

The Employment Effect of Intensive Support

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Abstract

Intensive Support is the label for the major form of assistance provided to jobseekers by private providers under contract to the Commonwealth Government (The Job Network). This paper analyses the effect of this assistance on employment outcomes using data from the Household, Income and Labour Dynamics in Australia (HILDA). Two common approaches to estimating a treatment effect are used, matching and hazard function. Using the matching approach the focus is on the probability of employment at some fixed time after treatment. Hazard function methods, on the other hand, focus on the timing of the outflow to employment and impose greater structure on the underlying process. The results suggest that Intensive Support has no effect on employment outcomes, but the estimates are imprecise.

JEL Classification: J680; J640; C140

1. Introduction

The Job Network is the term given to the managed market for the provision of employment services to Australian job seekers. When introduced in 1998 it was seen as a radical departure from the usual model of a dominant public provider and many countries have followed suit. But while the Australian Job Network method of contracting out employment services has received considerable international interest, it has been a closed book to independent researchers. Apart from an early review of the operation of the Job Network (Productivity Commission, 2002) only one study (of the Work for the Dole) has been undertaken by independent researchers (Borland and Tseng, 2004). This is in sharp contrast to the situation in most other developed countries. During the past decade there have been significant developments in the methodology of program evaluation and a large number of policy relevant studies have been undertaken (see Caliendo and Hujer (2005) for an overview). Regrettably, Australian researchers have not been able to contribute to these international

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Acknowledgement: This paper uses unit record data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. The HILDA project was initiated and is funded by the Australian Government Department of Families, Housing, Community Services and Indigenous Affairs (FaHCSIA) and is managed by the Melbourne Institute of Applied Economic and Social Research (MIAESR). The findings and views reported in this paper, however, are those of the author and should not be attributed to either FaHCSIA or the MIAESR.

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developments. The scant publicly available data provide only the most rudimentary information about the Job Network and has nothing to say about the causal effect of the assistance provided.

This lack of data is reflected in the public discourse about the Job Network. While a large number of papers have commented on the operation of the Job Network, most of the discourse consists of views and opinions. A recent discussion paper that outlined the Government's view on the post-2009 direction of the Job Network makes no reference to this literature. Instead, the paper relied on an OECD (2001) review of 'the Australian way' to underpin its arguments.

The aim of this paper is to fill some of the gap in our knowledge about the Job Network. It estimates the effect of Intensive Support, the major form of assistance given by Job Network providers, on labour market outcomes. Since administrative data is not available to researchers the analysis is based on data from the first six waves of the Household, Income and Labour Dynamics in Australia survey (HILDA). While this is a rich data set, it contains only one piece of information about Intensive Support – whether a person looking for work was receiving this support at the time of an interview. The more crucial information, when the support started, is not known. This clearly limits the type of analysis that can be undertaken and the confidence in the results obtained. It can still be argued, however, that an analysis of this data has something to say about the effect of the Job Network assistance and that it is important to do so for two reasons; the reintegration of the unemployed is a critical social and economic issue and little is known about how well the Job Network accomplishes this task.

2. The Job Network and Intensive Support¹

Intensive Support is the major form of employment services by Job Network members. Persons who claim unemployment related income support are referred to a Job Network provider when they register with Centrelink. Initially the provider's assistance is limited to job search support, including matching services and access to facilities to support their own job search. If a jobseeker has not found a job within three months the assistance is upgraded to Intensive Support. Those identified as being highly disadvantaged are referred directly to Intensive Support at registration. The first element of Intensive Support is a three week job search training course that seeks to improve job search skills and expand the jobseekers knowledge of job opportunities. Beyond that, providers have a great deal of flexibility in the type of assistance they provide including access to education and training, subsidised employment, public sector employment programs, vocational guidance, counselling and referrals to professional assistance with health, family and housing issues. They also have a compliance role. Thus persons who remain unemployed for another three months become subject to a mutual obligation requirements which may include a spell on the Work for the Dole program (a public employment program). Those that still remain unemployed after another six months then receive what was once called

¹ Prior to 2003 the term Intensive Support was called Intensive Assistance. In 2009 the name of the Job Network was changed to Job Services Australia (JSA). Instead of jobseekers qualifying for more intensive support the longer they remain unemployed, in JSA they are now assigned to one of four levels (streams) of assistance at referral.

'customised assistance', individual assistance to overcome barriers to employment and active support in their job search. This assistance can include expenditure on training and job search as well as subsidised employment. In total, a jobseeker can be in receipt of Intensive Support for a period of up to 18 months² but his period can be extended for those classified as highly disadvantaged.

In most other countries, active labour market policies comprise a suit of discrete programs; each aimed at particular groups with its own eligibility criteria and delivered by public agencies. Intensive Support departs from this norm in three ways. Firstly, all the activities are combined under the single Intensive Support heading. Secondly, providers have greater flexibility in how they go about meeting the needs of jobseekers. However, the extent of this flexibility should not be overstated. As the Job Network has evolved, the Government has become more and more prescriptive about what services should be delivered and when. Thirdly, the private provision is driven by financial incentives rather than targets. About two thirds of providers' income is outcome based; based on placing jobseekers in jobs that last for at least 13 weeks.

A summary description cannot capture all the rules governing the operation of the Job Network. How the system works also depends on how the rules are translated into administrative processes, and how providers and jobseekers then navigate the system. For these reasons there is an inevitable discrepancy between a notional description of the system and how it actually works. Two discrepancies are particularly relevant to this paper.

Notionally the Job Network, and the Intensive Support component, is a compulsory form of assistance; persons who receive unemployment related income support are required to register with a provider, agree on the activities they should undertake to gain employment, and then undertake these activities. Compulsion means that there is no explicit, rules based, selection into Intensive Support except the earlier entry of those classified as disadvantaged. But jobseekers do self-select into Intensive Support. Some jobseekers who should be referred are not, some of those who are referred never register with a provider, and some of those who register make no or little use of the assistance on offer. Needless to say, there are no official figures for these three categories. An informed guess is that during the 2001-2006 period covered by this study 20-30 per cent of jobseekers who should have made use of Intensive Support did not do so.

A second discrepancy relates to the timing of entry into the Intensive Support component. The program guidelines specify that jobseekers should start Intensive support during the early part of an unemployment spell. By 2006 this ambition had partly been achieved, but until that time jobseekers could start Intensive Support at all times during a spell. In 2003, for example, a quarter of jobseekers had been unemployed for more than 36 months when they started Intensive Support (DEWR, 2001-2006).

In recent years about 500,000 persons per year have been referred to Job Network providers. Many find jobs within a short period, while about 380,000 move on

² This description refers to the situation during the 2003-2006 contracting period. The data used in this study cover the longer period 2001-2006, but the rules governing the 2000-2003 contract period were not fundamentally different from those during 2003-2006.

to Intensive Support. Of these about 50 per cent are placed in jobs that last 13 weeks or more for which an outcome payment is made to the provider (Stromback, 2008). While there are no independent estimates of the effect of the Job Network several official studies have been undertaken. DEWR (2003) found that the 12-months 'net impact' – the increase in the proportion employed 12 months after starting Intensive Support was 6.2 percentage points. In a later study, the figure was put at 10.1 percentage points (DEWR, 2006). Recently the focus has shifted to the off-benefit outcome, estimated at 5.8 to 6.4 percentage points in DEEWR (2010). In all cases, however, the brief descriptions of the method(s) used do not permit an informed assessment of the quality of these estimates (Stromback, 2002, 2008).

3. Method

We use the two most common approaches to estimating a treatment effect, matching and hazard function (Kluve, 2006). Using the matching approach, the focus is on the probability of employment at some fixed time after treatment. To identify a treatment effect the conditional independence assumption must hold. That is we have to assume that, conditional on a set of observable variables, the assignment to treatment is independent of the outcome. Hazard function methods, on the other hand, focus on the timing of the outflow to employment. By imposing greater structure a treatment effect can be identified without the conditional independence assumption by explicitly taking account of unobserved heterogeneity. The hazard function approach also allows us to use more information to estimate a treatment effect.

What treatment effect can be estimated is, however, severely constrained by the available data.

In this paper we use the data from a panel survey in which individuals are surveyed annually over a number of years. For individuals looking for work when they are surveyed we observe their elapsed duration of unemployment, and whether they were in treatment at the time they were surveyed. Using the next waves of the survey, one or more years later, we can then observe whether and when they obtained employment. From this data we can determine the start and finish date of spells of unemployment in progress at the time persons were surveyed, and whether they were in treatment at that time.

There are two limiting aspects of this information. The spells in progress at a point in time are a non-random selection of all spells – they have been drawn from the stock of the unemployed. Using the hazard function approach we can allow for this in the estimation of a treatment effect. The details are discussed below in section 3.1. The other limiting aspect is that it is not known when jobseekers started their treatment; we only know whether they were in treatment at the time they were surveyed. As explained above, during the period covered by this study jobseekers did not start Intensive Support at some fixed time during a spell of unemployment. Effectively, the treatment could start at any time during a spell and the method used ought to recognise this. Using the hazard approach, the timing of the treatment has to be explicitly specified by a time varying indicator. Using matching the problem posed by varying start times is more subtle. In the literature this is referred to as the random

start or dynamic assignment problem (Fredriksson and Johansson, 2003; Caliendo and Hujer, 2005). Essentially, the problem is that to estimate a treatment effect one needs a control group that was never treated. This, however, involves conditioning on future outcomes when the treatment can start at any time.

In recognition of this problem the method used is developed in two steps. In the first instance we define the effect of interest; the effect one would seek to estimate if the relevant information was known. The second part explains what effect can be estimated using the available data.

To define the effect of interest we consider a set of individuals who enter unemployment at time zero and who, at some time during their spell of unemployment, may be referred to Intensive Support. Let T denote their duration of unemployment and S the time of referral to treatment. Conceptually, T and S are treated as stochastic and the realisations of these variables are denoted by the corresponding lower case letters. Using the potential outcomes framework of Rubin (1974) the unemployment duration if untreated is denoted $T(0)$ and $T(1)$ if treated. Ex-post, only one of these potential outcomes is observable. At any point in time individuals are subject to two types of risks; either they exit unemployment by getting a job or they are referred to Intensive Support. In this setup the duration until the start of treatment (S) is stochastically dependent on the potential unemployment if not treated $T(0)$. That is, the individual will be treated if their unemployment duration is longer than the duration until the start of the treatment.

Thus the treatment indicator is defined by

$$D = I(T(0) > S) \quad (1)$$

where $I(\cdot)$ is an indicator function that assigns the values 0 or 1 to D according to whether $T(0)$ is less or greater than S

If the treatment has to start before it has an effect it is natural to judge its effect with reference to its effect on the remaining (post assistance) duration of unemployment (T^s)

Let $T_i^s(0)$ denote the potential outcome if the individual is not treated and $T_i^s(1)$ if she is. Denoting the observed outcome by T_i^s we have

$$T_i^s = \begin{cases} T_i^s(0) & \text{if } D_i = 0 \\ T_i^s(1) & \text{if } D_i = 1 \end{cases} \quad (2)$$

Given this setup it is natural to define the parameter of interest to be

$$ATT = E(T^s(1) - T^s(0) \mid D = 1) \quad (3)$$

i.e. the average effect of treatment on the post-assistance duration of unemployment.

The central problem in estimating a treatment effect is the estimation of the counterfactual; what the post-assistance duration of the treated would have been had they not been treated. Even when the start date is known for the treated, this is a non-trivial problem that has been explored in the recent literature. This problem is not critical to this paper, but serves to motivate the estimator proposed below.

To see what the problem is we note that for persons treated at $S=s$ we observe $T = s + T^s(D=1)$. From the latter information we can compute the first term in (3). For non-treated persons we observe the potential duration if not treated and thus $T=T(0)$. However, it is never possible to observe the start date for the untreated. Hence it is not possible to use the post-treatment duration for the untreated to estimate the counterfactual mean $E[T^s(0)]$ for the treated population. Several methods for dealing with this problem have been suggested in the literature. Forcing the dynamic assignment to treatment into a binary framework Gerfin and Lechner (2002) propose a matching protocol that trims the duration distribution of the non-treated to make them more like the non-treated. A different approach is to estimate treatment effects that vary with the starting time (Sianesi, 2001; Fredriksson and Johansson, 2003). Recently, a variation of this method using a different matching process has been suggested (Crepon *et al.* 2009).

Stated in a less formal way the problem is that when the treatment can start at any time during a spell, the longer is the spell the more likely it is that a person will be treated. Thus the non-treated will tend to have short spells and the treated long spells. This is the result of conditioning on future outcomes. To avoid bias towards finding a negative treatment effect some way to overcome this must be found; by correcting for the unbalanced distribution or by conditioning on starting times.

Turning to the effect that can be estimated with the data at hand the problem is that we do not even know the starting date of the treated. All we know is whether a person was in treatment at the time of the survey. The only treatment effect that can be estimated with this data is the average effect of treatment on the post-survey duration. Thus the set up represented by equations 1 to 3 is redefined by replacing the unknown start time S with the known survey time U .

The treatment indicator defined in terms of potential duration is now

$$D^* = I(T(0) > S \mid U > S) \quad (1)$$

i.e. to be defined as treated a person must be surveyed after they started treatment.

The average effect of the treatment on the post-survey duration is then given by

$$ATT^* = E(T^u(1) - T^u(0) \mid D^* = 1) \quad (3')$$

where T^u denotes the potential post-survey duration.

This overcomes the problem that S is not observed for the treated. In addition there is also a potential counterfactual for the second term in (3') since the post-survey

duration for the non-treated is observable. Instead, the issue becomes whether ATT^* is informative about the effect of treatment as represented by ATT . In general terms, this question is difficult to answer. However, a simple Monte Carlo study can shed some light on the issue. The analysis reported in appendix 1 shows that if there is no treatment effect ($ATT=0$), the ATT^* is also very close to zero. Thus, using the survey rather than the program start as the reference date is unlikely to reveal a spurious treatment effect. When there is a treatment effect, ATT^* is biased towards zero; i.e. towards finding no treatment effect. However the extent of the bias is not so large as to invalidate the analysis.

The Method of Matching

For each individual a matching estimator imputes the missing outcome by using the outcomes of other individuals with similar characteristics who were not exposed to the treatment. More specifically, we estimate the average treatment effect (as reflected in the post-survey duration) on the treated by comparing the outcomes between the treated and control observations using nearest neighbour matching over a small number of variables; the level of education, age and sex. In addition we match exactly on the survey wave to ensure that any effects of calendar time are controlled for.

Consistent estimation of a treatment effect requires two assumptions; that the assignment to treatment is independent of outcomes, conditional on the matching variables, and that the probability of assignment is bounded away from zero and one. The first assumption is commonly known as the conditional independence assumption or selection on observables. The second assumption is an identification condition. If all individuals with a given set of characteristics are treated, there are no observations of similar individuals who were not treated. The seminal paper by Rosenbaum and Rubin (1983) provides intuitive explanations of these two assumptions. The subsequent literature contains numerous restatements and refinements.

For a given values of the covariates the assignment to treatment is not random; those with larger values of U (and lower values of S) are more likely to be observed to have been treated. While this invalidates the conditional independence assumption the resulting bias can be reduced by balancing the pre-survey distributions of the treated and non-treated (Lechner, 1999). As regards the conditioning on a set of matching variables, the satisfaction of the conditional independence assumption requires that the matching variables include all the variables that determine both the assignment to treatment and the potential outcomes. All the variables typically imply a large number of variables. In practice this means that the matching has to be done on a balancing score like the propensity score. However, this score has to be estimated. In view of the first problem, there are no variables that predict whether an individual is in treatment at the time of the survey, propensity score matching is likely to result in unbalanced samples. Instead the matching is done on a small number of core variables that capture most, but not all, factors that determine the assignment to treatment and the potential outcome.

To implement the matching approach we follow the usual approach of a fixed evaluation period; taking the probability of employment at a fixed time after the survey time as the outcome indicator. Thus we define the outcome indicator Y as

$$Y_i = \begin{cases} 1 & \text{if } t_i^u < u_i + E \\ 0 & \text{if } t_i^u > u_i + E \end{cases} \quad (4)$$

where E is the fixed evaluation period, the time until the next wave of the survey or 12 months.

As before we assume that we are interested in the average effect of the treatment on the treated defined by

$$ATT^* = E(Y(1) - Y(0) \mid D^* = 1) \quad (5)$$

For each treated observation the outcome while treated is observed directly, while the counterfactual is estimated from the set observations that most closely match the individual in question. For a given value $U=u$ an individual from the comparison sample (looking for work but not on Intensive Support) is only matched if the unemployment duration $T=t_c$ for this randomly assigned control satisfies $t_c > u$. This ensures that the pre-survey duration distributions of the treated and controls are balanced. In effect, a treated is only matched with a non-treated if this person's elapsed duration at the survey date is at least as large as the elapsed duration of the treated.

The Hazard Function Method

The other common technique for estimating a treatment effect is the hazard function approach. Letting $f(t)$ and $F(t)$ denote the density and distribution functions of the duration of unemployment, the hazard function $h(t) = f(t)/(1-F(t))$ is the instantaneous probability of leaving unemployment at time t conditional on remaining unemployed at t .

In applying the hazard function approach to the problem at hand the primary consideration is to take account of the sampling scheme. Stock sampling is a special case of delayed entry or left truncation. The term delayed entry refers to the fact that persons are at risk (to cease looking for work) before they are first observed, i.e. enter the sample. Alternatively, the sample might be said to be left-truncated in that a person is only included if a certain condition is satisfied (here that they remain looking for work until the survey date).

To deal with this problem it is convenient to work in discrete time. Thus we reinterpret survival time T to be a discrete random variable with probabilities $f_t = \text{Prob}(T=t)$ where $t=1, 2, 3, \dots$ is the set of positive integers. Similarly the discrete hazard is the probability of exiting the looking for work state at time t conditional on having survived until that time

$$h_{it} = \text{Prob}(T_i = t \mid T_i \geq t) = f_t / (1 - F_t - 1) \quad (6)$$

where F_t is the cumulative probability of exiting before t , $F_t = \text{Pr}(T < t)$. Hence $1 - F_t - 1$ is the probability of surviving until immediately before period t .

One attraction of using a discrete hazard function is that by reorganising the data the parameters of the hazard function can be estimated from a binary regression (Singer and Willett, 1993; Jenkins, 1995; Sueyoshi, 1995). To reorganise the data we first have to explicitly recognise that only some persons are observed to exit the looking for work state. For persons with completed spells define $c_i=1$ and let $t_i^u=\tau_i$ index the time at which the spell finishes. For other spells, all that is known is that their duration exceeds some threshold value. These incomplete spells have $t>\tau$, but τ is not observed. We can then reorganise the data by defining the variable y_{it} where

$$y_{it} = \begin{cases} =1 & \text{if } t_i = \tau_i \text{ and } c_i=1 \\ =0 & \text{otherwise} \end{cases} \quad (7)$$

In this reorganised data set there is one observation for each spell period experienced by each person. The indicator variable y_{it} is set to zero for all spell periods except the period from which a person exits looking for work to employment. Using this indicator variable it can be shown that the contribution to the (log) likelihood function of the i 'th person, written in terms of y_{it} , is identical to the likelihood for a sequence of t independent Bernoulli trials with parameters h_{it} . Hence the parameters can be estimated by a binary regression with y_{it} as the dependent variable (Singer and Willett, 1993; Jenkins 1995). Furthermore, it is now straightforward to correct for the sampling scheme (Jenkins, 1995). A person only becomes at risk (of leaving the looking for work state) if she has survived until the time she is sampled (at the survey date). But conditioning on surviving until that time simply amounts to discarding person-spell observations for spell periods prior to the time of the interview. Guo (1993) calls this a 'convenient cancelling of terms', referring to the fact that the hazard for periods prior to the interview cancel in the expression for the conditional probability and hence do not contribute to the likelihood.

The hazard function is assumed to have the logistic form

$$\ln(h_{it} \setminus 1-h_{it}) = \lambda_t + X_i b + \delta D_{it} + v_i$$

This means that the logit hazard is a linear function of three factors; a common baseline effect which describes how the hazard varies over time, a set of individual specific covariates X , a treatment indicator $D = I(T>t^s)$ and an individual specific factor v_i with zero mean and uncorrelated with X and D . The logistic specification implies that the conditional log-odd that the event (exiting the looking for work state) will occur in each period is a linear function of these four set of factors. Since the hazard function represents the hazard of the remaining duration, the X variables include the elapsed duration at the survey date. This allows for the risk of exit to depend on how long a person has been looking for work when they are first observed at the survey date. Other empirical applications of discrete hazard models also adopt this specification (Zuecchelli *et al.* 2010).

Given the logistic transform, the parameters can be estimated by a standard logistic regression with the data organised as described above.³ There is one observation for each person – spell period from the survey date to the time the person is last observed, and the dependent variable y_{it} takes the value one for the spell period in which a person exits and zero otherwise. The baseline effect on the hazard is specified to vary over a small number of intervals while the covariates X are constant over time. Finally, the treatment indicator D takes the value one for treated persons for spell periods after the survey date and zero for all other spell periods.

4. Data

The analysis is based on the first six waves of HILDA covering the period 2001-2006. HILDA is household-based panel survey that begun in 2001. It collects information about economic and subjective well-being, labour market dynamics and family dynamics. While the sample unit is a household, individual information is obtained from annual interviews with all adult persons within a household. In the first wave in 2001, 13,969 individuals were interviewed. As household or persons drop out of the survey new respondents are added to the survey.

The population of interest in this paper is defined by an affirmative answer to the question: ‘At any time during the past four weeks have you looked for work?’ Having looked for work does not imply that these persons were also unemployed using the standard (ABS) definition of unemployment. About 20 per cent are classified as being not in the labour force; mainly because they did not satisfy the availability test for being unemployed (table 1). Given that most empirical studies have found that the unemployed and not in the labour force are ‘behaviourally different’, there is a case for analysing the two categories separately (Jonas and Riddell, 1999; Gray, Heath and Hunter, 2005; Elliott and Dockery, 2006). However, the small number of observations, compounded by missing and inaccurate data, suggests that separate analysis would not yield any additional insight.

Given that a person was looking for work their treatment status is then defined by the answer to the question ‘Are you currently receiving Intensive Assistance/Support from a Job Network provider?’ This self-reported treatment status need not correspond to what administrative records would have shown had such data been available. Taking into account the characteristics of the jobseekers, the proportion who report that they receive assistance is lower than would be expected on the basis of the program rules. It is likely that many jobseekers who are on Intensive Support according to the administrative records do not report this because they make little use of the assistance being offered.

³ The independence assumption is normally made with respect to the population and need not hold in a sample drawn from the stock of persons looking for work. If, however, one is prepared to assume that v is normally distributed and independent of X and D in the stock sample, the model can be estimated using the technique described.

Table 1 - Looked for Work, Employment Status and Received Intensive Support (Assistance) (number of persons)

Wave	Not asked (employed)	Looked for work in the past four weeks			Currently receiving Intensive Assistance	
		Unemployed	Not in the labour force	Total	Number	Per cent of total who looked for work
1	8,525	609	119	728	145	20.3
2	8,088	516	116	632	126	20.2
3	7,991	461	102	563	90	16.1
4	7,822	413	117	530	89	17.1
5	8,247	399	130	529	115	22.0
6	8,357	432	124	556	106	19.2

Source: HILDA waves 1 to 6.

Note: The per cent figures have been adjusted for a small number of persons who answered 'Don't know' to the question.

To estimate the effect of Intensive Support using the matching method we use only the information at the survey dates. Successive waves are used pair wise; the treatment status is observed in one wave and the outcome, employed or not employed, in the following wave. The data from successive pairs are then combined as if they represented independent observations. This is simply an easy method for utilising all the information in the data. It means that new respondents are automatically incorporated in the data set and the same person is used more than once if a person remains in the survey and is looking for work.

While survey data records the elapsed duration of looking for work at the time of an interview, it is silent on when a person ceased to look for work or found a job. Thus, to estimate a hazard function there is a need to combine the survey data with the more detailed information from the HILDA calendar data. The calendar data records a person's labour force status for each HILDA period from July the previous year to the end of the month preceding an interview date. From the calendar one can in principle derive the start and end of each spell of looking for work that begun or ended during the time a person was in the survey. However, this retrospective information appears to be subject to considerable recall errors reflected in an excessive proportion of spells ending at the interview date. There is also a large number of missing calendar entries. How the survey and calendar data are combined is explained in more detail in appendix 1.

5. Results

Pooling the data from the first six waves (five successive pairs) there are 2,933 observations of persons looking for work for which we observe the 12 month employment status in 2,441 cases. Table 2 contains the further details. Overall, 42 per cent of those assisted were employed in comparison to 50 per cent for those not assisted.

Table 2 - 12 Months Employment Outcomes of Persons Looking for Work (per cent)

<i>Initial status</i>	<i>Treatment status</i>	<i>Status 12 month later</i>		
		<i>Employed</i>	<i>Unemployed</i>	<i>Not in the labour force</i>
Looking for work	On Intensive Support	42.6	35.8	21.6
	Not on Intensive Support	50.4	20.6	29.0

Source: HILDA waves 1 to 6.

To implement the matching estimates we match directly on sex, age and level of education using the algorithm proposed by Abadie *et al.* (2001). As pointed out, propensity score matching using a large set of variables is not necessarily superior when it is not possible to capture the factors that influence the treatment status. In addition, many of the factors that potentially influence leaving unemployment (such as previous employment history) have large number of missing values. We also note that studies of unemployment duration in Australia typically find that only a few variables actually have a significant impact (Stromback and Dockery, 2000; Carroll, 2006). In terms of the included covariates, the treated and the non-treated are fairly similar. This can be attributed the stock sampling and the universal character of Intensive Support. This, and the fact the covariates take on only a few values, makes it possible to find exact matches for all treated.

The matching estimate of the average effect of treatment on the treated is given in table 3 and shows that Intensive Support is estimated to have no effect on the probability of being employed 12 months later. Minor variation to the matching process – the number of matches, the matching variables, and the metric for computing the distance between an observation and its matches – make little difference to the estimated effect and are not reported in detail. Since those observed to be in treatment at the survey date have been in treatment for different lengths of time it is conceivable that the treatment effect varies with the elapsed duration. This further detail is given in table 4 and shows that the point estimates are all small and not significantly different from zero.

Table 3 - Matching Estimates of the Treatment Effect: Change in the proportion employed 12 months after interview date as a result of Intensive Support

	<i>Unmatched</i>		<i>Difference</i>	<i>Matched</i>
	<i>Received IA</i>	<i>Did not receive IA</i>		<i>ATT*</i>
Employed at 12 months	0.426	0.504	-0.078	-0.003 (0.029)

Note: Based on 2,573 spells using age, sex and level of education as matching variables and conditioning on the pre-survey date duration. Standard errors in brackets.

Table 4 - Matching Estimates of the Treatment Effect by Elapsed Duration

<i>Elapsed Duration</i>	<i>ATT*</i>
0-26 weeks	-0.021 (0.023)
27-52 weeks	0.037 (0.041)
52 or more weeks	0.013 (0.032)

Note: See note to table 3.

The estimate of the hazard function is based on the spells that were in progress at a survey date for which a corresponding calendar spell could be identified. The total number of such spells is 2,775. Of these, only 914 spells were observed to end in employment. The remaining spells, which had a missing destination or ended with the person ceasing to look for work, were treated as right-censored. Further details are given in appendix 1.

The estimates given in table 5 are based on a piecewise constant baseline logit hazard with five duration intervals. The estimates imply that the hazard is declining in duration after the first period. This finding is consistent with other findings reported in the literature (Carroll, 2006). Also in line with other findings is that the hazard is increasing in the level of education (Barrett, 2000; Knights, Harris and Loundes, 2002; Carroll, 2006). The two highest level of education are an exception to this rule, but based on a small number of observations. An additional variable is included to account for the high incidence of transitions at the survey dates. This variable takes the value one at $t=t^l+1$ and zero otherwise. The large positive value of the estimated parameter indicates that there is a large increase in the hazard at that point in time.

Intensive Support is estimated to decrease the hazard but this effect is negligible. To make this statement more precise, for a typical individual, having the default values of the covariates, the estimated coefficient translates into a decrease of the hazard from 0.031 to 0.030⁴. Alternatively the estimate can be translated into the effect of Intensive Support on the probability of having ceased looking for work 12 months after the assistance started. This effect can then be compared with the matching estimate stated in that form. This is an indicative comparison; the calculation can only be made for a particular person conditional on the covariates and an assumed starting time. Almost irrespective of the particular values assumed, Intensive Support decreases this probability by less than two percentage points.

Allowing for unobserved heterogeneity does not change these results. The estimates in the third column of table 4 are almost identical to the column 1 estimates and the estimated standard deviation of v is very small. Although the small number of covariates might not account for all differences between individuals, it is a common finding that a flexible baseline hazard mitigates the effect of unobserved heterogeneity (Dolton and van der Klaauw, 1995).

⁴ Given the specification of the logit hazard the hazard for a typical non-treated person at $t>72$ is given by $h = 1/1+\exp(3.46) = 0.031$ where 3.46 is the estimated constant term in the regression model. If that person were treated the hazard is $h = 1/1+\exp(3.46+0.018) = 0.030$.

Table 5 - Estimates of the Effect of Intensive Support on the Looking for Work to Employment Hazard

	<i>No unobserved Heterogeneity</i>		<i>Allowing for unobserved Heterogeneity</i>	
	<i>Parameter</i>	<i>Std. error</i>	<i>Parameter</i>	<i>Std. Error</i>
Constant	-3.46	(0.131)	-3.49	(0.143)
Elapsed duration at survey date (periods)				
1-3	0.858	(0.140)	0.924	(0.155)
4-8	0.870	(0.126)	0.972	(0.140)
9-17	0.884	(0.130)	0.931	(0.156)
18-35	0.648	(0.103)	0.689	(0.115)
36 -71	0.257	(0.099)	0.282	(0.105)
72 or more	-	-	-	-
Survey period indicator				
Female	0.659	(0.114)	0.665	(0.131)
Age	-0.203	(0.069)	-0.182	(0.075)
15-24	-	-	-	-
26-44	0.032	(0.080)	0.041	(0.091)
45 or more	-0.087	(0.103)	-0.098	(0.121)
Highest Level of Education				
Doctorate, Masters	-0.424	(0.299)		(0.312)
Graduate Diploma/Certificate	0.389	(0.258)	0.391	(0.263)
Bachelor	0.700	(0.130)	0.710	(0.139)
Advanced Diploma / Diploma	0.438	(0.149)	0.431	(0.152)
Certificate III or IV	0.206	(0.103)	0.215	(0.109)
Certificate I or II	-0.027	(0.224)	-0.030	(0.235)
Certificate not defined	-0.807	(0.508)	-0.786	(0.532)
Year 12	0.483	(0.093)	0.495	(0.102)
Year 11 or below	-	-	-	-
Intensive Support	-0.018	(0.116)	-0.029	(0.124)
σ_v			0.152	(0.302)

Note: Based 2,781 spells for which complete information could be obtained. Standard errors in brackets.

6. Conclusions

In view of the limited information at our disposal it would be pertinent to pretend that we can draw any precise conclusions about the effect of Intensive Support. The sample is not a random draw from the population of interest, and critical information – when the assistance started and a spell ends – is either not known or subject to recall errors. Because of these problems there is no satisfactory way to deal with other issues that should be addressed, including the problem posed by random starts.

The point estimates suggest that Intensive Support has no effect on employment outcomes. However, these point estimates are imprecise and the only thing one can confidently assert is that the effect, be it positive or negative, is not very large. This is a disappointing conclusion. The Job Network has now been operating for over a decade but the effectiveness of the assistance remains a conjecture.

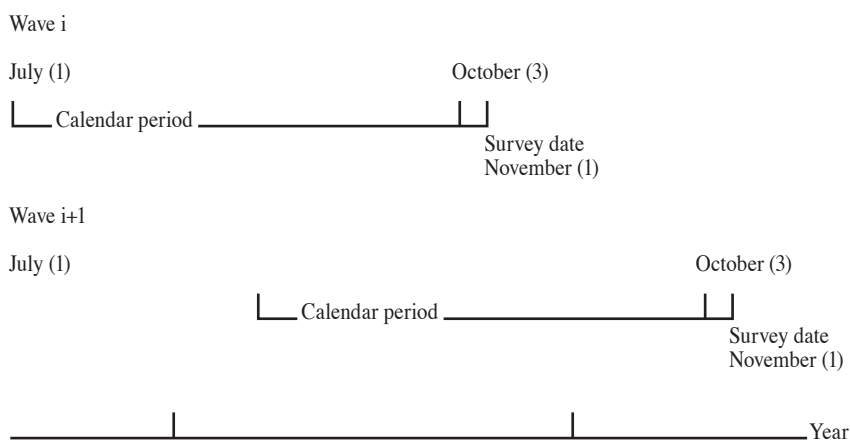
Although the analysis in this paper suffer from some limitations, it is unlikely that a precise estimate of the effect of Intensive Support can be obtained from publicly available data. Thus, unless the Commonwealth Government makes administrative data available to independent researchers, or undertake a credible evaluation themselves, we will never know. However, even with good administrative data, it is not certain that the causal effect of the Job Network, or its renamed successor, Job Service Australia, can be uncovered. Several features of the Job Network complicate the estimation of the effect. Foremost among these is the universal nature of the assistance. This means that it is difficult to identify a control group that can be used to estimate the counterfactual outcomes. Those who fail to register with a provider or refuse to use the assistance offered are two possibilities, but persons in these groups are likely to differ from those who actively use the assistance. There are better prospects for evaluating discrete elements of the assistance, but that depends on being able to use the timing of treatment to identify a causal effect.

Appendix

Appendix 1 - Spell Data from the HILDA Calendar

The HILDA calendar records whether a person was in one or more of three types of employment status, employed, looking for work and neither employed nor looking for work, and if employed in which of up to 12 different jobs they were employed. The calendar covers the period from July the year preceding the survey date (in most cases during October or November) until the period before the survey date. Time is measured in HILDA periods of one third of a month.

Figure A1 - Schematic Structure of HILDA Calendar Data (person surveyed during the first third of November)



To derive the end date of spells of looking for work the data for the up to 12 different jobs are first consolidated into a single employment calendar that records whether a person was employed in each period. Then the three types of employment

status are in turn consolidated into a calendar that records a person's employment status in each period. In this last step, precedence is given to the looking for work status if a person is recorded as having more than one employment status during the same period. From this consolidated calendar we then record the start and end of each looking for work spell and the origin (the preceding employment status) and destination (the following employment status) of these spells. If the calendar entry immediately prior to the interview period was missing the last recorded entry was carried forward until the interview period.

Using this process we identified 4,582 persons having a total of 8,321 spells of looking for work during the six waves spanning the period July 2000 to December 2006.

Table A1 - Origin and Destination of Looking for Work Spells

	<i>Origin</i>	<i>Destination</i>
No previous calendar entry	485	
Employed	3,959	5,033
Not in the labour force	1,970	952
Missing	1,907	2,336
Total	8,321	8,321

Source: HILDA waves 1-6.

Whether a person was receiving Intensive Support (and the values of all the covariates) is only known at the times they were surveyed. Thus the survey data must be matched with the calendar spell data to determine when a spell in progress at interview dates ended.

During the six waves there were total of 3,548 looking for work spells in progress at interview dates for 2,516 distinct persons. For these spells, a matching calendar spells, having a start and end periods that span the interview period, could be found in 2,568 cases. By allowing margin of plus or minus four periods, however, 3,096 interview spells could be associated with a corresponding calendar spell.

The duration of these spells were determined as the sum of elapsed duration at the interview plus the post-interview duration, as given by the difference between the end of the spell and interview period. If the elapsed duration was missing, the duration of a spell was derived from the difference between the end and start of the spell (if known). Deleting a small number (315) of very long spells left 2,781 spells for 2,102 distinct persons to estimate the hazard function. Of these 917 spells were observed to end in employment. The remaining 1,864, having a missing destination or ending with not in the labour force, were treated as right-censored in the estimation.

The calendar data has the property that the labour force status for the first period after an interview period is derived from the next interview 12 months later. Being a recall of what a person was doing almost one year ago, it is likely to be subject to recall errors. This is reflected in a very large number of spells that end one period after the interview period, i.e. have post-interview duration one period. About two thirds of the spells used in the analysis fall into this category. In most cases these spells also have an unknown destination, meaning that the first post interview calendar entry is missing. The excessive number of spells that end at the time of an interview is not specific to looking for work spells but applies to employment and not in the

labour force spell as well. Evidently, the quality of the retrospective information in the calendar can be quite poor. This must be borne in mind when interpreting the findings, even though we allow for this in the estimation of the hazard function.

Appendix 2 - Monte Carlo Study of the Effect of Using Post-survey Duration in Place of Post-treatment Duration to Estimate a Treatment Effect

This appendix explains the method we used to investigate the effect of using the mean of the post-survey duration as an indicator of the effect of treatment.

The probabilities of success (exiting unemployment or starting treatment) was simulated from a geometric distribution function with probability of success equal to $p_j = p(X_i)$. Specifically, taking p to be the logistic function the probability distribution of time until success was generated as

$$P_{ij} = 1/[1+\exp(-(\alpha_j + \beta_j X_i)] \quad j=0, 1, 2.$$

where α_j and β_j are constants and the X_i 's were generated from a (0,1) uniform distribution.

This implies that the probability distributions of time to success is given by

$$\text{Prob}(\Gamma = \tau | X_i) = p_j(X_i)[1-p_j(X_i)]^{\tau-1} \quad j=0, 1, 2.$$

This same method was applied to all three duration variables (corresponding to $j=0, 1, 2$), the no- treatment duration $T(0)$, the time until treatment S and the post-treatment duration T^s . With this parameterization about 50 per cent of persons are treated, i.e. have $s < t$.

Table A2.1 - Parameters Used to Generate the Duration Variables

	<i>Parameters</i>		<i>Means of $P_j(X_i)$</i>
Duration variable	α	β	
T(0)	-3.5	1.0	0.049
S	-3.5	1.0	0.049
T ^s	-3.75, -3.5, -3.25	1.0	0.037, 0.049, 0.060

A positive treatment effect (shorter post-treatment duration) represented by setting $\alpha = -3.25$ instead of -3.5 . This translates into an increase in the hazard of 22 per cent (last column of the table) Expressed in terms of the post-treatment duration, this treatment effect reduces the mean duration by about 20 per cent. Setting a negative treatment effect $\alpha = -3.75$ has the opposite effect.

To mimic a stock sample, spells are assigned a notional starting date B drawn from the uniform (0,K) distribution and the spells in progress at the notional survey date K , satisfying $B+T < K$, are extracted to form the stock sample where the 'observed' duration T is given by

$$T = \begin{cases} T(0) & \text{if } S > T(0) \\ S + T^s & \text{if } S < T(0) \end{cases}$$

The post-survey duration is then given by $T^u = B + T - K$ and the treated are identified by being in treatment at the survey date, i.e. having started treatment before the survey date, i.e. $D^* = I(B + S < K)$.

The estimates of the treatment effect, ATT^* is then computed by a matched comparison of each treated with the non-treated individual with the closest value of X . For a given value $U=u$ an individual from the comparison sample is only matched if the unemployment duration $T=t_c$ for this randomly assigned control satisfies $t_c > u$. This ensures that the pre-survey duration distributions of the treated and controls are balanced.

In table A2.2 the ATT is given as the change in the proportion of successes 12 periods after the start of the treatment to correspond to the estimation of ATT^* in the body of the paper. Similarly, the ATT^* is computed as the change in the proportion of successes 12 periods after the survey date. Both are calculated from 1,000 repetitions using a sample size of 2,000. As shown in the table, ATT^* is biased towards zero and the bias is larger when the treatment effect is positive. The magnitude of the bias might be said to be large, but if the treatment effect is large it should be possible to confirm this from observational data using ATT^* .

Table A2.2 - Bias of the Estimated Treatment Effect Based on Post-survey Duration

	<i>Treatment Effect (mean post-treatment duration)</i>		
	Negative	Zero	Positive
ATT	-0.063	0.000	0.095
ATT^*	-0.053	0.002	0.071
Bias (per cent)	15.9	-	-25.3

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