\begin{aligned} q &= \kappa'_{1s} s + \kappa'_{1z} z' + \zeta_3 \\ \lambda v + e &= \kappa'_{2s} s + \kappa'_{2z} z' + \zeta_4 \end{aligned}$$

Since z' is uncorrelated with q and also with s, it follows that $\kappa'_{1z} = 0$. Similarly, since s is uncorrelated with $(\lambda v + e)$ and also with z', it follows that $\kappa'_{2s} = 0$. We thus have:

$$\begin{aligned} b'_{z0} &= \left(\alpha_1 + \lambda\gamma_1\right)\kappa'_{1z} = 0 \\ b'_{z1} &= b'_{z0} + \kappa'_{2z} = \frac{\operatorname{cov}\left(z', \lambda v + e\right)}{\operatorname{var}\left(z'\right)} \end{aligned}$$

Since z' is purged of any correlation with information initially observed by employers, its effect at $x = 0$ is predicted to be zero. Also, as long as z' is positively correlated with the initially unobservable component of productivity, $(\lambda v + e)$, its coefficient should increase with experience.

The final proposition extends proposition 3.2 to the case where z is *not* first purged of its correlation with s, and the effects of both variables are permitted to vary with experience. This is the test of employer learning proposed by Altonji and Pierret (2001).

Proposition 3.3 (Altonji and Pierret 2001)

Now consider the least squares regressions of w_x on s and z, separately for x = {0, 1}:

$$w_x = E\left(w_x \left| s, z, x\right.\right) + \xi_{3x} = a_x + b_{sx} s + b_{zx} z + \xi_{3x}$$

Then the coefficients b_{sx} on s should decrease with experience, while the coefficients b_{zx} on z should increase with experience.

To see why this is the case, define the auxiliary regressions:

$$
\begin{aligned}
q &= \kappa_{1s}s + \kappa_{1z}z + \zeta_5 \\
\lambda v + e &= \kappa_{2s}s + \kappa_{2z}z + \zeta_6
\end{aligned}
$$

We now have:

$$
\begin{aligned}
b_{s0} &= \left(r + \lambda\gamma_2 + \alpha_2\right) + \left(\alpha_1 + \lambda\gamma_1\right)\kappa_{1s} \\
b_{s1} &= b_{s0} + \kappa_{2s} \\
b_{z0} &= \left(\alpha_1 + \lambda\gamma_1\right)\kappa_{1z} \\
b_{z1} &= b_{z0} + \kappa_{2z}
\end{aligned}
$$

Thus even with the inclusion in the model of z, the coefficient on s clearly does not estimate its structural effect upon productivity. Also, even though z is initially not observed by employers, its coefficient at zero experience need *not* in general be zero because z also picks up part of the effect of q – which is not observed by the researcher but which is taken into account by employers in predicting productivity. Finally, the reason why the coefficients on s and z evolve with experience is simply that employers eventually learn the value of ($\lambda v + e$) – but the researcher does not – and this information is priced into wages.

To see exactly how the wage coefficients on s and z evolve with experience, consider in detail the artificial regression of ($\lambda v + e$) on s and z. The N×2 data matrix is $\mathbf{X} = [\mathbf{s}\ \mathbf{z}]$, and since the variables are expressed in deviations from means we have:

$$
\mathbf{X}'\mathbf{X} = \begin{bmatrix} \mathbf{s}'\mathbf{s} & \mathbf{s}'\mathbf{z} \\ \mathbf{z}'\mathbf{s} & \mathbf{z}'\mathbf{z} \end{bmatrix} = N \cdot \begin{bmatrix} \operatorname{var}(s) & \operatorname{cov}(s,z) \\ \operatorname{cov}(s,z) & \operatorname{var}(z) \end{bmatrix}
$$

Also:

$$
\mathbf{X}'\mathbf{y} = \begin{bmatrix} \mathbf{s}'\left(\lambda\mathbf{v} + \mathbf{e}\right) \\ \mathbf{z}'\left(\lambda\mathbf{v} + \mathbf{e}\right) \end{bmatrix} = N \cdot \begin{bmatrix} 0 \\ \operatorname{cov}\left(z, \lambda v + e\right) \end{bmatrix}
$$

since ($\lambda v + e$) is uncorrelated with s. The normal equations for this regression are thus:

$$\begin{bmatrix} \text{var}(s) & \text{cov}(s,z) \\ \text{cov}(s,z) & \text{var}(z) \end{bmatrix} \begin{bmatrix} \kappa_{2s} \\ \kappa_{2z} \end{bmatrix} = \begin{bmatrix} 0 \\ \text{cov}(z, \lambda v + e) \end{bmatrix}$$

These solve for:

$$\begin{bmatrix} \kappa_{2s} \\ \kappa_{2z} \end{bmatrix} = \frac{1}{\left|\mathbf{var}(s,z)\right|} \begin{bmatrix} \text{var}(z) & -\text{cov}(s,z) \\ -\text{cov}(s,z) & \text{var}(s) \end{bmatrix} \begin{bmatrix} 0 \\ \text{cov}(z, \lambda v + e) \end{bmatrix}$$

Thus provided that $\text{cov}(z, \lambda v + e) > 0$ (measured ability is positively correlated with the initially unobserved component of productivity) we have $\kappa_{2z} > 0$, and so it follows that $b_{z1} > b_{z0}$. If in addition $\text{cov}(s, z) > 0$ (education is positively correlated with measured ability) then we have $\kappa_{2s} < 0$, and so $b_{s1} < b_{s0}$.

Altonji and Pierret label their result ‘employer learning with statistical discrimination’, and summarise it by stating that “as employers learn the productivity of workers, s will get less of the credit for an association with productivity that arises because s is correlated with z, provided that z is included in the wage equation with a time-dependent coefficient and can claim the credit” (Altonji and Pierret 2001, p. 321).

Corollary 3.1 (Altonji and Pierret 2001)

If employers have complete information about the productivity of workers, and / or they learn nothing new with experience, then b_{sx} and b_{zx} are constants.

Corollary 3.2 (Altonji and Pierret 2001)

$$\frac{\partial b_{sx}}{\partial x} = -\varphi \frac{\partial b_{zx}}{\partial x}$$

where φ is the coefficient of the regression of z on s.

This result follows directly from the above solutions for κ_{2s} and κ_{2z}, which imply that:

$$\frac{\kappa_{2s}}{\kappa_{2z}} = -\frac{\operatorname{cov}(s,z)}{\operatorname{var}(s)}$$
$$\kappa_{2s} = -\varphi\kappa_{2z}$$

This holds because s is initially observed by employers. Thus any change in the coefficient on s with experience must arise as a result of employer learning in relation to z, which is passed through to the coefficient on s via the relationship between z and s, as captured by φ. This is the result labelled as "Proposition 2" in Altonji and Pierret (2001).

Empirical implementation

In practice, the researcher observes data over a number of time periods, but this need not be long enough for true productivity to be fully observed by employers. Instead of estimating separate wage equations for each level of experience, the hypotheses of Altonji and Pierret can be tested by estimating a single pooled equation of the following form:

$$w_{it} = F(x_{it}) + \beta_s s_i + \beta_{sx} s_i x_{it} + \beta_z z_i + \beta_{zx} z_i x_{it} + \upsilon_{it} \qquad 3.1$$

This pooled model imposes the additional restrictions that $b_{sx} = \beta_s + \beta_{sx}x$ and $b_{zx} = \beta_z + \beta_{zx}x$. The predictions of the model can now be readily tested by examining β_{sx} and β_{zx}. Proposition 3.3 states that $\beta_{sx} < 0$ and $\beta_{zx} > 0$. Conversely, if employers have complete information, or there is no learning, then $\beta_{sx} = \beta_{zx} = 0$. Finally, corollary 3.2 predicts that $\beta_{sx} = -\varphi\beta_{zx}$.

Similarly, the propositions in Farber and Gibbons (1996) are tested by estimating the model:

$$w_{it} = F'(x_{it}) + \beta'_s s_i + \beta'_{sx} s_i x_{it} + \beta'_z z'_i + \beta'_{zx} z'_i x_{it} + \upsilon'_{it}$$

Then proposition 3.1 states that $\beta'_{sx} = 0$, while proposition 3.2 states that $\beta'_{zx} > 0$. However, as noted above, Farber and Gibbons specify and test their model in terms of the *level* of wages.

The effect of on-the-job training

Since wages in this model are determined by expected productivity, they will only grow with experience as a result of increasing productivity. This growth in productivity with experience is captured by the term $H(x)$, and may be due to factors such as on-the-job training and learning by doing. However, this productivity growth is assumed to be 'neutral' in the sense that it is uncorrelated with s and z. The effects of s and z on log productivity take the form of a parallel shift, and not a change in the slope of its profile with experience. As a result, the coefficients on s and z in the wage regressions vary solely on account of employer learning.

More generally, if employers infer productivity from education, they may also invest more heavily in training better-educated workers. More educated workers may also simply benefit more from a given amount of training provision. To see the implications in the simple two-period framework, suppose that training occurs between $x = 0$ and $x = 1$. Thus training has no effect on initial productivity, and wages at $x = 0$ are determined as before. However, suppose training increases productivity at $x = 1$ in such a way that $\text{cov}(s, H(1)) > 0$. Since productivity is fully revealed to all employers at $x = 1$, it follows that this is fully priced into w_1.

Then in proposition 3.1 we now have:

$$b''_{s1} - b''_{s0} = \frac{\text{cov}(s, H(1))}{\text{var}(s)}$$

So if the effect of training is positively correlated with education then the coefficient on education in the wage regressions in propositions 3.1 and 3.2 will no longer be constant, but should now *increase* with experience instead.

Further, if $H(1)$ is also positively correlated with z', the component of ability that employers cannot predict from their initial information – for example because more able workers also benefit more from learning on the job – then in proposition 3.2 we would have:

$$b'_{z1} - b'_{z0} = \frac{\operatorname{cov}(z', \lambda v + e)}{\operatorname{var}(z')} + \frac{\operatorname{cov}(z', H(1))}{\operatorname{var}(z')}$$

where the first term captures the effect of employer learning, and the second term captures the complementarity of training with ability.

Thus in proposition 3.3, when there is both employer learning taking place and on-the-job training is positively correlated with s and z, the coefficient on z will increase more rapidly with experience – while the coefficient on s declines more slowly – than when the effects of training are 'neutral'. This confounds interpretation of the coefficients by overstating the effect of learning upon b_{zx}, favouring *acceptance* of employer learning with regard to ability, but also by attenuating the expected decline in b_{sx}, thus favouring *rejection* of statistical discrimination on the basis of education. For this reason it is important to control for on-the-job training when estimating the model of employer learning.

Finally, if employer learning is *not* important, and wages growth is driven *solely* by training effects, then provided training is complementary to both s and z we would expect the coefficients on *both* variables to *increase* with experience. Nonetheless, Lange (2005) shows that it is still the case that the increase in the coefficient on s with experience will be *smaller* when z is included in the wage regression than when it is omitted. This is analogous to the earlier results in which this coefficient was predicted to be *constant* in proposition 3.1 when z was omitted, but *decreasing* in proposition 3.3 when z is included. It holds because the difference in the return to education when ability is included, compared to when it is not, is an omitted variable bias. Since the return to ability is positive, and education is positively correlated with ability, this bias will be positive. Further, since the return to ability increases with experience, the omitted variable bias is increasing.

Asymmetric learning and tenure

Thus far it has been assumed that all potential employers observe the same information regarding productivity. Equivalently, outside firms competing to employ a given worker learn at exactly the same rate as the incumbent employer, since all of the information that is revealed with experience is fully public. This makes it possible to maintain the assumption that wages are determined competitively (equated to expected productivity), and to abstract from worker mobility between jobs and firms.

Bauer and Haisken-DeNew (2001) were first to suggest that employer learning regarding workers' productivity may be asymmetric or private, with incumbent employers having an informational advantage over outside firms. To the extent that learning occurs privately, it will affect wages differently in incumbent and outside firms – such that the returns to education and ability will be affected differentially by experience within the current firm (i.e. tenure) as opposed to experience with other employers.

Conceptually, this raises two new issues. Firstly, even with competitive bidding among outside employers for a given worker's services, when the incumbent firm has superior information it need not pay a wage equal to expected productivity. This is because the competitive pressure from outside firms is diluted by their lack of information. For example, if an incumbent firm has private information that a worker's productivity is higher than initially expected, it need only raise its wage offer by enough to ensure that the worker will not accept an outside wage offer that *does not* incorporate this private information. Secondly, it becomes necessary to account for the heterogeneity that gives rise to variations in tenure among workers who have the same level of total labour market experience. In other words, it now becomes necessary to model the process of job mobility.

These issues have been analysed in recent papers by Galindo-Rueda (2003) and Schönberg (2005). Their models assume that at the time a worker enters the labour market ($x = 0$), all potential employers have the same (incomplete) information regarding productivity. In this period, wages are determined on the basis of readily observable information such as education. In the second period ($x = 1$), a worker's incumbent employer is assumed to have complete information, however outside firms may have less information than the incumbent.

In the first period, all employers make their wage offers simultaneously, and wages are determined competitively as before. In the second period, the worker receives an offer from an outside firm. This is observed by the incumbent firm, who makes a counteroffer. To generate endogenous mobility, it is assumed that the worker also draws a taste parameter, which represents the non-pecuniary value of moving as opposed to staying in their current job. The worker then chooses the job that offers the greatest utility.

Broadly speaking, there are three possible cases to be considered:

- If an outside firm has the same information as the incumbent, then learning is *purely public*. In this case, s and z affect wages in incumbent and outside firms in the same manner, and learning will be observed in the profile of the returns to education and ability with *experience*. This is the case analysed in the original model set out above.

- If outside firms receive no new information, then learning is *purely private*. Outside firms continue to base wage offers on publicly-observable information such as education, while incumbent firms incorporate their private information in their wage offers. In this case, there is no change in how s and z affect wages in outside firms (their experience interactions are zero), and the effects of learning will be evident solely in the profile of the returns to education and ability with *tenure*.

- If outside firms receive some new information, but do not fully observe productivity, then learning is *partially private*. In this case, z has a stronger effect on wages in the incumbent firm. Learning is evident in both the experience and tenure interactions, with the tenure effects reflecting the superior information of the incumbent.

The major substantive difference between the two versions of this model is that Galindo-Rueda (2003) assumes that outside wage offers are generated competitively, whereas Schönberg (2005) assumes a single outside employer. Letting tenure be denoted by τ, the implications of these models may be tested by estimating an equation of the form:

$$w_{it} = F\left(x_{it}, \tau_{it}\right) + \beta_s s_i + \beta_{sx} s_i x_{it} + \beta_{s\tau} s_i \tau_{it} + \beta_z z_i + \beta_{zx} z_i x_{it} + \beta_{z\tau} z_i \tau_{it} + \upsilon_{it} \qquad 3.2$$

If there were no learning at all by either incumbent or outside firms, we would have $\beta_{zx} = \beta_{z\tau} = 0$. Under purely private learning, outside firms receive no new information and so $\beta_{zx} = 0$; however there is learning by incumbents such that $\beta_{z\tau} > 0$. Under partially private learning we have both $\beta_{zx} > 0$ and $\beta_{z\tau} > 0$, where the interaction of z with tenure reflects the extent of additional learning by the incumbent firm. Thus under purely public learning we would have $\beta_{zx} > 0$ and $\beta_{z\tau} = 0$, indicating no additional learning private to the incumbent. Provided the ability interactions β_{zx} and $\beta_{z\tau}$ are driven purely by learning effects – and given that s is part of an employer's initial information set – the returns to education will only vary with experience or tenure because s is correlated with z, and so a generalisation of corollary 3.2 will continue to hold.

The remaining sections of this chapter review the empirical findings from the existing literature on tests of the employer learning model.

Evidence from tests of the basic learning model

Tests of propositions 3.1 and 3.2

Farber and Gibbons (1996) test propositions 3.1 and 3.2 using US data on young men and women from the National Longitudinal Survey of Youth (NLSY) through to the 1991 interview year. As z-variables, they consider the Armed Forces Qualifying Test (AFQT) score, and a dummy variable for the presence of a library card in the household at age 14 (a measure of the household intellectual environment). To obtain z', they take the residuals from a regression of the respective z-variables on all time-invariant characteristics observed by the researcher, plus the initial wage rate. The initial wage is included to purge z of the effect of characteristics observed by the employer but not by the researcher (q).

Farber and Gibbons specify their model, and undertake estimation, using the level of wages rather than its log. Consistent with proposition 3.1, they find no evidence that the return to education varies with experience. Further, consistent with proposition 3.2, they find that the interactions of both z' variables with experience are significantly positive. However, contrary to proposition 3.2, they also find a significant positive 'main' effect of the AFQT residual at zero experience. Their results continue to hold when they estimate the model separately for males and females.

Tests of proposition 3.3

To test their version of the employer learning hypothesis, Altonji and Pierret (1998) use NLSY data through to the 1992 release. They focus only on men, and use a standardised version of the AFQT score as their z-variable. They present results for five specifications of the wage equation, corresponding to estimates of equation 3.1 with different sets of restrictions imposed upon the parameters.

Specification 1 is a standard Mincer regression containing years of education alone, and does not include the AFQT score or any experience interactions. In effect, this specification restricts b_{sx} to be a constant ($\beta_{sx} = 0$), and restricts b_{zx} to be zero ($\beta_z = \beta_{zx} = 0$).

Specification 2 includes years of education and the test score, but no experience interactions. This specification restricts both b_{sx} and b_{zx} to be constants. The key finding here is that the AFQT score has a significant positive association with log wages, even after controlling for years of education.

Specification 3 includes s as well as s interacted with experience, but no terms involving z. This restricts $b_{sx} = \beta_s + \beta_{sx}x$, and restricts b_{zx} to be zero. This is the model considered in proposition 3.1 above. In their estimates, Altonji and Pierret report a slight negative interaction of years of education with experience.

Specification 4 includes s, $s*x$ and z, but does not permit z to interact with experience. This restricts $b_{sx} = \beta_s + \beta_{sx}x$, and restricts b_{zx} to be a constant. The key findings from specification 2 (in relation to β_z) and from specification 3 (in relation to β_{sx}) carry over to this model.

Specification 5 includes s, $s*x$, z and $z*x$. This is the model considered in proposition 3.3 above, with the restrictions that $b_{sx} = \beta_s + \beta_{sx}x$ and $b_{zx} = \beta_z + \beta_{zx}x$. The authors emphasise two key results from this specification. Firstly, the effect of AFQT increases significantly with experience (β_{zx} is positive and significant). Secondly, compared to specification 4, the coefficient on $s*x$ declines sharply when $z*x$ is added in specification 5.

In a second paper, Altonji and Pierret (2001) use the same data source and report estimates for three alternative *z*-variables: the standardised AFQT score, the log of a sibling's wage, and father's education. In this paper, they report results for specifications 4 and 5 as defined above. Their results for the AFQT score are similar to those reported in Altonji and Pierret (1998), and the same two key results noted above at specification 5 are emphasised. In addition, they report a formal Wald test of corollary 3.2, which fails to reject the coefficient restriction implied by their model. Similar results are found when a sibling's wage is used as the *z*-variable. However, when father's education is used, none of the key coefficients are statistically significant, although the sign pattern of the interaction terms is consistent with the predictions of the model.

Updated replications of the AFQT analysis in Altonji and Pierret (1998, 2001), making use of later releases the NLSY, have been reported in recent papers by Lange (2005) and Schönberg (2005). Lange uses data through to 1998 to report estimates of specifications 4 and 5 – both for a sample of men, and for the full sample including men and women. In both samples, the results emphasised by Altonji and Pierret continue to hold. Schönberg reports an estimate of specification 5 for men only, using NLSY data through to 2001. She confirms both the significant positive coefficient on $z*x$ and the negative one on $s*x$. Schönberg also reports a variation on specification 5, in which the effects of s and z are allowed to vary with experience squared. She finds that the effect of z increases at a diminishing rate with experience, indicating that employers learn most rapidly when workers are young.

Bauer and Haisken-DeNew (2001) test the employer learning hypothesis using data on West German prime-age males from the German Socio-Economic Panel (GSOEP), with the larger of the mother's and father's education used as the *z*-variable. As well as reporting estimates of specifications 4 and 5 as defined above, their paper makes three distinct contributions to the literature. Firstly, they are the first to distinguish between tenure in the current firm and experience with other employers. Secondly, they report separate estimates for blue and white-collar workers. Finally, they use quantile regression to investigate how the pattern of employer learning varies across the wage distribution. Discussion of the first two of these points will be deferred to the appropriate sections below.

In their estimate of specification 4, Bauer and Haisken-DeNew report positive and significant 'main' effects for *s* and *z*. In specification 5, the interaction of *z* with experience is positive and significant at the 10% level. However, in both of these specifications, the interaction of education with experience is strongly positive and significant, contrary to the prediction of the theory. This occurs in spite of the fact that the model includes a dummy variable to control for the presence of on-the-job training. When specification 5 is estimated by quantile regression (at the 25^{th}, 50^{th} and 75^{th} percentiles) β_{zx} is positive and significant at a level of at least 10% at every quartile. However, β_{sx} also remains positive and highly significant at every quartile, and for this reason the results do not support the employer learning hypothesis.

Galindo-Rueda (2003) reports estimates of specifications 2 and 5 for data on young men from two British birth cohort studies – the National Child Development Study (NCDS) and British Cohort Study (BCS) – taking the first principal component of a set of test scores as the *z*-variable. The estimate of specification 2 confirms that this *z*-variable has a highly-significant effect after controlling for years of education. In specification 5, the coefficient on $s*x$ is positive but not significant, while that on $z*x$ is both positive and significant. These results are thus consistent with employer learning; moreover they continue to hold after a control for on-the-job training is added to the model. Galindo-Rueda also reports separate estimates for blue and white-collar workers, and estimates that distinguish between experience and tenure. These will be discussed at the appropriate points below.

Pan (2005) reports estimates of specifications 1 through 5 for data on Canadian men from the Survey of Labour and Income Dynamics (SLID), using mother's education as the z-variable. In specifications 2 and 4, she confirms that maternal education has a positive and significant 'main' effect. In specifications 3 and 4, the coefficient on $s*x$ is not significant. Finally, in specification 5 the effect of $z*x$ is positive and significant, while that of $s*x$ is now negative and marginally significant. These results are all consistent with the employer learning model. Finally, a Wald test fails to reject the coefficient restriction implied by corollary 3.2.

Table 3.1 summarises estimates of specification 5 (or its equivalent) for the papers discussed to this point. This is the specification in which both s and z (or z') are interacted with experience, to perform the tests of employer learning described in propositions 3.2 and 3.3.

Evidence from tests by occupational status, hiring channels, and in specialised samples

Bauer and Haisken-DeNew (2001) suggest two reasons why employer learning may differ between blue and white-collar workers. Firstly, if the nature of white-collar work is such that the flow of information regarding productivity is noisier, then employer learning may play a less important role for white-collar workers. Secondly, if employers invest more heavily in screening applicants for white-collar positions, then they will start out with more information about such workers at the time of hiring, leaving less room for learning effects.

To investigate these hypotheses, the authors estimate specification 5 separately for blue and white-collar workers in their sample of West German males. It turns out that none of the s and z interactions with experience are significant for either group. When these models are estimated by quantile regression, there is evidence of employer learning for some blue-collar workers but not for white-collar workers. The coefficient on $z*x$ is positive and significant for blue-collar workers at the 50^{th} and 75^{th} percentiles; however the coefficient on $s*x$ is also positive and significant at the 75th percentile. These results are thus only fully consistent with employer learning for blue-collar workers at the median of the wage distribution.

Separate estimates of specification 5 for blue and white-collar workers are also reported by Galindo-Rueda (2003) for his data on young British males. In these results there is only evidence to support employer learning among white-collar workers, a conclusion that runs counter to both the hypotheses and results of Bauer and Haisken-DeNew (2001). For the white-collar group, β_{sx} is negative and significant, while β_{zx} is positive and significant. For the blue-collar group, β_{sx} is positive and significant, while β_{zx} is not statistically significant.

Strobl (2004) suggests that as an alternative to screening on the basis of education, employers may respond to asymmetric information by recruiting through direct contacts – such as relatives, acquaintances and existing employees – thereby gaining superior information on workers' abilities. Using data on Ghanaian manufacturing workers – and taking the larger of the mother's and father's education as the *z*-variable – Strobl estimates specification 5 separately for workers who were hired through direct contacts and those who were not. He classifies a worker as having been hired through a direct contact if they found out about their job because they were a friend or relative of the employer or another employee.

It turns out that the null hypothesis that both β_{sx} and β_{zx} are zero cannot be rejected for either group. Strobl thus further subdivides each of these groups into those who received on-the-job training and those who did not, estimating four separate regressions. The only group for whom the employer learning model is supported are those workers who did not have a direct contact in the firm and who did not receive training. For this group, β_{sx} is negative and significant, and β_{zx} is positive and significant. For all other groups – including the two direct contact groups about whom employers are posited to have superior information – it is not possible to reject the null hypothesis that both interaction terms are zero.

Heisz and Oreopoulos (2003) report results for a specialised sample of Canadian male MBA and law graduates, each of whom is matched to a brother or father using tax records. Instead of using years of education as the *s*-variable, they use a measure of the quality of the business or law school – as ranked by average Graduate Management Admissions Test (GMAT) or Law School Admissions Test (LSAT) percentile scores. The brother's or father's wages, adjusted for age, year, province and city, are used as *z*-variables.

The authors argue that the effect of having a better signal, in the form of graduating from a higher-ranked school, may persist beyond the time taken by employers to fully learn a graduate's actual ability. The reason is that graduates with better signals place into 'higher-quality' firms, where the opportunities for training are greater. As a result of this training they continue to earn more than graduates of similar innate ability who have a poorer signal, thereby confounding interpretation of the *s* coefficients in propositions 3.1 and 3.3.

Heisz and Oreopoulos report estimates of specifications 1, 2, 4 and 5 as defined above. In specifications 2 and 4 (which include *z* but not its interaction with experience), they confirm that the brother's and father's wages have significant 'main' effects after controlling for *s*. In specifications 4 and 5, they find significant positive interactions of *s* with experience, consistent with their training interpretation.

However, the authors argue that while differences in training may continue to affect wages, they will *not* affect movement between firms. For example, given that higher-quality firms provide more training, a higher-quality firm will only recruit a worker who was initially at a lower-quality firm because that worker has been revealed to be of high ability, and *not* *because of differences in training between the firms*. The authors thus propose replacing the wage rate by a measure of firm quality – such as the average wage – as the dependent variable in the regressions in propositions 3.1 and 3.3. Then in proposition 3.1 the coefficient on $s*x$ should now be zero *even in the presence of training*, since s is old information that has already been used to decide which firm a graduate is initially placed in – and thus should not be able to predict movement across firms. Further, in proposition 3.3, the coefficient on $z*x$ will then reflect employer learning alone, without any contamination from training effects.

When Heisz and Oreopoulos (2003) repeat their estimates using firm quality (as measured by the average wage) as the dependent variable, the results are consistent with their hypotheses. Firstly, the coefficient on $s*x$ is not significant. Secondly, the coefficient on z is positive and significant when $z*x$ is not included in the model. Finally, when $z*x$ is entered in the model it has a positive and significant effect.

Table 3.2 summarises the results of these four papers, for estimates of specification 5 in specialised subgroups of the workforce.

Evidence from tests of learning by experience versus tenure

Bauer and Haisken-DeNew (2001) were the first to test the proposition that learning may be asymmetric, or private to the incumbent employer. They split a worker's total labour market experience into tenure with the current firm (τ) and previous experience in other firms ($x - \tau$), and estimate a specification defined as follows:

Specification 6 includes s, $s*\tau$, $s*(x-\tau)$, z, $z*\tau$ and $z*(x-\tau)$. If learning is purely public then it makes no difference whether experience accrues within the current firm or elsewhere, and the interactions of current and previous experience should be equal. If learning is purely private then it is only experience in the current firm that matters – and the previous experience interactions will be zero. Finally if learning is partially private then the previous experience interactions will be non-zero, but smaller than the corresponding terms involving tenure.

In their estimate of specification 6 for their sample of West German males, Bauer and Haisken-DeNew find a significant positive interaction of s with tenure, contrary to the prediction of their theory, while neither of the interactions involving z is statistically significant. When the model is estimated by quantile regression, a significant positive interaction of z with tenure is found at the 25th percentile. However even this result is not consistent with employer learning, since the interaction of s with tenure remains positive and significant at every quartile.

When specification 6 is estimated separately for blue and white-collar workers, there is evidence of private learning for blue-collar workers only. For this group, there is a positive and significant interaction of z with tenure, while the interaction of s with tenure is not significant. For white-collar workers, the interaction of z with tenure is not significant, while the interaction of s with tenure is positive and significant. When this model is estimated by quantile regression, the positive interaction of z with tenure for blue-collar workers is found at every quartile, however a positive interaction of s with tenure is also found at the 50th and 75th percentiles. Thus these results are consistent with purely private learning for blue-collar workers at the 25th percentile, but not for any other group.

Throughout their paper, Bauer and Haisken-DeNew find that the interaction terms involving experience outside the current firm are generally not significant. Thus to the extent that their results support employer learning at all, they suggest that this learning is largely private.

Galindo-Rueda (2003) takes a slightly different approach to testing for asymmetric learning, by distinguishing between tenure in the current firm (τ) and total experience (x), as opposed to experience in other firms. The model he estimates is defined as follows:

Specification 7 includes s, $s*x$, $s*\tau$, z, $z*x$ and $z*\tau$. This is also the model described by equation 3.2 above. The interpretation of the tenure interactions in this specification differs from those in specification 6. In specification 6, each year of experience only contributed to *one* of τ or $(x - \tau)$. In specification 7, a year of experience with the current employer contributes to *both* τ and x. Thus in specification 7, β_{sx} and β_{zx} capture the effect of learning by *all* employers, while $\beta_{s\tau}$ and $\beta_{z\tau}$ capture the effect of any additional information observed by the current employer. In this specification, if learning is purely public then the tenure interactions will be zero; if learning is purely private then the experience interactions will be zero. If learning is partially private, both set of interactions will be nonzero, with the tenure interactions indicating the extent of private information.

Galindo-Rueda estimates specification 7 for his sample of young British males. The coefficient on $z*x$ is positive and significant while that on $z*\tau$ is not significant, a pattern that suggests purely public learning. However, the coefficient on $s*x$ is positive and significant (which contradicts the model) while the coefficient on $s*\tau$ is negative and significant (which suggests private learning). These findings are robust to the inclusion of a control for on-the-job training. The author emphasises the negative coefficient on $s*\tau$, suggesting that this may indicate the presence of private learning with respect to attributes not captured by z.

When specification 7 is estimated separately for blue and white-collar workers, Galindo-Rueda finds that β_{zx} is positive and significant for white-collar workers but $\beta_{z\tau}$ is not, while the opposite pattern holds for blue collar-workers. This pattern is suggestive of public learning for white-collar workers and private learning for blue-collar workers. Unfortunately, none of these effects remain significant after controls for industry, occupation, firm size and training are added to the model. Moreover, the interpretation of public learning for white-collar workers is complicated by a significant negative interaction of s with tenure, while the blue-collar estimate suffers from a significant positive interaction of s with experience.

Schönberg (2005) also tests for asymmetric learning by examining how the returns to measured ability are differentially affected by total experience and by tenure in the current firm. She partitions her NLSY data into two groups – those with less than 16 years of education, and those with 16 or more years – and estimates a specification that includes z, $z*x$ and $z*\tau$ separately for each of these groups.

For those with less than 16 years of education, Schönberg obtains a significant positive coefficient on $z*x$, while the effect of $z*\tau$ is not significant. This suggests that there is public learning for this group. For those with 16 or more years of education, neither of the z interactions is significantly different from zero.

Finally, Schönberg extends her specification to allow the effect of z to vary with the squares of experience and tenure. For those with less than 16 years of education, the results again indicate public learning. For those with 16 or more years of education, the effect of z is found to increase at a diminishing rate with tenure, while the hypothesis that the experience interactions are jointly zero cannot be rejected. Thus in this specification there is evidence of private learning for this group.

Table 3.3 summarises the results of these three papers that estimate the effects of s and z, interacted separately with experience and tenure.

Conclusion

This chapter has undertaken a review of both the theoretical and empirical literature on tests of employer learning, emphasising how the returns to education and measured ability evolve with experience. In chapters 7 and 8, results will be presented for a test of the employer learning model using a new dataset on young Australians. Firstly, however, this data source will be described in chapter 4.

Table 3.1. Reported estimates of employer learning effects

Authors	Country	Dataset	Approx. age	Gender	*z*-variable	*s*x* effect	*z*x* effect
Farber / Gibbons	USA	NLSY to 1991	16 to 34	Males / females	AFQT resid	(–)	+ + +
					Library resid		+ + +
Altonji / Pierret	USA	NLSY to 1992	14 to 35	Males	AFQT	–	+ + +
					Sibling wage	(+)	+ +
					Father's ed.	(–)	(+)
Lange	USA	NLSY to 1998	14 to 41	Males / females	AFQT	– – –	+ + +
Schönberg	USA	NLSY to 2001	14 to 44	Males	AFQT	– –	+ + +
Bauer / Haisken-DeNew	Germany	GSOEP	Up to 60	Males	Max parents' education	+ + +	+
Galindo-Rueda	Britain	NCDS / BCS	21 to 42	Males	Test scores	(+)	+ +
Pan	Canada	SLID	16 to 69	Males	Mother's education	–	+ + +

Note: + + +, + + and + denote positive and significant at the 1%, 5% and 10% levels respectively, while (+) denotes positive but not significant. An analogous convention applies for negative estimates.

Table 3.2. Estimates of learning effects in specialised subgroups

Authors	Country	*z*-variable	Subgroup	*s*x* effect	*z*x* effect
Bauer / Haisken-DeNew	Germany	Max parents' education	Blue-collar	(+)[a]	(+)[b]
			White-collar	(+)[c]	(+)
Galindo-Rueda	Britain	Test scores	Blue-collar	– – –	+ +
			White-collar	+ +	(+)
Strobl	Ghana	Max parents' education	No direct contact, no OJT	–	+
			Direct contact and / or OJT	(+ / –)[d]	(+ / –)[d]
Heisz / Oreopoulos	Canada	Father's wage	MBAs	+ +	+ + +
			Lawyers	+ +	+
		Brother's wage	MBAs	+ + +	(+)
			Lawyers	+	(+)

Notes: + + +, + + and + denote positive and significant at the 1%, 5% and 10% levels respectively, while (+) denotes positive but not significant. An analogous convention applies for negative estimates.
[a] Significant at 75th percentile. [b] Significant at 50th and 75th percentiles.
[c] Significant at 25th and 75th percentiles. [d] Three separate regressions.

Table 3.3. Estimates of learning effects by experience and tenure

Authors	Country	z-variable	***s*x*** **effect**	***s*τ*** **effect**	***s*(x−τ)*** **effect**	***z*x*** **effect**	***z*τ*** **effect**	***z*(x−τ)*** **effect**
Bauer / Haisken-DeNew	Germany	Max parents' education		+ + +	+		(+)[a]	(–)
Galindo-Rueda	Britain	Test scores	+ + +	– – –		+ +	(–)	
Schönberg	USA	AFQT		$s < 16$		+ + +	(–)	
				$s \geq 16$		(+)	(–)	

Note: + + +, + + and + denote positive and significant at the 1%, 5% and 10% levels respectively, while (+) denotes positive but not significant. An analogous convention applies for negative estimates.
[a] Significant at 25th percentile.

CHAPTER 4

DATA CONSTRUCTION

The purpose of this chapter is to describe the source of the data, the construction of the dataset and the definitions of the variables that will be analysed in the chapters that follow.

General description of the LSAY

The data is derived from the 1995 Year 9 cohort of the Longitudinal Surveys of Australian Youth (LSAY, Australian Council for Educational Research 2005). These surveys are jointly managed by the Australian Council for Educational Research (ACER) and the Commonwealth Department of Education, Science and Training (DEST). The purpose of the surveys is to track the education and labour market status of groups of young Australians from around the age of 14 through to around the age of 25.

The 1995 cohort initially comprised a nationally-representative sample of 13,613 Year 9 students (6,717 males and 6,896 females). The modal age of the respondents at the time of inclusion in the survey was 14 years. The sample was designed such that students from smaller states and territories were over-sampled, with students from larger states being correspondingly under-sampled. Within states and territories, students from government, Catholic and independent schools were selected on a proportionate basis. Mathematics and reading tests were administered to the students in their schools in 1995, as was a background questionnaire from which information on parental education and other individual characteristics has been drawn.

Starting in 1997, the respondents were contacted in a series of annual telephone interviews in which they were asked about their schooling, post-secondary education and training, and work history since they were last surveyed. At the time of the work reported here, data from these annual follow-ups were available through to the 2004 interview year, at which time the modal age of the respondents was 23 years.

The LSAY has a number of features that makes it attractive for the work reported here. Firstly, as is required for an investigation of ‘sheepskin’ or credential effects, the education data in the survey is structured in such a way as to permit the construction of separate measures of years of education and completed credentials. Secondly, as is required for a test of employer learning with statistical discrimination, the data contains variables correlated with cognitive ability which may reasonably be expected not to be observed by employers at the time a worker enters the labour market. These take the form of the mathematics and reading test scores, as well as a measure of parental education. Thirdly, the work data contains a variable recording how the respondent found their current job, which makes it possible to compare the pattern of returns to education and experience across hiring channels.

Further, the availability of detailed information on respondents’ complete work history makes it possible to construct a measure of their actual work experience, as opposed to the potential experience variable that is customary in much of the literature. Finally, the survey focuses on young people during the years in which they are entering and establishing themselves in the labour market. These are the years in which the information problems confronting employers are most pronounced, and in which initial screening on the basis of education followed by employer learning in relation to ability is most likely to be observed.

On the other hand, given that the respondents are quite young, care should be taken in extrapolating any conclusions to the broader labour market, and it would be desirable for future research to re-examine these questions as suitable data on more general populations becomes available. Also, as a result of agreements between ACER and the state and territory education authorities, the data is structured so as to not permit comparisons of mathematics and reading test scores across states and school sectors. For this reason, all state variables are omitted from the version of the LSAY containing test score data. Thus it is not possible to control for differences in local labour market conditions across states and territories.

The 1995 cohort is the oldest of three LSAY cohorts on which data is currently being collected. However, there are a number of earlier surveys which may also be candidates for this analysis. Unfortunately, in the case of the Australian Youth Survey (AYS, Commonwealth Department of Employment, Education and Training 1996), test score data is only available for the younger members of the survey, who are thus actually younger than the LSAY 1995 cohort respondents at the date of last follow-up. The AYS is also weakened by its comparatively small sample size of roughly 1,500 respondents in each year cohort. The Youth in Transition survey (YIT, Australian Council for Educational Research 1996) suffers from the fact that it was administered by mail-in questionnaire, with the result that response rates are poor and work histories incomplete. Moreover, these earlier surveys are themselves now somewhat dated, such that inferences drawn from them may no longer be applicable in the contemporary labour market for young people.

Construction of the dataset

To investigate the presence of credential and learning effects in the returns to education, the LSAY was used to construct a dataset linking wages to years of education, labour market experience and other key variables of interest. In relation to post-secondary education, it was decided to restrict attention to study at the level of undergraduate diploma or above, and to only include wage observations on individuals who had ceased any such study for the last time. Thus a pivotal step was to identify the last month in which a respondent was either at school or undertaking study at the level of undergraduate diploma or higher, and to only include wage observations after that date. Similarly, work experience and tenure in the current job were only counted from that date forward. As a result, once an individual enters the sample, their years of education and qualifications are time-invariant characteristics. This is important as it means that the information conveyed to employers by a given individual's education is the same in all years in which they contribute a wage observation to the sample.

Because of the manner in which data on post-secondary education was collected in the LSAY, a number of education-related variables could only be computed for those respondents who remained in the survey until at least 2001, when the modal age of the respondents was 20 years. For this reason, only wage observations for the 2001 to 2004 interview years are included in the analysis. Of the 13,613 respondents originally surveyed in 1995, 6,876 remained in the survey through to 2001 (3,201 males and 3,675 females); these respondents form the basis for the analysis reported here. Of these, 4,660 remained in the survey through to 2004, the most recent year for which data is currently available.

There is some evidence of selective attrition in the survey. For example, Marks and Long (2000) report that between 1995 and 1999, attrition was higher among males, Indigenous Australians, students attending government schools, and students who performed relatively poorly in the mathematics and reading tests. Some of these findings are corroborated in the detailed discussion of variable definitions and in the descriptive statistics set out below. It is also the case that respondents with poorer test scores were less likely to be in full-time work, and therefore to contribute a valid wage observation to the estimating sample. Nonetheless, all of the statistics and analysis reported herein make use of unweighted data.

The final dataset contains observations for the interview years 2001 through 2004 on those respondents who had ceased all schooling and post-secondary education for the last time, who were working 30 or more hours per week for wages or salaries, and whose hourly wage rate was not less than $5 an hour or greater than $100 an hour in 2001 dollars. In total, there are 4,782 person-year observations on 2,094 males, and 4,359 person-year observations on 2,128 females. Details of the variable definitions and dataset construction, as well as descriptive statistics for the final estimating sample, are set out in the sections that follow.

Individual characteristics

Achievement test scores

Literacy and numeracy achievement tests were administered to students in their schools when they were in Year 9 in 1995. Each of these tests was scored out of 20, and then ‘corrected’ to penalise incorrect multiple-choice responses. The test score variable is derived by summing the ‘corrected’ reading and mathematics scores, and then standardising the total to have a mean of zero and standard deviation of one over the full LSAY sample in 1995. When the sample is restricted to those respondents who remain in the survey through until at least 2001, the mean standardised total test score increases to 0.21, while the standard deviation shrinks to 0.93. (Except as noted in the text, the statistics discussed in this chapter are *not* conditional on the other restrictions imposed for inclusion in the final sample. Full descriptive statistics for the final sample are set out at the end of this chapter.) Where the mathematics and reading test scores are entered separately in selected models in chapter 7, these are also expressed in standard deviations from the mean for the full sample in 1995.

A small number of respondents (2.1 per cent of those who remain in the survey until at least 2001) have missing values for one or both components of the test score. Where the test score is a primary focus of the analysis (in some of the models in chapters 7 and 8), these respondents are dropped from the sample.

Parental education

In the first year of the survey in 1995, each respondent was asked the level of education attained by each of their parents. Although the parental education variables were originally recorded on a categorical basis, for the purposes of this research they were recoded into year equivalents as shown in table 4.1.

Where it is available for both parents, the measure of parental education is taken to be the average of the two. Where it is available for only one parent, that value is used alone. Somewhat surprisingly, it appears that respondents were not asked whether they were living with either or both of their parents. It is therefore not clear whether the information on a parent's education is missing because that parent was absent or simply because the information was not known by the respondent. Unfortunately, given that parental education was reported only in the first year of the survey, when the respondents were aged 14, it is missing for both parents for 22.3 per cent of the respondents who remain in the survey through to 2001. Again, where parental education is a primary focus of the analysis in some of the models in chapters 7 and 8, these respondents are dropped from the sample. For the full sample in 1995, the mean of valid responses for parental education is 12.07 and the standard deviation is 2.00. For respondents who remain in the survey through until at least 2001, the mean is 12.20 and the standard deviation is 1.96.

Aboriginal or Torres Strait Islander

This is a dummy variable indicating that the student reported being of Indigenous origin in the 1995 Year 9 survey (2.9% of all respondents, but only 1.8% of those remaining in the survey through until at least 2001).

Language other than English

This is a dummy variable indicating that the student reported speaking a language other than English at home in the 1995 Year 9 survey (9.9% of all respondents, and 8.7% of those who remain in the survey through until at least 2001).

Marital status

This is a dummy variable indicating that the respondent was either married or living with a partner (e.g. common law partner, de facto) in the applicable interview year.

Education variables

Because of gaps in the original data on post-secondary education and training courses that were collected in the interview years 1997 to 2000, retrospective data on *all* episodes of post-secondary education and training were collected from the LSAY participants in 2001. Since the work reported here makes use of this revised data (as updated in follow-up interviews through to 2004), it was necessary to restrict attention to those respondents who remain in the LSAY through until at least 2001 (6,876 of the original 13,613 respondents).

The interview structure of the LSAY maintains a distinction between vocational training programs (apprenticeships and traineeships) and educational courses at a university or college of Technical and Further Education (TAFE), although there is likely to be at least some overlap in the area of TAFE certificates. In the work reported here, primary interest was focused on post-secondary study at the level of TAFE diploma or higher. In particular, the measure of years of post-secondary education is derived from time reported studying toward qualifications at the level of undergraduate diploma or higher.

Secondary schooling

The LSAY reports the month and year in which the respondent left school, and the year-level they were in at the time, at the first interview at which they report that they are no longer in school. Where the respondent reports leaving school in the final three months of the calendar year, they are taken to have *completed* the applicable school year; otherwise, their years of schooling is taken to be one less than the year level they were in at the time of leaving school.

Respondents were also asked separately whether they were awarded a Year 12 certificate of education (New South Wales Higher School Certificate or equivalent), and this question is used as the basis for a separate high school credential variable. A very small number of cases (0.5% of all person-observations) have the high school credential variable missing. In chapters 5 and 6, where this variable is a primary focus of the analysis, these cases are dropped from the sample.

Finally, where a respondent first reports having left school, but then subsequently reports returning to study Year 12 at TAFE, their schooling is updated accordingly. They are also not included in the sample until they have finished study for the last time.

Post-secondary education

For each spell of post-secondary study, the LSAY records the starting and ending months and the mode of attendance (full- or part-time). For the purpose of the work reported here, it was decided to focus on study toward qualifications at the level of undergraduate diploma or higher, of which there are five recognised categories: diploma, advanced diploma / associate, bachelor, graduate diploma / graduate certificate, and postgraduate.

To construct a measure of years of post-secondary education, the time period covered by the survey was divided into six-month blocks, running from January through to June and from

July through to December of each calendar year. These blocks coincide approximately with the academic semesters adopted by most Australian higher education institutions.

For each course of study that the respondent reported having commenced at the level of undergraduate diploma or higher, the starting date and end date (or date at which the respondent last reported undertaking that study) were examined. Each six-month block spanned by these dates was given a weight of one for full-time study, or one-half for part-time study. This was repeated for each course of study reported by the respondent.

This procedure results in a measure of post-secondary education that is meaningful in relation to the academic calendar, and which avoids potential double-counting of overlapping spells of study. The total number of weighted 'semester' blocks was summed and divided by two, and then truncated downward to the nearest integer to approximate the number of *completed* full-time equivalent years of post-secondary education. This was added to the respondent's years of schooling to obtain the final measure of their total completed years of education.

When a respondent first reports that they are no longer studying toward a particular course of study, they are asked whether they completed it or if they stopped for some other reason. This question is used as the basis for the separate completed credentials variables. While it is possible to track credentials at each of the five levels noted above, it was decided to focus on two broader categories. Thus one dummy variable was created to identify respondents whose highest completed credential is at the level of diploma / advanced diploma / associate, while a second dummy variable identifies individuals whose highest completed credential is at the level of degree or above.

Errors and missing values

A respondent is dropped from the analysis if it is not possible to compute their total years of education. This occurs because either the month or year in which the respondent left school is missing, or the month or year in which they started or ended any spell of study toward a recognised post-secondary qualification is missing. Where a respondent is dropped from the sample because of missing education data, they are dropped for all of the interview years. A description of the full set of criteria for inclusion in the sample, and a breakdown of cases surviving each criterion, is provided in the final section of this chapter.

Work variables

Hourly wage rates

The dependent variable is the log of the gross hourly wage rate, expressed in 2001 dollars using the implicit GDP price deflator for household consumption expenditure (Australian Bureau of Statistics 2005). Only respondents who were working for wages or salaries at the time of interview (as opposed to being self-employed, or paid in some other way) were included in the analysis. Observations with gross real hourly wages of less than $5, or greater than $100, are also discarded from the analysis. By way of comparison, the federal minimum wage for full-time adult employees in 2001 was $413.40 per week, which corresponds to an hourly wage rate of $10.88 (Australian Industrial Relations Commission 2001).

All wages refer to the respondent's current main job at the time of interview. Where the respondent had more than one job, this was the job in which they usually worked the most hours. Each respondent was first asked to state the number of hours they usually worked in that job each week or, if their hours varied, the average number of hours worked per week over the past four weeks. They were next asked to state the frequency with which they were paid (weekly, fortnightly, monthly, or other). Depending upon their frequency of pay, they were then asked to state their gross weekly, fortnightly, or monthly pay from that job. Finally, if a respondent was unable to state their gross pay, but could give their hourly rate of pay, then this was recorded instead. Thus in general the LSAY records an individual's gross earnings, and the measure of hourly wages is derived from this. An actual hourly wage rate is only directly recorded where the respondent was unable to state their earnings.

Actual work experience and tenure

The sections of the LSAY used to compute work experience are complex and the exact structure and interview path of the available information shifts from year to year, introducing some limitations and complications to the calculation. As a general rule, respondents were asked for details of one or more specific jobs at each interview, such as the month in which they started that job and the number of hours worked each week. They were also asked separately to itemise the specific months in which they had worked since their last interview, and / or to state the total number of weeks they had worked in the interview period.

The measure of work experience is cumulated in months, starting from the month after the date at which the respondent was last recorded as either being in high school or studying toward a post-secondary qualification at the level of undergraduate diploma or above. Because it was not necessarily possible to map specific months of work to particular jobs, no distinction was made between full- and part-time work in computing experience. Also, since the data from the early interview years lacked interview dates, particular care had to be taken to avoid double-counting.

Because complete work history information is only available from January 1997 onwards, there is the possibility of a gap for those respondents whose last recorded study occurred before this date. Of the 6,876 respondents who remained in the survey through to 2001, there were 333 whose last recorded study occurred in 1995 or 1996. Given that the 1995 survey was administered between August and December of that year, the maximum possible length of this gap is sixteen months. Fortunately, partial data on the work experience of these respondents is available from two sources. Firstly, respondents were asked the length of time in months it took for them to find a job after leaving school. Secondly, where the respondent was working at the time of their 1997 interview, the date at which they started that job is observed, and may fall before January 1997. Information from these two sources was thus used to form a best estimate of work experience prior to the start of 1997.

The measure of tenure in the respondent's current job is again limited by the fact that it was not necessarily possible to map particular months of work to specific jobs. Instead, tenure was derived by simply summing the total number of months elapsed from the date at which the respondent commenced their current job (or ceased study for the last time, if this was later) through to the interview date. Should the resulting figure exceed the more detailed experience calculation described above, the latter was simply substituted instead. A small number of cases (1.1% of all person-observations) have missing tenure because the date at which they commenced their current job is not recorded. Where tenure is a primary focus of the analysis in some of the models in chapter 8, these cases are dropped from the sample.

Finally, months of experience (and tenure) were each divided by twelve to obtain a standard measure in years. Further, in chapters 7 and 8 where the experience (and tenure) profile of wages is modelled flexibly by a string of dummy variables, these were defined by rounding years of experience (or tenure) to the nearest integer, and creating dummies for one through to eight or more years of experience (and one through to seven or more years of tenure).

Occupation and industry

A respondent's job description is coded in the LSAY using the second edition of the Australian Standard Classification of Occupations (ASCO, Australian Bureau of Statistics 1997). Dummy variables were created for each of the top-level categories (nine in total) with the largest category, 'Intermediate Clerical, Sales and Service Workers', treated as the reference group in regression analysis. (This was the largest group for females, and for males and females combined. The largest group for males was 'Tradespersons'.) An additional dummy variable was also created to identify respondents whose occupation was missing. When the sample is partitioned according to occupational class in chapter 8, the respondents whose occupational classification is missing are dropped from the analysis.

Similarly, the LSAY codes the employer's main business activity using the Australian and New Zealand Standard Industrial Classification (ANZSIC, Australian Bureau of Statistics 1993). Dummy variables were created for each of seventeen top-level categories, with the largest group ('Retail Trade') treated as the reference group in regression analysis. An additional dummy variable was also created to identify respondents whose industry classification was missing.

Found job through direct contact

A respondent is defined to have found their job through a direct contact with their employer where they report that they found their current job through a friend or relative, or because they had been approached by their employer.

Respondents who did not find their job through a direct contact reported that they found their job through their school, college or university; that they found it through an employment agency; that the job had been advertised; that they had approached their employer for a job;

that they found their job by checking factory noticeboards; that they had posted a resume on the internet; or that they found their job through some other means. There is some variation in the set of available choices for this question in different interview years; however the two choices defined as constituting a direct contact remain unchanged in all years.

On-the-job training

In the models reported in chapters 7 and 8, a dummy variable indicates the presence of on-the-job training in the current interview year.

Errors and missing values

A respondent is dropped from the analysis if their current weekly hours of work is missing, and / or their current hourly wage rate could not be obtained. Where a respondent is dropped from the sample in one year because this information is missing, it is *not* necessarily the case that they are dropped in any other year, as the necessary information may be available for the other years. The full set of criteria for inclusion in the sample is described in the next section.

Final sample construction

The final sample comprises all person-year observations from 2001 to 2004 for whom:

- The respondent had ceased all schooling and post-secondary education for the last time at the date of the interview. In particular, where a respondent was not studying at the time of a given interview, but then resumed study at a later interview, they were *not* included in the analysis in either year.

- The respondent was working for wages or salary at the time of the interview.

- There was valid data on the respondent's hourly wage rate and weekly hours of work for their current job at the time of interview, as described above.

- The respondent was working at least 30 hours per week, and their gross hourly wage rate was not less than $5 an hour or greater than $100 an hour in 2001 dollars.

- There was complete data on the respondent's years of education, as described above.

Table 4.2 shows the total number of respondents in each interview year, and the number of surviving cases in each year, after successively applying each of the above criteria.

The final dataset takes the form of an unbalanced panel comprising 9,141 person-year observations (the sum of the entries along the bottom row of table 4.2) on 4,222 individuals. Of these, there are 4,782 person-year observations on 2,094 males, and 4,359 person-year observations on 2,128 females. The number of respondents lost in each year due to survey attrition is roughly offset by the number who enter upon completion of their post-secondary education, such that the total number of observations in each year is roughly stable.

Tables 4.3 through 4.5 report descriptive statistics for the final sample, presented separately for males and females. Table 4.3 summarises time-invariant characteristics – the variables observed in 1995, plus those pertaining to education. In this table, the first set of columns report means and standard deviations of the variables for those individuals who contribute *at least one* observation to the final sample; the second set of columns is reported on a person-observation basis. It may be noted that for both males and females, the mean levels of the test score variables, parental education, own years of education and the dummy variable for a degree are all slightly lower when reported on a person-observation basis. This most likely reflects the fact that those respondents with higher levels of the test scores and parental education typically also undertake more years of education and therefore enter the sample later, contributing fewer observations.

Table 4.4 reports the time-varying characteristics, including the key work-related variables. These are presented both by individual interview years from 2001 to 2004, and for the full sample. For both males and females, mean hours of work increase slightly over time. The same is true for real wages. Not surprisingly, the proportion of respondents who are married or living with a partner increases over time. While mean experience and tenure are both increasing, they each increase by less than one full year with each year of calendar time. Again, this partly reflects the later entry into the sample of individuals who complete more years of education; in the case of tenure it obviously also reflects the effect of job turnover. The proportion of respondents who found their job through a direct contact with their employer is fairly stable, at around 39 per cent for males and 29 per cent for females, while the incidence of on-the-job training increases over time.

Finally, table 4.5 reports the distribution of the industry and occupation classification variables for the full sample in all years.

Conclusion

This chapter has described the construction of the dataset that is to be analysed in the chapters that follow. In chapters 5 and 6, this data will be used to investigate the presence of credential effects in the returns to education. In chapters 7 and 8, the data will be used to estimate a model of employer learning with statistical discrimination.

Table 4.1. Coding of parental education

Parental education category	Imputed year equivalent
No secondary school	6
Some secondary school	10
All years of secondary school	12
Trade or technical qualification	13
Degree or diploma	15

Table 4.2. Summary of case selection criteria

Year	2001	2002	2003	2004
Total respondents	6,876	6,095	5,354	4,660
Of whom, respondents who had ceased all schooling and post-secondary education at the time of interview	3,549	3,730	3,796	3,813
Of whom, respondents who were working for wages or salary at the time of interview	2,913	3,064	3,163	3,215
Of whom, respondents with valid hours and wage rate	2,739	2,755	2,847	2,882
Of whom, respondents working at least 30 hours per week, whose hourly wage was not less than $5 or greater than $100	2,165	2,192	2,372	2,452
Of whom, respondents with complete data on education	2,160	2,185	2,362	2,434

Table 4.3. Descriptive statistics for time-invariant characteristics

Variable	By Persons			By Observations			Min	Max
	N	Mean	SD	N	Mean	SD		
Panel 1: Males								
Test score	2,056	0.117	0.967	4,706	0.060	0.958	–3.608	2.196
Mathematics score	2,056	0.212	0.973	4,706	0.160	0.959	–3.407	2.095
Reading score	2,056	0.013	0.960	4,706	–0.035	0.961	–3.202	1.783
Parental education	1,578	12.07	1.90	3,585	12.00	1.86	6	15
Aboriginal or Torres Strait (dummy)	2,094	0.02		4,782	0.02			
Language other than English (dummy)	2,094	0.06		4,782	0.05			
Years of education	2,094	12.69	2.09	4,782	12.36	1.85	8	18
High school certificate (dummy)	2,086	0.72		4,757	0.71			
Undergraduate diploma (dummy)	2,094	0.06		4,782	0.06			
Degree or higher (dummy)	2,094	0.20		4,782	0.14			
Panel 2: Females								
Test score	2,081	0.175	0.871	4,271	0.148	0.862	–3.164	2.048
Mathematics score	2,081	0.048	0.899	4,271	0.022	0.890	–2.856	2.095
Reading score	2,081	0.257	0.868	4,271	0.234	0.862	–2.940	1.783
Parental education	1,671	11.96	1.92	3,433	11.88	1.87	6	15
Aboriginal or Torres Strait (dummy)	2,128	0.02		4,359	0.02			
Language other than English (dummy)	2,128	0.08		4,359	0.07			
Years of education	2,128	13.44	2.05	4,359	13.12	1.91	9	18
High school certificate (dummy)	2,122	0.86		4,341	0.85			
Undergraduate diploma (dummy)	2,128	0.08		4,359	0.08			
Degree or higher (dummy)	2,128	0.34		4,359	0.27			

Table 4.4. Descriptive statistics for time-varying characteristics

Variable	2001		2002		2003		2004		All Years			
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Min	Max
Panel 1: Males												
Hours	41.58	6.96	42.50	8.03	42.59	8.10	43.04	8.36	42.42	7.90	30	100
Wage	13.08	4.95	14.47	4.55	15.90	4.73	17.40	6.02	15.20	5.34	5.00	100.00
Log wage	2.52	0.31	2.63	0.29	2.73	0.29	2.81	0.31	2.67	0.32	1.61	4.61
Age	20.38	0.54	21.38	0.53	22.39	0.53	23.38	0.54	21.88	1.24	19	26
Married (dummy)	0.07		0.13		0.20		0.24		0.16			
Experience	2.99	1.11	3.52	1.51	3.86	1.91	4.10	2.29	3.62	1.81	0.08	9.08
Tenure[a]	1.90	1.26	2.09	1.58	2.16	1.74	2.15	1.91	2.07	1.64	0.00	8.25
Direct contact (dummy)	0.41		0.41		0.37		0.39		0.39			
On-the-job training (dummy)	0.35		0.39		0.45		0.50		0.42			
N	1,201		1,200		1,189		1,192		4.782			
Panel 2: Females												
Hours	38.97	5.11	39.13	5.41	39.54	6.22	39.91	6.21	39.43	5.82	30	90
Wage	12.51	3.14	13.93	3.32	15.32	4.26	16.57	4.33	14.74	4.14	5.00	52.11
Log wage	2.50	0.25	2.61	0.24	2.69	0.26	2.78	0.26	2.65	0.27	1.61	3.95
Age	20.32	0.51	21.31	0.51	22.36	0.52	23.35	0.52	21.96	1.24	19	25
Married (dummy)	0.16		0.24		0.29		0.35		0.27			
Experience	2.57	1.08	2.88	1.52	2.95	1.86	3.34	2.02	2.96	1.72	0.08	8.08
Tenure[a]	1.46	1.04	1.55	1.26	1.62	1.38	1.77	1.48	1.61	1.32	0.00	8.00
Direct contact (dummy)	0.31		0.30		0.28		0.28		0.29			
On-the-job training (dummy)	0.39		0.48		0.58		0.58		0.52			
N	959		985		1,173		1,242		4,359			

Notes: [a] Excludes observations with missing tenure.

Table 4.5. Industry and occupation classifications: All years

Industry / Occupation	Males (%)	Females (%)
Agriculture	4.4	2.0
Mining	1.4	0.6
Manufacturing	13.6	5.3
Electricity	0.5	1.0
Construction	11.6	2.1
Wholesale	4.7	2.0
Retail (reference)	20.1	18.0
Accommodation	6.1	7.8
Transport	2.9	2.4
Communication	1.6	1.5
Finance	3.4	5.0
Property	12.1	14.7
Government	3.8	6.0
Education	2.9	6.9
Health	2.9	14.5
Cultural	2.9	3.2
Personal	2.2	4.6
Missing	2.8	2.5
Managers	2.3	2.3
Professionals	12.0	21.8
Associate professionals	15.8	14.8
Tradespersons	32.9	4.5
Advanced clerical	1.3	5.8
Intermediate clerical (reference)	8.9	33.0
Intermediate production	8.8	1.9
Elementary clerical	8.0	12.4
Labourers	9.9	3.4
Missing	0.1	0.1

CHAPTER 5

RETURNS TO CREDENTIALS FOR YOUNG AUSTRALIANS

This chapter reports estimates of the returns to years of education and completed credentials using the dataset derived from the 1995 Year 9 cohort of the Longitudinal Surveys of Australian Youth (LSAY) as described in chapter 4. A significant advantage of this data source is that the information contained within it on individuals' educational attainments is rich enough to permit the construction of separate variables to describe their years of education and completed qualifications. This makes it possible to model the effect of credentials separately from that of years of education, and thus to obtain a direct estimate of the additional return to completing a credential, holding years of education constant. By contrast, as discussed in chapter 2, much of the early research on credential effects relied on data on years of education alone, meaning that the presence of a credential had to be inferred from the number of years normally required to complete it. Conversely, many other data sources only contain information on the highest completed credential, such that if one wished to estimate a rate of return to years of education, it would be necessary to impute a mapping from credentials to years.

Measures of educational attainment

Table 5.1 reports cross-tabulations of years of education against highest completed credential separately for males and females for the data employed in this chapter. Results are reported for three levels of educational credentials. These are, firstly, the attainment of a high school graduation certificate (New South Wales Higher School Certificate or equivalent), secondly, an undergraduate diploma or advanced diploma from a university or college of Technical and Further Education (TAFE), and thirdly, a university degree at the level of bachelor or higher.

While the LSAY does include data on lower levels of post-compulsory education, in particular TAFE certificates, as well as vocational training in the form of apprenticeships and traineeships, these are not considered here. This is in keeping with the scope of studies undertaken in other countries, which focus on educational qualifications that normally require high school graduation as a prerequisite (associate's and bachelor's degrees and higher, in the case of the US), and thus facilitates comparison of the results with previous research.

Information on high school credentials is obtained from the first interview year after the respondent leaves high school. In addition, following the procedure adopted by Jaeger and Page (1996, p. 734) and Bauer, Dross and Haisken-DeNew (2005, p. 326), all individuals who report any study at the level of undergraduate diploma or above are assumed to hold a high school certificate. This is done in part because high school graduation is normally a prerequisite for admission into these courses. More importantly, this ensures that the estimated return to the two post-secondary credentials can be unambiguously interpreted as the marginal returns to these qualifications relative to high school graduation. This avoids one potential source of confusion that has hindered the interpretation of previous studies, as documented in table 2.1.

While the cross tabulations in table 5.1 are reported on the basis of persons, the panel structure of the dataset means that each person may contribute up to four wage observations. However, the construction of the sample ensures that once a person enters the estimating sample, their educational attainments are time-invariant. The case selection is as described in chapter 4, with one additional restriction. There are a very small number of respondents who have no post-secondary study, and whose high school credential status is missing. This occurs either because they finished school before the 1999 interview year in which the question on high school certificates was first asked, or because they completed their schooling in TAFE and were again therefore not asked the relevant question. These individuals are omitted from the analysis in this chapter, resulting in the loss of 9 males and 6 females. Thus the sample employed in this chapter contains a total of 4,757 person-year observations on 2,085 males, and 4,341 person-year observations on 2,122 females.

Finally, it may be noted from table 5.1 that the sample contains a small number of individuals whose recorded combinations of years of education and credentials would appear *a priori* to be implausible. However, rather than imposing any additional (and essentially arbitrary) selection restrictions, it was decided to follow earlier researchers (for example, Jaeger and Page 1996, table 1; Ferrer and Riddell 2002, table 2; Bauer, Dross and Haisken-DeNew 2005, table 2) in simply allowing these observations to remain in the sample.

Baseline specification and estimation

The baseline specification for the models reported in this chapter is a regression of the log of gross hourly wages on an intercept, a quadratic in experience, a measure of ability, and measures of educational attainment. The reported models differ only respect of the measures of educational attainment that are included. In addition to these variables of central interest, all models include as controls: dummy variables for Aboriginal or Torres Strait Islander status and language other than English spoken in the home (both as reported by the respondent in 1995), a dummy to indicate that the respondent was married or living with a partner in the applicable interview year, 16 industry dummies plus a dummy for missing industry classification, and 8 occupation dummies plus a dummy for missing occupation.

The results for the variables of central interest are summarised in table 5.2 for males and table 5.3 for females. In addition, complete results for models 1 and 5 as defined below – including the additional control variables omitted from the main tables – are reported in appendix 5.1.

All of the models are estimated by pooled ordinary least squares, and all reported inference is derived using a clustered variance-covariance matrix estimator that takes account of the panel structure of the data, and is robust to heteroskedasticity and to arbitrary serial correlation at the level of the individual (Arrelano 1987; Wooldridge 2002, chapter 7). Finally, all of the analysis reported in this chapter makes use of unweighted data.

Results

Model 1: Linear function of years of education

The first model is a traditional Mincer earnings equation in which the log wage is specified as a linear function of years of education, with no credential variables. Results for this model are reported in column 1 of tables 5.2 and 5.3. The point estimates of the private rate of return to education are 4.1% for males and 5.1% for females, and both estimates are highly significant. The experience profile of log wages does not display the concave shape that is usually expected, most likely reflecting the fact that the sample members are still at quite an early stage of their working careers. For males, the estimated experience profile is somewhat convex, while for females it is essentially linear. The evolution of wages with experience will be examined in much greater detail in chapters 7 and 8; the models in those chapters will allow for greater flexibility in the profile of earnings with experience.

The LSAY contains a measure of ability, in the form of the scores from mathematics and reading tests administered to the respondents in 1995. In the context of a study testing the implications of the educational sorting hypothesis, it is debatable whether such a variable rightly belongs in the model, as this will depend on whether or not the employer is assumed to observe the information captured by that variable (Arkes 1999). However, it will be argued in chapter 7 that the information contained in the LSAY test score does indeed appear to be observed by employers at the time of labour market entry, and for this reason the test score has been included in the model. The test score is missing for a small proportion of the sample (less than 2% of all person-observations). Since this omission is not critical to the focus of the present chapter, these cases are retained in the sample and a dummy variable to indicate a missing test score has been included in each of the models.

The results confirm that the test score is indeed positively and significantly correlated with wages. For males, a one standard deviation increase in the test score is associated with a 1.6% increase in wages, while the corresponding effect for females is 2.2%. The coefficient on the indicator for a missing test score is not statistically significant for either males or females in any of the models in this chapter, and this variable does not appear in the main tables.

Coefficient estimates for the remaining variables are not of primary interest to the present research, and are therefore not reported in the main tables. However these results are contained in appendix 5.1, and will now be briefly summarised.

The coefficient on Aboriginal or Torres Strait Islander status is slightly positive but not statistically significant for both males and females. This small positive effect may reflect a selection effect whereby more able members of the Indigenous population are more likely firstly to remain in the survey for a minimum of 7 years, and secondly to be in paid employment. Moreover, Indigenous respondents living in remote areas may be more likely to experience survey attrition because of the difficulty of tracking them for follow-up interviews. Individuals who reported speaking a language other than English in the home when they were in Year 9 earn slightly more (5.2% for males and 3.1% for females) than those who did not. This finding is only consistently significant for males. Finally, respondents who were married or living with a partner earn significantly more (5.0% for males and 2.8% for females) than those who were not. Although the point estimates vary somewhat between models, the above findings (including significance patterns) hold consistently across the set of models reported in this chapter.

The industry effects are defined relative to the largest group (retail trade), which was chosen as the omitted category in the regression analysis. Relative to this group, there are significant positive coefficients for both males and females in 9 of the 16 remaining categories: mining, manufacturing, electricity, construction, transport, communication, finance, government, and health. In addition, there is a significant negative coefficient for males employed in agriculture, and a significant positive coefficient for males employed in property. There are also significant positive coefficients for females employed in accommodation, education, and cultural services. It should however be noted that some of these cells contain relatively few observations, as shown in table 4.5 in chapter 4.

Similarly, occupation effects are defined relative to the largest category, which is 'intermediate clerical'. Relative to this group, there are significant positive coefficients for both males and females in 3 of the 8 remaining categories: professionals, associate professionals, and advanced clerical. There are also significant negative coefficients for both males and females employed as tradespersons, and as labourers.

Since one function of educational attainment and credentials is to act as a pathway to higher paying occupations, it is likely that the inclusion of occupational controls will result in attenuated estimates of the returns to both years of education and credentials. For this reason, the results reported in this chapter may be considered somewhat conservative. As a check of this, the analysis reported in figure 5.1 (which is discussed below) was repeated with the occupational controls omitted. The resulting profiles of log wages with years of education are indeed shifted upward; however their shapes and relative positions are entirely unaffected.

Thus the central qualitative findings of this chapter are not sensitive to the inclusion of the controls for occupation, while the quantitative results may indeed be considered conservative.

For both males and females, and in each of the models reported in this chapter, the null hypotheses that the industry and occupation effects respectively are jointly equal to zero are decisively rejected, with *p*-values of less than 0.0001.

Model 2: Step function of years of education

In the next model, the linear years of education term is replaced by a step function containing a full set of dummy variables for each individual year of education. This is one of the models originally estimated by Hungerford and Solon (1987), and is described by equation 2.1 in chapter 2. The omitted category is those with 8 or 9 years of education; these groups are combined since only two individuals are recorded as not having completed Year 9. Similarly, those with 17 and 18 years of education are also combined into a single group because of the small number of individuals with 18 years of education.

The coefficients in column 2 of tables 5.2 and 5.3 report total returns to each group relative to those with 8 or 9 years of education. The coefficients in column 3 represent the implied step sizes (or marginal returns) relative to the group with one less year of education. The corresponding *t*-statistics in column 3 are derived from the estimated variance-covariance matrix for the complete model. The profile of log wages with years of education for model 2 is also shown by the solid line in panel 1 of figure 5.1 for males, and in panel 2 for females.

While this type of model has sometimes been studied for evidence of credential effects (Hungerford and Solon 1987, Patrinos 1996), it is interpreted here as simply affording the greatest possible latitude for the variation in earnings to be explained by years of education – without any restriction imposed by functional form – before the effects of credentials are entered separately in the model. That is, the estimates in this model reflect total returns combining the effects of both years of education and credentials. Not surprisingly, the null hypothesis that these effects are jointly equal to zero is decisively rejected in this model for both males and females.

The results indicate some evidence of non-linearity in the returns to years of education. For both males and females, there are particularly sizable increases in earnings after both the 13th and 15th years of education. The first of these corresponds to the first year of post-secondary education, while the second corresponds to the length of time normally taken to complete a three-year undergraduate degree. The size of the step after 13 years is roughly twice as large for males as it is for females, while the size of the step after 15 years is of roughly the same size for both males and females. Hungerford and Solon (1987) similarly observe a large step after the 13th year of education, which they interpret as indicating that admission into post-secondary education may itself have a sorting effect in the labour market – an idea they attribute to Arrow (1973).

The step function estimates thus suggest that it would be worthwhile to explore both the possibility of credential effects to completed post-secondary qualifications – and to bachelor’s degrees in particular – as well as the role of post-secondary admission as a signal in its own right. The first of these two points is taken up in models 4 and 5 below, while the second proposition is examined in greater detail in chapter 6.

Model 3: Pure credential specification

At the opposite extreme to models 1 and 2, in which educational attainment enters purely through the number of years of education, column 4 in tables 5.2 and 5.3 reports results of a specification in which years of education is replaced by a set of three dummy variables, to identify individuals whose highest completed credential is at the level of high school certificate, undergraduate diploma / advanced diploma, and degree at the level of bachelor or above. The return to high school graduation is estimated relative to those with no completed qualifications, while the estimated returns to diplomas and degrees are defined relative to high school graduation.

This is sometimes interpreted as a ‘pure credential’ model in the sense that those who commence study toward a qualification without completing it are assumed not to derive any return from their study. However this model does not provide a test of the ‘sheepskin’ hypothesis since, as was the case with the returns to years of education in models 1 and 2, the coefficient estimates reflect combined returns to both completing a qualification and the years of education that go toward it. Once again, it is thus not surprising that the null hypothesis that the three credential effects are jointly equal to zero is decisively rejected in this model. For males, the fit of the pure credential model is superior to that of model 1, but inferior to that of the step function in model 2; for females, the fit of the credential model is inferior to both models 1 and 2.

Bearing in mind that the return to a dummy variable is $e^{\beta} - 1$, where β is the coefficient estimate, the results imply that relative to those who do not complete high school, high school graduates receive a return of 3.7% for males, and 3.9% for females. Relative to high school graduates, males holding an undergraduate diploma receive a return of 10.3%, while the corresponding return for females is 8.0%. Also relative to high school graduates, males with a qualification at the level of bachelor’s degree or higher earn 20.9% more than those who do not, while the corresponding figure for females is also 20.9%. These returns are all statistically significant, but this is especially so for the degree credential.

The results thus far have largely simply reaffirmed the importance of education, whether it is measured in years, or in terms of completed credentials. The next step is to enter both measures of educational attainment into the model simultaneously, and thereby to separate out the returns to years of education from those of completed credentials.

Model 4: Linear specification with credentials

The results in column 5 of tables 5.2 and 5.3 augment the linear specification of model 1 by adding the three credential dummies from model 3. This corresponds to the model estimated by Denny and Harmon (2001), and is described by equation 2.3 in chapter 2.

Compared to model 1, after the credential effects are added to the model, the estimated private rate of return to years of education falls from 4.1% to 2.8% for males, and from 5.1% to 3.7% for females. This comparison with model 1 suggests that (0.0410 – 0.0276) / 0.0410 = 33% of the return to years of education for males is attributable to credentials. The corresponding figure for females is (0.0510 – 0.0369) / 0.0510 = 28%.

Relative to an individual with the same number of years of education but no qualifications, the point estimate of the return to high school graduation is actually negative for both males and females, although it is not statistically significant. It seems likely that this result arises in part because the linear specification may overstate the return to years of education in the region of 12 years.

Relative to an individual with the same number of years of education but only a high school certificate, the marginal return to an undergraduate diploma is 8.2% for males and 4.4% for females. Both of these effects are statistically significant. The estimated credential effect for a bachelor's degree is 13.3% for males, and 10.8% for females. Thus the estimated credential effects for both post-secondary qualifications are higher for males than for females, while the return to years of education is lower.

Even after controlling for years of education, the null hypothesis that the credential effects are jointly equal to zero is rejected for both males and females, and thus model 1 is rejected in favour of model 4. Similarly, while the coefficient on years of education is diminished by the introduction of the credentials, it remains highly significant for both males and females. Thus the pure credential specification of model 3 is also rejected in favour of model 4.

Because model 4 constrains the return to years of education to be the same for all years, this may distort the estimates of the return to credentials. It was suggested above that this may account for the unexpected negative (but not significant) effect of high school graduation for both males and females in model 4. On the other hand, in the discussion of model 2 it was observed that not only was there evidence of nonlinearity in the returns to years of education, but also that in the case of the large increase in earnings after 13 years this did not appear to be associated with the award of a credential. The final step is thus to confirm the existence of credential effects in a specification that includes a more flexible model of the return to years of education.

Model 5: Step function with credentials

The final specification takes the step function from model 2 and augments it with the three dummy variables for completed credentials. This specification allows maximum freedom for the return to education to be explained by years of education, while allowing for credential effects. This model was introduced by Jaeger and Page (1996), and is described by equation 2.2 in chapter 2. Column 6 in tables 5.2 and 5.3 report estimates for the model, while column 7 reports the implied step sizes (or marginal returns) to individual years of education.

Looking firstly at the returns to individual years of education and the corresponding step sizes, the conspicuous jump in earnings after 13 years of education, first observed in model 2, is still present for both males and females. On the other hand, the large increase after 15 years is now no longer as prominent. Thus the increase after 15 years is accounted for by the introduction of credential effects into the model, while the one at 13 years is not. The latter point is revisited in chapter 6, which considers whether there are ‘sheepskin’ returns to post-secondary admission or attendance *without* completion of a credential.

The profile of the return to years of education implied by model 5 is also shown by the dashed line in panel 1 of figure 5.1 for males, and in panel 2 for females. The difference between this profile and the corresponding one from model 2 as shown by the solid line can be interpreted as the part of the total return to education that is a reward for completed credentials. For example, for males with 15 years of education, this is (0.2618 – 0.1598) / 0.2618 = 39% of the total return to education from model 2. For females with 15 years of education, the corresponding figure is (0.21840 – 0.1399) / 0.2184 = 36%. By way of comparison, Jaeger and Page (1996) report that credential effects account for 26% of the total return to 16 years of education, or 56% of the return relative to 12 years, in their US sample of white males.

After allowing this greater flexibility in the effect of years of education, the marginal return to high school graduation now takes the expected positive sign, although it is still not statistically significant for either males or females. On the other hand, the estimated return to an undergraduate diploma is now smaller than the corresponding estimates in model 4 (4.4% for males and 3.9% for females). Indeed the marginal return to completing a diploma is no longer statistically significant for males, and only significant at a 10% level for females (*p*-value of 0.065). Finally, the estimated credential effect for a completed degree remains large and highly significant for both males and females. Compared to an individual with the same number of years of education with only a high school certificate, the return to a completed degree credential is 13.9% for males, and 10.1% for females.

For both males and females, the joint hypothesis that the three credential effects are jointly equal to zero is decisively rejected. Thus the more restrictive step function only specification of model 2 is rejected in favour of model 5. Similarly, the hypothesis that the years of education effects are jointly equal to zero is also clearly rejected. Thus the credentials only specification of model 3 is also rejected in favour of model 5.

Conclusion

The results presented in this chapter clearly establish that credentials play an important role in explaining the returns to education for young Australians. In particular, there is a sizable and significant return to completion of a bachelor's degree for both males and females. There is no significant credential effect to high school graduation, while the results for undergraduate diplomas are mixed. If an undergraduate diploma is considered to be roughly equivalent to the US associate's degree, this pattern appears broadly consistent with the results that have been reported in other countries.

Overall, the portion of the total return to education that can be attributed to credentials is of the order of one-third, and this figure appears to be slightly larger for males than for females. However, even after allowing for credential effects, the returns to years of education also remain important, and there is some evidence that these returns are non-linear.

Having established that credentials play an important role, the next chapter investigates the pattern of credential effects in greater detail. This will be done through the application of quantile regression techniques, by investigating the return to admission into post-secondary education, and by investigating whether the pattern of credential effects varies between workers who were recruited through different hiring channels, which may convey differing amounts of information to employers regarding their productivity.

Figure 5.1. Education profiles of log wages

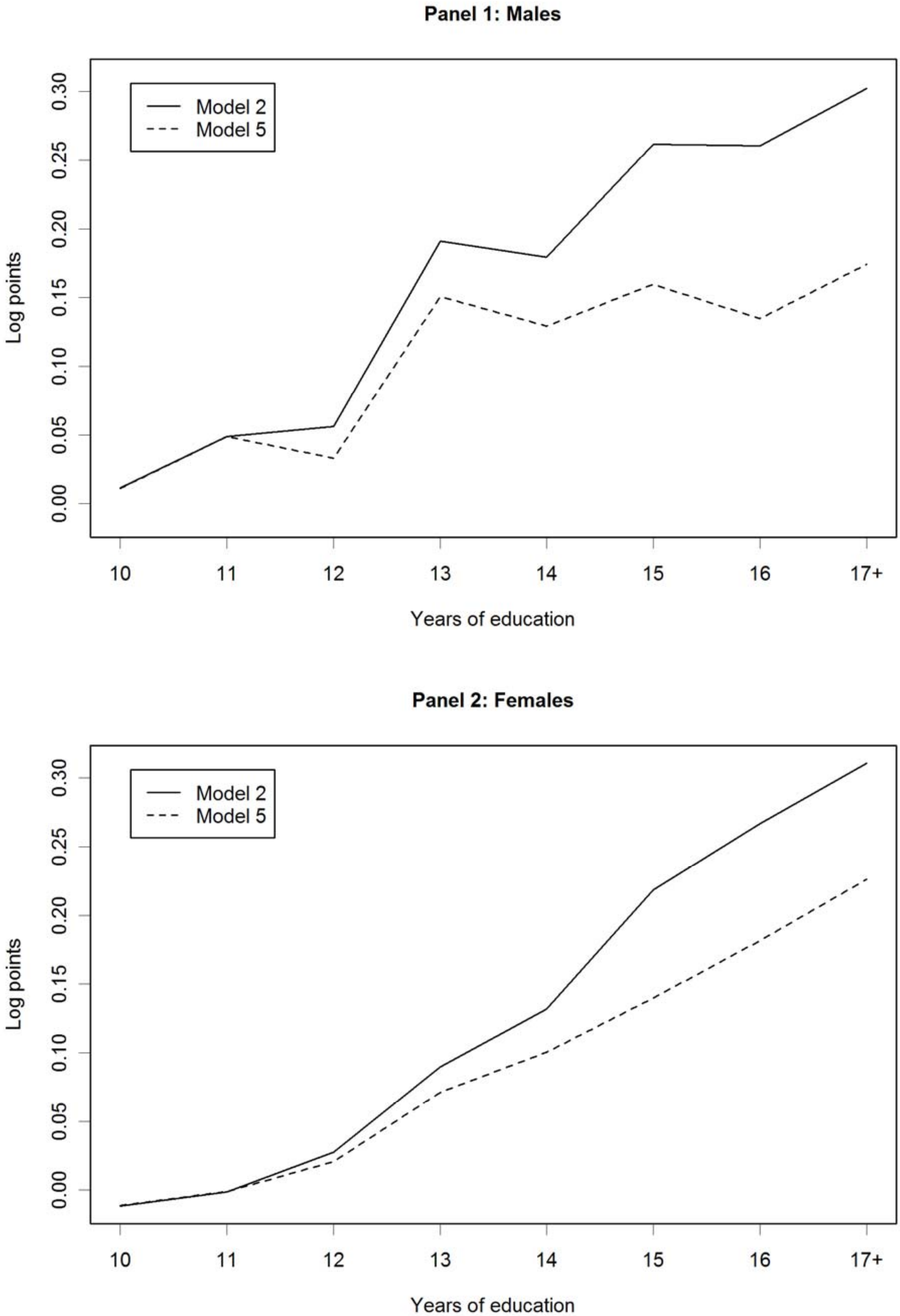


Table 5.1. Cross-tabulation of highest credential by years of education

Panel 1: Males

Years of education	Highest completed credential: No credential	High school certificate	Diploma	Degree or higher	Year total	Year percent
8	2				2	0.1%
9	42				42	2.0%
10	239		2		241	11.6%
11	243	2	4		249	11.9%
12	44	761	6	1	812	38.9%
13		86	37	8	131	6.3%
14	3	51	46	9	109	5.2%
15		42	22	105	169	8.1%
16		25	6	185	216	10.4%
17		10	1	93	104	5.0%
18				10	10	0.5%
Credential total	573	977	124	411	2085	100.0%
Credential percent	27.5%	46.9%	5.9%	19.7%	100.0%	

Panel 2: Females

Years of education	Highest completed credential: No credential	High school certificate	Diploma	Degree or higher	Year total	Year percent
9	6				6	0.3%
10	130	3	1		134	6.3%
11	123	8	3		134	6.3%
12	24	704	10	2	740	34.9%
13		106	43	5	154	7.3%
14		63	90	15	168	7.9%
15		33	22	222	277	13.1%
16		19	8	366	393	18.5%
17		3	2	105	110	5.2%
18		2		4	6	0.3%
Credential total	283	941	179	719	2122	100.0%
Credential percent	13.3%	44.3%	8.4%	33.9%	100.0%	

Table 5.2. Returns to years of education and credentials: Males

	Model 1	Model 2		Model 3	Model 4	Model 5	
		Estimate	Impl. step			Estimate	Impl. step
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Experience	0.0077	0.0295		0.0086	0.0256	0.0313	
	[0.695]	[2.447]		[0.831]	[2.301]	[2.676]	
Experience squared	0.0073	0.0049		0.0064	0.0052	0.0048	
	[5.700]	[3.557]		[5.097]	[4.008]	[3.523]	
Test score	0.0155	0.0157		0.0188	0.0162	0.0159	
	[2.428]	[2.445]		[2.977]	[2.528]	[2.465]	
Years of education	0.0410				0.0276		
	[8.734]				[3.563]		
s = 10		0.0113				0.0111	
		[0.281]				[0.276]	
s = 11		0.0488	0.0375			0.0489	0.0378
		[1.204]	[1.576]			[1.209]	[1.601]
s = 12		0.0561	0.0073			0.0328	-0.0161
		[1.449]	[0.386]			[0.616]	[0.383]
s = 13		0.1909	0.1349			0.1509	0.1181
		[4.252]	[5.616]			[2.491]	[4.640]
s = 14		0.1793	-0.0117			0.1294	-0.0215
		[3.729]	[0.353]			[2.007]	[0.638]
s = 15		0.2618	0.0826			0.1598	0.0304
		[5.703]	[2.596]			[2.483]	[0.830]
s = 16		0.2606	-0.0012			0.1347	-0.0251
		[5.714]	[0.049]			[2.013]	[0.984]
s ≥ 17		0.3025	0.0418			0.1742	0.0396
		[5.901]	[1.397]			[2.435]	[1.330]
Completed HS certificate				0.0367	-0.0037	0.0268	
				[2.634]	[0.205]	[0.704]	
Completed diploma				0.0980	0.0791	0.0433	
				[4.343]	[3.243]	[1.586]	
Completed degree				0.1896	0.1245	0.1303	
				[8.654]	[4.038]	[3.733]	
R-squared	0.1747	0.1815		0.1775	0.1818	0.1866	
p value for zero year effects		0.0000				0.0001	
p value for zero credential effects				0.0000	0.0000	0.0022	

Notes: There are 4,757 person-year observations on 2,085 males. The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner. The full set of estimates for models 1 and 5 are reported in Appendix 5.1.

Table 5.3. Returns to years of education and credentials: Females

	Model 1	Model 2		Model 3	Model 4	Model 5	
		Estimate	Impl. step			Estimate	Impl. step
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Experience	0.0392	0.0527		0.0328	0.0514	0.0555	
	[4.149]	[5.367]		[3.522]	[5.332]	[5.679]	
Experience squared	0.0014	-0.0006		0.0010	-0.0003	-0.0009	
	[1.098]	[0.450]		[0.803]	[0.217]	[0.649]	
Test score	0.0217	0.0210		0.0241	0.0210	0.0207	
	[3.998]	[3.899]		[4.434]	[3.890]	[3.837]	
Years of education	0.0510				0.0369		
	[13.658]				[6.320]		
s = 10		-0.0116				-0.0112	
		[0.154]				[0.150]	
s = 11		-0.0015	0.0101			-0.0011	0.0101
		[0.020]	[0.372]			[0.015]	[0.376]
s = 12		0.0276	0.0291			0.0205	0.0216
		[0.372]	[1.392]			[0.238]	[0.461]
s = 13		0.0898	0.0621			0.0711	0.0506
		[1.178]	[3.348]			[0.805]	[2.586]
s = 14		0.1317	0.0420			0.1004	0.0293
		[1.724]	[1.839]			[1.141]	[1.284]
s = 15		0.2184	0.0866			0.1399	0.0395
		[2.854]	[4.177]			[1.565]	[1.716]
s = 16		0.2668	0.0484			0.1816	0.0417
		[3.482]	[2.888]			[2.020]	[2.474]
s ≥ 17		0.3107	0.0439			0.2262	0.0446
		[3.936]	[1.859]			[2.462]	[1.904]
Completed HS certificate				0.0385	-0.0154	0.0083	
				[2.526]	[0.852]	[0.184]	
Completed diploma				0.0768	0.0431	0.0381	
				[4.115]	[2.200]	[1.843]	
Completed degree				0.1902	0.1025	0.0959	
				[12.955]	[5.080]	[4.189]	
R-squared	0.2493	0.2535		0.2470	0.2570	0.2576	
p value for zero year effects		0.0000				0.0000	
p value for zero credential effects				0.0000	0.0000	0.0005	

Notes: There are 4,341 person-year observations on 2,122 females.The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner. The full set of estimates for models 1 and 5 are reported in Appendix 5.1.

Appendix 5.1. Returns to years of education and credentials: Full results

	Panel 1: Males			Panel 2: Females		
	Model 1	Model 5		Model 1	Model 5	
		Estimate	Impl. step		Estimate	Impl. step
Intercept	1.9747	2.3335		1.7393	2.2889	
	[26.222]	[48.302]		[30.086]	[29.630]	
Aboriginal or Torres Strait Islander	0.0086	0.0099		0.0306	0.0199	
	[0.175]	[0.207]		[0.853]	[0.556]	
Language other than English	0.0503	0.0504		0.0308	0.0293	
	[2.143]	[2.163]		[1.634]	[1.578]	
Married or living with partner	0.0486	0.0487		0.0278	0.0299	
	[3.438]	[3.478]		[3.101]	[3.385]	
Industry: Agriculture	-0.0708	-0.0759		-0.0397	-0.0461	
	[2.307]	[2.518]		[0.969]	[1.151]	
Industry: Mining	0.4153	0.4155		0.1657	0.1548	
	[7.288]	[7.166]		[2.975]	[2.779]	
Industry: Manufacturing	0.0840	0.0882		0.0414	0.0360	
	[4.535]	[4.795]		[2.032]	[1.782]	
Industry: Electricity	0.1783	0.1623		0.1705	0.1650	
	[2.724]	[2.760]		[4.103]	[3.922]	
Industry: Construction	0.1059	0.1051		0.0866	0.0893	
	[4.878]	[4.910]		[3.020]	[3.095]	
Industry: Wholesale	-0.0106	-0.0057		0.0423	0.0451	
	[0.455]	[0.244]		[1.486]	[1.582]	
Industry: Accommodation	-0.0226	-0.0183		0.0539	0.0552	
	[1.104]	[0.896]		[3.087]	[3.118]	
Industry: Transport	0.0795	0.0844		0.1148	0.1142	
	[2.083]	[2.169]		[3.422]	[3.288]	
Industry: Communication	0.1288	0.1188		0.1929	0.1974	
	[3.206]	[2.933]		[5.174]	[5.099]	
Industry: Finance	0.1042	0.0912		0.1268	0.1267	
	[4.089]	[3.528]		[5.701]	[5.634]	
Industry: Property	0.0593	0.0592		0.0146	0.0107	
	[3.181]	[3.188]		[0.974]	[0.711]	
Industry: Government	0.0990	0.0969		0.1600	0.1532	
	[3.300]	[3.226]		[7.039]	[6.731]	
Industry: Education	0.0479	0.0533		0.1245	0.1168	
	[1.655]	[1.880]		[5.968]	[5.554]	
Industry: Health	0.0791	0.0749		0.0872	0.0810	
	[2.337]	[2.203]		[5.174]	[4.823]	
Industry: Cultural	0.0600	0.0579		0.0580	0.0542	
	[1.362]	[1.376]		[2.661]	[2.446]	
Industry: Personal	-0.0320	-0.0273		0.0066	0.0009	
	[0.689]	[0.603]		[0.239]	[0.031]	
Industry: Missing	0.1119	0.1145		0.0119	0.0047	
	[3.242]	[3.260]		[0.425]	[0.174]	
Occupation: Manager	-0.0207	-0.0372		0.0135	0.0097	
	[0.612]	[1.089]		[0.530]	[0.378]	
Occupation: Professional	0.0663	0.0441		0.1375	0.1117	
	[3.068]	[1.992]		[9.965]	[7.855]	
Occupation: Associate professional	0.0370	0.0339		0.0684	0.0635	
	[2.036]	[1.896]		[5.471]	[5.041]	
Occupation: Tradesperson	-0.0855	-0.0813		-0.0664	-0.0701	
	[4.956]	[4.711]		[2.213]	[2.358]	

Appendix 5.1. Returns to years of education and credentials: Full results

	Panel 1: Males			Panel 2: Females		
	Model 1	Model 5		Model 1	Model 5	
		Estimate	Impl. step		Estimate	Impl. step
Occupation: Advanced clerical	0.0748	0.0767		0.0769	0.0738	
	[2.124]	[2.151]		[4.534]	[4.377]	
Occupation: Intermediate production	-0.0127	-0.0069		0.0265	0.0217	
	[0.620]	[0.336]		[0.997]	[0.826]	
Occupation: Elementary clerical	-0.0050	0.0007		0.0140	0.0124	
	[0.256]	[0.034]		[1.070]	[0.944]	
Occupation: Labourer	-0.0763	-0.0715		-0.0651	-0.0714	
	[3.796]	[3.571]		[2.176]	[2.379]	
Occupation: Missing	0.1604	0.1456		0.0899	0.0558	
	[2.505]	[2.296]		[0.831]	[0.512]	
Experience	0.0077	0.0313		0.0392	0.0555	
	[0.695]	[2.676]		[4.149]	[5.679]	
Experience squared	0.0073	0.0048		0.0014	-0.0009	
	[5.700]	[3.523]		[1.098]	[0.649]	
Test score	0.0155	0.0159		0.0217	0.0207	
	[2.428]	[2.465]		[3.998]	[3.837]	
Missing test score	0.0846	0.0694		-0.0075	-0.0081	
	[1.349]	[1.201]		[0.183]	[0.198]	
Years of education	0.0410			0.0510		
	[8.734]			[13.658]		
s = 10		0.0111			-0.0112	
		[0.276]			[0.150]	
s = 11		0.0489	0.0378		-0.0011	0.0101
		[1.209]	[1.601]		[0.015]	[0.376]
s = 12		0.0328	-0.0161		0.0205	0.0216
		[0.616]	[0.383]		[0.238]	[0.461]
s = 13		0.1509	0.1181		0.0711	0.0506
		[2.491]	[4.640]		[0.805]	[2.586]
s = 14		0.1294	-0.0215		0.1004	0.0293
		[2.007]	[0.638]		[1.141]	[1.284]
s = 15		0.1598	0.0304		0.1399	0.0395
		[2.483]	[0.830]		[1.565]	[1.716]
s = 16		0.1347	-0.0251		0.1816	0.0417
		[2.013]	[0.984]		[2.020]	[2.474]
s ≥ 17		0.1742	0.0396		0.2262	0.0446
		[2.435]	[1.330]		[2.462]	[1.904]
Completed high school certificate		0.0268			0.0083	
		[0.704]			[0.184]	
Completed diploma		0.0433			0.0381	
		[1.586]			[1.843]	
Completed degree		0.1303			0.0959	
		[3.733]			[4.189]	
R-squared	0.1747	0.1866		0.2493	0.2576	
p value for zero year effects		0.0001			0.0000	
p value for zero credential effects		0.0022			0.0005	

Notes: There are 4,757 person-year observations on 2,085 males and 4,341 person-year observations on 2,122 females. The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level.

CHAPTER 6

FURTHER EVIDENCE ON THE PATTERN OF CREDENTIAL EFFECTS

In chapter 5, it was established that young Australians experience significant 'sheepskin' returns to completed credentials over and above the returns attributable to years of education. In particular, there is strong evidence of credential returns to bachelor's degrees, weaker evidence for undergraduate diplomas, and little evidence at all in the case of high school graduation. These results hold even when the effect of years of education is modelled flexibly by a non-parametric step function (model 5 in tables 5.2 and 5.3).

In the literature on credential effects, it has been customary to attribute the return to years of education to human capital effects, while the additional return to credentials is interpreted as a measure of the importance of sorting effects. However, as will be discussed in the final section of this chapter, this interpretation is subject to some qualification as there may be alternative human capital based explanations for why the return to education might increase discontinuously upon completion of a credential.

The purpose of this chapter is to examine the pattern of credential effects in greater detail across a number of dimensions. Firstly, the technique of quantile regression will be applied to examine how the pattern of credential effects varies across the wage distribution. Next, given the evidence of a sizable step in the returns to education after 13 years – which is apparently not explained by the completion of any credential – and building upon a suggestion by Arrow (1973), the question of whether admission into post-secondary education may itself have a sorting effect will be examined. Finally, building upon the insights of Albrecht (1981) and Albrecht and van Ours (2005), who argue that the importance of sorting may vary according to hiring channels, the pattern of credential effects will be estimated separately for those who found their job through a direct contact with their employer, and those who did not.

Quantile regression estimates of credential effects

This section reports estimates of models 2 and 5 from chapter 5 obtained by the method of quantile regression. Before turning to the results, the basic concepts of quantile regression will firstly be reviewed.

Overview of quantile regression

Ordinary least squares (OLS) regression is concerned with estimating the conditional mean of the dependent variable as a function of the covariates. By focusing exclusively on the effect of the independent variables upon the conditional mean of the response variable, this procedure implicitly disregards the possibility that the covariates may influence the response variable differentially at other parts of its conditional distribution. Quantile regression methods extend the idea of OLS regression to the estimation of models of the conditional quantiles, in which the quantiles of the conditional distribution are expressed as functions of the covariates (Koenker and Hallock 2001, Koenker 2005).

The basic ideas can be established by starting with the case of *unconditional* quantiles. As is well known, the sample median of a random variable can be found by minimising the sum of absolute residuals, since the minimum occurs when the number of positive and negative residuals is equated. This idea can be generalised to the other quantiles by simply attaching asymmetric weights to the (absolute) positive and negative residuals. For example, if the penalty for a positive residual is three times as great as that for a negative one, then the 75^{th} percentile will be selected. Thus the θ^{th} sample quantile ($\theta \in (0, 1)$) may be found by minimising, with respect to β:

$$\sum_{y_i \geq \beta} \theta |y_i - \beta| + \sum_{y_i < \beta} (1-\theta)|y_i - \beta|$$

Extending this to the case of linear regression, the θ^{th} *conditional* quantile of the dependent variable may be modelled as:

$$Q_\theta(y|X) = X'\beta(\theta)$$

where X is a vector of covariates and $\beta(\theta)$ is a conformable vector of quantile regression coefficients to be estimated. The θ^{th} quantile regression estimator is then found by minimising, with respect to $\beta(\theta)$:

$$\sum_{y_i \geq X_i'\beta(\theta)} \theta \left|y_i - X_i'\beta(\theta)\right| + \sum_{y_i < X_i'\beta(\theta)} (1-\theta)\left|y_i - X_i'\beta(\theta)\right|$$

This in turn may be formulated as a linear programming problem, which can be solved efficiently by means of modern computing technology. By varying θ, the effect of X at any quantile of the conditional distribution can be obtained. These coefficients are interpreted analogously to OLS coefficients, as the marginal effect of the independent variable on the θ^{th} conditional quantile of the dependent variable.

Having obtained a vector of quantile regression coefficients, the next problem is to obtain an estimate of its variance-covariance matrix. This may be done either by invoking some form of asymptotic theory, or by using the bootstrap. The bootstrapping procedure is succinctly described by Koenker and Hallock (2000, p. 14) in the following terms:

> "Pairs (x_i, y_i) $i = 1, \ldots, n$ are drawn at random from the original observations *with replacement*. For each resampling the estimator $\widehat{\beta}_n^*(\tau)$ is recomputed. Repeating this procedure B times yields a sample of B p-vectors whose sample

covariance matrix constitutes a valid estimator of the covariance matrix of the original estimator."

The quantile regression estimates reported in this chapter were obtained using the **quantreg** library developed by Koenker (2006) for the open-source **R** language (R Development Core Team 2006). Unfortunately, this package does not return a measure of goodness of fit for models fitted by quantile regression. The reported *t*-statistics and hypothesis tests are derived from an estimated variance-covariance matrix obtained by bootstrapping with 200 repetitions.

Quantile regression of step function model without credentials

Table 6.1 reports quantile regression estimates of the step function specification without credentials (model 2 of chapter 5), for males in panel 1 and females in panel 2. These estimates are reported at the 25^{th}, 50^{th} and 75^{th} percentiles of the wage distribution.

These results confirm that the large discrete increases found after the 13^{th} and 15^{th} years of education, as reported in chapter 5, hold consistently across the wage distribution for males. For females, the same pattern is also clearly evident at the 50^{th} and 75^{th} percentiles. As was the case in chapter 5, the step after 13 years is more pronounced for males than for females. For males, the total return to 13 years of education (relative to 9 or fewer) is largest toward the bottom of the wage distribution, while the return at 15 years is roughly equal across the three quartiles. On the other hand, for females the return to each of these years (again relative to 9 or fewer years of education) increases monotonically moving up through the quartiles. More generally, for males the total returns for each year of education from 13 onward are always greatest toward the bottom of the wage distribution, while for females the returns for each year always increase monotonically moving toward the top of the distribution.

Quantile regression of step function model with credentials

Table 6.2 reports quantile regression estimates of the step function specification with credentials (model 5 of chapter 5), for males in panel 1 and females in panel 2. Looking firstly at the returns to years of education, the distinctive jump in earnings after 13 years that was observed in model 2 is still found (for males at every quartile, and for females at the 50th and 75th percentiles), while the one after 15 years no longer stands out. That is, the jump at 15 years is accounted for by the addition of credential effects to the model, while the one at 13 years is not. For males, the cumulative return to each year of education from 12 onward (relative to 9 years or less) is greatest at the bottom quartile, while for females the return at every year is greatest at the top.

Turning next to the estimated credential effects, there is a significant return to high school graduation of 8.2% for males at the 75th percentile, with no evidence of any sheepskin return to this qualification anywhere else. There is also a marginally significant ($p = 0.082$) return of 4.7% to completion of an undergraduate diploma for males at the 25th percentile of the wage distribution, but again not elsewhere. For females, none of the credential returns for high school graduation or undergraduate diplomas are statistically significant, although the point estimates for the diploma are consistent with the view that – as was the case for males – this credential is more valuable toward the bottom of the wage distribution.

The strongest finding, however, is that there is a significant credential effect accruing to the completion of a bachelor’s degree for both males and females, and that this is present right across the wage distribution. The magnitude of this effect ranges from 7.1% for females at the 25th percentile to 11.7% for both males and females at the 75th percentile. For both males and females, the point estimates of the degree effect increase monotonically moving up the wage distribution, although this increase is steeper for females. In a final striking result, the point estimate of the degree effect turns out to be identical for both males and females at the 75th percentile.

As was the case in chapter 5, the null hypothesis that the years of education effects are jointly equal to zero is consistently rejected for both males and females at every quartile. The same is true for the credential effects, except for females at the 25th percentile of the wage distribution, where the *p*-value of this test is marginal at 0.058.

Finally, figure 6.1 presents an analysis similar to that of figure 5.1, comparing the profile of log wages with years of education before and after the introduction of credential effects, at each of the quartiles. In two of the panels (namely, males at the 25th percentile and females at the 75th percentile), the dashed line for model 5 unexpectedly passes above the solid line for model 2 for portions of the lower part of the profile. This appears to be related to the finding of a negative (but not statistically significant) point estimate of the credential effect of high school graduation at these points in the wage distribution.

Comparing the profiles as they progress up the wage distribution, the divergence between the solid and dashed lines, representing the part of the total return to education that is explained by credential effects, becomes more pronounced for males, especially at the 75th percentile. By contrast for females, there is less evidence that this divergence grows more pronounced.

For males at the 25th percentile of the wage distribution, the fraction of the total return to 15 years of education that is attributable to credential effects is 8.4% (i.e. (0.2491 – 0.2283) / 0.2491). This increases to 25.4% at the median of the wage distribution, and to 63.9% at the 75th percentile. By contrast, the corresponding figures for females are 50.0%, 32.5% and 19.8% respectively.

Thus the conclusion from chapter 5, that credential effects explain roughly one-third of the return to 15 years of education relative to 9, masks a degree of heterogeneity in the importance of these effects across the wage distribution. For males, the importance of credential effects appears to increase moving up the wage distribution. For females, if anything their importance is greater toward the bottom of the distribution.

‘Sheepskin’ returns to post-secondary attendance

One of the more striking features of the step function estimates, both as reported in chapter 5 and in the quantile regression results above, is the finding of a sizable increase in earnings after 13 completed years of education. This result is found for both males and females, and persists after the inclusion of credential effects in model 5. A similar result is reported by Hungerford and Solon (1987) for US data, using a step function specification without credentials. They interpret their result as suggesting that post-secondary admission or attendance may itself have a signalling effect, an idea they trace back to Arrow (1973). In the subsequent literature on direct estimates of credential effects, a number of authors have sought to test this conjecture, by estimating the return to a dummy variable for some college or university study with no degree as an additional ‘credential’ in the model. Jaeger and Page (1996) and Park (1994) report significant sheepskin returns to ‘some college’ in the US, while Bauer, Dross and Haisken-DeNew (2005) report a similar result for university dropouts in Japan, at least among those employed in small firms.

Table 6.3 reports OLS estimates of model 5 from chapter 5, augmented by the inclusion of an additional dummy variable to identify individuals who have some post-secondary study at the level of TAFE diploma or higher, but no completed post-secondary credential. This amounts to a test of whether the returns to these individuals accrue to their years of education, or to the mere fact that they attended a course of study without completing it. If it is the years of education that is important, then the inclusion of a ‘some post-secondary’ variable should have little effect on the returns to years of education compared to those reported in chapter 5, while the return to the dummy variable should be close to zero. Conversely, if it is the fact of attendance that matters, then there should be a positive ‘credential’ effect to ‘some post-secondary study’, while the returns attributable to years of education will diminish.

As it turns out, the returns to years of education do indeed diminish with the inclusion of ‘some post-secondary’ as an additional ‘credential’. This is most clearly seen in figure 6.2. This figure replicates the education-wage profiles from figure 5.1 for models 2 and 5 of chapter 5, and then shows the profile after including the ‘some post-secondary’ variable as the dotted line (‘model 6’). The profile for males is now close to horizontal, and indeed the null hypothesis that the years of education effects are jointly equal to zero now cannot be rejected for the males. The profile for females is also displaced downwards, but still displays a clear positive slope, and so the hypothesis that the year effects are jointly equal to zero is rejected for females.

Before discussing the estimates of credential effects, it is necessary to clarify how these are defined. As was the case in chapter 5, all individuals who report any post-secondary study at the level of diploma or above have the high school graduation dummy set equal to one. This applies both to those who hold a completed credential and those who do not. However, each such individual only has exactly one of the ‘some post-secondary’, diploma or degree dummies set, corresponding to their highest level of educational attainment. This set of variable definitions mirrors the one adopted by Jaeger and Page (1996), and implies that the credential effect to each level of post-secondary attainment is estimated *relative to high school completion*. We may also be interested in the *marginal credential effect* to completing a diploma or degree *relative to some post-secondary study* without completing a credential.

These implied marginal returns are shown in columns 2 and 4 of table 6.3, with *t*-ratios derived from the estimated variance-covariance matrix for the complete model.

Turning to the results, it will first be noted that there is once again no significant credential effect for high school graduation. The major new result is that there is a significant 'credential' effect to some post-secondary study, of 12.9% for males and 13.1% for females. These figures are comparable to those implied by the estimates reported for the US of 8.6% by Jaeger and Page (1996) and 16.8% by Park (1994). Taken in conjunction with the decline in the return to years of education as discussed above, this suggests an important sorting effect of admission into post-secondary education – for males in particular, there is very little room left for years of education to have any effect.

Because the estimated returns to years of education are diminished by the inclusion of a dummy for some post-secondary study, the estimated credential effects to completion of diplomas and degrees (over and above the return explained by years of education) are shifted upward correspondingly. The estimated credential effect for an undergraduate diploma is now 16.4% for both males and females, and this effect is now statistically significant. (By way of comparison, Jaeger and Page obtain an estimate of 21.0% for academic associate's degrees in the US.) The credential effect for a completed degree is now 28.4% for males, and 24.1% for females. These returns are all considerably larger than the ones reported in chapter 5.

However, the comparison implicit in these credential effects – between an individual who holds a post-secondary credential and one who only has a high school certificate, *with no post-secondary study* – is arguably misleading. A better measure of the 'sheepskin' return to completing a credential is given by the 'marginal' credential effects reported in columns 2 and 4 of table 6.3, which compare the returns of those who complete a credential to those who commence post-secondary study without completing it. These marginal credential effects are very similar to the original effects reported for model 5 in chapter 5, which did not include a 'some post-secondary' variable. In particular, the marginal returns to undergraduate diplomas are on the order of 3% and not statistically significant, while the marginal credential effect to completion of a bachelor's degree is 13.7% for males and 9.7% for females.

These findings are notable at two levels. Firstly, they confirm the presence of a 'credential' effect to some post-secondary study, in the sense that it is the fact of such study that appears to be rewarded rather than the number of years. This finding is particularly striking for males. Secondly, the results cast new light on the interpretation of previous studies, which did not compare results before and after including a 'some college' variable. For example, Jaeger and Page (1996) include a credential effect for 'some college, no degree' in their model, yet they report the credential effect of a bachelor's degree relative to high school graduation. The discussion above suggests that this will overstate the 'sheepskin' return to completing college (in effect, because it combines the credential effects of *both attendance and completion*), and that a more appropriate measure is the marginal return relative to 'some college'.

Patterns of credential effects by hiring channels

The results reported thus far, both in this chapter and in the previous one, provide strong confirmation of the presence of 'sheepskin' or credential effects to completed qualifications at the level of the bachelor's degree (and also, to a lesser extent, to undergraduate diplomas), over and above the returns attributable to years of education. However, while such effects are typically interpreted as evidence of the sorting effect of education, some caution should be exercised in reaching this conclusion. The reason is that a variety of alternative human capital explanations may be proffered for why the return to education might increase discretely upon attainment of a credential.

For example, if a program of study comprises a package of complementary skills, then worker productivity will increase disproportionately upon attainment of the complete package, as indicated by the completion of a credential (Ferrer and Riddell 2002, p. 881). Alternatively, the return to credentials may reflect a self-selection process, whereby individuals who are more efficient learners are more likely to complete a course, and therefore enjoy larger increases in productivity than their years of education alone would suggest (Belman and Heywood 1991, p. 724).

In view of these alternative interpretations, it would be helpful if the pattern of credential effects could be tested against an implication of sorting theory that is more difficult to reconcile with human capital theory. One possible avenue is suggested by a parallel strand in the literature on sorting, which focuses on how the sorting value of education varies between more and less informative hiring channels (Albrecht 1981, Albrecht and van Ours 2005). The basic insight of this literature is that if the reward to education reflects its sorting effect in conveying information about attributes not readily observed by employers, then employers will attach less value to education when workers are recruited through hiring channels that innately convey more information, for example recommendation by an existing employee.

In the context of interpreting the returns to credentials, if the reason why employers reward credentials is that they act as signals of unobserved ability, then the returns to credentials should be less pronounced for workers who are recruited through more informative hiring channels. The reason is that employers would then be able to observe and reward more directly informative indicators of ability instead. On the other hand, if employers reward credentials because they embody a greater amount of human capital, for example a complete package of complementary skills, then they might be expected to pay a premium to all workers who hold a credential, regardless of the hiring channel.

The LSAY dataset contains information on how a respondent found their job, enabling these implications to be tested. A respondent is classified as having obtained their job through a direct contact with their employer if they reported that they found their job through a friend or relative, or because they had been approached by their employer. It is hypothesised that these workers are recruited through more informative hiring channels, and that educational credentials should thus play less of a sorting role for this group. Workers are classified as not having obtained their job through a direct contact if they found their job through a job agency, advertisement, noticeboard, on the internet, or through some other means. These are taken to be less informative hiring channels, in which any sorting role of educational credentials may be expected to be more important.

Given that the existence of credential effects has already been established, the null hypothesis is that these are explained by human capital effects such as the explanations outlined at the start of this section. Under the null, there is no reason to expect the pattern of credential effects to differ across hiring channels. The alternative hypothesis is that the credential effects are attributable to sorting. Under this alternative hypothesis, the credential effects should be stronger for workers recruited through less informative hiring channels, i.e. the no direct contact group.

Table 6.4 reports estimates of model 5 from chapter 5 separately by hiring channels, in panel 1 for males and panel 2 for females. For both males and females, there is no statistically significant credential effect to high school graduation in either group. Turning to undergraduate diplomas, there is no significant credential effect to this qualification for either males or females in the direct contact groups. However, for the no direct contact groups, there is a significant credential effect to undergraduate diplomas of 7.1% for males and 6.2% for females. This result is particularly striking given that the returns to this credential reported for model 5 in chapter 5 were either not significant (for males) or only marginally significant (for females). That is, these earlier results masked the fact that this credential does indeed convey a significant return, at least in those situations where employers are initially less well-informed regarding workers' abilities.

Turning to bachelor's degrees, there is a significant credential effect for this qualification in both groups for males. However, the point estimate is larger for the no direct contact group (16.4%) than in the direct contact group (13.5%). For females, there is no significant degree effect in the direct contact group, while for the no direct contact group there is a highly significant degree effect of 13.7%. Indeed, for females in the direct contact group, it is not possible to reject the hypothesis that the credential effects are jointly equal to zero.

Overall, this pattern of results – in which the credential effects are consistently larger and more significant in the no direct contact groups – is very easy to interpret within a framework in which the returns to credentials are explained by sorting effects. They are much harder to reconcile with an explanation in which credentials are rewarded over and above years of education because they embody a greater amount of human capital. If this were the case, then the credential effects should be observed irrespective of the hiring channel. More generally, it is interesting to note that for both males and females, the fit of the model is superior for the no direct contact group. This is consistent with this being the group in which sorting is more important, as discussed in chapter 2.

Conclusion

This chapter has extended the analysis of chapter 5 by examining the variation in the pattern of credential effects across several dimensions. Firstly, the application of quantile regression confirms that there is some variation in the pattern and importance of credential effects across the wage distribution. For both males and females, the credential effect to completion of a bachelor's degree increases moving up the wage distribution. For males, the portion of the total return to education that is attributable to credentials also shows a clear increase moving up the wage distribution; for females, if anything the opposite is the case.

Secondly, there is a significant 'credential' effect for those with some post-secondary study but no completed qualification. This is accompanied by a marked decline in the estimated return to years of education, indicating that for those who do not complete their courses it is the fact of attendance that is rewarded and not the number of years. It was also argued that when a 'credential' for some post-secondary study is included in the model, the appropriate measure of the 'sheepskin' return to completing a degree or diploma is given by its *marginal credential effect* relative to some post-secondary study. This suggests that some previous authors may have overstated their estimates of credential effects, by reporting them relative to high school graduates with *no* post-secondary study.

Finally, it was found that the credential effects for undergraduate diplomas and bachelor's degrees are more important for workers who were recruited through less informative hiring channels. This result has a natural interpretation in the context of a sorting model of education, but is much harder to reconcile with the view that the returns to credentials (in excess of the return to years of education) arise because they embody a greater amount of human capital.

In chapters 7 and 8, the focus shifts to a second aspect of employer behaviour in the presence of incomplete information regarding workers' abilities. These chapters are concerned with the question of whether there is evidence that employers' information improves with experience.

Figure 6.1: Education-wage profiles for quantile regression

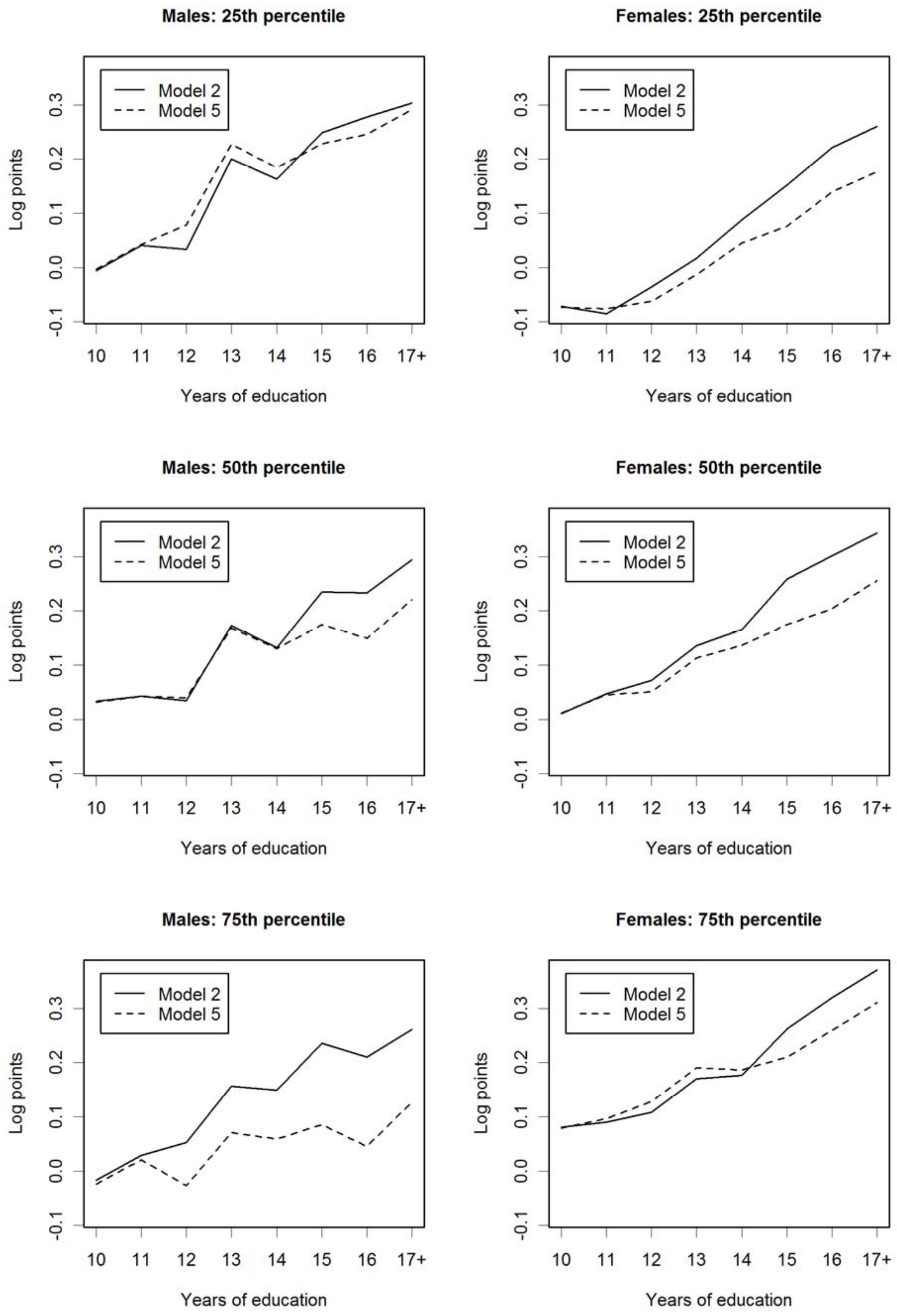
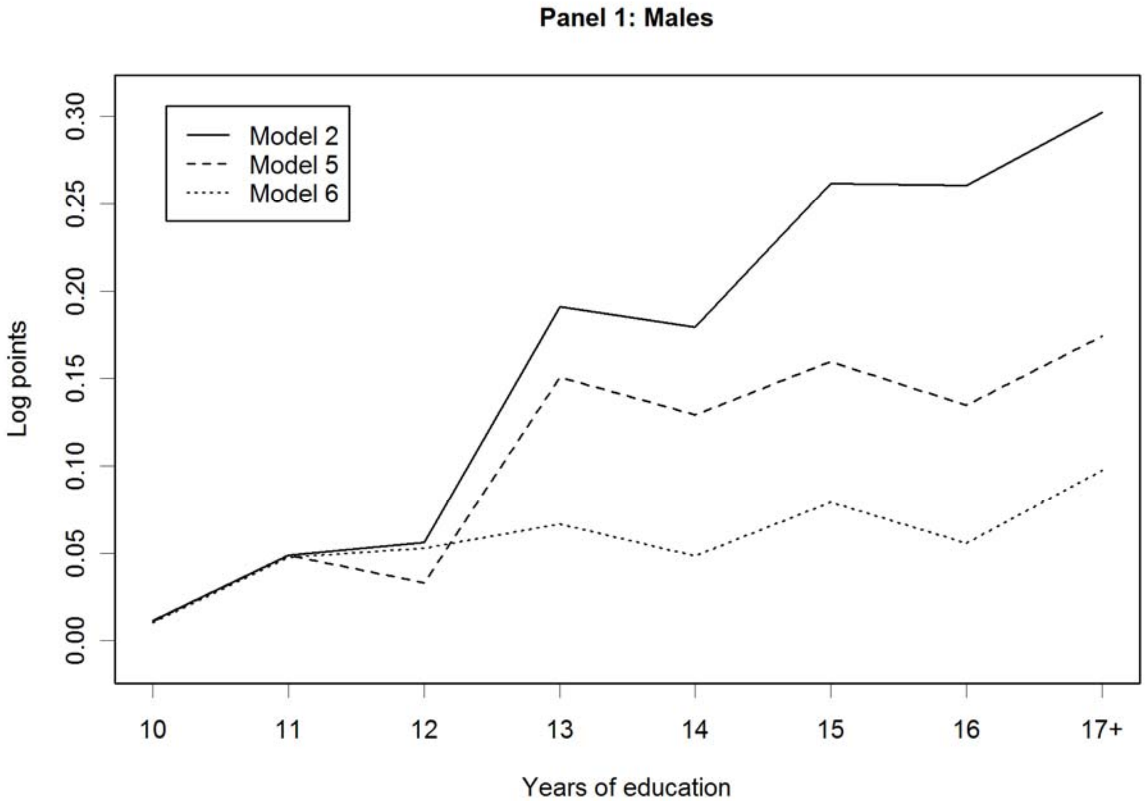


Figure 6.2. Education profiles of log wages



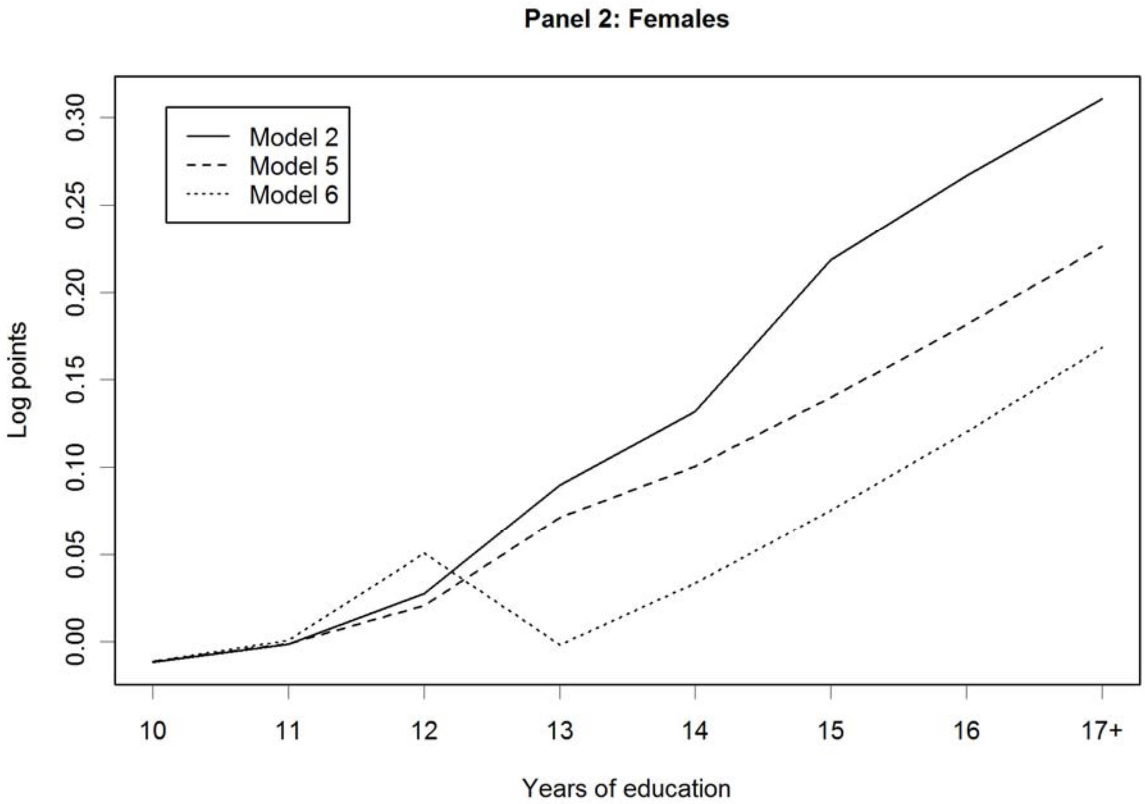


Table 6.1. Quantile regression estimates of model 2

	Panel 1: Males						Panel 2: Females					
	25th percentile		50th percentile		75th percentile		25th percentile		50th percentile		75th percentile	
	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step
Experience	0.0315		0.0171		0.0200		0.0670		0.0489		0.0476	
	[2.047]		[1.452]		[1.522]		[5.623]		[5.088]		[3.897]	
Experience squared	0.0040		0.0050		0.0062		-0.0019		-0.0004		-0.0013	
	[2.163]		[3.461]		[3.740]		[1.295]		[0.306]		[0.850]	
Test score	0.0161		0.0112		0.0162		0.0297		0.0171		0.0181	
	[3.103]		[1.980]		[2.656]		[5.488]		[3.417]		[3.401]	
s = 10	-0.0062		0.0332		-0.0163		-0.0718		0.0102		0.0807	
	[0.166]		[1.053]		[0.358]		[0.610]		[0.114]		[0.693]	
s = 11	0.0407	0.0469	0.0428	0.0096	0.0284	0.0447	-0.0851	-0.0133	0.0467	0.0366	0.0901	0.0094
	[1.056]	[2.147]	[1.269]	[0.492]	[0.636]	[2.669]	[0.723]	[0.497]	[0.519]	[1.818]	[0.789]	[0.383]
s = 12	0.0335	-0.0071	0.0344	-0.0083	0.0526	0.0242	-0.0362	0.0489	0.0716	0.0248	0.1084	0.0183
	[0.882]	[0.459]	[1.122]	[0.579]	[1.174]	[1.465]	[0.307]	[2.339]	[0.807]	[1.489]	[0.963]	[1.175]
s = 13	0.2007	0.1672	0.1734	0.1390	0.1567	0.1041	0.0169	0.0531	0.1355	0.0639	0.1698	0.0614
	[4.386]	[7.476]	[4.799]	[7.669]	[3.073]	[4.681]	[0.143]	[2.755]	[1.507]	[4.656]	[1.492]	[3.193]
s = 14	0.1633	-0.0374	0.1320	-0.0414	0.1492	-0.0075	0.0883	0.0714	0.1654	0.0299	0.1767	0.0069
	[3.333]	[1.126]	[3.403]	[1.685]	[2.781]	[0.232]	[0.755]	[3.347]	[1.855]	[1.646]	[1.539]	[0.322]
s = 15	0.2491	0.0858	0.2348	0.1028	0.2361	0.0870	0.1520	0.0636	0.2590	0.0936	0.2621	0.0854
	[5.039]	[2.522]	[5.932]	[3.683]	[4.801]	[2.727]	[1.290]	[3.358]	[2.860]	[5.405]	[2.288]	[4.243]
s = 16	0.2783	0.0292	0.2331	-0.0017	0.2108	-0.0253	0.2217	0.0697	0.3019	0.0429	0.3201	0.0580
	[5.710]	[1.090]	[5.416]	[0.072]	[4.147]	[1.117]	[1.874]	[3.395]	[3.315]	[2.814]	[2.792]	[3.167]
s ≥ 17	0.3038	0.0255	0.2947	0.0616	0.2611	0.0503	0.2609	0.0393	0.3438	0.0420	0.3713	0.0512
	[4.894]	[0.564]	[6.244]	[2.466]	[4.667]	[1.766]	[2.117]	[1.005]	[3.673]	[1.655]	[3.146]	[1.726]

Notes: There are 4,757 person-year observations on 2,085 males and 4,341 person-year observations on 2,122 females. The dependent variable is log real hourly wages in 2001 dollars. Numbers in square brackets are absolute values of t-statistics, derived from a variance-covariance matrix estimator obtained by bootstrapping with 200 repetitions. Estimation was by the quantreg package for R (Koenker 2006), which does not report goodness of fit for quantile regressions. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

Table 6.2. Quantile regression estimates of model 5

	Panel 1: Males						Panel 2: Females					
	25th percentile		50th percentile		75th percentile		25th percentile		50th percentile		75th percentile	
	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step
Experience	0.0326		0.0230		0.0291		0.0710		0.0477		0.0491	
	[2.069]		[1.904]		[2.241]		[5.365]		[5.074]		[3.990]	
Experience squared	0.0039		0.0044		0.0053		-0.0023		-0.0003		-0.0015	
	[2.101]		[2.997]		[3.284]		[1.460]		[0.286]		[0.964]	
Test score	0.0150		0.0128		0.0147		0.0262		0.0151		0.0164	
	[2.796]		[2.260]		[2.439]		[4.617]		[3.055]		[3.086]	
s = 10	-0.0035		0.0318		-0.0243		-0.0736		0.0114		0.0787	
	[0.093]		[1.020]		[0.528]		[0.622]		[0.129]		[0.689]	
s = 11	0.0422	0.0457	0.0425	0.0107	0.0209	0.0452	-0.0761	-0.0025	0.0451	0.0336	0.0960	0.0173
	[1.109]	[2.135]	[1.279]	[0.554]	[0.461]	[2.699]	[0.651]	[0.092]	[0.495]	[1.686]	[0.865]	[0.691]
s = 12	0.0779	0.0357	0.0397	-0.0028	-0.0266	-0.0475	-0.0620	0.0141	0.0510	0.0059	0.1279	0.0319
	[1.388]	[0.876]	[1.016]	[0.093]	[0.467]	[1.235]	[0.498]	[0.315]	[0.556]	[0.171]	[1.073]	[0.661]
s = 13	0.2281	0.1502	0.1683	0.1286	0.0710	0.0975	-0.0132	0.0488	0.1132	0.0622	0.1904	0.0625
	[3.739]	[6.426]	[3.745]	[6.469]	[1.134]	[4.299]	[0.105]	[2.332]	[1.203]	[4.038]	[1.574]	[3.193]
s = 14	0.1841	-0.0440	0.1302	-0.0381	0.0586	-0.0123	0.0451	0.0584	0.1362	0.0230	0.1870	-0.0033
	[2.917]	[1.258]	[2.658]	[1.418]	[0.953]	[0.423]	[0.364]	[2.588]	[1.467]	[1.250]	[1.549]	[0.157]
s = 15	0.2283	0.0443	0.1751	0.0449	0.0852	0.0266	0.0761	0.0310	0.1749	0.0387	0.2101	0.0231
	[3.572]	[1.120]	[3.705]	[1.335]	[1.353]	[0.867]	[0.595]	[1.057]	[1.842]	[1.894]	[1.721]	[0.863]
s = 16	0.2460	0.0177	0.1493	-0.0258	0.0455	-0.0397	0.1394	0.0632	0.2038	0.0289	0.2592	0.0490
	[3.684]	[0.642]	[2.934]	[1.037]	[0.701]	[1.470]	[1.093]	[3.053]	[2.108]	[1.874]	[2.110]	[2.715]
s ≥ 17	0.2917	0.0458	0.2204	0.0710	0.1266	0.0811	0.1767	0.0373	0.2563	0.0525	0.3111	0.0519
	[4.009]	[1.000]	[3.978]	[2.420]	[1.948]	[2.933]	[1.326]	[0.972]	[2.565]	[2.084]	[2.489]	[1.845]
Completed HS certificate	-0.0428		-0.0051		0.0785		0.0312		0.0208		-0.0181	
	[1.088]		[0.171]		[2.255]		[0.785]		[0.653]		[0.363]	
Completed diploma	0.0464		0.0294		0.0307		0.0312		0.0107		0.0136	
	[1.738]		[1.213]		[1.333]		[1.454]		[0.638]		[0.672]	
Completed degree	0.1016		0.1086		0.1103		0.0682		0.0849		0.1103	
	[3.077]		[3.723]		[3.568]		[2.475]		[4.190]		[4.017]	
p value for zero year effects	0.0000		0.0000		0.0000		0.0000		0.0000		0.0000	
p value for zero credential effects	0.0129		0.0018		0.0006		0.0582		0.0002		0.0008	

Notes: There are 4,757 person-year observations on 2,085 males and 4,341 person-year observations on 2,122 females. The dependent variable is log real hourly wages in 2001 dollars. Numbers in square brackets are absolute values of t-statistics, derived from a variance-covariance matrix estimator obtained by bootstrapping with 200 repetitions. Estimation was by the quantreg package for R (Koenker 2006), which does not report goodness of fit for quantile regressions. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

Table 6.3. Returns to 'some post-secondary' education

	Males		Females	
	Estimate	Impl. step	Estimate	Impl. step
	(1)	(2)	(3)	(4)
Experience	0.0389		0.0638	
	[3.400]		[6.591]	
Experience squared	0.0042		-0.0013	
	[3.108]		[1.000]	
Test score	0.0144		0.0192	
	[2.229]		[3.570]	
s = 10	0.0105		-0.0112	
	[0.262]		[0.149]	
s = 11	0.0478	0.0373	0.0009	0.0121
	[1.186]	[1.588]	[0.012]	[0.455]
s = 12	0.0529	0.0051	0.0506	0.0497
	[1.026]	[0.130]	[0.596]	[1.123]
s = 13	0.0666	0.0137	-0.0016	-0.0522
	[0.998]	[0.333]	[0.018]	[1.782]
s = 14	0.0483	-0.0183	0.0335	0.0351
	[0.702]	[0.544]	[0.382]	[1.533]
s = 15	0.0792	0.0308	0.0752	0.0417
	[1.136]	[0.846]	[0.847]	[1.806]
s = 16	0.0555	-0.0236	0.1200	0.0448
	[0.771]	[0.928]	[1.345]	[2.660]
s ≥ 17	0.0976	0.0420	0.1685	0.0485
	[1.283]	[1.415]	[1.849]	[2.069]
Completed HS certificate	0.0042		-0.0226	
	[0.117]		[0.533]	
Some post-secondary education	0.1215		0.1234	
	[3.318]		[5.129]	
Completed diploma	0.1522	0.0307	0.1522	0.0288
	[3.547]	[1.139]	[4.787]	[1.410]
Completed degree	0.2496	0.1281	0.2161	0.0927
	[4.972]	[3.684]	[6.556]	[4.026]
N	4757		4341	
R-squared	0.1898		0.2645	
p value for zero year effects	0.5889		0.0002	
p value for zero credential effects	0.0001		0.0000	

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

Table 6.4. Returns to credentials by hiring channels

	Panel 1: Males				Panel 2: Females			
	Direct contact		No direct contact		Direct contact		No direct contact	
	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step	Estimate	Impl. step
Experience	0.0254		0.0336		0.0495		0.0550	
	[1.326]		[2.272]		[2.679]		[4.942]	
Experience squared	0.0045		0.0052		-0.0008		-0.0004	
	[2.107]		[2.912]		[0.320]		[0.296]	
Test score	0.0132		0.0173		0.0137		0.0257	
	[1.365]		[2.112]		[1.421]		[4.012]	
s = 10	-0.0127		0.0041		0.0447		-0.0356	
	[0.188]		[0.084]		[0.292]		[0.397]	
s = 11	0.0148	0.0275	0.0510	0.0469	0.0857	0.0410	-0.0428	-0.0072
	[0.214]	[0.803]	[1.050]	[1.532]	[0.552]	[0.806]	[0.475]	[0.246]
s = 12	-0.0643	-0.0791	0.0676	0.0166	0.1373	0.0516	-0.0504	-0.0076
	[0.726]	[1.244]	[1.078]	[0.332]	[0.821]	[0.671]	[0.481]	[0.129]
s = 13	0.0969	0.1612	0.1559	0.0884	0.1529	0.0156	0.0106	0.0610
	[0.952]	[3.953]	[2.192]	[2.850]	[0.878]	[0.343]	[0.100]	[2.830]
s = 14	0.0931	-0.0039	0.1161	-0.0399	0.2254	0.0725	0.0274	0.0168
	[0.841]	[0.073]	[1.567]	[0.949]	[1.337]	[1.527]	[0.255]	[0.631]
s = 15	0.1095	0.0164	0.1629	0.0468	0.2987	0.0732	0.0450	0.0176
	[1.064]	[0.324]	[2.100]	[0.953]	[1.728]	[1.851]	[0.414]	[0.629]
s = 16	0.0514	-0.0581	0.1444	-0.0185	0.3351	0.0364	0.0915	0.0465
	[0.484]	[1.506]	[1.719]	[0.561]	[1.917]	[1.097]	[0.839]	[2.424]
s ≥ 17	0.0939	0.0425	0.1856	0.0412	0.4374	0.1023	0.1186	0.0271
	[0.839]	[0.927]	[2.095]	[1.077]	[2.425]	[2.121]	[1.066]	[1.008]
Completed HS cert	0.0533		0.0185		-0.0589		0.0615	
	[0.907]		[0.410]		[0.852]		[1.081]	
Completed diploma	0.0196		0.0688		-0.0044		0.0599	
	[0.424]		[2.139]		[0.110]		[2.473]	
Completed degree	0.1268		0.1515		0.0066		0.1280	
	[3.229]		[2.863]		[0.183]		[4.682]	
N	1875		2882		1263		3078	
R-squared	0.1828		0.2130		0.2158		0.2861	
p value for years	0.0014		0.0611		0.0001		0.0005	
p value for credentials	0.0075		0.0182		0.8448		0.0000	

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

CHAPTER 7

EMPLOYER LEARNING IN THE LABOUR MARKET FOR YOUNG AUSTRALIANS

As discussed in chapter 2, a critical distinction between the human capital and sorting theories of education is that the latter emphasises the role of worker characteristics (loosely described by the term ‘ability’) that are not initially observed by employers, and which may be correlated with years of education. It is on account of such correlations that education has informational value to employers, over and above its direct contribution to productivity. However, while it is plausible that employers might not initially observe all the characteristics of workers that contribute to productivity, it also seems likely that their information will improve with experience – and that this in turn will be reflected in wages. It is for this reason that evidence on how the returns to education and ability evolve with experience can shed light on the debate over the sorting hypothesis – and on the importance of incomplete information in the labour market more generally.

A modern test for employer learning has been proposed by Altonji and Pierret (2001), and was outlined in chapter 3. According to their model, when the coefficients on both a measure of ability as observed by the researcher (z) and education (s) are permitted to vary with experience in the wage equation, the coefficient on z is predicted to increase while the one on s will decrease. The reason is that z becomes increasingly correlated with the information gained by employers with experience, while the coefficients on s and z are linked through the correlation between these variables upon which sorting models are premised. As employers learn more about harder-to-observe aspects of ability, they will attach less weight to easy-to-observe variables such as education, which are valued in part because they are positively correlated with ability and can thus be used as the basis for statistical discrimination.

In this chapter, Altonji and Pierret's test of employer learning with statistical discrimination is implemented using data from the 1995 Year 9 cohort of the Longitudinal Surveys of Australian Youth (LSAY) as described in chapter 4.

Measures of ability

The LSAY dataset contains two candidate variables for measures of ability that are observed by the researcher but not initially by employers. The first is the score from the mathematics and reading tests administered to the students when they were in Year 9. The second is parental education as reported by the respondent when they were in Year 9. The test score is expressed in standard deviations from the mean for the full sample in 1995, while parental education has been recoded from a categorical variable into year equivalents and then averaged where it is available for both parents. Full definitions of the two z-variables, as well as the s-variable (completed years of education) are provided in chapter 4. It should be noted that all respondents are at the same level of schooling at the time of taking the test, which is in contrast to the AFQT score reported for the NLSY sample which has been extensively studied in US research on the employer learning model.

Table 7.1 reports correlations between years of education (s) and the z-variables, separately for males and females. These are reported on the basis that each individual who contributes at least one observation to the wage sample contributes exactly one observation to the correlation coefficients. All correlations are positive and highly significant in two-tailed tests, although they are also modest in magnitude. For both males and females, years of education is more highly correlated with the test score than it is with parental education. The correlation between the two z-variables is 0.247 for males, and 0.211 for females.

Approximately 2% of respondents have missing values for the test score. Unfortunately, given that the questions on parental education were only asked in the first year of the survey – when the respondents were aged 14 – parental education is missing for 23% of respondents. Moreover, as reported in table 7.2, these are not a random subset of the sample. Respondents with missing parental education also have lower mean values for both their own years of education and the test score. These differences are larger for females than for males. They are also more pronounced for the test score (which was collected at the same time as parental education) than it is for years of education. (However it should be noted that the test score and parental education do not both appear together in the same models below.)

Baseline specification and estimation

The baseline specification for the models reported in this chapter is a regression of the log gross hourly wage rate on an intercept, years of education, and a full set of dummy variables for one through to eight or more years of experience. This model of the experience profile allows maximal freedom for the variation in wages to be explained by experience, before the interactions of education and measured ability with experience are added to the model. As discussed in chapter 3, an alternative explanation for why the return to measured ability may increase with experience is that z (and s) may be positively correlated with the receipt of on-the-job training. For this reason, a control for on-the-job training is included in all of the models reported in this chapter. This takes the form of a simple dummy variable indicating that the respondent reported receiving formal job-related training during the interview year of the wage observation. The models differ in respect of which one of the two z-variables is included, and whether the s and z-variables are interacted with experience. In other words, they correspond to estimates of equation 3.1 in chapter 3, with different restrictions placed upon the coefficients β_{sx}, β_z, and β_{zx}.

Missing values of the *z*-variables are dealt with by simply dropping them from the applicable models. An alternative approach, as adopted in the paper by Altonji and Pierret (2001), is to include these observations identified by a dummy variable, and to also interact the missing value dummy with experience whenever the *z*-variable is itself interacted with experience. In Altonji and Pierret's sample, the father's education is missing in 12% of the observations, while the sibling's wage is missing for 49%. The effect of including these observations is simply to enable them to contribute toward the estimates of the coefficients on the remaining explanatory variables. In additional work not reported here, it was confirmed that doing so did not affect the central conclusions regarding the effect of the *s* and *z*-variables.

In addition to the variables of central interest described above, all models include 8 occupation dummies (plus a dummy for missing occupation), 16 industry dummies (plus one for missing industry), dummies for Aboriginal or Torres Strait Islander status and language other than English (both as reported by the respondent in 1995), and a dummy to indicate that the respondent was either married or living with a partner in the applicable interview year.

The models are estimated by pooled OLS, with inference based upon clustered standard errors that are robust to arbitrary heteroskedasticity and to serial correlation at the level of the individual (Arrelano 1987; Wooldridge 2002, chapter 7). In the final section of the chapter, which reports estimates of quantile regressions, standard errors are obtained by bootstrapping with 200 repetitions. All statistics and analysis in the chapter make use of unweighted data.

Results

Table 7.3 reports coefficient estimates of the key variables of interest for various specifications of the wage equation and for the two candidate *z*-variables. Results for males are shown in panel 1, while results for females are in panel 2.

Specification 1 (column 1): s only

Column 1 reports a baseline Mincer earnings equation containing years of education alone, without any measure of ability or interactions of s or z with experience. This differs from the corresponding model in chapter 5 in terms of the specification of the experience profile, as well as the inclusion of on-the-job training. In addition, the models in chapter 5 all included the test score, and omitted respondents whose high school credential status was missing. For males, the estimated private rate of return to education is 3.8%, compared to 4.1% in chapter 5. For females it is 5.1%, which is exactly the same as it was in chapter 5. The return to years of education is higher for females than for males, while the return to on-the-job training is higher for males than it is for females (6.6% compared to 4.7%).

Coefficient estimates for the remaining variables are not of primary interest and have not been reported in the main table. These are also very similar to the ones reported in chapter 5, and will therefore not be discussed again. However, complete results for the models reported in columns 1 and 8 of table 7.3 are presented in appendix 7.1.

Specification 2 (columns 2 and 3): s and z

Columns 2 and 3 add the measured ability variables, but do not include any interactions of s or z with experience. For males, these estimates confirm a significant positive association of each of the z-variables with wages, even after controlling for years of education. In column 2 a one standard deviation increase in the test score is associated with a 1.5% increase in wages, while in column 3 each additional year of parental education is associated with a 0.7% increase. For females, a one standard deviation increase in the test score is associated with a 2.1% increase in wages, while the effect of parental education is positive but not statistically significant.

*Specification 3 (column 4): s and s*x, and*

*Specification 4 (columns 5 and 6): s, z and s*x*

Columns 4 through 6 add the linear interaction of years of education with experience, where the effect of the *z*-variables is *not* permitted to vary with experience. The key result here is that for both males and females, and in each of these models, there is little evidence of any significant variation in the effect of education on wages with experience. This finding is consistent with proposition 3.1 in chapter 3 and the arguments advanced by Riley (1979) and Farber and Gibbons (1996) as discussed in chapters 2 and 3. Interacting *s* with experience also leaves the coefficients on the two *z*-variables essentially unchanged in columns 5 and 6, when compared to the corresponding estimates in columns 2 and 3.

*Specification 5 (columns 7 and 8): s, z, s*x and z*x*

Finally, columns 7 and 8 add the interaction of the *z*-variables with experience. For both males and females, there is little evidence that the return to the test score variable increases with experience, and thus little support for employer learning on the basis of this variable. Moreover, the point estimate of the 'main' effect of the test score (which is now interpreted as its effect at zero experience) is of comparable magnitude to the corresponding coefficients in columns 2 and 5, although the precision of the estimate is not as great as before. As noted in the discussion of proposition 3.3 in chapter 3, a positive coefficient on the *z*-variable at zero experience will arise when *z* is positively correlated with the variable *q*, which captures the effect of information observed by employers but not the researcher. In other words, while the estimates in columns 2 and 5 confirm that the test score indeed captures information about ability that is valued by employers, the estimate in column 7 suggests that this information is largely already observed by employers at the time of labour market entry.

Turning to the estimates in column 8, there is a significant increasing effect of parental education with experience for the males. Moreover, when the interaction of parental education with experience is added to the model, the significant 'main' effect of this variable that was found for males in columns 3 and 6 disappears. (The point estimate of the effect of parental education at zero experience is actually negative in column 8, but it is not statistically significant). Thus wages are initially not very correlated with parental education, but they become increasingly positively correlated as experience accumulates. This is consistent with the employer learning model, with parental education taken as the measure of ability not initially observed by employers. For each additional year of parental education, wages increase by an extra 0.35% with each year of labour market experience. In other words, the wages of a male whose parents both have 12 years of education increase at a rate of slightly over 1% a year faster than one whose parents have only 9 years of education.

Further, in a result emphasised by Altonji and Pierret (2001), when the interaction of parental education with experience is added to the model, there is a fall in the coefficient on education interacted with experience between column 6 and column 8. In particular, as predicted by their model, the interaction of education with experience is negative in column 8 (albeit not significantly different from zero). By contrast, adding the interaction of the test score with experience had almost no effect on the interaction of education with experience in column 7. It is also interesting to observe that the estimates of the 'main' coefficient on education are almost identical in columns 1 and 8. Taken as a whole, these results for males are highly supportive of the employer learning model when parental education is used as the z-variable.

In the case of females, there is a similar finding of a positive interaction of parental education with experience, although this result is only marginally significant with a p-value of 0.073. However, given the failure to find a significant 'main' effect of parental education for females in columns 3 and 6, it is precarious to attach an employer learning interpretation to this result. Nonetheless, one possible explanation of why the result is weaker for females is that they have less experience on average. Thus it is possible that with further experience the effects in columns 3 and 6 could become significant, and the employer learning hypothesis might then also be more convincingly supported in column 8. Obviously, this conjecture cannot be tested until more data becomes available as the cohort matures.

Tests of corollary 3.2

Corollary 3.2 in chapter 3 states that the negative of φ, the coefficient of the regression of z on s, multiplied by the coefficient on the interaction of z with experience, should equal the coefficient on the interaction of s with experience:

$$\beta_{sx} = -\varphi\beta_{zx}$$

For males, and taking z to be parental education, the coefficient of the regression of z on years of education is 0.2212. In column 8, the coefficient on the interaction of parental education with experience is 0.0035. The negative of the product of these is –0.0008, while the coefficient on years of education interacted with experience is –0.0016. The resulting Wald test statistic is 0.2357, which fails to reject the coefficient restriction implied by the model, with a p-value of 0.6273. For females, the coefficient in the auxiliary regression is 0.2412 and the Wald test statistic is 0.3412, which has a p-value of 0.5592. These tests confirm that the relative magnitudes of the interactions of education and measured ability with experience are broadly consistent with the employer learning model.

Experience profiles of log wages

As discussed above, in each of the models the experience profile of earnings is modelled by a full set of dummy variables representing one through to eight or more years of experience. These profiles are shown in figure 7.1 for the models in column 3 of table 5.3 (which includes education and parental education, but no experience interactions), column 6 (which adds the interaction of education with experience, and column 8 (which also includes the interaction of parental education with experience). These profiles show that as experience is permitted to enter the model in a progressively richer manner with the inclusion of more interaction terms, the direct effect of experience as shown by these profiles becomes progressively flatter.

Returns to mathematics and reading ability

The results in table 7.3 indicate that while both *z*-variables capture aspects of ability that are valued by employers, there is only evidence of employer learning in relation to parental education. The negative result for the test score is striking in light of the findings of similar tests that have been conducted in other countries. These typically find strong learning effects when a test score is used as the *z*-variable (Farber and Gibbons 1996, Altonji and Pierret 2001, Galindo-Rueda 2003, Lange 2005) and weaker results when parental education is considered (Altonji and Pierret 2001, Bauer and Haisken-DeNew 2001). The results in table 7.4 revisit this question by decomposing the test score into its separate mathematics and reading components, and treating each of these as separate *z*-variables. Columns 1 through 3 report models in which these variables are not interacted with experience; their interactions then enter the models in columns 4 through 6. The results indicate that for males it is only ability in mathematics that is strongly associated with wages, whereas for females the effects of mathematics and reading ability are of roughly equal strength. While this raises interesting questions in its own right, the results in columns 4 through 6 indicate no significant learning effects for either of these ability measures, for males or females.

Results of quantile regressions

The results reported thus far indicate that there is some support for the hypothesis of employer learning on the basis of parental education – but not the test score – and that this evidence is more compelling for males than it is for females. To investigate whether these conclusions hold generally, or if they vary across the wage distribution, results of quantile regressions are reported in table 7.5. These are reported at the 25th, 50th and 75th percentiles, for the models corresponding to columns 5 through 8 of table 7.3. These models correspond to specifications 4 and 5 as discussed above (i.e. before and after the ability variable is interacted with experience) for the test score variable and parental education respectively.

Looking firstly at the results in which measured ability is not interacted with experience, there is a significant positive association of the test score with wages for both males and females at every quartile. Consistent with table 7.3, this is always larger for females than for males; however there is no consistent pattern to the size of the effects across quartiles. Turning to parental education, there is no significant return to this variable for females at any quartile, a finding that is also consistent with table 7.3. For males, the coefficient on parental education increases moving up the wage distribution. This coefficient is positive and significant at the 75th percentile, and marginally significant at the median (*p*-value of 0.062). The estimate at the 25th percentile is very similar to the one at the median, but does not attain conventional levels of significance.

When the return to the test score is allowed to vary with experience, there is no significant interaction found for males or females at any of the quartiles. This is also consistent with table 7.3, and confirms that there is little evidence of employer learning on the basis of the test score. Finally, when parental education is considered as the measure of ability, its interaction with experience is positive and significant for males at the median of the wage distribution, and marginally significant at the 25th percentile (*p*-value of 0.087). These results are supportive of employer learning on the basis of parental education for males, but they qualify the result in table 7.3 in that the predictions of the model are only fully confirmed at the median. For females, there is no significant interaction of parental education with experience at any of the quartiles. This contrasts with the marginally significant effect reported in table 7.3. However it is consistent with the discussion of that earlier result, in which it was argued that it may be premature to infer a learning conclusion for the females.

Thus in short, the results of the quantile regressions confirm that there is only evidence of employer learning on the basis of parental education – and not for the test score – and that this is only seen to hold for males. Moreover, they indicate that these learning effects are only present at the lower to middle regions of the wage distribution.

Conclusion

The results in this chapter provide support for the employer learning model of Altonji and Pierret (2001), at least among males. Using parental education as a proxy for ability not initially observed by the employer, this variable is found to have an increasingly strong association with wages as experience in the labour market accumulates. A similar but less compelling result is also observed for females. However, when a standardised test score is used as the measure of ability, there is no evidence of employer learning in relation to this variable for either males or females. This appears to be because the information contained in the test score is already observed by employers at the time of labour market entry. In other words, both *z*-variables capture aspects of ability that are valued by employers (at least for males), however the information contained in the parental education variable is initially more difficult for employers to observe. Nonetheless (again, at least for males) there is evidence that employers' information does indeed improve with experience.

It is worth noting that these results are robust to the inclusion of a control for on-the-job training, since this provides an alternative explanation as to why the return to measured ability should increase with experience. The results also provide an interesting contrast with previous studies that have been undertaken in other countries, which usually find stronger evidence for employer learning when using test scores – whereas results using parental education have typically been mixed.

Although results such as the ones reported here are sometimes interpreted as vindicating a sorting view of education over the human capital view, such an interpretation should be treated with caution for two reasons. Firstly, for learning to take place, it is only necessary that there be attributes of workers that are not initially observed by employers. It is not strictly necessary that these attributes be correlated with education – as is a prerequisite for sorting theories to hold. Indeed, the estimates reported here provide stronger evidence for increasing returns to an ability variable than they do for a declining effect of years of education – the results reported in this chapter do not generally display a significant negative coefficient on education interacted with experience. That is, they provide more support for the hypothesis of employer learning in relation to ability than they do for the counterpart hypothesis of statistical discrimination on the basis of education.

Secondly, to the extent that sorting takes place, the finding of employer learning implies that the effect of this sorting will ultimately be undone – and that wages will eventually converge to actual productivity. If this is the case then the effects of sorting will merely be transitory, and this implies some bound to the resulting distortion of the private return to education.

While the data source used in this chapter is not yet extensive enough to permit this bound to be estimated, some recent US evidence is provided by Lange (2005), who argues that sorting contributes at most 25 per cent of the return to education.

The next chapter undertakes further exploration of the pattern of employer learning, by examining whether learning effects differ by occupational status and hiring channels, and by exploring whether learning is a function of experience or of tenure.

Figure 7.1. Experience profiles of log wages

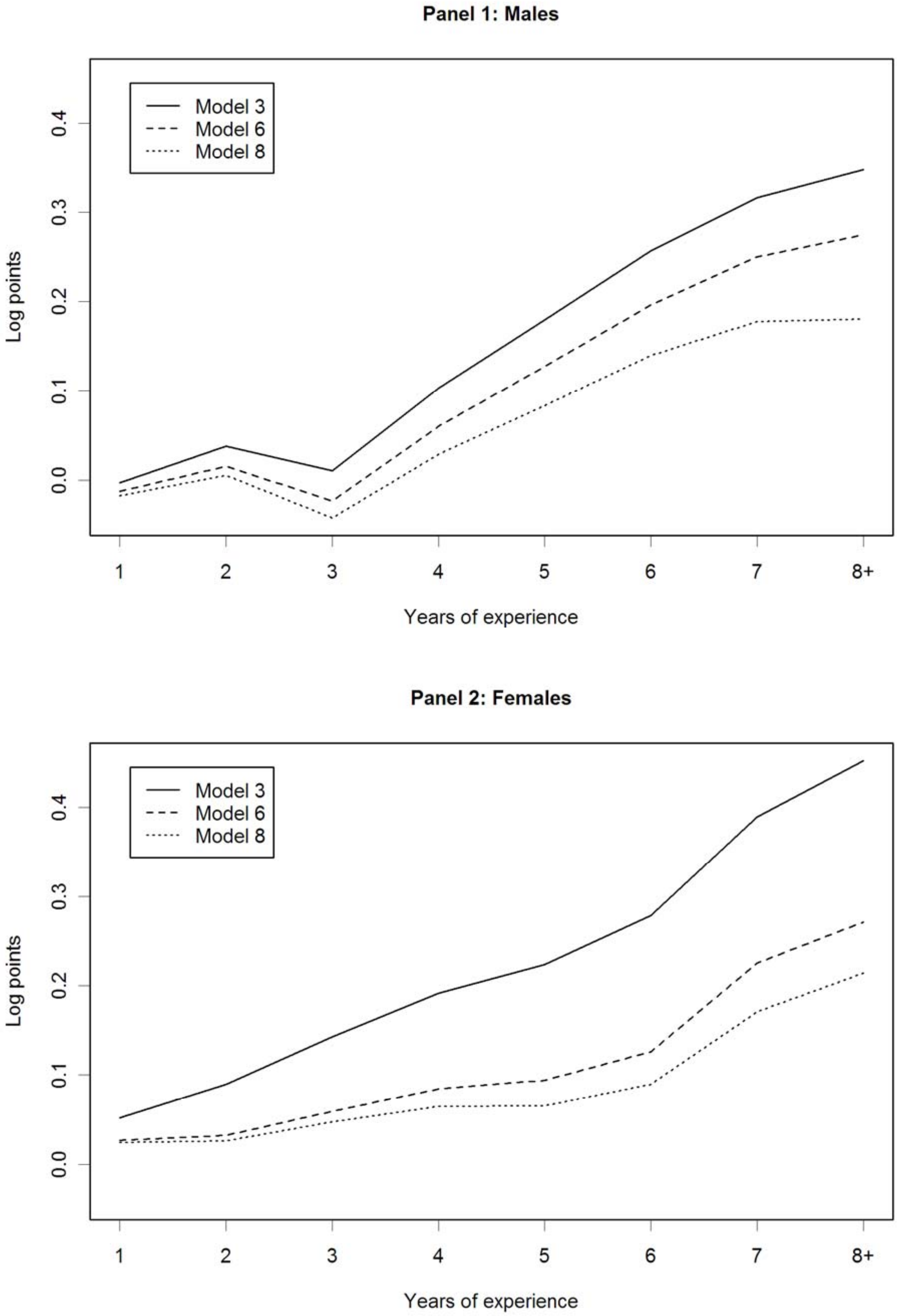


Table 7.1. Correlations of s and z variables

Panel 1: Males

		Education	Test score	Parental education
Years of education	Pearson correlation	1	0.381	0.247
	N	2,094	2,056	1,578
Test score	Pearson correlation	0.381	1	0.239
	N	2,056	2,056	1,552
Parental education	Pearson correlation	0.247	0.239	1
	N	1,578	1,552	1,578

Panel 2: Females

		Education	Test score	Parental education
Years of education	Pearson correlation	1	0.356	0.258
	N	2,128	2,081	1,671
Test score	Pearson correlation	0.356	1	0.211
	N	2,081	2,081	1,647
Parental education	Pearson correlation	0.258	0.211	1
	N	1,671	1,647	1,671

Table 7.2. Differences in means by parental education

Panel 1: Males

	Valid parental education		Missing parental ed.		Difference		
	Mean	SD	Mean	SD	Mean	SE	t
Years of education	12.777	2.120	12.409	1.973	0.368	0.102	3.610
Test score	0.187	0.944	-0.101	1.006	0.289	0.051	5.682

Panel 2: Females

	Valid parental education		Missing parental ed.		Difference		
	Mean	SD	Mean	SD	Mean	SE	t
Years of education	13.570	2.051	12.952	1.952	0.618	0.104	5.937
Test score	0.248	0.832	-0.103	0.957	0.351	0.050	6.983

Notes: Equal variances are not assumed in computing the t-ratios.

Table 7.3. Returns to education and measured ability, by experience

Panel 1: Males								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0379	0.0359	0.0315	0.0353	0.0342	0.0294	0.0344	0.0371
	[8.184]	[7.624]	[5.755]	[6.079]	[5.794]	[4.348]	[5.751]	[4.662]
Test score		0.0146			0.0147		0.0128	
		[2.327]			[2.343]		[1.005]	
Parental education			0.0068			0.0068		-0.0052
			[2.034]			[2.035]		[0.836]
Education * experience				0.0010	0.0007	0.0008	0.0006	-0.0016
				[0.721]	[0.463]	[0.525]	[0.439]	[0.824]
Test score * experience							0.0005	
							[0.160]	
Parental edu. * experience								0.0035
								[2.197]
On-the-job training	0.0635	0.0588	0.0553	0.0634	0.0587	0.0550	0.0587	0.0549
	[6.425]	[5.947]	[4.953]	[6.407]	[5.935]	[4.926]	[5.932]	[4.922]
N	4782	4706	3585	4782	4706	3585	4706	3585
R-squared	0.1856	0.1896	0.1842	0.1857	0.1897	0.1843	0.1897	0.1858
Panel 2: Females								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0513	0.0486	0.0515	0.0480	0.0449	0.0474	0.0445	0.0522
	[13.834]	[12.791]	[12.338]	[11.024]	[10.013]	[10.065]	[9.833]	[9.553]
Test score		0.0214			0.0217		0.0277	
		[4.018]			[4.053]		[2.636]	
Parental education			0.0012			0.0012		-0.0060
			[0.420]			[0.422]		[1.240]
Education * experience				0.0016	0.0018	0.0021	0.0019	0.0002
				[1.416]	[1.552]	[1.667]	[1.608]	[0.137]
Test score * experience							-0.0019	
							[0.650]	
Parental edu. * experience								0.0026
								[1.793]
On-the-job training	0.0455	0.0457	0.0389	0.0451	0.0453	0.0384	0.0453	0.0389
	[5.458]	[5.460]	[4.204]	[5.407]	[5.397]	[4.151]	[5.400]	[4.196]
N	4359	4271	3433	4359	4271	3433	4271	3433
R-squared	0.2503	0.2564	0.2517	0.2508	0.2569	0.2524	0.2570	0.2534

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. The experience profile of wages is controlled for with a set of dummy variables for 1 through to 8 or more years of actual experience. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner. Full estimates for the models in columns 1 and 8 are reported in Appendix 7.1.

Table 7.4. Returns to mathematics and reading ability, by experience

Panel 1: Males	(1)	(2)	(3)	(4)	(5)	(6)
Education	0.0342	0.0355	0.0341	0.0343	0.0357	0.0342
	[5.782]	[6.055]	[5.758]	[5.717]	[6.082]	[5.699]
Mathematics score	0.0160		0.0150	0.0146		0.0151
	[2.541]		[2.099]	[1.203]		[1.143]
Reading score		0.0091	0.0022		0.0050	-0.0007
		[1.518]	[0.326]		[0.410]	[0.053]
Education * experience	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006
	[0.432]	[0.416]	[0.446]	[0.411]	[0.385]	[0.424]
Mathematics score * experience				0.0004		-0.0001
				[0.129]		[0.019]
Reading score * experience					0.0011	0.0008
					[0.362]	[0.228]
On-the-job training	0.0592	0.0587	0.0591	0.0592	0.0587	0.0591
	[5.983]	[5.925]	[5.971]	[5.981]	[5.927]	[5.970]
N	4706	4706	4706	4706	4706	4706
R-squared	0.1900	0.1887	0.1901	0.1900	0.1887	0.1901
Panel 2: Females						
	(1)	(2)	(3)	(4)	(5)	(6)
Education	0.0453	0.0461	0.0449	0.0451	0.0458	0.0445
	[10.016]	[10.403]	[9.953]	[9.871]	[10.314]	[9.804]
Mathematics score	0.0173		0.0119	0.0217		0.0159
	[3.330]		[1.950]	[2.206]		[1.439]
Reading score		0.0182	0.0128		0.0232	0.0155
		[3.298]	[1.993]		[2.200]	[1.312]
Education * experience	0.0018	0.0018	0.0018	0.0018	0.0018	0.0019
	[1.506]	[1.520]	[1.551]	[1.532]	[1.573]	[1.606]
Mathematics score * experience				-0.0015		-0.0014
				[0.529]		[0.433]
Reading score * experience					-0.0016	-0.0008
					[0.525]	[0.236]
On-the-job training	0.0454	0.0458	0.0453	0.0453	0.0459	0.0452
	[5.406]	[5.455]	[5.398]	[5.405]	[5.462]	[5.403]
N	4271	4271	4271	4271	4271	4271
R-squared	0.2558	0.2559	0.2569	0.2558	0.2559	0.2570

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. The experience profile of wages is controlled for with a set of dummy variables for 1 through to 8 or more years of actual experience. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

Table 7.5. Quantile regressions of returns to ability with experience

	Panel 1: Males											
	25th percentile				50th percentile				75th percentile			
Education	0.0337	0.0304	0.0333	0.0353	0.0391	0.0300	0.0393	0.0377	0.0315	0.0322	0.0323	0.0344
	[5.296]	[4.579]	[5.269]	[4.634]	[7.311]	[4.408]	[7.136]	[4.838]	[5.897]	[4.759]	[6.539]	[4.697]
Test score	0.0140		0.0260		0.0114		0.0080		0.0161		0.0040	
	[2.465]		[1.862]		[2.056]		[0.719]		[2.665]		[0.292]	
Parental education		0.0057		-0.0046		0.0058		-0.0031		0.0073		-0.0005
		[1.543]		[0.590]		[1.868]		[0.660]		[2.010]		[0.065]
Education * experience	0.0013	0.0023	0.0013	0.0003	-0.0025	-0.0006	-0.0025	-0.0029	0.0005	-0.0010	0.0003	-0.0024
	[0.832]	[1.289]	[0.841]	[0.121]	[1.831]	[0.384]	[1.827]	[1.543]	[0.360]	[0.601]	[0.207]	[1.167]
Test score * experience			-0.0032				0.0009				0.0030	
			[1.069]				[0.306]				[0.913]	
Parental edu. * experience				0.0031				0.0027				0.0023
				[1.712]				[2.068]				[1.319]
On-the-job training	0.0353	0.0336	0.0379	0.0338	0.0518	0.0569	0.0529	0.0560	0.0564	0.0591	0.0594	0.0538
	[3.084]	[2.812]	[3.264]	[2.872]	[5.875]	[4.943]	[5.871]	[4.837]	[4.639]	[3.670]	[4.917]	[3.387]
	Panel 2: Females											
	25th percentile				50th percentile				75th percentile			
Education	0.0465	0.0494	0.0474	0.0494	0.0460	0.0457	0.0465	0.0501	0.0430	0.0436	0.0435	0.0479
	[8.394]	[8.446]	[8.508]	[7.280]	[9.699]	[9.541]	[9.699]	[9.151]	[7.328]	[7.509]	[7.288]	[6.851]
Test score	0.0278		0.0414		0.0175		0.0185		0.0182		0.0146	
	[4.763]		[3.592]		[3.702]		[1.933]		[3.357]		[1.358]	
Parental education		0.0019		0.0013		-0.0001		-0.0052		0.0015		-0.0040
		[0.629]		[0.225]		[0.032]		[0.977]		[0.540]		[0.728]
Education * experience	0.0021	0.0023	0.0022	0.0022	0.0013	0.0014	0.0011	0.0002	0.0003	0.0001	0.0003	-0.0016
	[1.514]	[1.446]	[1.508]	[1.185]	[1.141]	[1.294]	[1.022]	[0.101]	[0.297]	[0.056]	[0.236]	[0.816]
Test score * experience			-0.0042				-0.0002				0.0009	
			[1.420]				[0.096]				[0.311]	
Parental edu. * experience				0.0002				0.0018				0.0024
				[0.107]				[1.239]				[1.394]
On-the-job training	0.0451	0.0378	0.0461	0.0379	0.0373	0.0312	0.0366	0.0306	0.0360	0.0391	0.0358	0.0384
	[4.718]	[3.360]	[4.888]	[3.291]	[4.747]	[3.491]	[4.691]	[3.332]	[3.706]	[3.410]	[3.613]	[3.443]

Notes: The dependent variable is log real hourly wages in 2001 dollars. The numbers in square brackets are absolute values of the t-statistics, obtained by bootstrapping with 200 repetitions. Estimation was by the quantreg package for R (Koenker 2006), which does not report goodness of fit for quantile regressions. The experience profile of wages is controlled for with a set of dummy variables for 1 through to 8 or more years of actual experience. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

Appendix 7.1. Returns to education and ability, by experience: Full results

	Panel 1: Males		Panel 2: Females	
	(1)	(8)	(1)	(8)
Intercept	2.0066	2.1136	1.7202	1.7812
	[27.131]	[20.228]	[30.287]	[23.739]
Aboriginal or Torres Strait Islander	0.0017	-0.0044	0.0188	0.0545
	[0.036]	[0.075]	[0.506]	[1.422]
Language other than English	0.0470	0.0504	0.0261	0.0212
	[2.002]	[2.054]	[1.421]	[1.003]
Married or living with partner	0.0483	0.0344	0.0298	0.0145
	[3.473]	[2.156]	[3.350]	[1.463]
Industry: Agriculture	-0.0710	-0.0736	-0.0398	-0.0595
	[2.315]	[2.036]	[0.997]	[1.298]
Industry: Mining	0.3969	0.4105	0.1683	0.1434
	[6.870]	[5.386]	[2.959]	[2.028]
Industry: Manufacturing	0.0865	0.0874	0.0436	0.0352
	[4.674]	[3.895]	[2.195]	[1.543]
Industry: Electricity	0.1674	0.1619	0.1664	0.1158
	[2.631]	[2.547]	[4.157]	[2.413]
Industry: Construction	0.1103	0.0837	0.0875	0.0924
	[5.131]	[3.467]	[3.130]	[2.908]
Industry: Wholesale	-0.0104	-0.0378	0.0517	0.0580
	[0.450]	[1.380]	[1.825]	[1.806]
Industry: Accommodation	-0.0209	-0.0534	0.0529	0.0369
	[1.024]	[2.214]	[3.057]	[1.879]
Industry: Transport	0.0750	0.0750	0.1131	0.0944
	[2.028]	[1.733]	[3.521]	[2.666]
Industry: Communication	0.1184	0.1163	0.1976	0.2296
	[3.020]	[2.699]	[5.467]	[5.994]
Industry: Finance	0.0932	0.1208	0.1199	0.1195
	[3.627]	[4.455]	[5.425]	[4.918]
Industry: Property	0.0576	0.0442	0.0124	0.0080
	[3.167]	[2.209]	[0.828]	[0.467]
Industry: Government	0.0851	0.0966	0.1460	0.1510
	[2.903]	[3.250]	[6.436]	[6.394]
Industry: Education	0.0440	0.0137	0.1104	0.1092
	[1.519]	[0.407]	[5.242]	[4.638]
Industry: Health	0.0736	0.0764	0.0744	0.0903
	[2.200]	[1.938]	[4.420]	[4.787]
Industry: Cultural	0.0606	0.0346	0.0514	0.0052
	[1.333]	[0.808]	[2.449]	[0.233]
Industry: Personal	-0.0305	-0.0186	-0.0040	-0.0235
	[0.676]	[0.372]	[0.146]	[0.835]
Industry: Missing	0.1180	0.1226	0.0085	0.0118
	[3.489]	[2.913]	[0.307]	[0.387]
Occupation: Manager	-0.0186	-0.0433	0.0175	0.0251
	[0.559]	[1.148]	[0.689]	[0.961]
Occupation: Professional	0.0647	0.0675	0.1331	0.1285
	[3.034]	[2.728]	[9.554]	[8.387]
Occupation: Associate professional	0.0360	0.0195	0.0667	0.0628
	[1.995]	[0.965]	[5.379]	[4.508]
Occupation: Tradesperson	-0.0811	-0.0880	-0.0702	-0.0596
	[4.797]	[4.404]	[2.374]	[1.865]

Appendix 7.1. Returns to education and ability, by experience: Full results

	Panel 1: Males		Panel 2: Females	
	(1)	(8)	(1)	(8)
Occupation: Advanced clerical	0.0761	0.0675	0.0806	0.0707
	[2.189]	[1.694]	[4.769]	[4.031]
Occupation: Intermediate production	-0.0081	-0.0063	0.0296	0.0295
	[0.396]	[0.253]	[1.139]	[0.949]
Occupation: Elementary clerical	-0.0012	-0.0166	0.0089	0.0072
	[0.060]	[0.752]	[0.685]	[0.484]
Occupation: Labourer	-0.0646	-0.0664	-0.0610	-0.0467
	[3.261]	[2.793]	[2.040]	[1.201]
Occupation: Missing	0.1266	0.1258	0.0808	0.0641
	[1.899]	[1.043]	[0.788]	[0.624]
x = 1	0.0139	-0.0176	0.0510	0.0248
	[0.646]	[0.608]	[2.621]	[0.958]
x = 2	0.0516	0.0057	0.0858	0.0263
	[2.135]	[0.119]	[4.198]	[0.659]
x = 3	0.0323	-0.0421	0.1315	0.0478
	[1.244]	[0.610]	[6.090]	[0.878]
x = 4	0.1257	0.0293	0.1803	0.0647
	[4.575]	[0.340]	[7.920]	[0.937]
x = 5	0.2095	0.0834	0.2183	0.0655
	[7.334]	[0.808]	[9.199]	[0.801]
x = 6	0.2897	0.1396	0.2691	0.0897
	[9.572]	[1.156]	[10.656]	[0.937]
x = 7	0.3581	0.1778	0.3679	0.1707
	[10.080]	[1.314]	[10.367]	[1.553]
x ≥ 8	0.3946	0.1806	0.4192	0.2139
	[8.355]	[1.186]	[7.041]	[1.652]
On-the-job training	0.0635	0.0549	0.0455	0.0389
	[6.425]	[4.922]	[5.458]	[4.196]
Years of education	0.0379	0.0371	0.0513	0.0522
	[8.184]	[4.662]	[13.834]	[9.553]
Parental education		-0.0052		-0.0060
		[0.836]		[1.240]
Education * experience		-0.0016		0.0002
		[0.824]		[0.137]
Parental education * experience		0.0035		0.0026
		[2.197]		[1.793]
N	4782	3585	4359	3433
R-squared	0.1856	0.1858	0.2503	0.2534
Adj. R-squared	0.1789	0.1761	0.2436	0.2442

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level.

CHAPTER 8

FURTHER EVIDENCE ON THE PATTERN OF EMPLOYER LEARNING

In chapter 7, it was found that the wages of young Australians become increasingly correlated with parental education as their experience in the labour market accumulates. This result, which is consistent with the employer learning model of Altonji and Pierret (2001), is robust to controlling for on-the-job training, and is more compelling for males than it is for females. However, the same result was not found when a test score was used in place of parental education as the measure of ability.

The purpose of this chapter is to further explore the pattern of employer learning by considering several variations on the models reported in chapter 7. Firstly, following the lead of Bauer and Haisken-DeNew (2001) as well as Galindo-Rueda (2003), the models will be estimated separately for blue and white-collar workers, to examine whether employer learning effects differ by occupational status. Secondly, following the strategy in chapter 6 on credential effects, as well as Strobl (2004), the pattern of learning effects will be compared across groups of workers who were recruited through different hiring channels – which might be expected to convey differing amounts of initial information to employers. Finally, some preliminary results will be presented from a test of whether learning is a function of experience or tenure. These last results are tentative in that the members of the sample are still fairly young and have quite limited experience, such that their experience and tenure remain highly correlated.

Learning effects by occupational status

Table 8.1 reports results of estimating the employer learning model separately for blue and white-collar workers. The models in this table correspond to those in columns 5 through 8 of table 7.3 – or to specifications 4 and 5 as defined in chapters 3 and 7, with the test score and parental education used respectively as *z*-variables. The occupational groups classified as blue collar are tradespersons; production and transport workers; and labourers. These groups account for 51.6% of all person-year observations for males, but only 9.8% for females. The remaining groups classified as white collar are managers and administrators; professionals; associate professionals; and clerical, sales and service workers. Since the sample is partitioned on the basis of occupational categories, the dummy variables for occupation are not included in the models reported in table 8.1. Finally, a further 9 person-year observations have been excluded from the analysis because their occupation was missing.

Looking firstly at the results of the models in which the *z*-variables are not interacted with experience, these show that the test score has a significant positive association with wages in all groups except for white-collar males. Parental education has a marginally significant positive effect for blue-collar males (*p*-value of 0.070), but not for any other group.

When the *z*-variables are interacted with experience, there is once again no evidence of an increasing return to the test score in any of the subgroups. This is consistent with the findings reported in chapter 7. However in the case of parental education, a striking pattern emerges. For both males and females, there is a significant positive interaction of parental education with experience for white-collar workers, but not blue-collar workers. Moreover, both the magnitude and significance of these effects for white-collar workers are greater than in the corresponding models from table 7.3. Thus the central finding of chapter 7 – that there is evidence of employer learning on the basis of parental education for males, and possibly also females – is found to only hold for white-collar workers, but it is strengthened for this group.

There is one caveat, in that the ‘main’ effect of parental education when it is not interacted with experience is not significant for white-collar males or females in column 6 of table 8.1.

If parental education does not have a significant direct association with wages, then it might appear difficult to interpret a finding of increasing returns to this variable. However one possible explanation is that the white-collar workers in this sample have particularly limited work experience. It may thus be that they have simply not been in the workforce long enough for the value of the productive attributes captured by parental education to be fully reflected in their wages in column 6. At the same time, it is also during the early stages of the career that employer learning is likely to be most pronounced, as captured by the interaction term of parental education with experience in column 8. Some support for this hypothesis may be seen in the fact that the white-collar subgroups have less experience on average than the full samples considered in table 7.3. The mean experience of white-collar males is 3.05 years, compared to 3.62 years in the full sample. Bearing in mind that the overwhelming majority of females are employed in white-collar occupations, the mean experience for this group is 2.89 years, compared to 2.96 years in the full sample.

Having established that employer learning appears to be limited only to workers in white-collar occupations, there remains the question of how to interpret this result. As discussed in chapter 3, Bauer and Haisken-DeNew (2001) offered two explanations for why employer learning might be expected to be more important in *blue-collar* occupations. Firstly, they argue that productivity may be more difficult to measure in white-collar jobs. Secondly, they suggest that employers invest more heavily in screening white-collar employees, and therefore start out with more information about these workers at the time of hiring. As it turns out, Bauer and Haisken-DeNew find evidence of employer learning only amongst blue-collar workers. On the other hand, Galindo-Rueda (2003) only finds evidence of learning for white-collar workers, when estimating a specification similar to the one reported here.

In the present context, one possible reason for why employer learning is only found in white-collar occupations – and then only in relation to the parental education variable – is that parental education may reflect higher-level attributes (both cognitive and non-cognitive) than a simple test of mathematics and reading proficiency in Year 9. These attributes (which might include intellectual disposition, interpersonal communication skills and the like) may in turn be of greater value to employers in white-collar occupations.

Learning effects by hiring channel

The model of employer learning that is being tested here is premised on the notion that employers are not initially well-informed about worker attributes that contribute to productivity, but that their information improves with experience. However, as discussed in chapter 3, it is possible that other explanations may account for the increasing correlation of measured ability with wages. For example, better educated and more able workers may receive more on-the-job training, or they may be placed into 'fast track' positions with better opportunities for career advancement (Heisz and Oreopoulos 2003). In addition to including a control for the receipt of on-the-job training, one way to discriminate between these alternative explanations is to compare the experience profile of the returns to ability for workers who were recruited through different hiring channels.

Albrecht (1981) and Albrecht and van Ours (2005) argue that the information observed by employers may vary between hiring channels. In particular, they propose that employers may have better *a priori* information about workers when they were recommended by an existing employee (Albrecht 1981), or hired through an informal channel (Albrecht and van Ours 2005). If this is the case, and if the increasing returns to measured ability are due to employer learning, there should be a more pronounced interaction of experience with ability for those workers about whom employers are initially *less* well-informed, i.e. those who were *not* hired through a direct contact with their employer. On the other hand, if employers' information does not improve with experience as a result of learning, but more able workers enjoy access to superior training and promotion opportunities, then there should be a larger interaction of the *z*-variable with experience for workers about whom employers have *more* information. These observations may offer some leverage in determining whether the increasing returns to parental education are due to employer learning, or to some other explanation.

Table 8.2 reports estimates corresponding to the models in columns 5 through 8 of table 7.3, when the sample is partitioned according to whether or not the worker found their job through a direct contact with their employer. As discussed in chapter 4, a worker is defined as having found their job through a direct contact if they found their current job through a friend or relative, or because they were approached by their employer.

Consistent with the earlier findings, the results in table 8.2 indicate no evidence of employer learning on the basis of the test score in any subgroup. Turning to parental education, for males who found their job through a direct contact there is a strong 'main' effect of this variable in column 2, but no significant interaction effect in column 4. This suggests on one hand that parental education reflects aspects of ability that are valued by employers, but on the other hand that much of this information is already observed at the time of hiring for those with direct contacts. For males with no direct contact, there is a weaker 'main' effect of parental education in column 6, but a powerful interaction with experience in column 8. This indicates firstly that the return to parental education is not as fully realised in the no direct contact group, but also that it is increasingly realised with experience. This pattern of results for the males is thus very easy to interpret within an employer learning framework.

The results for females are, however, less encouraging. Firstly, consistent with the results in table 7.3, when parental education is not interacted with experience there is no significant ‘main’ effect for this variable in either subgroup. Secondly, there is a strong interaction effect of parental education with experience only for the direct contact group, and not in the no direct contact group. This is the opposite pattern to that which was found for the males. As a whole, these results for females are harder to reconcile with the employer learning model. If anything, they support the alternative view whereby more able workers in the direct contact group enjoy enhanced training and career advancement opportunities.

Learning effects by experience and tenure

In the model that has been considered thus far, it has been assumed that the information that is revealed about workers’ productivity with increasing experience is observed by all potential employers – or in other words, that employer learning is public or symmetric. However, a number of authors have recently suggested that incumbent employers may have superior information, such that learning may in fact be private or asymmetric (Bauer and Haisken-DeNew 2001, Galindo-Rueda 2003, Schönberg 2005).

To explore this possibility, table 8.3 reports results of estimates in which the *s* and *z*-variables are interacted with both total labour market experience and tenure in the current firm. These models correspond to equation 3.2 and specification 7 as defined in chapter 3. As discussed in chapter 3, in this specification the interaction of *z* with experience reflects information that is observed by all employers, while its interaction with tenure captures the effect of additional information observed by the incumbent firm. As before, the experience profile of log wages is modelled by a set of dummy variables for 1 through to 8 or more years of actual experience; these are now augmented by a set of dummies for 1 through to 7 or more years of tenure. The estimates in table 8.3 exclude 97 person-year observations whose tenure could not be computed, because the month in which they started their current job was missing.

In most respects, these estimates are consistent with the ones reported previously. In the models in which *z* is not interacted with experience or tenure, there is a significant positive coefficient on the test score for both males and females, and on parental education for males only. When the *z*-variables are interacted with experience and tenure, the estimates indicate that the return to the test score variable is fully realised at the time of labour market entry. Finally, there is a significant positive interaction of parental education with experience for both males and females.

Turning to the interactions of the *z*-variables with tenure, these turn out to be negative in all of the applicable models. Thus the sign of these coefficients is contrary to what might be expected if the interaction effects were due to employer learning – although they are also generally not statistically significant. However there is a marginally significant (*p*-value of 0.095) negative interaction of parental education with tenure for the males.

Thus these results do not support the suggestion that incumbent employers learn at a faster rate than outside firms. Instead, the employer learning that has been found in respect of the parental education variable appears to be symmetric, in that information is revealed publicly to all potential employers. However this finding is only tentative, given that the survey members are still fairly young and have quite limited experience (and thus also tenure). As a result, the experience and tenure variables remain quite highly correlated, making it difficult to disentangle their separate effects. For males the correlation between experience and tenure is 0.482, while for females it is 0.520.

Conclusion

The results reported in this chapter elaborate on the findings of chapter 7, by comparing the patterns of employer learning across a number of dimensions.

Firstly, for both males and females, the employer learning result with respect to parental education was shown to hold only among white-collar workers, and not for blue-collar workers. This runs counter to the hypotheses suggested by Bauer and Haisken-DeNew (2001). It was suggested that this result may be related to the types of attributes that are captured by using parental education as a measure of ability, which may be of greater value to employers in white-collar occupations.

Secondly, when the sample was split according to hiring channels, contrasting results were obtained for the males and females. For males, the finding of increasing returns to parental education was only obtained for those who found their job through a direct contact with their employer. Since this is the group about whom employers presumably have less information at the time of hiring, this finding is consistent with an employer learning interpretation. However for females, the opposite result was found – the interaction of parental education with experience is only significant for the direct contact group, about whom employers might be expected to have better information. If anything, this finding is more consistent with an alternative explanation according to which more able workers enjoy access to superior training and promotion opportunities.

Finally, when the employer learning effects were allowed to vary with both experience and tenure, it was found that they were in fact a function of experience alone. The interactions of measured ability with tenure are negative and generally insignificant, indicating that incumbent employers do not appear to observe better information than outsiders. However this finding should be regarded as preliminary, given the limited experience (and thus tenure) of the survey members.

Table 8.1. Returns to education, ability and experience, by occupational status

Panel 1: Males								
	Blue collar				*White collar*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0215	0.0133	0.0214	0.0148	0.0347	0.0325	0.0357	0.0414
	[2.085]	[1.126]	[2.072]	[1.023]	[5.267]	[4.526]	[5.358]	[5.166]
Test score	0.0205		0.0235		0.0083		-0.0017	
	[2.389]		[1.094]		[1.045]		[0.115]	
Parental education		0.0091		0.0070		0.0030		-0.0111
		[1.811]		[0.611]		[0.768]		[1.649]
Education * experience	0.0029	0.0033	0.0029	0.0029	0.0005	0.0006	0.0004	-0.0028
	[1.281]	[1.277]	[1.288]	[0.854]	[0.305]	[0.299]	[0.202]	[1.195]
Test score * experience			-0.0007				0.0031	
			[0.154]				[0.750]	
Parental education * experience				0.0006				0.0047
				[0.216]				[2.402]
On-the-job training	0.0588	0.0522	0.0588	0.0523	0.0579	0.0588	0.0582	0.0581
	[4.017]	[3.116]	[4.020]	[3.124]	[4.548]	[4.159]	[4.563]	[4.122]
N	2426	1799	2426	1799	2275	1784	2275	1784
R-squared	0.2159	0.2028	0.2159	0.2029	0.1438	0.1598	0.1441	0.1630
Panel 2: Females								
	Blue collar				*White collar*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0493	0.0397	0.0425	0.0374	0.0555	0.0593	0.0554	0.0647
	[2.701]	[1.778]	[2.250]	[1.576]	[12.478]	[12.883]	[12.291]	[12.391]
Test score	0.0449		0.1113		0.0249		0.0259	
	[2.096]		[2.512]		[4.586]		[2.491]	
Parental education		0.0113		0.0154		0.0015		-0.0067
		[1.003]		[0.549]		[0.500]		[1.387]
Education * experience	0.0019	0.0060	0.0030	0.0067	0.0022	0.0023	0.0022	0.0001
	[0.450]	[1.100]	[0.699]	[1.018]	[1.799]	[1.772]	[1.797]	[0.040]
Test score * experience			-0.0184				-0.0003	
			[1.625]				[0.110]	
Parental education * experience				-0.0011				0.0031
				[0.159]				[2.155]
On-the-job training	0.0638	0.0449	0.0625	0.0450	0.0531	0.0463	0.0531	0.0471
	[1.760]	[0.971]	[1.733]	[0.979]	[6.248]	[5.055]	[6.248]	[5.139]
N	418	306	418	306	3850	3123	3850	3123
R-squared	0.2286	0.2231	0.2340	0.2232	0.2148	0.2159	0.2148	0.2174

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. The experience profile of wages is controlled for with a set of dummy variables for 1 through to 8 or more years of actual experience. All models include an intercept, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

Table 8.2. Returns to education, ability and experience, by hiring channel

Panel 1: Males								
	Direct contact				*No direct contact*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0298	0.0262	0.0302	0.0284	0.0363	0.0296	0.0365	0.0416
	[2.992]	[2.225]	[3.023]	[2.056]	[5.097]	[3.782]	[5.050]	[4.544]
Test score	0.0131		0.0069		0.0157		0.0132	
	[1.407]		[0.345]		[1.923]		[0.819]	
Parental education		0.0112		0.0078		0.0044		-0.0142
		[2.088]		[0.790]		[1.079]		[1.857]
Education * experience	-0.0003	0.0005	-0.0004	-0.0002	0.0012	0.0010	0.0012	-0.0029
	[0.146]	[0.180]	[0.181]	[0.072]	[0.678]	[0.506]	[0.644]	[1.166]
Test score * experience			0.0015				0.0007	
			[0.331]				[0.168]	
Parental education * experience				0.0010				0.0055
				[0.395]				[2.685]
On-the-job training	0.0665	0.0585	0.0668	0.0587	0.0557	0.0541	0.0557	0.0535
	[4.002]	[3.147]	[4.015]	[3.154]	[4.622]	[3.977]	[4.624]	[3.935]
N	1861	1440	1861	1440	2845	2145	2845	2145
R-squared	0.1768	0.1607	0.1768	0.1608	0.2182	0.2208	0.2182	0.2242
Panel 2: Females								
	Direct contact				*No direct contact*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0431	0.0477	0.0416	0.0576	0.0433	0.0445	0.0434	0.0464
	[5.309]	[5.520]	[5.139]	[5.981]	[8.143]	[7.938]	[8.053]	[7.204]
Test score	0.0105		0.0329		0.0268		0.0243	
	[1.111]		[1.757]		[4.203]		[1.976]	
Parental education		0.0068		-0.0096		-0.0012		-0.0039
		[1.186]		[1.014]		[0.369]		[0.711]
Education * experience	-0.0004	-0.0009	-0.0001	-0.0051	0.0032	0.0034	0.0032	0.0027
	[0.207]	[0.354]	[0.037]	[1.678]	[2.356]	[2.296]	[2.319]	[1.420]
Test score * experience			-0.0065				0.0008	
			[1.224]				[0.259]	
Parental education * experience				0.0059				0.0010
				[2.136]				[0.604]
On-the-job training	0.0462	0.0284	0.0463	0.0287	0.0421	0.0393	0.0421	0.0395
	[2.895]	[1.566]	[2.915]	[1.581]	[4.317]	[3.686]	[4.319]	[3.700]
N	1235	947	1235	947	3036	2486	3036	2486
R-squared	0.2281	0.2283	0.2295	0.2332	0.2777	0.2758	0.2778	0.2760

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. The experience profile of wages is controlled for with a set of dummy variables for 1 through to 8 or more years of actual experience. All models include an intercept, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

Table 8.3. Returns to education and ability, by experience and tenure

	Panel 1: Males				Panel 2: Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.0330	0.0277	0.0331	0.0348	0.0439	0.0464	0.0435	0.0508
	[5.491]	[4.043]	[5.436]	[4.337]	[9.599]	[9.657]	[9.399]	[9.163]
Test score	0.0148		0.0134		0.0203		0.0271	
	[2.344]		[1.034]		[3.788]		[2.658]	
Parental education		0.0069		-0.0037		0.0016		-0.0052
		[2.064]		[0.593]		[0.553]		[1.076]
Education * experience	0.0000	0.0002	0.0000	-0.0039	0.0008	0.0011	0.0009	-0.0022
	[0.012]	[0.091]	[0.026]	[1.675]	[0.671]	[0.776]	[0.722]	[1.093]
Education * tenure	0.0017	0.0019	0.0017	0.0050	0.0024	0.0021	0.0025	0.0050
	[0.996]	[0.982]	[0.995]	[1.878]	[1.981]	[1.548]	[2.029]	[1.847]
Test score * experience			0.0005				-0.0012	
			[0.127]				[0.300]	
Test score * tenure			-0.0002				-0.0017	
			[0.038]				[0.392]	
Parental education * experience				0.0051				0.0043
				[2.832]				[2.284]
Parental education * tenure				-0.0036				-0.0033
				[1.666]				[1.263]
On-the-job training	0.0602	0.0562	0.0602	0.0558	0.0436	0.0349	0.0436	0.0350
	[6.037]	[4.995]	[6.031]	[4.977]	[5.169]	[3.747]	[5.170]	[3.770]
N	4647	3537	4647	3537	4233	3403	4233	3403
R-squared	0.1928	0.1884	0.1928	0.1909	0.2555	0.2523	0.2557	0.2543

Notes: The dependent variable is log real hourly wages in 2001 dollars. All models are estimated by pooled OLS. The numbers in square brackets are absolute values of the t-statistics, which are calculated using clustered standard errors that are robust to heteroskedasticity and serial correlation at the individual level. All models include an intercept, dummy variables for 1 through to 8 or more years of actual experience, dummies for 1 through to 7 or more years of tenure, 9 occupation dummies, 17 industry dummies, dummies for Aboriginal or Torres Strait Islander status and language other than English, and a dummy to indicate that the respondent was married or living with a partner.

CHAPTER 9

CONCLUSION

In the preceding chapters, results have been reported for two tests of information-based theories of the returns to education, in the context of the labour market for young Australians. Both of these tests are modern versions of tests originally proposed by Layard and Psacharopoulos (1974), in the course of the early debate over the educational sorting hypothesis. The first is a test of whether the returns to education increase discontinuously with the completion of credentials such as high school graduation or a university degree. The second is a test of whether the return to education declines with experience – and conversely, of whether there are increasing returns to measures of ability that may not initially be observed by employers at the time of hiring. It is believed that this is the first time that either of these tests has been conducted in an Australian setting.

These tests have been conducted using a new data source, in the form of the 1995 Year 9 cohort of the Longitudinal Surveys of Australian Youth (LSAY). This tracks individuals during the years in which they are entering and establishing themselves in the labour market – the period during which credential and employer learning effects are most likely to be important. In conducting the tests, three specific features of this data source have been exploited. Firstly, for the purpose of estimating credential effects, separate measures of years of education and completed credentials were constructed from the data. Secondly, for the purpose of estimating the employer learning model, two measures of ability – which are posited to not initially be observed by employers – were derived. The first measure is based on scores in mathematics and reading tests, while the second is based on parental education.

Thirdly, in conducting both tests, information on how a respondent found their current job has been used to partition the sample into those who found their job through a direct contact with their employer (that is, through a friend or relative, or because they had been approached by their employer), and those who did not. This distinction is of interest because employers may have more initial information about workers who were hired through these channels than for those who were not. As a result, they may rely less on educational credentials as a proxy for ability, and they may also have less need to adjust wages in response to new information that arrives with experience, compared to employers who do not have this superior initial information. Thus if credential and experience effects in the returns to education are driven by informational factors, it is likely there will be differences in the importance of these effects between hiring channels. However if the effects are due to alternative human capital explanations, then there is no reason to expect them to differ across hiring channels. Estimates of the employer learning model by hiring channels have previously been reported by Strobl (2004). However it is believed that the work reported here represents the first time that estimates of credential effects have been presented separately by hiring channel.

Results for the estimates of credential effects are presented in chapters 5 and 6. The results in chapter 5 indicate that there is no significant credential effect to high school graduation, but that there are large and highly significant credential returns to completion of a bachelor's degree. These effects are on the order of 14% for males, and 10% for females. There is some evidence of a smaller credential effect of roughly 4% for undergraduate diplomas; however the statistical significance of this result is sensitive to whether the return to years of education is modelled by a log-linear specification, or as a step function.

For males, the step function estimates imply that 39% of the returns to 15 years of education (relative to 9 or fewer years) are attributable to credential effects. The corresponding figure for females is 36%. The results also indicate that both years of education and completed credentials play an important role in explaining the returns to education: specifications which only include one of these two measures of educational attainment are decisively rejected in favour of hybrid specifications that incorporate both. Finally, even after controlling for credential effects, there remains a conspicuous jump in the returns to education after 13 years, corresponding to the first year of post-secondary education. This suggests that post-secondary admission or attendance may itself have a sorting effect, as first suggested by Arrow (1973).

The analysis in chapter 6 extends this work in several dimensions. Firstly, the technique of quantile regression is used to examine how the pattern of credential effects varies across the wage distribution. It is believed that this is the first time this technique has been applied to the estimation of credential effects. The results indicate that for both males and females, the credential return to completion of a bachelor's degree increases moving up the wage distribution. For males, the proportion of the return to education that is attributable to credential effects also increases moving up the wage distribution, while for females the opposite appears to be the case. Finally, the distinctive increase in earnings after 13 years is found everywhere, except for females toward the bottom of the wage distribution.

Next, the returns to those who commence post-secondary study without completing a credential are examined, with a view to establishing whether it is the fact of such study that is rewarded in the labour market, or the number of years. While several authors have estimated returns to post-secondary dropouts, it is believed that the work reported here represents the first time that estimates of returns to years of education have been compared before and after the inclusion of a variable indicating some post-secondary study, as depicted in figure 6.2.

It was found that there are significant returns to 'some post-secondary' study, and that the estimated return to years of education diminishes further with the inclusion of this variable. As a result of this decline in the estimated return to years of education, the estimated credential effects for diplomas and degrees increase accordingly. It was argued that this finding suggests that some previous authors may have overstated the credential effects of post-secondary qualifications. The reason is that they implicitly compare holders of these credentials to high school graduates *with no post-secondary study*.

Finally, chapter 6 also reports estimates of credential effects separately by hiring channels. In these results, the estimated credential effects to both diplomas and degrees, for both males and females, are consistently larger and more significant for those who did not find their job through a direct contact with their employer. For both males and females, the credential return to a diploma is only significant in the no direct contact group. For females, the same is also true for degrees, while for males the degree effect is larger in this group – although it remains significant in the direct contact group as well. Given that the employers of workers in the no direct contact group are likely to have less information about these workers' ability at the time of hiring, this pattern of results tends to support the view that the credential effects are attributable to education sorting. On the other hand, if employers reward credentials because they reflect a greater amount of human capital, then there is no reason to expect the credential effects to differ across hiring channels.

Results for the model of employer learning with statistical discrimination are reported in chapters 7 and 8. In these estimates two variables – namely a test score, and parental education – are considered as candidates for a measure of ability not initially observed by employers. In chapter 7 it was firstly confirmed that, for males, each of these variables has a significant positive association with wages, even after controlling for years of education. For females, the same is the case for the test score – but not for parental education.

Next, when the effect of the ability variables is allowed to vary with experience, there is no evidence found of any increasing return to the test score with experience. Instead, for both males and females, the return to the test score appears largely to be realised at the time of labour market entry, indicating that the information captured by this variable is readily observed by employers. On the other hand, there is no significant effect of parental education at zero experience, while the return to this variable is found to increase with experience. This finding is consistent with the hypothesis of employer learning on the basis of parental education, although this conclusion is more tenuous for females than it is for males. This is because, firstly, the interaction term is only marginally significant for females, and secondly, because there was no significant 'main' effect of parental education for females in the original estimates without experience interactions.

This pattern of results is striking in light of the findings that have been reported in other countries. These typically find strong evidence of employer learning in relation to test scores, but only mixed evidence for parental education. Decomposing the test score into its separate mathematics and reading components uncovers some interesting differences in the relative importance of these dimensions of ability between males and females. However, the main finding of no employer learning with regard to the test score remains unchanged. Finally, in the results of quantile regressions, the finding of employer learning in relation to parental education is only confirmed for males in the middle to lower regions of the wage distribution.

The work in chapter 8 elaborates on these results in three respects. Firstly, when the employer learning model is estimated separately by occupational class, the finding of increasing returns to parental education is found (for both males and females) to be restricted to white-collar workers, while the size and significance of the effect is strengthened in this group. It was suggested that this result may be related to the types of attributes that are reflected in parental education, which may be of greater value to employers in white-collar occupations.

Next, estimates of the model were reported separately by hiring channel. It was hypothesised that if the finding of increasing returns to parental education is due to employer learning, then this result should be stronger among those who did not find their job through a direct contact with their employer – since this is the group about whom employers are posited to be initially less well-informed. This prediction is indeed confirmed for males; however the opposite result is in fact obtained for females. Thus if anything, the result for females is easier to reconcile with an alternative explanation according to which more able females in the direct contact group enjoy superior access to opportunities for career advancement.

Finally, when the return to parental education is allowed to vary with both experience and tenure, it is found (for both males and females) that only the experience interactions are significant. This result may be interpreted as suggesting that employer learning is public, in other words that information is revealed symmetrically to all potential employers. However this result should only be regarded as tentative, since the limited work experience (and thus tenure) of the sample members implies that these variables remain quite highly correlated.

In light of these results, a number of fruitful avenues for future research may be suggested. Firstly, an obvious limitation of the work reported here is that it relates specifically to young individuals at quite an early stage of their working careers. Thus some caution should be exercised in extrapolating the conclusions to the broader labour market, and a priority for future research will be to corroborate these findings using data on more general populations.

This is especially the case for the model of employer learning with statistical discrimination, which is concerned with how the returns to education and measured ability evolve with labour market experience. Indeed, in several instances in chapters 7 and 8, a significant interaction of parental education with experience is juxtaposed with a non-significant ‘main’ effect of parental education when the interaction term is omitted. The first of these results indicates that employer learning may indeed be occurring; however the length of experience may be insufficient for the effect of such learning to be fully reflected in the latter coefficient.

Given data on more mature populations with more extensive experience, it should be possible to better discriminate between the returns to experience and tenure, and thus reappraise the question of whether incumbent employers benefit from superior private information. It would then also be possible to estimate more sophisticated models of the experience profile of returns to education and measured ability, as opposed to the simple linear interaction effects considered in chapters 7 and 8. This in turn would make it possible to estimate a bound on the total portion of the return to education that is attributable to sorting effects, in the manner outlined by Altonji and Pierret (1998) and Lange (2005).

Similarly, in the literature on credential effects, Belman and Heywood (1997) have suggested that the size of these effects may decline with experience. Once again, data on more mature samples will be required before this hypothesis can be investigated in an Australian setting.

A second avenue for future research concerns the differences in the patterns of credential and employer learning effects between males and females. Such differences were identified in a number of instances in the preceding chapters, but the underlying causes were not pursued. However it is well-known that the education and labour market participation decisions of females are subject to different considerations to those of males, and the implications for the results of tests such as the ones reported here are deserving of more detailed exploration.

These qualifications notwithstanding, the results reported here do indicate that considerations of incomplete information are one important influence upon the returns to education for young Australians. This is evident both in the pattern of returns to credentials (and bachelor's degrees in particular) and in the evolution of the returns to ability (as measured by parental education) with experience. As such, the results lend further support to the educational sorting hypothesis in augmenting, though not necessarily supplanting, the human capital model of education.

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