CASE STUDIES IN

AUSTRALIAN LABOUR ECONOMICS

John J. Beggs and Bruce J. Chapman

January 1989

Centre for Economic Policy Research
Research School of Social Sciences
Australian National University

For James Tobin, mentor

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FOREWORD

Most members of the economics group in the Research School of Social Sciences leave and follow illustrious careers. Since 1969, when I arrived, nine previous members have been appointed to Chairs in Australian Universities. Other members have also been very successful in the Public Service. This year, for example, a former student was appointed as Governor of the Reserve Bank of New Zealand, another former student held until recently the position of Secretary of Foreign Affairs and Trade. We always hire exceptional people.

John Beggs will be the most recent addition to this illustrious group,but it is a measure of the changing world that instead of joining the Public Service or accepting a University Chair he is joining the private sector where the opportunities seem to be boundless. I am sure that the next time I am called upon to write one of these forewords for another exceptional person I will be able to report that one of our ex members is a millionaire.

John first joined the Department in 1986 as Visitor on Secondment from the Faculties. The stay was so successful, and the pressure placed upon me by members of the Department to retain his services was so great, that we brought him back for a longer period. From the Department members' viewpoint they wanted a pleasant and well versed econometrician around. From my viewpoint I wanted a top ranking person. From Bruce Chapman's viewpoint he wanted someone who would help him double his publication rate. The reader has only to look at the papers in this volume to see how productive that stay and his partnership with Bruce Chapman was, and how lucky we were to be able to attract and hold John Beggs for as long as we did. It is just a pity that he could not stay longer.

The papers collected in this volume are a subset of the work that John Beggs completed while a member of the Department. This subset includes his important survey of diagnostic testing and some of the many papers written with Bruce Chapman. Each of the joint papers relates to issues that are of fundamental policy relevance, the more important being: the role of relative public-private wage structures as an incentive for public sector labour turnover; the influence of incomes policy on industrial disputation; the relative performance of immigrants in the Australian labour market; and the income consequences of child-rearing. There is no doubt that the research of Beggs and Chapman documented here will have an important bearing on future analyses and policy development in these areas, and further enhance the reputation of the Department and Centre for Economic Policy Research as leading centres of applied economics.

For my part it is a sadness that such a productive partnership has come to a close, at least for the near future. However, to return to my earlier remarks, I look forward to the new Chair that a very successful John Beggs will no doubt endow for us. It will go part of the way for compensating us for such a great loss, understandably felt most keenly by Bruce Chapman.

Professor R.G. Gregory

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PREFACE

This volume is a gift to my closest colleague, John Beggs. We worked together for over four years, and there is no doubt in my mind that due to our collaboration this has been the most interesting and productive period of my professional life. I would be very surprised if it is ever surpassed.

John Beggs has skills that are extremely rare in the profession. His talent has the potency to change the nature of applied investigation and, more obviously, to substantially improve the quality of the policy debate. He will be a great loss to applied economics research.

Our research matured through the frequent exchange of questions, ideas and other information, a process which was always characterised by openness and enthusiasm. An important point to be understood from our partnership is that since it is very unlikely that any particular person has the capacity to fully develop and present an idea that is both technically and conceptually compelling, joint work can be extremely rewarding and productive. In short, useful collaboration can result in a sum far in excess of the addition of the parts, a point highlighted by the papers included in this volume.

Paper No. 1, "Diagnostic Testing in Econometrics", was written by John alone and is included because it is such a lucid statement of a methodological position that influenced profoundly our joint research. The other papers are grouped in terms of subject matter and are presented chronologically within the themes explored.

Catherine Baird, Marti Pascall and Winnie Pradela made this volume possible.

Bruce Chapman

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Paper 1

DIAGNOSTIC TESTING IN APPLIED ECONOMETRICS

John J. Beggs

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May 1987

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I. INTRODUCTION

Economic theory is rich in its conception of models and in its ability to draw out the implications of those models. It is demonstratively weaker when describing the links between the elements of a model and the data available to assess the correctness of the model. Economic theory can usually "sign" the effect of change in control variables (for example when price goes up quantity demanded goes down), but rarely can theory provide the correct functional form of relationships among economic variables. Economic theory can often advise the reader that dynamic processes are anticipated. But since time in theory models is dimensionless there is little *a priori* knowledge of the length or the pattern of the adjustment in the relevant economic variables. The challenge of applied econometrics is to fill the gap between theory and data, to identify *adequate* functional forms and to provide *adequate* descriptions of their dynamics.

Diagnostic testing in applied econometrics is concerned with establishing whether an estimated model is an *adequate* description of an economic phenomenon. This paper reviews many recent advances in diagnostic model evaluation for the non-specialist econometrician. The survey is not exhaustive and can be augmented with other papers by Gilbert (1986), Hendry and Richard (1982), McAleer, Pagan and Volker (1985) and Pagan (1984). The emphasis throughout this survey is upon the practical ease of using diagnostic tests and their intuitive attractiveness.

A caveat is necessary at the outset. The "artistic" element of economic modelling strongly flavours the paper. Good judgement and experience (neither easily quantified) play the central role in selection and interpretation of estimated economic models. Empirical reality is always a good deal more complex than the menial world of most theoretical models. Yet rigorous theory provides essential frameworks and benchmarks against which empirical results can be interpreted. A less than careful reader may gain the impression elsewhere in the paper that such theory plays a minor role in the work of the applied economist. This is not intended.

Formal mathematical representations of arguments have been eschewed in preference for more heuristic perspectives. The exposition of ideas is carried on with the aid of an actual example. The example is a demand function for Australian per capita beef meat consumption (Fisher, 1979). This example provides a rich set of illustrative opportunities. Each diagnostic test is motivated in simple language and an attempt is made to explain the role of each test when forming an overall assessment of the model. Technical details are not reported and the interested reader can follow-up the relevant citations.

II. A METHODOLOGICAL PERSPECTIVE

Classical statistical inference holds out a possibility which is only rarely available to the applied economist. To crystalize ideas, I argue that classical statistical inference can be characterised as four steps: (i) propose a model (ii) collect relevant data (iii) perform statistical tests to accept or reject the model and, (iv) if reject, start again with a new model and a new set of data. The old data have been compromised because any new model has almost inevitably been moulded to fit the empirical (and random) characteristics of the old data set.

The central problem of conducting classical inference in economics situations is the need for new data. Statistical theory provides few insights into the consequences of the repeated application of different statistical models and tests to a single set of data. The one thing we do know is that repeated modelling and testing on a given data set means that the apparent size and power of the tests are no longer their true size and power.

But economic data are scarce. They are frequently the real time product of the economic system, which cannot be speeded-up or replicated. Once collected they form the historical record which the economist must unravel to understand the nature and quantitative dimensions of the economic system which produced them. Hence repeated modelling and testing of a given data set is the rule rather than the exception.

Recent debate over the use of statistical inference in economics has been stimulated by the idea that econometric results are being manipulated (Leamer, 1983 and McAleer, Pagan and Volker, 1985). The mechanism of manipulation is simple. With one set of data, the analyst can continue to fit different models until finding, for example, a regression model with coefficients having the *correct* sign and *good* t-statistics. Modern high speed computers and their software greatly facilitate this activity. Furthermore, the notion of *correct* sign on coefficients is a slippery concept, which too often refers to the author's ability to construct an ex post rationalisation of the coefficient sign, rather than the validation of a priori theory. Data mining, as this is termed, is the antithesis of the classical statistical rubric.

This situation has encouraged the development of a new set of testing guidelines, more in step with actual empirical practice. The new rubric is this: *subject the estimated model to a large number of diagnostic statistical tests (including the common sense test) at conventional levels of statistical significance, and if it passes all these tests it is an adequate model.* The motivation for the new rubric is simple. If the problem is that empirical workers mine the data until they find results which satisfy certain simple criteria (correct signs and good t-statistics on coefficients) then increase the number of criteria. Thus mindless data mining becomes a very cumbersome way of finding adequate models, and also the chances of reporting empirical results with a model which is a poor representation of reality are much reduced.

The idea of battery testing of economic models is not without difficulties. Two of these are discussed now. First, when a large number of tests are used they are not usually independent tests, so it is difficult to formally establish the joint significance of a group of tests. But this purely statistical problem often pales to unimportance beside the more substantial problem of understanding the economic problems in the model. For example, an econometrician could find that an estimated regression model fails both a Chow test of structural stability and a Durbin-Watson test. This result could indicate parameter instability, or unmodeled serial behaviour of the disturbances, or both. Alternatively, it may simply indicate a missing trending variable. The actual diagnosis of the problem depends on intimate unformalised knowledge of the subject under study. In this instance it is extremely doubtful if knowledge of the joint distribution of the Durbin-Watson statistic and the Chow test statistic under the null hypothesis of parameter constancy and no serial correlation would be any meaningful contribution to the knowledge of the applied econometrician.

The second issue is the choice of how many tests to perform. The more tests that are carried out, the the greater the chance of finding reason to reject a perfectly good model and the less the chance of accepting a poor model. The decision depends on the relative costs of rejecting a good model versus those of accepting a poor model, and the relative prior probability that the model is a poor model.

One view is that there are far too many unreliable econometrics results in the literature and that these misguide both policy formation and the development of economic theory. An immediate research goal in applied econometrics is to clean-up the resulting popular perception that econometric results are unreliable and/or barely useful.

Another popular view is that empirical analysis *must* provide answers. The compulsion is that policy makers make their decisions and economic theorists proceed with model development even if no econometric advice on empirical magnitudes is given. In this context, diagnostic testing of econometric models is a negative activity. The more tests are performed the greater the chance of finding cause to reject a model. Thus the more difficult it is to present results which purport to offer an adequate quantitative representation of economic reality to policy makers.

The position of this survey is that estimated econometric models should be subjected to a larger array of diagnostic tests than is current practice in the profession. The high incidence of poor empirical results finding their way into public policy debate has done a great deal of harm to the stature of the economics profession. The profession will be well served if it can have greater confidence that reported econometric results are reliable.

III. SCOPE OF THE EXPOSITION

The following sections of this paper consider only the testing of single linear equations. Most of what is discussed in later sections can be extended to non-linear single equations. However, the discussion is relevant to the issue of "systems estimation".

A great deal of effort in econometrics has focussed on estimating systems of equations. This has been particularly true in demand and cost studies and also in some macroeconomic studies. Economic theory frequently tells us that there is information in one equation which can be used to buttress related information in another equation to improve the efficiency of the overall set of parameter estimates. This is especially true when cross-equation restrictions can be imposed on parameter values such as, for example, symmetry restrictions in a system of demand equations.

However, the history of systems estimation has not been an entirely happy one. Often systems estimation has not produced the anticipated improvement in the plausibility of results. This has prompted the development of more flexible functional forms for application in areas of demand and production theory. Particular examples are Jorgenson, Lau and Stoker (1982), Gallant and Golub (1985) or Deaton and Muellbauer (1980). The development is motivated by the idea that cross-equation restrictions, such as symmetry in a demand system, are a fundamental product of economic theory (in this example the Slutzky equation) and *must be* correct. If estimated models produce unreasonable results when the requirements of economic theory are imposed it must be because the functions being estimated are not flexible enough to accord with the underlying behaviour of the economic agent as governed by such criteria as his/her utility function.

A more sanguine view sees a need to look at equations one at a time before going to systems estimation. In any system of equations there are usually both good and poor equations. The poor equations are the result of serious mis-specification of the functional form and dynamics of the equation, or else (and more often) the result of data problems. Almost all data series represent aggregations across unlike microeconomic units, and frequently across time periods, so the possibilities for confusing or confounding underlying features of economic behaviour are considerable. Mis-measured data induce bias and inconsistency in the parameter estimates of the equation based on that data. Systems estimation combines the information in good equations with the information in poor equations, and rather than each equation helping the other to produce more efficient parameter estimates, the poor equations can contaminate the good equations to cause a deterioration in the quality of the entire system.

Systems estimation is a powerful tool which can exploit both economic and statistical theory but, when possible, it is worthwhile to check each equation in the system to ensure that as a stand-alone entity it is a sensible representation of the empirical reality. Having established the reliability of each equation then combine information across equations by means of systems estimation to improve the overall efficiency of the estimates.

IV. AN ILLUSTRATIVE EXAMPLE

The diagnostic tests in this paper are illustrated on an estimated model of the domestic Australian demand for beef meat. Domestic meat demand has been the subject of several econometric studies, with previous papers on the subject by Main et al (1976), Fisher (1979), Martin and Porter (1984) and Chalfant and Alston (1986). Naturally the paper focuses narrowly on econometric diagnostic issues, and leaves for other occasions the discussion of the substantive economic underpinnings of meat demand models.

The best illustrative advantage is gained by concentrating on the double logarithmic form of the demand function employed in Martin and Porter (1984). In this form the regression coefficients are interpreted as constant demand elasticities. To emphasise the comparative aspects of model evaluation two alternative demand functions are also presented. The first is a function which is linear in the same consumption price and expenditure variables, and the second is the Deaton and Muellbauer (1980) almost ideal demand system (AIDS) form of the demand equation. The AIDS model is actually the preferred model, but it does not yield the same pedagogic opportunities in this data set. Murray (1984) presents a wide selection of demand functions for Australian meat demand, and the interested reader could refer to Murray for other comparisons of models.

The general form of the demand function for beef meat can be written:

$$(1) \quad q_b = f(P_b, P_c, P_m, P_l, P_p, X, P)$$

where

q_b - quantity of beef sold

P_b, P_c,

P_m, P_l,

P_p - the prices of beef, chicken, mutton, lamb and pork respectively

X - total of consumption expenditure

P - a composite price index for all other goods, proxied by the CPI.

Implicit in this demand function is the assumption that changes in the <u>relative</u> prices of non-meat products which do not change the composite price index do not affect the level of meat consumption or the types of meat consumed. Invoking the assumption of no money illusion, the demand function must be homogenous of degree zero in prices and incomes. Consequently, equation (1) may be re-expressed as:

$$(2) \qquad q_b = g\left(\frac{P_b}{P}, \frac{P_c}{P}, \frac{P_m}{P}, \frac{P_l}{P}, \frac{P_p}{P}, \frac{X}{P}\right)$$

where all money terms are now in constant dollars. The three forms of the demand equation considered in this paper are for (i) g() linear in its arguments, (ii) g() which is the exponentiation of a linear function in the logarithms of its arguments, the log-log form, and (iii) the approximate AIDS form where:

$$(3) \qquad q_b = \frac{X}{P_b}\left(\beta_0 \sum_{j=c,m,l,p} \beta_j \, \ell n\left(\frac{p_j}{P}\right) + \beta_x \, \ell n\left(\frac{X}{P.PBAR}\right)\right)$$

with

$$PBAR = \prod_{j=1}^{5}\left(\frac{p_j}{P}\right)^{S_j} \qquad \text{and } S_j = \frac{p_j q_j}{X}$$

The AIDS model is estimated in expenditure share form and this is achieved by multiplying both sides of equation (3) by (p_b/X).

Table 1 reports the average characteristics of the data, giving mean values and standard deviations. The data are an extension of those used by Martin and Porter (1984). One limitation of the data is that in later parts of the time series the mutton price has been inferred from the price of lamb, so the two series are perfectly co-linear in this sub-sample. This is worth noting since it may partially explain some anomalous results in estimated cross-price demand elasticities for lamb and mutton.

TABLE 1

Means and Standard Deviations of Study Variables Quarterly Data 1962/1 to 1985/1

Variable			Mean	Standard Deviation
C_b	-	per capita kg consumption of beef per quarter	12.18	2.51
p_b/P	-	per kg price of beef in 1980 prices, $'s	3.48	0.47
p_c/P	-	per kg price of chicken in 1980 prices, $'s	2.50	0.66
p_l/P	-	per kg price of lamb in 1980 prices, $'s	1.73	0.23
p_m/P	-	per kg price of mutton in 1980 prices, $'s	2.77	0.31
p_e/P	-	per kg price of pork in 1980 prices, $'s	3.83	0.27
X/P	-	per capita total consumption expenditure 1980 prices per quarter, $'s	1080	152
S_b	-	share of beef expenditure in total consumption expenditure	0.039	0.006
S_c	-	share of chicken expenditure in total consumption expenditure	0.007	0.001
S_m	-	share of mutton expenditure in total consumption expenditure	0.005	0.004
S_e	-	share of lamb expenditure in total consumption expenditure	0.012	0.003
S_p	-	share of pork expenditure in total consumption expenditure	0.012	0.001

The three models described above were initially estimated by ordinary least squares. All prices and expenditures were measured in 1980 real prices. The estimated coefficients of the models, their OLS t-statistic and the implied price and income elasticities are reported in Tables 2a, 2b and 2c. A linear trend was added to the linear demand function model to account for changes in consumption not explained by price and expenditure effects. Arbitrarily adding trend variables is not an attractive methodology but it is done here to create a viable model to illustrate some points later in the paper.

TABLE 2a

Linear Form of the Demand Function for Beef
(Dependent Variable is per capita beef consumption)

Variable	Coefficient Estimate	t-Statistic	Elasticity at Mean of Data
Price of beef	-5.030	18.24	-1.43
Price of chicken	-0.291	0.71	-0.06
Price of mutton	2.747	2.37	0.39
Price of lamb	-0.222	0.23	-0.05
Price of pork	2.581	4.45	0.81
Total expenditure	0.016	2.22	1.47
Linear trend	0.068	1.60	
June quarter dummy	0.028	0.07	
September quarter dummy	0.073	0.02	
December quarter dummy	-2.579	3.01	
Constant	2.303		

$R^2 = 0.89$

TABLE 2b

Logarithmic Form of the Demand Function for Beef
(Dependent Variable is log of per capita beef consumption)

Variable	Coefficient Estimate	t-Statistic	Elasticity at Mean of Data
Log price of beef	-1.212	18.43	-1.21
Log price of chicken	0.195	1.76	0.19
Log price of mutton	0.458	3.37	0.46
Log price of lamb	-0.199	1.13	-0.20
Log price of pork	0.854	5.49	0.85
Log total expenditure	0.743	3.52	0.74
June quarter dummy	0.023	1.08	
September quarter dummy	0.028	1.33	
December quarter dummy	-0.147	4.73	
Constant	-2.989	1.27	

$R^2 = 0.88$

TABLE 2c

Almost Ideal Demand System form of Demand Function for Beef
(Dependent Variable is the share of total consumption on beef)

Variable	Coefficient	t-Statistic	Elasticity* at Mean
Log price of beef	-0.0115	4.79	-1.27
Log price of chicken	-0.0042	1.16	0.04
Log price of mutton	0.0059	1.25	0.33
Log price of lamb	0.0084	1.42	0.30
Log price of pork	0.0166	2.97	0.51
Log total expenditure deflated by geometric meat price index	-0.0248	4.96	0.36
June quarter dummy	0.0012	1.71	
September quarter dummy	0.0011	1.59	
December quarter dummy	-0.0024	2.34	
Constant	0.1320	1.61	

$R^2 = 0.85$

Note: *Although the cross-price elasticities of demand are all positive at the mean of the data, an examination of the implied year-by-year cross elasticities shows some negative elasticities, particularly in the most recent years of the data.

In each case the direct price elasticity of demand for beef is greater than unity, and seems to be in the range of values 1.2 to 1.4. The estimated standard errors from the linear and logarithmic forms of the demand function suggest an elasticity which is statistically significantly greater than unity. The other two variables which consistently display statistically significant coefficients are the price of pork and also total consumption expenditure. The cross-price elasticities of demand with pork seem reasonably high, in the order of 0.5 to 0.8. This is interesting since the evidence from Table 1 shows pork to be the most expensive meat type, and suggests that increases in the price of pork cause substitution into the nearly as expensive beef. The cross-price elasticities to the cheaper meats, chicken, mutton and lamb, are much lower (and have relatively large standard errors). The incorrect lamb cross-price elasticity could be due to deficiencies in the lamb and mutton price series. Since the matter is not of intrinsic concern to this paper, and since the relevant t-statistic is small, the issue is not considered further. The three models show quite different demand elasticities with respect to total expenditure. In the order of the Tables the estimated expenditure elasticities of demand are 1.47, 0.74 and 0.36. However, it should be noticed that the expenditure elasticity is determined with only a very low level of precision. The 95 percent confidence intervals for the above estimates are respectively, 0.17-2.77, 0.33-1.15 and 0.11-0.61. On this basis one is not entitled to say that any one model gives a significantly different expenditure elasticity result from the others.

V. DIAGNOSTIC EVALUATION

Three (at least) competing models of beef demand are available. The research methodology recommended in this paper is to examine each model for evidence of internal defects, such as evidence of functional mis-specification, which permits us to either eliminate that model from the set of contenders, or other evidence which suggests ways to improve the empirical performance of the model. In the second stage of testing, those models which have survived stage one are contrasted to each other using non-nested tests to find if one model dominates the other models or some subset of the other models.

Only the diagnostic tests of the logarithmic demand function in Table 2b are reported in the following sections of the paper. The linear and AIDS versions of the demand function are accepted as they stand, and are only used in the second stage of testing to illustrate the application of non-nested tests.

Reviewing the Data

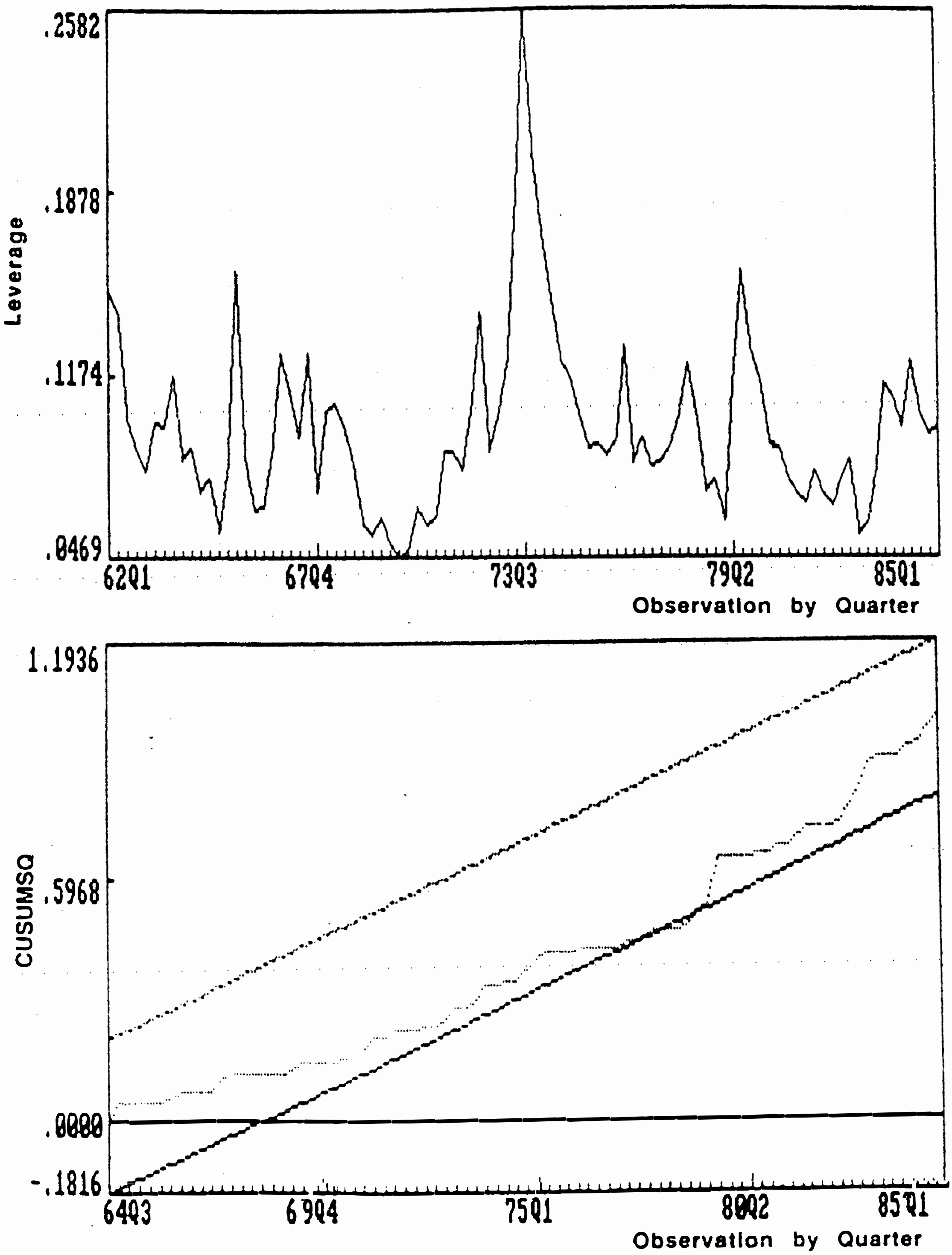
Each observation in the data provides an unequal amount of information about the underlying model. In the extreme, if prices and incomes remain the same in all periods nothing can be learned about the demand function. Conversely, the most informative data are periods when prices and incomes are a long way from their usual values since then the effect of price and income movements can be easily distinguished from other unmodeled random effects on demand. If, however, a large proportion of the useful information in a given sample is concentrated in just a few observations it is important to ensure that there is nothing abnormal or unrepresentative about the unmodeled (random) contributions to demand in these periods, since these abnormalities will prominently affect the coefficient estimates of the model.

A useful starting point in the evaluation of an estimated model is to identify those sample observations which make a large contribution to the coefficient estimates of the model and, where such observations are found, review other sources of information to ensure that the unmodeled aspects of demand in those periods are representative of other periods in the data. This practice has long been part of the model evaluation process, but mostly has been limited to spotting outliers in the regression residuals.

The first step is to obtain a broad sense of where the data is most informative, that is, where it has most impact on the estimated coefficients. One measure is called the leverage of the data. Leverage of a datum is measured as the diagonal element of the $X(X'X)^{-1}X'$ matrix for that datum (where X is the matrix of data regressor variables in deviation form and has dimension equal to the number of observations times the number of varying regressors). Krasker, Kuh and Welsch (1983) provide several motivations for this measure. Leverage is imply a measure of the share of total regressor variance attributable to each point in the data.

Figure 1 plots the leverage of data for the logarithmic demand model in Table 3b. The situation in these data sets is reasonably unexceptional. There are three quarters in the period 1973(2) to 1974(1) which have high but not exceptional leverage. The proper course of inquiry is to ensure that the unmodeled aspects of beef meat demand in these quarters are representative of the longer historical experience. This is done by detailed problem oriented investigation.

Figure 1



Economics applications are one area where high leverage points are especially likely to produce atypical patterns in the unmodeled component of behaviour represented by the disturbance term. For example, sharp rises in prices, interest rates, trade deficits etc., frequently induce behaviour not accounted for in the model. Non market responses include direct government intervention using, for example, tariffs and quotas, lending restrictions on banks, or other restrictions which circumvent politically unattractive market outcomes. Similarly, in times of market stress, large buyers and sellers may offer non-price preferential treatment to large trading partners as a form of implicit long-run trading contract. In these situations it is necessary to extend the model to account for exceptional behaviour, or limit the sample to a more representative time period and explicitly forgo explaining exceptional situations (e.g. periods of major wars are routinely excluded from econometric studies).

The overall importance of any single data point is a combination of both the leverage of the regressors and the outcome of the dependent variable. For example, a very large disturbance term occurring when the regressors are at or near the mean of the data has little effect on the estimated slope coefficients of the model and only raises or lowers the estimated intercept term. Conversely, large disturbances when regressors are far from the mean of the data have an important impact on the slope coefficients. Belsley, Kuh and Welsch (1980) have suggested computing the t-statistics for the changes in the estimated coefficients of the model as each observation is sequentially removed from the sample and then replaced. A second procedure is to compute the t-statistic on the difference between the prediction of the model at each data point as that data point is removed from the sample and then replaced. An excellent discussion of these procedures can be found in Fiebig (1987).

Serial Dependence

Since the data under study is a *time series* of beef consumption, a natural early concern is to ensure that the dynamics of the model have been properly specified. Traditional concern is with serial dependence in the model disturbances which, if not accounted for, results in inefficient estimation of the model, and more importantly incorrect formulae variances and test statistics. But the issue of dynamic specification cannot be considered separately from many other aspects of model evaluation. Mis-specification of the model, or the omission of relevant variables, can produce many of the same symptoms as those of true omitted dynamic phenomena (such as serial dependence in the disturbance term of the equation).

The Durbin-Watson statistic provides the most familiar test for serial correlation in regression disturbances. This is a small sample test of independent normally distributed disturbances against an alternative with first order serial correlation. The critical values of this test statistic allow for sampling variability in the regressors. Thus the rejection region of the statistic contains a doubtful region where the issue of rejection of the maintained hypothesis cannot be resolved without exact accounting for the effects of the particular regressor variable in the model. An early extension of the Durbin-Watson statistic was Durbin's (1970) h-statistic. This is an asymptotic test statistic for situations where the set of regressors contains lagged dependent variables.

Though the Durbin-Watson statistic is still regarded as a useful testing device in small samples, studies employing data samples of the size used in this paper usually directly invoke asymptotic arguments when applying test statistics. Asymptotically, the first and higher order correlation coefficients of the residuals of non-dynamic regression are normally distributed with variance equal to the reciprocal of the number of observations in the sample. Thus the residual correlation (of a given lag order) divided by the square root of the number of observations in the sample has a limiting standard normal distribution and is usually referred to as the autocorrelation function (ACF) t-statistic (for that lag order). Breusch (1978) and Godfrey (1978) have shown that this framework can be extended to test for k^{th} order serial correlation in dynamic regressions. This is done by estimating the correlation coefficient in a joint regression of the residual on itself lagged k periods and the current regressors of the equation under study. This test was developed using the Lagrange multiplier testing principle and the resulting t-statistics on the lagged residual is referred to as the LM t-statistic for k^{th} order serial correlation. For dynamic regression, the LM t-statistic for first order serial correlation is asymptotically equivalent to Durbin's h-statistic. Thus the computationally troublesome Durbin h-statistic is now rarely reported. For non-dynamic models the Durbin-Watson test for first order serial correlation is asymptotically equivalent to both the ACF t-statistic and the LM t-statistic. For higher order serial correlation in non-dynamic models the ACF t-statistic and the LM t-statistic are asymptotically equivalent.

Common practice in empirical studies has been to report only the Durbin-Watson or Durbin h-statistics for first order serial correlation. In some applications using quarterly data the Wallis (1972) test for fourth order serial correlation is reported. By limiting attention to first (and fourth) order correlation a great deal of the information about possible anomalous serial behaviour of the regression residuals is neglected. Better empirical practice is to report the ACF or LM t-statistics out to a reasonably high order of lag. Table 3 reports these statistics for the logarithmic demand function. The statistics indicate little or no evidence of serial correlation. There is a single spike in the residual correlogram at the seventh order lag. This is not statistically different from zero at conventional significance levels, and since there seems no economic rationale for such a seventh order lag structure in this data it is reasonable to conclude that the observation is a chance occurrence which does not require remedial attention.

TABLE 3

Tests for Serially Correlated Disturbances in the Logarithmic Demand Model

Order of Log	Correlation Coefficient	ACF t-Statistic	LM t-Statistic	Durbin-Watson Statistic
1	0.050	0.47	0.50	1.85
2	0.007	0.07	0.07	
3	0.096	0.93	0.97	
4	0.073	0.70	0.75	
5	0.066	0.63	0.69	
6	0.83	0.80	0.87	
7	0.188	1.81	1.92	
8	-0.021	-0.20	0.22	
9	0.007	0.07	0.08	
10	-0.022	-0.21	0.23	

Heteroskedasticity

Efficient estimation and the correctness of formula test statistics requires that the variance matrix of the model's disturbances be correctly specified. We have already checked for serial dependence, and found none. Our next step is to test for heteroskedasticity or non-equal variances of the regression disturbances. Both formal and informal testing approaches are available. The most commonly used informal test is to compare the OLS t-statistic of the coefficients (which are inconsistent in the presence of heteroskedasticity) with a set of t-statistics which are consistent under heteroskedasticity even of an unknown form. The first, and most used, heteroskedastic consistent t-statistic was proposed by White (1980b) and involves taking the true formula for the coefficient variance-covariance matrix of OLS estimators in the presence of heteroskedasticity and replacing the parameter for the variance of the disturbance on each observation by the squared OLS residual on that observation. Refinements to improve the small sample performance of this technique are also available (Cragg, 1983, MacKinnon and White, 1985). In the absence of heteroskedasticity, the OLS and White t-statistics are anticipated to be of similar magnitude. If they are not, heteroskedasticity may be present. The practical problem is to decide when the two sets of t-statistics are sufficiently different to be of concern. I recommend the following guide to action. Consider each coefficient of the model in turn. If either the OLS or the White t-statistic is greater than two, compare their relative magnitudes, and if one is more than twice the other conclude that some form of heteroskedasticity is present. Table 4 below presents comparison of the OLS and White t-statistics for the logarithmic demand model. The t-statistics computed by both methods are quite similar so there is no reason at this stage to be concerned about heteroskedasticity in the model.

Heteroskedastic consistent t-statistics make it possible to perform statistical inference on individual coefficients without knowing the form of the heteroskedasticity. But efficient coefficient estimates can only be obtained by identifying the parametric form of the heteroskedasticity. Furthermore, there are certain difficulties in using White-type heteroskedastic consistent t-statistics. The testing procedure outlined above suffers from its informality. But perhaps more seriously, methods which estimate heteroskedastic consistent t-statistics without modelling the form of the heteroskedasticity impose very little structure on the problem. In general, the more structure given to a testing problem the more powerful the test. There is danger that the method of comparing OLS t-statistics with White's t-statistics will not be a powerful method of discerning heteroskedasticity.

TABLE 4

Comparison of OLS and Heteroskedastic consistent t-Statistics
(in the Logarithmic Demand Model)

Variable	OLS Absolute t-Statistic	White's Absolute t-Statistic
Log price of beef	18.43	20.10
Log price of chicken	1.76	1.93
Log price of mutton	3.37	2.75
Log price of lamb	1.13	1.15
Log price of pork	5.49	4.97
Log total expenditure	3.52	3.75
June quarter dummy	1.08	1.24
September quarter dummy	1.33	1.56
December quarter dummy	4.73	5.54
Constant	1.27	1.21

The heteroskedasticity issue can also be addressed by testing explicit functional form models of the disturbance term variables. Asymptotically valid tests have been developed using the Lagrange multiplier testing principle (Breusch and Pagan, 1980). These tests are executed by regressing the squared residuals from the OLS regression on a set of explanatory variables. Under the null hypothesis that none of these explanatory variables determine the heteroskedasticity pattern of the model, then the number of sample observations times the R^2 statistic from this secondary regression has a chi-squared distribution with degrees of freedom equal to the number of non-constant explanatory variables in the secondary regression. The t-statistics of the coefficients in this secondary regression have an unusual characteristic, quite different from that in a standard regression problem (Pagan and Hall, 1984). Here the t-statistic on a regressor coefficient measures the correct statistical significance of that coefficient if all other coefficients (other than the intercept) are zero. If there is only one varying regressor in the secondary regression the t-statistic on its coefficient provides an asymptotically equivalent test to the chi-square test described above.[1]

Neither past empirical experience nor economic theory regularly provide good insight into modelling heteroskedasticity, so a seat-of-the-pants approach is often necessary. In time series models it is not unusual to test for trend related heteroskedasticity on the grounds that economic systems go through more stable and less table long term phases. Another common test is for a relationship with the predicted value of the dependent variable, the prior being that when absolute levels of the systematic component of the dependent variable are large the variance of the unsystematic component is large (or small). Fortunately the use of estimated rather than true model coefficients to create the dependent variable prediction for the secondary regression does not contribute to the limiting distribution of this test statistic. In Table 5, below, a number of explicit functional form tests for heteroskedasticity in the logarithmic beef demand function are reported. These test results are in accord with the evidence from the heteroskedastic consistent t-statistics reported in Table 4, as they show no compelling evidence of heteroskedasticity. However, the test in item (iv) in the Table falls just beyond the 90% critical value. To be cautious, and also to illustrate procedure the demand function was re-estimated by feasible GLS using the heteroskedasticity correction implied by item (vi) of Table 6. The coefficient estimates and their t-statistics are reported in Table 6. As anticipated, the feasible GLS results are very similar to the original OLS findings. Two small changes are noted. First, the cross-price elasticity with chicken has a smaller standard error and is now significant at a 2 percent level rather than the previous 8 percent level on a two tailed test. Secondly, the feasible GLS estimate of June quarter per capita beef consumption

TABLE 5

Lagrange Multiplier Tests for Explicit Functional Form of Heteroskedasticity
(of the Logarithmic Demand Model)

Variables Explaining Heteroskedasticity		Test Statistic	95% Critical Value
(i)	Three quarterly dummies	$X^2(3) = 4.94$	7.81
(ii)	Prediction of dependent variable	$X^2(1) = 2.35$	3.84
(iii)	Linear trend	$X^2(1) = 2.55$	3.84
(iv)	Prediction of dependent variable and quarterly dummies	$X^2(4) = 5.99$	9.94
(v)	Linear trend and quarterly dummies	$X^2(4) = 7.33$	9.49*
(vi)	Prediction of dependent variable, linear trend and quarterly dummies	$X^2(5) = 9.37$	11.1*
(vii)	Log prediction of dependent variable	$X^2(1) = 2.18$	3.84

Note: *The 90 percent critical value for $X^2(4)$ is 7.78 and for $X^2(5)$ is 9.24.

is 1.3 percent higher than the OLS estimate. This almost doubles the t-statistic but it is a result of no substantive economic importance.

TABLE 6
Feasible GLS Estimate of Logarithmic Demand Function

Variable	GLS Estimate t-statistic in parentheses		Comparison Table 3b OLS Estimate t-statistic in parentheses	
Log price of beef	-1.189	(19.55)	-1.212	(18.43)
Log price of chicken	0.208	(2.16)	0.195	(1.76)
Log price of mutton	0.432	(3.29)	0.458	(3.37)
Log price of lamb	-0.160	(1.00)	-0.199	(1.13)
Log price of pork	0.728	(5.08)	0.854	(5.49)
Log total expenditure	0.722	(3.81)	0.743	(3.52)
June quarter dummy	0.036	(1.92)	0.023	(1.08)
September quarter dummy	0.028	(1.52)	0.028	(1.33)
December quarter dummy	-0.138	(4.50)	-0.147	(4.73)
Constant	2.386	(1.09)	2.989	(1.27)

The subsequent analysis in this paper assumes homoskedastic disturbances. In the event that the investigations of the preceding pages had uncovered strong evidence of heteroskedasticity, the analysis which follows would have to be couched in the form of feasible GLS tests. This is computationally straightforward since it involves only rescaling the dependent and explanatory variables. Feasible GLS has consequences for the small sample behaviour of test statistics (for example, Rothenberg, 1984). My recommendation is that particular care should be taken when there are fewer than 70 observations and when the R^2 statistic is small (say less than 0.4).

Functional Form

The functional form testing literature deals with two types of comparison. One type of comparison is to a *non-specific* alternative model, and the other type of comparison is to a *fully estimated* alternative model. Conventional model testing deals with testing restrictions which make a large model smaller, in the sense that one tests to remove unnecessary (uninformative) variables from the regression model, and, where possible, make use of across-equation restrictions to reduce the number of parameters to be estimated. Fortunately most of the modern functional form tests can be thought of in the same terms as conventional tests, even when the alternative model is not specified oris not a nested subset of the maintained model.

The intuition behind most functional form testing comes very naturally to those involved in applied econometric research. The idea is this: *if the maintained model is the correct model then adding extra regressor variables, from whatever source, which are asymptotically uncorrelated with the disturbances of the maintained model, will not asymptotically improve the performance of the maintained model.* The testing problem comes down to choosing variables with which to augment the maintained regression model and then testing the significance of those variables. The power of the tests depends on the judicious choice of the added regressors.

Old hands will find nothing novel about adding extra variables to a model to check the completeness of the functional form. One common, and long standing, practice is to augment the linear form of variables in a regression model with the square root, square, reciprocal and logarithm forms of those variables and then test the significance of the added variable. The justification for this approach is that often little is known *a priori* about the functional form of the model and the empirical analyst must experiment to identify suitable forms. Though valuable, this approach is frequently viewed with deep suspicion. The manipulation of the functional form on individual regressors creates an environment which is prone to the worst aspects of data mining, in particular the reporting of only the most favourable estimation outcomes.

(i) *Non Specific Alternatives*

Ramsey (1969) promoted the idea of a general test for functional form not involving manipulation of individual regressors of the existing model. His best known test is the RESET test. The RESET test has power against many forms of mis-specification but is particularly useful when the maintained model has under-represented the curvature of the function it intends to estimate. The RESET test is performed by testing the asymptotic significance of polynomial terms formed from the predictions of dependent variables from the maintained model. For example, RESET2 involves augmenting the regression with the squared predictions of the model and applying the usual t-test to the new coefficient (see Ramsey and Schmidt, 1976). RESET3 and RESET4 involve adding additional third and fourth powers of the predictions of the dependent variable and testing the joint significance of the added coefficients by the usual F-test or chi-squared test.

Breusch and Godfrey (1986) have recently shown how two other tests against non-specific alternative functional forms can be computed as variable addition tests. Plosser, Schwert and White (1982) observed that if a linear-in-parameters models is correctly specified, the coefficient estimates from the model estimated from first differences of the data should by asymptotically equal to the coefficient estimates obtained from the variables in levels. This proposition may be tested by augmenting the maintained model with a new set of regressors. Each of the new regressors is formed from the existing regressors by summing the regressor on the observation one period ahead and the observation one period lagged. Two observations are lost from the sample due to the process of constructing the added regressors. The appropriate test is then an F-test or chi-squared test on the joint significance of the added regressors.

White (1980a) suggests comparing the OLS and weighted least squares estimates of the model coefficients. The latter is an unbiased though inefficient estimator if the model is correctly specified, in which case the expected difference between the OLS and GLS is zero. If the model is not correctly specified the expected difference between the coefficient estimates is not necessarily zero, and this forms the basis of the test. Equality of the coefficient estimates can be tested by augmenting the original regressors of the maintained model with a set of extra variables, constructed by multiplying the original regressors by an observation specific weight, and testing their joint significance. The practical difficulty with this test is the selection of weights, since the power of the test depends on judicious choice of weights. One of White's (1980) weighting schemes is based upon the work of Prais and Houthakker (1971) and involves using weights equal to the reciprocal of the square of the predictions of the maintained model. This form of the test is reported below in Table 8.

Utt's (1982) Rainbow test for non-specific functional mis-specification can also be given a variable addition interpretation. Utt's suggestion is to sort the data into high leverage and low leverage observations, where leverage is as defined above. The intuition is that if the model is mis-specified the same model fitted to the points a long way from the mean of the data will be unlike the model fitted to the closely bunched points around the mean of the data. In particular, linear approximations should be better in the low dispersion (low leverage) sample than in the high dispersion (high leverage) sample. The test involves splitting the sample into equal sized sub-samples of low and high leverage points and then performing a familiar Chow (1960) test for equality of the coefficient estimates in the two samples. Utt's tests cannot be implemented in time series situations where the disturbances of the true model are known to exhibit true serial time dependence. In such cases the correct variance-covariance matrix of disturbances cannot be recovered, so the sampling behaviour of the coefficient estimates is unknown.

A variation on Utt's idea is to examine the serial behaviour of the regression residuals when the data are ranked by leverage. Approximating curbed functions by linear functions causes the residuals of the fitted model to behave in a systematic fashion as one moves sequentially away from the centre of the data. The appropriate tests are described in the Section above on Senal Dependence.

A set of results for *non-specific alternative* tests of functional form are reported in Table 7. Each of the tests is reported as an F-test on the added regressors as described in the preceding paragraphs. Only the Plosser-Schwert-White first differencing test statistic falls marginally beyond the 95 percent critical value indicating mild cause for caution. It is not wise to discard a model which barely fails one test. Rather, this knowledge should be stored and used in conjunction with other findings from the diagnostic evaluation to assess the overall adequacy of the model.

TABLE 7

Tests of Functional Form Against Non-Specific Alternatives
(of the Logarithmic Demand Function)

Test		95% Critical Value
RESET 2	F(1,82) = 1.02	3.84
RESET 3	F(2,81) = 1.48	3.10
RESET 4	F(3,80) = 1.49	2.70
First Differencing[1]	F(6,75) = 2.28	2.24
OLS vs GLS (Prais - Houthakker)	F(9,74) = 0.94	2.04
Utt's Rainbow Test[2]	F(46,37) = 0.66	1.80

Notes: 1. The 99% critical value for F(6,75) = 2.62.
2. The Durbin-Watson statistic on the leveraged ranked data is 1.91 which does not indicate any first order serial pattern in the residuals. An examination of the ACF t-statistics shows no higher order serial correlation present.

(ii) *Specific Non-Nested Alternatives*

Non-nested specific alternative models can also be tested using variable addition methods. The theoretical econometric development of non-nested tests has evolved from the application of Cox's (1961, 1962) modified likelihood ratio. Perhaps the best known of these contributions is the Davidson and MacKinnon (1981, 1984) J-test which is a variable addition test, but other contributions are surveyed in McAleer (1986). For most practical purposes the question which non-nested tests seek to answer is whether by augmenting a maintained model with the attributes of a non-nested alternative one can improve the explanatory power of the maintained model. This test is asymptotically straightforward and involves augmenting the maintained model by including the predictions of the alternative model as an additional regressor. When the maintained model is the true model this extra regressor is asymptotically uncorrelated with the disturbances of the maintained model and the associated estimated regression coefficient is asymptotically zero. The t-statistic on the added regressor then provides a test of the maintained model.

Other slightly more complicated tests have been proposed for special situations. One common situation is to test the linear form of a model against the logarithmic form, such as arises in the comparison of the models estimated in Table 2a and 2b. One asymptotic test which can be applied in this situation is the MacKinnon, White and Davidson (1983) PE test. Davison and MacKinnon (1985) have also proposed a test of linear versus log-linear models which has good power properties when the disturbances of the model are normally distributed (Godfrey, McAleer and McKenzie, 1986).

Each of the three models reported in Tables 2(a, b, c) was tested against the remaining two models by augmenting each model with the predictions of the dependent variable as obtained from the remaining two models. For example, the linear equation was augmented by the exponential of the predictions from the logarithmic equation, or by the predicted expenditure share of the AIDS model multiplied by total expenditure and divided by the price of beef. The results are summarised in Table 8.

The results in Table 8 indicate a hierarchial rankings of the models. The logarithmic model is preferred to the linear model while the AIDS model is preferred to both the linear and logarithmic models. The t-statistics measuring the contribution of the AIDS model to the predictive power of the linear and logarithmic models are 8.18 and 9.20 which are very large and decisive rejections. Furthermore, the respective contributions of the linear and logarithmic models to the AIDS predictions are inconsequently small with t-statistics of 0.51 and 0.39. In this situation there is very good reason to reject the other two models in preference for the AIDS model.

TABLE 8

Non-Nested Test of Beef Demand Functions

	Maintained Model (H_0)		
Alternative Model (H_1)	Linear	Logarithmic	AIDS
Linear	-----	$t = 0.41$ Accept H_0[1]	$t = 0.51$ Accept H_0
Logarithmic	$t = 2.17$ Reject H_0[2]	-----	$t = 0.39$ Accept H_0
AIDS	$t = 8.18$ Reject H_0	$t = 9.20$ Reject H_0	-----

Notes: (1,2) The MacKinnon White Davidson (1983) PE test to compare the linear and log-linear models involves augmenting the linear model with the difference between the logarithm of the predictions of the linear model and the predictions of the logarithmic model. The t-statistic on this added regressor is 3.99. The logarithmic equation is augmented by the difference between the the predictions of the linear equation and the exponential of the prediction of the logarithmic equation. The t-statistic on this regressor is 0.60. The substantial result remains the same as that reported in Table 9.

One simple criterion for choosing among models is to choose the model with the highest maximised log-likelihood value (this is often referred to as the Sargan criterion). A pedagogical advantage of such a criterion-based model selection procedure is that one model will always be preferred. This can be helpful when non-nested model tests of the type shown in Table 9 produce inconclusive ranking of preference for the models. Implicit in this approach is the assumption that the disturbances of each model can be characterised as being drawn from a normal distribution. The likelihood of the dependent variable must be transformed, where necessary, to describe a common random variable across the models being compared. The Sargan criterion rankings are reported in Table 9. Again the AIDS model is preferred. Akaike (1973) proposed an adjustment to the log likelihood function to account for the number of estimated parameters in the model. The Akaike criterion (AIC) is to choose the model which maximises twice the log likelihood function minus twice the number of parameters in the model. For the results reported in Table 3 the linear model contains eleven parameters while the logarithmic and AIDS models each have ten parameters. The AIC rankings are also reported in Table 9. The AIDS model is preferred.

TABLE 9

Models Ranked by Maximised Log-Likelihood Function
(for the dependent variable measured as per capita beef consumption)

Model	Maximised Log-Likelihood	AIC
AIDS (Table 3c)	-102.28	-182.56
Logarithmic (Table 3b)	-110.37	-200.74
Linear (Table 3a)	-115.02	-210.04

V. *Simultaneous Equations Effects (Weak Exogeneity)*

If the regressors of a model are asymptotically correlated with the contemporaneous disturbances of the model the OLS estimates of the model coefficients are inconsistent. The regressors are said to be contemporaneously endogenous, and the resulting problem is often referred to as simultaneous equations bias. The beef demand functions provide a good example of how simultaneity arises.

All else equal, a random rise in the quantity of beef demand in any period (a shift of the demand curve) should cause a rise in the price of beef as a new market clearing equilibrium emerges further up the supply curve. This is the classic simultaneous equation problem. But in this instance all is not immediately lost. Beef meat is a major export product and domestic beef meat prices are primarily driven by the price prevailing on world markets, so there is reason to hope that this price can be taken as exogenously determined. The issue has to be resolved empirically.

Before going on to discuss tests of regressor exogeneity, it is important to highlight the scope of these tests. Some endogeneity is always present in economic systems. Tests for the exogeneity of regressors cannot reject endogeneity, but they can find that endogeneity is of sufficiently minor statistical concern that it is unlikely to produce inconsistent coefficient estimates.

Durbin (1954) proposed testing for exogeneity by comparing OLS estimates of model coefficients with instrumental variable IV estimates. The OLS estimates are consistent under exogeneity but inconsistent when endogeneity is present. The IV estimates are consistent in both situation. Thus, under exogeneity, the two sets of coefficient estimates are asymptotically equal, but they are not equal in the absence of exogeneity. This type of test was popularised by Hausman (1978) who proposed a variable addition procedure for estimating the test statistic to measure the distribution of the difference between the OLS and the IV estimates under the null hypothesis of exogeneity. An identical form of the test, proposed by Nakamura and Nakamura (1981), is reported below.

The test involves augmenting the OLS regression with residuals from the regression of each of the regressors suspected of endogeneity upon the set of available instruments, and then testing the joint significance of the added variables in the original OLS regression. The test has a reasonably simple intuitive basis. If the original regressors are indeed exogenous then these added regressors contain no new information and are asymptotically uncorrelated with the true disturbances, so behaviour under the null hypothesis is straightforward. Under conditions of endogeneity this is no longer the case. The test is expected to have good power since the added variables are an explicit measure of the potential endogenous component of the original regressors, that is, the part that is not explained by the instruments.[2]

The exogeneity of the meat price variables in the logarithmic demand function is Table 3b was tested by the above procedure. Instruments were constructed from sine and cosine functions displaying cycles of six months, one year, eighteen months, three years and five years, and also included linear trend and quadratic trend terms. The F-statistic for the joint significance of the five added regressors equals 1.61. The 95% critical value for the F-statistic is 2.18. Thus we accept the exogeneity of prices in the beef meat demand equation.

Exogeneity testing of the type described here presents ample opportunity for "cheating". The cheat is executed by the discerning choice of instrumental variables. Poor instruments (i.e. those weakly correlated with the original regressors) introduce a good deal of sampling variability into the estimate of the difference between the OLS and IV coefficient estimates. This weakens the power of the test and biases the results towards accepting exogeneity. The exogeneity test is asymptotically consistent, but in the sample size frequently encountered in applied econometrics, especially in macroeconomics, the sample size are not sufficiently large to remove the opportunity of judicious manipulation of the sort described above.

Before undertaking systems estimation of the five possible meat demand equations it is wise to test the exogeneity of the prices individually for each equation, since those demand equations for product not sold on an international market may exhibit simultaneity. Using systems estimation and applying cross-equation restrictions (such as symmetry of elasticities) when one equation suffers from endogeneity this can transmit the inconsistency in that equation to the other equations in the system. Systems instrumental variables estimators overcome this problem asymptotically, but frequently impart a large amount of additional sample variance in moderate size samples, which more than obviates the efficiency gains from sharing information across equations.

(VI) *Stability Testing*

The most frequently used test of stability is to enter dummy variables for sample periods when model parameters are suspected to differ from the norm. The dummy variables enter either as stand-alone variables which correct the intercept term of the model, or interacted with the varying regressors of the model to correct the slope coefficients. Structural change is tested by considering the statistical significance of the coefficients on the dummy variables.

A popular test is to break the time series sample into two sub-samples and to test the null hypothesis that the coefficients are equal in the two sub-samples. This is usually known among econometricians as the Chow test, following Chow (1960). This test is easily executed. It involves estimating the model on the entire sample and one sub-sample, and forming an F-test statistic from the error sum of squares.[3]

The Chow test relies on the regression disturbances having the same variance in the two sub-samples. Homoskedasticity of the disturbances can be tested by an F-test on the ratio of the estimated variances of the disturbances from the two sub-samples, or by an asymptotic Lagrange multiplier test such as described in Section V(3) where the artificial regressor is a zero-one dummy. The testing sequence is to test for homoskedasticity and then coefficient stability (see Phillips and McCabe, 1983).

Chow tests were performed on the logarithmic demand function. The sample was split into sub-samples containing 36 and 37 observations respectively. The F-test for homoskedasticity was $F(36,37) = 1.82$. The 95% critical value of this test statistic is 1.84 which suggests some prospect of homoskedasticity. The subsequent Chow test statistic was $F(9,73) = 1.70$. The 95% critical value of this statistic is 2.04. While there is no overwhelming evidence of structural instability, the F-statistics are not small, and so again mild concern is warranted.

Recursively estimated coefficients provide another source of insight into possible structural evolution in the model. Both forward and backward recursions are possible. A forward recursion starts with a small number of observations at the beginning of the sample and sequentially adds one observation at a time. The coefficients of the model are re-estimated as each observation is added. Plots of the estimated coefficients reveal how the estimated model is changing as the sample grows. In a stable model the coefficient estimates should remain reasonably unchanged after the sample reaches a respectable size. This is an informal testing procedure, but one which is frequently very useful for revealing distortions in the data or in the model.

The behaviour of the model as the sample is recursively increased can be formally evaluated using tests proposed by Brown, Durbin and Evans (1975). These tests are based on the cumulative sum of the recursive residuals (CUSUM) and on the cumulative sum of the recursive residuals squared (CUSUMSQ). The CUSUMSQ statistic breaks its 95 percent confidence interval, and is further evidence of some mis-specification or structural break.

The CUSUM and CUSUMSQ statistic confidence intervals are based on an underlying normality assumption. Practical experience suggests that the CUSUMSQ statistic is particularly sensitive to outliers and to severe non-normality in the regression disturbance. Thus it is possible to fail a CUSUMSQ test due to an outlying observation even when that observation has negligible leverage in the entire sample.

When the recursive coefficient estimates show that coefficients are evolving slowly, rather than changing sharply at discrete points in time, then simple tests can be devised by interacting a time trend with those coefficients which are thought to be time-varying. The appropriate test is an F-test on the additional coefficients created by the new interacted variable. This is conceptually the same as the ideas of slope and intercept dummies introduced in the beginning of this section of the paper. To illustrate, the five price variables and the expenditure variable of the Table 2b model were interacted with a linear trend. The resulting F-statistic on the additional six regressors was $F(6, 77) = 2.75$. The 95 percent critical value for this statistic is 2.20 and the 99 percent critical value is 3.08. Again the test is just over the margin of rejecting the null hypothesis of coefficient stability.

Non-Normality

Most of the diagnostic tests examined in this paper do not rely on normality of the regression disturbances in large samples.[4] As the sample grows the central limit theorem takes over and the asymptotic validity of the results is assured. It is never possible to know *a priori* what size sample is necessary for the validity of the asymptotic argument. Presumably, though, there are situations where the further the underlying disturbances are from normality the larger the sample that is required to justify the presumption of asymptotic normality.

Jarque and Bera (1981) have proposed a Lagrange multiplier asymptotic test for the normality of disturbances. This test correctly accounts for the fact that third and fourth moment must be estimated from the regression residuals rather than the true disturbances. Though useful in some contexts, the test does not answer the nagging question of whether the sample is large enough to justify asymptotic arugments since the test is itself valid only asymptotically.

One computer intensive means of establishing the approximate small sample properties of estimators is the "bootstrap", which does not involve assuming initial normality of the regression disturbances. This is a useful technique in the first instance since it is a direct means of establishing small sample coefficient confidence intervals. Also by comparing the small sample behaviour of the estimates with that inferred from asymptotic theory we get a direct sense of how well the asymptotic approximations are working.

In the "bootstrap", the empirical distribution function of the regression residuals is used as an approximation to the true distribution function of the regression disturbances. By drawing a random sample with replacement from the residuals of the original regression model and adding these one-by-one to the predictions of the dependent variable from the original regression, one creates a new sample of observations on the dependent variable with approximately the same distributional characteristics as those of the original sample. This new sample can be used to re-estimate the parameters of the model. Iterating on this process by repeatedly drawing new samples (with replacement) from the residuals of the original model allows us to repeatedly estimate the coefficients of the original model and so establish their sampling characteristics (see Effron and Tibshirani, 1986).[5]

The coefficients of the logarithmic demand function were bootstraped for 500 iterations. The estimates of each of the coefficients were sorted into ascending order and the empirical 95 percent confidence intervals were obtained. Table 10 compares the bootstrap confidence intervals with those implied from the t-statistics reported in Table 2b. Here we find a very close similarity betwen the two estimates of the confidence interval. This suggests the OLS coefficient estimators are very close to being normally distributed in the full sample of 93 observations.

Some authors, such as Theil et al. (1985), have recommended Monte Carlo methods which involve drawing random samples for the disturbances from a normal distribution with variance equal to that of the original regression residuals. This procedure is useful when samples are not large enough to justify the central limit theorem when it is in fact needed. However, they do not address the consequences of possible non-normality of the original disturbances.

TABLE 10

Bootstrap Ninety-Five Percent Confidence Intervals
(Logarithmic Demand Function) for 500 Iterations

Variable Coefficient	Bootstrap 95% Confidence Interval (Percentile Based)			Comparison Confidence Interval Implied by 1.96 Times the Sandard Error of the coefficient Estimate		
Log price of beef	-1.33	to	-1.09	-1.34	to	-0.99
Log price of chicken	-0.01	to	0.38	-0.03	to	-0.41
Log price of mutton	0.19	to	0.70	0.19	to	0.72
Log price of lamb	-0.53	to	0.14	-0.54	to	0.15
Log price of pork	0.57	to	1.12	0.55	to	1.16
Log total expenditure	0.34	to	1.12	0.33	to	1.15
2nd quarter dummy	-0.01	to	0.06	-0.02	to	0.04
3rd quarter dummy	-0.01	to	0.07	-0.01	to	0.07
4th quarter dummy	-0.20	to	-0.08	-0.20	to	-0.08
Constant	-5.46	to	0.69	-5.69	to	0.61

Summary

The logarithmic form of the demand function performs quite well against a large battery of tests. Two of the tests, the first differencing test (Table 8) and the CUSUMSQ test show failure of the model at conventional significance levels. In the absence of a superior alternative we might accept this model as an adequate representation of the demand function for beef. At the same time it is necessary to remain aware of the potential weakness of the model.

Yet our non-nested tests provide an excellent illustration of the importance of economic theory in applied research. The AIDS model is a recent contribution from the area of economic theory, and the evidence in the section above on Functional Form is that this model clearly dominates the linear and logarithmic models. Before 1980, the year the original AIDS paper was published, we would have been content with the logarithmic model. Now we can do better, indeed much better.

We are reminded that just because a model passes a series of diagnostic tests does not mean it is the *best* model. There is always the prospect that other insights from economics or elsewhere will lead one to propose a better model.

On the other hand, a model which seriously fails a number of diagnostic tests should be viewed with deep suspicion. In some cases there will not be another competitor available. If the flawed model must be used then it is helpful to report the nature of its weaknesses so that other researchers can improve on the economic or statistical theory or obtain access to better data.

VI. CONCLUSION

There is one ultimate motivation for this type of research. The economics profession has to improve the quality of the evidence it uses to sustain its public policy input and which it uses as the foundation of economic theorising.

Diagnostic testing is one means of identifying inadequacies in estimated models. When no superior model is available the tests can be used to help temper the reliance placed on the model.

Diagnostic testing can help create a clearer picture of where the problems lie in existing models. In this sense diagnostic testing is a positive activity which stimulates recourse to improved economic and statistical modelling.

The techniques discussed in this paper can be implemented with most modern econometric software. I hope that the necessarily simplified exposition in this survey encurages thoughtful adoption of these techniques in the profession.

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[1] When only one variable drives the heteroskedasticity or when the data can be ranked by likely level of heteroskedasticity it is possible to exploit the one-sided nature of the test to improve its power. See Evans and King (1988).

[2] The identical Hausman form of the test is a regression of the dependent variable on the predictions of the regressors after regressing them on the instruments augmented by the residuals of that regression. These residuals clearly measure the separate endogenous component of the regressors of the original model, and one tests if the coefficients on those residuals are jointly zero by a conventional F-test.

[3] This idea is extended in Ashley (1984) to multiple sub-sectioning of the data set.

[4] Exact tests, when available and when the normality assumption holds, can be most useful, for example, see Bera and McAleer (1983).

[5] A modified bootstrap procedure has been suggested by Hall (1987). This involves saving the t-statistics on the coefficients from each bootstrap iteration, and using the original regression estimates of the coefficient and its standard error to transform the empirical distribution of the bootstrapped t-statistics, to find tail probabilities for the coefficients.

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Paper 2

LABOR TURNOVER BIAS IN ESTIMATING WAGES

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Introduction

In this paper it is postulated that previous cross-sectional research using the earnings function is inadequate in its treatment of the relationship between unobserved ability and the probability of labor turnover. This relationship implies that individuals observed at a point in time are an unrepresentative sample of the cohort originally joining the firm, because of the nature and incidence of past quits and permanent lay-offs. Ordinary regression estimates of the earnings function are inconsistent. In particular, this is the case for the time-on-the-job coefficient, one of the more important parameters in the analysis of, for example, sex and race wage differentials. A novel use of instrumental variables estimators is proposed as a solution to the problem.

The paper proceeds as follows. Section II presents a brief and selective review of studies relevant to the analysis. Theoretical issues are examined in section III in order to motivate the empirical work. In section IV a solution to the problem is explained and as an example the technique is applied to a large sample of Australian government workers and the results are reported in section V. A concluding section summarises the major findings.

II. An Overview of the Literature

Bloom and Killingsworth's (1982) primer on the use of regression in pay discrimination research identifies the following problem. They refer to the testimony of a statistical expert witness, Dr. Paul L. Meier, in the trial of Presseisen v. Swarthmore (1977), who

> "questioned the reliability of cross-section studies of Swarthmore faculty who were employed at Swarthmore as of a given date on the grounds that such studies necessarily omitted "inactives" - faculty who were employed as of some previous data but were no longer employed as of the date referenced by the study." (p.337)

Our goal is to examine the implications of this view.

The issue is that because of attrition some proportion of the original cohort of workers joining a firm will have left by the time the cohort is observed in the cross-section. This constitutes a problem for econometric investigation of wage determination if workers' unobserved ability is not independent of the turnover decision. If this is the case, there is a correlation between unobserved ability and time in the firm (tenure). That is, as the original cohort ages, workers remaining become increasingly more homogeneous and increasingly less representative of the original group as either high or low unboserved ability workers quit. A form of selectivity bias is present and the parameter estimated for tenure in the wage equation is inconsistent.

For want of an adequate methodological treatment, research has proceeded in this area as if the problem did not exist. Corcoran and Duncan (1979) and Blackmore and Low

(1983), for example, treat tenure exogenously in analyses, respectively, of the role of work history on earnings differences by race and sex and in considering returns to on-the-job training of youth. Importantly for the point at hand, returns to tenure have significant explanatory power in these models.

Similarly, Blinder (1973), Filer (1983), Abowd and Killingsworth (1983) and Johnson and Lambrinos (1985) attempt to explain wage differentials between groups without reference to the effects of non-random quitting. All three analyses include tenure exogenously in the wage estimation, although Johnson and Lambrinos apply the usual Heckman selectivity bias correction to their data to minimise the influence of self-selection in or out of the labor force.

The problem applies also to less specific applications of the earnings function. For example, Medoff and Abraham's (1981) rejection of the efficacy of human capital theory in an explanation of earnings relies substantially on their finding that company experience is a significant salary determinant holding constant direct measures of productivity. But because the turnover selection process can generate upward or downward bias in returns to experience (see section III) their exogenous treatment of tenure implies their case is yet to be established.

There are many other examples of such uses of earnings data (Killingsworth and Reimers (1983), Griliches (1976), Duncan and Hoffman (1979), Shaw (1984) and Garen (1985)). Indeed, even investigations of earnings or wage determinants that do not include time on the job as an explanatory variable suffer from a variant of the same problem because the experience variable used, time in the labor force, is likely to be positively correlated with tenure, an issue analysed by Chapman and Tan (1980). No wage determination exercise can avoid the problem.

Some empirical research has treated time on the job as an endogenous variable in the wage equation (Chapman (1982), Chapman and Tan (1980)) but without formal investigation of the process. This research indicates some sensitivity of results to specification. More recently Beggs and Chapman (1985) have developed a model of worker self-selection which has important implications for the issue. The essence of this framework is presented diagrammatically in section III.

III. Wage Profiles and Labor Turnover

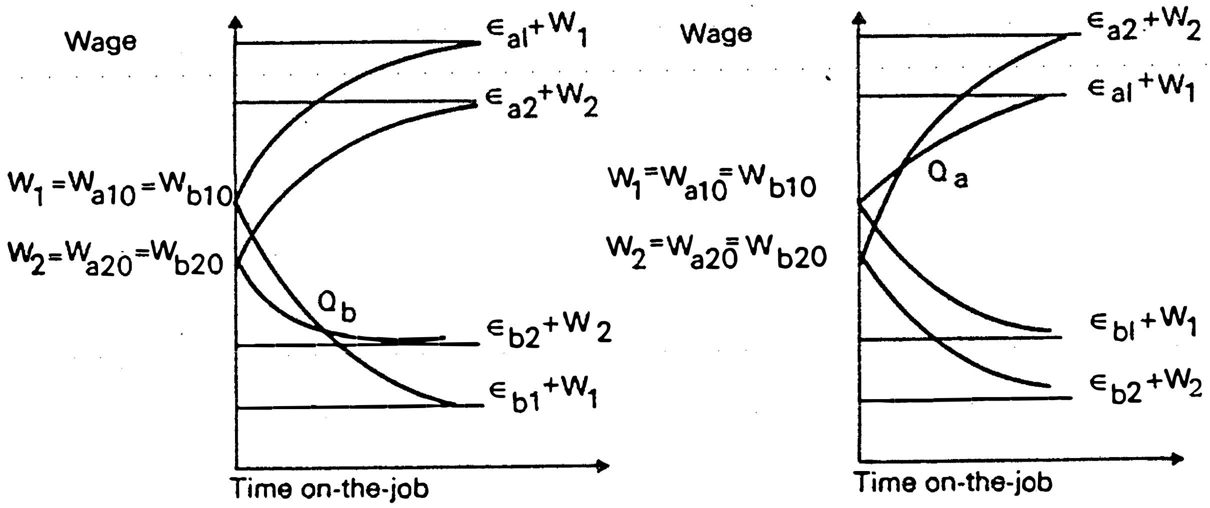
One way of understanding job turnover is to assume that firms and workers are initially poorly informed about the suitability of the worker in the current job, but over time both parties become more aware of the appropriateness of continuing the employment relationship. This is the essence of job mismatch models (Jovanovic (1979)) in which jobs are "experience" goods, where agents learn of the match of their ability with particular job requirements through having experience on the job. Extra dimensions can be added to this framework.

They are that, over time, workers gain, (i) greater insight into their general ability through experiencing a particular job, and (ii) knowledge of which employers reward their type of ability more. Our model, developed formally in Beggs and Chapman (1985), is thus in the flavor of a generalised job shopping framework, and the motivation is similar to that of Johnson (1978).

The basic issues can be illustrated simply. Figures 1(a) and 1(b) show possible wage streams for jobs 1 and 2. The wage stream is made up of an initial wage in each job W_1 and W_2 plus a payment for intangible ability. At the point of first hire the wage offer is for measured ability only.

FIGURE 1: WAGE PROFILES SHOWING RETURN TO ABILITY NOT OBSERVED AT TIME OF HIRE

FIGURE 1(a) FIGURE 1(b)



To help exposition, returns to tangible (measurable) characteristics remain fixed. This abstraction is not maintained elsewhere in the paper. Wages are initially higher in job 1 for both workers because the initial job choice decision requires this (i.e. we are specifically interested in persons hired to job 1).

Through time employers learn about workers' intangible ability and offer wages to them commensurately. Consider two workers, a type "a" and a type "b" worker. Let the true value of their intangible ability in the two jobs be (ε_{a1}, ε_{a2}) and (ε_{b1}, ε_{b2}). Three cases are relevant:

(i) Figure 1(a): Worker "a" has above average intangible ability in both jobs and worker "b" has below average intangible ability **and** these intangible characteristics are absolutely more important in job 1 than job 2. For this case, worker "b" quits job 1 at time Q_b. This means that the worker with relatively low ability for job 1 leaves. If this is a representative case then over time more of the low ability workers will have left so that in the cross-section the average intangible ability of those with high tenure will be greater than that of those with low tenure. A cross-sectional regression will show a **positive correlation** between wage and tenure that is partly due to the endogeneity of tenure.

(ii) Figure 1(b): Worker "a" has above average intangible ability in both jobs and worker "b" has below average intangible ability in both jobs **and** intangible characteristics are absolutely more important in job 2 than in job 1. In this case worker "a" quits at the point Q_a. Hence the worker with relatively high ability leaves job 1. This is the converse of case (i) and implies a flatter cross-sectional wage-tenure profile than if no self-selection existed.

(iii) A final set of cases correspond to the situation where workers with low intangible ability in one job have high intangible ability in the other job. The solution in this case is straight-forward. Low ability workers in job 1 quit to earn a higher wage in job 2, while high ability workers in job 1 have low ability in other jobs and hence stay. The implications for the cross-sectional wage equation are the same as those for case (i).

The firm's cross-sectionally observed wage structure is affected by which workers have quit in the time between hiring and sample observation. If workers with relatively low ability (low ε_{i1}) leave, the cross-sectionally observed wage-tenure profile is steeper than if no quitting occured. The converse is true if high ability workers leave. Just who quits depends on two things: (i) the magnitude of the rewards to ability in the two jobs; and (ii) the correlation between the value of ability in each job. (To highlight point (ii), negative correlation is possible since, for example, a person whose intangible skills are well valued in a large bureaucratic organization may be of questionable value in a small fast-moving corporation).

These cases mean that worker self-selection has ambiguous implications for wage equations estimated on job stayers. In case (ii) those workers with relatively high ability for the particular job are also those workers most likely to quit. This possibility is not embodied in conventional job mismatch theory, in which it is predicted that over time the average ability of workers remaining with the firm increases due to the attrition of less well-matched workers. The issues are addressed empirically below.

IV. Econometric Methodology

The essential point explained above is that if workers learn of their intangible worth with time on the job and, on the basis of that knowledge decide whether or not to quit, the usual assumptions of the regression model are not satisfied. If better workers (those with high intangible skills) quit, this induces a negative correlation between the disturbances and the tenure variables, resulting in downward biased estimates of the regression coefficients on tenure. The opposite is true if poorer workers are more likely to quit.

In this section a novel use of the instrumental variables (IV) technique for estimating the wage equation where the available data have been subjected to sample self-selection is explained. To demonstrate the strategy take the wage equation

$$W = X\beta + u \tag{1}$$

and rewrite it in deviation form as:

$$w = x\underset{\sim}{\beta} + (u - \bar{u})$$

where $\bar{u}$ is a vector containing the means of the in-sample disturbances and the parameter vector is partitioned as:

$$\beta = \begin{matrix} \beta_o \\ \underset{\sim}{\beta} \end{matrix}$$

with β_o the intercept term and $\underset{\sim}{\beta}$ the slope coefficients. The slope coefficients can be estimated consistently by identifying instruments, Z, which are asymptotically uncorrelated with the in-sample disturbances. That is:

$$\text{plim } Z'(u - \bar{u}) = 0 \tag{2}$$

This requirement is slightly different from the usual condition placed on instruments, namely that they be asymptotically orthogonal to the true disturbances of the regression model. The importance of this distinction is that in the IV procedure proposed here the regression intercept term is not consistently estimated by:

$$\hat{\beta}_{o_{IV}} = \bar{W} - \bar{X}\,\hat{\underset{\sim}{\beta}}_{IV} \,. \tag{3}$$

where $\overline{W}$ and $\overline{X}$ are the means of the wage and explanatory variables respectively. This is because $\overline{W}$ is not the unconditional mean of the dependent variable in the absence of sample self-selection. In many IV applications the slope coefficients are the central concern and inability to estimate the intercept consistently is of no consequence. But in some exercises, such as comparisons of male-female or black-white wage differentials, it is important to predict the average wage level without self-selection. In cases of this type it is central that the intercept term in the wage equation be estimated consistently.

In the problem studied here, a simple procedure is proposed. The idea is to select a region of the regressor space in which no turnover self-selection is present and re-estimate the model. The estimate of the intercept term from this sub-sample regression is then combined with the previously estimated slope coefficients to provide a consistent overall set of regression coefficients.[1]

In the situation at hand the economic model implies that the sample self-selection of the type discussed is related to tenure. This means that the wage equation can be estimated consistently only for that part of the sample where tenure equals zero. But, obviously, this regression does not reveal how the firm rewards its employees for tenure, a coefficient which is obtained consistently from the IV estimates.

Taking the intercept term from the sub-sample regression and combining it with IV estimates of the slope coefficients is not the most efficient available procedure. Since the intercept term itself is a long way from the mean of the data, it is estimated with a considerable amount of sampling variability. Hence a slightly different method was employed, which is now explained.

Let $\overline{W}^S$ and $\overline{X}_S$ denote the sample means of the wage and explanatory variables in the sub-sample not subject to turnover selectivity bias, and let $\hat{\beta}_S$ be the ordinary least squares parameter estimates from this sample. Denote:

$$\overline{W}^S = \overline{X}^S \beta_S \tag{4}$$

The predicted wage at the mean of the sub-sample is simply the average wage in the sub-sample. Denote:

$$\hat{W}^{S}_{IV} = \bar{X}_S \hat{\beta}_{IV} \tag{5}$$

where W^S_{IV} is the predicted wage from the IV estimates evaluated at the mean of the regressors in the sub-sample not subjected to self-selection. Asymptotically the estimable slope coefficients $\hat{\beta}_S$ and the corresponding elements of $\hat{\beta}_{IV}$ are identical. The difference between $\bar{W}^S$ and $\hat{W}^S_{IV}$ can only be due to the asymptotic inconsistency in the estimate of the intercept coefficient. That inconsistency is estimated here as:

$$\text{est}(\hat{\beta}_{oS} - \hat{\beta}_{oIV}) = \bar{W}^S - \hat{W}^S_{IV} \tag{6}$$

which is the difference between the mean wage in the sub-sample and the predicted wage by the IV procedure at the mean of the sub-sample. The actual predicting equation used in the analysis is thus:

$$\hat{W}_i = (\hat{\beta}_{oIV} + \bar{W}^S - \bar{\hat{W}}^S_{IV}) + \hat{\beta}_{1IV}X_{1i} + \ldots + \hat{\beta}_{kIV}X_{ki}\,. \tag{7}$$

V. An Application

The data chosen to illustrate the issue are the total set of full-time Australian government workers all employed in 1969 in what is essentially the same job, the clerical/administrative (CA) occupation. These are very useful data for several reasons. First, the ability requirements for success in the job are fairly clear-cut. They are skills concerned with the preparation of government reports and management of programs and personnel. The implication is that the usual mis-specification associated with applying a single earnings function to workers with different human capital formation paths is avoided. Secondly, there are a large number of observations allowing fairly confident statistical inference. Even so, the point of the exercise is not to emphasise wage determination and labor turnover processes in the Australian government at this time. Rather, it is to illustrate the potential importance of the theoretical and econometric issues with cross-section data, those commonly used for earnings function analysis.

To focus on a group most likely to have been involved in job shopping the analysis was restricted to workers aged less than 45 and with less than 16 years experience in the CA. This resulted in a sample of 14,870 men and 4,941 women, with information available on age (AGE), years of schooling (YOS),[2] time in the CA (OJEX) and annual salary (SAL).[3] The availability of age and YOS information allowed the calculation of total time in the labor force, TEXP, given by (age minus YOS minus 5).

The interpretation of TEXP as total time in the labor force assumes that all schooling was completed before labor force participation began, that work immediately followed the completion of schooling, and that labor force participation was continuous once commenced. The relative intermittency of women's labor force participation implies that TEXP is an overstatement of true experience for this group in particular. This results in important econometric and interpretation difficulties (Chapman (1985)). Consequently, women's TEXP was adjusted downwards slightly in order to gain a closer approximation of true experience.[4]

The statistical characteristics of the data are shown in Table 1.

TABLE 1. Statistical Characteristics of the Data

	Men		Women	
	Mean	Standard Derivation	Mean	Standard Derivation
SAL ($A)[5]	4236.80	1362.80	3040.40	931.10
AGE (years)	25.90	6.47	22.32	4.07
YOS (years)	12.47	1.12	12.28	0.90
OJEX (years)	4.15	3.95	2.19	2.50
TEXP (years)	8.43	6.38	4.56	3.70

These data allowed the estimation of several models, all of which are variants of the traditional wage equation. Several modifications were incorporated to the usual form. First, for flexibility the equation was estimated with fourth order polynomials in the tenure and general labor market experience variables.[6] Second, a quadratic was included on the schooling variable to allow non-linearities. Third, returns to formal education were allowed to depreciate by interacting schooling (quadratically) with length of time since its completion. This permits a test of the hypothesis that formal schooling becomes less important as a promotion determinant as the vintage of education increases.

The above innovations led to the estimation of the following model:

$$\begin{aligned}\log(SAL_i) = {} & \alpha_o + \alpha_1 YOS_i + \alpha_2 YOS_i^2 + \alpha_3 OJEX_i + \alpha_4 OJEX_i^2 + \alpha_5 OJEX_i^3 \\ & + \alpha_6 OJEX_i^4 + \alpha_7 TEXP_i + \alpha_8 TEXP_i^2 + \alpha_9 TEXP_i^3 + \alpha_{10} TEXP_i^4 \\ & + \alpha_{11} TEXP_i.YOS_i + \alpha_{12} TEXP_i^2.YOS_i + \varepsilon_i\end{aligned}$$

The predictions of this model are that $\alpha_1, \alpha_3, \alpha_5, \alpha_7, \alpha_9, \alpha_{12} > 0$ and $\alpha_2, \alpha_4, \alpha_6, \alpha_8, \alpha_{10}, \alpha_{11} < 0$.[7]

The IV procedure was made operational by the assumption that the learning associated with intangible ability is solely a function of tenure. Thus, the self-selection bias in the wage equation is ascribed to the correlation between the disturbance of the wage equation and the tenure variables. The wage equation disturbances were assumed to be asymptotically uncorrelated with educational qualifications and outside experience. A caveat is that learning about intangible ability may be associated with these variables. However, if the dominant effect is due to the learning associated with tenure the assumption provides a workable basis on which to proceed.

The IV method used was as follows. All the variables in the basic wage equation which contain tenure ($OJEX^{1\text{-}4}$, $TEXP^{1\text{-}4}$, TEXP.YOS and $TEXP^2$.YOS) were instrumented from the schooling and outside experience variables. The generalised instruments were obtained by reference to the unit record data on the type of educational qualification held by each individual (e.g. high school, 3-year Bachelor's degree, Honor's degree, Master's degree, etc.) and using 16 dummy variables for each year of "outside" work experience (TEXP minus OJEX). The correlation between tenure and the schooling and "outside" experience variables is relatively low, hence the final instrumental variables estimates of the regression equation are subject to a reasonable amount of sampling variability.[8] The OLS and IV estimates of the wage equation for men and women are presented in Table 2.

The OLS results reported in Table 2 are as predicted. The coefficients on the experience and schooling variables display concavity, having negative signs on the even-order polynomials. Interacting schooling with time since entering the workforce shows the anticipated depreciation with depreciation occurring at a decreasing rate. All other coefficients give plausible results and their implied sizes are reported in Table 3. The R^2 s of 0.68 and above are high, indeed unusually so, for such cross-sectional estimations.

TABLE 2*.
OLS AND IV ESTIMATES OF THE WAGE EQUATION

	Men		Women	
Variable	OLS	IV	OLS	IV
INTERCEPT	5.508 (21.28)	4.953 (9.71)	3.647 (4.94)	4.521 (3.39)
OJEX	.122 (28.26)	.244 (2.26)	.109 (15.41)	.185 (1.12)
$OJEX^2$	-.0220 (15.65)	-.0382 (1.19)	-.0174 (6.18)	-.0378 (0.59)
$OJEX^3$	.00190 (11.90)	.00224 (0.67)	.00124 (3.37)	.00389 (0.52)
$OJEX^4$	-.0000574 (9.98)	-.0000411 (0.37)	-.0000291 (1.99)	-.000134 (0.49)
TEXP	.260 (23.55)	.291 (1.60)	.369 (16.63)	.320 (2.40)
$TEXP^2$	-.0218 (27.70)	-.0322 (2.54)	-.0345 (20.25)	-.0298 (3.15)
$TEXP^3$	.000820 (21.36)	.00170 (3.45)	.00136 (14.38)	.00132 (3.00)
$TEXP^4$	-.0000129 (17.80)	-.0000283 (3.04)	-.0000236 (11.14)	-.0000204 (2.37)
YOS	.192 (5.03)	.223 (3.59)	.428 (3.92)	.276 (1.43)
YOS^2	-.00214 (1.54)	-.00318 (1.11)	-.0101 (2.54)	-.00455 (0.64)
TEXP.YOS	-.00617 (7.69)	-.000303 (0.02)	-.0119 (6.98)	-.00590 (0.60)
$TEXP^2$.YOS	.000253 (7.12)	-.000210 (0.32)	.000576 (6.27)	.0000768 (0.15)
R^2	.684	.200	.696	.160
F	2681.6	.310.1	940.9	77.4
N	14,870		4,941	

* Absolute t-statistics in parentheses.

TABLE 3
Percentage Increase in Salary Associated with a One-Year Increase in Independent Variables, Calculated at the Mean
(from Table 2)

	Men		Women	
Variable	OLS	IV	OLS	IV
OJEX	5.09	9.27	6.45	10.31
TEXP	4.20	4.79	7.21	8.29
YOS	11.46	11.77	14.38	13.92

The IV coefficient estimates of the wage equation are similar in sign to those found for the OLS although the standard errors are large, a result probably attributable in part to the narrow set of instruments available. The important finding concerning the relative parameter sizes is stressed below.

The OLS and IV estimates reveal that, for these data, treating tenure exogenously leads to a substantial underestimate of the true effect on wages of experience on-the-job. At the mean for males and females respectively, a one year increase in tenure increased wages by 9.3 per cent and 10.3 per cent in the IV estimation, compared to 5.1 per cent and 6.5 per cent from the usual technique. The results thus indicate that workers of both sexes with relatively high levels of intangible ability were more likely to quit. Clearly, for these data, labor turnover was not random. This suggests that earnings functions estimated as if tenure is exogenous have within them the scope for serious misinterpretation of relationships.

The above result implies the possibility of important differences in the OLS and IV-corrected predictions of average wages. The adjustment necessitated on the intercept term of the IV equation to ascertain the true gap proceeded as follows.

The OLS equation (8) was estimated for the sample of men and women workers for whom OJEX equals 0, using the mean of the data for other variables, to derive a selection-free estimation of the starting wage. These data were used to adjust the IV equation by the amounts shown in Table 4.

TABLE 4

OLS Selection-Free and IV Predicted Starting Wages

Men			Women		
(i) Average Starting Wage, W^S	(ii) IV Predicted Starting Wage W^S_{IV}	(iii) Adjustment Factor (i - ii)	(i) Average Starting Wage, W^S	(ii) IV Predicted Starting Wage,	(iii) Adjustment Factor (i - ii) W^S_{IV}
2770.4	2314.5	455.9	2289.7	2257.6	32.1

The results of Table 4 demonstrate that, for these data, the IV technique understates the selection-free starting wage by about 465 and 32 dollars per year for men and women respectively. That there is an understatement is consistent with the finding that the IV OJEX slope coefficients are higher than the OLS OJEX slope coefficients for both men and women. The fact that the adjustment is greater (in percentage terms) for men than women implies that the relative disparity in intangible ability of quitters and stayers is higher for the former group. This may be due in part to the relatively low average tenure of females, which suggests that the selection process is less complete for this group.

The usual OLS and IV-corrected estimates may now be used to investigate the empirical significance of the inappropriate application of OLS to the question of predicted average wages. As noted, this is an important issue for the measurement of sex and race discrimination, since standard OLS techniques can lead to incorrect predictions.

The OLS and IV-corrected wage predicted for males are $4237 and $4693 respectively, and for females the figures are $3040 and $3072. That is, the incorrect method understates true male and female predicted wages by 10.8 and 1.1 per cent respectively. This implies that the male-female average wage differential is increased from $1197 to $1621, or from 39.4 to 52.8 per cent. This is an important variation highlighting the relevance of the suggested correction for these data. We stress that results could be quite different for other samples.

VI. Conclusion

It has been argued that if a relationship exists between labor turnover and intangible ability, the OLS parameter estimated between tenure and wages is inconsistent. No **a prior** predictions can be made about the sign of the bias since it depends, in the model developed, on both the correlation between workers' ability inside and outside the firm, and the relative variance of firms' wage structures. For the data examined it was found that relatively able men and women workers were more likely to quit, a result implying that OLS cross-section estimations not correcting for this type of self-selection systematically underestimate the returns to on-the-job experience for both sexes. Other results may be true for different firms.

A technique was developed to correct the wage equation and estimates of predicted average wages for the presence of non-random quitting. For the sample considered several points stand out. First, OLS rates of return to on-the-job experience were substantially less than those derived with the selectivity-corrected process. Secondly, the OLS technique understates importantly the male-female wage gap predicted at the mean. Finally, the recommended technique allowed insight into the labor turnover process, a possibility previously not exploited with cross-section data.

---

[1]The suggestion is trite in the case where there is a large enough sub-sample of the rgeressors where there is no self-selection and where there is sufficient variability in the regressors themselves. In such a case one would only run the regression on the sub-sample not subject to self-selection and not be concerned with the IV procedure.

[2]The years of schooling variable was computed by inferring time equivalents from educational qualifications. Information on the distribution of qualifications and the assumptions imposed in order to derive YOS is presented in the Appendix.

[3]Annual salary data as such were unavailable from the sample. They were inferred by taking the midpoint of the (fairly narrow) salary range of each individual's job class.

[4]The adjustment estimate was calculated from the data under a set of restrictive assumptions. Time in the labor force before joining the APS was assumed to overstate true experience by $1/\alpha$, with $0 < \alpha < 1$, where α is the proportion of women remaining in employment each year from 1969 to 1974. Given that is is possible to identify which individuals were not employed in the CA in 1974 but nevertheless were in 1969 an approximation of α is given by $(1 - x/y)/5$ where x is the number of leavers by 1974 over the original population y. In other words, the average general labor market experience accumulation of women prior to 1969 is assumed to be given by the proportion of each year, on average, that the original population remained in employment. For this example, α is .904.

[5]To facilitate understanding of the orders of magnitude involved, note that in 1969 \$A1 = \$US1.1. From 1969 to 1986 Australian wages increased about six-fold in nominal terms.

[6]We adopted this approach because, while the results of estimating the usual earnings function were statistically satisfactory - in terms of explanatory power and significance tests - a particular characteristic was discomforting. Predicted wages turned down at tenure levels of around 10 years, and did so rapidly after this point. While a turning point is commonly found in cross-sectional estimates, the rapidity of decline was unsatisfactory. We recommend this type of diagnostic testing for other researchers in the area.

[7]The conventional model in the literature sets $\alpha_2=\alpha_5=\alpha_6=\alpha_9=\alpha_{10}=\alpha_{11}=\alpha_{12}=0$. Results for this and an intermediate range of models are available from the authors on request.

[8]The R^2 statistics for the regression of the tenure variables on the 16 outside experience dummies and the 9 schooling dummies lie between .06 and .20. In cross-sectional studies it is rarely possible to find the highly correlated instruments which are familiar in time series macroeconomic applications.

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Appendix

Education Qualifications of the Sample

Qualifications	Assumed Year of Schooling Equivalent	Per Cent of Sample	
		Men	Women
High School Graduate	12	82.62	90.75
Diploma	14	4.10	1.32
Bachelor - Ordinary	15	10.56	6.94
Bachelor - Honors	16	2.22	0.83
Masters	18	0.45	0.16
Doctorate	20	0.054	0

Paper 3

RIGID REWARD RULE RAMIFICATIONS

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INTRODUCTION

Recently, there has been extensive public discussion concerning salary structures in the Australian Public Service (APS). There are (at least) two dimensions to this question: the appropriate wage differential between different job classifications, and whether or not there is sufficient flexibility in the APS to reward (or punish) particularly talented (or incompetent) workers. This paper focusses on the latter possibility, it being argued that large bureaucracies —particularly of the public sector variant—are relatively unable to offer wage flexibility. The important and generally unexplored implication here is that rigid reward rules (such as seniority based promotion) will encourage the most able employees to quit, and encourage the least able employees to remain.

The ability-attrition nexus is explored using a large cross-sectional sample of APS male employees observed in 1969. The major finding is that, *ceteris paribus*, had they stayed the expected wages of those who quit the APS prior to 1969 were *very much* higher than the expected wage of those who stayed. We conclude that a relatively high proportion of the most talented employees had resigned and a relatively high proportion of the least talented employees had stayed, a finding which is the opposite to that predicted in conventional job match theory (Jovanovic, 1979). One explanation for this is that in 1969 the Australian public service probably had limited scope to wage reward good performers, and was restricted in the extent to which it could wage punish poor performers. The basic finding has implications for all employers in the setting of wage and salary packages, and is pertinent to the current public debate concerning public sector employee attrition.

The conceptual framework for addressing this question has been developed in the literature dealing with female wage rates (Heckman, 1979), education decisions (Willis and Rosen, 1979) and comparative wage performance of immigrants (Borjas, 1987). The modelling problem is to infer from those currently employed with a given firm the average wage rates had all persons originally employed by the firm not quit. Since we are interested in whether all persons ever employed by the firm are of higher or lower average quality than those who remain with the firm, the problem is resolved by including information on past job quit rates in the regression model relating wage rates to worker characteristics.

EFFECTS OF QUITS ON OBSERVED WAGES

The wage or salary reward paid to a worker can be thought of in two parts. The first is the reward due to the objectively measurable characteristics of the individual, which include educational attainment, years of workforce experience and years of experience with the current employer. In some instances more detailed work histories and supervisor evaluations are available, but outside analysts normally do not have access to such information. The second component of the wage rate is the return to the unmeasured worker attributes which include intrinsic intelligence, aptitude for the tasks of the firm, initiative, energy and motivation.

Formally, the wages of the individual, W_i, may be written:

$$W_i^c = f^c(\text{Measured Attributes}_i) + u_i^c \qquad (1)$$

where $f^c(.)$ is a function of the measured characteristics and u_i^c represents the wage return to unmeasured worker attributes. The superscript "c" refers to the wages for the individual within the current firm. The term u^c is normally thought of as having an average value of zero implying that those with above average unmeasured skills have a positive value, and below average skills persons have a negative value.

Since each individual has alternative employment opportunities it is also useful to characterise the wage rate the individual might receive in his/her next best job prospect. Thus,

$$W_i^a = f^a(\text{Measured Attributes}_i) + u_i^a \qquad (2)$$

where the variables are defined as above, and the superscript "a" refers to next best alternative employment.

Workers move between jobs according to how employers reward both their measured and unmeasured attributes with job movement associated with measured characteristics being easiest to understand, and can be explained with the following example. A firm moving into an expansionary phase or into higher technology production processes or product markets may have a need for more highly educated workers (Bartel and Lichtenberg, 1987). If so, it bids staff away from other employers by raising the wage reward for education (which is a measurable attribute). Presumably sorting of individuals among firms occurs regularly in response to special needs for individuals with specific skills and other measurable attributes.

Unmeasured attributes also play an important role in the movements of workers, and this paper may be understood to be an analysis of a specific case. Consider a current employer firm, c, which is constrained in its powers to reward (by wage increases) especially well motivated or innovative employees, and constrained in its powers to punish (by wage cuts) workers who show an unduly low level of motivation or interest in their work. Employers most easily characterised in these terms include the various levels of federal, state and municipal government, but other large firms may have relatively inflexible wage policies as a consequence of the high costs of monitoring the performance of individual workers (Lazear, 1978).

Since governments are the custodians of the public purse they may be politically constrained not only to act fairly in rewarding their employees, but also be *seen* to be acting fairly. This probably leads to a reward system which focuses primarily on the measurable (and verifiable) attributes of the employees, implying that characteristics such as seniority and formal educational qualifications are relatively important in the decision to promote individuals to higher wage scales. Conversely, the intrinsic political nature of government implies the introduction of a portfolio of measures which protect the individual employee from political harassment or discrimination. As a consequence, at most levels of government it may be difficult and politically expensive to demote or dismiss staff, even though in many circumstances there may be a moderately strong economic case for doing so.

The employer with rigid wage reward rules is less likely or even unable to offer such incentives as project bonuses for a job well done, generous bonuses for ideas which raise firm level productivity, or very rapid promotion on the basis of high levels of worker initiative only. The constrained employer also finds it difficult to demote, fine or dismiss lazy and somewhat incompetent workers. Thus the conjecture is that when an alternative unconstrained employer is present workers with high unmeasured ability are more likely to quit to this alternative firm where their skills will be more fully rewarded. Also, workers with poor unmeasured attributes are more likely to stay with the current employer since they would be relatively punished by the alternative more flexible employer. The important point from the story is that over time the average quality of a cohort of workers originally joining the firm with rigid reward rules will fall.

To get an empirical handle on the extent of these effects it is necessary to reconsider equations (1) and (2). A worker leaves his/her current employment if $W^a > W^c$, that is, the alternative employer pays more. But these quits have implications for statistical estimation of the wage equation (1) with the problem being summarised as follows. A sample of workers in the firm, c, at any point in time only includes those workers who have not already quit. Equation (1) is written as follows, where the asterisk on the variable refers to the fact that they have not as yet quit the firm and so are still observed.

$$W_i^{c*} = f^c(\text{Measured Attributes}_i) + u^{c*} \qquad (3)$$

The disturbance term on equation (3), u^{c*}, is no longer simply random. Since the worker stays with the firm only if $W_i^c > W_i^a$ then it must be the case the $u^{c*} > f^a(.) - f^c(.) + u^a$. The expected value of u^{c*} cannot be zero.[1] Equation (3) may be re-written as follows:

$$W_i^{c*} = f^c(\text{Measured Attributes}_i) + K + \varepsilon_i$$

where ε_i is a zero mean random variable, and K = [Expected value of the unmeasured characteristics of individuals still with the firm].

If K is negative the average ability of those still with the firm is less than the average ability of those who started. In the following section of the paper we estimate $f^c(.)$ and K, concluding that for this sample the latter is a large negative quantity.

DATA AND MODEL ESTIMATION

The data consist of information on full-time male clerical/administrative (CA) officers employed in the Third Division of the Australian Public Service (APS) in 1969. The individuals involved performed public sector white collar duties such as the preparation of reports on various aspects of government, and the administration of government projects and personnel.

In order to focus on what is arguably a single job, CA officers of a more specialised variety were excluded from the analysis.[2] Also, in order to distinguish voluntary turnovers from age or invalidity retirements, the analysis was limited to individuals aged less than 45 years in 1969. Further, the analysis was confined to workers who had been on the job for less than 16 years in 1969.[3] This left a sample of 14,870 workers.

Data were available on the following characteristics: age, number of years in the CA (OJEX), job level and education qualifications. Thus it was possible to infer annual salary (SAL) by taking the midpoint of the salary range for the job level of the individual. Further, educational qualifications were converted to year-of-schooling equivalents (YOS). From the YOS and age variables it was possible to infer the length of time individuals had been in the labour force (TEXP). Assuming that all schooling was completed before labour force participation began, that work immediately followed the completion of schooling and that labour force participation was continuous once commenced, the following formula may be used:

$$TEXP = AGE - YOS - 5.^{4} \qquad (5)$$

Statistical averages and standard deviations for the sample are reported in Table A1.

A traditional earnings equation was then specified. The disturbance term on this regression, ε_i, captures the individual worker's reward for unmeasured ability. The estimated functional form is:

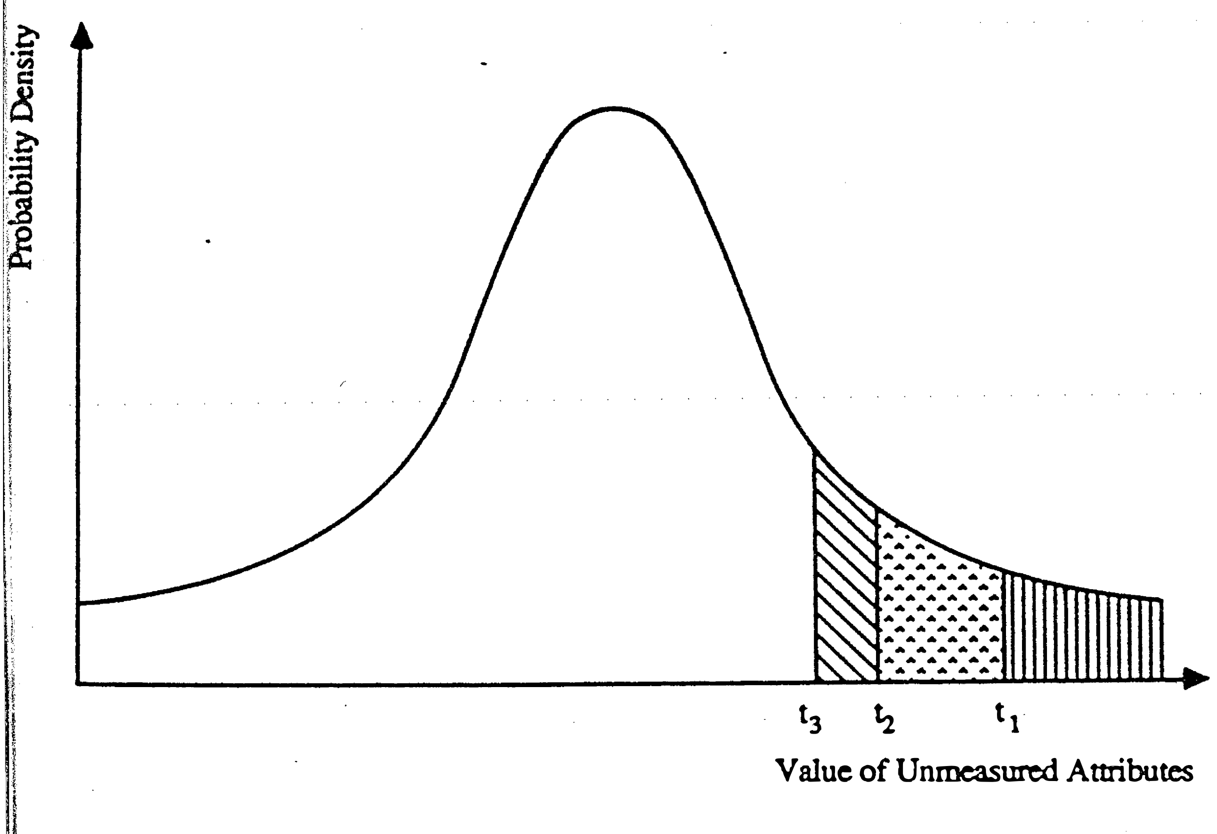
$$\begin{aligned}\log(Wage_i) = \; & \alpha_0 + \alpha_1 OJEX_i + \alpha_2 OJEX_i^2 + \alpha_3 OJEX^3 + \alpha_4 OJEX^4 \\ & + \alpha_5 TEXP_i + \alpha_6 TEXP_i^2 + \alpha_7 TEXP_i^3 + \alpha_8 TEXP_i^4 \\ & + \alpha_9 YOS_i + \alpha_{10} YOS^2 + \alpha_{11}(TEXP_i \, . \, YOS_i) \\ & + \alpha_{12}(TEXP_i^2 \, . \, YOS_i) + K + \varepsilon_i \qquad (6)\end{aligned}$$

It is anticipated that earnings will increase with schooling, general labour market experience and job tenure. The increase in earnings is anticipated to occur at a decreasing rate to reflect both the depreciation of skills with time and the likelihood that worker investments in acquiring new skills and staying up-to-date decrease as retirement approaches. In general the standard hypotheses on earning equations predict that $\alpha_1, \alpha_3, \alpha_5, \alpha_7, \alpha_9 > 0$ and $\alpha_2, \alpha_4, \alpha_6, \alpha_8, \alpha_{10} < 0$. In this specification the effect of formal education has been allowed to depreciate by interacting it quadratically with length of time since completion of education. This functional form allows for the hypothesis that formal schooling becomes less important as a promotion determinant as the vintage of education increases. Thus it is anticipated that $\alpha_{11} < 0$ and $\alpha_{12} > 0$.

Initially the model was estimated under the assumption that K = 0, that is, that worker quits do not affect the average wage of those who are still with the firm with the regression results being reported in column one of Table A2. The model shows the expected signs on coefficients and generally displays high levels of statistical significance. This regression was then used to construct a novel test of the effects of quitting, which is now explained.

At the time workers are hired some workers have above average unmeasured characteristics and some are necessarily below average, with this information being carried in the ability probability distribution. The relevant concern is what happens to the probability distribution of ε_i as workers' tenure with the firm increases, an issue clarified through consideration of Figure 1.

Figure 1: Changes in Skewness of the Value of Unmeasured Attributes if More Able Workers Quit.



The diagram shows a probability density function for ε_i assuming all workers are still with the firm. Now suppose that in the first year of tenure the most able workers (in terms of unmeasured characteristics) quit. That is, those persons to the right of t_1 disappear from the observable sample. As years of potential tenure increases more workers quit from the top end of the distribution, so it becomes progressively more truncated, as shown by the shading on the diagram.

For heuristic reasons the diagram illustrates the extreme case, since in practice there will be quitting by all types of workers, even those with low unmeasured ability. However, if the *dominant effect* is that relatively more workers with the highest unmeasured ability quit first, the *distribution of ε_i becomes progressively more negatively skewed as tenure increases.*

Skewness of a residual is measured by the behaviour of the distributions' third moment (Mood, Graybill and Boes, 1974). In this case the tests are performed by examining the behaviour of the cube of the regression residual from the wage equation denoted $\hat{\varepsilon}_i^3$. The test procedure is to form a new regression to find what factors explain $\hat{\varepsilon}_i^3$ with the central hypothesis being that skewness becomes more negative as tenure (OJEX) increases.

The hypothesis was tested by regressing $\hat{\varepsilon}_i^3$ on the outside experience and tenure variables. A new variable OJO equal to one for years of tenure less than one, and zero otherwise, was included in the estimation and found to be important. The variable GEXP equals (TEXP - OJEX) and is years of work experience before joining the APS. The result is shown below, with absolute t-statistics given in parentheses:

$$\hat{\varepsilon}_i^3 = \underset{(2.22)}{0.000988} + \underset{(2.18)}{0.00145}\ \text{OJO} + \underset{(2.39)}{0.000117}\ \text{GEXP} - \underset{(4.39)}{0.000285}\ \text{OJEX}$$

$$F = 15.68$$

The result suggests that there is some positive skewness in the valuation of workers' ability at the time of hiring, given the coefficient on OJO, and that this is larger the higher the worker's prior employment experience. But most importantly, the *skewness becomes more negative as tenure increases.* This suggests that it is workers with a high level of unmeasured ability who are more likely to quit, their attrition shortening the tail of the distribution at the top end causing it to skew towards low levels of unmeasured ability.

A second, and independent, approach taken to validating the hypothesis that the best workers quit involved introducing the term K into the regression model which corrected for the loss of the right-hand tail of the distribution shown in Figure 1. K, in effect, re-centres the distribution to allow for quitting the formulation of the statistical correction term being explained in Maddala (1983).[5] The regression results for the full model in equation (6) are reported in column 2 of Table A2.

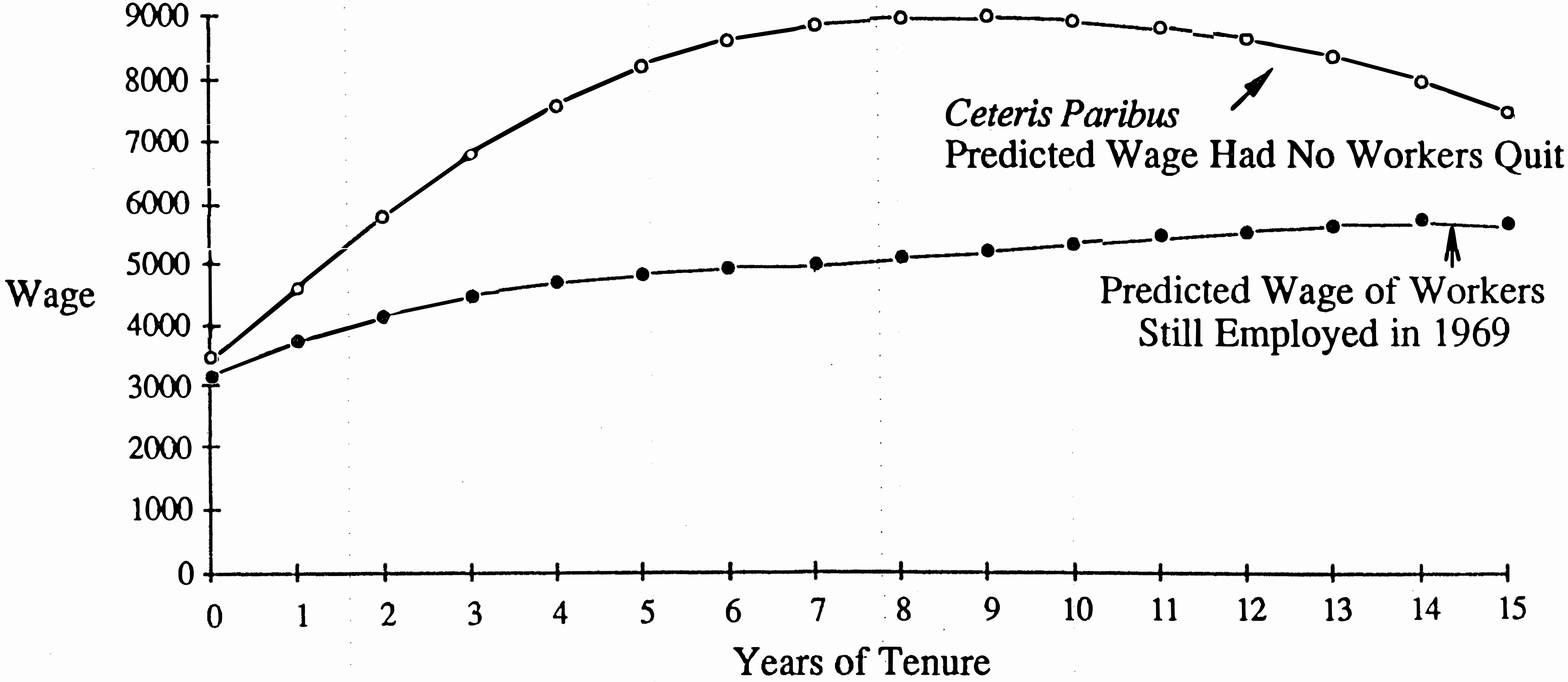
Because of the complexity of the model it is difficult to directly interpret the meaning of individual coefficients, particularly given that K was interacted with many variables to allow a flexible functional form. However the essence of these models is distilled in Table A3 and the accompanying Figure 2. The table shows the predicted wage profile of the actual sample of APS workers in 1969, along with that estimated *ceteris paribus*, had no workers quit, the profiles being constructed for a worker who enters the APS with the sample average level of schooling and prior workforce experience. The big point highlighted is that had those workers who quit actually stayed the average wages of the sample would have been much higher than that observed in 1969. This is interpreted as strong evidence that it is the better workers, in the sense of having more valuable unmeasured attributes, who were quitting the APS before 1969.

An alternative interpretation of the results is that the APS wage hierarchy had a pyramidal structure in 1969. This possibility implies negative skewness of the residuals since as the tenure of an entering cohort increases, workers are more bunched in lower paying positions. For the following empirical reason the existence of the wage pyramid as an explanation of the results is not compelling. The aggregate quit rate data illustrate that practically all voluntary labour turnover occurs in the first 5 years of tenure (about 35 per cent compared to a total of only 44 percent after 15 years). Thus if the attrition hypothesis developed in the paper is convincing, it should be the case that most of the inferred wage consequences show up relatively early in the data. This is clearly the case from Figure 2. For the alternative hypothesis to be powerful it would be expected that the wage profile differences grow systematically with tenure, which is not so.

It is important to emphasise the *ceteris paribus* nature of the expected wage rates in Table A3. The predictions describe a hypothetical case in which there are no repercussions upon the wage rate of a change in quit rates. But if all those relatively high quality workers had indeed stayed in the APS there would have been a over-supply of such people which may have operated to decrease their wage advantage somewhat. This possibility does not invalidate the basic point since the issue is whether or not overall average wages would have been greater.

Finally, a caveat is that the analysis concerns male worker quits from the Australian public service prior to 1969. There may have been major changes to public service promotions and appointments procedures in the past two decades, so it would be unwise to infer the full force of the findings of this paper to the institutional framework in 1988. On the other hand, the recent rapid expansion in alternative employment opportunities which are likely to be characterised by more flexible wage systems—such as in the burgeoning financial sector—implies a strengthening of the 1969 tendencies.

Fig. 2: Predicted Wage-Tenure Profiles



CONCLUSION

The inability to flexibly reward workers for their efforts is likely to encourage a relatively high proportion of the workers with the best unmeasured attributes to move to firms with more flexible wage and salary structures. However, it may still be rational for employers to choose to operate a relatively inflexible reward system, a decision which may be made to meet political requirements, as in the case of government, or to reduce the substantial costs of monitoring work performance which are necessary to run a truly flexible wage and salary system.

The empirical findings of this paper suggest that the loss of high quality staff in a rigid wage and salary environment is substantial. There seems to be a clear need for a more detailed case study micro-analytic investigation of this issue, a recommendation influenced by the considerable implications for a firm of having many of its best workers leaving prematurely.

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[1]Its expected value varies with the measured attributes of the employee, the variance of u^a and u^c and their covariances.

[2]For example, Auditors, Computer Systems Officers, Naval Stores Officers and Interpereter/Translators. Dr George Rothman clarified this issue.

[3]There is evidence that workers joining the CA in the post WW II and Korean War periods (i.e. 1945-1953) received promotion advantages not available to later entrants. In order not to confuse the wage analysis with this issue, and to focus on workers more likely to be experiencing job-shopping, those joining the public service before 1954 were excluded.

[4]See Mincer (1974).

[5]Assuming the disturbance terms are normally distributed the conditional expectation of the disturbance term from equation (1), given the worker has not quit, can be written:

$$K = \sigma \frac{\phi_i}{\Phi_i}.$$

where Φ_i is the probability that the worker is still with the firm in 1969, ϕ_i is the density function of the normal distribution function where the distribution function has value Φ_i, and σ is a parameter which depends on the relative variance of u^c and u^a, and their covariances. The variable Φ_i represents aggregate quit rates for workers depending on their tenure. The ϕ_i was then inferred from the estimate of Φ_i using statistical tables. The cumulative probability of having quit by the end of any tenure period was estimated from aggregate male quit rates by tenure of Australian Public Service workers for 1973, obtained from the *Public Service Annual Report, 1973.*

To capture the fact that the variability of rewards to unmeasured attributes (the variance of u_i^c and u_i^a) change as the individual ages and as he/she acquires more firm skills we have modelled σ in the quite general form:

$$\sigma = (a_0 + a_1 OJEX + a_2 OJEX^{3/2})(b_0 + b_1 AGE + b_2 AGE^{3/2}).$$

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TABLE A1
STATISTICAL CHARACTERISTICS OF THE SAMPLE

Variable	Mean	Standard Deviation
AGE (years)	25.91	6.36
W ($ earning per annum)	4434	1346
YOS (equivalent years of schooling)	12.47	1.13
OJEX (years working with APS)	4.18	3.84
TEXP (years in the workforce)	8.42	6.28

TABLE
ESTIMATED COEFFICIENTS FOR THE 1969 WAGE EQUATION WITH AND WITHOUT A CORRECTION FOR JOB QUITS

	Model 1		Model 2	
	Estimated Coefficient	t-statistic	Estimated Coefficient	t-statistic*
INTERCEPT	5.507	(21.29)	4.245	(11.72)
OJEX	.124	(28.39)	.232	(1.26)
$OJEX^2$	-0.0288	(15.98)	-0.0343	(-0.67)
$OJEX^3$	.00199	(12.27)	.00223	(1.12)
$OJEX^4$	-0.0000604	(10.37)	-0.0000578	(-1.44)
TEXP	.260	(23.52)	.351	(18.75)
$TEXP^2$	-0.0218	(27.61)	-0.0221	(-22.85)
$TEXP^3$	.000815	(21.24)	.000762	(16.49)
$TEXP^4$	-0.0000128	(17.59)	-0.0000119	(-13.71)
YOS	.192	(5.03)	.313	(7.54)
YOS^2	-0.00214	(1.54)	-0.00456	(-3.18)
TEXP.YOS	-0.00618	(7.71)	-0.00914	(-8.78)
$TEXP^2$.YOS	.000254	(21.28)	.000193	(4.94)
K* L			6.444	(5.64)
OL			-1.304	(-3.81)
AGL			-0.475	(-7.51)
02L			.299	(1.55)
AG2L			.00752	(7.18)
OG2L			-0.00124	(-5.52)
02AGL			-0.0168	(-4.42)
0AGL			.00791	(5.27)
02AG2L			.000257	(4.70)
	R^2 = 0.684	F = 2476	R^2 =	F=

* For the following nine variables, $L \equiv \phi_i / \Phi_i$ (inferred from quit rates), $0 \equiv$ OJEX, AG $\equiv$ AGE, 02 $\equiv$ $OJEX^{3/2}$, AG2 $\equiv$ $AGE^{3/2}$. Combinations of these variables represent products.

TABLE A3
PREDICTED WAGE-TENURE PROFILES*

of Tenure	Predicted Wage of Workers Still Employed in 1969	*Ceteris Paribus* Predicted Years Wage Had No Workers Quit
0	3185	3447
1	3733	4622
2	4162	5759
3	4470	6762
4	4675	7572
5	4808	8173
6	4898	8579
7	4922	8821
8	5050	8937
9	5144	8956
10	5258	8901
11	5387	8780
12	5515	8589
13	5617	8314
14	5652	7930
15	5575	7411

* Schooling and previous years of work experience have been set at the sample mean values.

Paper 4.

AN EMPIRICAL ANALYSIS OF AUSTRALIAN STRIKE ACTIVITY: ESTIMATING THE INDUSTRIAL RELATIONS EFFECT OF THE PRICES AND INCOMES ACCORD

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I. Introduction

> "Because of its attention to interest issues which form the substance of many industrial disputes, and because of its formalisation of the commitment required from unions to make the Accord's objectives at all achievable, the Accord is a critical new element in the industrial relations scene." [Charles (1986) p.119]

Since the institution of the prices and incomes Accord in 1983 both the government and union representatives have frequently claimed[1] that the Accord has delivered very low levels of industrial disputation.[2] The goal of this paper is to directly test that assertion.

To attribute the relatively low strike activity of the 1983-85 period solely to the effects of the Accord, as is frequently done, may not be justified. Overall macroeconomic conditions are generally considered determinants of the level of strike activity, and these have recently changed markedly. Unless one controls for the influence of macroeconomic conditions little can be said about the efficacy of this consensual incomes policy in diminishing industrial disputation.

The approach is to estimate models of strike activity for the period prior to the Accord, and using these models predict the level of industrial disputation for the period 1983(3) to 1985(4). A comparison of actual strike data and that predicted gives insight into the industrial relations value of the Accord after adjusting for changed macroeconomic conditions. The method allows several important insights into an interpretation of the determinants of strike activity.

First, the estimations reveal that industrial disputation is pro-cyclical. This is an important finding given the attention accorded this issue in the literature. Further, strike activity is positively associated with inflation, and lower profits and, less strongly, higher inventories, appear to positively influence strikes. As well, average strike duration is apparently lengthened when the job vacancy rate is higher and when profits are lower. We interpret these results to be reflections of misinformation, union muscle-flexing and firms encouraging disputes to run down inventories. However, other conclusions are possible.

Second, by forecasting strike activity over the period of the Accord, the analysis provides evidence of the effect of the policy initiative on industrial disputation. The models overpredict strikes considerably in the 1983(2)-1985(4) period, a result that can be intepreted as the Accord contributing significantly to improvements in industrial relations. Other unmeasured factors may be important in explaining this finding and conclusions are, as always, matters of judgement. At the very least the indications are that it is extremely dubious to dismiss the influence of the Accord in reducing strike activity.

Third, the forecasts help determine the extent to which some commentators have exaggerated the direct influence of the Accord on strike activity. Statements attributing the entire decrease in industrial disputes to the policy implicitly give no weight to the effect of changes in macroeconomic conditions. But because there have recently been movements in these variables which have tended to decrease measured industrial militancy, such assertions exaggerate the industrial relations effect of the Accord. We find the extent of the overstatement to be about 44 per cent.

There are several difficulties which must be confronted when engaging in such an analysis. These relate to economic theory, measurement, and econometric method, and are outlined below.

First, a theoretical basis is essential to the correct interpretation of statistical relationships, but it is clear that an all-encompassing and rigorously specified model explaining Australian strike activity does not exist. The framework most commonly used for overseas empirical research (Aschenfelter and Johnson (1969), Pencavel (1970)) has important limitations when applied to an economy with a centralised system for arbitrated settlements, such as the Commonwealth Conciliation and Arbitration Commission (CCAC). The existence of CCAC introduces a third party into the dispute settling process, and is highly likely to affect approaches and solutions to conflict. The implications this has for theory are explored in section II.

Second, there is no universally accepted correct measurement of strike activity. Some of the candidates usually considered are the number of strikes, the number of workers involved in strikes, the average duration of strikes, and the number of working days lost from disputes. For theoretical reasons some are preferred to others, and for practical reasons the focus is on a subset of measures. This becomes clear in section III, which examines Australian empirical work and justifies the econometric specifications to be employed.

Finally, there are at least three major econometric complexities encountered in the analysis. One, there is in theory endogeneity between strike activity and several of the explanatory variables. Two, we employed generator regressors techniques which imply the need to recover unorthodox test statistics (Pagan (forthcoming)). Three, analysis of economic data covering the turbulence of the 1970s requires stability tests. These issues are addressed in section IV which presents the estimation results. Section V investigates the effect of the Accord on recent strike activity, and Section VI examines the extent of exaggeration of its industrial relations benefits implicit in the usual assertions.

II. Theoretical Issues

Hicks' (1932) seminal work on the economics of strikes provides the basis for the work of modern analysts (Aschenfelter and Johnson (1969), Hayes (1984)). Because of the foregone income associated with a strike, Hicks' model assumes that a union will moderate its wage demand as the expected length of time of the strike increases. The employer, on the other hand, increases its wage offer as the expected length of a strike increases because of the foregone profits associated with the strike. Strikes are assumed to be costly to both parties, thus if the employer is fully informed about the union's "resistance curve" and the union fully informed of the employer's "concession curve" the parties will agree to avoid the strike with the union accepting the wage offer consistent with both parties' needs. Given this basis, the model attributes strike incidence to poor information on the part of either party of the other's bargaining position.

Hayes' analysis is also based on notions of misinformation. By assuming the firm has more information than the union concerning the "state of nature" (the employer's concession curve?), she interprets strikes as attempts by unions to gain information, and with it to settle on the highest possible wage gain. The rate of decline of union resistance will be determined by the costs of striking to the worker. Strike costs, in turn, are a function of two things: the availability of funds for the period of the strike, and the extent of overtime expected as a consequence of the strike.

In Australia strike funds *per se* do not exist, so the availability of short-term alternative employment is likely to be a consideration if the strike is expected to be of long duration. The relevance of expected overtime is straightforward.

The employer concession curve is determined by the cost to the firm of the strike. Firm strike costs are a function of expected profit losses which in turn will be determined by profit levels at the start of the strike, the level of inventories, and the nature of the market (for example, future customer loss may be more important in those industries relying on the establishment of long-term customer relationships).

In Hayes' formalisation of this perspective, strikes only result from misjudgement by the union of the employer's concession curve. This may arise, for example, from workers believing the firm's profits to be relatively high and thus its resistance to the wage demand relatively low.

The way in which this model applies in reality can be explained with the following example. Expecting that overtime will be available to make up for the lost income as a consequence of the strike, a wage demand is made by the union. If the union and employer agree (implicitly) on the firm's profit level (i.e. on the position of the employer's concession curve), an agreement is reached without a strike. But if the two parties have different perceptions of this variable, the employer resists the demand and a strike results. The strike leads to negotiations (increased information) and a resolution in terms of mutual interest.

While not a popular way of understanding the Australian strike process, there are examples which appear to fit the misinformation model. The 1964 Australia-wide strike against General Motors-Holden and the 1977 La Trobe Valley power workers' strike are seen by Ford and Hearn (1980) to be the result of poor information by both the unions involved. In the first instance they allege the strike resulted from:

> "inadequate understanding of a firm's position by the union leadership involved" (Ford and Hearn (1980) p.10),

in particular, inadequate understanding of profit levels. The latter strike they attribute to:

> "...the difficulties associated with the pursuit of claims by workers in geographically remote areas, largely dependent upon centralised management and union organisation" (Ford and Hearn (1980) p.10).

A second type of misinformation is identified by Hicks and has been developed formally by Aschenfelter and Johnson. In this model the existence of information differences between union leaders and the rank and file encourages the former to use strikes to demonstrate to their members the true position of the employer's concession curve. It is a framework particularly well-suited to U.S. experience where union leadership is often distant from the rank and file, but its essence is unappealing in the Australian context (Bentley and Hughes (1970)).

These analyses focus solely on wage demand and wage offers by the respective parties. A different type of strike motivation is also frequently recognised. This is the notion of "muscle flexing" in which

> "the union might periodically call strikes to keep credible the threat of a strike" (Hayes (1985) p.59).

This can be rational behaviour in the long-run, even if in the short-term costs are incurred and no concessions achieved.

There are good reasons to believe that this motivation for strikes is particularly apposite in Australia. The most compelling of these is that Australian strikes are overwhelmingly of short duration and work is typically resumed without negotiation.[3] Examination of the industrial relations literature reveals that a consensus exists as to the typical cause of industrial disputes in this country: strikes are not seen to be bargaining tools aimed at the gaining of short-term concessions, nor are they generally believed to be attempts to increase information to bring about maximum possible wage increases from employers. They are usually interpreted instead as signalling devices aimed at demonstrating the seriousness of the conflict. The essential reasons appear to be:

> "to speed up the resolution of unattended grievances and ... to influence the outcome of conciliation and arbitration processes" (Charles (1986) p.121).[4]

This is not to say, however, that the *timing* of industrial disputes is unrelated to economic variables. Indeed, an obvious point is that the most opportune time for unions to flex their muscles is when the costs of so-doing are low. This implies that protest strikes are more likely when overtime is plentiful, so that the lost wages are recovered relatively painlessly. Alternative hypotheses are proferred in section III.

Two other reasons for strikes are not covered directly by the above analyses. The first concerns the role of inflation and may be understood to be a subset of misinformation models (Phipps (1977)). If workers expect future real wage loss as a consequence of price increases, or wish compensation for current real wage loss, they may make wage demands that employers are unable to meet. Phipps' model assumes that workers perceive the firm's expected price rise to be equivalent to economy-wide inflation. If the firm perceives it to be less, it resists the wage bargain and a strike results. His model implies that both union demands and employer resistance are increased with inflation. This presumably implies that the inflation effect on (wage) strikes is diminished in periods of full wage indexation, an hypothesis testable with the data.

Also, employers may initiate industrial action (Ford and Hearn (1980)), including strikes and lock outs, as a way of running down excess supplies of (presumably unanticipated) inventories. Little modelling of this process has been undertaken in the literature, but its potential lends itself to empirical testing.

III. Measuring and Modelling Strike Activity in Australia

Since our goal is to model the determinants of strike activity, and how policy initiatives change these relationships, we need to evaluate the various measures of union militancy. Following Perry (1979), strike activity can be analysed as follows:

$$\mathrm{WDL/U} = (\mathrm{NS/U}) . (\mathrm{WI/NS}) . (\mathrm{WDL/WI}) \qquad (1)$$

where WDL/U is working days lost per unionist, NS/U is number of strikes per unionist, WI/NS is number of workers involved in each strike and WDL/WI is working days lost per member involved. This can be simplified to:

$$\mathrm{WDL/U} = \mathrm{WI/U} . \mathrm{WDL/WI} \qquad (2)$$

where WI/U is workers involved in strikes per unionist. Respectively the variables represent total time lost, average worker involvement and average strike duration. We chose to model each of those components rather than the full set in equation (1) essentially because their economic content is relatively easy to interpret.

The major advantage of WDL/U is its composite nature. It increases with a greater number of strikes per employee, a larger number of workers involved per strike and a longer average strike duration. It is probably the broadest available measure of trade union militancy. WI/U only partially captures militancy because it fails to take into account average strike duration. WDL/WI, on the other hand, because it represents average strike duration, does not account for the number of workers participating in strikes. Thus it treats equivalently the effect of few workers striking for long periods and a large number of workers striking for short periods (Perry (1978)). There is no perfect measure of trade union militancy. A defensible position is to choose dependent variables reflecting the essence of change in industrial disputation. With these perspectives in place it is useful to examine previous Australian analyses.

Oxnam (1953) first identified the relationship in Australia between strike activity and economic variables, in particular the state of the macro-economy. With reference to annual data beginning in 1913, he noted the clear procyclical swings in industrial disputation. The explanation offered for this phenomenon has a distinct industrial relations flavour:

> "In short, economic change necessitates a revision of the rules of work, a process in which a certain amount of conflict is inevitable. To a considerable extent therefore strikes represent merely frictions in the process of rule making in response to economic change" (Oxnam (1975) p.30).

This perspective is supported, in part, by the first multivariate examination of Australian strike activity, that of Bentley and Hughes (1970).

The Bentley and Hughes analysis investigated the influence of economic activity on strike frequency, duration, magnitude and days lost from 1952 to 1968. They concluded that, for the non-coal sector, frequency and days lost are negatively related to the unemployment rate, and duration is positively related to the change in the unemployment rate. Because of the close associated between unemployment and other variables likely to influence strikes, they do not take their results to support a particular hypothesis of the causes of strikes. They do stress, however, the pro-cyclical nature of strike activity. Unfortunately, several variables of interest, such as inflation and inventories, were not included in the estimations.

Phipps (1977) estimated a less restrictive model on quarterly data for the 1960-72 period. Both profits and inflation appear to positively influence strike frequency. This result should be interpreted cautiously because of the following problems associated with his method. First, he uses the number of strikes per employee as his strike activity variable. While all trade union militancy variables have their weaknesses, this variable is particularly questionable (Perry (1978)). Implicitly it assumes that any change in the number of workers striking is irrelevant. Unfortunately, no other trade union militancy variables were analysed. Second, Phipps found his business cycle variable, vacancies minus unemployment, to be significantly negatively related to strike incidence. This finding is contrary to theory, to less formal analyses and to the results of Bentley and Hughes.

The other major analysis of the determinants of strikes is that of Perry (1979), in which annual data for the inter-war period were examined. His result found support for two hypotheses: working days lost per employee per year was negatively and positively related to the unemployment and inflation rates respectively. Given that his focus was primarily on factors contributing to nominal wage change, relatively little exploration of the strike equation was undertaken.

In summary, several conclusions are apparent from the above brief survey. First, there is no consensus as to the ideal trade union militancy variable. Given this, it is appropriate to estimate models using different strike indicators. Second, both inflation and unemployment are apparently significant explanators of strike activity in Australia, although these results do not constitute strong evidence in support of particular hypotheses. Related to this is the third and major point that associations between explanatory variables are too strong, and data limitations so severe, that circumspect interpretation of tests of specific models of strike activity is called for. Every effort can be made to ensure the proposed models have good statistical properties, but even so such estimates provide only modest support for theory.

With this background it is appropriate to specify the econometric models to be estimated. While predictions of coefficient signs are offered which are consistent with the theoretical issues noted in section II, alternative interpretations are also recorded.

The theoretical conjectures outlined suggest that there are three independent motivations for strikes: misinformation on the part of the union concerning the employer's concession curve; union muscle flexing as attempts to ensure improved future outcomes; and as the consequence of firms manipulating an inexpensive run-down of unexpectedly high inventories. The *timing* of strikes differs according to which of these factors take precedence.

If misinformation is the cause of friction we can identify some situations in which strike action is more likely. First, if unions are not aware of the firm's contemporaneous profit level it is plausible that they are more likely to overestimate it when (current) profits (PROF) (gross operating surplus/wage bill) are relatively low. This will only lead to a strike when a wage demand is not met. Since wage demands are more likely when overtime is expected to be high, an appropriate proxy variable is given by OP, the interaction of overtime, when it is above trend, and profits, when they are below trend. In this misinformation case two factors matter: the increased likelihood of wage demands by workers and an increased likelihood of resistance to them by firms. The second misinformation proxy is inflation (INF). Following Phipps, inflation leads to greater worker agitation because of expected or current real wage decreases. Some firms will expect their likely price increases to be lower than do their workers, and it is because of this that strikes are predicted to occur.

Muscle flexing strikes are presumed to be timed to minimise costs to workers. They are thus assumed to be more likely when expectations of overtime, proxied by current economy-wide average overtime levels (OT), are high.

Strikes initiated by firms, and lock-outs (included in the strike data), are presumed to be timed to minimise foregone revenue. Accordingly it is assumed they are more likely when inventories are unexpectedly high. This variable is proxied by the level of inventories relative to gross domestic product, given by INV.

Strike *duration* is particularly difficult to model given that the evidence suggests the prevalence of short protest strikes after which work resumed without negotiation. Several predictions can be offered. First, for strikes not motivated by muscle flexing, it is expected that a high job vacancy rate (VAC) allows workers to minimise the foregone income of strikers. High vacancy levels increase the probability of workers attaining alternative temporary employment. Second, if the strike is being used by workers to gain information of the true level of the firm's profits, the lower are profits the longer is a strike expected to last. The reasoning is that it takes a greater length of time for workers to accept that profits are low the further they are from workers' expectations. Third, INV may be relevant to duration. The firm will prefer longer strikes and will thus be less willing to acquiesce to workers' demand, the higher are inventories.

These predictions lead to the suggestion of the following models:

$$(WIL/U)_t = b_o + \beta_1 \, INF_t + b_2 \, INV_t + b_3 \, OP_t + b_4 \, OT_t + b_5 \, PROF_t + e_1 \qquad (3)$$

$$(WDL/WI)_t = \gamma_o + \gamma_1 \, INF_t + \gamma_2 \, INV_t + \gamma_3 \, PROF_t + \gamma_4 \, VAC_t + e_2 \qquad (4)$$

$$(WDL/U)_t = \alpha_o + \alpha_1 \, INF_t + \alpha_2 \, INV_t + \alpha_3 \, OP_t$$
$$+ \alpha_4 \, OT_t + \alpha_5 \, PROF_t + \alpha_6 \, VAC_t + e_o \qquad (5)$$

where the subscript t refers to the time period.

From theory the following coefficient signs are expected: a_1, a_2, a_4, a_6, b_1, b_2, b_4, γ_1, γ_2, $\gamma_4 > 0$, and a_3, a_5, b_3, b_5, $\gamma_3 < 0$. Negative signs on a_3 and b_3 imply that strike activity is relatively high the greater are profits below trend (so long as overtime is above trend).

Before considering the estimates it is important to recognise two fundamental methodological points. First, in some instances, the predicted coefficient signs are consistent with different theoretical conjectures. In particular, a positive relationship between overtime and strike activity could reflect three other factors. One is Oxnam's proposition that firms are more likely to change work practices in periods of high product demand, which increases industrial conflict and leads to muscle flexing strikes. The second is the possibility that high overtime is simply associated with more hours of work and thus a greater (statistical) likelihood of unresolved conflict (Bentley and Hughes(1970)). The third is that striking is a normal good, and thus more likely when income - proxied by overtime - is relatively high (Bentley and Hughes (1970)). Also, inflation could lead to greater worker agitation if higher overall nominal wage growth is associated with more disruptions to wage relativities. In this case the misinformation model has less credence if such strikes are attempts to draw CCAC's attention to the conflict. For the above examples the data are not rich enough to allow confident conclusions on the validity of specific theoretical hypotheses.

Second, a very large number of estimations could be undertaken using different proxies to represent particular perspectives. Because of the complexities inherent in the exercise and the forecasting goal of the project we chose to focus on relatively simple models, and thus interpret the estimations as indications only of underlying relationships. The intention is to estimate statistically robust models that capture the essence of the determinants of strike activity.

IV. The Data, Estimations and Diagnostics

The three models were estimated using quarterly Australian data for the period 1959(3) to 1983(1). Explanatory variables were seasonally adjusted and dummies (S1, S3 and S4, representing the first, third and fourth quarters, the second quarter effect being in the regression intercept) were included to correct for seasonal variations in strike activity. As well time (T) was included to account for unmeasured dynamics.

During this period three major one-off strikes were initiated as a result of political factors: in response to the gaoling of Clarrie O'Shea in 1969(2), the Medicare protest of 1976(3) and as a consequence of the gaoling of several Western Australian unionists in 1979(2). Dummies have been included to account for these events and are identified as 692, 763 and 792.

The first step in the process was to use the data in (approximate) reproductions of previously used Australian models. The Bentley and Hughes, Phipps, and Perry specifications were estimated, and the results are reported in Appendix 2. In all cases these estimations failed fairly straightforward diagnostic tests in terms of t-statistics and serial correlation. They are not adequate to explain the 1959(3) - 1983(1) data.

As well, the opportunity was taken to test several political and institutional hypotheses, as suggested by Creigh, Poland and Wooden (1984). These were that strike activity is decreased: one, when the ALP is in power; two, in the quarter when a Commonwealth election (Senate and/or House of Representatives) is being held; and, three, for periods of wage indexation.

Several tests of the effect of indexation were undertaken. The first used a dummy variable for quarters of full wage indexation. Second, separate dummies were included for quarters of full and partial indexation. Third, to reflect the hypothesis that indexation acts as an anaesthetic to disputes, a dummy variable was included for quarters following full wage indexation. As well, the indexation dummies were interacted with inflation to test the proposition that inflation contributes less to strike activity when workers have their real wages protected.

In all cases relationships between strike activity and political and indexation variables were found to be not significantly different from zero. Importantly, other coefficient signs and sizes were robust to their inclusion or exclusion. Consequently subsequent model estimation omitted these variables. Comprehensive specifications of the basic models were estimated and subjected to a wide range of specification tests. These resulted in re-estimations of the models using subsets of explanatory variables until fairly parsimonious equations were obtained. The preferred estimations and diagnostic tests are presented in Tables 1 - 3. The elasticities are reported in Table 4.

Table 1(a)
Preferred Estimation of Workers Involved in Strikes Per Unionist
(x1000)

		Absolute t-statistics		
Independent Variables	Coefficient	OLS	White's Heteroskedasticity Adjusted	Bootstrap+5
Intercept	-172.588	1.61	1.72	1.54
INF	1122.781	2.08	1.84	2.30
INV	239.052	1.70	1.91	1.65
OT	74.936	2.88	2.66	3.18
OP	-192.801	1.49	2.01	1.48
S1	-26.006	1.98	2.06	2.11
S3	10.822	0.82	0.75	0.98
S4	-25.056	1.90	2.01	2.04
692	227.461	5.04	18.99	5.41
763	394.835	8.81	37.33	9.13
792	3421.581	7.59	26.36	8.15
T	-0.380	0.90	1.04	0.92
R^2	.74			
F	22.04			

+500 iterations.

Table 1(b)
Diagnostic Tests of Workers Involved in Strikes Per Unionist Equation

Test for Serial Correlation

	Auto-Correlation Function	t-statistic*
1st order lag	0.0496	0.48
2nd order lag	0.0610	0.59
3rd order lag	0.0185	0.18
4th order lag	0.0712	0.69
5th order lag	0.0700	0.68
6th order lag	-0.123	-1.20

* 95% critical value of t-statistic is 1.96.

RESET Test of Functional Form

	t-statistic*
RESET 2	0.178
RESET 3	-0.157
RESET 4	-0.446

* 95% critical value of t-statistic is 1.96.

Chow Test for Structural Stability

Ho: Equation coefficients for 1959(3)-1973(1) = equation coefficients for 1973(2)-1985(1).

Critical value = 2.01.

Test statistic = 0.82.

Ho accepted.

Hausman Test for Exogeneity of OT and PROF[6]

Ho: OT and PROF are exogeneous.

Critical value = 21.01.

Test Statistic = 1.508.

Ho accepted.

Table 2(a)
Preferred Estimation of Average Strike Duration

Independent Variables	Coefficient	Absolute t-statistics: OLS	Absolute t-statistics: White's Heteroskedasticity Adjusted	Absolute t-statistics: Bootstrap+
Intercept	3.795	2.73	2.37	2.75
PROF	-0.413	2.43	2.20	2.50
VAC	82.986	2.76	2.61	2.97
S1	0.232	1.08	1.26	1.24
S3	0.172	0.79	0.92	0.83
S4	0.282	1.31	1.19	1.47
692	-0.392	0.53	2.19	0.63
763	-1.080	1.46	6.21	1.48
792	-0.825	1.11	4.66	1.13
T	0.0153	3.70	3.36	3.52
R^2	.38			
F	5.91			

+500 iterations.

Table 2(b)
Diagnostic Tests of Average Strike Duration

Test for Serial Correlation

	Auto-Correlation Function	t-statistic*
1st order lag	-0.062	-0.60
2nd order lag	0.00609	0.059
3rd order lag	-0.140	-1.37
4th order lag	-0.0498	-0.24
5th order lag	-0.0247	-0.24
6th order lag	-0.000530	-0.0052

* 95% critical value of t-statistic is 1.96.

RESET Test of Functional Form

	t-statistic*
RESET 2	0.326
RESET 3	0.249
RESET 4	0.175

* 95% critical value of t-statistic = 1.96.

Chow Test for Structural Stability

Ho: Equation coefficients for 1959(3)-1973(1) =
equation coefficients for 1973(2)-1985(1).
Critical value = 2.13.
Test statistic = 2.38.
Ho rejected. (Accepted at 1% level).

Hausman Test for Exogeneity of OT and PROF

Ho: OT and PROF are exogeneous.
Critical value = 18.30.
Test statistic = 1.527.
Ho accepted.

Table 3(a)
Preferred Estimation of Working Days Lost Per Unionist
(x1000)

Independent Variables	Coefficient	Absolute t-statistics: OLS	Absolute t-statistics: White's Heteroskedasticity Adjusted	Absolute t-statistics: Bootstrap+
Intercept	-89.940	1.31	0.93	1.32
INF	2493.881	1.87	1.90	2.22
OT	156.015	2.61	1.83	2.80
OP	-1162.440	3.63	4.22	3.67
S1	-28.830	0.88	0.80	0.91
S3	26.433	0.81	0.89	0.92
S4	-40.815	1.25	1.42	1.30
692	250.436	2.24	11.29	2.58
763	473.959	4.25	17.37	4.57
792	419.336	3.67	14.49	3.75
T	1.436	2.43	3.00	2.51
R^2	.62			
F	13.68			

+ 500 iterations.

Table 3(b)
Diagnostic Tests of Working Days Lost Per Unionist Equation

Test for Serial Correlation

	Auto-Correlation Function	t-statistic*
1st order lag	0.0778	0.75
2nd order lag	-0.120	-1.17
3rd order lag	0.0793	0.77
4th order lag	0.0311	0.30
5th order lag	0.195	1.90
6th order lag	0.0816	0.79

* 95% critical value of t-statistic is 1.96.

RESET Test of Functional Form

	t-statistic*
RESET 2	-0.838
RESET 3	-3.14
RESET 4	-3.96

* 95% critical value of t-statistic = 1.96.

Chow Test for Structural Stability

Ho: Equation coefficients for 1959(3)-1973(1) =
equation coefficients for 1973(2)-1985(1).
Critical value = 2.06.
Test statistic = 0.946.
Ho accepted.

Hausman Test for Exogeneity of OT and PROF

Ho: OT and PROF are exogeneous.
Critical value = 19.67.
Test statistic = 0.946.
Ho accepted.

The data of Tables 1(a) and (b) show that the equation for the proportion of unionists involved in strikes per quarter performs well. This measure of activity is clearly pro-cyclical (as represented by overtime), a result reinforcing both Australian (Oxnam (1953); Bentley and Hughes (1970)) and international (Rees (1952); Aschenfelter and Johnson (1969); Pencavel (1970)) evidence. Strike involvement also increases with inflation. To a lesser extent in terms of statistical significance strike activity increases with inventories, and rises with overtime and falls with profits when these variables are respectively above and below trend. A lower proportion of unionists are involved in the first and fourth quarters, and the "political" strikes of 1969(2), 1976(3) and 1979(2) substantially increased overall worker participation in strikes. The equation passes a broad range of diagnostic tests. There is no serial correlation, the RESET tests suggest no evidence of functional form problems and the coefficients appear to be stable according to a Chow test. As with all the estimated equations, we report t-statistics based on bootstrapping the sample for 500 iterations. These t-statistics do not rely on homoskedasticity or normality of the underlying regression disturbances.

From Tables 2(a) and (b) it appears that average strike duration lengthens as the job vacancy rate rises and as profits fall. The equation performed adequately on the diagnostic tests. Again, there is no serial correlation, the t-statistics are satisfactory and the equation passes RESET tests of functional form. There is mild evidence of coefficient instability across the two time periods as the Chow test fails to support coefficient equivalence at the 5, although not at the 1, per cent level.

Tables 3(a) and (b) report the estimation of the working days lost equation. It shows that working days lost per unionist apparently increase with inflation and overtime, and as profits decrease and overtime increases when these variables are, respectively, below and above trend. There is no clear seasonal pattern, and the "political" strikes of 1969(2), 1976(3) and 1979(2) added considerably to days lost. While the equation performs well according to most of the diagnostic tests, it fails RESET3 and RESET4 which indicates some functional mis-specification. We were unable to identify another functional form which overcame this problem. For this reason the equation is not used in the forecasting undertaken and reported in section V.

Interestingly, OT and PROF are apparently exogenous to strike activity, all equations passing Hausman tests. This implies that if overtime and profits are affected by strikes, the empirical significance of this direction of causality is not high.

The results, summarised in elasticity form in Table 4, provide some support for the hypotheses outlined in section III. If misinformation is associated with industrial disputation we should expect INF and OP to be, respectively, positively and negatively associated with working days lost and unionists involved, as was found. Protest strikes are more likely to occur when OT is high, a result confirmed. As well, duration was found to be affected by both VAC and PROF. The hypothesis that firms manipulate strikes in order to run down inventories is supported weakly.

Importantly, these results should not be taken as definitive evidence for the original hypotheses as they may be consistent with alternative explanations, as previously discussed. Nevertheless, it is worth noting the general robustness of results, an issue of some significance given the forecasting motivation of the exercise.

It is also of some interest to note which independent variables performed poorly in the estimations. In particular, conventionally used cyclical indicators such as the unemployment and job vacancy rates were generally far less satisfactory explanators than was overtime. This finding provides further support for the view promoted originally by Gregory and Smith (1985) that the state of the labour market is more accurately reflected in firm-based data, such as overtime, than it is in aggregated measures of excess labour supplies.

Table 4
Estimated Elasticities (at means)

Independent Variables	Dependent Variables WDL/U	WI/U	WDL/WI
INF	0.209	0.196	
OT	0.843	0.837	
OT*	2.38	1.850	
PROF			-1.423
PROF*	-5.82	-2.90	
INV		1.800	
VAC			0.269

* Calculated for when overtime is one standard error above trend and profits are one standard error below trend.

In summary, the models of average strike duration and proportion of unionists involved in strikes perform well under a broad range of diagnostic tests. We feel justified in using these models to investigate the industrial relations value of the Accord in terms of its effect on strike activity.

V. Projections Over the Period of the Accord

In theory, there are two methods available to estimate the direct effect of the Accord in reducing strike activity. The first would be to use the coefficients reported in Tables 1-3 to predict the relevant dependent variables over the 1983(2)-1985(4) period, and compare these values with actual experience. This approach has been adopted with respect to the proportion of unionists involved in strikes and average strike duration equations. But because the working days lost per unionist equation did not perform adequately in terms of diagnostic tests, we choose the second method, explained below, to forecast this variable.

The second method recognises that working days lost per unionist is the product of the proportion of unionists involved and average strike duration (see equation (2)). Thus the product of the projections of these variables provides a satisfactory way of projecting working days lost per unionist. Forecasts based on these methods are presented in Table 5.

Table 5
Actual and Forecast Measures of Strike Activity

Year	Quarter	(i) Proportion of Unionists Involved in Strikes		(ii) Average Strike Duration		Working Days Lost Per Unionist	
		Actual	Projected	Actual	Projected	Actual	Projected (Product of Projections from Columns (i) and (ii))
1983	2	0.041	0.084	4.18	2.44	0.171	0.206
1983	3	0.051	0.096	2.61	2.51	0.133	0.241
1983	4	0.044	0.060	2.61	2.62	0.115	0.158
1984	1	0.032	0.038	2.67	2.44	0.084	0.094
1984	2	0.031	0.067	2.98	2.45	0.093	0.164
1984	3	0.051	0.095	2.68	2.61	0.135	0.249
1984	4	0.077	0.056	1.56	2.57	0.120	0.144
1985	1	0.042	0.071	2.52	2.50	0.106	0.179
1985	2	0.048	0.097	1.68	2.46	0.084	0.241
1985	3	0.054	0.114	1.49	2.49	0.110	0.283
1985	4	0.051	0.095	2.32	3.01	0.118	0.286
Quarterly Average		0.047	0.079	2.482	2.555	0.115	0.204
Standard Error*			0.018		0.294		0.051[7]

* The standard errors should not be used uncritically to compute tail probabilities. Jarque-Bera statistics revealed that there was severe non-normality of the disturbances of the regression equations. These distributions are highly positively skewed.

The data of Table 5 reveal the following. First, the proportion of unionists involved in strikes equation substantially overpredicts for the period of the Accord. The projections of this variable for the 1983(2)-1985(4) period averaged .079 compared with the actual experience of .047, a difference of 51 per cent. Second, and on the other hand, the average strike duration equation *under*predicted slightly. From column (ii) Table 5 the equation projected strike duration to average about 2.56 days, compared with the actual experience of 2.48 days, a difference of 3 per cent. The standard error implies that these numbers are not significantly different from each other.

Third, using the product of the forecasts of these two variables to project working days lost per unionist reveals that the models considerably overpredict strike activity in the Accord period. The projection for working days lost per unionist averaged .204 compared with the actual .115, a difference of about 56 per cent. As well, the forecasts lie above the actual data for 11 consecutive quarters. Since the test on the autocorrelation function suggests no serial correlation in the disturbances, we conclude that the probability of getting such a sequence randomly is the power to the 11^{th} order of the probability of working days lost per unionist falling below its forecast value in any quarter. The forcast errors are skewed so we use the in-sample regression residuals which reveal that working days lost per unionist fall below the in-sample forecast 67 per cent of the time. The chance of having 11 consecutive quarters below prediction is thus $(0.67)^{11}$ or 1.22 per cent. This is a very small number and compelling evidence of a structural break.

In summary, the results imply that correcting for the influence of macroeconomic variables, the 1983(2)-1985(4) period was characterised by a significant decrease in strike activity. Working days lost per unionist and the proportion of unionists involved in strikes have both fallen substantially recently for reasons other than changes in the macroeconomy. The strikes that did occur in this period were of slightly longer duration than projected by the model, although the standard errors imply that the difference is not statistically different from zero.

It is important to note a qualification to the results. This is that the method employed implicitly assumes that the Accord has had no effect on the explanatory variables. This is unlikely to be true given the (approximate) maintenance of real wages in a period of considerable economic recovery, a situation which has resulted in large decreases in real unit labour costs with concomitant increases in the profit share (Chapman (1986)). From the models, higher profits decrease strike activity and some part of this is attributable to the Accord. A reverse argument may be applied to overtime if the Accord has contributed to recovery. On the other hand, any decreases in inflation as a consequence of the Accord have the opposite effect on strike activity. The method employed only allows potential identification of direct improvements in the industrial relations environment.

VI. Estimating the Error Implicit in Attributing All The Decrease to the Accord

Additional insight into the policy debate is provided by posing the counter-factual implicit in government and union statements concerning the effect of the Accord on working days lost. Simple comparisons of strikes pre- and post-Accord attributing the difference solely to industrial relations aspects of the policy change take no account of changed macroeconomic circumstances. In other words, they assume constancy of the macroeconomic variables when projecting what strike activity would have been without the Accord. Table 6 illustrates such a forecast under the assumption that the independent variables took on their average 1976(1)-1983(1) values.

The data of Table 6 reveal the following. Not taking into account changes in macroeconomic variables results in a projection of working days lost per unionist of 0.258 compared with the actual .115. Thus this approach implicitly ascribes to the industrial relations effect of the Accord an average decrease of 0.143 working days lost per unionist, or about 72 per cent.

Table 6

Actual and Forecast Measures of Strike Activity, Assuming No Changes in Macroeconomic Variables

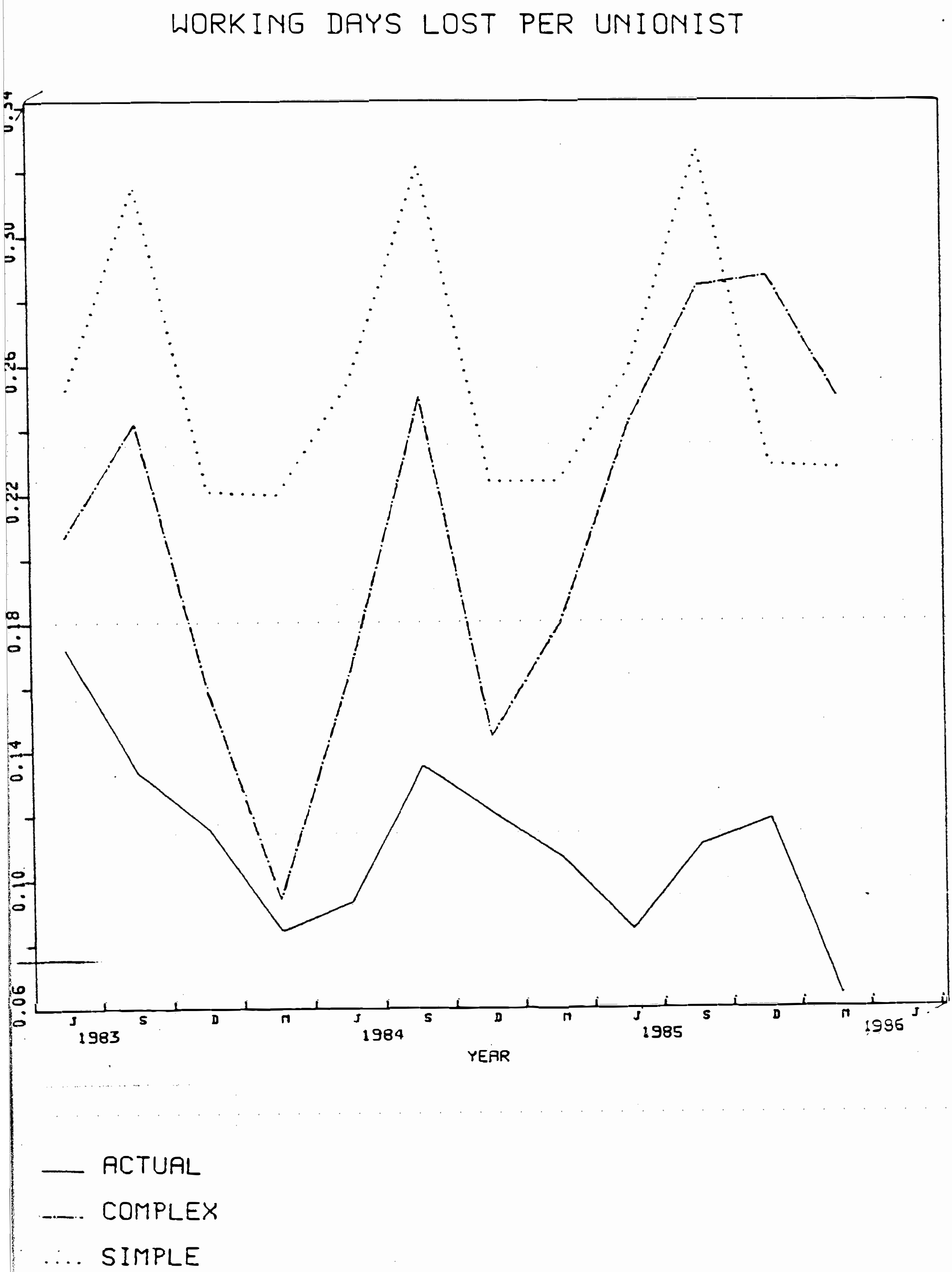
Year	Quarter	(i) Proportion of Unionists Involved in Strikes	(ii) Average Strike Duration Per Unionist	Working Days Lost
		Projected	Projected	Projected (Product of (i) and (ii))
1983	2	0.104	2.39	0.249
1983	3	0.121	2.59	0.314
1983	4	0.082	2.69	0.220
1984	1	0.081	2.68	0.219
1984	2	0.103	2.45	0.253
1984	3	0.121	2.65	0.320
1984	4	0.081	2.75	0.223
1985	1	0.081	2.75	0.223
1985	2	0.102	2.51	0.257
1985	3	0.120	2.71	0.325
1985	4	0.085	2.81	0.238
Average		0.098	2.63	0.258
Standard Error		0.016	0.27	0.042

It is interesting to compare this result with the projections reported in Table 5 allowing changing macroeconomic conditions to affect estimates. This provides information as to the extent of the error implicit in the view that the industrial relations consequences of the Accord have been the only factors affecting strike activity. After adjusting for changed macroeconomic conditions the models overpredict working days lost per unionist by 0.091, but assuming no change in macroeconomic conditions the overprediction is 0.143. This difference is of the order of 44 per cent. The point is illustrated in Figure 1.

Figure 1 compares strike activity forecasts based on different assumptions concerning the role of macroeconomic variables on actual working days lost per unionist. In each quarter the actual data fall below projections based on the product of the forecasts of worker involvement and average duration equations which allow macroeconomic variables to have an effect (complex projection). In turn these forecast lie below those assuming no change in the macroeconomic variables (simple projection). The figure illustrates both that a clear structural break has occurred and that changed macroeconomic circumstances independently decreased strike activity.

Overall, the results imply the following. First, as measured by strike activity, there have been considerable improvements in the industrial relations environment since early 1983. It is plausible to attribute a substantial part of this improvement to the institution and maintenance of the prices and incomes Accord.[8] Second, statements attributing all the decreased working days lost per unionist to the Accord are in error, because changing macroeconomic conditions have played a role. The extent of this error is conspicuous.

Figure 1


WORKING DAYS LOST PER UNIONIST
0.06
0.10
0.14
0.18
0.22
0.26
0.30
0.34
J
S
D
M
J
S
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J
S
D
M
J
1983
1984
1985
1986
YEAR
ACTUAL
COMPLEX
SIMPLE

VII. Conclusion

This paper presented econometric evidence of Australian strike activity. Although unambiguous interpretation of results is not possible, it is at least clear that the models performed more adequately than those previously used. Estimations were subjected to, and in general passed, a battery of diagnostic tests. The results suggest the following. First, inflation, overtime and the interaction of overtime and profits influence working days lost per unionist. Second, the proportion of workers involved in strikes is associated with inflation and overtime, and to a lesser extent, inventories and the interaction of overtime and profits. Third, average strike duration is increased and decreased respectively with the vacancy and profit rates. Because these variables could be representing a host of factors, the results cannot be taken to be strong evidence for particular hypotheses.

Forecasting the models over the period of the Accord revealed that strike activity fell markedly in this time in a way not explainable by macroeconomic conditions. It is not unreasonable to attribute a substantial part of this decrease to improved industrial relations engendered by the Accord but, of course, other unmeasured factors may have contributed.

Commentators attributing the entire decrease in working days lost to the industrial relations consequences of the Accord overstate the true situation. The extent of this overstatement is of the order of 44 per cent. It is thus apparent that there is considerable merit in *ceteris paribus* approaches to such policy questions. Even so, the bottom line is that while changing macroeconomic conditions have been influential in diminishing industrial disputation, recent levels of strike activity are significantly lower than forecast on the basis of these variables alone.

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[1] See, for example, the comments of Mr MacBean (senior vice-president, ACTU) as reported in the Sydney Morning Herald, July 27, 1985.

[2] Appendix 1 presents data on strike activity for the 1959(3) to 1985(4) period.

[3] Wooden and Creigh (1983) note that "Resumption without negotiation was the major mode of dispute termination throughout the [post World War II] period ... in 1976-1980 ... it accounted for almost 59 per cent of total days lost through stoppages" (p.35). We note, however, that in this five year period there were two major politically motivated strikes (see section IV).

[4] There is general support for this view. See Bentley and Hughes (1970, 1971), Isaac (1982), Oxnam (1953) and Wooden and Creigh (1983).

[5] See Efron and Gong (1983) for an explanation of the bootstrap.

[6] See Hausman (1978).

[7] This standard error is computed from the asymptotic approximation of linearising the product of the two equations around the true values of the parameters by a first-order Taylor's series expansion.

[8] Some commentators argue that strike activity has fallen all over the world and thus, by implication, that estimates such as those presented substantially exaggerate the effect of the Accord. The point is not obvious, however. In the UK, for example, working days lost from disputes *increased* by 22 per cent on average in 1983-85 compared with 1975-82. As is usually noted there was a significant fall in days lost in the US over the same period—of the order of 54 per cent. For Australia the decrease was about 25 per cent.

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DATA APPENDIX

WDL - quarterly working days loss due to strikes including stand-downs in establishments where strikes are in progress. Secondary idleness or stand-downs in establishments where strikes are not directly in progress in not measured by available statistics. Source – ABS. Data pre-1963 were obtained from the now defunct Monthly Bulletin of Employment Statistics from which monthly data were used to obtain the quarterly estimate.

WI - quarterly number of workers involved in strikes. Source – ABS.

U - number of registered unionists. Data are available only on an annual basis. Source – ABS.

INF - deseasonalised quarterly rate of inflation based on the Consumer Price Index for All Groups, Weighted Average of Siz State Capitals. Source – ABS.

OT - average weekly hours of overtime worked per employee, quarterly average. Source – ABS.

PROF - seasonally adjusted quarterly gross operating surplus of corporate trading enterprises divided by quarterly national wage bill. The wage bill was constructed as the product of the average earnings of non-farm wage and salary earners and the number of non-farm civilian wage and salary earners (National Accounts Basis). Source – ABS.

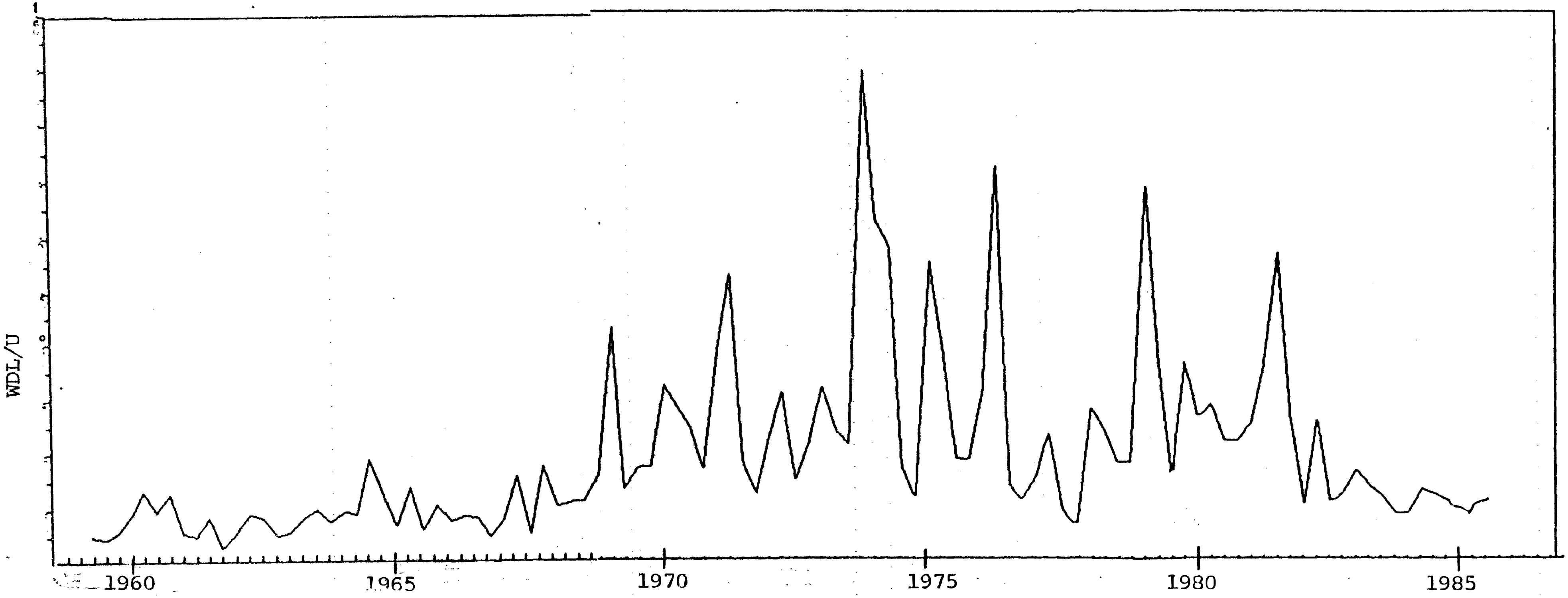
VAC - deseasonalised number of job vacancies registered with the Commonwealth Employment Service per non-farm civilian wage and salary earner. Source – ABS. Missing data (not officially available) for the first quarter 1981 to the first quarter 1983 were filled by splicing in the vacancy rate data from the ANZ Bank Survey of Job Vacancies. The vancancy rate data were deseasonalised as follows: the June quarter was taken as the reference quarter and vacancy rates were adjusted down by 7.0 percent for the September quarter, 25.8 percent for the December quarter and 22.2 percent for the March quarter. This pattern reflects the major hiring of school leavers in the months November through February.

INV - average quarterly number of inventories divided by gross non-farm product at market prices both seasonally adjusted. Inventories were constructed from the ABS seasonally adjusted series on changes in inventories. Inventories were set at $6899m in 1979-80 prices for the second quarter of 1959, figure estimated from the national accounts. This number was then adjusted quarterly to build up the inventory series.

OP - this is a generated regressor constructed by first regressing OT and PROF on a linear trend. In the 12 quarters when residuals of this regression were respectively positive and negative the pair of residuals were multiplied together to form OP. In quarters when this condition did not hold OP was set equal to zero. This is a variation on the usual form of interacting regression variables, which is to either multiply them together or divided one by the other.

Appendix 1

Working Days Lost per Unionist



	Phipps	Bentley and Hughes				Perry
Dependent Variable / Independent Variable	Strikes per Unionist(S/U)	S/U	WDL/U	WI/U	WDL/WI	WDL/U
Intercept	42.323 (1.15)	158.724 (18.64)	109.964 (3.27)	0.448 (8.49)	1.239 (5.73)	117.163 (3.31)
PROF-1	5.605 (1.19)					
INF	14.342 (0.03)					6029.281 (5.97)
Unemployment Rate (UN)		-26.797 (9.20)	-49.028 (4.26)	-0.0315 (1.75)	-0.0120 (0.16)	-7.641 (1.01)
Δ Unemployment Rate		-5.968 (0.74)	5.226 (0.16)	0.0139 (0.28)	-0.196 (0.96)	
(VAC - UN)	9803.99 (4.02)					
T	1.614 (6.03)	2.596 (12.61)	5.564 (6.83)	0.00322 (2.52)	0.0174 (3.34)	
S1	29.688 (2.51)	30.630 (3.38)	19.904 (0.56)	-0.0300 (0.53)	0.164 (0.71)	23.564 (0.63)
S2	-16.533 (1.42)	-10.482 (1.17)	-55.373 (1.56)	-0.117 (2.10)	0.301 (1.32)	-55.247 (1.49)
S3	-8.369 (0.72)	-4.094 (0.45)	-39.229 (1.11)	-0.126 (2.26)	0.220 (0.97)	-40.666 (1.10)
692	-19.820 (0.49)	-36.974 (1.20)	184.875 (1.52)	1.310 (30.92)	-0.600 (0.77)	285.863 (2.25)
763	-47.114 (1.18)	-51.191 (1.67)	449.982 (3.71)	2.277 (11.96)	-1.0400 (1.33)	447.216 (3.52)
792	-12.859 (0.32)	4.492 (0.14)	434.703 (3.54)	1.747 (9.08)	-1.086 (1.38)	432.706 (3.37)

Lag	ACF	t-statistic	ACF	t-statistic	ACF	t-statistic	ACF	t-statistic	ACF	t-statistic	ACF	t-statistic
1	0.629	6.13	0.509	4.96	0.324	3.15	-0.0066	-0.065	0.0393	0.38	0.253	2.46
2	0.508	4.95	0.311	3.03	0.0328	0.32	0.239	2.33	0.0870	0.85	-0.111	-1.08
3	0.482	4.69	0.304	2.96	0.0695	0.68	-0.0354	-0.34	-0.0446	-0.43	-0.00799	-0.078
4	0.268	2.62	0.102	0.99	0.193	1.88	-0.0998	-0.49	0.0618	0.60	0.164	1.60
5	0.154	1.51	0.0348	0.39	0.271	2.64	0.0117	0.11	0.0372	0.36	0.241	2.35

* absolute t-statistics in parentheses.

Paper 5

AUSTRALIAN STRIKE ACTIVITY IN INTERNATIONAL CONTEXT: 1964-85

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I. INTRODUCTION

The relative level of strike activity delivered by an industrial relations system involving a Conciliation and Arbitration Commission (CAC) is central to current Australian policy debate. International comparisons of industrial disputation provide important (albeit imperfect) insights into this issue. Simple comparisons of average working days lost are frequently reported and used to support arguments about the efficacy of the Australian system. A more accurate and different picture emerges when judgements make allowance for definitional differences in data collection and in the general macroeconomic conditions of countries. This paper addresses the question in the light of these factors.

Section II considers the issue of international comparisons of strike activity statistics. It is rarely recognised[1] that industrial disputes data differ markedly between countries in terms of methods of compilation and the criteria used for inclusion of stoppages, a fact that warrants suspect strong conclusions of the relative efficacy of industrial relations systems based on such data. We document the differences, note some biases and report average annual time series data for fourteen OECD countries for the 1964-85 period.

Section III makes a second methodological point. To be meaningful reflections of industrial relations evironments, international dispute data must control for (independent) influences on strikes. Multiple regression techniques, holding constant the effects of inflation and unemployment, *inter alia*, allow more useful analysis. The major findings are as follows. If the Australian industrial relations system had been *hypothetically* transposed to other countries the English speaking and Southern European countries in the sample would have experienced fewer working days lost per employee from strikes than was their actual historical record. Conversely, Northern European countries and Japan would have experienced higher levels of industrial disputation with the Australian system than was the case.

This counterfactual experiment, in conjunction with features of the data to be explained, leads to the conclusion that the Australian industrial relations system has performed at about the international norm, at least with respect to its ability to deliver low levels of strike activity. The progress made in dealing with the complexities of data and method in international comparisons of industrial disputation is summarised in a concluding section.

II. AGGREGATE COMPARISONS OF INTERNATIONAL STRIKE ACTIVITY

Strike activity may be measured in several different ways. This point is clarified by considering the following identity:

$$WDL/E = WDL/SF \, . \, SF/E \qquad (1)$$

where WDL/E is working days lost from strikes per employee and SF is strike frequency. It follows that WDL/SF is the average duration of a strike. Thus WDL/E measures the influence of both strike frequency and average strike duration. Because of its composite nature we have chosen to focus on this variable for the analysis following.

The countries for which data are available are Australia, USA, Canada, UK, Japan, Italy, Sweden, Finland, France, West Germany, Norway, New Zealand, Ireland and Spain. No consistent criteria are used across countries for inclusion or collection of strike statistics, a fact that makes simple comparisons of strike levels an inappropriate indicator of relative industrial disputation. This point can be highlighted by considering Table 1, which presents information on the collection basis for each of the countries concerned.

Table 1 shows marked inter-country differences in the meaning of published disputes data. The following points are important. First, in France, Japan, New Zealand, the UK and the USA so-called "political" stoppages are not counted. If this was the case for Australia, for example, the major strikes related to the Clarrie O'Shea (1969) and Medibank (1976) conflicts would not be included. It is difficult to assess the impact of this criterion, without a case-by-case consideration of each country. However, it is clear that Australia, Canada, Finland, West Germany, Ireland, Italy (since 1975), Norway, Spain and Sweden have higher reported strike activity than would be the case if their data inclusion was similar to the other countries in this respect.

Secondly, the statistics of Canada, France, West Germany, Italy, Japan, Norway and Sweden do not include layoffs of indirectly affected workers in the same firm.[2] Again, no information is available to allow useful insights into the quantitative significance of this issue, but it is clear that countries including such workers in industrial disputes data inflate relatively the dimensions of the stoppages. The countries in this category are Australia, Finland, Ireland, New Zealand, Spain, the UK and the USA.

Thirdly, the minimum size criteria for inclusion of industrial stoppages in the strike statistics vary greatly. It would be fair to say that Australia, Canada, Finland, France, West Germany, Ireland, Italy, Spain, Sweden and the UK are relatively stringent in inclusion requirements, while Japan, New Zealand, Norway and the United States are not.

In summary, considerable caution is appropriate in comparisons of international strike activity data. Importantly, however, for the major issue of this paper, it is apparent that the Australian data are relatively comprehensive. It follows that collection and inclusion criteria are such as to exaggerate industrial disputation in this country compared to most other countries.[3] To put the point differently, some component of identified relative Australian strike activity is a reflection of measurement and collection stringency and is not attributable simply to the nature of the industrial relations system.

TABLE 1: Industrial Disputes: International Comparisons of Coverage and Methodology, 1985

	Minimum criteria for inclusion in statistics	Are political stoppages included	Are indirectly affected workers in the same firm included	Sources and Notes
Australia	10 or more working days lost.	Yes	Yes	Information gathered from arbitrators, employers and unions.
Canada	10 or more working days lost or of more than a half day's duration.	Yes	No	Reports from Canada Manpower Centres also Press and Provincial Labor Depts.
Finland	More than 4 hours' duration unless 100 or more working days lost.	Yes	Yes	Returns from mail questionnaires to employers and employees.
France	No restrictions on size. However, public sector and agricultural employees are excluded from statistics.	No	No	Labour inspectors' reports.
Germany (F.R.)	More than 10 workers involved and more than 1 day's duration unless 100 or more working days lost.	Yes	No	Compulsory notification by employers to Labour Offices.
Ireland	10 or more days lost or of more than one day's duration.	Yes	Yes	Reports from local employment offices.
Italy	No restrictions on size.	Yes since 1975	No	Local police reports sent to Central Institute of Statistics.
Japan	More than half a day's duration.	No	No	Interviews by Prefectorial Labour Policy Section or local Labour Policy Office of employers and employees.

Table 1 continued

New Zealand	More than 10 working days lost. Statistics exclude public sector strikes.	No	Yes	Information gathered by district offices of Dept. of Labour.
Norway	More than one day's duration.	Yes	No	Questions to employees' and employers' organisations.
Spain	No restrictions on size.	Yes	Yes	Monthly returns made by local province delegates of Ministry of Labour Statistics. Figures exclude Catalonia.
Sweden	More than one hour's duration.	Yes	No	Press reports compiled by State Conciliation Service are checked by employers' organisations and sent to Central Statistical Office.
United Kingdom	More than ten workers involved and of more than one day's duration unless 100 or more working days lost.	No	Yes	Local unemployment benefit offices make reports to Department of Employment HQ, which also checks press, unions, and large employers.
United States	More than one day's or shift's duration and more than 1,000 workers involved.	No	Yes	Reports from press, employers, unions and agencies, followed up by questionnaires.

Source: Employment Gazette, July 1986, p.268.

With these observations in mind, it is useful to consider average data for each of the countries for particular sub-periods from 1964 to 1985. They are presented in Table 2, and the aggregate data are represented pictorially in Figure 1.

The data of Table 2 and Figure 1 illustrate the following. First, for the period as a whole Australian strike activity was about 20 percent greater than the (unweighted) average of the other thirteen countries, WDL/E per annum being .475 and .396 respectively. Australia had the 10th highest level of the fourteen countries. From these data it appears that Australia experienced relatively high WDL/E.

Secondly, apart from Ireland and Japan, the 1971-82 period was characterised by increased levels of disputation relative to 1964-70, a point that is particularly apposite for Australia. Although Australia's ranking was the same as for 1964-70, WDL/E increased by 123 percent in 1971-82, compared to other countries' average increase of about 40 percent.

Thirdly, while strike activity decreased markedly on average for these OECD countries in 1983-85 (about 36 percent), the decrease for Australia was relatively great (about 59 percent). This is (weak) evidence that the environment generated by the Prices and Incomes Accord contributed to decreases in industrial disputation,[4] an issue addressed more rigorously in Beggs and Chapman (forthcoming). The relative decrease in Australian WDL/E in 1983-85 is reflected in a change of ranking from 10th in both 1971-82 and 1964-70 to 7th in the most recent period.

As has been pointed out, however, it is important to be wary of strong conclusions drawn from the data of Table 2. Measurement and reporting factors tend to overstate Australian strike activity relative to the average of other countries. As well, the information presented takes no account of country-specific macroeconomic influences, independent of the industrial relations environment, that affect disputation. This issue is examined below, and a solution to the problem is proposed and implemented.

III CONTROLLING FOR THE INFLUENCE OF NON-INSTITUTIONAL STRIKE DETERMINANTS

The data presented above have the potential for seriously misrepresenting comparisons of countries' industrial relations environment. Many economic factors affect strike incidence and duration, and these have differed between the countries concerned. Because average statistics hide the influence of these variables there is a need for caution in assessing strike activity differences as manifestations only of country-specific employee-employer relationships.

Economic theory (Hicks (1932); Aschenfelter and Johnson (1968)) and econometric analysis (Bentley and Hughes (1970); Phipps (1977); Creigh (1986); Beggs and Chapman

TABLE 2: Average Annual Working Days Lost Per Employee for Fourteen OECD Countries

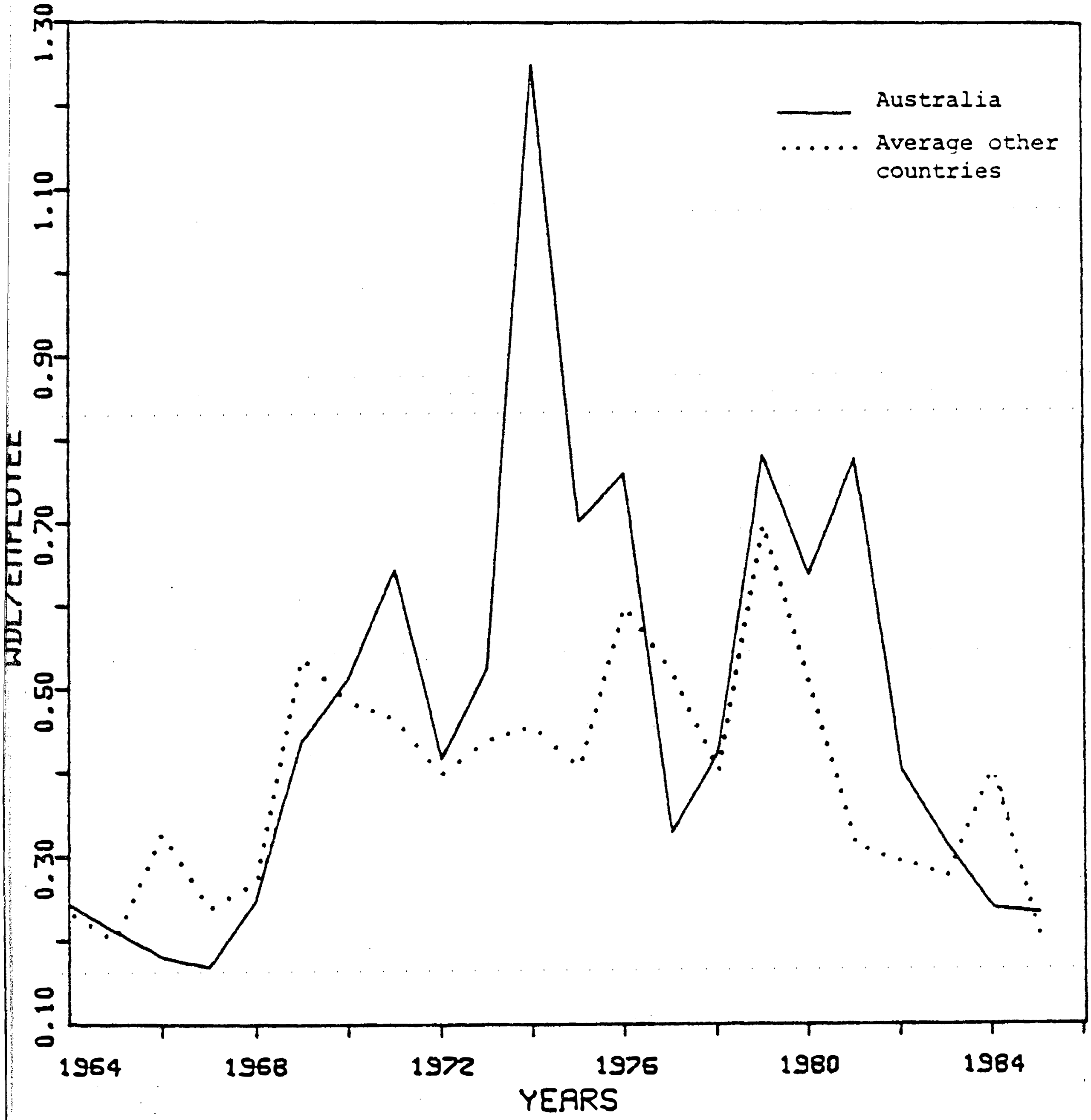
Country	Period									
	1964 - 85		1964 - 70		1971 - 82		1983 - 85		1964-70 to 1971-82 per	1971-82 to 1983-85 per
	Level	Rank	Level	Rank	Level	Rank	Level	Rank		
Australia	.475	10	.286	10	.638	10	.262	7	123.1	-58.9
Canada	.751	13	.704	12	.869	13	.390	8	23.4	-55.1
Finland	.419	8	.110	5	.605	9	.399	9	450.0	-34.0
France	.158	5	.140	7	.191	5	.070	4	36.4	-63.4
Ireland	.694	12	.893	13	.639	11	.453	10	-28.4	-29.1
Italy	1.238	14	1.273	14	1.379	14	.592	13	8.3	-57.1
Japan	.095*	4	.112	6	.098	3	.010*	2	-12.5	-89.8
New Zealand	.285	6	.150	8	.314	6	.486	11	109.3	54.8
Norway	.038	1	.012	2	.055	2	.034	3	358.0	-38.2
Spain	.541	11	.049	4	.803	12	.640	14	1538.8	-20.3
Sweden	.087*	3	.029	3	.134	4	.009*	1	362.0	-93.3
UK	.426	9	.207	9	.514	8	.584	12	148.3	13.6
USA	.375	7	.583	11	.318	7	.119	6	-45.5	-52.6
W.Germany	.038	1	.006	1	.045	1	.088	5	650.0	95.6
Average	.396*		.328		.459		.298*		39.9	-36.1[a]
excluding Australia	.451**						.322**			-29.9[b]

Notes: * WDL/E data for 1985 were unavailable for Japan and Sweden. We assumed 1984 experience was repeated in 1985.
** Excluding Japan and Sweden.

Source: International Labour Organisation (1986) and information offices of embassies.

FIGURE 1

Strike Activity: Australia and Thirteen OECD Countries



(forthcoming)) support particular hypotheses concerning the role of macroeconomic variables as strike determinants. In this research it is argued typically that measures of both labour demand (such as the unemployment rate) and informational uncertainty (such as the rate of price inflation) are explanators of strike activity. Because theoretical perspectives differ in emphasis, it is wise to be circumspect in interpreting results (Phipps (1977); Beggs and Chapman (forthcoming)). Nevertheless, there is general agreement that macroeconomic variables influence industrial disputation.

It follows from the above that if there are international differences in macroeconomic conditions, these will be manifested to some extent in strike activity data. Clearly, distinguishing the impact of the industrial relations environment from these conditions is important, a challenge which can be approached in the following way. The method is to estimate an Australian strike equation and use it to predict other countries' disputation on the basis of the latters' macroeconomic experience. If predicted strike activity exceeds that actually experienced, this suggests that imposing the Australian industrial relations environment would have increased industrial disputation in the country concerned, the opposite being the case if predicted outcomes are less than actual. Implicitly, but clearly, this approach interprets the intercept and coefficients of the strike equation as a reflection of the impact of the industrial relations environment on disputation.

In adopting this method we estimated the following model with annual Australian data for the period 1964 to 1985:

$$WDL/E_i = a + bINF_i + cUN_i + dTIME_i + eACCORD + \varepsilon \qquad (2)$$

where, for year i, WDL/E is working days lost from strikes per employee, INF is the percentage change (from the previous year) in the consumer price index, UN is the aggregate unemployment rate, TIME is self-explanatory, and Accord is a dummy variable equal to 1 for the Accord period (1983-85), and equal to zero otherwise. Theory and other empirical analyses suggest that $b > 0$, $c,e < 0$ and $d < 0$. Estimation of equation (2) in log-log form gave:

$$\underset{}{WDL/E} = \underset{(9.53)}{-1.867} + \underset{(3.71)}{0.583INF} - \underset{(3.19)}{1.00UN} + \underset{(3.12)}{0.107TIME} - \underset{(2.28)}{0.621ACCORD}$$

$$\overline{R^2} = 0.66 \qquad D.W. = 1.98$$

where absolute heteroscedastic consistent t-statistics are in parentheses.

Several comments on this result are in order. First, the equation passed a RESET2 (Ramsey (1969)) functional form test and exhibited no sign of serial correlation in the disturbances up to fourth order lags. The absence of evidence of functional or dynamic mis-specification, coefficient significance and the consistency of signs with theory suggest that the model is statistically adequate.

Secondly, the estimated relationships are very similar to those obtained in a much more tested analysis of Australian quarterly data for the 1959(1) to 1986(2) period (Beggs and Chapman (forthcoming)). For example, the decrease in WDL/E for the period of the Accord is almost identical in the two estimations (i.e. 62 percent).

Thirdly, as a criticism of the simple equation it is arguable that the industrial relations environment itself influences inflation and unemployment and that the model is misspecified. While this may be true in theory we note two things. One, if union activity increases both inflation (through nominal wage increase) and unemployment (through real wages being higher than otherwise)—the common perspectives—these effects will have counter-balancing implications for WDL/E because of the positive and negative signs, respectively of the regressors. Two, endogeneity tests in the quarterly estimations (Beggs and Chapman (forthcoming)) did not reveal any significant empirical problem in this regard. In short, the estimation appears to be useful for its intended purpose.

Fourthly, a comment on the TIME variable is warranted. We interpret the time coefficient to be a reflection of changes in industrial structure and their associated impact on strikes. However, other views are plausible, one of which is consistent with the basic thrust of the research. This is the possibility that time is correlated with exogenous changes in the industrial relations environment. One the other hand, time may be picking up systematic measurement errors in the other regressors, a potential difficulty that remains uncorrected.

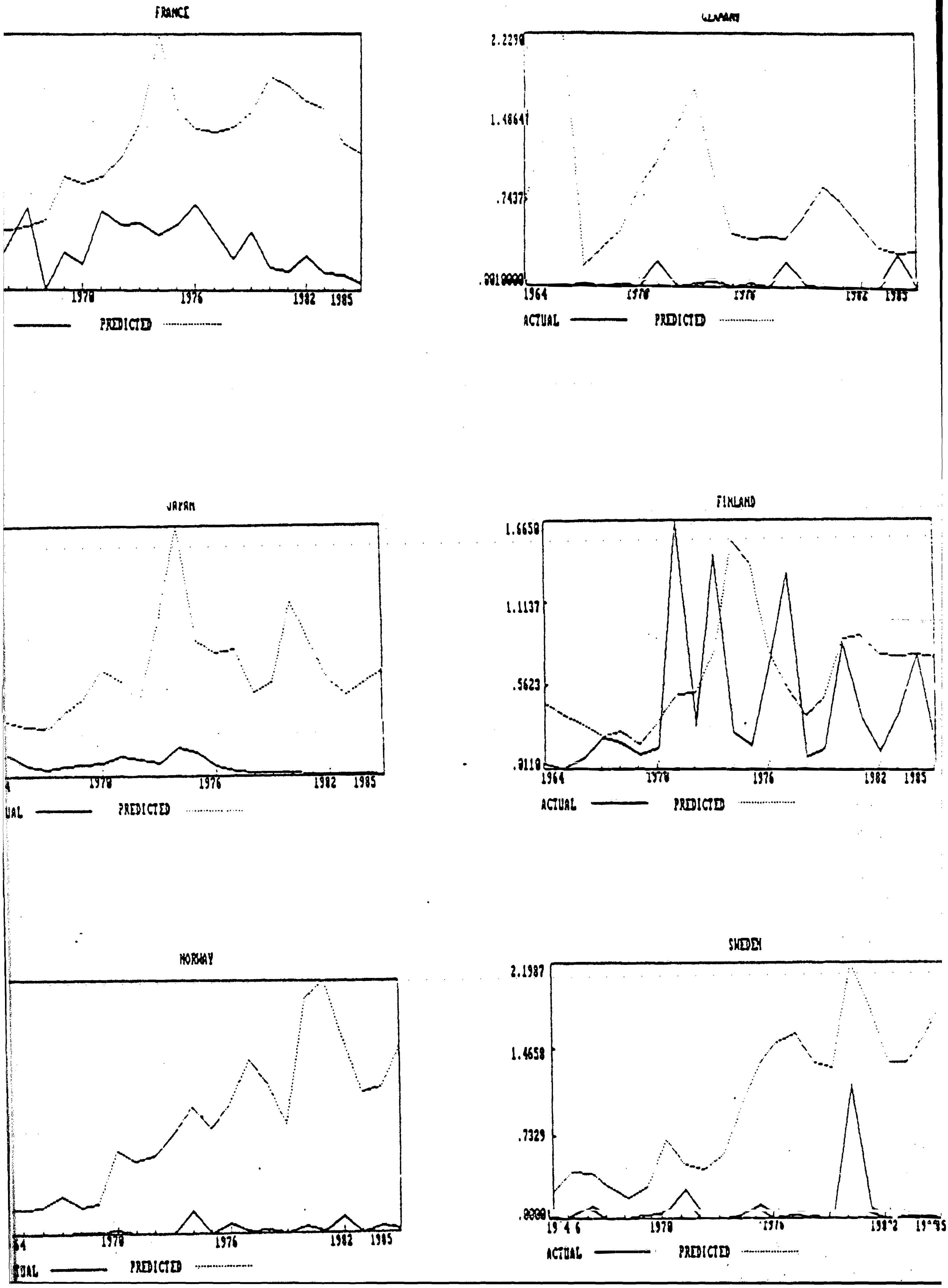
The coefficients were used to predict strike activity for 12 OECD countries for which data were available in the 1964-85 period.[5] The actual and predicted average WDL/E are presented in Table 3, and the plots of the data are shown in Figure 2.

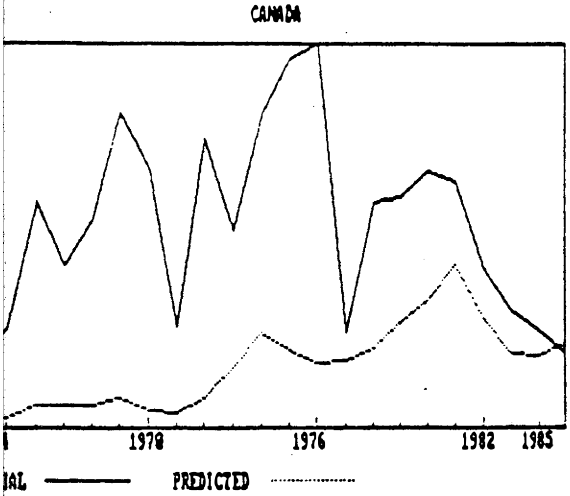
TABLE 3: Actual and Predicted Average WDL/E, 1964-85

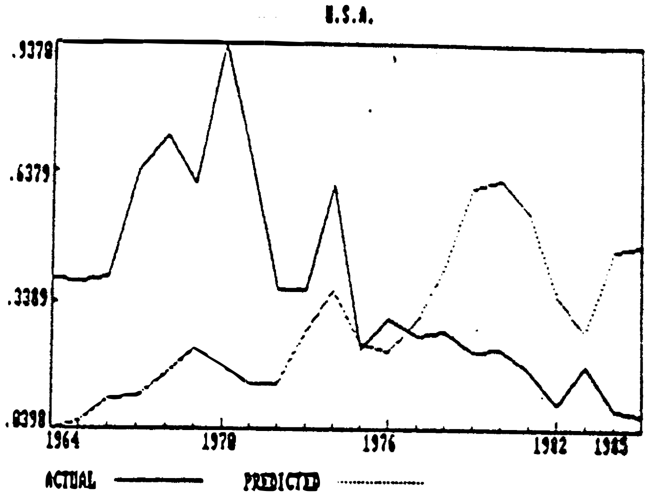
Country	Actual (i)	Predicted (ii)	(iii) (i) - (ii)
Canada	0.751	0.268	0.483
USA	0.375	0.290	0.0851
UK	0.426	0.418	0.00804
Ireland	0.694	0.327	0.368
Italy	1.238	0.373	0.865
Spain	0.541	0.542	-0.00100
France	0.158	0.468	-0.310
Germany	0.0382	0.749	-0.710
Japan	0.0902	0.942	-0.852
Finland	0.419	0.618	-0.198
Norway	0.0384	1.081	-1.043
Sweden	0.0831	1.000	-0.918

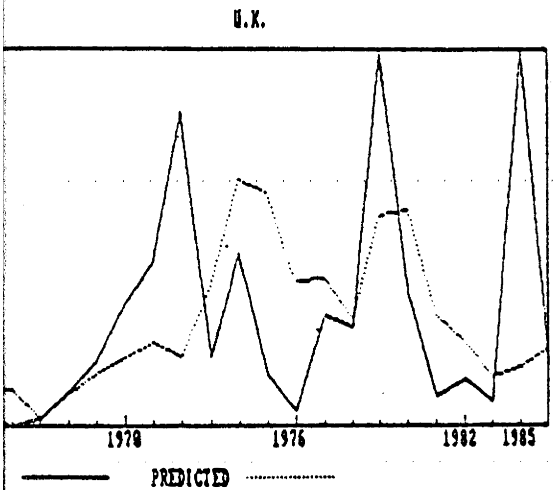
FIGURE 2

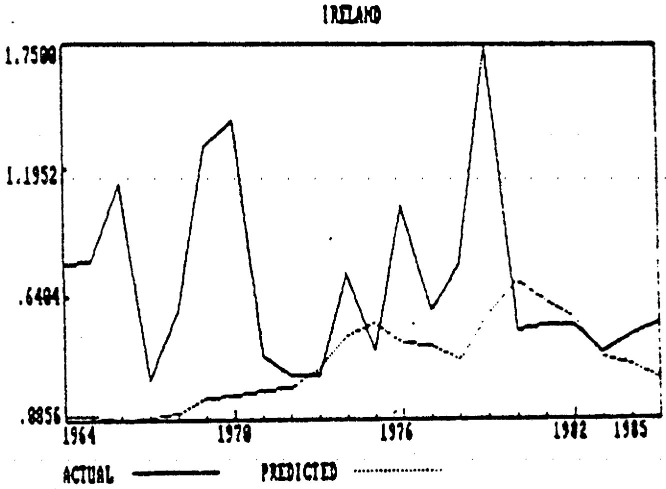
Actual and Predicted Strike Activity Using Australian Regression Coefficients

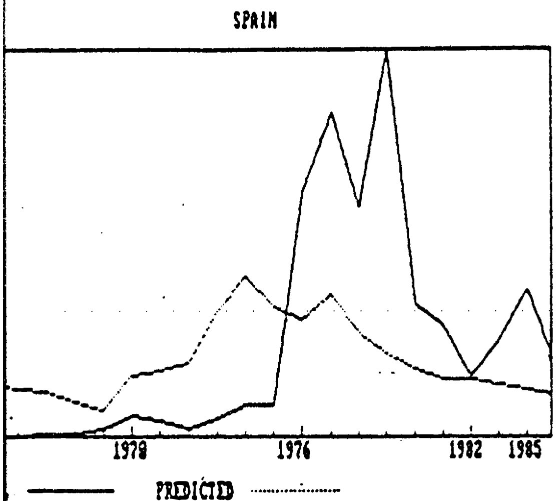


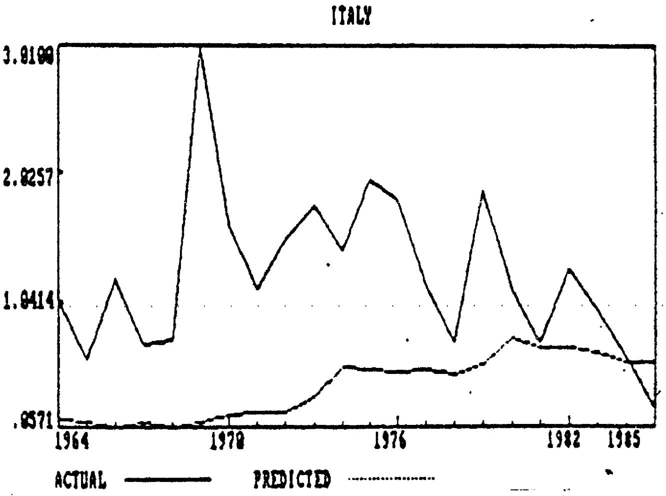

CANADA
1970
1976
1982
1985
PREDICTED


U.S.A.
.9378
.6379
.3389
.0398
1964
1970
1976
1982
1985
ACTUAL
PREDICTED


U.K.
1970
1976
1982
1985
PREDICTED


IRELAND
1.7500
1.1952
.6404
.0856
1964
1970
1976
1982
1985
ACTUAL
PREDICTED


SPAIN
1970
1976
1982
1985
PREDICTED


ITALY
3.0100
2.0257
1.0414
.0571
1964
1970
1976
1982
1985
ACTUAL
PREDICTED

The data and plots reveal some interesting results. Apparently, by this test the Australian industrial relations environment delivers lower strike activity than the other (mainly) English speaking countries (US, UK, Canada and Ireland) and the two Southern European countries (Italy and Spain). Greater strike activity is predicted in the Norther European countries and Japan.[6] Canada, the country most like Australia in terms of economic and industrial structure, would have achieved a very substantial reduction in strike activity if it had been hypothetically endowed with the Australian industrial relations system. It is also remarkable that, hypothetically, the Australian system would have delivered very much higher WDL/E in Norway, Sweden, France, Germany and Japan.

Interestingly, it is apparently the case that Australian strike activity was lower than, or not different to, the politically and culturally quite similar countries of the US, the UK and Canada. These last two countries have about the same union coverage as Australia but, obviously, no Conciliation and Arbitration Commission. This poses a problem for the argument that the presence of the Commission has a marked deleterious effect on the Australian industrial relations environment. A cautious and fair conclusion is that the data offer no compelling evidence that the Australian system is more strike prone than similar countries.

It is important to keep in mind that the above conclusions are reinforced by the cross-country differences in the measurement and collection criteria for strike activity outlined in section II. The general point from this analysis is that Australian statistics are relatively comprehensive or, to put the issue differently, some part of measured (higher) Australian industrial disputation levels are a consequence only of the data. This means that the regression exercise exaggerates Australian relative strike proneness since the measurement factor is most likely captured in the intercept term, implying that the use of the Australian equation in predictions of other countries' strike activity overstates the relative disharmony of our industrial relations environment.

IV. CONCLUSION

The use of aggregate statistics in assessments of the efficacy of countries' industrial relations systems reveals that Australia experienced high levels of industrial disputation in the 1964-85 period. Of the 14 OECD countries for which data are available, Australia had the tenth highest level of strike activity, although both the ranking and level fell significantly in 1983-85, the early period of the Prices and Incomes Accord. There are, however, (at least) two important reasons to be wary of the use of the aggregate comparative data as reflections of countries' industrial relations environments.

The first concerns the inclusion criteria and coverage of strike activity statistics. Countries differ widely in both, and there is no doubt that straightforward comparisons of levels have within them the potential for serious misrepresentation of the implications for industrial disputation of particular systems. Australian data, it appears, are relatively comprehensive. This implies that some part of Australia's relatively high level of reported strike activity is attributable to measurement.

Secondly, macroeconomic variables influence the incidence and duration of strikes. This simple observation renders suspect the use of international comparisons of disputes data as reflections only of industrial relations systems, since differences in macroeconomic experience will manifest themselves, to some extent, in these data. Through control for inflation and unemployment, *inter alia*, a more accurate assessment of the Australian industrial relations system's effect on industrial disputation in an international context has been made possible. An important point arising from the exercise is that the Australian system apparently delivers lower, or about the same, strike activity as do culturally and politically similar countries. Clearly, this result is of value to the current debate. At the minimum it renders dubious strong statements concerning the influence of Australia's idiosyncratic labour market arrangements on industrial disputation.

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[1]Creigh (1986) is a refreshing exception to this rule.

[2]Secondary lay-offs (that is, workers stood-down outside the firms involved in the strike) are not included in the data of any of the countries in this study.

[3]Finland, Ireland and Spain are the other countries with equivalently comprehensive collection criteria to those used in Australia.

[4]It is important to note that there is no reason to believe this change to be permanent in Australia. More recently, WDL from strikes have increased somewhat: by about 11.5 and 7.5 percent comparing the year ending August 1986 with the years ending August 1985 and August 1984 respectively. Source: ABS, *Industrial Disputes*, Cat. No. 6321.0.

[5]New Zealand was excluded because of the low quality of the unemployment rate data. 1985 information on WDL/E was unavailable for Japan and Sweden, the analysis being restricted to 1964-84 for these two countries.

[6]After the correction for macroeconomic conditions Finland and Australia have approximately the same strike proneness.

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Paper 6

AUSTRALIAN STRIKE ACTIVITY IN INTERNATIONAL CONTEXT: 1964-85 REJOINDER

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We wish to thank Dick Blandy for encouraging debate in this very important policy area.

Professor Blandy's spirited critique of our paper allows us the welcome opportunity to emphasise the measurement problems inherent in international comparisons of strike data, to highlight the overall meaning of the results, and to update the econometric estimations. All three endeavours serve to reinforce our so-called "strong" and "unwarranted" conclusion that "At the minimum, [the result] renders dubious strong statements concerning the influence of Australia's idiosyncratic labour market arrangements on industrial disputation." (Beggs and Chapman, 1987a, p.149).

The first cannon ball fired at us is that "These data show ... that if these countries had had the Australian system, Germany, France, Sweden, Norway and Japan would have had a persistently *worse* strike outcome over the whole period since 1964, mostly by a huge margin" (Blandy, 1988, p....). We don't hide from this insight, and even claim it as ours, given the surprisingly similar sentence in the original paper: "It is also remarkable that, hypothetically, the Australian system would have delivered very much higher WDL/E[1] in Norway, Sweden, France, West Germany and Japan" (Beggs and Chapman, 1987a, p.146).

Even though it is obvious that we have explicitly acknowledged the 5 out of the 12 countries which apparently had far less strike activity than Australia over the 1964-85 period, here is an opportune space to emphasise the very substantial differences between countries in the measurement and reporting of strike activity. As is clear from Table 1 of the original paper, Australian coverage is probably more comprehensive than any other country. Of the five referred to above, unlike Australia *none* include in their strike data indirectly affected workers in the same firm, and neither France nor Japan include political stoppages. As well, the criteria for inclusion in Australian statistics is relatively stringent. The very important point, not acknowledged by Professor Blandy, is that measurement differences make the Australian system appear more strike-prone; our original conclusion appears comfortably moderate.

The second shot fired misses also, by a very large margin. This is the contention that *now* the Australian system is worse than all countries other than Spain and Ireland, because of the positive time trend in the (Australian) estimation. But this allegation ignores the large (62%) diminution in Australian strike activity since the institution of the prices and income Accord in 1983. Even though there is apparently an upward trend in strike activity 1964-85, the considerable structural decrease after 1982 means that even now the recent diminution is very large.

As well, and more importantly, re-estimations of the model including recent (1986 and 1987) data, reveal that strike activity has continued to decrease, and even more strongly than before. The results are presented in Table 1.

TABLE 1: Models of Australian Strike Activity

Dependent variable is the logarithm of working days lost per employee

Period	log Inflation	log Unemployment	Time Trend	Dummy Variable for years 1983 onwards	Intercept	R^2
(i) 1964-1982	0.591 (3.26)	-1.134 (3.44)	0.120 (3.08)		-1.866 (8.85)	0.71
(ii) 1964-1985	0.812 (6.79)	-1.064 (3.17)	0.794 (2.06)		-1.983 (9.76)	0.66
(iii) 1964-1985	0.582 (3.71)	-1.004 (3.19)	0.107 (3.11)	-0.621 (2.28)	1.867 (9.52)	0.72
(iv) 1964-1987	0.889 (6.95)	-0.668 (2.00)	0.024 (0.68)		-2.023 (8.80)	0.58
(v) 1964-1987	0.563 (3.12)	-0.715 (2.31)	0.078 (2.34)	-0.823 (2.92)	-1.860 (8.60)	0.69

Note: Absolute heteroskedastic consistent t-statistics in parentheses.

Two big points stand out from Table 1. One is that including the 1986 and 1987 observations increases, in absolute terms, the coefficient on the post-1983 dummy variable from 62 to 82 per cent, and decreases the coefficient on the trend rate of increase from 10.7 to 7.8 per cent. Two, the equation can be re-estimated omitting the post-1983 dummy on the basis that the so-called Accord period is a small part of the overall Australian system over the 1964 to 1987 period. When estimated in this way the time trend falls to 2.4 per cent and becomes statistically insignificant. Either way, it cannot be the case that at present the Australian industrial relations environment is delivering greater strike activity than previously. By implication, the alleged increase relative to other countries is non-existent.

In other work (Beggs and Chapman, 1987b) we explicitly tested the view that recent decreases in Australian strike activity were part of a world-wide phenomenon, the implication for Professor Blandy being that if this is the case his conjecture about recent changes is not outlandish. Estimating the models separately for each country revealed that only for Australia was there a significant diminution in WDL/E from strikes post-1983. The story, then, is unambiguous. Recent evidence wholly supports the contention of continued and unusual decreases in Australian strike activity.

We found Professor Blandy's methodological remark difficult to understand. It is only "playing tennis with the net down" to say that Australian strike activity is lower than or about the same as culturally and politically similar countries if it was our (hidden) *intention* to derive such a conclusion (it wasn't). We're not members of any Club, for reasons Groucho Marx helps us understand.

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[1] Working days lost per employee from strikes.

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Paper 7

DECLINING STRIKE ACTIVITY IN AUSTRALIA 1983-85: AN INTERNATIONAL PHENOMENON?

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I. Introduction

It is widely accepted that, relative to previous periods, Australian strike activity fell markedly after the beginning of 1983. There is as yet no concensus as to the underlying factors which contributed to this phenomenon. One view is that the successful institution of the prices and incomes Accord lead to increased industrial harmony which, by itself, delivered low levels of disputation. The other is the idea that decreases in strikes have been world-wide and, by implication, the Accord has not been relevant to falling industrial disputation in Australia. The first view is commonly asserted by members of the Labor Government and trade union officials,[1] while the second has been publicised by the Leader of the Opposition[2] and some economic journalists.[3]

Average data for 13 OECD countries tend to support the view of aggregate world-wide decreases in strike activity after 1982. Beggs and Chapman (1987b) report data for Australia and thirteen other OECD countries which show average annual working days lost per employee during the 1983-85 period to be respectively 59 percent and 36 percent below the average values for the period 1971-82. This paper contributes to the debate through the reporting of econometric analyses of the determinants of strike activity in four OECD countries for 1964-85 with a particular focus on the last three years of the period. The analysis considers both the evidence for a discrete fall in world-wide strike activity in 1983-85, and, in longer run context, considers the evidence on whether high levels of strike activity in one country can be associated with more strikes in other countries.

It is important to note at the outset that in theory, and from the results of econometric estimation, there is considerable evidence that macroeconomic conditions influence the extent, incidence and timing of industrial disputation. Previous work with Australian data (Beggs and Chapman, 1987a) has demonstrated the contribution of these factors to strike activity decreases in the post-1982 period. The point is salient to international comparisons because of the need to control for these influences in an interpretation of the implications of particular industrial relations systems for strikes. This is the approach adopted in the analysis following.

In section II the theoretical framework and econometric methodology are outlined. Issues pertinent to the data and the results, including diagnostic evaluations of the models, are presented in section III. A concluding section interprets the findings, the major one being that the statistically significant reduction in Australian strike activity during the 1983-85 period is idiosyncratic to this country and cannot reasonably be interpreted as part of a common international phenomenon. Also we find no evidence over the longer 1964-85 period that Australian strike activity is affected by the levels of disputation in the U.S., Canada or the U.K.

II. Theoretical and Econometric Issues

Economic theory (Hicks, 1932; Aschenfelter and Johnson, 1968) suggests that information differences between employees and employers, or between the rank and file and union leaders, result in unmet wage demands and industrial disharmony. These perspectives imply that economic uncertainty increases strike activity, a prediction supported generally in time series analysis of different countries using the price inflation rate as a proxy for misinformation (Pencavel, 1970; Beggs and Chapman, 1987a).

A second factor alleged to increase industrial disputation is the state of the labour market. In simple terms, high levels of labour demand encourage workers to make demands because at such times their bargaining power is relatively high,[4] ready availability of overtime decreases the foregone costs of earnings resulting from a strike. In such periods a short strike demonstrating the seriousness of the conflict to the Commonwealth Conciliation and Arbitration Commission (CCAC) may yield a speedy résolution of the dispute at little foregone wages cost to labour. An additional contributing factor is that strikes of long expected duration are less expensive to workers when vacancies are high. The availability of alternative employment diminishes the extent of foregone wages expected from the strike.

Whatever the underlying causes, it is clear that proxies for labour shortage are generally accepted to perform an important role as explanators of strike activity (Rees, 1952; Bentley and Hughes, 1970; Perry, 1979; and Beggs and Chapman, 1987a). For our purposes we chose to use the aggregate unemployment rate, an approach motivated in part by data availability.

There is little doubt that other economic variables influence industrial disputation. As examples the following are pertinent. A, firms' profits are likely to be relevant since their level will help determine both workers' propensity to make demands and firms' willingness to concede to such. Also, in periods of (presumably unanticipatedly) high inventories firms may encourage strikes in order to run down stocks. In the analysis following, these possibilities have not been modelled because of the sometime unavailability of data, and also the need to maintain a parsimonious model specification in light of the few degrees of freedom available.

The implication of the above is that the estimations should be interpreted as attempts to capture the essence, and not the detail, of strike activity determinants. To test the restrictiveness of this approach the estimations were subjected to a range of diagnostic tests. But even given statistical robustness, it is important to keep in mind the approximate nature of the method.

The simple models used in the analysis for the 1964-85 period using annual observations are of the following log-log form

$$(WDL/E)_{it} = a_i + b\,INF_{it} + c\,UN_{it} + d\,TIME_i + e\,D8385_{it} + \varepsilon_{it} \qquad (1)$$

where, for year t in country i, WDL/E is working days lost from industrial disputation per employee, INF is the annual rate of change in the consumer price index, UN is the aggregate unemployment rate, D8385 is a dummy variable equal to 1 for the years 1983, 1984 and 1985 and zero otherwise, and TIME is a linear trend taking value one in 1964.

Several comments on the model are pertinent to the investigation. First, a variety of indicators of strike activity exist, but WDL/E is the broadest measure since it combines both strike frequency and average duration. Because of its composite nature it is the most commonly used index of disputation. For further consideration of this point, see Beggs and Chapman (1987a).

Second, we chose to focus on four countries only—Australia, Canada, the United Kingdom and the United States. There are three main reasons for this decision. The first is that, culturally and politically these countries are similar, at least compared to others for which data are available.[5] In particular, Canada is often likened to Australia in terms of union coverage and economic structure. As well, apparently the UK and US governments adopted the antithesis of Australia's consensual approach to trade unions in the most recent period. Clearly, an important policy question is the implication of these approaches for industrial disputation, an issue which can be clarified through the comparisons adopted. Finally, in several of the other countries which could have been analysed (for example, Spain and Sweden) there is some evidence of marked and irregular changes in disputation over the 1964-82 period,[6] indicating the need for more exhaustive investigation were these to become central to the analysis. However, given the focus of the analysis on a subset of countries for which data are available, we also chose to report a less detailed analysis for a larger sample of 13 countries. The results, consistent with the other findings, are discussed below.

The time coefficient to be a reflection of changes in industrial structure and their associated impact on strikes. However, other views are plausible, one of which is consistent with the basic thrust of the research. This is the possibility that time is correlated with exogenous changes in the industrial relations environment. On the other hand, time may be picking up systematic measurement errors in the other regressors, a potential difficulty that remains uncorrected.

A final issue concerns the flexibility of the models used given the limitations of available information. Faced with only 22 observations, it is clear that fully convincing hypothesis testing is not possible. An approach to the lack of data is to impose cross-country restrictions on the size of particular parameters on strike activity, a method which makes more efficient use of the available information. The validity or otherwise of the restrictions is open to statistical tests. Further, the results of restricted estimations may be compared to those from more flexible forms, in order to determine the robustness of results. These methods were employed and the results are reported in section III.

An important methodological point from the above is that, while consistent findings over a range of functional forms and tests increase confidence in the accuracy of results, no illusions should be held that estimations with so few data points can offer definite evidence for particular points of view. Nevertheless, if robust in their findings, such analyses are highly suggestive, at least in terms of evidence for the rejection of extreme views.

III. Data, Results and Diagnostic Tests

Annual information on working days lost per employee, unemployment rates and inflation were collected for each of the countries, the statistical characteristics of the data being reported in Appendix 1. While the information is suitable for the purpose of the investigation, two issues related to measurement warrant reporting. The first concerns the international comparability of strike data, and is as follows.

There are differences in the criteria used among countries for inclusion or collection of industrial disputation statistics. For example, Canadian and Australian data include so-called "political" stoppages, but UK and US data do not. As well, Australia and Canada's minimum criteria for inclusion are more stringent than those of the UK and the US.[7] Clearly, the former two countries will have higher levels of measured strike activity, ceteris paribus, than is the case for the latter countries. For a more complete discussion of these issues, see Beggs and Chapman (1987b).

There are no obvious econometric implications of measurement differences across countries, so long as the reported data do not misrepresent the true situation in a way systematically related to the included explanatory variables. That is, inferences are valid if the measurement differences in WDL/E between countries are manifested only in the intercept term. While from some points of view this is not necessarily true,[8] at this stage the most that can be done is to recognise the approximations inherent in the data.

The second measurement issue relates to the unemployment data. It is widely recognised that countries differ in definitions of joblessness, a fact that may muddy the estimations. However, a comparison of adjusted and unadjusted unemployment rates for the four countries reveals little impact on levels of this potential problem (OECD, 1985).[9] Econometrically, then, the problem is not likely to be significant. Various estimations of the model are presented in Table 1. The first unconstrains all coefficients, the second restricts inflation and unemployment effects to be the same for all countries, with the third doing likewise and restricting the 1983-85 dummy to be the same for Canada, UK and US.

The estimations reported above were subjected to tests of dynamic and functional specification, the results if which are shown in Table 2.[10]

TABLE 1: Four Country Strike Equation
(Dependent variable is logarithm of working days lost per employee)

Variable	Equation	Coefficient (Absolute t-statistics)* (1)	(2)	(3)
Log Inflation Rate:	Australia	0.582 (3.06)	0.545 (4.29)	0.513 (4.06)
	Canada	0.906 (2.79)	0.545 (4.29)	0.513 (4.06)
	UK	-0.142 (0.29)	0.545 (4.29)	0.513 (4.06)
	US	0.761 (3.20)	0.545 (4.29)	0.513 (4.06)
Log Unemployment Rate:	Australia	-1.004 (2.68)	-0.566 (2.35)	-0.565 (2.39)
	Canada	-0.221 (0.31)	-0.566 (2.35)	-0.565 (2.39)
	UK	-2.026 (2.07)	-0.566 (2.35)	-0.565 (2.39)
	US	0.107 (0.24)	-0.566 (2.35)	-0.565 (2.39)
1983-85 Dummy:	Australia	-0.621 (1.95)	-0.666 (2.41)	-0.639 (2.34)
	Canada	0.185 (0.39)	-0.239 (0.77)	0.074 (0.27)
	UK	0.003 (0.00)	0.538 (0.96)	0.074 (0.27)
	US	0.698 (2.60)	0.322 (0.99)	0.074 (0.27)
Linear Trend:	Australia	0.107 (2.61)	0.068 (2.16)	0.069 (2.22)
	Canada	-0.046 (0.79)	0.006 (0.24)	-0.002 (.11)
	UK	0.229 (2.14)	0.059 (1.61)	0.076 (2.20)
	US	-0.151 (3.95)	-0.101 (4.41)	-0.091 (4.34)
Intercepts:	Australia	-1.867 (7.89)	-1.891 (9.49)	-1.848 (9.40)
	Canada	-1.036 (1.02)	-0.315 (0.84)	-0.207 (0.57)
	UK	-0.301 (0.26)	-2.137 (5.17)	-2.196 (5.67)
	US	-0.962 (1.41)	0.047 (0.12)	0.020 (00.05)
R^2	Australia	0.72		
	Canada	0.58		
	UK	0.40		
	US	0.82		

* For those interested, bootstrap confidence intervals are reported in Beggs and Chapman (1987c).

TABLE 2
Summary Tests for Dynamic Behaviour and Correctness of Functional Form for Four Country Strike Equations, Models 1 and 2, Table 1

Test		Australia	Canada	UK	US
		(1) (2)	(1) (2)	(1) (2)	(1) (2)
Durbin-Watson Statistic		1.98,1.80	2.04,2.22	1.85,1.81	1.88,2.06
Auto-correlation Function					
t-statistic	1 year lag	0.23,0.29	0.29,0.28	0.28,0.82	0.17,0.13
	2 year lag	1.41,0.88	0.08,0.07	0.45,0.55	0.09,0.32
	3 year lag	1.14,1.37	0.09,0.25	0.74,1.49	0.59,0.01
	4 year lag	0.12,0.53	0.66,0.57	1.58,1.94	0.67,0.54
RESET*		0.72,0.56	0.62,1.97	0.22,0.24	0.43,0.20
Heteroskedasticity Criterion**		0.22	0.26	0.54	0.23

* RESET statistics are the t-statistics on the predictions of the model squared re-inserted in the regression equation (Ramsey, 1969). The application to the systems estimation of model (2) is non-standard. A test that the added variables in each equation of this model are jointly zero is a Wald test which is c2 (4) and equal to 6.15 compared to the 95% critical value of 9.94, meaning an acceptance of the null hypothesis.

** This refers to the maximum of the difference between the OLS coefficient t-statistic and White's t-statistic (White, 1980) divided by White's t-statistic. The maximum is taken over all coefficients for which White's t-statistic is greater than |1.96|. A number greater than 0.50 in absolute value is taken as preliminary evidence of heteroskedasticity. Follow-up investigation of the UK equation was unable to establish direct evidence of any functional form for heteroskedasticity, but close scrutiny indicates that unusually high levels of strike activity in the UK in 1972 and 1984 are contributing to unreliable small sample behaviour in White's asymptotically heteroskedastic consistent t-statistics.

As well as the above diagnostics, two cross-country restrictions were tested. The first is an examination of the validity of constraining the effects of inflation and unemployment to be identical using an LM test. That is, estimation (2) is compared to estimation (1). The test statistic is distributed χ^2 (6) with a value of 10.38, compared to a 95 per cent critical value of 12.60. Thus the restriction is not rejected, a result implying the usefulness (in this respect) of estimations (2) and (3).[11]

Secondly, a Wald test was computed to examine the validity of the assumption that the coefficient for the 1983-85 dummy common to Canada, the UK and the US, equals the coefficient for the 1983-85 dummy for Australia which compares estimation (3) with estimation (2). The test statistic is distributed $\chi^2(1)$ with a value of 10.31, compared to the 95 per cent critical value of 3.84. The implication of this result is that the Australian reduction in strike activity was (strongly) statistically significantly greater than reductions in strike activity for the other countries.

The data allow a further test of the hypothesis that 1983-85 decreases in Australian industrial disputation are part of a world-wide experience. The "international phenomenon" perspective implies that levels of disputation are determined by political and sociological factors which are transmitted among countries by the media. These unmeasured determinants of strike activity, if they exist, will be manifested in the disturbance term of the regression model. The proposition that these factors move collectively internationally can be tested through an investigation of the extent of cross-country correlation among the individual country regression disturbances. The results of this approach are reported in Table 3.

TABLE 3

Cross-Country Correlation of Unexplained Component of Strike Activity*

Correlation Coefficients for the Residuals of Model 2, Table 1			
Australia	Canada	UK	US
Australia	0.514	0.269	0.268
Canada	1	0.144	0.217
UK	-	1	0.135
US	-	-	1

* The Lagrange multiplier test that the off-diagonal elements of the correlation matrix are jointly zero is distributed c2 (6) with a value of 10.37. The 95% critical value is 12.60, which means that the null hypothesis is not rejected.

The data of Table 3 do not support the "international phenomenon" explanation of decreases in strike activity. Only between Australia and Canada is there some correlation between the regression disturbances of the strike estimations. Importantly, the US and the UK, probably the "fashion" leaders in such areas, have low correlations with one another and with both Australia and Canada. A test accepted the joint null hypothesis that all the cross-country correlations are zero.

A more specific conjecture in the spirit of the above approach is that a causal mechanism operates among the disturbance terms of the model. For example, unmodelled determinants of strike activity in the US and the UK may causally affect strike activity in Australia (e.g. by television or newspaper transmission), yet the mechanism may not be present in the reverse direction—from Australia to the US or the UK. This idea generalises the simple pair-wise country correlations reported in Table 3.

The causality proposition can be tested by examining the joint associations of the disturbances of countries' strike equations to explain the disturbances in a specific country's equation. The method is to regress estimated residuals of one equation on the estimated residuals of the other explanator equations. Unfortunately, this regression only has asymptotically correct t-statistics under the null hypothesis that all coefficients on the residuals appearing as explanators in the model are jointly zero (Pagan, 1984). Consequently, only the F-statistic for this joint null hypothesis is reported below.

Three models of causal transmission in the regression residuals are considered and the results are reported in Table 4. In each case the dependent variable is the residual from the unconstrained single country equations reported in Table 1, column 1. The first set of regressions use the contemporaneous residuals of each of the other equations as explanators, and the second use these residuals lagged by one year to allow time for the transmission of effects.

The third set of results allow for a cruder information system in the cross-country transmission mechanism, an idea which is best explained with the following example. Suppose that unionists in Australia do not recognise the role of inflation and unemployment on the level of strike activity in the US. Suppose they have a simpler model which only includes the level of and trend in industrial disputation in the US. When the level of disputes is seen to rise above this trend the unspecific sociological model takes this as a signal to step-up industrial claims on employers. To model the simpler view of the world, the logarithm of working days lost per employee variable was regressed on a constant term and a linear trend for each of the four countries and the residuals were saved to be used as explanators in the residual on residuals regressions.

The results reported in Table 4 show no evidence that unmodelled disturbances in the Canadian, UK or US strike levels are transmitted in the Australian industrial relations system. F-statistics for the regression equations generally suggest no significant statistical associations. The results are reported for Australia both with Canada included and excluded, without altering the findings.

Table 4 also shows negligible international evidence of a causal link in the disturbances of the strike equations. The only exceptional result is in the one year lagged effect of Australian and US strike disturbances on UK strike activity. Because the US coefficient is too large to be plausible, it is quite possible that this is a fluke finding given the small sample. Further research is required here. The bottom line from these approaches is that there is no statistical evidence supportive of the view that some unmeasured phenomenon is systematically influencing recent Australian industrial disputation.

TABLE 4

Cross-Country Regression Models of the Unexplained Component of Strike Activity

(Dependent Variable: Residuals from single country equations, Table 1, column 1)

(a) Explanatory Variable: Residuals from single country equations, Table 1, column 1

Country Residual	Coefficients of Explanatory Variable				F-statistic: all
	Australia	Canada	UK	US	coefficients equal zero
Australia	-	0.418	0.078	-0.048	1.96
Australia	-	-	0.118	0.014	0.63
Canada	0.467	-	0.041	0.143	2.02
UK	0.389	0.185	-	0.485	0.89
US	0.057	0.152	0.115	-	0.59

(b) Explanatory Variable: Residuals from single country equations, Table 1, column 1, lagged by 1 year

Country Residual	Coefficients of Explanatory Variable				F-statistic: all
	Australia	Canada	UK	US	coefficients equal zero
Australia	-	-0.107	-0.037	0.253	0.42
Australia	-	-	-0.047	0.53	
Canada	0.085	-	0.101	-0.086	0.32
UK	0.489	0.035	-	1.569	9.31
US	-0.543	0.483	0.014	-	2.33

(c) Explanatory Variable: Residuals from regressing log of working days lost per employees on a constant and a trend.

Country Residual	Coefficients of Explanatory Variable				F-statistic: all
	Australia	Canada	UK	US	coefficients equal zero
Australia	-	0.317	0.151	0.267	2.26
Australia	-	-	0.110	0.270	1.62
Canada	0.056	-	0.055	0.203	0.41
UK	-0.113	-0.549	-	0.267	0.84
US	-0.015	-0.263	-0.022	-	0.62

Notes: (1) The 95 percent critical values for F317 and F218 are 3.20 and 3.55 respectively. Interpretation of results is not altered if asymptotically correct chi-square test statistics are used.

(2) Coefficients have an elasticity interpretation, so a 1% rise in unexplained strike activity in one country produces a corresponding percentage change in strike activity (in the dependent country) equal to the coefficient.

(3) The first row for Australia includes Canada, UK and US; the second row excludes Canada.

TABLE 5: Thirteen Country Model of Strike Activity
(Dependent Variable is the Logarithm of Working Days Lost Per Employee)

Variable		Coefficients (absolute t-statistics) (1)	
Log Inflation Rate		0.544	(8.25)
Log Unemployment Rate		-0.499	(3.64)
1983-85 Dummy:	Australia	-0.667	(2.51)
	Canada	-0.246	(0.89)
	Finland	-0.403	(0.44)
	France	-0.352	(0.80)
	Germany	0.062	(0.04)
	Ireland	0.580	(1.23)
	Italy	-0.410	(0.99)
	Japan*	-0.367	(0.95)
	Norway	-0.861	(0.93)
	Spain	-0.912	(1.74)
	Sweden*	-0.793	(0.52)
	UK	0.515	(0.93)
	US	0.327	(1.24)
Linear Trend:	Australia	0.062	(2.95)
	Canada	0.002	(0.14)
	Finland	0.102	(2.01)
	France	0.003	(0.12)
	Germany	0.108	(1.25)
	Ireland	-0.033	(1.25)
	Italy	-0.042	(1.75)
	Japan	-0.091	(4.47)
	Norway	0.118	(2.37)
	Spain	0.263	(8.17)
	Sweden	0.047	(0.61)
	UK	0.054	(1.72)
	US	-0.104	(6.39)
Intercept:	Australia	-1.899	(11.04)
	Canada	-0.396	(1.60)
	Finland	-3.168	(5.46)
	France	-2.377	(8.55)
	Germany	-6.611	(6.83)
	Ireland	-0.440	(1.20)
	Italy	0.406	(1.20)
	Japan	-2.454	(9.73)
	Norway	-6.090	(10.48)
	Spain	-4.898	(14.16)
	Sweden	-5.457	(6.16)
	UK	-2.180	(6.08)
	US	-0.036	(0.15)

R^2 Australia 0.69; Canada 0.55; Finland 0.18; France 0.28; Germany 0.30; Ireland 0.10; Italy 0.26; Japan 0.86; Norway 0.33; Spain 0.83; Sweden 0.21; UK 0.29; US 0.80.

* Data for Japan and Sweden do not include 1985.

The final approach adopted was to apply models 2 and 3 of Table 1 to a sample of 13 OECD countries for which data are available. As noted in section II, there are reasons to be wary of applying simple functional forms to such a diverse range of international experience, an issue that motivated the focus on the four fairly similar countries. Even so, important additional information is contained in the estimations of the larger sample. Results are shown in Table 5.

The results of the 13 country estimations add further insight into the question of the determinants of 1983-85 Australian strike activity. In no case did any country other than Australia experience decreases in industrial disputation that were statistically significantly different to zero in this period, although Spain, Sweden and Norway had relatively large coefficients. Again the story is the same: Australian diminution of strike activity in the three years beginning 1983 was, if not unique, most unusual in the international context.

The empirical results of the paper offer consistent and statistically compelling evidence on the major issue. They suggest that the fall in Australian strike activity after the beginning of 1983 was large, and substantially greater than was the case for Canada, the UK and the US, and possibly for all countries for which data are available. Apparently changes in Australian industrial relations have not been simply a manifestation of international events and forces. The reasons for believing this to be the case are documented below.

The first is that the estimates of the decrease in Australian strike activity for the 1983-85 period are reasonably insensitive to the functional form used. When no cross-equation restrictions are imposed, as in column (1) of Table 1, the coefficient implies a 62 per cent fall in industrial disputation after the beginning of the Accord. The more restrictive models of Table 1 delivered parameter values of about 67 and 64 per cent. These estimates are remarkably similar to those found in a more complex investigation of the issue using Australian quarterly data (Beggs and Chapman, 1987a).

Secondly, the 1983-85 period was not characterised by a statistically significant diminution in industrial disputation for any model in Canada, the UK or the US. Formal tests of the difference between Australia on the one hand and Canada, the UK and the US on the other supported this conclusion.

Thirdly, it is important to note the robustness of the results across different specifications for the four countries. This is the case for inflation, unemployment and the trend parameters, with few exceptions. Consistent with these findings is the statistical acceptance of the restrictions imposed.

Fourthly, perhaps even more important than the general robustness of results noted from Table 1, is the evidence of dynamic and functional stability of the models, presented in Table 2. These data indicate that the estimations are characterised by a lack of both serial dependence and specification bias. The models are statistically healthy.

Fifthly, there is only a low correlation between the estimated disturbances of the regression equations across the countries and poor fits for regression estimates using different countries' residuals as estimators of particular countries' residuals. This implies difficulty for the argument that there was a world-wide decrease in industrial disputation since such a phenomenon would be manifested in high positive correlations of the disturbances and better fits for the regression models. That is, from these methods, there is no evidence of unmeasured factors influencing results across countries.

Finally, the conclusions are not contradicted by a broader investigation of 13 OECD countries for which data are available. No other country experienced a statistically significant decrease in strike activity in 1983-85. Importantly, other parameter estimates are very similar to those found for the four country investigations. In short, Australian experience in the period was extraordinary.

IV. Conclusion

Through a range of methods it has been demonstrated that the case for the diminution of Australian industrial disputation in the 1983-85 period being part of an international phenomenon is not supported by the data. The models used are statistically healthy, passing a variety of diagnostic tests and exhibiting a consistency of parameter values across functional form and data sets. From the methods employed it is apparently the case that Australian strike outcomes in this period were not experienced generally.

The above is not to say, however, that the prices and incomes Accord was solely responsible for increased industrial harmony and the delivery of relatively low strike activity. Data limitations necessitate caution in attributing causality to the outcomes experience. Even so, it is implausible to argue that industrial relations policy changes after the beginning of 1983 were irrelevant to changes in Australian disputation.

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[1]See, for example, the comments of Mr MacBean (Senior Vice-President, ACTU) as reported in the *Sydney Morning Herald*, 27 July 1985.

[2]John Howard said on *The 7.30 Report* (27 May 1987): "As for that strike record, what's happened in Australia over the past four years has happened all over the world".

[3]For example, see David Clark, "Pooh-Bear Commentaries on Accord Hide Some Brothers Grimm Realities", *Australian Financial Review*, 29 September, 1986.

[4]Creigh and Makeham (1982) offer an alternative conjecture, which is that strike activity is greater when unemployment is higher because, even though there are fewer disputes, they tend to be longer. Other than for their own sample, there is no empirical support for this proposition.

[5]Industrial disputation in the four countries and Japan, West Germany, Italy, Spain, Norway, Finland, Sweden, France, Ireland and New Zealand have been considered in earlier research (Beggs and Chapman, 1987).

[6]This is obvious from consideration of Figure 2 in Beggs and Chapman (1987b).

[7]For the former, the criterion is 10 or more working days lost (or of more than half day's duration for Canada), for the UK, more than ten workers involved and of more than one day's duration unless 100 or more working days are lost, and for the US, more than one day's or shift's duration and more than 1000 workers involved (Employment Gazette, July 1986). Discussion of the implications of these criteria is in Beggs and Chapman, 1987b.

[8]For example, average strike duration changes systematically with the level of inflation and unemployment. This implies that with changes in these variables countries such as the US, that under-report (relatively) working days lost due to short strikes, have increased (or decreased) measured industrial disputation compared to the true situation, and that this measurement error is related to the level of inflation or unemployment.This suggests biased parameter estimates, a possibility that will be manifested in a (non-linear) relationship between the residuals of the regressions and inflation or unemployment. We investigated these possibilities and found no evidence of bias. This implies that if the problem exists its empirical significance is slight.

[9]Only the UK adjusted rates differ from the rates used, and not by a significant amount.

[10]We also tested for the possibilities that in the UK and the US the changes in government and the Presidency, respectively, influenced strike activity. We found no evidence of Thatcher or Reagan effects, or the so-called "air-traffic" controllers effect.

[11]Separate tests restricting the unemployment coefficients, and then the inflation coefficient to be equal across countries were carried out. The likelihood ratio test statistics were 5.96 and 5.39. The test statistic has a $\chi^2(3)$ distribution under the unconstrained with hypothesis, with a 95 percent critical value of 7.81. We do not reject the restrictions.

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Appendix 1
Statistical Characteristics of the Data

Variable	Means and Standard Deviations			
	Australia	Canada	UK	US
Annual days lost per employee	0.47(0.27)	0.75(0.32)	0.43(0.36)	0.38(0.23)
Annual Inflation Rate	7.63(4.18)	6.44(3.28)	9.32(5.75)	5.93(3.33)
Annual Unemployment Rate	4.30(2.70)	6.79(2.53)	5.78(3.73)	6.02(1.83)

Paper 8

IMMIGRANT WAGE ADJUSTMENT IN AUSTRALIA: CROSS-SECTION AND TIME-SERIES ESTIMATES

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I. INTRODUCTION

The question of how long it takes for immigrants to adjust to the Australian labour market is of considerable interest to policy-makers. It is also an issue of importance for both economic theory and applied economics, since implicit in it is the potential for examining the processes of and constraints to human capital transference and investment. These latter points are pertinent to understanding the operation of the labour market generally, given the prevalence of the human capital explanation of wage structures.

In the immigrant context the usual story is that newcomers are initially disadvantaged but as a consequence of investments in country-specific skills their wages "catch-up" with those of natives. Typically, cross-sectional estimation is used to study these adjustment processes. A basic theme exploited in this paper is that there are important dangers inherent in such an approach, a point first developed by Borjas (1985). The major problem is that cross-sectional data may provide false conclusions concerning the underlying wage change mechanisms, because of the potential to confuse variations in the (unmeasured) ability of immigrant cohorts entering in different years with returns to length of residency. Section II develops this issue with reference to the existing literature.

A third section examines the matter empirically using both cross-sectional and time series approaches. The latter method, which involves a comparison of wage estimates for like persons in 1973 and 1981, avoids the unobserved ability problem and is consequently a more valid way of measuring relative wage changes. The technique thus allows insight into both the true extent of immigrant wage catch-up, and the statistical significance of the problems associated with the use of cross-sectional data in this area.

The results are of considerable interest since, at least in part, they throw some doubt on orthodoxy. From our data there is no evidence of catch-up for immigrants from non-English speaking countries (NESM), and only a very slow wage catch-up for immigrants from English speaking countries (ESM). In 1973 the cross-sectional estimates adequately represent the experience of ESM, but they give a misleading impression for the wage adjustment processes of NESM. For the latter group, the data imply that the average unobserved ability of entering cohorts increased from 1965 to 1973.

II. CONCEPTUAL ISSUES

The notion of immigrant wage catch-up and the problems associated with cross-section estimations are most easily explained through consideration of the following, simplified but conventional, wage model (Mincer, 1964):

$$\ell n\, W_i^N = \alpha_0 + \alpha_1 \, YOS_i + \alpha_2 \, GEXP_i + \varepsilon_i^N \qquad (1)$$

and

$$\ell n\, W_i^I = \beta_0 + \beta_1\, YOS_i + \beta_2\, GEXP_i + \beta_3\, PER_i + \varepsilon_i^I \qquad (2)$$

where, for individual i, ℓn W is the log of hourly wages, YOS is years of schooling, GEXP is years in the labour force, PER is, for immigrants, years in Australia, and N and I are native and immigrant superscripts respectively. Immigrant wages are said to "catch-up" to native wages if immigrant wages are relatively low initially and $(\beta_2 + \beta_3) > \alpha_2$. The underlying theoretical perspective implicit in this prediction is that immigrants are initially disadvantaged in terms of country-specific skills (e.g. language, culture and understanding local processes of job search) and it is thus in their interests to invest in the acquisition of such skills. This implies that, with all else equal, the average immigrant wage is initially lower than the average native wage, and the average immigrant wage increases more rapidly than the average native wage as the Australian labour market experience of both increases.

It is important to set out these conditions explicitly, for two reasons. The first concerns Chiswick and Miller's (1985) analysis of the 1981 Census. On the basis of the findings of cross-section earnings (income) functions they argue that "at the end of the first year of residence the overseas born's income is about 10.5 per cent less than that of the native born, and the gap narrows by 0.2 percentage points per year" (p.545) which seems to imply (very slow) catch-up in the sense explained above. However, their conclusion is based on the positive PER (β_3) coefficient, and pays little explicit attention to the relative sizes of α_2 and β_2. Since in their data, $\alpha_2 > (\beta_2 + \beta_3)$[1], immigrant wages increase *less* quickly than native wages as Australian labour market experience increases. Indeed, some of their estimations imply cross-over of the immigrant and native wage streams, but with the former wage starting higher but finishing lower than that of like natives. We seek to clarify the point.

The second reason for setting out conditions explicitly, and the main motivation of this paper, is that if (unobserved) immigrant average ability differs between cohorts depending on their year of arrival, the parameter β_3, estimated on cross-section data, is biased. It will reflect both ability differences between cohorts and returns to years of residency. Moreover, the bias cannot be signed *a priori*. The coefficient will tend to be lower (higher) if recent immigrants are more (less) able than immigrants with greater Australian length of residency. A solution is presented below.

III. THE EMPIRICAL ANALYSIS

An approach to the problem explained above suggested by Borjas (1985) (and implemented with US Census data) is to examine two cross-sections including average representations of the same individuals. Survey data from 1973 and the Australian Census of 1981 allow a replication of his method, which is now examined.

Consider the following four (simplified) estimating equations:

$$\ell n\, W^N_{i,1973} = \theta^N_{1973}\, X^N_{i,1973} \tag{3}$$

$$\ell n\, W^I_{i,1973} = \theta^I_{1973}\, X^I_{i,1973} \tag{4}$$

$$\ell n\, W^N_{i,1981} = \theta^N_{1981}\, X^N_{i,1981} \tag{5}$$

$$\ell n\, W^I_{i,1981} = \theta^I_{1981}\, X^I_{i,1981} \tag{6}$$

where θ are parameters and X is a vector of productivity characteristics for the relevant periods.

The predicted average hourly wages of individuals entering the Australian labour market is given by $\overline{W}^N_{i,1973}$ and $\overline{W}^I_{i,1973}$ for natives and immigrants respectively in 1973, and by $\overline{W}^N_{i,1981}$ and $\overline{W}^I_{i,1981}$ for natives and immigrants respectively in 1981. Thus the extent of wage increase over the eight year period for like individuals on average is given by $(\overline{W}^N_{i,1981} - \overline{W}^N_{i,1973})$ and $(\overline{W}^I_{i,1981} - \overline{W}^I_{i,1973})$ for natives and immigrants respectively. If immigrant wages are initially lower than native wages, catch-up occurs if $(\overline{W}^N_{i,1981} - \overline{W}^N_{i,1973}) < (\overline{W}^I_{i,1981} - \overline{W}^I_{i,1973})$

This method has a distinct advantage over cross-sectional analysis because we are able to observe directly estimated wage changes for like individuals. It also provides insight into the question of (unobserved) ability differences in the immigrant pool by year of arrival. By way of explanation, if the wage catch-up predicted from the 1973 cross-section for a length of residency increasing by 8 years exceeds that directly observed in comparing the same cohort after 8 years, the implication is that immigrants who entered in 1965 (1973 minus 8 years) had greater (unobserved) ability than immigrants who entered in 1973. The converse is true if the 1973 cross-section prediction is less than directly observed.

The data are drawn from two cross-sections, the (ANU) 1973 Social Sciences Mobility survey of Australian residents aged 30-64, which includes a sample of about 2000 wage or salary-earning males, and the 1981 1/100 Census tapes. Neither data set is ideal, problems being the availability of income instead of wage information and, for the 1981 Census, very broad hours categories. The former problem is of little concern because of the comparison method employed. For the latter problem we have erred on the side of simplicity and used the midpoint of the hours category. This may not be of major concern since the great majority of individuals fell into the highest hours category. An important point encouraging the use of the data sets for this exercise is the general similarity of questions and survey methodology.

Australian-born:

$$\ell n\, W_i^N = \alpha_0 + \alpha_1\, YOS_i + \alpha_2\, GEXP_i + \alpha_3\, GEXP_i^2 + \alpha_4\, MAR_i + \varepsilon_\alpha^N \qquad (7)$$

and, for immigrants:

$$\ell n\, W_i^I = \beta_0 + \beta_1\, YOS_i + \beta_2\, AYOS + \beta_3\, GEXP_i + \beta_4\, GEXP_i^2 + \beta_5 PER_i$$

$$+ \beta_6\, PER_i^2 + \beta_7\, MAR_i + \varepsilon_\beta^I \qquad (8)$$

where $\ell n\, W_i$ is hourly income, AYOS is years of Australian schooling,[2] and MAR is a dummy variable = 1 if the person is currently married with spouse present, = 0 if not. Table 1 presents the statistical characteristics of the data and Table 2 the OLS estimations.

TABLE 1
Statistical Characteristics of the Data

Variable	Australian Born		English-Speaking Country Born		Non-English Speaking Country Born	
	1973	1981	1973	1981	1973	1981
YOS (years)	10.16	11.05	10.48	10.99	10.03	10.80
	(2.29)	(2.31)	(2.33)	(2.31)	(3.41)	(3.04)
GEXP (years)	30.05	24.29	29.31	25.93	29.16	25.57
	(10.49)	(11.56)	(10.55)	(11.25)	(9.97)	(11.23)
MAR	0.90	0.803	0.91	0.83	0.93	0.83
	(0.30)	(0.39)	(0.29)	(0.37)	(0.25)	(0.37)
PER (years)			16.30	16.38	16.07	16.57
			(13.04)	(9.53)	(8.13)	(9.35)
AYOS (years)			1.05	2.13	0.54	1.97
			(3.15)	(4.52)	(2.36)	(4.44)
INCOME*	2.95	7.94	2.85	8.00	2.41	7.86
	(1.55)	(5.10)	(1.30)	(5.78)	(1.16)	(7.11)
Number of Observations	1234	8380	248	1284	1284	2330

\+ Means, standard deviations in parentheses.
* Weekly nominal dollars.

Not surprisingly, the cross-sectional results, noted as follows, are similar in many respects to those reported in other research (Chiswick and Miller (1985); Stromback (1984); BLMR (1986)). First, for both samples, ESM (principally from the UK and Eirie) receive returns to pre-migration experience and schooling of similar magnitude to Australian returns to experience and schooling. This implies that skills are easily transferred between like-countries. Secondly, NESM (principally from Italy and Greece) receive lower returns to pre-migration experience and schooling than do the Australian-born receive from experience and schooling, a finding consistent with the hypothesis that skills acquired overseas are only imperfectly transferable to dissimilar countries. Thirdly, ESM receive lower returns to Australian schooling than do NESM. Again, the implication is that immigrants from countries more dissimilar to Australia benefit more from country-specific education investments than do immigrants from countries more similar to Australia.

TABLE 2
OLS WAGE ESTIMATIONS*
(dependent variable is log of hourly income)

Variable	Australian Born		NESM		ESM	
	1973	1981	1973	1981	1973	1981
GEXP	.02571	0.02077	0.00555	0.01070	0.03371	0.00868
	(0.00633)	(0.00235)	(0.01122)	(0.00549)	(0.01438)	(0.00665)
$GEXP^2$	-0.00035	-0.000320	-0.00016	-0.00021	-0.00050	-0.00019
	(0.00010)	(0.00004)	(0.00017)	(0.00009)	(0.00022)	(0.00014)
YOS	0.10466	0.09000	0.02479	0.04933	0.0887	0.08382
	(0.00503)	(0.00260)	(0.00633)	(0.00414)	(0.01162)	(0.00658)
AYOS			0.02425	0.00791	0.00884	-0.00929
			(0.01029)	(0.00288)	(0.01123)	(0.00332)
PER			0.00535	-0.00286	0.00115	0.00916
			(0.00610)	(0.00449)	(0.00538)	(0.00502)
PER^2			0.00012	0.00011	-0.00001	-0.00010
			(0.00016)	(0.00011)	(0.00010)	(0.00011)
MAR	0.07272	0.11395	0.11361	0.08330	0.27361	0.06429
	(0.03465)	(0.04628)	(0.07501)	(0.03026)	(0.08239)	(0.03372)
Intercept	-0.56502	0.57208	0.19383	1.1554	-0.72913	0.81421
	(0.12057)	(0.04628)	(0.20217)	(0.08319)	(0.28876)	(0.11516)
R^2	0.276	0.138	0.247	0.089	0.236	0.152

* Standard errors in parentheses.

As far as returns to residency are concerned, the PER coefficients are generally both small in size, have large standard errors and, at least for the 1981 sample, are less significant for NESM than they are for ESM. These results do not square easily with a perspective implying greater initial investments and higher eventual returns to immigrants from countries relatively unlike Australia. As has been stressed, however, cross-sectional estimates impose potentially important restrictions which need to be relaxed as far as is possible before any firm conclusions are established.

The question of ability differences in cohorts as an explanation of relative wage change may now be examined for a particular cohort, those entering Australia in 1965. A comparison of 1973 and 1981 results serves to illuminate the concept of catch-up from both cross-section and time-series approaches. Tables 3 and 4 present the wage increments attributable to additional Australian labour market experience for the following male (YOS = 10, AYOS = 0, MAR = 1, GEXP = 10 initially and PER = 0 in 1965).

TABLE 3

Predicted Wages Using 1973 Regression Estimates ($/hour)*

Australian Residency (years)	Australian Born		ESM		NESM	
	Wage	Percentage Increase	Wage	Percentage Increase	Wage	Percentage Increase
8	2.47		2.41		2.28	
	(0.05)		(0.11)		(0.08)	
16	2.68	8.50	2.63	9.12	2.28	0.01
	(0.04)	(1.71)	(0.10)	(3.94)	(0.06)	(3.23)
24	2.78	3.73	2.74	4.18	2.21	-3.18
	(0.04)	(0.89)	(0.12)	(2.32)	(0.07)	(2.22)

* Standard errors in parentheses.

The cross-sectional results of Table 3 suggest the following. ESM wages start slightly below native wages, but after this they increase slowly with catch-up occurring after 24 years of Australian labour market experience (for the individual considered this is about age 49). The two points of interest are, that the 1973 profiles support weakly a simple human capital model of initial country-specific investments for ESM, and that the wage structures of the two groups are very similar. Neither point is true for NESM.

NESM have a slightly lower earlier wage than natives, but because the profile is flat, indeed, declining, there is no catch-up, at least as reflected in the cross-section. For the individual considered the comparison of native to NESM wage profiles does not support the human capital model. Interestingly, varying the characteristics of the individual considered changes the interpretation somewhat: assuming that GEXP = 0 when PER = 0 results in NESM and native profile cross-over, *but from above*, a result confirmed for the 1981 Census by Chiswick and Miller (1985) and Stromback (1984).

Table 4 presents calculations of immigrant wage convergence or divergence to native wages over the course of 8 years (1973 to 1981) for the standardised individual entering in 1965. That is, 1981 predicted wages are compared with 1973 predicted wages for individuals who have aged 8 years holding constant education and marital status.

TABLE 4

Immigrant Wages Relative to Native Wages for the 1965 Entering Cohort
(per cent)*

	ESM	NESM
Predicted Percentage Difference from Native Wage in 1973	-2.43 (4.87)	-7.69 (3.73)
Predicted Percentage Difference from Native Wage After 16 Years Residency from the 1973 Cross-Section	-1.86 (4.00)	-14.92 (2.57)
Predicted Percentage Difference from Native Wage After 16 Years Residency from the 1981 Cross-Section	1.92 (0.95)	-5.47 (0.88)
Predicted Wage Catch-Up Cross-Section+	0.57	-7.23
Predicted Wage Catch-Up Time Series+	4.35	2.22
Difference Between Cross-Sectional and Time Series Predicted Wage Catch-Up	3.78 (4.23)	9.45 (2.72)

* Standard errors in parentheses.
\+ Standard errors for these numbers are complex to compute. The relevant difference between the time series and cross-section is accorded a standard error in the line below.

The data of Table 4 provide additional evidence on the relative wage structures of the different groups. They reveal that for NESM there is a significant difference in the story using actual wage change 1973-81 compared to using the 1973 predictions. There is increased divergence using the 1973 cross-section (of about 7.23 percentage points), but very slow catch-up revealed by the time of the 1981 cross-section (of 2.22 percentage points). The combined difference between the between cohort and within cohort estimates is then 9.45 percentage points with a standard error of 2.72. This difference is statistically significant at conventional levels. This suggests that the 1973 cross-section provides an inaccurate picture of NESM and native relative wage structures for the 1965 entering cohort. One interpretation of the results is that the unobserved ability of entering NESM cohorts changed from 1965 to 1973; the data being consistent with the view that NESM immigrating before 1965 were of lower (labour market) quality than those entering around 1965 and after.

The above is not the case for ESM. The 1973 cross-section predicts slow convergence between ESM and native wage structures, with the 1981 sample showing slightly faster ESM wage growth and take-over after around 12 years of residency. However, the differences are not statistically significant. Tentatively, this could suggest that ESM who arrived before 1965 were of about the same ability as those entering around 1965. Thus the 1973 cross-section seems to provide a reasonably accurate description of the adjustment process for this group. Catch-up exists, but it is very slow, the empirical magnitudes confirming the view that ESM and natives have very similar wage profiles.

Cautionary notes on the above are in order. There are several possible explanations of the differences between or similarities of the static and dynamic results.[3] One of the most important of these is that the Australian labour market changed substantially between 1973 and 1981. Most commentators acknowledge that the first period was one of excess labour demand and that the second was one of excess labour supply, it being unlikely that the movement into recession had neutral effects on the relative wage changes of the groups. A possibility is that immigrants, NESM in particular, were more likely than natives to be unemployed in 1981 relative to 1973. This could have two, opposite, effects. One is that the higher unemployment experience of immigrants in 1981 resulted in decreased relative wages for the group at this time, which would tend to understate catch-up from the time series approach. On the other hand, the average quality of immigrants observed as full-time workers in 1981 would have been relatively high at this time compared to 1973, if it is low quality workers who are more likely to be unemployment in recessions. If this is the case, the time series approach tends to overstate that part of wage change attributable purely to catch-up. It is not obvious which effect dominates, but it is relevant to note that the implications may not be substantial given that immigrant unemployment rates—at least as estimated from the 1981 Census (Beggs and Chapman, 1987)—were not markedly different to those of natives.

Another interpretation of the results is that the wage changes for immigrants between 1973 and 1981 reflect, in part, differences in the treatment accorded non-natives by employers. That is, the relative wage increases of NESM in this period may be a manifestation of diminished statistical discrimination given greater information on the work characteristics of the group. We have no data as to the likely extent of this phenomenon, but note the possibility as a caveat to the more straight-forward interpretation.

IV. CONCLUSIONS

Until now the data employed to study immigrant relative wage performance were cross-sectional and, as such, the usual tests imposed potentially important restrictions. As a contribution we have addressed empirically the possibility that differences in the average ability levels of entering immigrant cohorts contaminate cross-sectional analysis. In so doing insights are forthcoming into the process of immigrant wage catch-up, a phenomenon alleged to be the result of country-specific human capital investments.

The wage changes of cohorts of like individuals were examined over an 8 year period, employing the 1973 ANU Social Mobility Survey and the 1981 Census. Three points emerged. The first is that wage structures of ESM were quite similar to natives, no matter what technique is employed. This implies that unobserved ability differences within this group have not dominated relative wage structures.

Secondly, for NESM the 1973 cross-section analysis revealed low starting wages and a rapid wage divergence, compared to natives. This result changed using the more valid time series technique, in that NESM wages apparently caught-up to native wages, but at an extremely slow rate. Importantly, the rate of convergence is so sluggish as to suggest that catch-up does not characterise the NESM wage adjustment process in Australia, which implies that the country-specific human capital investment model may not be useful as an explanation of NESM wage experience.

Finally, with some caveats, the results from the different techniques can be interpreted to mean that there have been changes over time in the unmeasured ability of Australian immigrants from non-English speaking countries. In particular, the data are consistent with the view that NESM males entering the country after 1965 were more talented (in a labour market sense) than was the case for earlier cohorts.

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[1] This is a simplification of their analysis since it excludes the influence of quadratic terms. Conclusions are the same with the more complex form.

[2] AYOS was computed using the technique adopted by Chiswick and Miller (1985). That is it is given by years of education minus age at migration plus 5 (and equal to zero if negative).

[3] These include the distortions introduced from re-migration and changes in government policy towards new immigrants. For a full account of the possibilities, and some statistical tests, see Beggs and Chapman (1986) and Paper 10 of this volume.

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Paper 9

SEARCH EFFICIENCY, SKILL TRANSFERABILITY AND IMMIGRANT RELATIVE UNEMPLOYMENT RATES IN AUSTRALIA

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I. INTRODUCTION

Search theory is the conventional framework used for analysing the factors influencing unemployment. But the empirical implementation of the theory poses major challenges since many of the variables of conceptual interest are not observed in the data. The goals of this paper are to test a prediction of search theory which *is* possible given the available data on Australian native and immigrant unemployment experience, and to examine the operational weaknesses inherent in the theory when proxy variables must be used to capture unobserved parameters of the model.

Two major points follow from the analysis, neither of which has found its way to the fore-front of policy or method. First, participants in the debate on the economic assimilation of immigrants (Borjas, 1985; Chiswick, 1978) miss the very important point that conclusions concerning the relative labour market success of immigrants differ considerably depending on the measurable skill levels of groups. Second, the usefulness of search theory in providing testable hypotheses is demonstrated to be so limited as to constitute a credible case against its general application.

Section II describes the theoretical model and explains its scope for formulating direct tests of particular hypotheses. In section III possible empirical estimation of unemployment determinants is examined, there being apparently (only) one unambiguous relationship that is predicted from the theory. This is that, *ceteris paribus*, low skill (schooled) immigrants are less likely to be unemployed than like natives, an hypothesis supported by the empirical results reported in section IV in which immigrant and native unemployment determinants are examined. We conclude, in section V, that while empirical constraints frequently inhibit useful applications of search theory, in this (unusual) instance a (policy relevant) insight related to immigrant unemployment has been forthcoming.

II. THE CONVENTIONAL MODEL OF UNEMPLOYED JOB SEARCH

Assume that an individual maximises his or her expected lifetime utility which is assumed to be the present discounted value of each period's utility. Let z denote income received per period and let leisure hours be fixed. The utility function for each period is written U(z) with U' > 0.

The unemployed individual receives non-wage income, y, which is net of search costs incurred at the start of each period. Job offers arrive at random at a constant average probability of γ per period a rate assumed to be independent of search costs. Each job offer carries a wage offer which has a known distribution function $G(w|\theta)$ and the person accepting the job offer faces the same wage in all future periods of employment.

Let $\psi(w)$ denote th lifetime utility of a person who accepts a job offer with a wage rate with $\psi' > 0$. If V(t) is the maximum expected lifetime utility from search of an individual who has been unemployed for t periods and who adopts an optimal job search policy in all periods after t, then

$$V(t-1) = U(y) + \rho\gamma[1 - G(w|^*\theta)]E[\psi(W)|W > w^*] + \rho\{1 - \gamma[1 - G(w^*|\theta)]\}V(t) \qquad (1)$$

where ρ is the discount factor, and w* is the reservation wage. The second term on the right hand side represents the expected return of receiving a wage above the reservation wage and accepting the offer. The second term captures the opportunity cost of not accepting employment. The opportunity cost of choosing to continue searching is a composite effect of non-wage income, the component of search cost in non-wage income and the wage offer distribution. The discount rate and job arrival rate matter in a way which is discussed below.

At the utility optimised interior solution the reservation wage is selected so that the return from accepting a job offer with wage rate w* equals the expected return from remaining unemployed and searching for at least one more period. Also, it is the case that $V(t) = \psi(w^*)$, $\psi' > 0$ and, with an infinite time horizon, V(t) = V(t-1) = ... = V, and the reservation wage is constant in each period. Rearranging:

$$V = \frac{U(y) + p\gamma[1 - G(w^*|\theta)]\ E[\psi(W)|W > w^*]}{1 - p\ \{1 - \gamma[1 - G(w^*|\theta)]\}} \qquad (2)$$

The comparative static effects of changes in job search costs, including foregone earnings, changes in the job arrival rate and changes in the discount rate may be examined through equation (2). Hui (1987) shows that the first derivatives of V(.) with respect to s, ρ and γ are negative, positive and positive (usually) respectively, and since $\psi' > 0$ these results translate into negative, positive and positive effects respectively, on the equilibrium reservation wage. A fall in the direct cost of search reduces the cost of continuing in job search so the unemployed searcher raises his or her reservation wage thus increasing the probability of remaining unemployed. A rise in the discount factor ρ signifies a shift in preference for present income over future income. Because the unemployed searcher becomes less willing to forego current wage income while waiting for a higher wage offer in the future, he or she lowers the reservation wage and thus the probability of remaining unemployed decreases. Finally, an increase in the job arrival rate improves the chances of sampling a high wage offer from a *given wage offer distribution.* The unemployed searcher is then more willing to wait for a higher wage (that is, the reservation wage is increased).

The theoretical appeal of the above analysis is the ability it provides to sign the effect on the unemployment probability of changes in job search costs, the discount factor and the job arrival rates. But this is possible only because the wage offer distribution is held fixed which necessarily means that both G(.) and the parameters θ remain constant in the model outlined. The practical difficulty is that changes in individual characteristics which are likely to affect search costs or the job arrival rate are very likely to be associated with changes in the job offer distribution. It follows that it is difficult to find empirical circumstances which correspond to the *ceteris paribus* experiment described in the comparative static analysis.

Furthermore, the theory is relatively unhelpful in advising us of the consequences of changes in the job offer distribution. For example, consider a change in the parameter θ which alters the dispersion of the wage offer distribution. Comparative static analysis involves consideration of the term $\frac{\partial G}{\partial \theta}$. Since G(.) must take values between zero and one, it follows that $\frac{\partial G}{\partial \theta}$ must be positive at some wage levels and negative at others.[1] In general, the optimum reservation wage along the wage offer distribution is not known so it is not possible to sign the effect on the reservation wage, or the unemployment probability, of comparative static changes in the parameters of the wage offer distribution.

A final observation on the model is pertinent. This is that while the framework at first blush appears complex, it probably misses some important aspects of the search process, one being individual differences in search intensity. Again, operational problems exist in that more intensive search could lead to an increase in the job arrival rate with the distribution of offers unchanged, or could increase the mean wage of job offers. Either way, fair interpretation of the results needs to acknowledge the potential influence of differences in search intensity between groups.

III. INDIVIDUAL CHARACTERISTICS AND NATIVE AND IMMIGRANT UNEMPLOYED JOB SEARCH

The empirical thrust of the analysis is to determine whether or not parameters of the job search model presented above can be related to observed characteristics of individuals, in the context of our major interest which is the effect of immigrant status upon the unemployment probability. But this issue cannot be resolved without considering the effects of schooling and of labour market experience, both in Australia and abroad. In Table 1 we advance a number of conjectures about the relationship between the parameters of the search model and individual characteristics. These illustrate the difficulty of making predictions, given available data.

Job search costs are argued to be lower for persons with more schooling and more experience in the labour market, and to be lower for natives than for immigrants. These primarily derive from the idea that efficiency of job search depends on the knowledge of institutions and of the work environment, knowledge which is acquired by learning in schools or in the Australian work place. Immigrants may be additionally disadvantaged due to poor language skills.

Dex (1982) considered the role of search costs in a job search model explaining black British youth unemployment. She concluded that blacks have higher job search costs, but contrary to the theory experience longer job search durations. She identified non-constancy of the wage offer distribution between blacks and whites as the likely source of violation of the *ceteris paribus* conditions. The remainder of this section examines the applicability of the assumptions for comparing natives' and immigrants' job search behaviour, the discussion of which is summarised in Table 1.

TABLE 1

EFFECT OF INDIVIDUAL CHARACTERISTICS ON THE PARAMETERS OF THE JOB SEARCH MODEL

ffect on:	Increase in Years of Schooling	Increase in Years of Labour Market Experience	Being an Immigrant Relative to Being Native Born
ob search costs, s	Decrease	Decrease	Higher
iscount factor, ρ	No conjecture	No conjecture	No conjecture
ob arrival rate, γ	No conjecture	No conjecture	Lower
lean wage job offer istribution	Increase	Increase	Negligible difference for low skill migrants Lower for high skill migrants
Vage variance of job ffer distribution	Increase	Increase	Negligible difference for low skill migrants Lower for high skill migrants
ligher movements of job ffer distribution	No conjecture	No conjecture	No conjecture

There is little *a priori* reason for supposing that the time preference discount rate for ndividuals varies systematically with any of the individual characteristics listed in Table 1. In a nore general model incorporating a capital market, scenarios arise where capital market 'imperfections" may bias the individual choice of a discount rate for the job search decision. To illustrate, if immigrants have limited access to capital markets, for example due to lifferences in bank assessment of default risk, then their ability to borrow now against future ncome is restricted. It follows that the constrained optimum solution is to choose a lower wage ob now, rather than the unconstrained optimum of waiting for the higher wage job in the uture. Similar arguments can be made for aged persons (high labour market experience) and nay be possible for very low skills and/or itinerate workers (low years of schooling), but since here is little compelling theory for this line of reasoning we do not advance any particular onjectures regarding the relative magnitudes of the discount factor in the model.

However, we do conjecture that the job arrival rate from any given wage offer listribution is lower for immigrants that it is for like natives. This is due to the possibilities of anguage difficulties and inadequate knowledge of local economic, legal and institutional rrangements, which make immigrants ineligible for some proportion of all jobs.[2]

The major difficulty in the application of theory to predict the consequences for unemployment of individual characteristics arises due to the effect these characteristics have on the job offer distribution. When changes in individual characteristics change the job offer distribution, the theory prediction of the unemployment consequences are ambiguous. However, an important conjecture in this paper is that among low skill workers the job offer distribution is similar for natives and immigrants, which derives from human capital theory and focusses attention on the issue of the transferability of (measured) skills between countries.

For concreteness of the above consider some examples. The human capital requirements of a low skill occupation such as a "cleaner" are likely to be highly comparable between countries. That is, the quality of the work is not significantly impeded by lack of understanding of local political, institutional or economic conditions, nor presumably is it much affected by the ability to speak the local language, since the communications skills required for such a job are small. By contrast the human capital of, say, an accountant, presumably contains a large country specific component, including knowledge of accounting conventions, tax law and company law. These possibilities are further compounded by the requirements in many high skill occupations that immigrants satisfy accreditation requirements of professional organisations which may be acting in such a way as to minimise competition from overseas.

From the above it must be clear that unambiguous predictions from theory must then be limited to those concerning the relative unemployment rates of low skill (educated) immigrants and like natives. The summary in Table 1 shows that we expect immigrants to have both higher job search costs and lower job arrival rates than like natives. Both these factors imply that for a given wage offer distribution *(among the low skill groups) immigrants will choose a lower reservation wage and hence have a lower probability of unemployment than like natives* .

The general effect of schooling, length of labour market experience, and immigrant status cannot be resolved from prior conjectures, it following that these remain essentially empirical questions where the resolution of the competing determinants of the unemployment probability is revealed in the estimated reduced form equations. This is so because in all these cases we anticipate that changes in individual characteristics affect the dispersion of the wage offer distribution.

IV. DATA AND ECONOMETRIC ESTIMATION

The data assembled to test our models are a stratified sample of 10,900 males between the ages of 25 and 64 drawn randomly from the data files of the 1981 Australian Population Census. The sample is composed of approximately equal number of persons in three groups, Australian born, immigrants from English speaking countries (ESM) and immigrants from non-English speaking countries (NESM). The average characteristics of the data are summarised in Table 2 for the variables available for the analysis.

TABLE 2 +

Statistical Characteristics of the Data

Variable	Australian Born	ESM	NESM
Years of Schooling (YOS)	11.65 (2.42)	12.12 (2.46)	11.15 (3.33)
Years in Labour Market (LMX)	25.54 (12.22)	25.87 (11.94)	27.33 (11.63)
Married Dummy (MAR) (=1 if married or de facto)	0.78	0.79	0.84
Years in Australian Labour Market (ALMX)		15.68 (10.28)	17.80 (9.53)
Years of Schooling in Australia (AYOS)		1.72 (4.06)	1.78 (4.17)
Language Dummy (LANGD) (= 1 if language difficulty)			.44
Aggregate Unemployment Rate (per cent)*	3.44	4.36	4.60
Number of Observations	3634	3668	3607

+ Means, with standard deviations in parentheses.
* Derived from individuals' response to the question concerning whether or not he/she is currently jobless and searching for employment.

Unemployment rate determinants were estimated using a probit model, where the explanators are years of labour market experience both prior to and since arriving in Australia, years of schooling similarly decomposed, a dummy variable for marital status and a dummy variable for the English language capability of immigrants from non-English speaking countries. Both labour market experience terms have been entered as quadratics in order to capture the likely decreased marginal potency of these variables on unemployment rates at higher levels of labour market experience.

TABLE 3*

Probit Estimate of Probability of Unemployment

Variable	Australian -Born	ESM	NESM
Intercept	0.359	0.397	-0.630
	(0.95)	(1.23)	(2.22)
LMX	-0.0213	-0.0533	0.00137
	(1.15)	(2.96)	(0.075)
LMX^2	0.000254	0.00104	0.0000770
	(0.76)	(3.30)	(0.25)
YOS	-0.129	-0.0773	-0.0335
	(5.81)	(4.20)	(2.72)
AYOS		-0.000104	-0.0134
		(0.00932)	(1.16)
ALMX		-0.0252	-0.0250
		(1.92)	(1.71)
$ALMX^2$		0.000299	0.000240
		(0.96)	(0.61)
MAR	-0.568	-0.559	-0.468
	(6.38)	(6.67)	(5.18)
LANGD			0.123
			(1.62)
McFadden R^2	0.075	0.076	0.043
Percentage of Right Predictions	92.1	90.8	92.4
Likelihood Ratio Tests	Ho: $\beta_{LMX}=\beta_{LMX^2}=0$ Reject (95% level)	Ho: $\beta_{LMX}=\beta_{LMX^2}=0$ Reject (99% level)	Ho: $\beta_{LMX}=\beta_{LMX^2}=0$ Accept (99% level)
		Ho: $\beta_{ALMX}=\beta_{ALMX^2}=0$ Reject (95% level)	Ho: $\beta_{ALMX}=\beta_{ALMX^2}=0$ Reject (95% level)

* Absolute t-statistics in parentheses.

TABLE 4

Percentage Point Impacts on Unemployment Rates (Evaluated at Mean Values of the Data)

Variable	Australian Born	ESM	NESM
Five Additional Years of Labour Force Experience	-0.20	+0.16	+0.30
Two Additional Years of Schooling	-1.16	-0.86	-0.64
Being Married	-4.57	-4.78	-6.12
Five Additional Years of Labour Force Experience Spent in Australia	*	-0.51	-0.71
Two Additional Years of Schooling Spent in Australia	*	-0.17	-0.26
Experiencing a Language Difficulty	*	*	+1.29

The coefficient estimates and their standard errors are reported in Table 3, the results ollowing the pattern of signs that were *a prior* anticipated. The strongest single effects are the chooling and marital status variables. The labour market experience variables exhibit low ndividual t-statistics which reflects the fact that LMX, LMX^2, ALMX and $ALMX^2$ are highly orrelated in the sample and it is difficult to distinguish their individual contribution to nemployment. The log-likelihood ratio statistics indicate that only the joint test of the null ypothesis $\beta_{LMX} = \beta_{LMX^2} = 0$ for NESM fails to be rejected by the data.

To assist interpretation of results, Table 4 reports the average impact on unemployment ates for representative changes in the values of the explanatory variables. The biggest impacts re those associated with being married (4.6 to 6.1 percentage points) and additional years of chooling (1.6 to 1.2 percentage points from 2 extra years). Since the highly non-linear form of the probit probability model hampers straight-forward comparison of immigrant and native coefficients, the following section represents diagrammatically the predicted unemployment ates obtained from the models. Interpretative comment is offered on their form.

V. INTERPRETATION

Figures 1 through 4 show predicted unemployment rates for different schooling, national origin and years in the labour force. Each of the figures corresponds to a different level of total schooling with AYOS set equal to zero, the schooling levels chosen being 8, 10, 12 and 14 years intended to capture workers in progressively higher skilled occupations. On each figure we plot the expected unemployment rate against years of labour market experience for five categories of workers, which are:

i) AUST - Australian born workers

ii) ESM - migrants from English speaking countries with no labour market experience before entering Australia

iii) NESM - migrants from non-English speaking countries with no labour market experience before entering Australia

iv) ESMIO - migrants from English speaking countries with 10 years labour market exprience before entering Australia

v) NESMIO - migrants from non-English speaking countries with 10 years labour market experience before entering Australia.

Figure 1

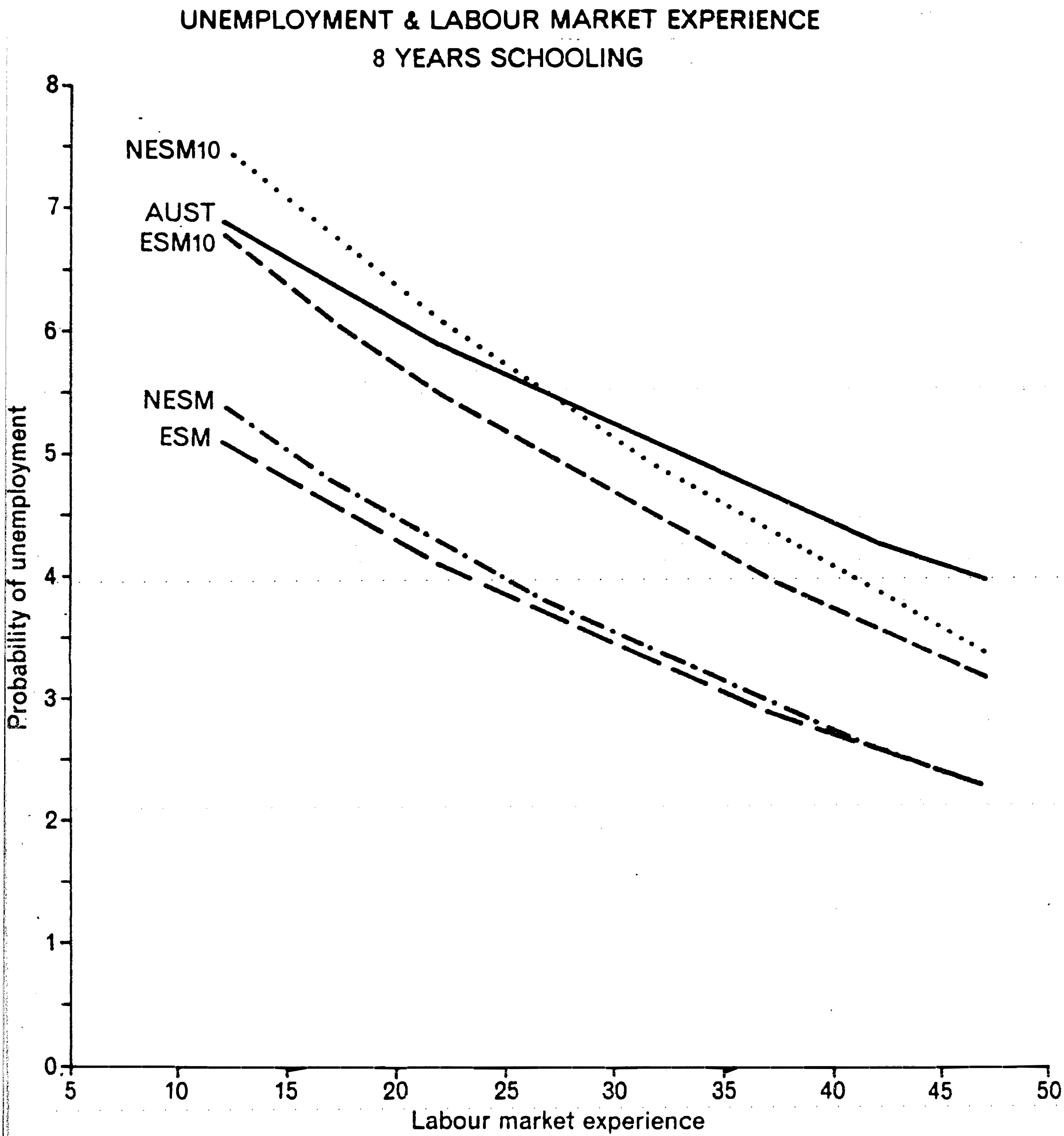

UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
8 YEARS SCHOOLING
NESM10
AUST
ESM10
NESM
ESM
Probability of unemployment
Labour market experience
0
1
2
3
4
5
6
7
8
5
10
15
20
25
30
35
40
45
50

Figure 2

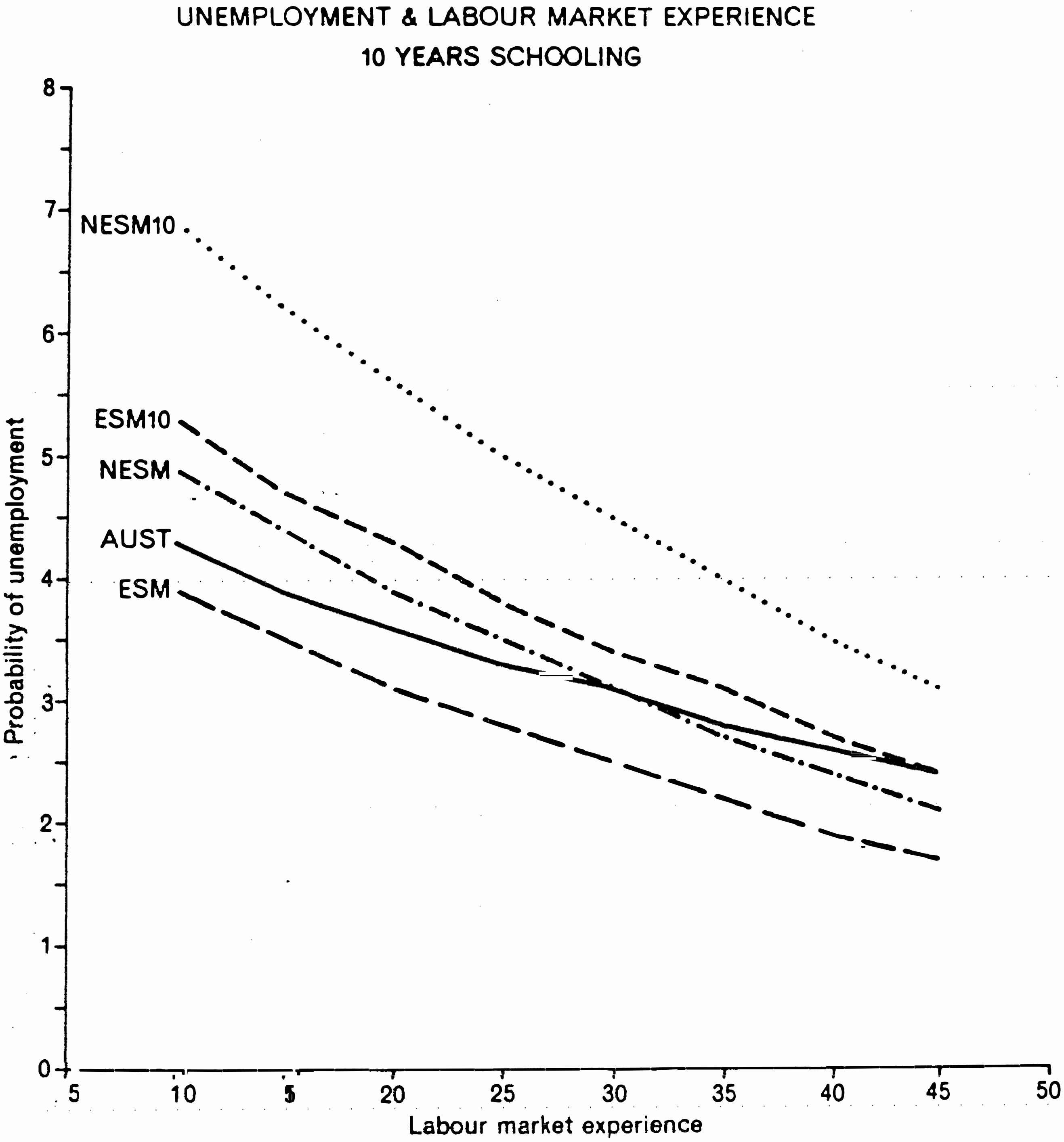

UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
10 YEARS SCHOOLING
NESM10
ESM10
NESM
AUST
ESM
Probability of unemployment
Labour market experience
0
1
2
3
4
5
6
7
8
5
10
15
20
25
30
35
40
45
50

Figure 3

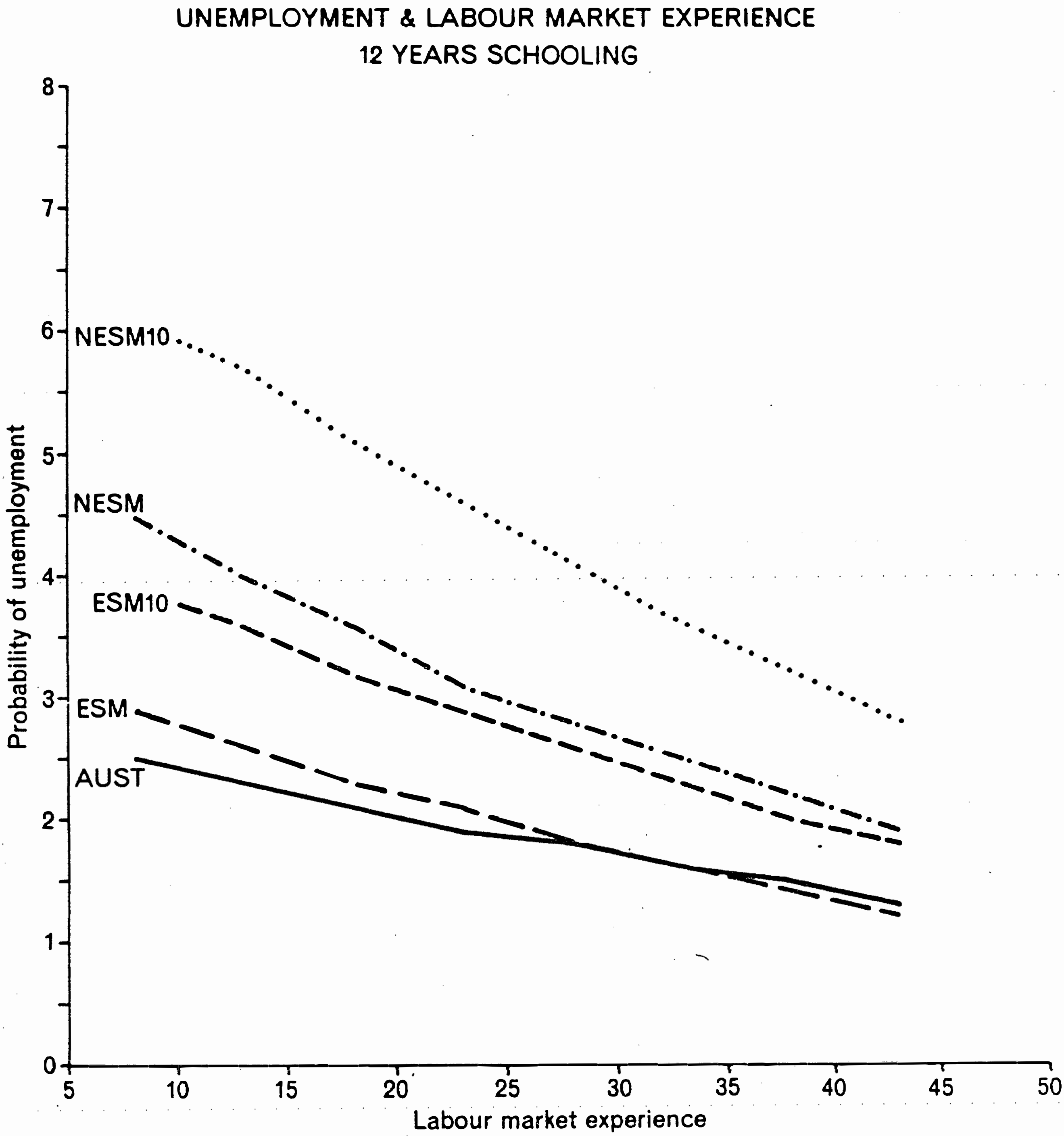
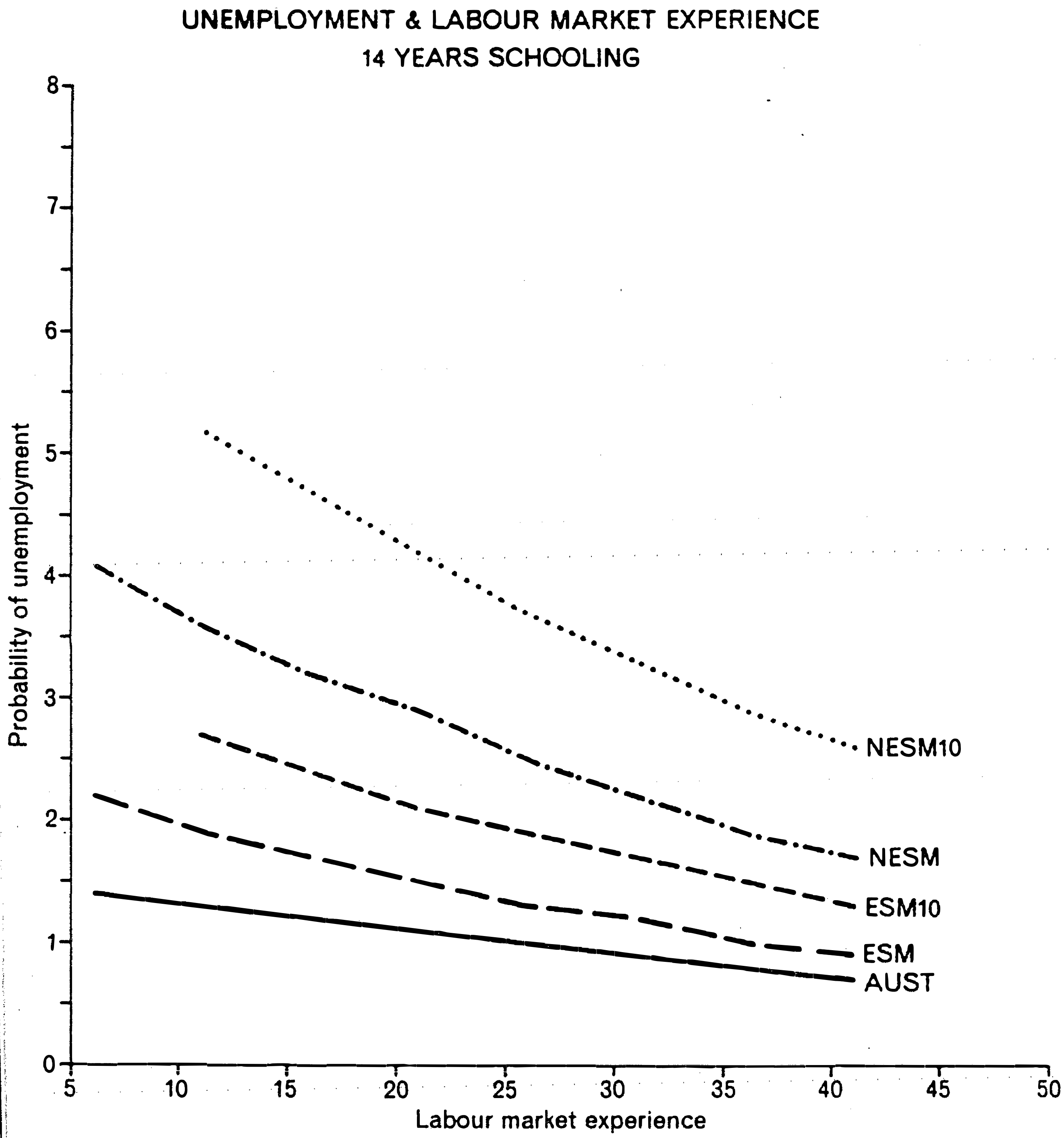

UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
12 YEARS SCHOOLING
Probability of unemployment
Labour market experience
NESM10
NESM
ESM10
ESM
AUST
0
1
2
3
4
5
6
7
8
5
10
15
20
25
30
35
40
45
50

Figure 4


UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
14 YEARS SCHOOLING
Probability of unemployment
Labour market experience
NESM10
NESM
ESM10
ESM
AUST
0
1
2
3
4
5
6
7
8
5
10
15
20
25
30
35
40
45
50

The figures reveal some important similarities. First, ESM have lower probabilities of unemployment than do NESM, for all levels of schooling and pre-immigration work experience. This implies that the greater search costs of the latter group outweigh the (presumed) higher reservation wages of ESM. Secondly, pre-immigration work experience increases the probability of unemployment for both groups implying higher reservation wages of immigrants with greater home country experience. Thirdly, as period of residence increases the gap between immigrant and native unemployment changes only very slowly. In general over the range of normal life-cycle labour market experience, there is no cross-over.

The major conjecture from the theoretical development was that the unemployment rate would be lower among low schooling immigrants than among like low schooling natives, a result confirmed in the probit analysis illustrated in Figure 1. Consistent with expectations is the finding that as education increases, so too does the unemployment rate of immigrants relative to similarly schooled natives.

In a search theory context the major empirical pattern revealed can be interpreted as evidence for the proposition that as immigrants' skill increases, there is a decrease in the relative average wage and wage variance from immigrants' job offer distribution compared to that of like natives. A caveat to this interpretation relates to the (unmodelled) role of search intensity in that if low skill immigrants are more search intensive than all other groups, this might explain their lower unemployment probabilities. However, while immigrants may search more intensively than natives as a group (Chiswick, 1988), there is no compelling *a priori* reason for believing that a significant difference in intensity exists between schooling levels within the immigrant group.

Additional Australian labour market experience tends to narrow the unemployment gap between natives and immigrants, which are likely to be due to immigrants slowly acquiring the work characteristics of natives. In the model estimated, the narrowing of the gap is more pronounced among high skill immigrants. This result is consistent with the view that more highly educated immigrants assimilate more quickly than those with less education, an issue better considered in the context of relative wage rates (see Beggs and Chapman, 1986).

Interestingly, progression through Figures 1 to 4 reveals a striking pattern which is that more schooling has a substantial effect in reducing the unemployment rate. Since additional schooling is conjectured to lower search cost (due to improved efficiency), the fall in the unemployment rate in this framework must be attributed to changes in the wage offer distribution which makes unemployment a less attractive alternative.

The tendency of unemployment rates to fall with time in the labour force can be due to decreases in the job offer rate, changes in the wage offer distribution, or to the finite time horizon aspect of the work career. As workers age the discounted present value of the benefits from a higher wage job fall, so it is optimal to lower the reservation wage to increase the probability of earning short-run wages, rather than to hold out for higher wages some time in the future. The consequence is a fall in the unemployment probability as workers approach retirement. It is unclear how important each of these factors is, it being likely that they all act together and have differing degrees of importance over the work career.

VI. CONCLUSION

Two major conclusions arise. One is that conventional search theory leads to very few unambiguous predictions, given the nature of available data. In general this is due to the likelihood of changes in the distribution of wage offers associated with variations in the job arrival rate. The second is that if the employment attributes of low skill workers are valued relatively similarly across countries, the model predicts that such immigrant workers will experience lower unemployment rates than like natives interpreted in the context of the model to be because of their relatively low reservation wages. This contention is supported by the data.

The other main empirical results are as follows. First, extra schooling decreases unemployment probabilities for all groups. Secondly, NESM experience higher unemployment rates than ESM. Thirdly, as Australian labour market experience increases, unemployment rates fall and this decrease is slightly greater for immigrants than for natives.

The existing Australian literature (Miller, 1986; Inglis and Stromback 1986) supports many of the general conclusions reported, the one major exception being that they are silent on the issue explored here. This is because their analyses constrain the unemployment effects of ncreased schooling to be identical for immigrants and natives, and as such the approaches do ıot allow substantial insights into the relative transferability of particular levels of schooling. The relaxation of this assumption offers important information concerning labour market djustment processes, the fundamental implication for research being that future explorations of ll countries' immigrant relative labour market success should take explicit account of the ıossibility.

For example, if the wage offer distribution is uni-modal symmetric, a fall in the variance of the listribution causes G(.) to fall at wages less than the mean wage and rise at wages greater than the mean vage. Skewness considerations add considerably to the complexity of the comparative statics.

Australia's selective immigration policy of the last several decades has given preference to immigrant ıpplicants whose job skills are in scarce supply in Australia. This policy has tended to raise the relative ob arrival rate for immigrants. However, a high percentage of immigrants enter as accompanied family ır under family re-union programs and thus are not chosen because they have especially good job ırospects. Indeed, the job arrival rate for these persons may be quite low.

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Paper 10

IMMIGRANT WAGE AND UNEMPLOYMENT EXPERIENCE IN AUSTRALIA

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I. INTRODUCTION AND SUMMARY

The question of how immigrants fare in their new labor markets is of considerable interest to policy-makers. It is also an issue of importance for both economic theory and applied econometrics, since implicit in it is the potential for examining the processes of and constraints to human capital transference and human capital investment. The Australian labor market, characterised as it is by a relatively high proportion of non-native workers, is an ideal testing ground for such an inquiry.

This paper analyses the relative labor market success of immigrants using the 1 per cent sample of the 1981 Australian Census, data which have previously been exploited for this purpose. Our major contribution lies in the adoption of flexible estimation techniques. This allows two insights into the operation of the Australian labor market, both of which are related to the role of education as a determinant of immigrant success. Neither has been recognised in previous research.

The first is that it is important to allow the wage effects of labor market experience and ethnicity to differ by education levels. As well, it is clear the role of schooling in the determination of unemployment can only be adequately understood by estimating relationships in a disaggregated way. The clear and consistent result from our methods is that, relative to similarly education natives, immigrants with the highest levels of education receive the lowest wages and experience the highest unemployment. We do not explore fully the reasons for these outcomes, but touch on some possibilities.

Apart from the insights allowed through disaggregated estimation of the role of schooling, the paper offers the following technical innovation. For one of the first times, the results of non-parametric estimations of wage functions are presented. The major benefit of this approach is the flexibility afforded, but the method is not unambiguously superior to OLS regression analysis. It is at least clear that useful (graphical) interpretation of causal mechanisms may be highlighted through the use of non-parametric techniques.

The data used are cross-sectional and, consequently, estimations could be contaminated by important problems associated with the unobserved ability of immigrants. Because this possibility is highly relevant to understanding results, some effort is directed to understanding the empirical significance of this potential. It is to the role of unobserved ability in cross-sectional data that we turn first.

II. EXAMINING THE USEFULNESS OF CROSS-SECTION DATA

Several difficulties arise in analysis of relative immigrant labor market outcomes using cross-sectional data. Essentially the problem is that it is difficult to believe that the usual *ceteris paribus* assumptions hold. In particular, the concern is that immigrant cohorts differing in length of residency are also dissimilar in terms of unobserved ability or motivation. If this is the case some parameters of major interest, such as the elasticities found between length of residency and both wages and unemployment, cannot be estimated without bias.

There are two obvious dimensions to the unobserved ability issue noted above. The first is that differences in economic conditions or government policy will undoubtedly affect the average quality of the entering immigrant pool in particular years. Secondly, the act of migration is not irreversible, and a sizeable (but variable) proportion of immigrants from different countries eventually leave their new countries. If the probability of remigration is correlated with (unobserved) immigrant quality, cross-sectional data will misrepresent underlying relationships, at least as indicators of expected immigrant success.

These issues have been examined with US data by both Borjas (1985) and Chiswick (1986). The former argues that considerable variation exists in the ability of immigrant cohort, suggesting bias in the interpretation of the effect of period of residence on wage growth. Chiswick's analysis implies that remigration does not markedly affect wage estimates.

Since the wage and unemployment analyses reported in sections III and IV use a single cross-section of data, the one per cent sample of the 1981 Australian Census, it is pertinent to attempt to establish the empirical significance of the unobserved quality issue in the Australian context. Both the quality of entering cohorts and the effects of remigration are investigated below using wage data. It is important to stress, however, that the unobserved ability issue applies as much to unemployment as it does to wages, the focus on the latter being a consequence only of data availability.

The methodology suggested by Borjas (1985) to examine immigrant quality may be applied to Australian data. The approach compares predicted wages of different and similar immigrant cohorts relative to natives, and can be used to gain insight into both the returns to local experience for a particular cohort of immigrants, and the differences between immigrant cohorts in unmeasured wage ability.

The data available allowing a similar investigation to that of Borjas were drawn from the (ANU) 1973 Social Sciences survey of Australian male residents aged 30-64, which includes a sample of about 1800 wage or salary-earning individuals, and the 1981 1/100 Census tapes. The results of the analysis are reported in Beggs and Chapman (1988b). They imply that the likelihood of cross-sectional data being markedly contaminated by significant changes over time in unobserved variables is small.

The other major possibility rendering questionable cross-sectional analysis concerns the effects of re-migration on the average unobserved ability of remaining cohorts. In Australia this issue is potentially much more than trivial, particularly if the question concerns the differential relative labour market outcomes of immigrants from English-speaking countries (ESM) and immigrants from non English-speaking countries (NESM). This is because marked differences exist between these groups in the probability of re-migration, as suggested by the data of Table 1.

TABLE 1

Arrivals and Departures of Male Immigrants, 1959-82

Country of Birth	Arrivals	Departures	Departures/Arrivals
UK & Ireland	1086054	231810	0.213
New Zealand	118157	33878	0.287
Italy	187368	10660	0.0569
Greece	159763	5374	0.0336

Source: Department of Immigration and Ethnic Affairs (1982).

These data, while not ideal as reflections of re-migration probabilities over 1959-82 (since both some of the arrivals may still depart, and some of the departures may yet return), are useful as broad indications of the empirical dimensions of the process. They suggest that about 20-30 per cent of migrants from English-speaking countries are likely to leave, but only 3-6 per cent of migrants from non English-speaking countries do likewise. The two salient points are: one, that the proportion of the former group leaving is high, implying that if a relationship exists between unobserved ability and re-migration the use of cross-sectional data for this group is suspect; and, two, that the problem is much less likely to be the case for NESM.

The biases introduced by re-migration could go either way, but they are usually believed to have the following pattern. The least successful immigrants eventually return home or seek economic success elsewhere. Thus the average ability of identified immigrants could be expected to increase with period of residency, and the coefficient on this variable is thus biased upwards in absolute size in both wage and unemployment estimations. alternatively, successful immigrants are more capable of moving because they have accumulated sufficient wealth. From theory an unambiguous prediction of the direction of bias is not forthcoming.

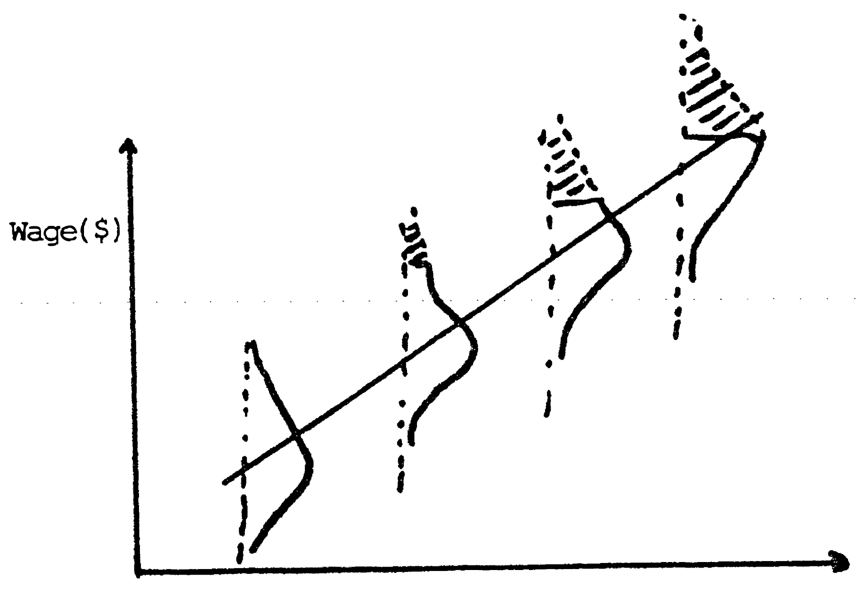
Related to the above is the following general issue. Immigrants' decision to remain in or leave new countries cannot be analysed only on the basis of economic performance in the host country. What will be of importance is expected income in alternative countries. Even if success has been relatively low in the new country this does not necessarily imply an increased incentive to return.

The above point is analogous to that analysed by Beggs and Chapman (1988a) concerning Australian Public Service workers' incentives to quit given poor performance. Their model predicts that the choice depended on the variance of inside compared to outside wages. For their sample it appeared that relatively poor workers had disincentives to quit, the opposite being true for relatively able workers. For immigrants similar forces may be in operation, the important point being that host country performance *per se* tells us little about the likelihood of return migration. This complexity renders questionable one of Chiswick's (1986) tests of the return migration proposition. He argues that if immigrants from countries with relatively high period of residency coefficients are also those immigrants who from other evidence are less likely to return, the self-selection process is empirically unimportant. The indications are that this is the case (Cubans, for example, have very high rates of return to residency, but few returnees), but the noted theoretical ambiguity casts doubt on the result.

Insight into an indirect test of the importance of return migration is to be found in Beggs and Chapman (1985), where the focus is on identifying attrition bias in the Australian Public Service. In particular, the argument is that the residuals of the wage equation should exhibit skewness related to job tenure, the direction of which will be determined by whether or not high or low ability persons eventually quit. Negative skewness implies that the top part of the intangible ability distribution (the residual) shortens with tenure, that is, that the more able increasingly leave the sample. The opposite is the case if lower ability persons are more likely to leave as tenure increases.

The analogous proposition is illustrated in Figure 1. It shows that as the highest ability immigrants leave, the residuals become increasingly positively skewed.

FIGURE 1



Note: Shaded area self selected out of sample.

Given the aggregate data, systematic biases from attrition imply the following. If lower ability immigrants are generally more likely to return, skewness of the residuals with period of residency (PER) should be positive for both ESM and NESM, and greater for the former group. On the other hand, if higher ability immigrants are generally more likely to return, skewness of the residuals with PER should be negative for both ESM and NESM, and absolutely greater for the former group. The finding that skewness does not exist implies that the attrition process is unrelated to unmeasured ability, at least as reflected by the residual of the wage equation.

The test took the form of estimating the following equation:

$$RES3 = a + bPER + cPER^2 + dYOS + e_R \qquad (1)$$

where RES3 is the cube of the OLS residuals obtained from the 1881 cross-sectional estimations reported in Beggs and Chapman (1988b) and YOS is years of schooling, included as a control. The results are presented in Table 2.

TABLE 2

Skewness Tests of the Return Migration Hypothesis

ESM:	0.120	-	0.00311 PER	+	0.0000143 PER^2	-	0.00554 YOS
	(0.50)		(0.25)		(0.048)		(0.34)
	$\bar{R}^2$ =		-0.0015				
NESM:	0.206	-	0.0106 PER	+	0.000103 PER^2	-	0.00345 YOS
	(1.08)		(0.74)		(0.27)		(0.31)
	$\bar{R}^2$ =		-0.0002				

* Absolute t-statistics in parentheses.

The results of Table 2 imply that there is no evidence from the wage data of the 1981 Census of a relationship between unmeasured ability and the likelihood of re-migration of either ESM or NESM. The nature of the tests suggests that this cannot be taken as strong confirmation of the randomness of the ability re-migration process. The appropriate conclusion is that the test has not uncovered any information with respect to re-migration that places in question the validity of the usual estimations based on cross-section data.

In summary, the above data and tests on wage relationships reveal that, for Australia, there is no compelling evidence that major differences in the unobserved ability of immigrant cohorts significantly undermine the value of wage and unemployment analyses based on cross-sectional data a point more obviously true for ESM than NESM. If either immigrants entering in particular years are of considerably different quality than others, or persons re-migrating come from either end of the ability distribution, we have not been able to uncover powerful evidence for these effects. This could imply problems with the methods used, a possibility that cannot be tested easily. Given this as background we proceed now to an examination of immigrant wage and unemployment outcomes as documented in the 1981 Australian Census under the assumption that tests based on such a cross-section are at least moderately sound.

III. IMMIGRANT WAGE EXPERIENCE IN AUSTRALIA

Analysis of immigrant relative wages in Australia typically use cross-sectional data and conventional earnings functions. This literature, and the concensus emerging from it, are examined below.

The first multiple regression analysis of immigrant earnings in Australian was conducted by Haig (1980) who used cross-sectional data collected by the Australian Bureau of Statistics for the Henderson Inquiry into Poverty in August 1973. The study attempted to determine the role of endowments and discrimination as explanators of immigrants' relative earnings with a conventional application of the methodology popularised by Oaxaca (1973) and Blinder (1973). Control variables used were, among others, age, hours, education, sex, and country of origin. Because he restricted slope coefficients to be identical for men and women, assumed hours worked to be exogenous, and used age as an experience proxy, the results should be treated with caution. Nevertheless, for the purposes of the present investigation it is of interest to note his finding that immigrants generally, and immigrants from Southern Europe in particular, had relatively flat age-earnings profiles in 1973. Period of residency was apparently an insignificant earnings determinant.

More disaggregated approaches have been adopted by Stromback (1984), Chiswick and Miller (1985) and Beggs and Chapman (1986) using the 1 per cent household sample of the 1981 Australian Census. The analyses used similar specifications and, thus unsurprisings, drew similar conclusions. They were that ESM experienced wage structures similar to the Australian-born, but other immigrants received relatively low returns to schooling and experience. The data reveal no catch-up for NESM.

These results could be interpreted only imperfectly in the context of the transferability of further investment in human capital: while they imply that skills acquired in like-countries are rewarded more highly than skills acquired in dissimilar countries (non-English speaking countries), they also imply that those immigrants starting with a wage disadvantage relative to the native-born remain with a wage disadvantage over their Australian working lives. This finding is at variance with US and Canadian conclusions derived from cross-sectional analyses.

While it is important to acknowledge that cross-sectional analyses such as these implicitly impose the assumption that unobserved ability is uncorrelated with immigrant length of residency, the estimations presented in section II above suggest that for Australia this point is not of great empirical significance. What is, perhaps, of much more importance in an understanding of immigrant wage adjustment processes is the role of education, but due to functional form inflexibilities the Australian research so far has allowed only very limited insights into the part played by schooling. In particular, the above-noted approaches have typically imposed the restriction that wage returns to length of residency and labor market experience do not vary with education. The major contribution here is to relax this assumption, an approached revealing quite different insights into immigrant wage outcomes in Australia to those reported above.

As well as allowing experience effects to vary with schooling, an innovation of our approach is the use of non-parametric techniques to estimate the wage functions.

The choice of the kernel smoothing (non-parametric) technique was motivated by the following issue. This approach obviates the need to pre-specify the precise analytic functional relationship between the explanatory variables and the wage rate, and it is this major benefit we have taken advantage of. As well, the technique is ideally suited to large samples because of the covergence properties (Bierens, 1985), and is ill -suited to small data sets, which explains why we did not adopt it for the 1973 and 1981 wage comparisons reported in section II.

The non-parametric approach is not, however, without costs. Specifically, the approach lacks the familiar summary of a model as a small number of estimated parameters, nor is there easy access to the usual test statistics, although point-wise confidence intervals may be computed. As well, for reasons of presentation, it is inappropriate to estimate the expected wage rate given values of more than just a few of the regressor variables of interest. In short, the approach trades-off flexibility, and in its adoption we have chosen more of the former.

The basic model is that hourly income depends on individuals" human capital characteristics. In the analysis reported below we allow for the effect of years of schooling (YOS), years in the labour market (GEXP), years of Australian schooling (AYOS), and years of labor market experience before migrating to Australia (ALMX). As well, there is a separation of the sample according to country of origin. Three groups are distinguished: Australian born males (AUST); male immigrants born in English speaking countries (ESM); and male immigrants born in non-English speaking countries (NESM). The wage equations are:

$$W_{AUST} = f_{AUST}(YOS, LMX) \qquad (2)$$

$$W_{ESM} = f_{ESM}(YOS, LMX, AYOS, ALMX) \qquad (3)$$

$$W_{ESM} = f_{NESM}\ (YOS,\ LMX,\ AYOS,\ ALMX) \qquad (4)$$

and the statistical characteristics of the data are reported in Appendix 1. The results of the non-parametric regressions are presented in graphical form, the interpretation of which is straight-forward. The computed confidence intervals are not shown for reasons of clarity, their size being such as to not affect conclusions.

Since a major thrust of the analysis is to investigate the potential of schooling impacting differently on wages for particular groups, we considered four education levels: 8, 10, 12 and 14 years of completed schooling, with the results showing the cross-sectional relationship between wage and age (GEXP + YOS + 5) for natives, ESM and NESM. The immigrants are hypothetically given AYOS = 0 and ALMX = GEXP. That is, the relationships for these groups should be interpreted as representations of the experience of male individuals entering Australian immediately after completing their schooling abroad. They are shown in Figures 2 - 5.

The data of Figure 2 reveal the following. First, natives with very low schooling earn, overall, lower hourly incomes than all immigrants, although there is no obvious difference between natives and NESM until about aged 36, after which NESM have higher incomes. Secondly, ESM experience higher incomes at all ages than the other two groups, with the difference being maximised for those in their mid-40s. Thirdly, and related to the above, at relatively young ages the age-earnings profiles are steeper for immigrants than natives, and steeper for ESM than NESM.

For males with 10 years of schooling (Figure 3), the following points are pertinent. One, unlike the situation for the lowest level of schooling, natives earn higher incomes than NESM at all ages. Two, similar to the results for the lowest level of schooling, ESM earn higher incomes than natives at all ages, with the difference being the greatest for persons aged in their late 40s.

From Figure 4, it is apparent that for those with 12 years of schooling, native incomes are higher than those of NESM and that this advantage is greater than is the case of persons with 10 years of formal education. As well, apart from those younger than 30 years - where income per hour is about the same - natives earn more than ESM. As is the case with other schooling levels, ESM earn higher incomes than NESM, and the profile of the former is relatively steep, at least until about age 50.

Figure 5 reveals that at high levels of schooling (14 years) natives earn substantially higher incomes than immigrants. Importantly, the relative income advantage of the former group is apparently greater than was the case for those with 12 years of schooling. As is the case with the 10 and 12 years of schooling groups, NESM earn less than ESM and natives at all ages, and have flatter profiles.

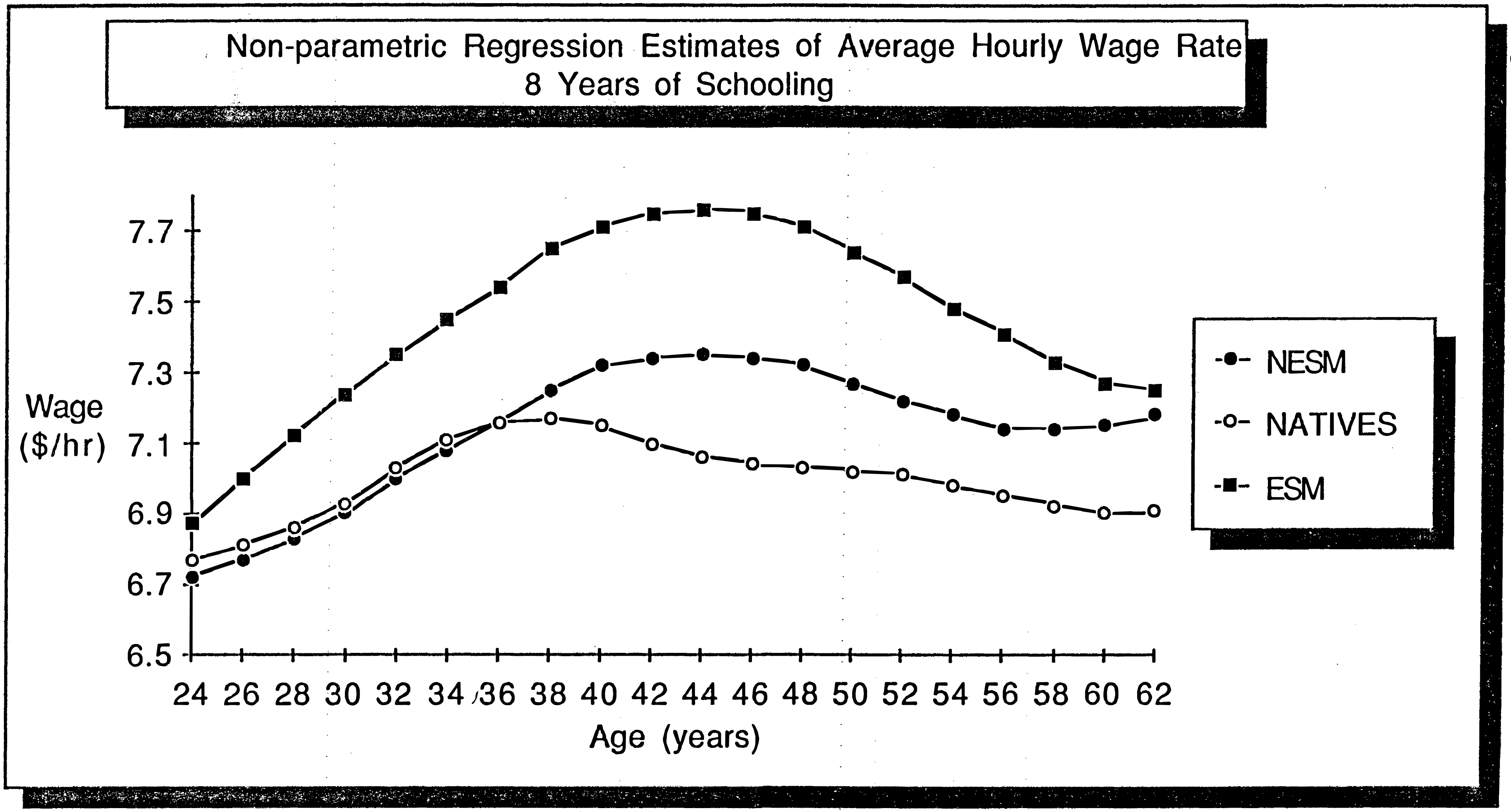

Non-parametric Regression Estimates of Average Hourly Wage Rate
8 Years of Schooling
Wage ($/hr)
7.7
7.5
7.3
7.1
6.9
6.7
6.5
24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62
Age (years)
NESM
NATIVES
ESM

Fig. 2

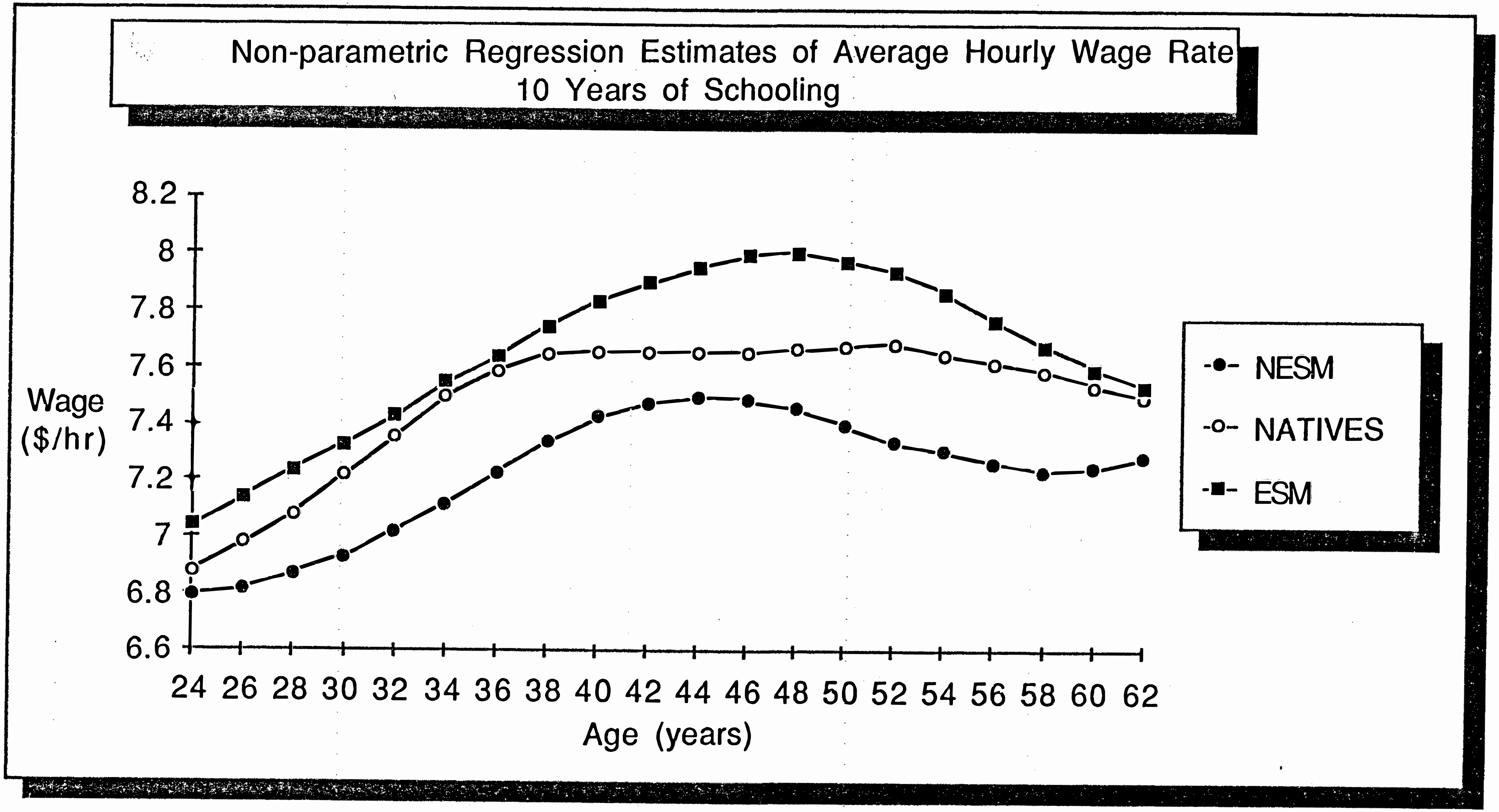
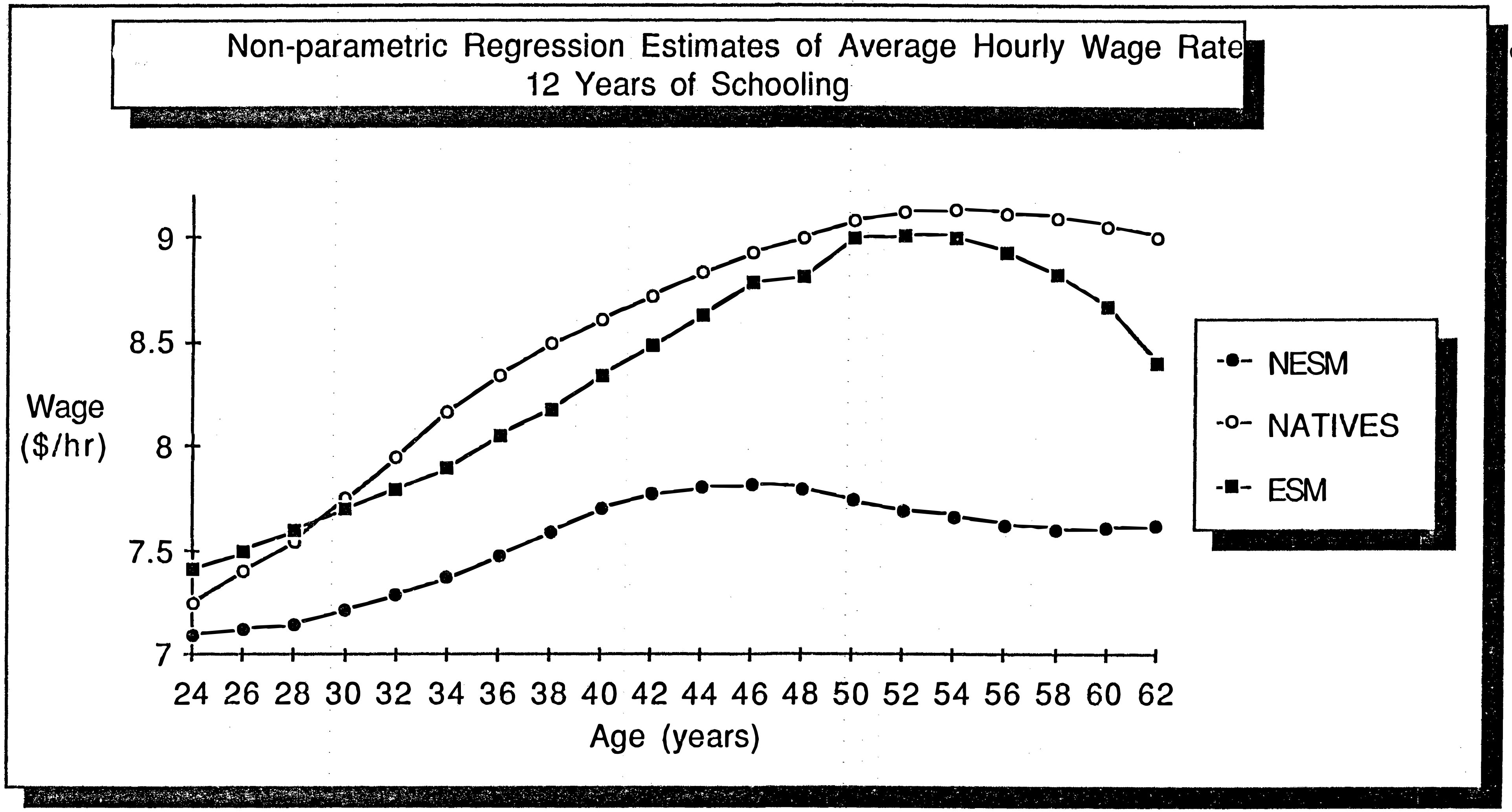

Non-parametric Regression Estimates of Average Hourly Wage Rate
10 Years of Schooling
8.2
8
7.8
7.6
7.4
7.2
7
6.8
6.6
Wage ($/hr)
24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62
Age (years)
NESM
NATIVES
ESM

Fig. 3

Fig. 4



Non-parametric Regression Estimates of Average Hourly Wage Rate
12 Years of Schooling
Wage ($/hr)
9
8.5
8
7.5
7
24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62
Age (years)
NESM
NATIVES
ESM

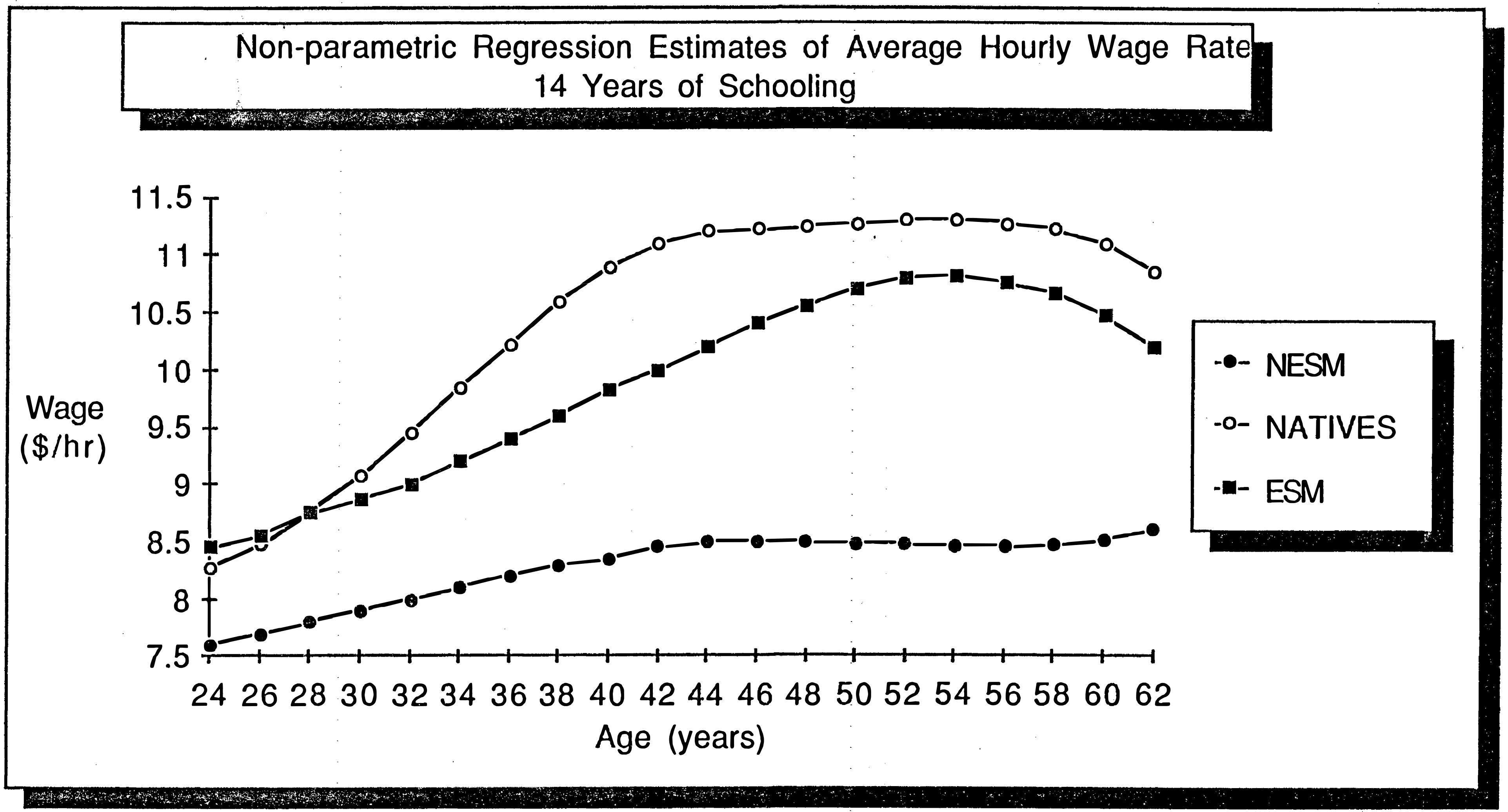

Non-parametric Regression Estimates of Average Hourly Wage Rate
14 Years of Schooling
Wage ($/hr)
11.5
11
10.5
10
9.5
9
8.5
8
7.5
24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62
Age (years)
NESM
NATIVES
ESM

Fig. 5

The figures, considered sequentially, reveal a striking pattern. This is that *as education increases so too does the relative income advantage of natives.* At the lower levels of schooling immigrants earn the same, or more than, natives, but at the highest levels of schooling this situation is reversed. As well, NESM earn lower incomes than ESM for all levels of schooling, and have generally flatter age-earnings profiles. These findings highlight the importance of disaggregated analysis of immigrant relative wage outcomes, in particular as regards education.

Most importantly for Australian analysis the non-parametric approach reveals the importance of flexible estimation. Two salient points are clear. One, it is obviously not the case that returns to Australian labor market experience are identical for different schooling levels, the assumption usually imposed in OLS wage estimations. Two, interpretation of which immigrant groups are apparently relatively disadvantaged in wage outcomes is inadequate without considering the impact of schooling. Interestingly the essence of this story is replicated with respect to unemployment, an investigation to which we now turn.

IV IMMIGRANT AND NATIVE UNEMPLOYMENT DETERMINANTS

Analyses of the relative unemployment experience of immigrants in Australia (Miller, 1986; Inglis and Stromback, 1986) have, as with wage determination research, imposed restrictive functional forms on their investigation. In particular, the approaches have not allowed the effect of unemployment determinants to vary between groups, a method which restrict the effects of schooling to be the same irrespective of ethnicity. In the analysis reported below we allow the effects of schooling (and other variables typically associated with unemployment) to vary between natives, ESM and NESM. Although the estimations constrain Australian labor market experience to have the same impact on unemployment for different schooling levels, they nevertheless offer compelling evidence on the effect on relative immigrant labor market outcomes of education entirely consistent with the wage analysis reported above.

Search theory is used to motivate the empirical approach adopted, and we have described the advantages and problems of this framework elsewhere (Beggs and Chapman, 1987). The mode only allows one prediction unambiguously, in essence because the distribution of the wage offer curve will not be the same between groups. This is that the relative unemployment rate is lower for unskilled immigrants than skilled immigrants. The prediction follows from our critique of the theory, but is not crucial to understanding the results following.

The simple reduced form derived from search theory is:

$$P(U_i) = \delta_0 + \delta_1 LMX_i + \delta_2 LMX_i^2 + \delta_3 YOS_i + \delta_4 YOS_i + \delta_5 ALMX_i$$
$$+ \delta_6 ALMX_i^2 + \delta_7 MAR_i + \delta_8 LANGD_i + \varepsilon_\delta \quad (5)$$

where $P(U_i)$ equals 1 if the individual is unemployed, equals 0 if employed, and the explanatory variables are labor market experience (LMX), Australian labor market experience (ALMX), a dummy variable equal to 1 if currently married, spouse present, and equal to 0 otherwise, (MAR), and a language dummy variable equal to 1 if English language problems, equal to 0 otherwise (LANGD).

The probit estimations were run separately for natives, ESM and NESM, a procedure allowing important insights into the role played by education. Statistical characteristics of the data, random samples from the 1 per cent of males in the 1981 Census, and the results are reported below.

From Table 4 the strongest single effect on unemployment are from schooling and marital status. The labor market experience variables exhibit low individual t-statistics which reflects the fact that LMX, LMX^2, ALMX and $ALMX^2$ are highly correlated in the sample, implying that it is difficult to distinguish their individual contribution to unemployment. Consistent with this interpretation, the log-likelihood ratio statistics indicate that only the joint test of the null hypothesis $\beta_{LMX} = \beta_{LMX^2} = 0$ for NESM fails to be rejected by the data.

The highly non-linear form of the probit probability model hampers straightforward comparison of immigrant and native coefficients. Interpretation of the main relationships is facilitated through consideration of Figures 6 to 9. To allow comparisons with the results reported in section III, each of the figures corresponds to a different level of total schooling with the levels chosen being 8, 10, 12 and 14 years. AYOS and LANGD are set equal to zero, and MAR is set equal to 1. Five categories of workers are considered. They are:

(i) AUST: Australian Born Workers

(ii) ESM: migrants from English speaking countries with no labor market experience before entering Australia

(iii) NESM: migrants from non-English speaking countries with no labor market experience before entering Australia

(iv) ESMIO: migrants from English speaking countries with 10 years labor market experience before entering Australia

(v) NESMIO: migrants from non-English speaking countries with 10 years labor market experience before entering Australia.

TABLE 3+

Statistical Characteristics of the Data

Variable	Australian Born	English-Speaking Country Born	Non-English Speaking Country Born
YOS	11.65	12.12	11.15
	(2.42)	(2.46)	(3.33)
LMX	25.54	25.87	27.33
	(12.22)	(11.94)	(11.63)
MAR	0.78	0.79	0.84
ALMX		15.68	17.80
		(10.28)	(9.53)
AYOS		1.72	1.78
		(4.06)	(4.17)
LANGD			0.44
Aggregate Unemployment Rate (per cent)	3.44	4.36	4.60
Number of Observations	3634	3668	3607

+ Means, standard deviations in parentheses.

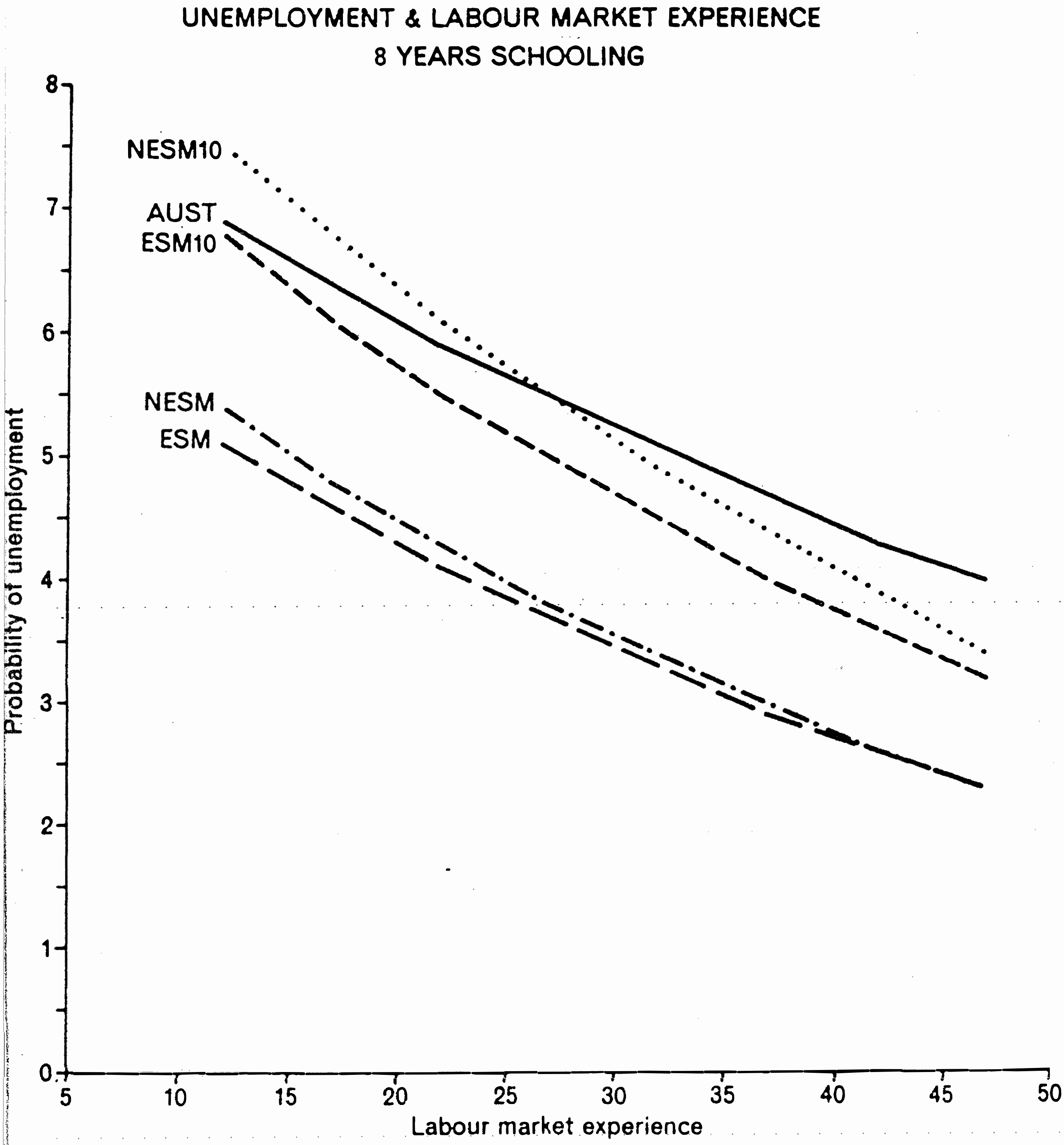
TABLE 4*

Probit Estimates of Probability of Unemployment

Variable	Australian Born	ESM	NESM
Intercept	0.359	0.397	-0.630
	(0.95)	(1.23)	(2.2)
LMX	-0.0213	-0.0533	0.00137
	(1.15)	(2.96)	(0.075)
LMX^2	0.000254	0.00104	0.0000770
	(0.76)	(3.30)	(0.25)
YOS	-0.129	-0.0773	-0.0335
	(5.81)	(4.20)	(2.72)
AYOS		-0.000104	-0.0134
		(0.00932)	(1.16)
ALMX		-0.0252	-0.0250
		(1.92)	(1.71)
$ALMX^2$		0.000299	0.000240
		(0.96)	(0.61)
MAR	-0.568	-0.559	-0.468
	(6.38)	(6.67)	(5.18)
LANGD			0.123
			(1.62)
McFadden R^2	0.075	0.076	0.043
Likelihood Ratio Tests	Ho:$\beta_{LMX}=\beta_{LMX^2}=0$ Rejected (95% level)	Ho:$\beta_{LMX}=\beta_{LMX^2}=0$ Rejected (99% level)	Ho:$\beta_{LMX}=\beta_{LMX^2}=0$ Accepted (99% level)
		Ho:$\beta_{ALMX}=\beta_{ALMX^2}=0$ Rejected (95% level)	Ho:$\beta_{ALMX}=\beta_{ALMX^2}=0$ Rejected (95% level)

* Absolute t-statistics in parentheses.

Fig 6



UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
8 YEARS SCHOOLING
NESM10
AUST
ESM10
NESM
ESM
Probability of unemployment
Labour market experience
0
1
2
3
4
5
6
7
8
5
10
15
20
25
30
35
40
45
50

Fig 7

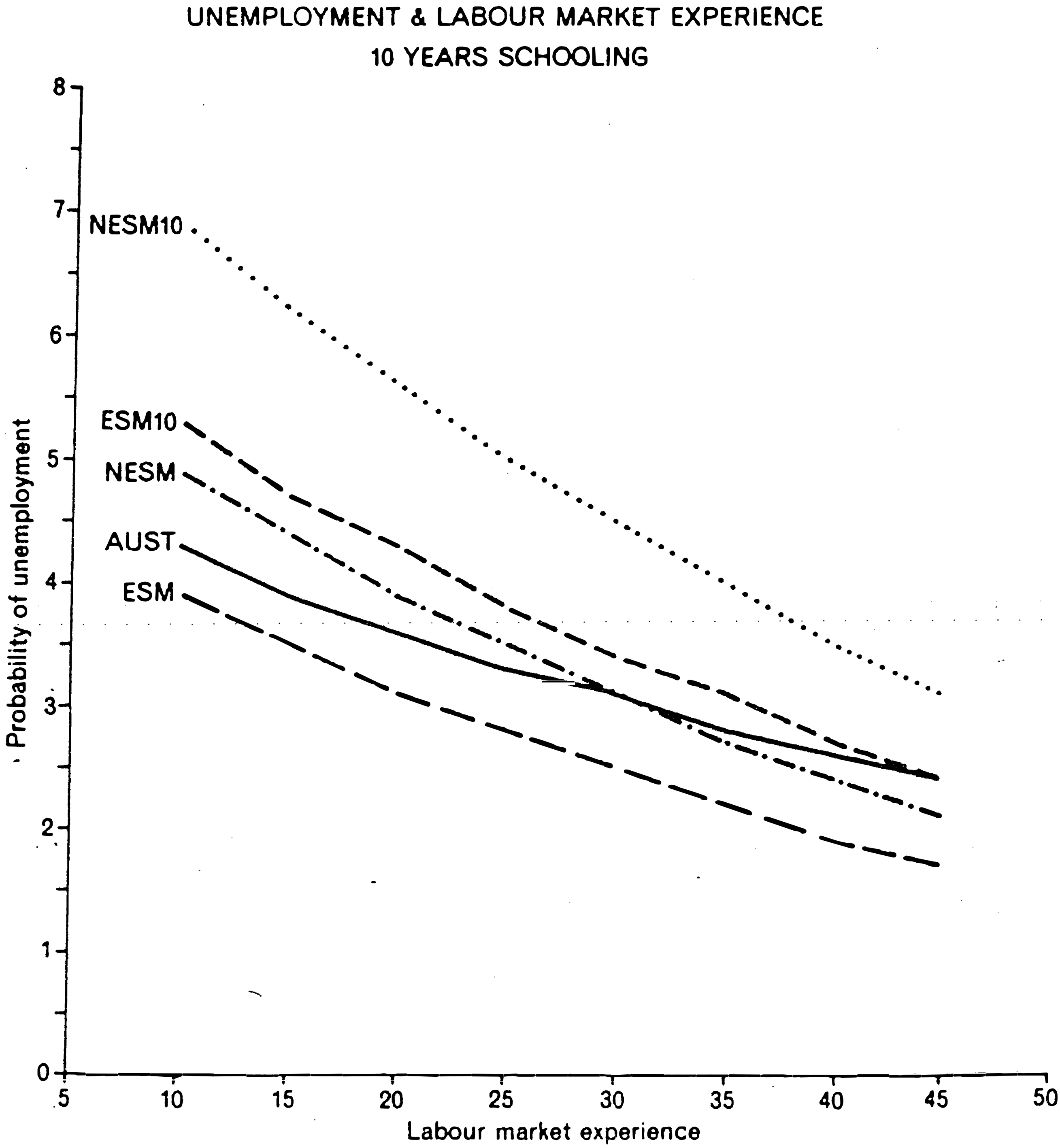
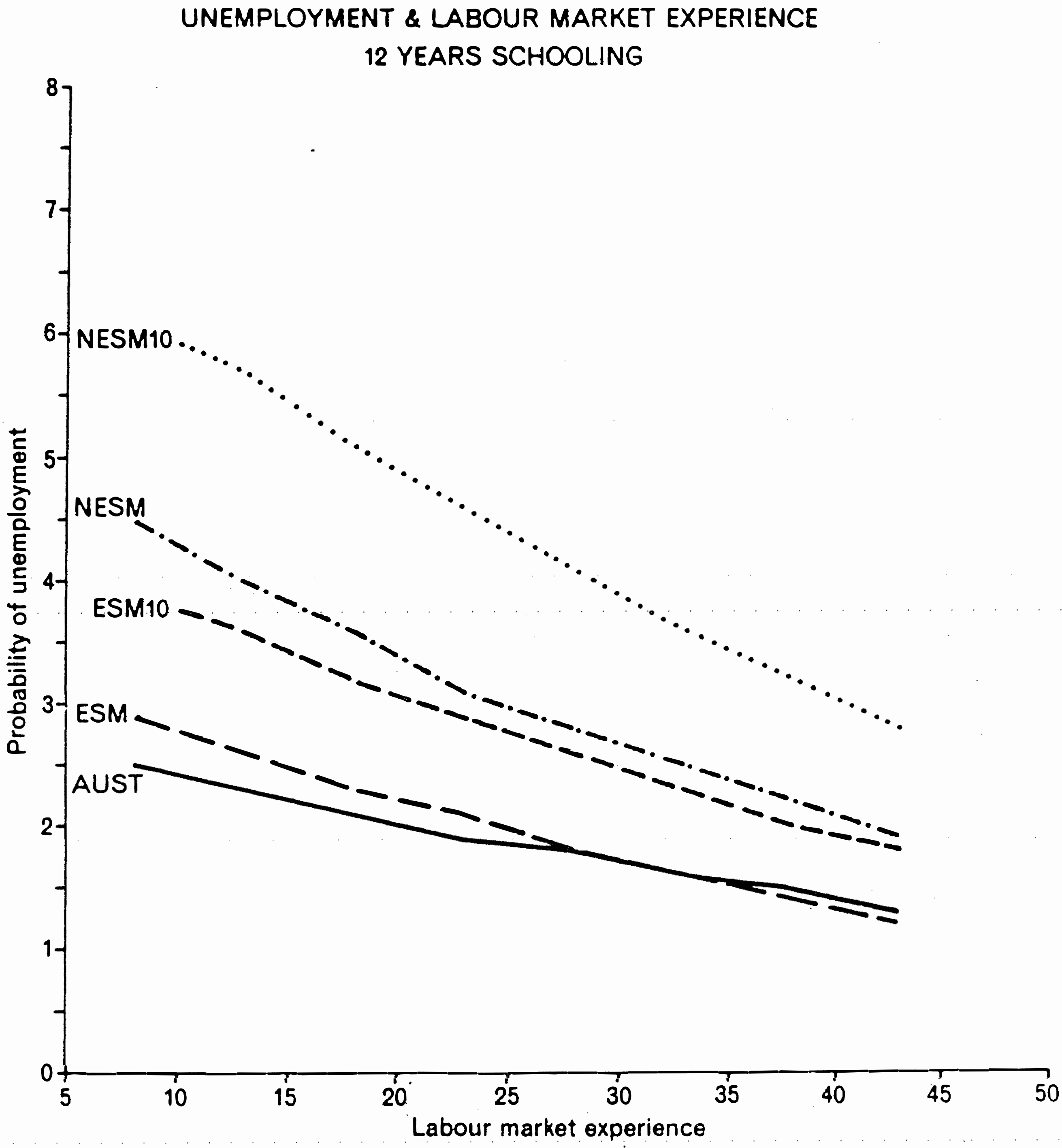

UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
10 YEARS SCHOOLING
Probability of unemployment
Labour market experience
NESM10
ESM10
NESM
AUST
ESM
0
1
2
3
4
5
6
7
8
5
10
15
20
25
30
35
40
45
50

Fig 8


UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
12 YEARS SCHOOLING
Probability of unemployment
Labour market experience
NESM10
NESM
ESM10
ESM
AUST
0
1
2
3
4
5
6
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25
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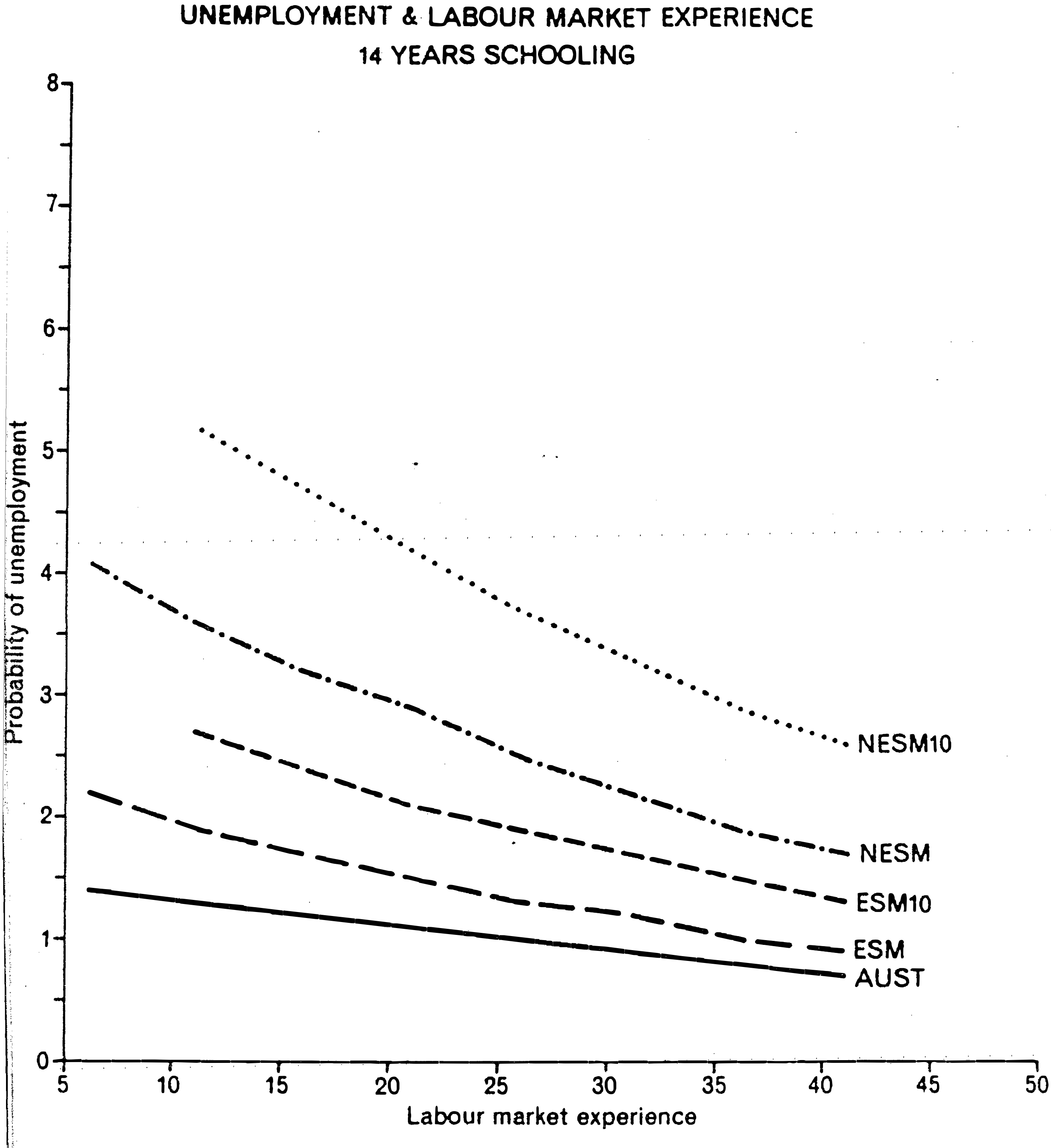
35
40
45
50

Fig 9


UNEMPLOYMENT & LABOUR MARKET EXPERIENCE
14 YEARS SCHOOLING
Probability of unemployment
0
1
2
3
4
5
6
7
8
5
10
15
20
25
30
35
40
45
50
Labour market experience
NESM10
NESM
ESM10
ESM
AUST

The figures reveal some important similarities. First, ESM have lower probabilities of unemployment than do NESM, for all levels of schooling aNd pre-immigration work experience. From theory this implies that the greater search costs of the latter group are outweighed by other factors of job search related to ethnicity. Second, pre-immigration work experience increases the probability of unemployment for both groups (i.e. ESM and NESM have lower unemployment probabilities than ESMIO and NESMIO, respectively), implying higher reservation wages of immigrants with greater home country experience. Thirdly, as period of residence increases the gap between immigrant and native unemployment changes only very slowly. In general over the range of normal life-cycle labor market experience, there is no cross-over.

Most importantly for the theme of this paper, it is clear that as the level of schooling increases so too does the relative immigrant unemployment rate. From Figure 6, immigrants with low levels of schooling (8 years) have lower probabilities of unemployment than native over the life-cycle range of labor market experience. On the other hand, at the highest level of schooling considered (Figure 9) immigrants have higher probabilities of unemployment than like natives over the life-cycle range of labor market experience. Clearly, and interestingly, the story is the same as that revealed for wages: *as education increases the labor market position of immigrants relative to natives systematically deteriorates.*

The important upshot of these results is that it is not possible to understand influences on relative immigrant unemployment status without distinguishing the differential impacts of schooling. As with Australian wage analysis, existing research has missed an important point by the restriction of education effects. We offer some tentative conjectures on the results below.

V INTERPRETATION

Through a disaggregated analysis of immigrant relative wages and unemployment a distinct phenomenon has been revealed. For these labor market outcomes it is apparent that, relative to like natives, immigrants with low levels of education fare well. However, as years of schooling increase immigrants' relative labor market success decreases. At the highest level of education considered immigrants both earn less and have a higher probability of unemployment than is the case for natives. Importantly for Australian research, our estimation techniques have highlighted the potential for misinterpretation inherent in existing approaches.

The interesting and difficult challenge is to explain satisfactorily the consistent relationships uncovered between relative educational status and immigrant labor market outcomes. Tentatively, we offer four possibilities.

First, and most obviously, is the issue of accreditation of education qualification. This could result from two possibilities: one, Australian employers, if risk-averse and with less than full information about the value of overseas schooling, systematically devalue immigrant formal training; two, local employers or professional agencies act in such a way as to protect domestic special interest groups. There is considerable anecdotal and other evidence for this perspective (Iredale, 1988).

Secondly, a lower valuation of overseas schooling at high levels of schooling may be because education is less transferable internationally at higher levels. In other words, education is positively correlated with the acquisition of country-specific skills. To take an extreme example consider the transferability of accountancy qualifications relative to the transferability of street-sweeping g skills. In the former case there is presumably a high level of country-specific knowledge, such as in the understanding of tax and company law. For the latter, sweeping a Rome street is probably very similar to sweeping a Melbourne street, and such work would require very little understanding of Australian institutions or the legal system.

A third possibility is that of an unobserved variable, motivation or ability, and its relation to immigrant selection procedures. If low education immigrants are more likely to be selected by immigration authorities if they are particularly work-oriented, it follows that relative to the native-born their Australian labor market outcomes would be favourable. In this view immigration authorities are trading-off work orientation or talent for formal qualifications in selection of applicants. This explanation recognises a possible shortcoming of the estimation techniques in that the models may be mis-specified because of omitted variables and their correlation with education. A similar econometric issue motivates the last explanation of results.

This is that the quality of schooling is actually higher in Australia than overseas, or at least is perceived to be so by local employers, a point obviously related to the first possibility offered. Thus, as schooling increases and the quality issue becomes more important, more highly education immigrants will have their overseas credentials increasingly devalued.

As noted, we do not have strong priors as to which possible explanation of results is most compelling, the goal having been to demonstrate the importance of more flexible estimation of immigrant relative labor market outcomes than has so far characterised the Australian literature in this area. The very important finding, unrecognised in the literature, is that Australian immigrant labor market outcomes become more adverse - relative to like natives - as schooling increases. The challenge highlighted is the explanation of the consistency of results found for the role of education.

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APPENDIX 1

Sample Characteristics for Males Aged 25 to 64 in 1981

Variable	Australian Born	English-Speaking Country Born	Non-English Speaking Country Born
Years of Schooling	11.65 (2.42)	12.12 (2.46)	11.15 (3.33)
Years of Labor Market Experience	25.54 (12.22)	25.87 (11.94)	27.33 (11.63)
Years of Australian Labor Market Experience		15.68 (10.28)	17.80 (9.53)
Australian Years of Schooling		1.72 (4.06)	1.78 (4.17)
Weekly Income (dollars)	301.90	314.39	262.95
Number of Observations	10532	2917	3083

* Means with standard deviations in parentheses.

Paper 11

THE FORGONE EARNINGS FROM CHILD-REARING IN AUSTRALIA

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Introduction

Raising children is an expensive process. The obvious direct costs include food, shelter, education and clothes. Less obvious, but undoubtedly of greater magnitude, are the labour market earnings forgone by child-rearers, typically mothers. In this paper we seek to determine the opportunity costs involved in bringing-up Australian children, an exercise not previously attempted.

There are several dimensions to this question.The basic story is that raising children takes time, and that the opportunity cost issue concerns both how much time out of the labour market is involved and at what rate the market values this time. Since it is only possible to earn labour market income if employed, the effect of children on labour force participation is fundamental. As well, even if parents do work while raising children the presence of the latter affects the number of hours of paid employment. The final part of the analysis concerns the effect of children on hourly wage rates.

Child-rearing affects the hourly wage rate in two ways. First, it impacts on women's labour market experience, an important wage determinant. For this reason it is not possible to estimate accurately the effects of child-rearing on earnings without direct measures of actual time spent in and out of employment. Secondly, periods of absence from the labour market are presumably associated with a diminution of the value of labour market skills, a process known as atrophy (Mincer and Polachek, 1974). Unfortunately, almost no labour market data set - and certainly none in Australia - allows this particular effect to be isolated accurately.

The data set used in the analysis, the Family Formation Survey, collected under the auspices of the Australian National University Research School of Social Sciences in April 1986 (Bracher, 1987), is ideal for our purposes in many respects. The sample is of about 2500 women aged 20 to 64, and includes information on a host of variables that must be controlled for to ascertain the independent effects of the presence of children on women's labour force participation and hours worked. Importantly, there are available direct measures of labour market experience, enabling some understanding the effect of children on hourly wage rates. No other broadly-based sample of Australian women has this essential control.

Before proceeding to a discussion of the methods employed and the results obtained, it is useful to make some broad observations on the usefulness and limitations of the exercise. For a host of reasons estimates of the opportunity costs of child-rearing matter. They allow insight into some aspects of the social and private costs and benefits of population growth, fertility control and alternative child-care arrangements. As well, the technique used enables a decomposition of the forgone earnings costs of child-rearing into the effects on labour force participation, hours worked and wage rates.

But it is important to stress that the results do not offer a complete picture of the relative merits of different fertility decisions, nor do they have direct implications for assessment of marital obligations. If people are informed, a person's forgone earnings from child-rearing must be less than the benefits from the process. Since it is likely that fertility decisions are made by two consenting adults, identifying only the costs involved does not offer a prima facie case for recompense given marital separation. It is one part only of a complex issue.

A more technical concern is that because the data are collected at one point in time, they do not allow us to capture the effects of changes in attitudes or tastes of women over time. Implicitly such analyses assume that in 1986 the values of 20 year old women are the same as the values of 60 year old women. The consistent increases in the labour force participation of women over recent years tell us that this is unlikely to be the case. The consequences of the problem are examined in the earnings analysis.

II. Conceptual and Technical Issues

For individuals, particularly women, labour force participation, hours worked and wage rates are influenced by the presence, age and number of children. Some of the other more important variables affecting labour market outcomes are a person's age, education, marital status and work experience. Because the independent effect of children cannot be ascertained without controlling for the influence of other variables, it is essential that the latter be held constant in the exercise. For this reason regression analysis must be employed.

The basic approach is as follows. The determinants of labour force participation, hours worked if employed, and hourly wage rates are estimated separately as functions of individual characteristics, including the presence, age and number of children. This enables the relative effects of children on each of the labour market variables to be identified and to be assigned values.

The parameters of each of these models are then used to simulate the effects of children on forgone earnings. Because the measured differences between women have many dimensions, a huge number of possible scenarios and outcomes could be illustrated. To simplify the reporting we have chosen several cases to highlight the major relationships. For example, the differences in outcomes for various levels of education are explored, as are distinctions between younger and older women. The simulation results are presented in aggregate in section IV.

The remainder of this conceptual and technical discussion concerns a brief theoretical examination of the functional forms employed. Below we consider in general terms respectively the models of labour force participation, hours worked if employed and hourly wage rates.

Modelling of the decision of whether or not to participate in the work-force can be traced to Bowen and Finegan (1969). The short-hand language usually employed is that women will work if their market wage rate exceeds their reservation wages, the latter meaning in this context the value placed on not working. The reservation wage is determined by, among other things, husband's income, and the presence, age and number of children.

Two difficulties encountered in such exercises are these. One, the wage rate for non-working women is unobserved. Two, the reservation wage is unobserved for all women. A typical solution to these problems is to use variables that are thought to influence wages and reservation wages, such as labour market experience, years of education, unearned income and husband's income. Controlling for these factors allows an isolation of the effect of children on participation. The model is presented more fully in section III.

A similar set of variables to those used in labour force participation models are employed in analyses of the number of hours worked, for those in the labour market. There are several pertinent points here. One is that actual hourly wages are observed for this group, and can be used as an explanatory variable. However, such an approach implicitly assumes that hourly wage rates are not influenced by the number of hours worked, a dubious proposition if, for example, part-time wage remuneration differs systematically from full-time wage remuneration. An alternative approach that avoids this problem somewhat is to use variables that affect wages, such as labour market experience and years of education, in place of wages in the hours equation. This is not uncomplicated, however, since these variables will affect both wages and reservation wages; interpretation is therefore not straight forward.

A second characteristic of the hours estimation is the possibility that working women differ systematically, in some unobserved way, to non-working women. This problem is known as selectivity bias, and its presence makes interpretation of coefficients difficult. A commonly used solution to the problem is to include in the hours estimate a variable designed to correct for selection, constructed from the participation equation (Heckman, 1979). These and other innovations are discussed more fully in an examination of the results.

The final area of modelling in this disaggregated exploration of the effects of children on forgone earnings concerns hourly wages. Typically, the approach adopted uses human capital theory (Becker, 1975; Mincer, 1974) in which wage outcomes are presumed to be the result of individuals' training, both on-the-job and in formal education. The former variable is generally approximated by length of time in the labour force, which is influenced by past and current child-rearing practices. Variations on the human capital wage estimation are presented in the next section.

III. The Data, Estimations and Results

The data are from a sample of women aged 20-64 which numbered 2358, 1159 who were in paid employment and 1199 who were not. Variable definitions are presented in Table 1, with the major statistical characteristics of the data being shown in Tables 2-4.

Employed women are similar to not-employed women in the following respects. The average age of both groups is mid- to late-30s, average years of education is around 12, there are similar proportions of migrants from non-English speaking countries (12-13 per cent) and husband's weekly after tax wage incomes are about 300 dollars.

The major differences between the groups concern: other unearned weekly incomes, with the wage earning group receiving about 60 dollars less per week; the number of children ever born, an average of about 1 less for the wage earning group; and years of work experience, with the wage earning group having about 20 per cent more. In general, the sample characteristics conform with expectations and are broadly consistent with data from the 1981 Australian Census.

TABLE 1
Definitions of Variable Names

Variable Name	Definition
LMX	Years of labour market work experience
LMX2	Square of LMX
HRS	Average hours worked per week
W[1]	Hourly gross wage rate
MW	Marginal after tax net hourly wage rate
LW	Logarithm of W
YOE	Years of education
CHILD	Dummy variable equal to 1 if ever had a child, 0 otherwise
CHILD*	Dummy variable equal to 1 if has any child less than 15 years, 0 otherwise
CHILD5	Number of children aged less than 5 years
CHILD15	Number of children aged 5 but less than 15 years
AGE25	Dummy variable equal to 1 if less than age 25, 0 otherwise
AGE35	Dummy variable equal to 1 if age 25 to 34, 0 otherwise
OINC	Average weekly unearned income
ATHINC[2]	Average weekly husband's after tax wage income
MAR	Dummy variable equal 1 if married or de facto, 0 otherwise
MIG	Dummy variable equal 1 if migrant from non-English speaking country, 0 otherwise
TEN	Years of tenure with current employer,
LAM	Inverse of the Mill's Ratio, used to correct self-selection bias in the sample

+ Explained in discussion following.
Notes: 1. Part-time wage rates were correct for the absence of annual leave and holiday loadings in casual wage rates.
2. This variable equals zero for husband not employed, or no husband present.

TABLE 2

Sample Characteristics of Women Both In and Out of Wages Employment
(Sample Size 2358)

Variable	Mean	Standard Deviation
Age	36.82	10.80
Children less than 5 years of age	0.161	0.369
Children less than 15 years and at least 5 years of age	0.262	0.448
Years spent in Education	12.19	1.93
Migrant from Non-English Speaking Country (proportion)	0.126	0.33
Married (proportion)	0.69	0.46
Other Unearned Weekly Income, $	68.82	340.02
After Tax Husband's Weekly Earned Income, $	302.55	152.07
Years of Work Experience	9.53	7.29
Number of Children Ever Born	2.06	1.64
Never Married (proportion)	0.09	0.29

TABLE 3

Sample Characteristics of Women in Wages Employment
(Sample Size 1159)

Variable	Mean	Standard Deviation
Age	34.90	9.93
Hourly Wage Rate, $	10.31	7.75
After Tax Marginal Wage Rate	6.89	3.98
Hours Worked Per Week	31.39	12.42
Children Less than 5 years of age	0.095	0.294
Children Less than 15 years and at least 5 years of age	0.236	0.431
Years Spent in Education	12.55	1.84
Migrant from Non-English Speaking Country (proportion)	0.12	0.32
Married (proportion)	0.64	0.48
Other Unearned Weekly Income, $	38.61	25.2
After Tax Husband's Weekly Earned Income, $	321.41	131.95
Years of Work Experience	10.48	7.36
Years of Tenure with Current Employer	4.90	5.40
Number of Children Ever Born	1.61	1.52
Never Married (proportion)	0.14	0.35

TABLE 4

Sample Characteristics of Women Not in Wages Employment
(Sample Size 1199)

Variable	Mean	Standard Deviation
Age	38.67	11.27
Children less than 5 years of age	0.224	0.419
Children less than 15 years and at least 5 years of age	0.287	0.462
Years spent in Education	11.85	1.95
Migrant from Non-English Speaking Country (proportion)	0.13	0.34
Married (proportion)	0.74	0.43
Other Unearned Weekly Income, $	98.01	405.20
After Tax Husband's Weekly Earned Income, $	286.66	165.56
Years of Work Experience	8.62	7.11
Number of Children Ever Born	2.49	1.64
Never Married (proportion)	0.044	0.205

III(i) Modelling Labour Force Participation

The first estimations relate to labour force participation (Ross, 1984). A probit model was employed, with the dependent variable LFP_i for individual i being equal to one if currently in the labour market, and equal to 0 otherwise. The functional form is:

$$\begin{aligned} LFP_i = \; & a_0 + b_0LMX_i + c_0LMX2_i + d_0YOE_i + e_0CHILD_i + f_0CHILD_i^* \\ & + g_0CHILD5_i + h_0CHILD15_i + i_0AGE25_i + j_0AGE35_i + k_0OINC_i \\ & + l_0ATHINC_i + m_0MAR_i + n_0MIG_i + \varepsilon_0 \qquad (1) \end{aligned}$$

From theory (Woodland, 1984) the expected coefficient signs are: $b_0, d_0, i_0, j_0 > 0$;

c_0, l_0, f_0, g_0, h_o, k_0, $m_0 < 0$; and l_0, $n_0 \gtrless 0$. The sign on l_0 is ambiguous since while husband's income might decrease participation, it is possible that a sorting process is going on such that high income men tend to marry work-oriented women (Apps, 1981). We also expect that as children's age increases women's labour force participation probabilities increase, so that $|g_0| > |h_0|$. The results are presented in Table 5.

TABLE 5

Probit Model of Labour Force Participation
(Dependent Variable equals 1 if in paid employment, 0 otherwise)

Variable	Model 1	Model 2	Model 3
LMX	0.154 (12.28)	0.155 (12.29)	0.149 (11.72)
LMX2	-0.0032 (8.51)	-0.0032 (8.53)	-0.0031 (8.25)
YOE	0.130 (9.03)	0.140 (9.06)	0.123 (7.80)
CHILD			-0.633 (5.86)
CHILD*		-0.490 (1.29)	
CHILD5	-1.137 (12.69)	-0.661 (1.75)	-0.759 (6.92)
CHILD15	-0.275 (3.91)	0.194 (0.52)	-0.046 (0.57)
AGE25	1.552 (14.04)	1.555 (14.06)	1.106 (8.23)
AGE35	0.063 (8.33)	0.636 (8.38)	0.379 (4.36)
OINC	-0.0004 (4.33)	-0.0004 (4.33)	-0.0004 (4.25)
ATHINC	0.0008 (4.44)	0.0008 (4.45)	0.0008 (4.74)
MAR	-0.204 (2.86)	-0.199 (2.79)	-0.167 (2.32)
MIG	0.0594 (0.70)	0.0602 (0.71)	0.0481 (0.56)
INTERCEPT	-3.0219 (13.30)	-3.0272 (13.32)	-2.263 (8.69)
Log-Likelihood Value	-1361.0	-1360.2	-1343.7

Note: Asymptotic t-statistics are shown in parentheses.

Three alternative models are reported in Table 5. These describe different treatments of the "ever-had-child" effect. All models include variables for number of children aged 0 to 4 and number of children aged 5-14. In model 1 this is the only adjustment for child bearing. Model 2 includes an additional dummy variable, CHILD*, to capture the effect of any child aged 0-14. Model 3 includes an alternative variable CHILD which is assumed to effect the workforce participation rate of any woman who has ever had a child, regardless of the child's present age.

The model 3 specification is strongly favoured by the data as indicated by the larger (less negative) log-likelihood ratio, which suggests that there are some effects of child-bearing which persist throughout the woman's life. A key factor involved in interpreting a 1986 cross-sectional model is the extent to which this type of effect lasts. While women now in their forties and fifties who have ever had children are less likely to be in paid employment, it remains to be seen whether the present younger generation of women will have similar mid-life and late-life workforce participation behaviour.

The results from Table 5 can be used to calculate the effect on the participation probability of changes in the explanatory variables These are derived from model 3 and presented in Table 6.

TABLE 6

Effects on Average Workforce Participation of Changes in Explanatory Variables

Variable Change	Change in the participation probability (all explanatory variables set at sample average)
One additional year of workforce experience	+3.7%
One additional year of schooling	+3.8%
One additional child less than 5 years of age	-28.6%
One additional child less than 15 years and more than 5 years of age	-1.5%
Being married or de facto	-5.3%
Being migrant from non-English speaking country	+1.5%
Additional $20 in weekly unearned personal income	-0.3%
Additional $20 in weekly after tax husband's wage income	+0.5%
Effect of ever having had at least one child	-23.3%

The results of most importance are these. For the women of the sample, of whom 49.2 per cent were in paid employment, the probability of labour force participation increased significantly, but at a decreasing rate, with labour market experience, each additional year implying about an extra 3.7 per cent probability of being in paid employment. More educated women were more likely to have market jobs, with each additional year of schooling increasing the labour force participation probability by about 3.8 per cent. Married and de facto women were less likely on average to participate, the difference between them and single, divorced, separated and widowed women being about 5.3 per cent.

Non-earned income decreases the probability of being in paid work, the coefficient implying that each additional 20 dollars per week diminishes labour market participation by 0.3 per cent. The sign on ATHINC indicates that the greater is the income of the husband, the more likely is the wife to have paid employment. Each additional 20 dollars of a husband's weekly income is associated with a 0.5 per cent increase in the wife's participation. This suggests that the sorting process alluded to earlier is swamping the pure income effects. As well, migrant women from non-English speaking backgrounds have similar participation rates to others.

Most importantly for the exercise at hand, the presence of children has a substantial negative effect on women's participation. Moreover, the younger are the children, and the more there are of them, the lower is the probability of female labour force involvement. The approximate effects are the following. Having ever had a child decreases the probability of being in paid work by 23.3 per cent. The presence of a child aged less than 15 and more than 5 years is associated with an additional 1.5 per cent diminution in participation. Most significantly, having a child aged less than 5 years lowers the participation probability by about a further 28.6 per cent. This means that ever having had a child, which is currently less than 5 years of age, lowers participation by over 50 per cent. The results imply that older children have a lower absolute effect on participation probabilities than infants and toddlers.

In general, the estimations of equation 1 are consistent with both expectations and findings from similar studies (Joshi, 1987; Calhoun and Espenshade, 1986). The strongest and clearest finding is the considerable effect on women's participation of the presence, age and number of children. These demographic factors apparently substantially outweigh the direct influence of measured economic variables.

To some readers tables of econometric results are not particularly meaningful. For this reason we show below, in graphical terms, what these relationships represent for several case studies of women in the sample. Imagine a woman first married at age 23 who has a child two years later. She can choose to have a second child at age 28, or to not have any further children. For the women choosing to have a second child at age 28, there is a further choice of whether or not to have a third child at age 31. These scenarios, of one, two or three children, compared to never having any children, are the subject of Figure 1. The three diagrams represent the distinctions in the scenarios for a women with high (16 years), average (12 years) and low (10 years) of education.

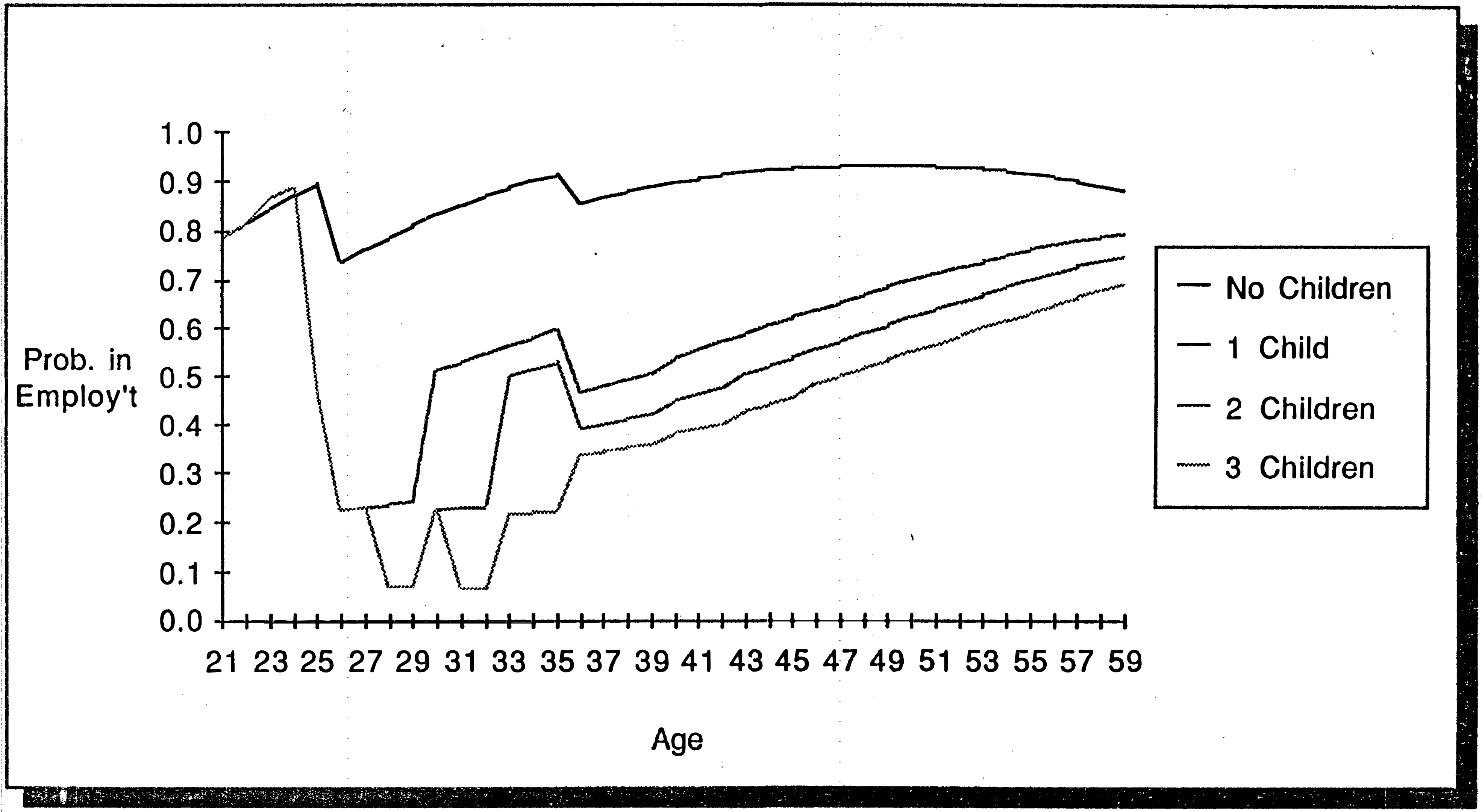


Figure 1(a): Labour Force Participation Profile for Woman With 16 Years Schooling

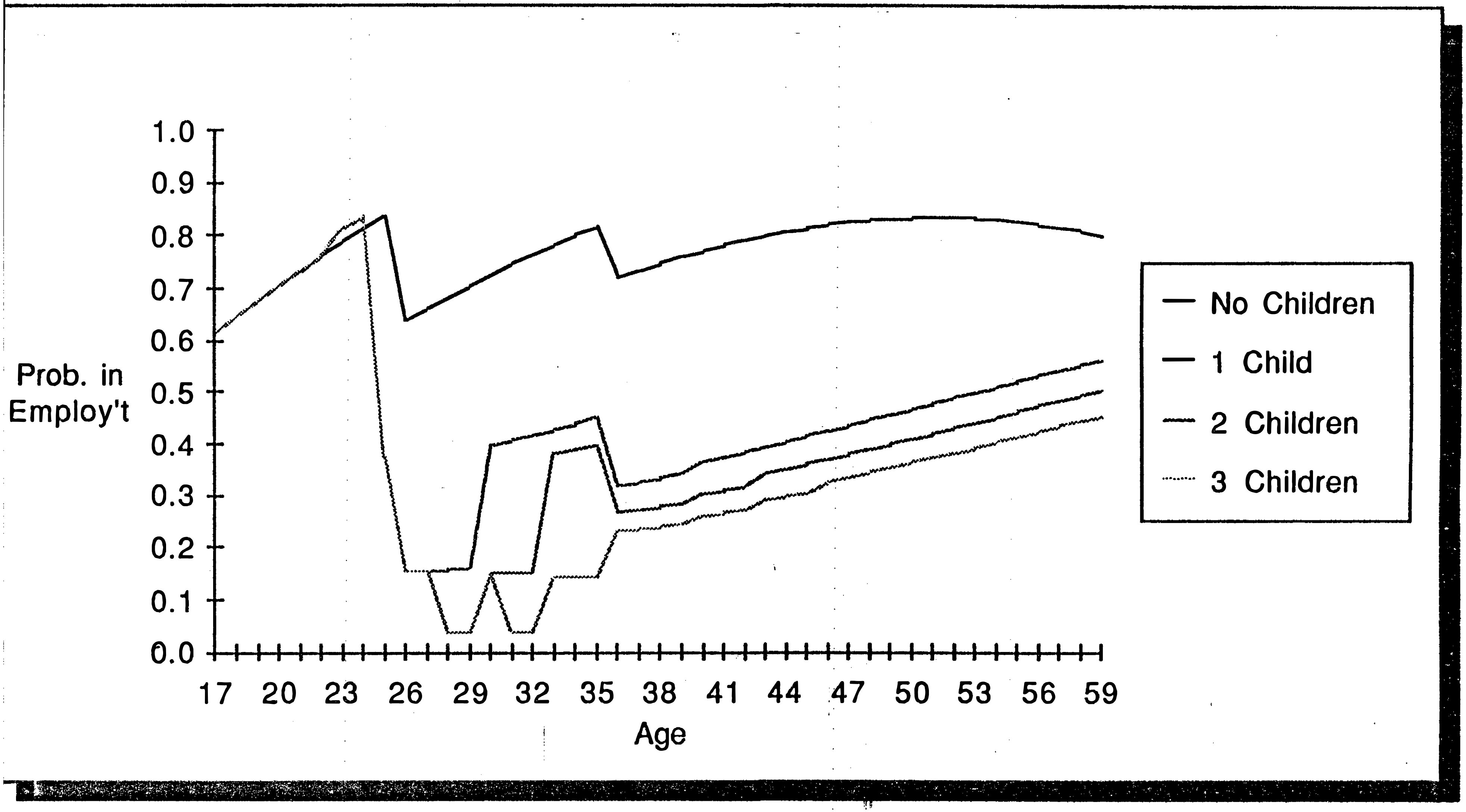


Figure 1(b): Labour Force Participation Profile for Woman With 12 Years Schooling

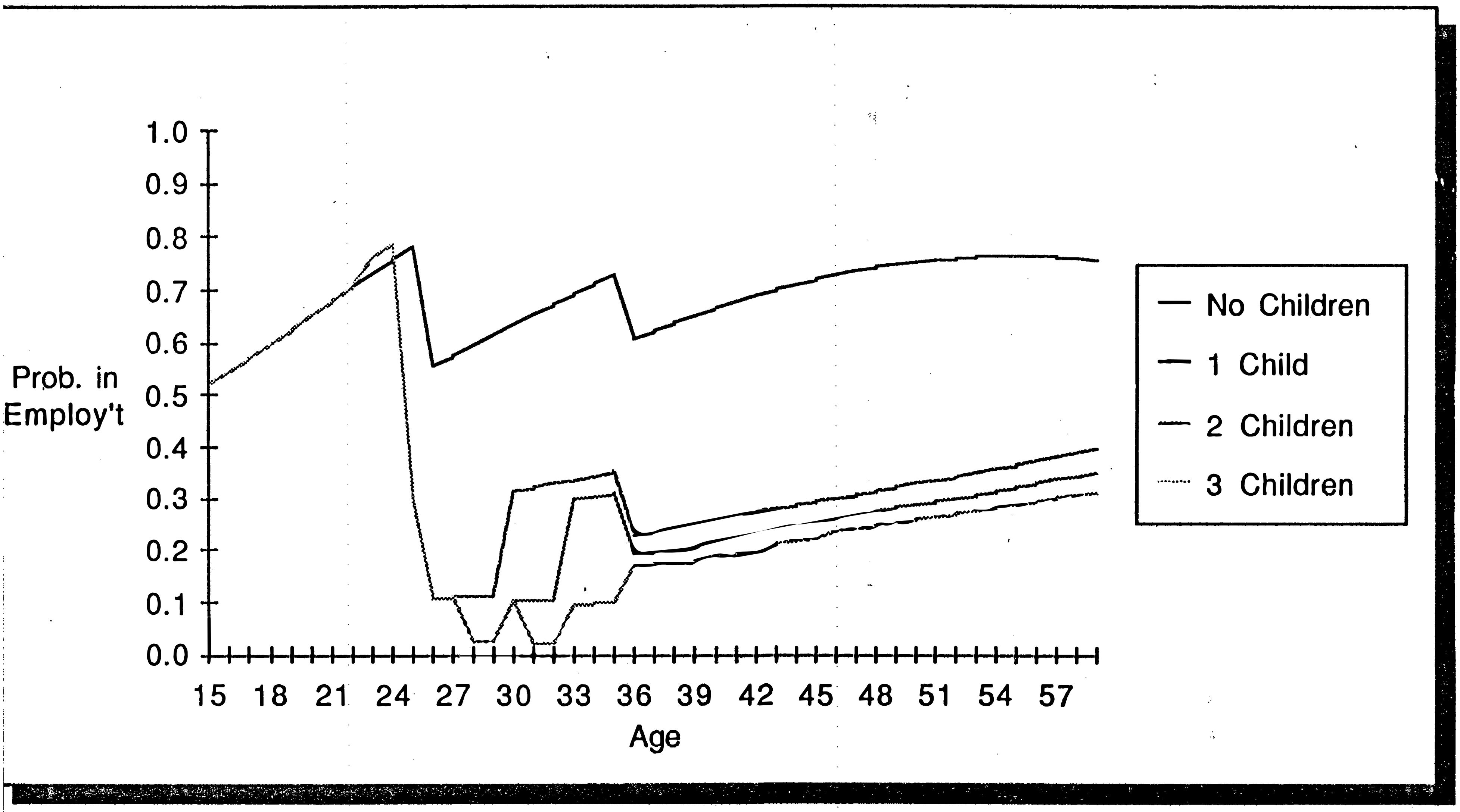


Figure 1(c): Labour Force Participation Profile for Woman With 10 Years Schooling

The essential point illustrated in Figure 1 is that having a child substantially reduces women's labour force participation, by as much as 60 percentage points before the age of 30. Each additional child reduces participation further, but to a much lesser extent. The clear result is that a major consequence of the effect of children on forgone earnings is through a considerable diminution of labour force participation rates.

III(ii) Modelling Hours Worked

The next part of the exercise concerns the estimation of the determinants of the average weekly hours worked (AWH_i) for employed women (Cain and Dooley, 1976). A linear regression model was employed with the following functional form:

$$\begin{aligned} AWH_i = {} & a_1 + b_iW_i + c_1W2_i + d_1CHILD5_i + e_1CHILD15i + f_1MAR_i \\ & + g_1MIG_i + h_1OINC_i + i_1ATHINC_i + j_1LMX_i + k_1LMX2_i \\ & + l_1YOE_i + m_1AGE25_i + n_1AGE35_i + p_1LAM_i + \varepsilon_1 \qquad (2) \end{aligned}$$

where LAM is the inverse of the Mill's ratio, computed from the probit model of participation 3 of Table 5. Its presence is designed to correct for the selection bias inherent in the strong likelihood that working women differ in an important but unobserved way from non-working women.[1]

From theory the expected coefficient signs are:

$d_1, e_1, f_1, h_1\ i_1 < 0$;

$b_1, c_1, g_1, j_1, k_1, l_1, m_1, n_1 \lesseqgtr 0$;

and $|d_1| > |e_1|$.

This last prediction suggests that the absolute effect of children on hours worked is greater the more young children are present. From theory it is not possible to sign the coefficients related to the wife's wage or wage determinants, the reason being that the effect on hours worked of changes in these variables depends on whether the substitution effect outweighs the income effect. Put simply, higher hourly wages could increase working time because the opportunity cost of not working is greater, or higher hourly wages could decrease working time because a given target income can be achieved with fewer working hours. Economic theory predicts that the pure substitution effect of an increase in wages should be positive. The models reported below satisfy this condition for all women in the sample[2] . The results of the estimation are presented in Table 7.

Two types of model are presented. Models 1 and 2 include the major demographic variables plus wage rates. Model 3 is a reduced form of the model where variables such as labour market experience and education are included to account for the wages the woman is likely to earn.

The large t-statistic on the inverse Mill's ratio variable, LAM, tells us that there is severe correlation between the random factors which determine whether a woman is in the workforce or not, and those which determine how many hours she works when in the work force. We are led to the conclusion that model 1 (and the models like it in the literature which do not account for this issue) are severely mis-specified.

The subsequent analysis in the paper is based on model 2. This model was chosen because of its good statistical behaviour and its close proximity to the model suggested by economic theory.

TABLE 7

Regression Model of Average Weekly Hours Worked Given Woman is in Employment
(Dependent variable is average weekly hours worked)

Variable	Model 1	Model 2	Model 3
W	0.228	0.151	
	(2.73)	(1.86)	
W2	-0.0024	-0.0018	
	(4.26)	(3.10)	
CHILD5	-7.099	-2.282	-1.277
	(5.45)	(1.50)	(0.55)
CHILD15	-6.410	-3.767	-3.0599
	(6.85)	(3.64)	(2.82)
MAR	-1.932	-0.410	0.0992
	(2.21)	(0.46)	(0.11)
MIG	1.524	1.828	
	(1.46)	(1.77)	
OINC	-0.0012	0.0006	0.0007
	(0.58)	(0.32)	(0.34)
ATHINC	-0.0042	-0.0068	-0.0068
	(1.73)	(2.81)	(2.59)
LMX			-0.0041
			(1.41)
LMX2			0.0182
			(2.71)
YOE			-0.469
			(1.60)
AGE25			-0.318
			(0.11)
AGE35			1.419
			(0.94)
LAM		7.596	9.232
		(7.43)	(3.94)
INTERCEPT	33.770	25.050	30.95
	(35.6)	(16.1)	(7.41)
$\bar{R}^2$	0.10	0.15	0.16

* White's heteroskedastic consistent absolute t-statistics are shown in parentheses.

The results from Table 7 may be used to illustrate coefficient sizes. These are presented in Table 8, which shows the increase in a woman's average hours worked for given changes in explanatory variables calculated from model 2.

TABLE 8

Effect on Average Hours Worked Per Week (given that a woman is in the work force) of Changes in Underlying Explanatory Variables

Variable Change	Change in average hours worked per week (all explanatory variables set at sample averages)
One additional year of labour market experience	+0.51
One additional year of schooling	+0.72
Being married or de facto	-1.39
Being a migrant from a non-English speaking country	+2.11
Additional $20 in weekly unearned personal income	-0.03
Additional $20 in weekly after tax husband's wage income	-0.04
Effect of ever having had at least one child	-3.48
Effect of a 10 per cent increase in gross wage rate	0.12
One additional child aged less than 5 years	-6.34
One additional child aged less than 15 years and more than 5 years of age	-4.03

The results of Table 8 imply the following. Additional years of labour market experience and schooling add only about half an hour a week to labour market hours. Married and de facto women in employment work about 1.4 fewer hours a week, and migrants from non-English speaking countries work more than two extra hours a week. Changes in unearned income, after tax husband's income and the hourly wage rate are virtually irrelevant to women's weekly hours of work.

Most importantly, the working hours of wage earning women are affected by the presence, age and number of children. Having ever had a child reduces working hours by 3.5 per week and if the child is under the age of 5 there is a reduction in hours of a further 6.3 per week, that is, a total of 10. If the child is between the ages of 5 and 15 the total reduction in hours is about 7.5 per week. As is the case for labour force participation probabilities, the most important factor reducing hours is children. However, their effects are considerably lower than they are for participation.

The same simulation exercise adopted for the graphical representation of the effect on children on labour force participation can now be used in illustrations of the effect of different fertility patterns on hours reduction for wage earning women. These are presented in Figure 2.

The diagrams highlight the major relationships between women's education and age, and the presence, number and age of children. They demonstrate the clear and significant diminution in hours worked given additional children at the ages of 25, 28 and 31. As with participation the most obvious impact is from the birth of the first child.

III(iii) Modelling the Hourly Wage Rate

The final econometric exercise concerns the estimation of the determinants of the hourly wage rate (Mincer, 1974). A log-linear wage model was employed with the following function form:

$$\begin{aligned} LW_i = {} & a_2 + b_2LMX_i + c_2LMX_i + d_2YOE_i + e_2TEN_i + f_2MAR_i \\ & + g_2MIG_i + h_2LAM_i + \varepsilon_2 \end{aligned} \qquad (3)$$

From theory (Becker, 1975), the expected coefficient signs are:

$b_2, d_2, e_2 > 0$; $c_2, f_2, g_2 < 0$.

These predictions mean that the wage is expected to increase with schooling, tenure and labour market experience, but at a decreasing rate for the last of these. Married women are expected to earn lower wages because of family responsibilities, or perhaps through lower investments in on-the-job training, and migrant women from non-English speaking backgrounds may have lower job skills because of language difficulties. The results are presented in Table 9, the signs and magnitudes of the coefficients being as anticipated.

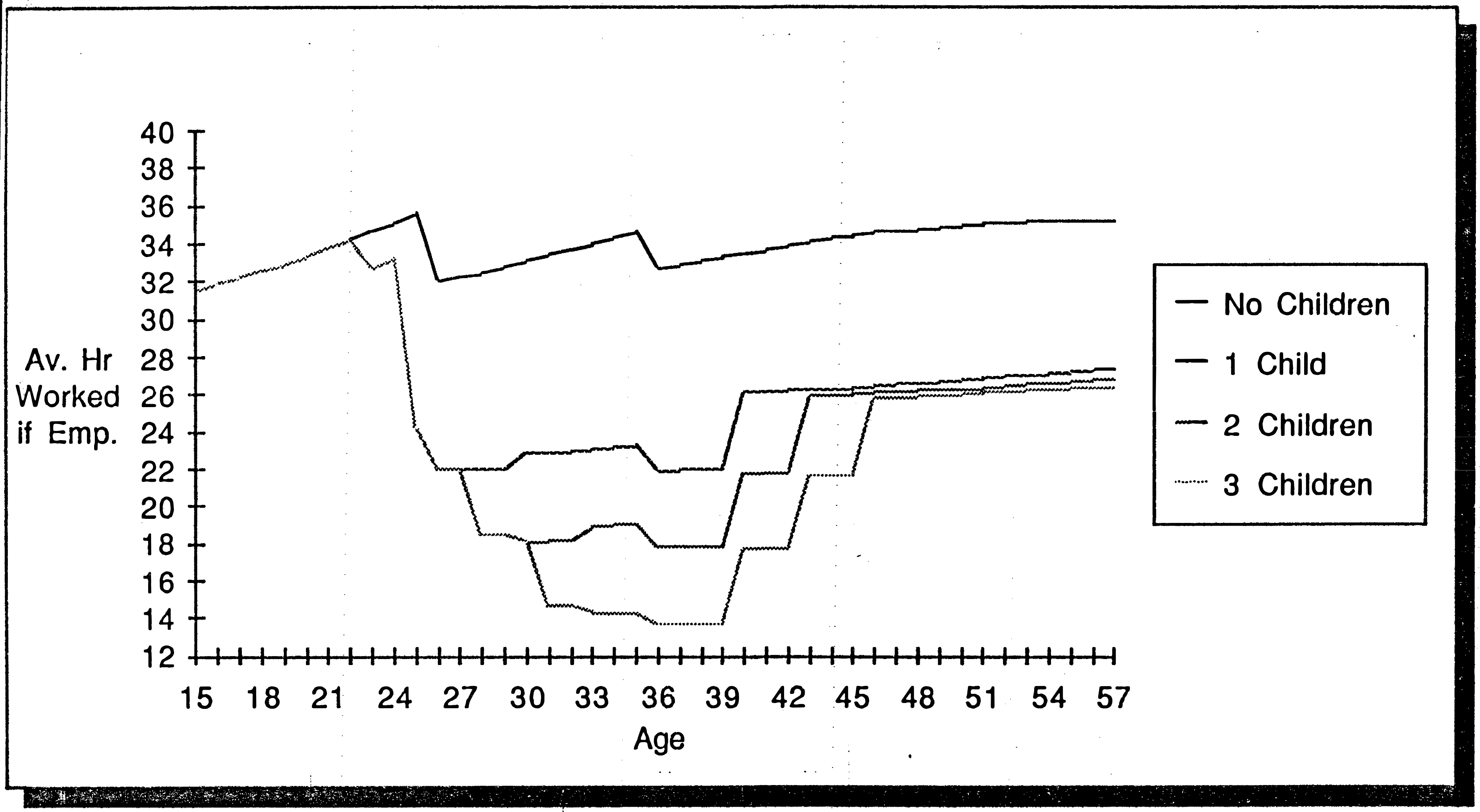
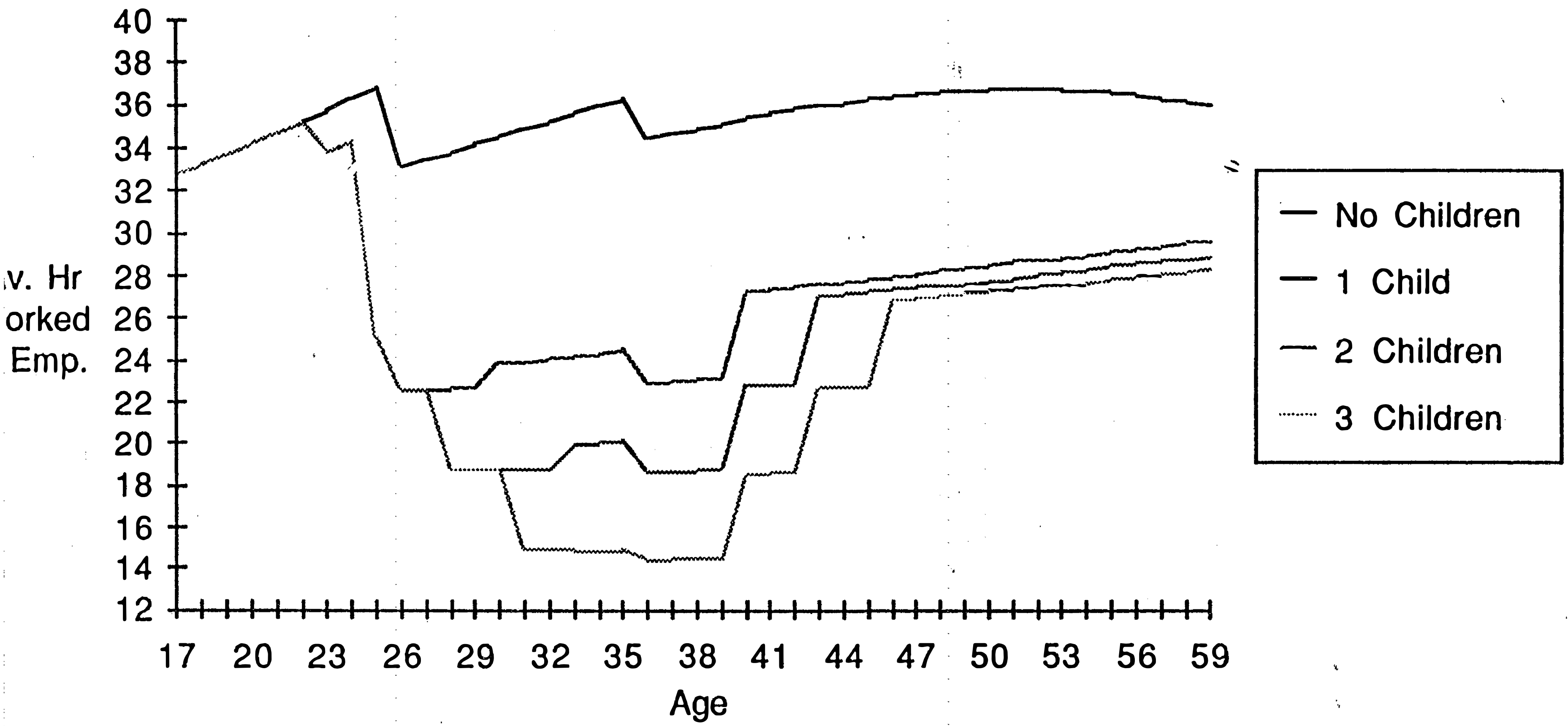


Figure 2(a): Average Hours Worked Profile for Woman in the Labour Force With 16 Years Schooling



gure 2(b): Average Hours Worked Profile for Woman in the Labour Force With 12 Years Schooling.

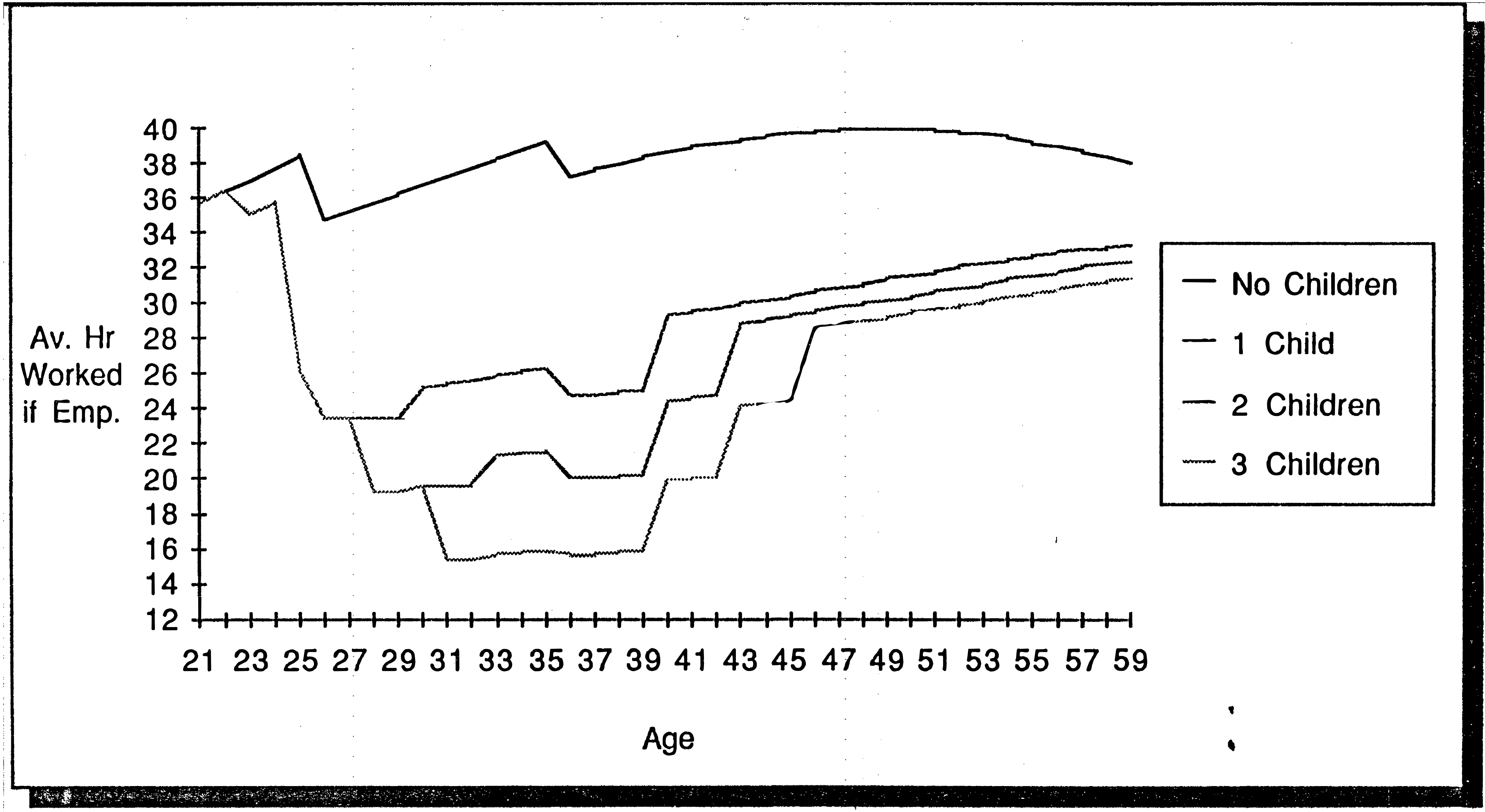


Figure 2(c): Average Hours Worked Profile for Woman in the Labour Force With 10 Years Schooling

The results of Table 9 may be used to illustrate the effects on the hourly wage rate of changes in the independent variables. They are presented in Table 10, calculated from model 2.

TABLE 9

Regression Model of Hourly Wage Rate
(Dependent Variable is the Logarithm of the Before Tax Hourly Wage Rate)

Variable	Model 1	Model 2
LMX	0.0287 (4.45)	0.0284 (4.42)
LMX2	-0.00077 (3.96)	-0.00077 (3.93)
YOE	0.07766 (8.23)	0.0764 (7.34)
TEN	0.0078 (2.31)	0.0078 (2.30)
MAR	-0.0159 (0.51)	-0.0131 (0.42)
MIG	-0.057 (1.11)	-0.0573 (1.11)
LAM		0.0144 (0.36)
Intercept	1.0008 (7.46)	1.0096 (7.47)
$\bar{R}^2$	0.086	0.087

Note: White's heteroskedasticity - corrected t-statistics are shown in parentheses.

TABLE 10

Effect on Average Hourly Gross Wage Rates of Changes in Underlying Explanatory Variables

Variable Change	Change in hourly wage rate (all explanatory variables set at sample averages) \$A,1986
One additional year of workforce experience	0.13
One additional year of schooling	0.80
One additional year of tenure with current employer	0.06
Being married	-0.16
Being a migrant from a non-English speaking country	0.56

The results of Tables 9 and 10 are consistent with both expectations from theory and a myriad of similar empirical work (Beggs and Chapman, 1988). In percentage terms, additional years of labour market experience and schooling are associated respectively with increases in the hourly wage rate of about 2 and 8 per cent. Marital and migrant status are apparently of little importance in wage determination.

The same simulation method used in the construction of Figures 1 and 2 may be employed to investigate the effect of children on women's wage rates. The method is more complicated in this case, because children are not part of the model and have an indirect effect only on the wage rate. This effect operates through labour market experience which accumulates less quickly for married women and, particularly, with the presence of children. That is, since the birth of a child substantially reduces labour force participation and somewhat reduces weekly hours worked for those remaining in the labour force, one year increases in age for mothers are associated with considerably lower increases in total labour market experience than is the case for childless women. Thus as the hypothetical woman used in the simulation has more children, her wage rate relatively decreases with age because she has fewer years of direct labour market experience than would otherwise be the case, the consequence being a slower increase in wages. These points are clarified through consideration of Figure 3.

TABLE 10
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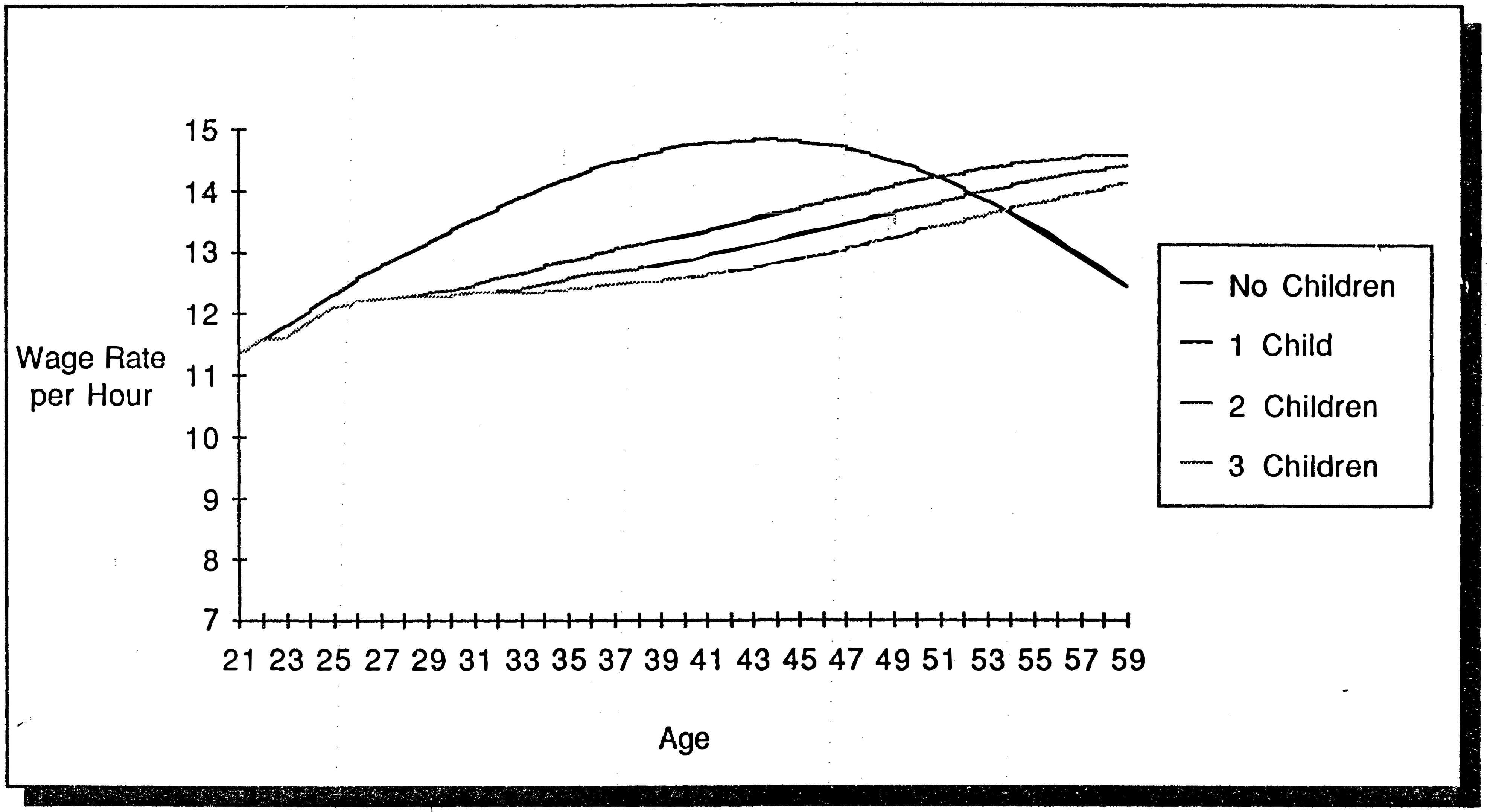

Figure 3(a): Wage Rate Profile for Woman with 16 Years Schooling

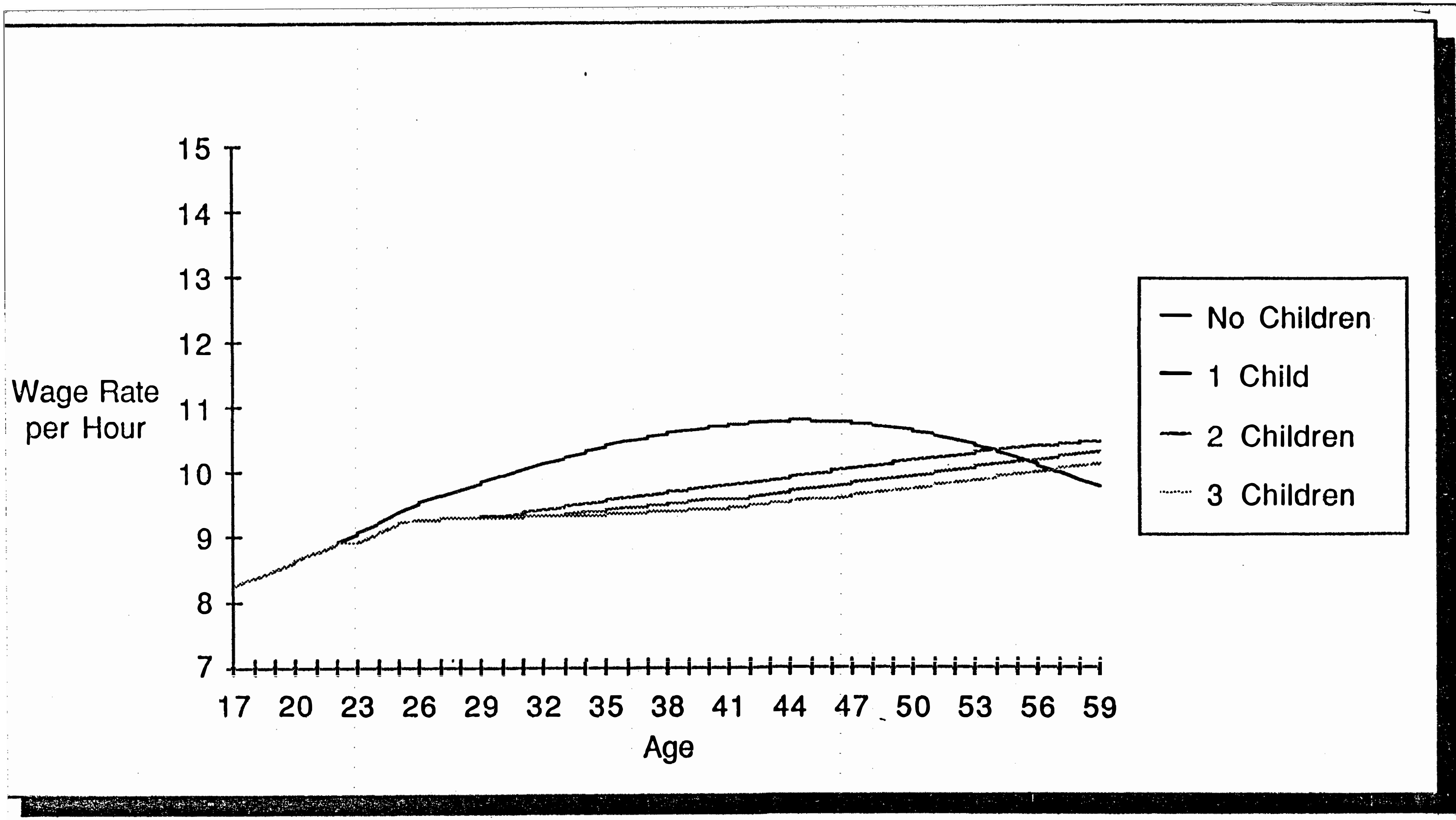

Figure 3(b): Wage Rate Profile for Woman with 12 Years Schooling

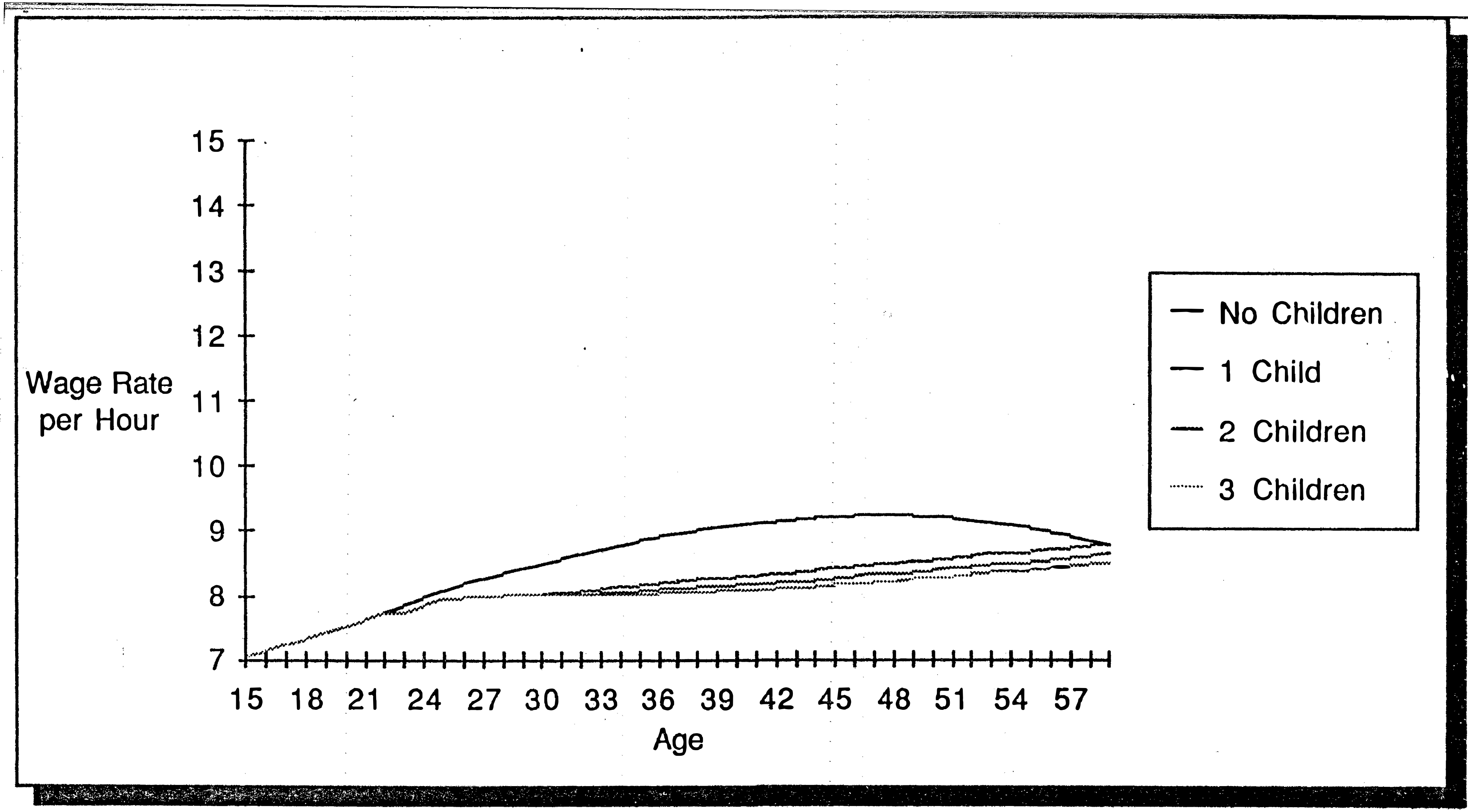

Figure 3(c): Wage Rate Profile for Woman with 10 Years Schooling

The diagrams illustrate that additional children diminish women's hourly wage rates, particularly for the years immediately following births. The effect of children on wages is much less than is the case for both labour force participation and hours worked. The greatest difference in wage rates between a childless women and a mother of 3, at about the age of 40, is somewhat less than 20 per cent.

A difficulty for the wage analysis is apparent from Figure 3. This concerns the rapidity of the wage decrease predicted for childless women after about the age of 50. Cross-sectional studies always show a decrease in wage rates at older ages, but the extent of the decline in this instance is mildly disquietening[3]. It could be due to the possibility that older childless women increasingly prefer less demanding jobs as they age due in part to this group of women having considerable savings, or could be a consequence of the functional form being increasingly inappropriate at the extremes of the data. The important point to note is that the hourly wage rate similarities of all women imply that the overall empirical significance of this anomaly for the predictions of earnings is negligible.

Overall the econometric results are highly satisfactory in terms of their consistency with theory, coefficient stability and plausibility. The most important general finding is that the presence, age and number of children are fundamental and substantial determinants of labour market outcomes. The effect of child birth on labour force participation in particular, and to a lesser extent, hours worked if currently participating, is dramatic. In these respects children are much more influential than economic variables, such as labour market experience, education, unearned income or after tax husband's income.

The method employed has allowed a decomposition of the role of child-rearing on women's involvement and remuneration in the work force. It is desirable to now piece together the various components to address the issue of the overall effect of child-rearing on women's earnings. To put the matter differently, how much annual and lifetime income is forgone by mothers through the interaction of the presence of children on labour force participation, hours worked and hourly wage rates? The answer is in section IV.

IV Using the Econometric Results to Estimate Forgone Earnings From Child-Rearing

Women's forgone earnings from child-rearing are given by the number of hours spent outside the labour market valued at the hourly wage they would have received given greater participation. The econometric results reported above reveal that the number, presence and age of children have considerable consequences for the hours women work in the labour market, most importantly because of the effects of children on female labour force participation.

The econometric models reveal that many of the measurable differences between women affect their hours of work and hourly wage rates. In particular, labour market experience, years of education, age and family income levels are significant explanators. Through control of these factors the independent influence of children on labour market outcomes has been uncovered in statistically appealing ways. Our purpose in this section is to illustrate the overall extent of forgone earnings from child-rearing, and to examine how these estimates vary between women characterised by different socio-economic backgrounds and for particular fertility patterns.

The labour force participation probabilities, hours worked if employed and wage rate estimations, in combination, can be used to illustrate the effect of children on women's earnings. This is illustrated in Figure 4 for the hypothetical women considered in section II. Table 11 reports the overall life-time affects on earnings for the cases of no, one, two and three children for women at three education levels. The tables describe income earned from the time of entry to the labour force to age 60. Three scenarios are reported showing the effects of different assumptions about the real rate of interest at which earnings could be re-invested. We show results for 0%, 5% and a 7% interest rate.

The earnings are gross before tax wage earnings. Since single childless women have higher earnings and no tax allowances for dependent children they can be expected to experience higher marginal taxation rates. The after tax forgone earnings cost of children are thus smaller than those implied in Table 11. But importantly, from a macroeconomic perspective, Table 11 measures the gross societal loss of earnings associated with child rearing.

Table 12 shows the life time earnings of married child-bearing women relative to other women, derived directly from Table 11. The extent of the earnings loss depends on all the major elements of the analysis, yet there is a striking pattern in the table. It is clear that the major loss of earnings is associated with the birth of the first child, which in round figures causes the loss of about one-half of life-time wage earnings. Subsequent children have a smaller effect, each generally costing between 5 and 10 per cent of life-time potential earnings. We interpret this as strong (cross-sectional) evidence of economies of scale in child rearing with respect to labour market earnings[4].

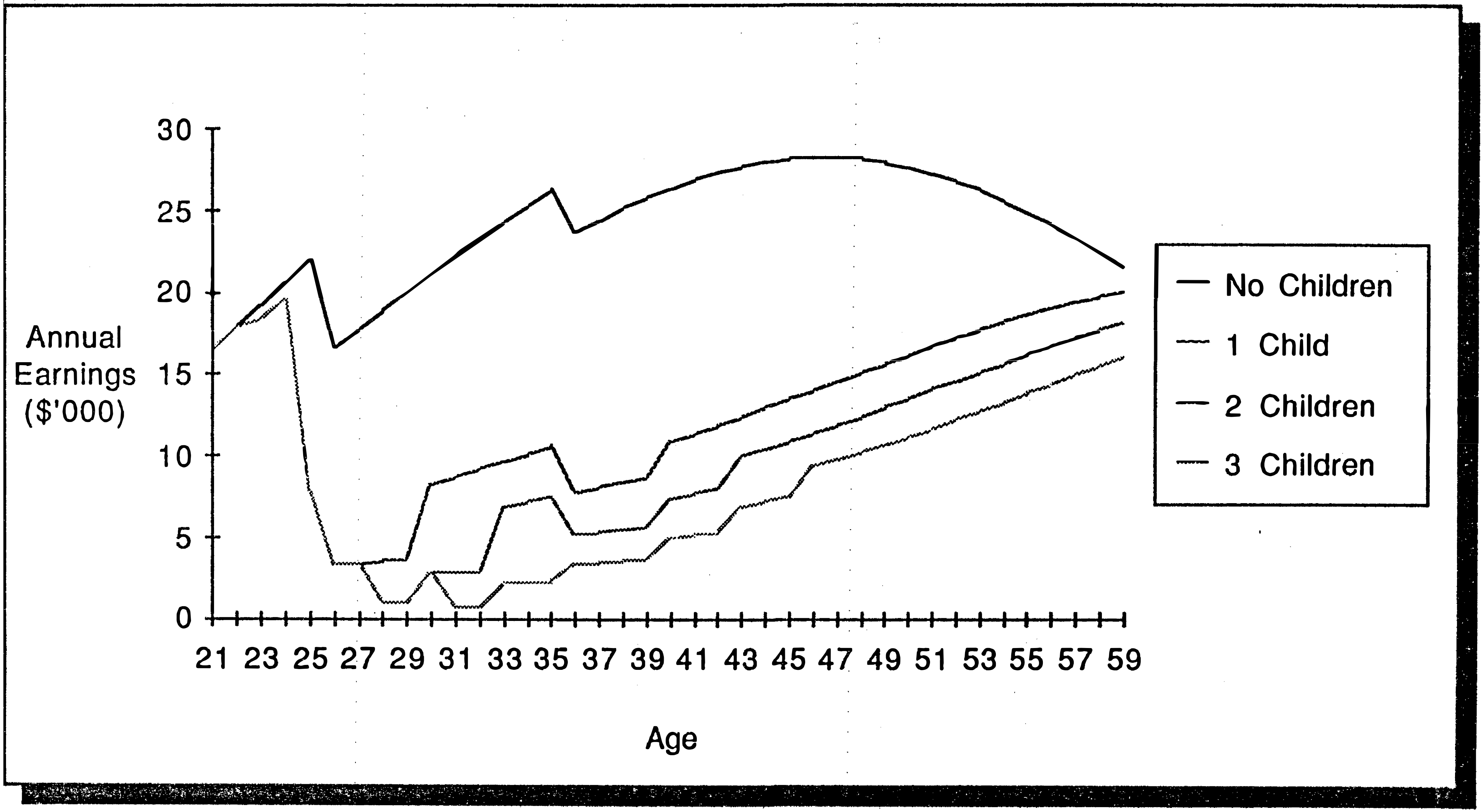

Figure 4(a): Earnings Profile for Woman with 16 Years Schooling

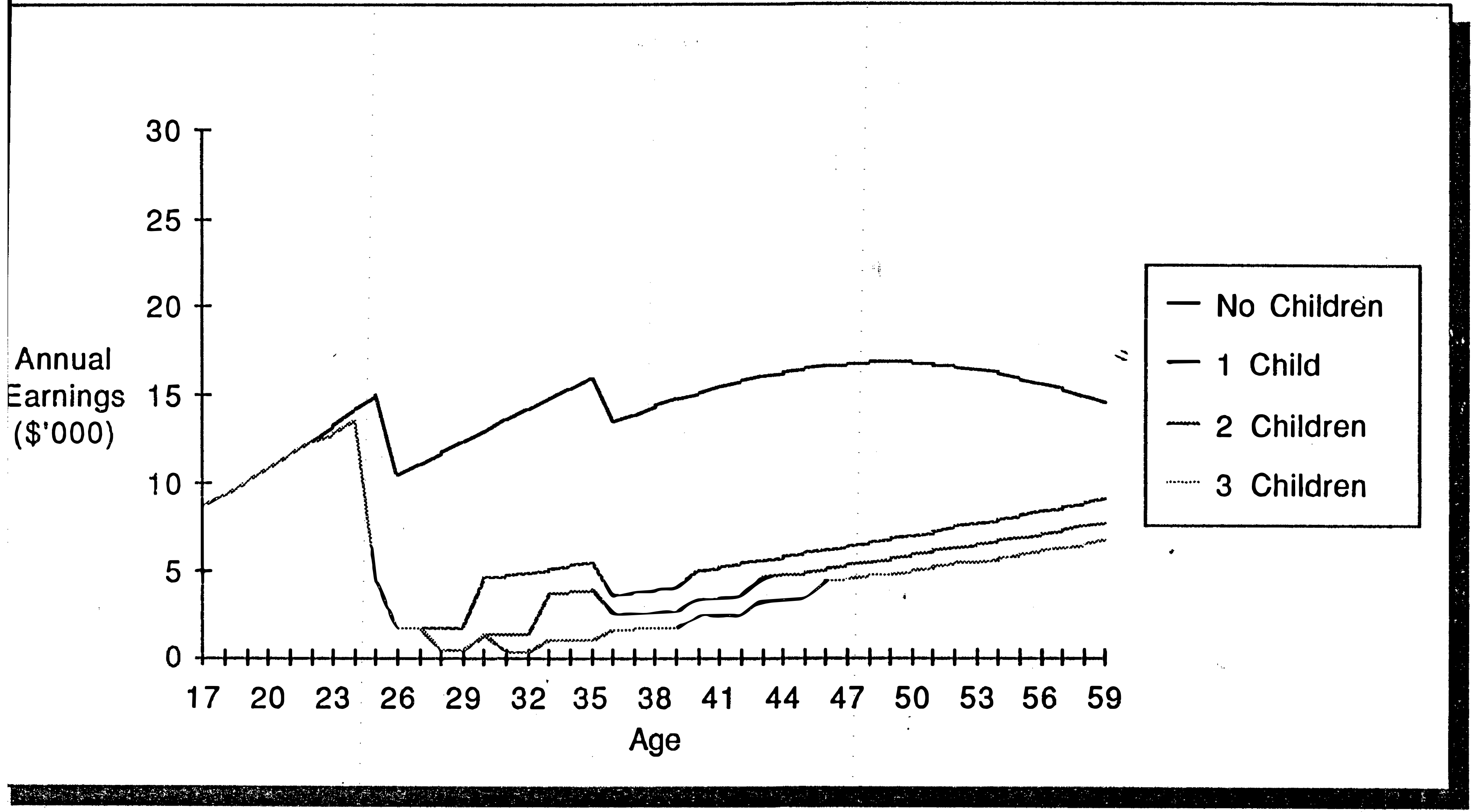

Figure 4(b): Earnings Profile for Woman with 12 Years Schooling

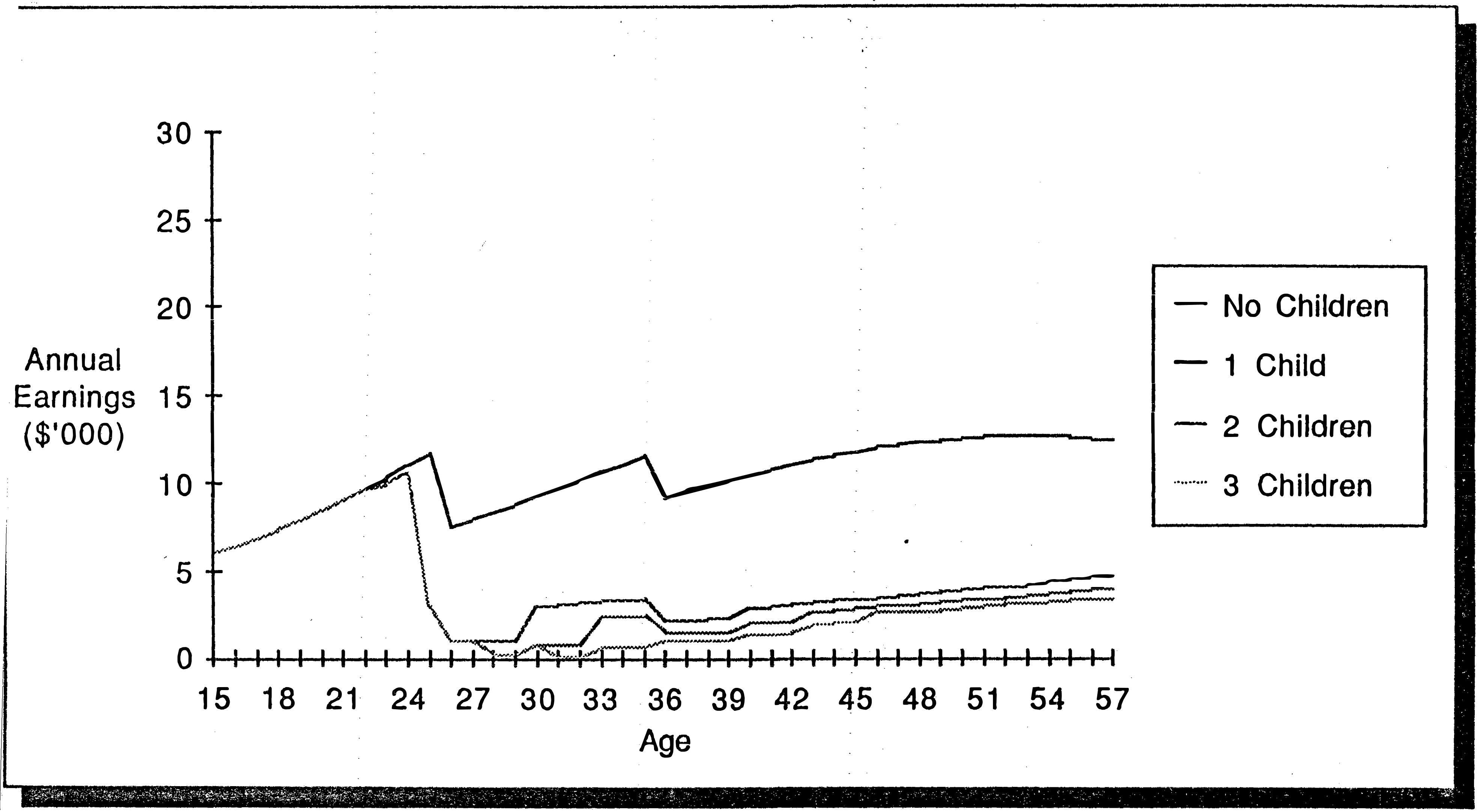

Figure 4(c): Earnings Profile for Woman with 10 Years Schooling

TABLE 11

Total Lifetime Earnings at Age 60 From Different Education and Child Bearing Scenarios
($000)

	Investment Rate of Interest		
	0%	5%	7%
Number of Children		High Education	
0	959 (959)	2733 (388)	4403 (294)
1	520 (520)	1417 (201)	2303 (154)
2	422 (422)	1157 (164	1904 (127)
3	344 (344)	957 (136)	1641 (110)
		Average Education	
0	630 (630)	1993 (283)	3399 (227)
1	294 (294)	1064 (151)	1944 (130)
2	246 (246)	934 (133)	1743 (116)
3	211 (211)	848 (120)	1617 (108)
		Low Education	
0	483 (483)	1597 (227)	2800 (187)
1	201 (201)	859 (122)	1659 (111)
2	173 (173)	780 (111)	1536 (103)
3	153 (153)	729 (103)	1460 (98)

Note: The numbers in parentheses show the present value at age 20 of each life-time earnings stream at the assumed rate of interest.

TABLE 12
Comparisons of Total Lifetime Earinings at Age 60 From Different Education and Child Bearing Scenarios

	Investment Rate of interest		
	0%	5%	7%
Number of Children		High Education	
0	1.00	1.00	1.00
1	0.54	0.52	0.52
2	0.44	0.42	0.43
3	0.36	0.35	0.37
		Average Education	
0	1.00	1.00	1.00
1	0.47	0.53	0.57
2	0.39	0.47	0.51
3	0.33	0.42	0.47
		Low Education	
0	1.00	1.00	1.00
1	0.42	0.54	0.59
2	0.36	0.49	0.55
3	0.32	0.45	0.52

The data may be expressed as dollar amounts of lifetime forgone earnings associated with the presence of children. These are shown in Table 13 for various scenarios.

Some summary statistics from Table 13 are useful to reinforce the major points. For a woman with average education, having one child is associated with a lifetime income loss of about \$336,000, assuming there is no capacity to receive interest on the sum. Second and third children are associated with about a further \$50,000 and \$35,000 income loss.

The most substantial income loss is for highly educated women with access to a high interest rate capital market. The first child costs over two million dollars, the second an additional \$400,000, and the third an extra quarter of a million dollars.

Some (limited) comparisons may be made with similar overseas studies. Joshi (1987) found from a 1980 cross-sectional study of British woman that a, typically, having two children was associated with an income loss of £122,000. This translates into about \$370,000 in 1986, a remarkably similar figure to the \$384,000 loss for an average Australian woman with two children.

Calhoun and Espenshade (1986), using panel data, found a considerably lower opportunity cost for comparable child-bearing of around \$US43,000 in 1981, which translates to something around \$A75,000 in 1986. They argue that cross-section estimates systematically over-estimate the forgone earnings of child-rearing, a point that needs qualification with respect to our technique. If it is true that young Australian women are more likely to participate than were their mature counterparts, our estimates are too high for the former group. On the other hand, the estimates of forgone earnings for older women are too <u>low</u>, since they would have participated less than the young women of the sample. The estimates should be taken as approximate, there being no way to correct for these biases.

TABLE 13

Forgone Total Earnings at Age 60 From Different Education and Child Bearing Scenarios ($000)

	Investment Rate of Interest		
	0%	5%	7%
Number of Children		High Education	
0	0	0	0
1	439	1316	2100
2	537	1576	2499
3	615	1776	2762
		Average Education	
0	0	0	0
1	336	929	1455
2	384	1059	1656
3	419	1145	1782
		Low Education	
0	0	0	0
1	282	738	1141
2	310	817	1264
3	330	868	1340

V Conclusion

Through cross-sectional analysis of the determinants of labour force participation, hours worked if employed and wage rates, it has been possible to estimate the forgone earnings associated with children, an exercise not previously attempted in Australia. The striking and clear conclusion is that children have a considerable effect on women's earnings. A rough overall figure is about $(1986)300,000 for the first child, with second and third children being associated with around $50,000 and $35,000 respectively, assuming there is no access to capital markets to invest earnings. Women with the capacity to invest earnings at an interest rate of 7 per cent have overall forgone lifetime income from children of one to 2.5 million dollars.

Importantly, the major effect of the process operates through diminished labour force participation rates, which fall by about 40 percentage points after child-birth. For employed women the presence of children reduces hours worked by around 10 per week, but the consequences of children on women's hourly wage rates are neglible.

It is very obvious that the big decision in terms of forgone earnings is whether or not to have a first child. Second and subsequent children matter, but the consequences are relatively slight. Also, in absolute dollar terms, forgone income is greatest for highly educated women, but in a relative sense the effects of education are not great.

It should be emphasised that the cross-sectional estimates have several problems, it following that our results should not be taken too literally. Nevertheless, the equations are statistically appealing in terms of their consistency with theory and coefficient significance. This adds weight to the view that we have uncovered rough orders of magnitude of the forgone income implications of the presence of children, which can be described accurately as substantial. By itself this finding does not imply a particular policy prescription, but is certainly useful information in the social policy arena, such as for those questions related to population growth and the provision of subsidised child-care.

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1 The procedure for this correction is described fully in Heckman (1979).

2 The models reported here are estimated using before tax wage rates and before tax unearned income. From a pure theory perspective, after tax values are preferred. This transformation of the data requires computing so-called "virtual income" to account for the progressivity in the income tax schedule. Models were estimated using this methodology and were found to be unreliable. In part the problem is purely statistical in that both the after tax wage rate and the "virtual income" are endogenous to the decision on hours worked. We suspect that the resulting simultaneous equation bias is the cause of the failure of this type of model.

3 The actual aggregate data support the simulation results in Figure 3, i.e., that childless women in late life have lower wage rates. The average hourly wage of women in the sample over the age of 50 who have had a child is \$9.07, on a sample of 94 observations. Comparable childless women have an hourly wage rate of \$8.70, but this is on a sample of only 10 observations.

4 This finding is consistent with that found by Ross (1986).

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