A Couples-based Approach to the Problem of Workless Families

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Abstract
The goal of this paper is to evaluate a "couples-based" policy intervention designed to reduce the number of Australian families without work. In 2000 and 2001, the Australian Government piloted a new counselling initiative targeted towards couple-headed families with dependent children in which neither partner was in paid employment. Selected women on family benefits (who were partnered with men receiving unemployment benefits) were randomly invited to participate in an interview process designed to identify strategies for increasing economic and social participation. While some women were interviewed on their own, others participated in a joint interview with their partners. Our results indicate that the overall effect of the interview process led to lower hours of work among family benefit recipients in the intervention group than the control group, but to greater participation and hours in job search and in study or training for work-related reasons. Whether women were interviewed with their partner or not had no effect on the level of economic activity of participants.

Keywords: Income-support policy, randomised experiment, treatment effects
JEL Codes: H3, I38, J68

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1. Introduction

The shares of fully-employed and workless families have both risen over the past two decades leaving employment in many countries increasingly concentrated within certain households.[1] Shifts in family composition toward more single-adult households—in which rates of non-employment are typically higher—account for only a small fraction of the rise in the overall fraction of workless households (for example, Gregg and Wadsworth, 1996; 2000; Dawkins et al. 2002b). More important has been the increasing concentration of non-employment within households.

The impact of these changes on children is of particular concern. While the proportion of couple-headed households in which both partners are employed has increased, so too has the incidence of joblessness (for example, Gregg and Wadsworth, 1996; OECD, 1998; Dawkins et al. 2002a; Gregory, 1999; Dorsett, 2001). Substantial numbers of children now grow up in families reliant on income support. Between 1986 and 1999, the number of Australian children living in workless households more than doubled, leaving 1.2 million children—almost one in four—living in families reliant on income support (McCoull and Pech, 2000). Similarly, despite near-record levels of employment, nearly one in five British children now live in families in which no adult is in paid employment (Gregg and Wadsworth, 2000). Most troubling are the links between joblessness and poverty and the fear that children growing up in poor households have above-average probabilities of adverse outcomes as adults.[2]

Given these trends, it seems sensible to specifically target workless families when implementing labour market programs. In the U.K, for example, the New Deal

[1] See OECD (1998) and Gregg and Wadsworth (1996, 1998, 2000) for evidence on employment polarization in OECD countries generally, and in the United Kingdom in particular. Dawkins et al. (2002a, 2002b), Gregory (1999), and Miller (1997) discuss the Australian evidence.

[2] In Britain, 89.2 per cent of workless couples with children live in poverty (Gregg and Wadsworth, 2000), while 74 per cent of similar Australian families are in the poorest income quintile (Dawkins, et al, 2002b). Israel and Seeborg (1998) discuss factors influencing whether impoverished youth will escape poverty. In related reviews Haveman and Wolfe (1995) and Haveman et. al. (2001) discuss the results of a large literature linking family and community investments in children with children's subsequent outcomes.

for Partners offers–on a voluntary basis–job search assistance and training opportunities to partners of income-support recipients. Many young, workless couples without dependent children are required to file a Joint Claim for Jobseeker's Allowance (JSA).[3] The joint claim process requires both partners to be available for work and to accept equal responsibility for reporting any change in circumstances. This "couples-based" approach is consistent with recent research (see Dorsett 2001) which suggests that to be effective employment policies must explicitly take into account the joint (as opposed to individual) nature of labour supply decisions within families.

There is a large international literature pointing to an inverse relationship between husbands' unemployment and wives' labour supply (see Davies et al. 1992; Dilnot and Kell, 1987; and the references therein). While much of this can be accounted for by correlation between husbands and wives in key factors associated with non-employment (for example, low skills or poor labour market conditions), a large share is due to cross-couple state dependence (Davies et al. 1992). Tax and benefit-induced disincentives to work, inherent in many income-support systems (Dilnot and Kell, 1987), may contribute to low employment rates among the wives of unemployed men. Given this, policy initiatives to help workless couples must be undertaken within the context of the income-support system.

The goal of this paper is to evaluate one such "couples-based" policy intervention in Australia. Between September 2000 and April 2001, the Australian Department of Family and Community Services (FaCS) trialed a new counseling initiative targeted towards couple-headed families with dependent children in which neither partner was in paid employment. Selected women on Parenting Payments Partnered (see below), who were partnered with men receiving Newstart (unemployment) benefits were invited to participate in an interview process designed to

[3] See the website for the UK Department for Work and Pensions (http://sss,dwp.gov.uk) , and Bonjour et al. (2001; 2002) for more details about these two programs.

identify strategies to increasing economic independence. Although all unemployed individuals are obliged to look for work and some are required to undertake additional activities to increase their chances of employment, in practice most of the participants in this trial had been on income support for more than one year and had little contact (other than receiving their payment) with the income-support system after the initial 12 months of benefit receipt.

Given this, the intervention trailed by FaCS and analysed here was designed to address the following questions. Is the increased contact inherent in an intensive interview process helpful in increasing the economic participation of women in workless families? Further, are outcomes for family benefits recipients enhanced if—rather than attending on their own—they and their unemployed partners attend a joint interview in which a joint plan for increased economic activity is developed? Although our primary focus is on the economic activity of the family benefit recipient, we will also discuss the implications of the trial for the activity levels of unemployed partners.

Our results indicate that the interview process led to lower work hours among family benefit recipients in the intervention group than in the control group, but to greater participation and hours in job search and in study or training. At the same time, there were few significant differences in the effect of the interview process on the economic activity of women interviewed with and without their unemployed partners.

Both the background to and the implementation of the pilot are discussed in Section 2, while Section 3 outlines several methodological issues and describes the estimation strategy. Estimates of the impact of the intervention on the economic activity of women in workless families are presented in Section 4 of the paper. These estimates are based on two data sources—survey data from the pilots themselves and administrative data from the income-support system. Finally, we conclude in Section 5.

2. The Workless Families Pilot

The Workless Families Pilot was targeted towards workless Australian couples with school-aged children. This pilot was one of three randomised trials conducted by FaCS between September 2000 and April 2001 involving interviews with 10,504 income-support recipients nationwide. These trials were targeted towards especially disadvantaged groups—in particular, workless families, the very long-term unemployed, and mature-aged unemployment benefit recipients—who are in some sense outside the mainstream of Australian service delivery.[4] Evaluation of these trials was undertaken in order to inform a broader process of welfare reform.

2.1 Background

Australia—like many countries worldwide—is currently undergoing a process of welfare reform. Two key features of the system which are worth noting are: 1) Unemployment benefits are non-contributory and funded from general revenue; and 2) Program participants (for unemployment and other benefits) are entitled to receive benefits for an unlimited time period provided that they meet eligibility requirements.

The welfare reform process in Australia has primarily involved tightening the training and job search requirements for unemployment recipients and the introduction of job search requirements for other types of payments. The concept of "mutual obligations" has led to reforms that also require participation in government make-work programs or voluntary, or other socially constructive, activities. Against this backdrop, reducing the numbers of workless families has become a key policy objective. In particular, a recent task force (McClure, 2000) recommended that reducing the numbers of Australian families without work should be one of the government's three principal targets for welfare reform.

[4] See Breunig et al. (2003) for results of the trial targeting very long-term unemployed individuals.

2.2 Income support programs

The Workless Families Pilot affected participants in two separate welfare programs, which we describe briefly below.

Parenting Payment is paid to the primary carer of a dependent child under the age of 16 in low-income families. It consists of two separate programs—*Parenting Payment Single (PPS)* and *Parenting Payment Partnered (PPP)*—depending upon the relationship status of the parent who receives the payment. In this study, we only consider participants receiving PPP. PPP is paid to only one member of the couple. The level of payment is determined by the income of both partners, each of whom are allowed to work. On September 1, 2000, parents earning less than $558 per fortnight whose partner earned less than $531 per fortnight were eligible for payment.[5] A household asset test also applied. In our data, the woman received PPP in 90 per cent of cases. Thus, we frame our discussion in those terms. At the time of this study there were no job search or other activity requirements for PPP recipients.

Newstart Allowance (NSA) is a payment to unemployed individuals over the age of 21 who are capable of undertaking work and available to begin employment immediately. NSA is subject to income and asset tests and has a formal requirement that recipients engage in active job search. Individuals are allowed to work for a small number of hours before benefit reductions occur. A person's NSA status does not affect the receipt of PPP by his/her partner, except through the income test.

Both PPP and NSA are components of an income-support system managed by FaCS and administered by a large, service delivery organization known as Centrelink. There are also childcare allowances (Child Care Benefit) and tax credits (Family Tax Benefit) for low-income families who work. Families whose earnings make them ineligible for PPP or NSA payments would still receive some support through these

other programs. The important distinction is that families on PPP and NSA are receiving the majority of their income from the income-support system.

2.3 Implementation

The pilot was designed to assess whether an intensive interview with Centrelink staff and the development of a participation plan would improve economic and/or social activity among PPP recipients and their unemployed partners (NSA recipients). Some PPP recipients were interviewed with their unemployed partners, while others were interviewed alone. Interviewers specifically asked participants to begin thinking about and planning for the time when their children would reach the age threshold and the family would no longer be eligible for family benefits. Individuals (and couples) developed participation plans involving the identification and take-up of referrals to other government services, courses of study or training. As with Joint Signing for JSA claims in the UK (see Bonjour, et al, 2002), one goal of the intervention was to bring recipients in closer contact with Centrelink offices.

The process of the trial was as follows. Eligible Centrelink sites from across Australia were randomly chosen to participate in the trial.[6] Sites were selected with a probability proportional to the population in the pilot target groups. Selected sites were randomly assigned as 'intervention' (thirty-two sites) or 'control' sites (twenty-four) with three sites with large populations of the pilot target groups selected as both intervention and control group sites. Next, eligible customers from each site were randomly selected until specified quotas for the Parenting Payment target group had been reached.[7] Family benefit (PPP) recipients were randomly assigned for interview with or without their unemployed (NSA) partners.

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[5] Alternately, the partner could earn up to $974 per fortnight provided combined partner and parent income was less than the $1,089 per fortnight maximum. The maximum allowable household income increased by $15 per fortnight in mid-September of 2000.

[6]Eligibility was restricted to sites that had at least 30 PPP recipients. Each site is a cluster of Centrelink offices located in the same area and serviced by the same specialist Centrelink staff member who conducted the interviews. Intervention group sites comprised eighty-four separate Centrelink offices.

[7]One intervention site subsequently became unavailable so 31 sites were used.

Centrelink contacted each individual, including partners, selected for the intervention group by mail asking him or her to attend an interview (with or without his or her partner.) These letters formed one part of the intervention, which also involved two face-to-face interviews.[8] In the letter, individuals were told that failure to attend the interview would result in cessation of payments, but, in fact, no sanctions were applied to those who failed to attend or to schedule an interview. Subsequent participation in the trial was voluntary. The first set of interviews was conducted in September and October 2000 by Centrelink staff. For those who agreed to participate, interviewers administered a detailed questionnaire covering individuals' (and, where relevant, their partners') employment and educational background, current circumstances, and goals and aspirations regarding economic and/or social participation. The questionnaire also canvassed any barriers to increased participation faced by individuals so as to facilitate discussion about how participants could become more economically and socially active.[9] The outcome of that discussion was formalized in a participation plan, which may have included referrals to other government programs or assistance.

A second interview was conducted in November or December 2000. This interview was used to identify how participants' circumstances had changed and to determine implementation of the participation plans, such as the take up of referrals to job training or educational programs. An independent market research company conducted a final telephone interview in March and April 2001.

Comparison of survey data from the control and intervention groups from the follow-up telephone interview (Wave 3) and comparison of administrative data from both groups form the basis of the analysis of the impact of full participation in the trial.

[8] Copies of the letters sent to intervention and control group members are available from the authors.

[9] The intervention was designed to increase social as well as economic participation and there is some evidence that the interviews led to small increases in some forms of social participation. These results are available from the authors on request. In the remainder of the paper we focus on economic activity.

We define “full treatment” to be receipt of the letter and participation in the two face-to-face interviews. We analyse both the levels of economic activity reported at Wave 3 and the changes in economic activity from Wave 1 to Wave 3.

Control group members were likewise sent letters informing them of the interview process and were interviewed at the same points in time as the intervention group. The control group interviews were designed to gather comparable information to that obtained from intervention group members at the various stages of the trial.

There are thus two primary differences between the control and intervention groups—the formation of a participation plan and the much longer (and obviously costlier) face-to-face interviews with a Centrelink officer. Even if the same questions were asked of both groups, one might expect individuals’ responses (and subsequent actions) to differ when the questions, in the first case, are being asked over the phone by an independent market research firm relative to the second case where a member of the organization controlling the individual’s welfare payments is asking the questions.

The intervention was modest and there were no negative consequences for those who chose not to participate or who dropped out of the interview process before completing the entire intervention. This reflects the constraints under which FaCS attempted to test extending “mutual obligations” while avoiding exposure to criticism of being too harsh on income-support recipients.

2.4 Data sources

In this analysis we will make use of two data sources: detailed survey data from the pilot and administrative income-support data from FaCS's Longitudinal Data Set (LDS) merged to the pilot data. The LDS provides fortnightly observations on benefit details (including benefit levels, reported earned and unearned income, and duration of benefit receipt) and limited demographic characteristics (age of payment recipient, age of youngest child, and geographic area). The availability of these administrative data for

all individuals selected for the pilot (irrespective of whether or not they participated) allows us to test random assignment and to assess the factors related to an individual's decision to fully participate in the treatment. We discuss in more detail in the following sections how the administrative data were used.

3. Methodological Issues and Estimation Strategy

Random assignment into the control and intervention groups was intended to simplify estimation of the impact of the interview process on the economic activity of family benefit recipients (see Heckman et al. 1999). However, a failure to achieve complete randomisation, the change in interview methods between Waves 1 and 3, and dropout from both the intervention and control groups (all discussed further below) lead us to prefer a non-experimental, propensity-score matching estimator over the simpler experimental estimator. Still, the initial randomised design of the trial implies that intervention and control group members have similar observed characteristics and that outcomes and characteristics are generally measured in the same way for both groups. These data features greatly enhance our ability to use propensity score matching to estimate the impact of the intervention.[10]

3.1 Randomisation, Interview Methods, and Dropout

Analysis of our administrative data suggests that the initial assignment into the intervention versus control group is not completely random with respect to geographic location and nativity. Members of the control group are significantly more likely to live in large, capital cities, while intervention group members are significantly more likely to reside in towns with populations between 2,000 and 40,000 residents.

[10] In particular, Heckman, et al. (1997) point to these data features—along with subjects facing the same economic conditions—as being crucial in reducing the bias in evaluation studies. Similarity in economic conditions is less clear in this study as randomisation took place on the basis of geographic sites. Nevertheless, the regional variables we include should account for much of the variation in labour market conditions faced by subjects in the pilot. The control and intervention groups are fairly homogeneous and are amongst the most disadvantaged households in Australia.

Similarly, relative to intervention group members, individuals in the control group are more likely to be immigrants from a non-English speaking country, and less likely to be Australian-born.[11] These differences in local labour markets and nativity may be quite important in influencing the relative economic participation of pilot participants. This is one of the central reasons why we use an estimation approach (see below) to control for these differences instead of a mean comparison across treatment and control groups.

At the same time, comparing the characteristics of the family benefit recipients assigned to the "individual" as opposed to the "joint" interview intervention groups suggests that randomisation is valid for this comparison. Differences in the geographic distribution of these individuals—though significant—are small in magnitude. Thus, it appears that the overall difficulty in achieving randomisation between the control and the aggregated intervention group may stem from the process used to select intervention and control group sites and not with randomisation within site.

It is also important to note that although the same questionnaire was administered to intervention and control groups, different data gathering techniques—i.e., face-to-face and via telephone—were used for the control and two intervention groups in Waves 1 and 2. Wave 3 data were gathered by the same market research firm in the same way for all groups (see Table 1). Systematic differences in responses across the groups may therefore be due to the survey method itself and not due to the effect of the intervention. As we discuss below, this will complicate the interpretation of the results to a degree.

Table 1 Here

Finally, a substantial amount of dropping out occurred in both the control and intervention groups. This is perhaps not surprising as the interview process is lengthy and poor couples with children may face high costs in participating in the interview.

[11] These patterns are likely to be related to the geographic clustering of immigrants to Australia. Foreign-born individuals—in particular, those from non-English speaking countries—are heavily

Furthermore, due to ethical and political considerations, we had no way of compelling individuals to participate. Individuals who dropped out suffered no adverse consequences in terms of their income-support receipt. This explains the large dropout between Waves 1 and 2. There is also a large dropout between Wave 2 (when interviews were face-to-face) and the Wave 3 phone interviews.[12] The differential dropout between the control (72 per cent) and intervention (29 per cent) groups between the letter mailout and the first interview is due to the (unrealised) threat of stopping payments for intervention group members who did not participate in the face-to-face interview. There was no similar threat for the control group.

Correlation between the decision to participate in the pilot once selected and individual characteristics could easily confound the effects of those characteristics and participation in the treatment on subsequent outcomes.[13] Treatment dropout is not an insurmountable problem and there are several strategies in the literature for dealing with treatment group dropout.[14] Heckman et al. (1998), for example, propose a method of estimating the "effect of the intention to treat" which can be calculated in the face of treatment dropout. For programs that will be imperfectly implemented this may in fact provide a more realistic estimate of the 'real-world' policy impact.

Dropout is a particular problem in our case, however, because we do not have complete survey data for intervention and control group members who chose not to participate in an interview (or who could not be contacted). Although FaCS was able to deal with any ethical concerns associated with the initial random assignment, legal and

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concentrated in Australia's capital cities. Results of these randomisation tests are available upon request.

[12] Dropout between Waves 2 and 3 among the intervention group was high among those from non-English speaking backgrounds, presumably associated with the difficulties of communication in phone interviews. The differential dropout between those interviewed with and without partners primarily reflects an administrative error. The market research company collected information only from the first individual on each household record lowing the response rate of PPP recipients interviewed with their partners.

[13] In our case, participants who did not drop out were more likely to be Australian-born or immigrants with English-speaking backgrounds, live in major cities or towns, and own homes. Not surprisingly, individuals who had moved in the last six months were less likely to participate. Detailed results are available upon request.

ethical constraints regarding data privacy precluded collection of data from individuals opting out of the interview process. This complicates the analysis, but fortunately the availability of administrative data from the income-support system for all individuals (and their partners) selected for the trial allows us to adopt a non-experimental, propensity score matching approach to estimate the effect of the intervention.

3.2 Estimation Strategy

We pursue a two-pronged approach. First, we use survey data from the trial itself and attempt to estimate the "effect of treatment on the fully-treated". Second, we use administrative LDS information—which is available for all individuals selected for the trial—to estimate the "effect of the intention to treat". Two sorts of comparisons will be made: first, between family benefits (PPP) recipients in the aggregated intervention group and family benefits (PPP) recipients in the control group and second, between family benefits (PPP) recipients in the two intervention groups. This later comparison allows us to assess the marginal impact of participating in a joint interview (and developing a joint participation plan) as opposed to individual interview.

To illustrate, consider the first comparison. We wish to compare the economic activity of those who fully participated in the interview process and developed a participation plan to that of individuals in the control group who would have done the same had they been selected for the intervention. In other words we wish to estimate

$$\Delta^{TOFT} = E(Y^1 - Y^0 \mid X, P = 1) \tag{1}$$

where Y^1 and Y^0 are potential activity levels given completion and non-completion of the interview process respectively, X is a vector of controls, and $P = 1$ when an individual completes the entire treatment and 0 otherwise. We use propensity score matching techniques to overcome the practical difficulties associated with determining

[14] Control group dropout is an uncommon problem that has not been discussed in the literature.

which comparison individuals would have completed the interview process had they been assigned to the intervention group.

More specifically, we use the administrative LDS data for intervention group members to estimate a logit model of the probability of completing the final interview.[15] Using these estimates, we then create a propensity score ($\hat{p}_i$) (predicted probability) for each family benefit recipient in the intervention and control group. Using kernel propensity score matching, individuals in the intervention group are then matched to a weighted average of control group members with similar propensity scores. Weights are positively related to the similarity in propensity scores. The effect of full treatment for an individual *i* completing treatment (δ_i) is then given by

$$\delta_i = y_i^1 - \frac{1}{n_0}\sum_{j=1}^{n_0} K\left(\frac{\hat{p}_i^I - \hat{p}_j^C}{h}\right) y_j^0 \tag{2}$$

where $\hat{p}_i^I$ and y_i^1 are the propensity score and realized outcome for individual *i* in the fully-treated intervention group, $\hat{p}_j^C$ and y_j^0 are the propensity score and realized outcome for individual *j* in the control group, and n_0 is number of individuals in the control group. We use a standard normal kernel for K and choose the bandwidth (h) using Silverman's (1986) suggested robust bandwidth for density estimation. The δ_i from equation (2) are then averaged across members of the fully-treated intervention group to generate a cross-sectional estimate of the effect of full-treatment on fully-treated individuals based on activity levels at the third interview. We also use δ_i to construct a standard difference-in-difference estimate of changes in activity levels between the first and third interviews. Results from both measures are presented in Section 4.

[15] The results appear in Appendix Table A.1. The variables included in the logit equation included age, gender, marital status, number of dependent children, birthplace, language background, indigenous status, housing type, rurality, income-support duration and previous participation in labour market programs.

In addition to the overall comparison between family benefit recipients in the intervention and control groups, we would also like to assess whether participation in the interview process with one's partner (as opposed to alone) had any additional effect on economic activity. In order to make this comparison, we repeat the above matching process taking family benefit recipients participating in an individual interview as the "control" group and those participating in a couple interview as the "intervention" group. These results are also discussed in Section 4.

The probability density functions of the propensity scores for the intervention and control groups of those who survived to Wave 3 are presented in Figure 1 in the Appendix. Common support holds quite well, although the propensity score density for the control group has slightly more mass at smaller values, reflecting the greater concentration of individuals from non-English speaking backgrounds among that group and the negative effect that characteristic has on the probability of full-participation. In general, the matching procedure appeared to be satisfactory.[16] No match was found for three intervention group observations and these were dropped from the analysis.[17]

4. The Impact of the Interview Process on Economic Participation of Family Benefit Recipients

4.1 The Interview Process and Economic Activity: Survey Data Results

Detailed survey data for pilot participants allows us to estimate the impact of the interview process on the economic participation of those individuals who completed the final interview. We concentrate on five measures of economic participation (paid employment, study or training for work-related reasons, voluntary work for work-

[16] We followed the procedure proposed in Dehejia and Wahba (2002) to assess the balancing of the covariates. This involves splitting observations into strata based on their propensity scores and testing for each stratum whether each covariate differs between the intervention and control groups. About five per cent of the (500 or so) tests that the covariates were equal were rejected at the five per cent level. The evidence from this procedure does not point to problems with covariate balancing.

[17] There were three intervention group observations whose propensity scores exceeded the maximum propensity score among control group observations.

related reasons, job search, and "total economic participation" which is defined as participation in any of these). In each case, we consider both total hours and overall participation in the specific activity.

Both the cross-sectional Wave 3 and the difference-in-difference estimates of the overall impact of FaCS's interview process on the economic participation of family benefit (PPP) recipients partnered with men receiving unemployment benefits are presented in Table 2. While difference-in-difference estimators have the advantage of 'differencing out' any time-invariant group-specific effects that might remain after matching, their validity rests on the assumption that any differential change in the relative activity levels of the two groups can be attributed solely to the effects of the treatment itself. Changes in the method of interview (from face-to-face to telephone) for the intervention (but not control) group imply that this assumption may not hold in our case. This—along with our relative confidence in our ability to match individuals participating in the full interview process to comparable control group members (see Section 3.2)—leads us to have a preference for the cross-sectional estimates.

Table 2 Here

Our estimates imply that individuals participating in the full interview process had lower hours of (and participation in) paid work than members of the control group, but higher weekly hours of (and participation in) work-related study or training and job search.[18] In particular, the time spent in work-related study or training was one and a half hours per week higher amongst those participating in the full treatment. These women spent more time in job search (approximately one hour per week), but less time (approximately one hour and 45 minutes) in paid employment each week. Voluntary work for work-related reasons was also more common amongst women participating in the interviews with Centrelink advisors, though there was no significant difference in

[18] Van der Berg and van der Klaauw (2001) found that a Dutch counselling and monitoring program for the unemployed made more formal job search more likely but no impact on exits to employment.

the hours the two groups spent in work-related volunteering. Overall, although the average total hours spent in these economic activities was not significantly affected by the intervention, there was a slight increase (seven percentage points) in the proportion of individuals engaged in some form of economic activity.

These results provide evidence that interviews centring on future planning and can lead to modest increases in the economic activity of family benefits recipients whose partners are unemployed. Are these outcomes enhanced further when family benefit recipients participate in these interviews jointly with their unemployed partners? In addressing this question, we compute both cross-sectional and difference-in-difference estimates that compare family benefit recipients participating in joint interviews (the "intervention group") with family benefit recipients participating in individual interviews (the "control" group) (see Table 3). This provides estimates of the marginal impact of a joint as opposed to an individual interview. Because the move from face-to-face interviews to telephone interviews occurred between Waves 2 and 3 for both groups (see Table 1), we are more confident that the identifying assumptions of the difference-in-difference estimator hold leading us to have a slight preference for the difference-in-difference estimates.

Table 3 Here

There is no evidence that requiring family benefit recipients to participate in a joint interview and planning process with their partners leads to higher levels of economic activity. Hours of (and participation in) paid work, study and training, and volunteer work are all unaffected by the inclusion of one's partner in the interview process. In fact, difference-in-difference estimates suggest that participation in a joint–rather than single–interview resulted in a reduction in the hours that family benefit recipients spent looking for work each week. Recall that both groups are interviewed alone by phone at the third wave, but at the first wave one group was interviewed in-

person with partners while the other was interviewed in-person, but alone. This estimate will therefore also reflect any differential effect on reported job search arising from interview technique.[19]

4.2 The Interview Process and Economic Activity: Administrative Data Results

One might reasonably be concerned about residual selection on unobservables in the take-up of the treatment (and in the full response to all three interviews for the control group). In particular, recipients who are already planning on going back to work might avoid the hassle of going to the interviews. If that were the case and these recipients were more likely to take up jobs, then our estimate of the treatment effect presented above will be biased downward. This is one possible explanation for the finding of a small, negative impact on working (hours and participation) in the intervention group. Interestingly, Kamionka and Lacroix (2003) find a large bias in the experimental matching estimator in a situation with a similarly large amount of dropout—the Canadian Self-Sufficiency Project.

We can address this possibility, and at the same time, expand the number and type of outcome measures that we consider, using administrative data from the FaCS LDS that allow us to assess the impact of the intervention both on those who participated fully in the intervention and those who were assigned to it but did not participate fully. This approach is similar to Kamionka and Lacroix who also use administrative data available for all participants, even the dropouts. The outcome measures that are available to us in the administrative data are movement off of income-support payments; the presence of earned income; and average earned income.

These outcomes are measured in June 2001, about two months after the completion of the trial. This allows us to isolate any effects of assignment to the trial (the effect of intention to treat) from full participation (treatment on the fully treated). We use

[19] There was an increase in job search activity among the unemployed partners of family benefits recipients who were part of the joint interview process relative to the partner control group.

average values[20] over two fortnights of data (from 17 May through 14 June 2001) to construct the outcome measures, which are reported in Table 4 for the total family benefits recipient group.

The first and second columns of Table 4 provide estimates of the impact of the intention to treat, comparing the outcomes of all individuals assigned to the intervention group with all those assigned to the control group. The first column is a comparison of group means, while the matching estimator in the second column controls for non-random assignment to control and intervention group discussed in Section 3. The third column compares the intervention group who participated fully in the intervention with the total (assigned) control group and the last column compares the fully-participating intervention group with control group members who participated in the interviews through Wave 3. The last two columns are both matched estimates, not simple means.

Table 4 Here

Overall, the administrative data provide important support for one key feature of the survey results: the impact of the intervention generally was small. Nearly all (over 95 per cent) members of both intervention and control groups remained on some type of income support immediately after the conclusion of the trial.[21]

There are very few significant differences between the outcomes of members of the intervention group, either those assigned or participating fully, and those of the control group (see Table 4). Intervention group members who participated at Wave 3 may have been more likely to remain on benefits than control group members. This is consistent with the survey results that indicate greater intervention group participation in job search and education and training as an outcome of the formation of their participation

[20] We use average values to eliminate high frequency variation in the data. Individuals sometimes disappear from the administrative data for one fortnight, only to return the following fortnight on the same payment type. Logically, these cannot be thought of as true departures from welfare receipt.

[21] Of individuals who began the trial on PPP, 85.1 per cent were still on PPP at the end. Only 3.9 per cent had left payments altogether. Half of the remaining 11 per cent moved to family assistance and the remainder moved to either NSA or disability payment. Of individuals who began the trial on NSA 80.8

plan. While these activities may eventually provide better longer-term outcomes, they result in a higher likelihood of remaining on payments in the short-term.

In contrast to the survey results, intervention group members appear more likely to report earned income than control group members. This is perhaps evidence that there is downward bias in the matching estimator based upon the experimental data (consistent with the findings in Kamionka and Lacroix, 2003). The matching procedure may not fully control for all factors affecting selection into full treatment.

From the survey data, we found a small increase in job search activity for NSA partners of PPP recipients. In the administrative data, we find no significant impact of the intervention on the NSA partners. Intervention group members are no less likely to be on payments, to have earnings, or to have more earned income than control group members once we control for systematic differences between the intervention and control groups using the same matching procedure described above.

In general, the effects of the intervention appear to be modest. Taken together, the survey and administrative data point to a significant take-up of training and education activities and consequently a higher probability of remaining on payments. The results regarding short-term employment effects are more ambiguous, but clearly small.

4.3 Robustness of the Results

Alternative matching techniques produced estimated effects that were similar to those reported in Tables 2 through 4.[22] Similarly, where the matching procedures included use of the characteristics of the partners of family benefit recipients the impact estimates were similar to those already presented. One explanation for this outcome is that the partners' data added little new information to improve the matching procedure. After all, the partners were all unemployed, overwhelmingly male and lived in the same

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per cent were still on NSA, 6.6 per cent had moved to disability payments, and 6.6 per cent had left payments altogether. The remainder were receiving family assistance or PPP.

regions in the same types of housing as the family benefit recipients. The ages of members of the couples were also strongly correlated.

The impact of the interview process on the outcomes of the NSA partners of the family benefit recipients was also similar to those achieved by the recipients themselves. The survey data suggest that unemployed NSA partners who participated fully in the pilot worked less in a job or as a volunteer and undertook more job search than NSA partners who were included in the control group.[23] There were similarly few differences in outcomes between unemployed partners in the intervention group and those in the control group in the FaCS administrative data.

4.4 Discussion

These results provide evidence that interviews centred on future planning and the development of participation plans can lead to modest increases in economic activities by family benefits recipients and their unemployed partners. That modest interventions lead to only modest successes is perhaps not surprising given the high level of correlation within couples in terms of characteristics and unemployment outcomes. Worklessness may simply be concentrated within households that are particularly hard to help (Dorsett, 2001). The U.K.'s experience with JSA also suggests that it may take time for effects of policy interventions to materialize (Bonjour et al., 2002), and the outcomes we have measured here are rather short-term.

What is more surprising is the apparent substitution between market work and other activities. Compared with the control group, the planning process and its implementation may have lowered the hours and incidence of work by members of the intervention group. This effect is observed in both the cross-sectional Wave 3 and difference-in-difference estimates reported in Table 2 based upon the survey data. What

[22] These alternatives, available from the authors, include other kernel weighting methods and nearest neighbour techniques. We also tried bandwidths ranging from 0.001 to 0.050 in the kernel matching and found that the qualitative results are insensitive to this choice.
[23] Survey and administrative data outcomes for this group are available on request.

behavioural responses or features of the trial may have brought about this employment effect?

One possible explanation for the difference-in-difference result (though not the cross-sectional Wave 3 result) is that intervention group members may have overstated their participation and hours of work in their initial face-to-face interviews with Centrelink advisors. The answers of individuals may have been more accurate in response to questions asked over the telephone by an employee of a market research company. Alternatively, both the difference-in-difference and cross-sectional results could be explained by specific family responsibilities that constrain the time that family benefit recipients can allocate to other activities. Any increase in non-work activities associated with the implementation of the participation plan may only have been possible at the expense of participation in or time spent on current employment.

The data do not support either explanation, however. Both reported participation in employment and reported average hours worked by those employed increased for members of the intervention group between the Wave 1 (face-to-face) and Wave 3 (telephone) interviews. The increases in employment and hours worked were simply greater for the control group. In fact, participation in and total hours of economic activity (see Table 2) increased for both the intervention and control groups between the interviews. These changes in reported economic participation are summarized in Table 5. The increase in economic activity is similar for both groups between the interviews. However, the increased activity is less employment-focused for the intervention group than the control group.

Table 5 here

Individuals' responses to Wave 3 interviews also do not suggest that they are so time constrained that they might not have been able to work if offered a job. While these families all had dependent children, the trial was restricted to those families in

which the youngest child was school-aged. Furthermore, over one in four members of the intervention group engaged in voluntary work, most without specific work-related objectives. This work may serve very valuable community purposes. Nevertheless, such participation indicates that there was potential flexibility among the intervention group in their allocation of time towards economic activities.

Table 6 provides a comparison between survey responses and administrative data for the one measure which is comparable across the two data sources—participation in work. It shows that overstatement of work is a feature of both the telephone interviews and the face-to-face interviews and that in fact over-statement was greater for the control group. Furthermore, overstatement increases for both groups between Waves 1 and 3. We see that participation is higher for the intervention group than for the control group and that it increases for both. However, recalling the results of Table 4, once we control for differences in dropout and non-randomisation these differences are not significant.

Table 6 here

It is difficult to know why the marginal effect of a joint interview was not greater. Evaluations of Joint Claims for JSA in the U.K. suggest that individuals—particularly men—participating in an interview with their partner were more likely to feel that the interview process had been helpful (Bonjour, et al, 2002). In addition to facilitating the provision of required information, couples found joint interviews to be helpful because they allowed partners to support one another. In their evaluation Bonjour, et al (2002), however, did not attempt to measure the impact of the mode of interview on subsequent outcomes. Unlike in our case, couples were not randomly assigned to joint versus individual interviews, suggesting that selectivity may play a role in generating the U.K. results. In this trial, however, the family payments recipients

interviewed with their partners were no more likely than counterparts interviewed alone to indicate that they had found the interview quite or very helpful.[24]

5. Conclusions

The increasing concentration of unemployment and dependence on welfare within families is a serious policy concern. Children growing up in such families are at particular risk of academic failure, social exclusion, and welfare dependence in adulthood. With this in mind, the Australian Department of Family and Community Services conducted a randomised experiment to test a policy of intensive interviews with couples and individuals in workless families. The interviews resulted in the formation of individual roadmaps toward increased economic and social participation. This paper has reviewed that experiment and its outcomes.

Over the three waves of data collection associated with the trial, we find that both the control and intervention groups showed significant increases in economic activity. For control group members, this manifested itself as increased participation in paid work, while intervention group members showed significant increases in work-related study and training. Both control and intervention groups participated in three interviews in a six-month period—a stark contrast to the limited contact that this group would normally have with the welfare system. That both groups responded to this contact is therefore not surprising. The differential response may perhaps be explained by the formation of participation plans in the face-to-face interviews with the intervention group. For this group of individuals who are entrenched in unemployment, job counsellors may help in moving people towards richer economic participation through training and study programs.

The differences we find between the control and intervention groups are fairly small. Three things mitigate against finding larger results. First, both groups increased

[24] Family benefit recipients (who are predominately female) interviewed with their partners were

economic participation in response to the trial. Given this, there may have been less potential for there to be a marginal impact of the interview process itself. Secondly, the interviews for both groups were voluntary. There was no penalty for refusing to participate in the trial or for dropping out of the trial. Thirdly, the time frame of analysis is fairly short. Interestingly, we also find no differential impact on outcomes for individuals interviewed together as a couple compared to individuals interviewed alone.

Our finding that the effect of treatment was small is consistent with Breunig et al. (2005) who also find modest effects from a similar intervention targeted towards the very-long term unemployed in Australia. In fact, one of Martin's (1998) five main conclusions in analysing European active labour market programs is that interventions are less likely to work well if applied to people already entrenched in disadvantage such as the target group in this intervention. Of course, the long-term effects of this couples-based approach may differ from the short-term effects measured here as highlighted by research in California (Hotz et al., 2000) and West Germany (Lechner et al., 2004).

This study provides further evidence that moving individuals entrenched in unemployment off welfare is a difficult task. Unemployed individuals in workless families are among the most disadvantaged of welfare recipients. Nonetheless, the small, voluntary intervention studied here was successful in increasing certain forms of economic participation. It would seem however that any welfare reform process that has as its goal the reduction in workless families requires a longer-term perspective than the time frame examined here. The resources required by a successful program are also likely to be greater than those expended in this intervention.

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significantly more likely than their (male) partners to indicate that they had found the interview helpful.

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Table 1

Sample sizes at each stage of intervention and interviews

	Intervention		**Control**
	Interviewed with partners	Interviewed without partners	
Letters sent	1380	991	1413
Interviewed in Wave 1	983	715	396
Interviewed in Wave 2	430	652	315
Interviewed in Wave 3	147	309	244

Data gathered in face-to-face interview
Data gathered in phone interview

Table 2:
Economic Participation for PPP Recipients: Intervention versus Control Group
(Cross-Sectional and Difference-in-Difference Propensity Score Matching Impact Estimates)

	Economic Participation Measures			
	Wave 3	***Difference in Difference***	***Wave 3***	***Difference in Difference***
	Average Weekly Hours		**Proportion Working**	
Intervention Group	1.56	0.49	0.11	0.04
Control Group	3.30	2.24	0.17	0.11
Impact Estimate	-1.73**	-1.75 ***	-0.06 *	-0.07 **
Standard Error	(0.71)	(0.66)	(0.03)	(0.03)
	Hours Study or Training (Work)		**Proportion Studying or Training (Work)**	
Intervention Group	2.22	2.05	0.11	0.10
Control Group	0.92	-0.36	0.06	0.00
Impact Estimate	1.30 ***	2.41 ***	0.05 **	0.10 ***
Standard Error	(0.47)	(0.50)	(0.02)	(0.02)
	Hours of Voluntary Work (Work)		**Proportion Volunteering (Work)**	
Intervention Group	0.22	-0.11	0.05	0.01
Control Group	0.24	0.15	0.01	-0.01
Impact Estimate	-0.02	-0.26	0.03 **	0.02
Standard Error	(0.18)	(0.21)	(0.01)	(0.02)
	Hours Looking for Work		**Proportion Looking for Work**	
Intervention Group	2.38	0.29	0.35	0.02
Control Group	1.35	-0.31	0.27	0.03
Impact Estimate	1.03 ***	0.60	0.08 **	-0.01
Standard Error	(0.38)	(0.56)	(0.04)	(0.04)
	Total Hours Economic Participation		**Proportion in Economic Participation**	
Intervention Group	6.46	2.87	0.48	0.11
Control Group	5.78	1.77	0.41	0.10
Impact Estimate	0.68	1.10	0.07*	0.00
Standard Error	(0.98)	(0.97)	(0.04)	(0.04)

Notes: 1. Bandwidth for kernel match is 0.027. Standard errors are bootstrapped.
2. *** significant 1 per cent; ** significant 5 per cent; * significant 10 per cent.
3. Sample sizes vary due to missing data for some questions and range between 236 – 244 (control) and 438 – 457 (intervention). For this reason, the total hours estimates are not the sum of the individual elements. Total participation is also not the sum of the individual elements because individuals may participate in more than one activity.

Table 3:
Economic Participation for PPP Recipients Interviewed with and without Partners
(Cross-Sectional and Difference-in-Difference Propensity Score Matching Impact Estimates)

	Economic Participation Measures			
	Wave 3	***Difference in Difference***	***Wave 3***	***Difference in Difference***
	Average Weekly Hours		**Proportion Working**	
Interview with Partner	1.27	0.72	0.09	0.03
Interview without Partner	1.71	0.36	0.13	0.04
Impact Estimate	-0.44	0.37	-0.04	-0.01
Standard Error	(0.71)	(0.58)	(0.03)	(0.03)
	Hours Study or Training		**Proportion Studying or Training**	
Interview with Partner	1.66	1.60	0.08	0.07
Interview without Partner	2.39	2.22	0.11	0.11
Impact Estimate	-0.73	-0.62	-0.03	-0.04
Standard Error	(0.77)	(0.76)	(0.03)	(0.03)
	Hours of Voluntary Work		**Proportion Volunteering**	
Interview with Partner	0.23	-0.24	0.07	0.03
Interview without Partner	0.24	-0.03	0.04	0.01
Impact Estimate	-0.01	-0.21	0.03	0.02
Standard Error	(0.13)	(0.34)	(0.03)	(0.03)
	Hours Looking for Work		**Proportion Looking for Work**	
Interview with Partner	2.74	-1.16	0.38	-0.03
Interview without Partner	2.30	0.92	0.34	0.03
Impact Estimate	0.44	-2.07**	0.04	-0.06
Standard Error	(0.73)	(1.00)	(0.06)	(0.06)
	Total Hours		**Proportion Participating**	
Interview with Partner	6.00	0.94	0.47	0.03
Interview without Partner	6.68	3.61	0.48	0.14
Impact Estimate	-0.69	-2.68*	-0.01	-0.11*
Standard Error	(1.33)	(1.47)	(0.06)	(0.06)

Notes:

1. Bandwidth for kernel match is 0.027. Standard errors are bootstrapped.
2. ***significant at 1 per cent level; **significant at 5 per cent; *significant at 10 per cent.
3. Sample sizes vary due to missing data for some questions and range 127 – 136 (partner interviewed) and 297 – 307 (partner not interviewed). For this reason, the total hours estimates are not the sum of the individual elements. Total participation is also not the sum of the individual elements because individuals may participate in more than one activity.

Table 4:
Economic Participation for PPP Recipients: Intervention versus Control Group Administrative Data Measures

	All individuals assigned to intervention and control groups		Wave 3 intervention group participants compared to:	
			Matching estimates	
	Randomized Experiment Estimator	Matched estimate	All individuals assigned to the control group	Wave 3 control group participants
On payments June 2001 (%)				
Intervention	0.965	0.965	0.991	0.991
Control	0.965	0.965	0.967	0.971
Impact estimate	0.000	0.000	0.024***	0.020*
Standard error	(0.006)	(0.008)	(0.007)	(0.011)
Has earnings June 2001 (%)				
Intervention	0.047	0.047	0.059	0.059
Control	0.027	0.031	0.030	0.035
Impact estimate	0.020***	0.016	0.029**	0.024
Standard error	(0.007)	(0.013)	(0.013)	(0.017)
Average earnings June 2001 ($)				
Intervention	11.8	11.8	15.9	15.9
Control	7.9	9.5	8.9	12.0
Impact estimate	3.9*	2.3	6.9	3.8
Standard error	(2.3)	(5.5)	(5.5)	(7.2)
Average earnings June 2001 given had earnings ($)				
Intervention	251.3	252.1	278.4	284.1
Control	294.8	290.6	304.9	403.0
Impact estimate	-43.5	-38.5	-26.4	-118.9
Standard error	(48.0)	(95.4)	(153.8)	(153.8)

Notes:

1. Bandwidth for kernel match for column two is 0.019; for columns three and four it is 0.027. Standard errors are bootstrapped for columns two to four.
2. ***significant at 1 per cent level; **significant at 5 per cent; *significant at 10 per cent.
3. Sample sizes: for column two, 2346 intervention group members, 1413 controls; for column three, 457 intervention group members, 1413 controls; for column four, 457 intervention group members, 244 controls.

Table 5: Change in Economic Participation for PPP Recipients between Waves 1 and 3: Intervention and Control Groups[1]

	Intervention (per cent)	Control (per cent)
Between Wave 1 and Wave 3:		
Increase in the proportion participating in economic activities among those not working[2]	9.0	5.9
Proportion employed who were previously not economically active	2.3	5.5
Proportion employed who were previously economically active, but not employed	4.3	6.4
Between Wave 1 and Wave 3:		
Increase in the proportion working	4.3	11.0
Increase in the proportion participating in other economic activities	10.6	11.6

Notes:

1. This categorization of activities or outcomes is incomplete. For example, small numbers of individuals employed at Wave 1 were not employed at Wave 3 and some were no longer participating in economic activities.
2. These proportions are measured relative to the total intervention and control groups. For example, the increase in individuals who participated in non-work economic activities between Waves 1 and 3 constituted 9 per cent of the total intervention group.

Table 6: Proportion Working: Comparison of Administrative and Survey Data for Control and Intervention Groups

	Intervention (per cent)	Control (per cent)
Wave 3 survivors		
Report working at Wave 1 (September 2000):		
Survey	7.6	6.2
Administrative data	3.3	2.1
Report working at Wave 3 (March 2001):		
Survey	11.2	16.9
Administrative data	4.2	3.3
Working in June, 2001:		
Administrative data	5.9	3.3
All individuals who received letters		
Report working in administrative data:		
September, 2000	2.5	1.3
March, 2001	3.8	3.0
June, 2001	4.7	2.7

APPENDIX

Figure 1: Density of Propensity Scores for the Intervention and Control Groups

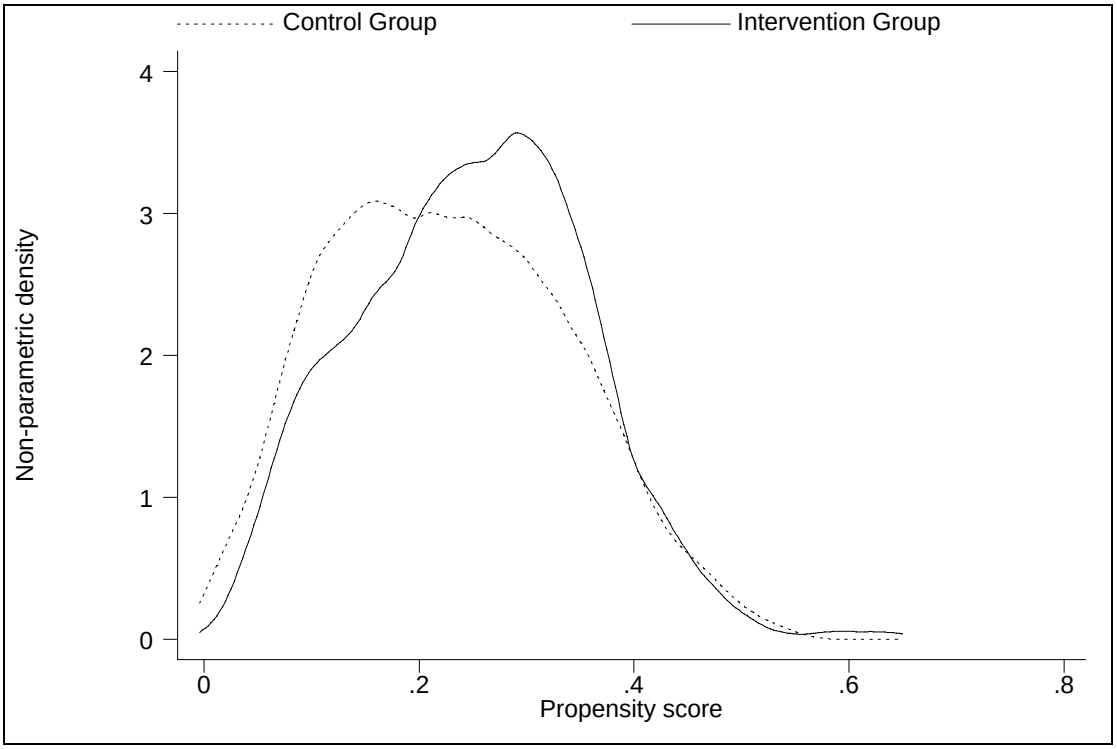

Table A.1:
Propensity score equations – Total and Joint Interview Intervention Groups

	Total Interview Group			Joint Interview Group		
	Beta	Std. Err.	z	Beta	Std. Err.	z
Demographic Characteristics						
Age	0.18	0.09	2.05	0.30	0.16	1.89
Age Squared	-0.21	0.10	-2.05	-0.35	0.19	-1.85
Female	0.16	0.20	0.80	-0.001	0.31	0.00
Married (Not Defacto)	0.17	0.14	1.21	0.09	0.25	0.36
Aboriginal	-0.96	0.45	-2.16	-0.89	0.75	-1.18
Has disability	-0.62	0.55	-1.14	0.53	0.66	0.81
Dependent Children						
Age of Youngest Child	0.01	0.02	0.49	0.02	0.04	0.52
Two Children	0.08	0.14	0.56	0.58	0.23	2.49
Three Children	0.22	0.17	1.34	0.37	0.30	1.25
Four of More Children	0.13	0.25	0.50	0.21	0.46	0.46
Birthplace						
Overseas NESB	-0.96	0.16	-5.81	-0.93	0.30	-3.11
Overseas ESB	0.38	0.20	1.91	0.26	0.36	0.72
Living Circumstances						
Moved in Last 6 months	-0.68	0.15	-4.51	-0.22	0.21	-1.02
Home Owner	0.42	0.13	3.20	0.23	0.22	1.05
Government Rental	0.27	0.17	1.59	-0.13	0.32	-0.40
Boarding	-1.62	1.03	-1.57	-0.53	1.06	-0.50
Other Arrangements	-0.22	0.29	-0.76	-0.40	0.51	-0.79
Capital City	-0.01	0.18	-0.06	-0.41	0.33	-1.24
Major City	0.52	0.21	2.40	0.23	0.35	0.66
Towns	0.37	0.19	1.98	0.15	0.31	0.49
Reported Income (1/00 to 6/00)						
Earnings	0.06	0.21	0.27	-0.69	0.46	-1.52
Unearned Income	0.32	0.14	2.35	0.50	0.23	2.21
Payment History (Since 7/95)						
Time on Parenting Pay.	-0.02	0.03	-0.56	-0.09	0.06	-1.59
Participated in						
Intensive Assistance	-0.57	0.64	-0.89	-0.18	0.88	-0.20
Training Program	-0.35	0.64	-0.55	0.93	0.81	1.15
Received an Exemption	-0.06	0.53	-0.11	-0.45	1.11	-0.40
Had Admin. Breach	1.11	0.96	1.16	(a)		
Had Activity Breach	1.92	1.05	1.82	2.67	1.52	1.76
Observations	2358			1374		
Likelihood Ratio	X^2 (28 df) = 163.3			X^2 (27 df) = 61.8		
Prob. > X^2	0.000			0.0002		
Psuedo R^2	0.0702			0.0693		

(a) There was no variation in this variable for this equation.