Assisting the Long-Term Unemployed: Results from a Randomized Trial

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Abstract

Welfare reform in Australia centres on the concept of both economic and social participation. The policy concern is that people who fail to participate in economic and social life may become entrenched in disadvantage. In 2000 - 2001, a randomized trial was conducted by the Department of Family and Community Services in order to assess whether an intensive interview with successive follow-ups would result in increased economic and social participation for unemployed individuals who had been on income support for more than five years. We find evidence that participation in the trial led to a reduction in average weekly hours of work, but increased the amount of hours spent in study or training programs and the proportion of individuals engaged in such programs. We also find evidence of increased social integration associated with participation in the trial.

1 Introduction

In the face of historically high levels of unemployment in many countries, there is mounting international evidence that unemployment is increasingly concentrated among a subset of individuals who spend substantial periods of time without work. Much of the dramatic increase in unemployment over the 1970s and 1980s, for example, can be attributed to increases in the average time spent unemployed rather than an increased propensity to be unemployed (for example, Layard, et. al., 1991; Meyer, 1995; Dolton and O'Neill, 1995). The policy response has been a dramatic increase in the use of active labour market policies–training, job search assistance, and increased monitoring and counseling–to stimulate the employment of unemployed workers. In this paper we evaluate the effects of one such policy (increased monitoring and counseling) on the economic and social participation of the long-term unemployed in Australia.

Australia provides an interesting case study in the growing international literature evaluating active labor market policies. First, Australia has not been immune from the problem of persistent unemployment with 14.4 per cent of unemployed individuals in Australia having been unemployed for more than two years (Dockery and Webster, 2001).[1] Second, in spite of similar income levels and labour market environment as other OECD countries, important institutional differences in the Australian income-support system add depth to the existing literature. In Australia, unlike in many other countries, unemployment benefits are non-contributory, funded from general revenue, and comprise one component of a broader system of income-support payments administered by the Department of Family and Community Services (FaCS). Finally, there has been a willingness on the part of FaCS to use randomized trials and administrative panel data to evaluate the effects of particular intervention strategies.

To this end, FaCS ran three randomized trials involving interviews with 10,504 income-support recipients between September 2000 and April 2001 to determine whether expanded counseling and monitoring would result in increased economic and social participation. The interventions were fairly modest and were targeted towards individuals who are in some sense outside the mainstream of service delivery.[2]

This paper reports on the results of one of these trials in which unemployed people

[1] Miller (1997) demonstrates that in Australia unemployment is also increasingly concentrated within the same family units. In 1974, 8.6 per cent of all unemployment occurred in couple families where both partners were out of work, by 1994 this had risen to 23.8 per cent.

[2] In particular, the groups targeted included older individuals of working age, unemployed people on income support for more than five years, and jobless parents of school-aged children.

who have been on income support for five years or more, and who often face multiple barriers to employment, were requested to undergo a counseling and monitoring process. We match survey data from trial participants to administrative benefits data to address the following question. Does an intensive interview with follow-ups lead the very long-term unemployed to increase their level of economic and social participation? Since the intervention is fairly small and the analysis time frame short, we are concerned not only with complete movement off of payments, but also with any deeper engagement in the social and economic life of the community which may lead to decreased dependence on the social welfare system in the future.

Our results indicate that individuals who participated in the intensive interview and follow-up engaged in higher levels of study and training. We also found that the proportion of people who engage in study and training increased due to the intervention. Consistent with this result, individuals who participated in the full intervention are more likely to remain on payments. Number of weekly hours worked fell as a result of the intervention, but we found no change in the proportion of individuals engaged in paid work. We also found evidence that the level of social engagement increases for those who participate in the intervention.

The remainder of this paper reviews the Australian social security system and the trial itself: who was involved, the processes, and some of the difficulties of and limitations in the trial design. It then moves on to consider the methodology used to evaluate the randomized trial and the results. We conclude by discussing these results in the context of international experience.

2 The Australian Income-Support System

> "Central to our vision is a belief that the nation's social support system must be judged by its capacity to help people participate economically and socially, as well as by the adequacy of its income-support arrangements (McClure, 2000, p. 3)."

The income-support system in Australia is quite different to systems in other parts of the world, which are most often based upon a social insurance model. The Australian system is non-contributory and funded from general revenue. Payment levels are uniform across the country, so that people in particular circumstances, such as unemployed people, have exactly the same entitlements as one another, irrespective of previous work history. Payment levels are determined by individual circumstances such as marital status, number and age of dependent children, housing situation, income and assets. In-work benefits for low-income workers are a significant feature of the system, particularly for people with dependent children.

A process of welfare reform has made participation–both economic and social–the focal point for future directions in income-support policy. Australian policy makers are increasingly embracing the notion that in order for society to function effectively, any expansion in the rights of individuals to make choices must be balanced with a growth in individuals' responsibilities and obligations to society.[3] This concept of "mutual obligations" between various members of society has lead policy makers in Australia to demand more of income-support recipients. Many recipients now face a requirement to fulfill their social obligations through meaningful activity in the form of work (either paid or voluntary), caring, job search, or education and training. In its own right, economic and social participation is seen more generally as the primary mechanism for avoiding the persistent disadvantage accompanying long-term receipt of income support.

More specifically, all unemployed people are obliged to look for work, or to undertake other activities to increase their chance of employment, and to accept any suitable offer of work. Some income-support recipients are also expected to undertake part-time paid work, voluntary work, training, or participation in community-based projects. Currently, the obligations placed upon unemployed people are at their most intense in the first 12 months of a spell of unemployment[4]. The enforcement of the job search requirement is also at its most intense during the first year of unemployment. For those who are unemployed for an extended period beyond 12 months, there is only a weakly-enforced obligation of continued job search. A further implication of the decline in intensity of contact with the income-support system after the first 12 months is that there is relatively little information about these customers, their activities, their motivation to take up new participation activities, and the outcome when they do. The randomized trial provides the opportunity to gather data about this group of unemployed people who have had, in practice, few obligations within the income-support system.

Possible future policy changes include extending strict mutual obligations requirements to this group of unemployed. Another option, and the one explored in this trial, is a voluntary program where participants are helped to form a "Participation Plan" which

[3]See the final report of the Reference Group on Welfare Reform, McClure (2000).

[4]The same holds true for the Job Network. Many of these people have been unemployed since before the Job Network started and therefore were not included on their case-loads. Others have dropped off and not been followed up since Job Network members tend to focus more intensively on the newly unemployed.

involves educational and employment activities with the long-term goal of moving the individual off of income support. Under this policy, program administrators act more as facilitators than enforcers in helping income-support recipients to learn about training and work opportunities.

Beyond the immediate imperative of welfare reform is a broader reform environment associated with a new public management structure. One of the largest public management reforms in Australia has been the disaggregation of the former Department of Social Security into a smaller policy-oriented purchasing department, now known as the Department of Family and Community Services (FaCS), and a large, service-delivery organization, known as Centrelink. This arrangement, operational from July 1997, accounts for one-third of Commonwealth expenditure. It is associated with the development of performance measures and standards, and a shift to a focus on output controls through a service level agreement. As a "one-stop shop" for customers, Centrelink manifests new public management principles through a strong customer focus, the removal of levels of decision-making, refinement of the client relationships with purchasing departments, and an increasing specification of outputs.

Reforms which separate purchasing functions from service provision in government welfare and social service areas have been particularly favoured in other English-speaking countries, notably New Zealand and Britain. Because the income-support workload of Centrelink is governed by a contract agreement, both it and FaCS have increased their skill in and focus on specifying service delivery outcomes. This focus on outcomes is an important context for the randomized trial; it is designed to test the outcomes of a basic intervention, with a view to informing the delivery of income support.[5]

3 The Trial: Assisting the Long Term Unemployed

The randomized trial targeted unemployed individuals (currently on Newstart Allowance) who had been on one income-support payment or another for five years or more. There are currently around 79,000 unemployed Newstart Allowance recipients in this situation, and this group is thought to be the most disadvantaged of all long-term income-support recipients. The proportion of people on unemployment benefits who are out of work for

[5] A similar issue arrises in the United States because although responsibility for the Job Training Partnership Act (JTPA) lies with the federal government, in particular the U.S. Department of Labor, it is implemented in hundreds of service delivery areas at the state and local level. This raises the possibility that state and local governments may wish to pursue different objectives than the federal government agency providing the funding. Barrow (2000) explores the relationship between the performance indicators given to individual service delivery areas and measured program impacts.

long periods has grown, despite a downward trend in the overall number of unemployed people.

The policy concern is that people who have been on payment for more than twelve months tend to have little contact with Centrelink. They are generally subject to an administrative review to ensure that they are receiving the correct rate of payment, but tend to receive minimal assistance to address their barriers to returning to work. The question of interest is whether an intensive interview with Centrelink staff would be helpful in improving economic and/or social participation. The interview was designed to provide a more holistic intervention than usual, in an attempt to understand some of the more complex barriers and difficulties facing this group.

The process of the trial was as follows. First 20 Centrelink sites were chosen on a random basis from the pool of sites across Australia. Next, a list of clients meeting the selection criteria in those sites was randomly selected from FaCS's administrative system. This resulted in the selection of some 4700 participants, who were then allocated to control and intervention groups on a random basis.

Centrelink sent a letter to each individual selected for the intervention group, asking them to attend an interview at a Centrelink office. This letter formed one part of the intervention.[6] The full intervention consisted of the letter and two face-to-face interviews. The first of these interviews took place in September and October 2000. Interviews were conducted by Centrelink staff who had been trained in research interviewing techniques and who administered a detailed questionnaire eliciting information about the individual's employment and educational background, current circumstances, goals and aspirations regarding economic and/or social participation, and any barriers he or she faced in increasing participation levels. The interviews and the data gathered went well beyond the usual employment-oriented survey and included detailed questions about the level of interaction which individuals had with their families and local communities and their view of themselves within Australian society. Participants and interviewers discussed ways in which the participant could become more economically and socially integrated and jointly developed a Participation Plan, codifying the agreements made in the discussion.

The second interview was conducted two months later in November and December 2000. It was intended that these second interviews would be conducted face-to-face with the same customer service officer who conducted the first interview. In most cases, this did happen. The purpose of the follow-up interview was to find out how individuals'

[6] A copy of the letter is in Appendix 2.

situations had changed over the previous two months and to discover whether they had taken up referrals and carried out other activities in their Participation Plans.

Full or complete treatment is defined for the purpose of this study as receipt of the letter and participation in both face-to-face interviews. Those who received the letter but did not participate in both interviews are considered as having received partial treatment.[7]

Finally, a third interview was conducted in March and April 2001. This interview involved a 20-minute follow-up telephone survey conducted by an independent market research company. The purpose was to find out how participants had progressed with their plans for economic and social participation, and the take-up of referrals. Comparison of data from the first face-to-face interview (Wave 1) and the follow-up telephone interview (Wave 3) forms the basis of the analysis of the impact of full participation in the trial.

In September to October 2000, individuals randomly selected for the control group were sent letters informing them that a market research company would telephone them.[8] Control group members who agreed to the interview were subsequently interviewed by telephone by the same market research firm that conducted the final intervention group interviews. Control group participants were subsequently re-interviewed by telephone (again by the same market research firm) at intervals corresponding to the interviews conducted with the intervention group.

In addition to the detailed information from the trial, we made extensive use of administrative data which was merged with the trial survey data. These administrative data come from FaCS's Longitudinal Data Set (LDS) which provides fortnightly observations on benefits received and limited demographic and human capital characteristics (age, age of youngest child, geographic area, etc.). The fact that these administrative data are available for all individuals selected for the trial (irrespective of whether or not they participated) allows us to test our random assignment and to assess the factors related to an individual's decision to fully participate in the treatment (or in the case of the control

[7] There is reason to believe that receipt of the letter alone may cause payment recipients to take action, as noted in the work of Black et. al. (1999) and Richardson (2001). These authors find that contact between payment recipients and funding organizations often leads recipients to change their behaviour even in the absence of any actual training or other program. Considering that this is a group of the long-term unemployed for whom no contact with the government agency distributing their payments is the norm, any contact must appear unusual and may spur them to some type of action.

[8] Appendix 2 includes all seven letters which were sent to control and intervention group participants at the various stages of the intervention. Different letters were sent to people who agreed to be interviewed in Wave 3 and those who declined. In addition an effort was made to solicit phone numbers from people for whom no phone number was available in Centrelink records. See the discussion of dropout in the methodology section below.

group to agree to be interviewed in all three waves). We discuss in more detail in the following sections how the administrative data were used.

4 Methodological Issues and Estimation Strategy

The randomized design of the trial was intended to simplify the measurement of the impact of the intervention on the outcomes of interest. To illustrate, we begin by letting the population relationship between an outcome, Y, and some characteristic, X, for people when they do not participate in the intervention be given by

$$Y^0 = X\beta_0 + u_0 \tag{1}$$

where $E[u_0|X] = 0$.[9] Likewise, the population relationship between Y and X, for people when they do participate is given by

$$Y^1 = X\beta_1 + u_1 \tag{2}$$

where $E[u_1|X] = 0$. The superscripts on Y indicate the outcome under participation and non-participation. We will suppress these whenever possible. We refer to realizations of the outcome measure for participants and non-participants as $y^1$and y^0, respectively.

Given this framework, there are several parameters which might be of interest when considering the impact of treatment on individual outcomes. The average treatment effect, defined as

$$\Delta^{ATE} = E\left(Y^1 - Y^0|X\right) \tag{3}$$

measures the change in expected outcomes across the entire population as a result of participation in the intervention. When only some individuals in the population receive the treatment, for example because the intervention is voluntary or because resource constraints limit the number of individuals able to be treated, the effect of "treatment on the treated" can be defined as

$$\Delta^{TOT} = E\left(Y^1 - Y^0|X, P = 1\right) \tag{4}$$

where (P) which takes value 1 if the person participated in the intervention and 0 otherwise. This latter parameter is often of greater interest since this measures the effect of the treatment for those people who actually do participate in the treatment. The challenge in deriving estimates of these two parameters, however, is that we do not observe

[9]The linear outcome equation is not particularly restrictive, but is used merely for ease of exposition.

both $y^1$and y^0 for any particular individual. This is the well-know "evaluation problem" (Heckman et. al., 1999).

Random assignment provides one means of solving this evaluation problem. Define a dummy variable (S) which equals 1 if the person is randomized into the intervention group and 0 if the person is randomized into the control group. With no dropouts and obligatory participation, $P = 1$ whenever $S = 1$. Assuming sample sizes of n_0 and n_1 in the control and intervention group respectively, one possible estimator of the average treatment effect is

$$\begin{aligned} \widehat{\Delta}_1 &= \overline{y}^1 - \overline{y}^0 \\ &= \frac{1}{n_1}\sum_{i=1}^{n_1} y_i^1 - \frac{1}{n_0}\sum_{j=1}^{n_0} y_j^0. \end{aligned} \tag{5}$$

It measures the effect of taking a randomly selected individual from the population and treating him or her. Under the assumption that all people assigned to the intervention group fully participate in the treatment, $\widehat{\Delta}_1$ also captures the effect of treatment on the treated. If only some individuals selected into the intervention group receive treatment this is no longer true, see Heckman et. al., 1999.

The Department of Family and Community Services' initial intention was to conduct a randomized trail in which all individuals were required to participate and then subsequently use $\widehat{\Delta}_1$ to measure the resulting change in outcomes (the average treatment effect). Randomization allows us to analyze results without controlling for the covariates, X.[10] Whether treatment effects are homogenous or heterogenous across individuals does not particularly matter, and Δ^{ATE}–as estimated by $\widehat{\Delta}_1$–can be seen as the expected treatment effect averaged across all individuals.

Several practical difficulties arose which cast doubt upon the validity of $\widehat{\Delta}_1$ as the appropriate estimator, however:

1. In examining the data, we grew concerned that the initial allocation into control and intervention groups appeared to not be random.

[10] To see that this is an unbiased estimator in a randomized trial, consider a regression framework where Δ_1 is the coefficient on P

$$y = \alpha + \Delta_1 P + v. \tag{i}$$

v includes X, but provided the randomization is properly carried out

$$E(P|X) = E(P|v) = 0. \tag{ii}$$

2. Significant dropout took place from both the treatment <u>and</u> the control groups. Control group dropout is an uncommon problem and has not been addressed in the literature. Both dropout processes are likely to be correlated with outcomes. Where dropout is correlated with outcomes, the simple mean comparison is no longer an unbiased estimator of the average treatment effect.

3. Different data gathering techniques were used for the treatment and control groups for the first two waves of data. There may be systematic differences in how individuals respond to the two types of interview technique used–even for identically posed questions.

We discuss each of these issues in turn in the remainder of this section.

4.1 Randomization

In order to use the simple mean comparison of (5), the treatment and control groups must be randomly chosen samples from the population. Using administrative data from the LDS, we tested whether the means of key demographic variables for the full intervention group of 2940 individuals were the same as those of the 1800 individuals in the control group. The characteristics–in particular age and income levels–of individuals in the two groups were quite dissimilar suggesting that the initial sample assignment had not achieved randomization. (See Table 1, columns 2 and 3).

Investigation into the procedure used for the sampling provided some insight into the source of the problem. FaCS, in addition to this trial, was simultaneously conducting a similar study of mature-aged payment recipients. Because of concerns about potentially small sample sizes for that trial, last minute alterations were made to the frame from which the sample for this study was drawn. As a result, no individuals over the age of 50 were assigned to the control group, although about 10 per cent of the individuals in the intervention group are over age 50. It seems unlikely that this discrepancy arose by chance. For obvious reasons, age is an important factor in determining the outcomes of interest, thus (ii) will be violated.

[Table 1 about here]

Unfortunately, dropping those aged 50 and over from the intervention group was not sufficient to rescue the random assignment. Using the LDS, we compare the means of important variables for the age-restricted intervention group and the full control group in column 4 of Table 1. These results indicate that there remain many significant differences between the means of important variables which may be correlated with outcomes, indicating that the randomization may have also failed in other significant ways.[11]

Further investigation revealed that the source of the problem appears to be the mode of data collection. The people choosing the sample recognized that the data gathering from the control group was to be conducted by phone, thus it made no sense to include those without phone numbers in the control group sample. Consequently, any individual assigned to the control group who did not have a phone number registered with FaCS was discarded from the control group.[12] Surprisingly, around 50 per cent of all payment recipients have not registered a phone number. In modern society, the lack of a phone is likely to pose a large barrier to economic and social connectedness. Not reporting a phone number, of course, is something quite different. But there may be systematic differences between people who report phone numbers and those who do not. This break down in random assignment leads $\widehat{\Delta}_1$ to be a biased estimator for the average treatment effect.

The results of randomization tests for the sample of individuals in the intervention group under 50 years of age with a recorded phone number are presented in column 5 of Table 1. This restricted sample exhibits only insignificant variation from the control group for all the variables considered. Furthermore, we fail to reject that the age and earning distributions are the same across the two groups. We considered–and rejected– the possibility that mean characteristics are now not significantly different because of the increased standard errors which arise from the smaller sample size.[13] Randomization thus appears to hold for this subsample of the data. The large reduction in sample size necessary to use the random experiment framework (from 409 Wave 3 intervention group participants to 239) is an unfortunate consequence of the failure to achieve randomization in the initial assignment. Table 2 provides the original sample sizes which were selected, the reduction in sample size implied by the age only and phone and age restrictions, and the attrition in sample size from Wave 1 to Wave 3. The different arrows from the population to the selected samples indicate that different processes were used to draw the samples and that they are not, in fact, random.

[11] For age and earnings, we compare the entire distribution of the variable using a Kolmogorov-Smirnov test. (See Ashenfelter, Ashmore, and Deschênes (1999)). We reject the equality of the distributions.

[12] This was unrelated to the age problem. In fact, reporting a phone number is positively correlated with age.

[13] We informally checked this using the standard errors from the larger sample size with the estimates from the smaller sample size.

[Table 2 about here]

4.2 Dropout

An additional challenge for the experimental nature of the trial is the large numbers of people who were assigned into both the control and intervention groups, but who chose not to participate in the entire sequence of interventions and interviews.[14] In order for $\widehat{\Delta}_1$ to be an unbiased estimator of the average treatment effect, it must be the case that the decision to drop out is unrelated to those individual characteristics influencing outcomes. Correlation between the decision to fully participate once selected and individual characteristics can easily confound the effects of those characteristics and full program participation on outcomes.

This problem of dropout in and of itself is not a severe one.[15] One strategy is to consider the "effect of the intention to treat". This involves comparing average outcomes for the entire intervention group against average outcomes for the entire control group and is a parameter of interest that can easily be calculated in the face of treatment dropout. In fact, in many cases, this parameter better captures the true expected result from a new policy. Since any policy is likely to be imperfectly and incompletely implemented, including dropouts as well as those who are successfully fully treated provides a more realistic estimate of expected changes in outcomes resulting from the policy initiative. Furthermore, since Centrelink–which implemented the intervention–would also be charged with implementing any new policy, this "effect of the intention to treat" would in fact capture the impact of the actual contractual arrangements and incentive structures which would exist under any new policy.[16] As Reicken and Boruch (1978) note, "a carefully designed experiment with carelessly implemented treatments" may be the wisest method for exploring social innovation.

In our case, the main difficulty arises because we lack complete survey data for intervention and control group members who chose not to participate in an interview (or who were unable to be contacted). While FaCS was able to deal with any ethical concerns associated with the initial random assignment, legal and ethical constraints regarding data

[14] Although the language of compulsory attendance in the initial letter was strong, there was little effort made to penalize individuals who did not come in for an interview. Others who received letters contacted Centrelink by telephone and were told they did not actually have to attend interviews. Only limited records of these 'exemptions' and the reasons they were given were kept and we are therefore unable to distinguish between 'non-response' and 'dropout.' We will refer to all these as 'dropouts' in what follows.

[15] See Heckman et al, 2000.

[16] See Barrow (2000) for U.S. evidence on this point.

privacy precluded collection of data from those individuals opting out of the interview process. The magnitude of the dropout problem is quite substantial, as evidenced by Table 2, and as we discuss below, this makes the evaluation problem much more difficult. Fortunately, however, we can go some ways towards addressing this problem by merging survey data to administrative payments data for individuals selected for the trial. This is discussed in section 4.4 below.

4.3 Interview Technique

It is also important to note that different data gathering techniques–i.e., face-to-face or via telephone–were used for the intervention and control groups in the first two waves. Data for Wave 3 was gathered by the same market research firm in the same way for both groups. (See Table 2.) Systematic differences in responses across the groups may therefore be due to the survey technique itself and not due to the effect of the intervention. As we note below, this will complicate the interpretation of the results to a degree.[17]

4.4 Estimation Strategy

We adopt a two-pronged estimation strategy. First, the survey data from the trial will be used to construct the principle outcomes to be evaluated. These data are only available for those individuals participating in all waves of the survey, however. In what follows, therefore, we will focus on developing an estimation strategy to identify the "effect of full treatment on the fully treated" using this survey data. Second, we will estimate the "average treatment effect" and the "effect of intention to treat" using the administrative data which has subsequently become available. This requires complete outcome data for all participants. Therefore, using LDS data through June, 2001, we construct simple outcome measures for all individuals selected into both intervention and control groups–even those who did not participate in any part of the survey. If individuals no longer appear in the LDS database–i.e. have they moved off payments–then this itself may be considered as one outcome.

For LDS-based outcome measures we use the randomized experimental estimator of (5) on the subset of the data for which randomization holds: i.e., the control group

[17] One potential difficulty arises from the fact that Centrelink carried out the pilots with staff whose normal function is to assist customers with income-support and work needs. This raises the common problem that while keen and committed for the most part, many of these staff may have unintentionally subverted the experimental and data aspects of the project because of contrary beliefs about what was best for the customer. (Riecken and Boruch, 1978, discuss this problem in the context of ecological experiments carried out by environmentalists.)

compared to the age and phone restricted intervention group. Estimation of the effect of full treatment on the fully treated from the survey data, however, will require a more sophisticated strategy. It is to this that we now turn.

The comparison we wish to make is between those who fully participated in the intervention and those in the control group who would have fully participated had they been selected for the intervention group. This entails estimating an altered version of (4),

$$\Delta_2^{TOFT} = E(Y^1 - Y^0 | X, P^* = 1) \tag{6}$$

where $P^* = 1$ when an individual completes the entire treatment and 0 if an individual drops out at any point. This parameter is the effect of treatment on the fully treated. The practical difficulty in estimating this parameter is that we need to determine which control group members would have participated in the treatment had they been assigned to the intervention group.

Sample selection models (Heckman, 1979) and matching estimators (see Smith and Todd, 2000; Blundell and Costa Dias, 2000 for reviews) both provide ways to estimate (6).[18] In this paper, we adopt a matching estimator approach because it allows us to account for dropout without overly restrictive structural assumptions. (See Heckman, et. al., 1997.) The basic idea is to match intervention group members participating in the full treatment with control group members who have similar characteristics. If observable characteristics capture all key determinants of participation, then differences in outcomes for the matched individuals may be attributed to the intervention. The hope is that those control group individuals (with similar observed characteristics) would also have participated in the full treatment had they been randomized into the intervention group. We thus deal with the problem of not observing P^* when $S = 0$ and this allows estimation of "full treatment on the fully treated"–the effect of complete program participation on those who choose to participate.

To implement this we use the LDS data to combine multiple characteristics into a "propensity score" which summarizes the relationship between participation and individual characteristics. Specifically, for the intervention group, we estimate a logit model of the probability of participating in the full treatment as follows

$$p_i^I = F\left(z_i' \delta_1\right) \tag{7}$$

[18] The possible set of estimators in the case of non-random assignment is large. (See Heckman, et al. (1999).) Alternatively, it may be possible to examine the outcomes of trial participants using the LDS administrative data which will become available in coming years. In addition to providing a more long-term analysis, the different partial treatments that were received might also be evaluated. The LDS will contain information on all people who continue to stay in the income-support system, even those who did not participate in the intervention or control group data collection process.

where p_i^I takes value 1 if an individual fully participates in the treatment, $p_i^I = 0$ whenever the person drops out at any point in the treatment, and z_i are characteristics from the administrative data. Using these estimates, we can create the propensity score (predicted probability) for each individual in the intervention and the control groups. Individuals with similar propensity scores have similar probabilities of complete participation in treatment. We then use the matched data to estimate

$$d_i = y_i^1 - y_{i*}^0 \tag{8}$$

where the outcome for individual i in the intervention group, y_i^1, is compared to the outcome for his or her counterpart(s) i^* in the control group. Each intervention group member can be compared to a single individual in the control group or to multiple control group members. We employ kernel propensity score matching, which involves taking a weighted average of multiple control group individuals with propensity scores similar to the specific individual in the intervention group. Weights are positively related to the similarity in the propensity scores for the specific control group member under consideration and the intervention group member for whom we are seeking a match. The difference estimator for individual i is then

$$d_i = y_i^1 - \frac{1}{n_o}\sum_{j=1}^{n_o} K\left(\frac{\widehat{p}_i^I - \widehat{p}_j^C}{h}\right) y_j^0 \tag{9}$$

where $\widehat{p}_i^I$ is the propensity score from (7) for individual i in the fully treated group, $\widehat{p}_j^C$ is the propensity score from (7) for individual j in the control group, and y_j^0 is the observed outcome for that individual.[19]

In order to interpret the matching estimator as a measure of the impact of full treatment on the fully treated, we make two critical (though standard) assumptions:[20]

(A1) First, once observable characteristics have been accounted for, outcomes are independent of participation status; and

(A2) second, there is some range of observable values of X that are common to both the control and the intervention groups.

[19] We use a standard normal kernel for the weight function K and a bandwidth, h, chosen by using Silverman's (1986) suggested robust bandwidth for density estimation. We tried bandwidths ranging from .001 to .05 and the results are insensitive to choice of bandwidth.

[20] These assumptions are discussed in detail elsewhere, see recent review articles by Smith and Todd (2000) and Blundell and Costa Dias (2000).

Given random assignment and full participation (A2) would hold trivially. While we do believe that randomization holds for the age and phone restricted samples, dropout from the control group remains a problem. In the face of differential dropout from the control group, we can no longer assume that the common support condition will hold in Wave 3 when we measure outcomes–even if we have random assignment. We investigate this by estimating a logit model of the probability of remaining in the sample at Wave 3 separately for the intervention and control groups. (See Table A1 in Appendix 1). Using a likelihood ratio test, we are unable to reject the hypothesis that the two participation processes are the same.[21] Further, Figure 1 provides nonparametric density estimates of the propensity scores for the intervention and control group. Their similarity provides some evidence that the common support condition (A2) holds.

We are somewhat less sanguine about assumption (A1). The administrative data used to generate the propensity scores is lacking several key variables that would seem to be important in accounting for participation. However, Augurzky and Schmidt (2001) argue that the success of propensity score matching does not depend necessarily upon having a consistent estimator of the selection equation (in contrast to the standard, parametric selection model discussed further in the robustness section below). Mis-specification of the participation equation does not invalidate the matching procedure so long as important variables affecting both participation and outcomes are included in the model.

5 The Impact on Social and Economic Participation

5.1 Results from the Survey

Our focus is on five specific outcome measures: paid work, job search, study and training, voluntary work, and social integration.[22] We will consider total hours of each of these activities, the percentage of people participating in each of these activities, and a combined measure of job-market related activity where we aggregate the first three outcomes. The left-hand side of Table 3 contains the estimated impact of the intervention on hours while the right-hand side provides the effect on participation.

For each activity, we present the intervention group average, which is based upon the sample average of the age and phone restricted group, and a weighted control group "average" based upon the matched individuals from the control group. The estimated

[21] We also find no significant difference in the participation process in Wave 1 for the two groups.

[22] We define social integration as a dummy variable which equals 1 for people who meet socially with friends more than once a week or who belong to a club or community association and 0 otherwise.

impact of the intervention is

$$\widehat{\Delta}_{2,Wave3} = \frac{1}{n_m}\sum_{i=1}^{n_m} d_{i,Wave3} \tag{10}$$

where d_i is given by (9) measured using the data at Wave 3 and n_m is the matched sample size.[23] We refer to this as the "cross-sectional" estimator since it is based upon comparison of control and intervention groups at one point in time (Wave 3).

[Table 3 about here]

The intervention resulted in a significant positive effect on study or training.[24] The number of hours spent in study or training each week was higher for intervention than control group members and there was a significant 5.3 per cent difference in the proportion of individuals who took up study or training between the two groups. This appears to be the result of people undertaking study and training activities which were agreed to in the Participation Plans formed during the initial face-to-face interview.

Intervention group members also had a higher rate of social integration. The participation rate in social activity with friends or club membership was 7.5 per cent higher for the intervention group than for the control group. These propensity score matching estimators control for differential dropout processes in the control and intervention groups. They are based upon the age and phone restricted sample since we feel that this is the group for whom the assumptions required for the matching estimator to be valid are most likely to hold. Despite the focus of numerous questions on voluntary activity, we find no significant effect on hours or participation in voluntary work from the intervention.

The other significant result of the intervention is a significantly smaller number of reported hours worked by the intervention group. There is no significant difference in participation in paid work between the two groups. The significant difference in hours may be driven by a differential tendency to under-report as a result of the intervention. Control group members knew they were not being interviewed by Centrelink. Intervention group members were told that a benefit review was part of the interview process, creating

[23] We drop observations for which there is no 'good' match. This is defined as observations for which there are no similar propensity scores in the control group. In our estimates we never drop more than two observations.

[24] In our analysis, we compare outcomes from full treatment to outcomes for individuals in the control group. If one is willing to assume that being in the control group does not affect behaviour, then we can think of this comparison as being between full treatment and no treatment. Given the previous discussion about the effect of receiving a letter for this group of long-term unemployed, it may be better to view this as a comparison of two different kinds of treatment. The treatment for the control group is receipt of a letter and the data gathering process itself.

some incentive for under-reporting of hours worked. Even though the Wave 3 data were collected by a market research firm for both intervention and control groups, it is possible that this incentive to under-report hours worked remained since intervention group members were aware that this third interview was a continuation of the treatment and data collection process.

5.2 Results from Administrative Data

Availability of the LDS provides an opportunity to examine longer-term outcomes and to examine outcomes for all individuals assigned to the intervention group, including those who chose not to participate. Recently, FaCS has made available further LDS data which extend through 28 June 2001–two months beyond the end of the trial. Future releases of the LDS will provide additional opportunities to examine the outcomes beyond this date.

Using data through 28 June 2001 does not add much in terms of time to the observation window, but it does allow us to answer several important questions which can not be addressed using the survey data. Did assignment to intervention or control group, irrespective of participation in the trial, have any effect on outcomes? Were outcomes different for those who fully participated in the trial, compared to an average individual?[25] We can also use the administrative data to create further measures of "full treatment on the fully treated"–provided that selection into Wave 3 control and intervention group participation is the same.

Focusing on these questions, we consider five outcomes: movement off payments and to a different payment type, presence of earned income, average earned income, and average earned income for those with income. We use average values over the last two fortnights of data (1 June through 28 June, 2001) to construct each outcome measure in order to eliminate high-frequency variation[26]. We compare the control group to the intervention group restricted by phone and age since we believe randomization holds across these two groups. The second column of Table 4 addresses the effect of intention to treat. We do not find any significant difference in outcomes between those assigned to the intervention and control groups. The third column of Table 4 compares the intervention group for whom we have Wave 3 data (the fully treated) to the full control group. This corresponds to treatment on the treated–comparing those who fully

[25] These correspond to the "effect of intention to treat" and "treatment on the treated" discussed above.

[26] People often leave the data for one fortnight only to return soon thereafter. This may not be reasonably viewed as movement off payment.

average individual and allowing the decision to participate to be part of the effect. For this group, we find that intervention group members are more likely to remain on payment and are more likely to still be receiving Newstart allowance.[27] This is not surprising. We know that intervention group members are more likely to have enrolled in study or training programs. These programs are compatible with continuing receipt of payments and participation in such programs may mean that people are less likely to leave payments for employment. Although the short term effect of this may be that program participants are more likely to remain on payments, it is possible the long run effect may be to move people off payments more effectively. Furthermore, additional study and training is expect to help people move to better quality outcomes when they leave payments. Future study of the LDS should be enlightening in this regard.

[**Table 4 about here**]

Comparison of Wave 3 control and intervention groups is in column four. The patterns of significance are the same as for our estimate of "treatment on the treated." Interestingly, we find further confirmation of our result from the survey data that average earnings and the proportion of people with paid work are not affected by the intervention.

5.3 The Robustness of the Results

In addition to the cross-sectional propensity score matching estimator, we consider three other estimators of the intervention impact. The first is the cross-sectional randomized experiment estimator (5) based upon comparison of intervention and control group means at Wave 3. This estimator does not allow for any persistent, time-invariant differences between the two groups in initial levels of economic and social activity. To deal with this, we also calculate a difference-in-differences (DID) estimator by comparing the change in activity level between Wave 3 and Wave 1 for both intervention and control groups. Time invariant differences in the two groups are thus "differenced away." The experimental difference-in-difference estimator becomes

$$\widehat{\Delta}_{1,DID} = \left(\overline{y}^1_{Wave3} - \overline{y}^1_{Wave1}\right) - \left(\overline{y}^0_{Wave3} - \overline{y}^0_{Wave1}\right) \tag{11}$$

while the matching difference-in-difference estimator is

$$\widehat{\Delta}_{2,DID} = \widehat{\Delta}_{2,Wave3} - \widehat{\Delta}_{2,Wave1}. \tag{12}$$

Tables A2, A3, and A4 in Appendix 1 present all four estimates for hours of economic activity, participation in economic activity, and voluntary and social participation.

[27]Movement to disability payment or parenting payment single were the two most common non-Newstart payments for both intervention and control groups.

The cross-sectional randomized experiment estimator is only valid if randomization holds and if dropout from initial assignment to Wave 3 is identical. The two difference-in-difference estimators allow for systematic differences in the initial starting levels of activity for intervention and control groups, but the assumption is that all changes over time are due to the effect of the intervention.[28] Although we present the DID results for completeness, we prefer the cross-sectional estimators. First, although our analysis of the LDS data suggests the dropout process was the same for both the control and intervention groups (Table A1), the LDS data provide us with only a limited set of covariates with which to test this proposition. Second, and more importantly, the method of data collection changed from face-to-face interview to phone interview between Waves 1 and 3 for the intervention group. This is problematic since it seems likely that the method of data collection affects the measured levels of economic and social participation.[29] The DID estimates will erroneously attribute any effect of this change in interview technique to the intervention.

The main point to note from these other estimators is that the cross-sectional randomized experiment estimators are quite similar to the cross-sectional propensity score matching estimators. This would seem to provide evidence that the dropout processes from selection to Wave 3 were in fact similar. The randomized design appears to work for the age and phone restricted sample.

To further test our results, we attempted to improve the quality of our propensity score matching by including the Job Seeker Classification Index (JSCI) score for each individual in the participation equation. This index is designed to be a summary measure of a person's employability and is used to determine eligibility for various employment assistance programs. However, the JSCI was not significantly related to the probability of fully participating in the treatment. In addition, a lack of JSCI scores for all participants entailed further decreases in sample size which increased the standard errors of our estimates and led to insignificant estimates of treatment impact. We thus decided to drop

[28] One reason to believe that initial starting levels may have in fact not been identical is that information about which sites were selected for the intervention was provided to some other government agencies. We know that at least some potential participants at those sites were chanelled into another government program which made them ineligible for this intervention. This failure of randomization might not be picked up by the variables available in the LDS, but the oberved difference in levels of economic activity at Wave 1 may be a result of this.

[29] In separate work, we have analyzed the detailed information on barriers to economic and social participation available in Wave 1 data. Intervention group members were much more likely to report multiple barriers than control group members. It appears that this was due to prompting by interviewers in the face-to-face interviews. This provides evidence that interview technique matters in the measurement of some outcomes.

the JSCI score and use the larger samples.

We also investigate whether the results of the matching estimates were sensitive to the choice of matching algorithm. Instead of a kernel approach, we used k-nearest neighbor (k-NN) matching. In k-NN matching, each intervention member is compared to the k individuals in the control group whose propensity score most closely matches his own. We set k equal to 1, 5, 10, 20, and 30. When k=1, we found that almost every estimate was insignificant due to the high standard errors. For the other values of k, we found results that were nearly identical to the results from the kernel matching technique reported here[30]. Thus, our results appear robust to a number of alternative specifications.

Finally, we estimated a standard Heckman (1979) selectivity correction model. This model produced large and unbelievable, negative estimates of the impact of the intervention. Identification of this model requires correctly specifying both the participation and outcome equations and including all relevant variables in the estimation. It also relies upon strong parametric assumptions. We conclude that the assumptions necessary for the matching estimator to generate valid estimates are much less burdensome, and much more likely to be met, than those needed for the structural modelling approach. (See Augurzky and Schmidt, 2001.)

6 Discussion

It is useful to put these results in the context of the growing international literature on active labour market program evaluation. The intensive interview process that formed the basis of the current intervention had elements of 1) counseling, 2) increased monitoring of program eligibility, and 3) the provision of job search assistance. "Counseling" was a major part of the intervention and took the form of referrals to other educational, training, or counseling programs. Individuals were also encouraged to engage in goal setting and planning for the future. At the same time, the letter recruiting the intervention group[31] and the establishment of regularized contact with government officials may have also affected behaviour through a type of monitoring effect. Job search assistance was not specifically an objective of the intervention, but may have formed a small part of the interview process.

Previous researchers have generally focused on evaluating the effects of such interventions on unemployment-related outcomes including: the intensity of job search, time

[30] Full results using the JSCI score and the results of the k-NN matching are available from the authors.

[31] The initial contact letter sought participation in the interview process by linking it to a review of benefit eligibility. See Appendix 2.

unemployed (or the exit rate to employment), total benefit receipt, or employment earnings. We are unaware of any research specifically analyzing the effect on hours spent in training, volunteer work, or the broader measure of social participation we have considered here. Nonetheless, this literature is useful for shedding light on the results of the current study.

Björklund and Regnér (1996), Meyer (1995), and Heckman, et. al. (1999) review the results of a number of previous studies evaluating the effects of job search assistance on the unemployed using random assignment.[32] Intensive job search assistance seems to significantly increase exit into employment in a variety of situations. For example, Meyer (1995) reports the results of five experiments in which increased monitoring was combined with job search assistance. Although the mix of enforcement and job search assistance (specifically, referrals and job placements) differed between the experiments, in four out of five cases the benefits of the overall intervention exceeded the costs by a wide margin leading to the conclusion that a range of approaches seem to be successful.[33] At the same time, there is evidence that enforcement or monitoring–on their own–may not be effective in altering the employment outcomes of unemployed individuals. Ashenfelter, Ashmore, and Deschênes (1999) study the effects of increased monitoring of job search activity on the employment outcomes of unemployed individuals in the United States. They conclude that more intensive monitoring did not significantly reduce the length of unemployment or overall benefit receipt and that the enforcement of sanctions was not cost-effective.

Not surprisingly, both the intensity of the intervention and the degree of disadvantage faced by the target population are key factors in the measured success of monitoring, counseling, and job search assistance programs in moving unemployed individuals into work. Relative to these previous studies, the intervention considered here is very modest indeed. Individuals undergoing the complete treatment participated in two face-to-face interviews approximately two months apart. In these interviews individuals were asked a range of questions designed to identify their barriers to employment and to assist with long-term goal setting. Counselors also made referrals to other programs.[34] In contrast, van den Berg and van der Klaauw (2001) evaluate a counselling and monitoring program

[32] Results based on non-experimental methods appear to result in somewhat different conclusions–at least with respect to the effects of increased monitoring (Meyer, 1995)–and will not be reviewed here.

[33] Meyer (1995) also notes that one advantage of job search assistance programs relative to measures such as re-employment bonuses is the potential for the latter to increase the supply of unemployed individuals.

[34] There was a final phone interview of individuals in the intervention group approximately four months after the initial interview, though this was part of the data collection process rather than the intervention per se.

which consisted of a 45 minute meeting with a counselor at the local unemployment agency every four weeks for six months. During these meetings, the quality of applications and resumes are examined, methods of job search are discussed, and a job search strategy for the intervening four weeks is developed. Gorter and Kalb (1996) also use Dutch data to evaluate the effectiveness of an intensive job assistance program in which–in addition to "counselling and monitoring"–counselors provided a general overview of job vacancies pointing out particularly suitable vacancies to each individual. These interventions were obviously much more intensive than the one considered here, yet the estimated impact was modest. van Den Berg and van der Klaauw (2001) conclude that the "monitoring" component of the intervention was inefficient because it simply resulted in the substitution of "formal" for "informal" methods of job search, while the counseling component of the intervention did not entail an increase in search effectiveness. The more intensive job assistance program increased the job finding rate of unemployed individuals who previously had been employed permanently, but reduced it for those moving into unemployment from temporary employment (Gorter and Kalb, 1996).[35]

The relative degree of disadvantage–both in terms of personal job readiness and general labour market conditions–faced by unemployed individuals is important as well. In the van den Berg and van der Klaauw (2001) study unemployed workers had relatively good labour market prospects and were seeking work in a period of favourable conditions in the Dutch labour market. Larger effects were observed in the Gorter and Kalb (1996) study perhaps because individuals were relatively more disadvantaged or because macroeconomic conditions were worse. (See also Dolton and O'Neill, 1995, 1996; White and Lakey, 1992). In our case, although Australian labour market conditions were relatively favourable during the period in which the trial was conducted, the unemployed individuals participating in the trial were extremely disadvantaged. All were unemployed and had been on income support for at least five years, making them among the most entrenched in the Australian welfare system. Thus, it may be the case that while counselling and monitoring is relatively ineffective in increasing the rate at which unemployed individuals with relatively good labour market prospects find jobs (as in van den Berg and van der Klaauw, 2001), it appears it is also not effective for marginalized workers with larger barriers to employment.

[35] See also the studies reviewed in Meyer (1995).

7 Conclusion

Welfare reform in Australia centres on the concept of both economic and social participation. The policy concern is that people who fail to participate in economic and social life may become entrenched in disadvantage. The data gathering and randomized trial discussed here was initiated by the Department of Family and Community Services in order to assess whether an intensive interview with successive follow-ups would result in increased economic and social participation for those unemployed individuals who had been on income support for more than five years. Our evaluation of this trial suggests that this intervention was mildly successful in increasing economic or social participation. Hours of study and training are significantly greater for those individuals who participated in the trial relative to the control group. We also found a higher proportion of individuals were engaged in study or training in the intervention group than the control group. At the same time, reported weekly hours of work fell, though this may be a result of including a benefit review as part of the intervention. Social participation, as measured through interaction with friends and family and membership in social organizations, was higher for intervention group members than for control group members. We conclude that the intervention had no significant effect on job search, volunteer work or the proportion of individuals in employment.

That these results are modest is perhaps not surprising. The Department of Family and Community Services, in designing the intervention, had no expectation that several short interviews would result in large-scale movements of the long-term unemployed into jobs and off of payments. Instead the trial was intended to quantify barriers to economic and social participation and to begin to identify strategies for removing them. The result may in fact be increased social participation, which though not apparent in voluntary work, is reflected in the social integration measure we considered. It may be that such increases in social participation, even those unrelated to job skills, might put people on a path towards greater economic and social participation which will yield long-term results.

Further research in several areas therefore seems desirable: first, has social participation, as gauged by measures broader than the ones considered here, increased as a result of the experiment?; second, do short-term increases in social participation lead to long-term economic outcomes including movement off of payments?; third, do stronger and more intensive interventions lead to changes for the very-long term unemployed?; and fourth, are there any long-term effects of this intervention? This latter question can be addressed by using administrative data which will become available in the future.

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Table 1: Randomization Tests

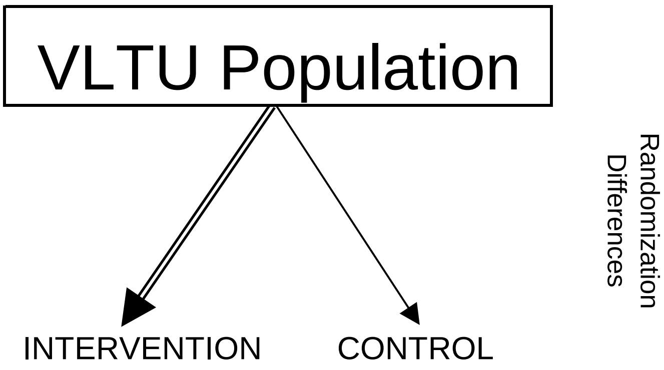
Average of Variable	Control Group	Intervention Group: Full Sample	Intervention Group: Age Restricted	Intervention Group: Age & Phone Restricted
Age[a]	36.01	36.71 **	35.23 *	35.72
Gender				
Female	0.28	0.25 *	0.25 *	0.31
Marital Status				
Married	0.27	0.23 **	0.22 **	0.26
Separated, widowed, divorced	0.23	0.27 **	0.27 **	0.22
Never married	0.50	0.50	0.52	0.51
Family Status				
Has dependent children	0.20	0.16 **	0.16 **	0.19
Number of children	0.49	0.36 **	0.38 **	0.43
Number of children (for those with children)	2.38	2.28	2.33	2.29
Age of youngest child	1.20	0.95 **	0.94 **	1.21
Age of youngest child (for those with children)	5.93	5.96	5.73	6.47
Current Earnings (6 months to 16 June 2000)				
Has reported earnings	0.30	0.23 **	0.23	0.28
Average earnings[a]	65.50	43.19 **	42.98 **	62.13
Average earnings[a] (for those with earnings)	215.14	185.37 **	184.12 *	225.60
Country of Birth				
Australian born	0.86	0.85	0.86	0.85
Overseas born: English speaking background	0.05	0.06	0.06	0.05
Overseas born: non-English speaking background	0.09	0.09	0.09	0.10
Indigenous status	0.02	0.03 *	0.03 **	0.02
Location				
Inner capital city	0.12	0.14	0.14	0.14
Outer capital city	0.27	0.26	0.26	0.26
Other urban	0.25	0.25	0.25	0.23
Rural	0.36	0.35	0.35	0.37
Education				
Less than year 10	0.32	0.39 **	0.39 **	0.35
Completed year 10	0.33	0.31	0.31	0.31
Completed year 11	0.08	0.07 **	0.08	0.09
Completed Year 12	0.13	0.10 **	0.10 **	0.12
More than Year 12	0.15	0.13 *	0.12	0.13
Sample size	1800	2940	2729	1082

* Control and intervention group mean or distribution is significantly different at the 5 per cent level.

** Control and intervention group mean or distribution is significantly different at the 1 per cent level.

a: Kolmogorov-Smirnov distribution test for equality of the entire distribution

Table 2: Sample Sizes and Data Issues



	INTERVENTION	CONTROL
Letters Sent	**2940**	**1800**
Participated Wave 1	**1520**	**728**
Participated Wave 2	**1221**	**560**
Participated Wave 3	**409**	**429**

Dropout

Sample sizes for restricted subsamples of the intervention group

	INTERVENTION (AGE RESTRICTED)	INTERVENTION (AGE & PHONE RESTRICTED)
Letters Sent	**2729**	**1082**
Participated Wave 1	**1373**	**578**
Participated Wave 2	**1097**	**473**
Participated Wave 3	**334**	**239**

Dropout

Data gathered in face-to-face interview: ☐
Data gathered in phone interview: ▨

Figure 1
Propensity Scores for Control Group
and Age and Phone Restricted Intervention Group

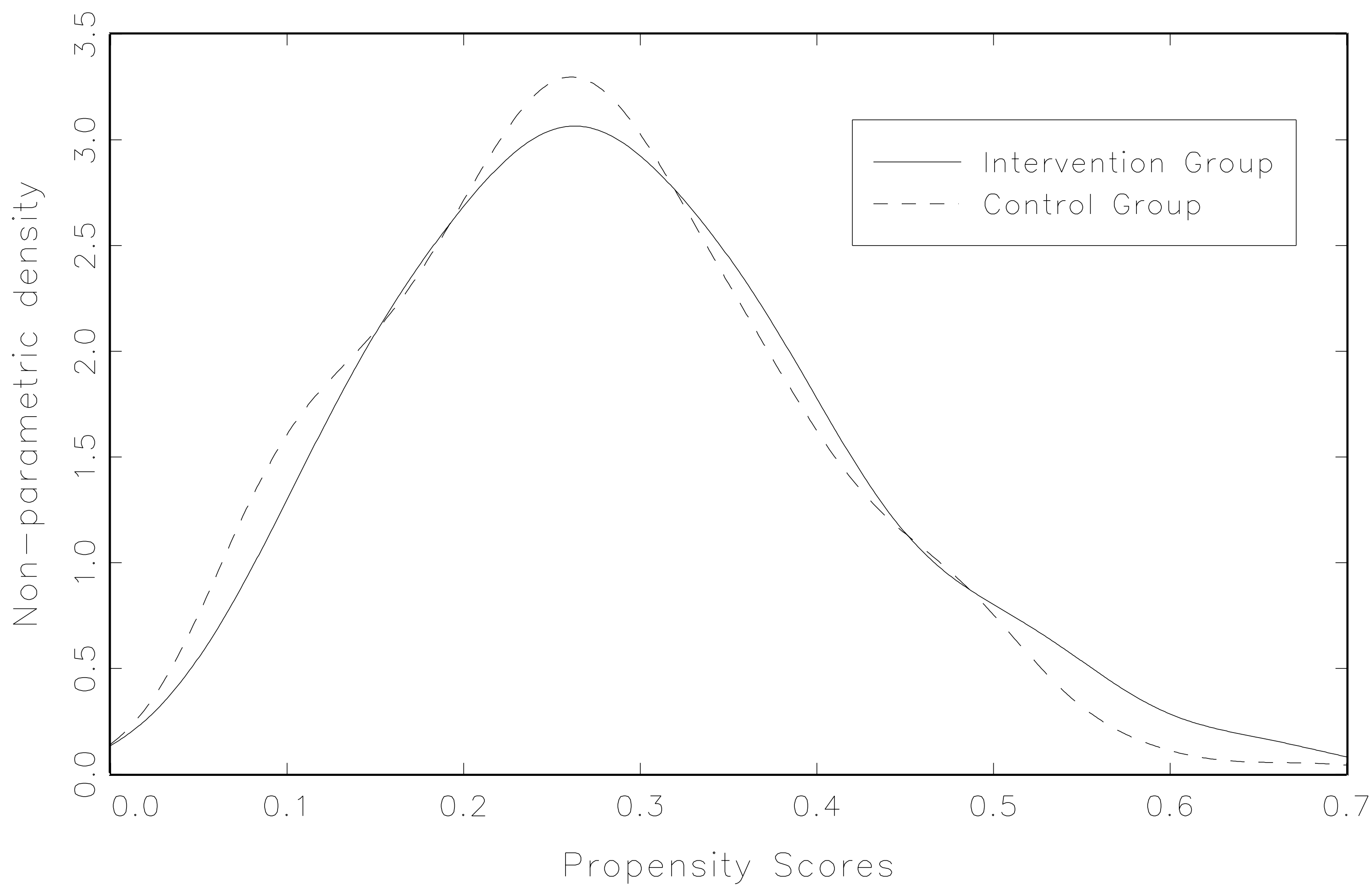


Table 3: Results from Intervention

Economic Participation Measures

Average weekly hours working		
Intervention	3.64	
Control	5.88	
Impact Estimate	**-2.24**	***
Standard Error	**(0.75)**	

Proportion working		
Intervention	0.299	
Control	0.349	
Impact Estimate	**-0.05**	
Standard Error	**(0.038)**	

Average weekly hours looking for work		
Intervention	7.04	
Control	7.56	
Impact Estimate	**-0.52**	
Standard Error	**(0.76)**	

Proportion looking for work		
Intervention	0.751	
Control	0.755	
Impact Estimate	**-0.004**	
Standard Error	**(0.036)**	

Average weekly hours studying or training		
Intervention	2.72	
Control	1.57	
Impact Estimate	**1.15**	**
Standard Error	**(0.55)**	

Proportion studying or training		
Intervention	0.176	
Control	0.123	
Impact Estimate	**0.053**	*
Standard Error	**(0.030)**	

Average weekly hours all three above		
Intervention	13.65	
Control	14.97	
Impact Estimate	**-1.31**	
Standard Error	**(1.16)**	

Proportion any three above		
Intervention	0.841	
Control	0.846	
Impact Estimate	**-0.005**	
Standard Error	**(0.030)**	

Voluntary and Social Participation Measures

Average weekly hours voluntary work		
Intervention	1.73	
Control	1.24	
Impact Estimate	**0.49**	
Standard Error	**(0.406)**	

Proportion Engaged in Voluntary Work		
Intervention	0.236	
Control	0.222	
Impact Estimate	**0.014**	
Standard Error	**(0.035)**	

Proportion Socially Integrated		
Intervention	0.717	
Control	0.642	
Impact Estimate	**0.075**	**
Standard Error	**(0.038)**	

Cross-sectional propensity score matching estimates. Bandwidth for kernel match is 0.038. Standard errors are bootstrapped.

***statistically significant at 1 per cent level; **statistically significant at 5 per cent level; *statistically significant at the 10 per cent level

Sample sizes vary due to missing data for some questions

Working: 234 Intervention, 402 Control.

Looking for work: 225 Intervention, 410 Control.

Studying/training: 239 Intervention, 429 Control.

Total economic activity: 220 Intervention, 387 Control.

Voluntary work: 236 Intervention, 425 Control.

Social integration: 239 Intervention, 429 Control.

Table 4: Results from Intervention
Age & Phone Restricted Group
Measures from Administrative Data

	All individuals assigned to intervention and control groups	Wave 3 intervention group participants compared to: All individuals assigned to control		Wave 3 intervention group participants compared to: Wave 3 Control group participants	
On payments during period June 1 - 28, 2001					
Intervention	0.916	0.983		0.983	
Control	0.913	0.913		0.937	
Impact Estimate	**0.003**	**0.070**	**	**0.046**	**
Standard Error	**(0.011)**	**(0.019)**		**(0.017)**	
Still on NewStart payment as of June 1 - 28, 2001					
Intervention	0.773	0.854		0.854	
Control	0.776	0.777		0.788	
Impact Estimate	**-0.003**	**0.077**	**	**0.066**	**
Standard Error	**(0.016)**	**(0.028)**		**(0.032)**	
Has earnings during period June 1 - 28, 2001					
Intervention	0.212	0.268		0.268	
Control	0.236	0.236		0.286	
Impact Estimate	**-0.024**	**0.032**		**-0.018**	
Standard Error	**(0.017)**	**(0.030)**		**(0.037)**	
Average earnings during period June 1 - 28, 2001					
Intervention	54.06	70.46		70.46	
Control	60.14	60.15		82.83	
Impact Estimate	**-6.08**	**10.31**		**-12.37**	
Standard Error	**(6.89)**	**(11.99)**		**(16.17)**	
Average earnings (for those with earnings) during period June 1 - 28, 2001					
Intervention	255.55	262.84		262.84	
Control	254.71	254.72		289.58	
Impact Estimate	**0.84**	**8.12**		**-26.74**	
Standard Error	**(24.14)**	**(36.85)**		**(45.07)**	

**statistically significant at 5 per cent level; *statistically significant at the 10 per cent level.

Sample sizes are as follows:

Column 2: 1082 assigned intervention and 1800 assigned control; 988 intervention and 1643 control in LDS June 2001; 209 intervention and 388 control with earnings

Column 3: 239 Wave 3 intervention and 1800 assigned control; 235 Wave 3 intervention and 1643 control in LDS June 2001; 209 intervention and 388 control with earnings

Column 4: 239 Wave 3 intervention and 429 Wave 3 control; 235 Wave 3 intervention and 402 Wave 3 control in LDS June 2001; 63 intervention and 115 control with earnings

Appendix 1

Table A1: Determinents of Participation in Wave 3
Age & Phone Restricted Group

	Age & Phone Restricted Intervention Group		Control Group	
	Odds Ratio	z-value	Odds Ratio	z-value
Age	1.027	2.18	1.034	3.72
Female	1.551	2.58	1.351	2.29
Marital Status				
Married	1.235	0.88	1.286	1.42
Separated	0.793	-1.08	0.599	-3.03
Never married				
Dependent Children				
Age of Youngest Child	1.015	0.54	1.026	1.21
Birthplace				
Australian born				
Overseas English speaking	1.347	0.86	1.025	0.09
Overseas non-English speaking	0.590	-1.73	0.414	-3.44
Living Circumstances				
Has moved in last 6 months	0.485	-2.29	0.597	-2.22
Own home	1.575	1.79	0.914	-0.46
Private rental				
Government rental	1.582	1.79	1.238	1.08
Boarding	1.712	2.30	1.718	3.07
Other arrangements	1.350	1.09	1.391	1.71
Location				
Inner Capital City	0.971	-0.12	0.609	-2.34
Outer Capital City	0.774	-1.22	0.658	-2.67
Other urban centre	0.665	-1.87	0.885	-0.81
Rural				
Earnings				
Reported earnings in six months to June 2000	1.044	0.24	1.096	0.70
Unearned Income				
Reported unearned income in six months to June 2000	1.111	0.46	1.441	2.08
Previous Activities since July 1995				
Length of time on income support	1.033	1.38	0.995	-0.29
Has participated in				
Intensive Assistance	1.583	2.61	1.326	2.14
Training Program	0.951	-0.28	1.168	1.12
Work for the Dole	2.026	2.70	1.526	2.16
Voluntary work	1.765	1.91	1.021	0.09
Has received an exemption	1.239	1.30	1.129	0.97
Has had administrative breach	0.620	-1.39	0.696	-1.39
Has had activity breach	0.630	-1.45	0.460	-3.06
Education				
Less than year 10	0.738	-1.53	0.721	-2.18
Completed year 10				
Completed year 11	0.806	-0.71	0.616	-1.90
Completed year 12	1.264	0.90	1.377	1.73
Post year 12	0.942	-0.24	1.050	0.28
Sample size	1082		1800	
Pseudo R square	0.085		0.080	
Per cent correctly allocated	63.31		62.56	

Table A2: Economic Participation Measures (Levels)
Age & Phone Restricted Group

	Randomized Experiment				*Propensity Score Matching* [a]			
Group	*Wave 1*	*Wave 3*		*Difference-in-Difference*		*Wave 3*		*Difference-in-Difference*	
Economic Participation Measures									
Average weekly hours working									
Intervention	3.13	3.64		0.51		3.64		0.52	
Control	4.51	5.78		1.27		5.88		1.28	
Impact Estimate		**-2.14**	***	**-0.76**		**-2.24**	***	**-0.76**	
Standard Error		**(0.75)**		**(0.63)**		**(0.75)**		**(0.66)**	
Average weekly hours looking for work									
Intervention	6.59	7.04		0.45		7.04		0.44	
Control	8.92	7.66		-1.26		7.56		-1.30	
Impact Estimate		**-0.62**		**1.71**	*	**-0.52**		**1.74**	*
Standard Error		**(0.74)**		**(0.98)**		**(0.76)**		**(1.01)**	
Average weekly hours studying or training									
Intervention	1.51	2.72		1.21		2.72		1.21	
Control	1.80	1.59		-0.21		1.57		-0.22	
Impact Estimate		**1.13**	**	**1.42**	**	**1.15**	**	**1.43**	**
Standard Error		**(0.56)**		**(0.65)**		**(0.55)**		**(0.65)**	
Average weekly hours all three above									
Intervention	10.82	13.65		2.83		13.65		2.83	
Control	15.12	15.05		-0.07		14.97		-0.06	
Impact Estimate		**-1.40**		**2.90**	**	**-1.31**		**2.89**	*
Standard Error		**(1.14)**		**(1.24)**		**(1.16)**		**(1.32)**	

a. Bandwidth for kernel match is 0.038. Standard errors are bootstrapped.

*** statistically significant at 1 per cent level; **statistically significant at 5 per cent level; * statistically significant at the 10 per cent level

Sample sizes vary due to missing data for some questions

Weekly hours working: 234 Intervention, 402 Control.

Weekly hours looking for work: 225 Intervention, 410 Control.

Weekly hours studying/training: 239 Intervention, 429 Control.

Total economic activity: 220 Intervention, 387 Control.

Table A3: Economic Participation Measures (Proportions)
Age & Phone Restricted Group

	Randomized Experiment			Propensity Score Matching [a]	
Group	*Wave 1*	*Wave 3*	*Difference-in-Difference*	*Wave 3*	*Difference-in-Difference*
Economic Participation Measures					
Proportion working					
Intervention	0.274	0.299	0.025	0.299	0.026
Control	0.301	0.341	0.040	0.349	0.042
Impact Estimate		**-0.042**	**-0.015**	**-0.05**	**-0.016**
Standard Error		**(0.038)**	**(0.032)**	**(0.038)**	**(0.033)**
Proportion looking for work					
Intervention	0.827	0.751	-0.076	0.751	-0.076
Control	0.781	0.759	-0.022	0.755	-0.024
Impact Estimate		**-0.008**	**-0.054**	**-0.004**	**-0.052**
Standard Error		**(0.036)**	**(0.036)**	**(0.036)**	**(0.038)**
Proportion studying or training					
Intervention	0.105	0.176	0.071	0.176	0.071
Control	0.126	0.124	-0.002	0.123	-0.001
Impact Estimate		**0.052 ***	**0.073 ****	**0.053 ***	**0.072** **
Standard Error		**(0.029)**	**(0.033)**	**(0.030)**	**(0.033)**
Proportion any three above					
Intervention	0.859	0.841	-0.018	0.841	-0.018
Control	0.840	0.850	0.010	0.846	0.007
Impact Estimate		**-0.009**	**-0.028**	**-0.005**	**-0.026**
Standard Error		**(0.031)**	**(0.027)**	**(0.030)**	**(0.028)**

a. Bandwidth for kernel match is 0.038. Standard errors are bootstrapped.

*** statistically significant at 1 per cent level; **statistically significant at 5 per cent level; *statistically significant at the 10 per cent level

Sample sizes vary due to missing data for some questions

Weekly hours working: 234 Intervention, 402 Control.

Weekly hours looking for work: 225 Intervention, 410 Control.

Weekly hours studying/training: 239 Intervention, 429 Control.

Total economic activity: 220 Intervention, 387 Control.

Table A4: Voluntary and Social Participation
Age & Phone Restricted Group

Group	Wave 1	*Randomized Experiment* Wave 3		Difference-in-Difference		*Propensity Score Matching* [a] Wave 3		Difference-in-Difference	
Voluntary and Social Participation Measures									
Average weekly hours voluntary work									
Intervention	1.74	1.73		-0.01		1.73		-0.01	
Control	0.90	1.24		0.34		1.24		0.32	
Impact Estimate		**0.49**		**-0.35**		**0.49**		**-0.33**	
Standard Error		**(0.41)**		**(0.39)**		**(0.406)**		**(0.391)**	
Proportion of Individuals Engaged in Voluntary Work									
Intervention	0.249	0.236		-0.013		0.236		-0.013	
Control	0.172	0.224		0.052		0.222		0.050	
Impact Estimate		**0.012**		**-0.065**	**	**0.014**		**-0.063**	*
Standard Error		**(0.034)**		**(0.032)**		**(0.035)**		**(0.033)**	
Social Integration									
Intervention	0.728	0.715		-0.013		0.717		-0.008	
Control	0.622	0.641		0.019		0.642		0.030	
Impact Estimate		**0.074**	**	**-0.032**		**0.075**	**	**-0.038**	
Standard Error		**(0.037)**		**(0.039)**		**(0.038)**		**(0.040)**	

a. Bandwidth for kernel match is 0.038. Standard errors are bootstrapped.

*** statistically significant at 1 per cent level; **statistically significant at 5 per cent level; *statistically significant at the 10 per cent level

Sample sizes for Voluntary Work measures: 236 Intervention, 425 Control.

Sample sizes for Social Participation: 239 Intervention, 429 Control.

APPENDIX 2
Letters sent to participants

CONTROL GROUP LETTER – 1ST INTERVIEW

«TITLE» «FIRST_NAME» «LAST_NAME»
«HOME_ADDRESS_LINE_2»
«HOME_SUBURB» «HOME_STATE»
«HOME_POSTCODE»

Dear «TITLE» «LAST_NAME»,

The Commonwealth Department of Family and Community Services would like to gather information on activities undertaken by customers in order to help us develop services that best meet your needs.

Your name was randomly selected from Centrelink records to participate in this very important research. The Department has contracted ACNielsen, an independent research company, to conduct this research and a representative from ACNielsen may contact you shortly by telephone. In order that the results will truly represent the situations of all customers it is important that each person contacted by ACNielsen participates. They may also ask you to take part in some shorter follow-up

Your comments will be treated confidentially. Participation in the survey will not affect any social security payments you currently receive. The research company will not identify individual respondents to the Department and the information gathered will only be used for research purposes.

We understand that your telephone number is «ISD» «PHONE». If it is not suitable for ACNielsen to call you on this number, please contact this office as soon as possible on 1800 018 312 . This is a toll-free number attended between 8.30am and 5.00pm Eastern Standard Time. You may also call 1800 018 312 if you wish to be interviewed in a language other than English, or if you have any queries.

Participation in the survey is voluntary and you may decline to participate at any time. However, we would greatly appreciate your co-operation as we can only improve our service if we know what customers need.

Thank you for your assistance.

Yours sincerely,

15 August 2000

Intervention group letter for 1st Interview for VLTU Pilot

Dear

I am writing to you to let you know you are required to attend an interview to discuss employment assistance options that may be available to you and to check that you are getting the right amount of Newstart Allowance. We will also discuss with you any plans you may have for the future and how we can help you.

As part of a new pilot programme, we are especially interested in finding out more about people in situations similar to yours and whether they might like more help in gettting access to services that are helpful to them. This pilot programme is confidential.

At this interview you will be required to negotiate and sign a Preparing for Work Agreement which will include activities you will undertake in order to maximise your chances of finding work.

If you do not attend this interview or make other arrangements, your payments will be stopped and a penalty will apply when you reclaim payments. If your payments are stopped we will write to you about this.

You need to ring the Centrelink Call Centre on *XXXX within the next 14 days* to make an appointment with a customer service officer. If, after you have booked an appointment, you cannot attend the interview please get in touch with us as soon as possible to make new arrangements. Our address and phone number are at the top of this letter.

If you have a Jobseeker diary, please bring it and this letter to the interview.

<u>Intervention group letter for 2nd Interview – VLTU</u>

Dear

I am writing to confirm an appointment for you to follow up on our meeting about two months ago. At that meeting we discussed your current activities and your plans for the future.

We have arranged to meet with you at:

Centrelink (address of office)
At (time) on (day and date).

If you cannot make the appointment at this time, please get in touch with me as soon as possible to make new arrangements. I can be contacted on [phone number]. It is important to note that if you do not attend this appointment your payment may be affected..

At the interview I will confirm your circumstances and discuss progress towards your goals and any further assistance Centrelink is able to offer.

The interview should take around 40 minutes. When you come to the interview please bring this letter with you. On arrival at the office, please hand this letter to the officer at reception.

Yours sincerely

[name]
[date]

INTERVENTION GROUP – SAID 'YES' TO 3RD INTERVIEW BUT NO PHONE NUMBER IN RECORDS – ASKED TO PROVIDE PHONE NUMBER

Box 7788
Canberra Mail Centre
ACT 2610
Telephone: (02) 6244 7788
Facsimile:
Email:
Website: www.facs.gov.au
TTY: 1800 260 402

Please quote: <<Unique ID>>

<<TITLE>> <<FIRST_NAME>> <<LAST_NAME>>
<<HOME_ADDRESS>>
<<HOME_SUBURB>> <<HOME_STATE>>
<<HOME_POSTCODE>>

Dear <<TITLE>> <<LAST_NAME>>,

I'm writing to thank you for being a part of the trials last year when you attended two interviews held by Centrelink staff. Most people have said how helpful and positive they found these interviews. We are now about to start on the third and final phase of these trials and need your help one more time. We would really appreciate the opportunity to talk with you **but do not have your telephone number on our record.**

This final phase will involve a short telephone interview lasting about 15-20 minutes, undertaken by A.C. Nielsen, a market research company. **Nothing you say in the interview will affect any payment that you or your family may be receiving.** If there is a telephone number where we can reach you, please contact this office as soon as possible on
1800 809 819. This is a toll-free number attended between 8.30am and 5pm (Eastern Standard Time) Monday to Friday. You may also call 1800 809 819 if you wish to be interviewed in a language other than English, or if you have any queries.

I sincerely hope that you will contact us very soon on the above number. Thanking you for your assistance.

Yours sincerely,

22 February 2001

INTERVENTION GROUP – SAID 'NO' TO 3RD INTERVIEW AND NO PHONE NUMBER IN RECORDS, ASKED TO RECONSIDER

Please quote: <<Unique ID>>

Box 7788
Canberra Mail Centre
ACT 2610
Telephone: (02) 6244 7788
Facsimile:
Email:
Website: www.facs.gov.au
TTY: 1800 260 402

<<TITLE>> <<FIRST_NAME>> <<LAST_NAME>>
<<HOME_ADDRESS>>
<<HOME_SUBURB>> <<HOME_STATE>>
<<HOME_POSTCODE>>

Dear <<TITLE>> <<LAST_NAME>>,

I'm writing to thank you for being a part of the trials last year when you attended two interviews held by Centrelink staff. Most people have said how helpful and positive they found these interviews. We are now about to start on the third and final phase of these trials and need your help one more time.

This final phase will involve a short telephone interview conducted by A.C. Nielsen, a market research company. At a previous interview with Centrelink you said you would rather not be phoned for a third interview. I would really like you to reconsider. The phone interview should take only about 15 minutes and it is very important to us, and vital for the project, that you take part. **Nothing you say in the interview will affect any payment that you or your family may be receiving.**

If you are able to take part in this short interview and there is a te;ephone number where we could reach you, please contact this office as soon as possible on 1800 809 819. This is a toll-free number attended between 8.30am and 5pm (Eastern Standard Time) Monday to Friday. You may also call 1800 809 819 if you wish to be interviewed in a language other than English, or if you have any queries.

I sincerely hope that you will contact us very soon on the above number. Thanking you for your assistance.

Yours sincerely,

22 February 2001

INTERVENTION GROUP – SAID 'NO' TO 3RD INTERVIEW – PHONE NUMBER IN RECORDS, ASKED TO RECONSIDER

Box 7788
Canberra Mail Centre
ACT 2610
Telephone: (02) 6244 7788
Facsimile:
Email:
Website: www.facs.gov.au
TTY: 1800 260 402

Please quote: <<Unique ID>>

<<TITLE>> <<FIRST_NAME>> <<LAST_NAME>>
<<HOME_ADDRESS>>
<<HOME_SUBURB>> <<HOME_STATE>>
<<HOME_POSTCODE>>

Dear <<TITLE>> <<LAST_NAME>>,

I'm writing to thank you for being a part of the trials last year when you attended two interviews held by Centrelink staff. Most people have said how helpful and positive they found these interviews. We are now about to start on the third and final phase of these trials and need your help one more time.

This final phase will involve a short telephone interview conducted by A.C. Nielsen, a market research company. At a previous interview with Centrelink you said you would rather not be phoned for a third interview. I would really like you to reconsider. The phone interview should take only about 15 minutes and it is very important to us, and vital for the project, that you take part. **Nothing you say in the interview will affect any payment that you or your family may be receiving.**

If you are able to take part in this short interview, please contact this office as soon as possible on 1800 809 819. This is a toll-free number attended between 8.30am and 5pm (Eastern Standard Time) Monday to Friday. You may also call 1800 809 819 if you wish to be interviewed in a language other than English, or if you have any queries.

I sincerely hope that you will contact us very soon on the above number. Thanking you for your assistance.

Yours sincerely,

22 February 2001

INTERVENTION GROUP – ASKING CUSTOMERS TO CONTACT ACNIELSEN FOR 3RD INTERVIEW – NO PHONE NUMBER RECORDED

Please Quote: <<UNIQUE ID>>

Box 7788
Canberra Mail Centre
ACT 2610
Telephone: (02) 6244 7788
Facsimile:
Email:
Website: www.facs.gov.au
TTY: 1800 260 402

<<TITLE>> <<FIRST_NAME>> <<LAST_NAME>>
<<POSTAL_ADDRESS >>
<<POSTAL_SUBURB>> <<POSTAL_STATE>>
<<POSTAL_POSTCODE>>

Dear <<TITLE>> <<LAST_NAME>>,

I wrote to you recently about being involved in a short telephone interview for the Department of Family and Community Services (FaCS). I would really value your involvement in this third and final interview, and would like to offer you another way to participate that might suit you better.

To make it easier to participate, ACNielsen, a market research company, have set up a **freecall** number for you to phone them on. It is **1800 642 837** and is open between **9.30am** and **9.00pm** from **2 April to 8 April 2001**. If you call this number, someone at ACNielsen will be able to organise an interview with you over the telephone.

This interview will only take about 15 minutes and will be a shorter version of the interviews you had with Centrelink late last year. The information you provide to ACNielsen will only be used for research purposes. **Your comments will be treated confidentially and nothing you say in the interview will affect any payment that you or your family may be receiving.**

If you have any other questions about the interview or would like to be interviewed in a language other than English, you can contact this office on 1800 809 819. This is a toll-free number attended between 9.00am and 5.00pm (Eastern Standard Time) Monday to Friday.

Your participation in this final interview is very important and will help us to better understand the needs of our customers and so improve our services to you.

Thank you for your assistance.

Yours sincerely

27 March 2001