The Impact of Recent Changes in Family Assistance on Partnering and Women's Employment in New Zealand*

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This paper estimates the effects of recent changes to Family Assistance tax credits on the partnering and employment outcomes for New Zealand women. We use a difference-in-differences approach to control for economic and other confounding factors. Specifically, we investigate differences in partnering, employment and work hours over time across groups who are and are not likely to be affected by these policy changes. We define groups based on education, wages, and presence of children. Subject to qualifications, we conclude that the Family Assistance expansion beginning in 2005 had little effect on partnering, but increased work hours for both partnered and unpartnered women.

JEL Classification Codes: I38, J12, J21

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1. Introduction

Recently enacted changes in Family Assistance, by offering more generous benefits to families, are expected to reduce child poverty by one-third (MSD 2006). As side effects, the changes alter the relevant benefit of both partnering (marriage or cohabitation) and work for low- income families. The new rules offer expanded benefits which increase the number of families eligible, and reduced abatement rates which raise net wages and could affect labour supply. Since benefits are abated based on family income, the programme can also result in "partnering penalties" whereby a partnered couple receives less total benefits than the sum of individual benefits. One concern is that these penalties could result in more sole parent families. As in other countries such as the US, marriage rates in New Zealand have been falling over the past few decades (Johnson 2005). There is evidence that children raised by parents who are partnered or married have advantages that yield long run gains (Wilson and Oswald 2005, Lerman 1996, Haveman and Wolfe 1994, Sandefur and McLanahan 1994 and Haskins et al. 2005). Such concerns should be considered together with the poverty reduction and work changes brought about by the policy.

The Working for Families Programme contains the most recent set of policy changes related to Family Assistance. Past changes in the Domestic Purposes Benefit (DPB) and prior changes in Family Assistance also altered the relative value of partnering. The decision to partner is complex and depends on both economic and social factors. Labour market conditions, availability of spouses, and personal traits affect partnering in addition to the effects of government transfers. In this paper, we address family structure decisions by low-income women and families and assess the impact of changes in government transfers on those decisions while controlling for economic and social environmental conditions. Additionally, we consider how the policy changes affect employment and hours of work for both single and partnered women.

Answering any of these questions requires that we control for confounding environmental influences. Since there is no geographical variation in Family Assistance payments (as there is in the US), we cannot contrast women's behaviour in areas with and without the policy change. However, there is variation over time and in benefit levels available to different groups. Therefore, using a difference-in-differences methodology helps control for background factors when estimating policy impacts. We compare changes in partnering and work behaviour over time for demographic groups that differ in their eligibility or likely use of the programmes. We use three variations of this approach in identifying these policy effects. Firstly, families with children receive additional benefits that can be contrasted to childless families. Secondly, women with low education are more likely to qualify for these benefits than women with high education. Finally, women with low wages are more likely to be eligible for these programmes than those with high wages. We also estimate these impacts using the preferred child/no child identifier with the sample restricted to the low wage or low education subgroup.

2. Changes in Family Assistance

Aid to low-income families in New Zealand is largely administered through the tax system in the Family Assistance set of tax credits: family support, child tax credit, family tax credit and parental tax credit. Family support is not conditioned on work, but the remaining credits are only available to working families. The Working for Families programme altered a number of the dimensions of Family Assistance. We focus on changes to family support (renamed Family Tax Credit), a benefit available to income qualified families with children, and the child tax credit, a benefit available to families that have children and meet a work test. In April 2005, Family Support Rates increased by $25 per week for the first child and $15 per week for additional children. In April 2006, the child tax credit was replaced by the In-Work Payment, and amounts were raised for families qualifying by working at least 30 hours per week for a couple or 20 hours for a sole parent. The new payment provides $60 per week for up to three children, and then $15 for each additional child. For a family with one child, this amounts to an increase of $45 per week from $15 to $60, or an annual increase of $2,340. For families with two children, the increase is $30 per week from $30 to $60, or an increase of $1,560 annually. The abatement schedule was also changed, increasing the no abatement range to $35,000 (from $20,356), removing the 18 percent benefit reduction rate, and lowering the 30 percent rate to 20 percent for income above this threshold. This expands eligibility to more families by raising the breakeven level. In addition, the Family Tax Credit, a guaranteed income now called the Minimum Family Tax Credit, was raised from $15,080 to $17,680. Given the size of these changes beginning in 2005, we choose this year as the start date from which we might potentially see behavioural changes.

Other changes in policy have taken place over our study period. These may have affected employment for low-income women and thus should be borne in mind when interpreting our results. For example, work tests in the Domestic Purposes Benefit (DPB) that were adopted in February 1999 were removed and personal and employment plans were developed for recipients from March 2003. We doubt that such changes have had substantial effects on either partnering or employment over our sample period, or would have been responsible for observed changes in these outcomes beginning in 2005.

The change in abatement rates alters work incentives. The 2006 reduction in Family Assistance abatement rates for income and the raising of the no-abatement threshold both raise effective wages as well as family incomes. The higher after tax wage has both income and substitution effects with theoretically ambiguous effects on labour supply, but empirically would be expected to increase work hours for women. The higher incomes available would be expected to reduce work effort. However, the child tax credit, renamed the In-Work payment, is available only to those families with substantial work effort (20 hours per week for sole parents and 30 hours per week for couples). Consequently, increases in its generosity should make entering employment more attractive and encourage increased work hours to meet these thresholds. This study will test for whether this policy causes more families to meet these hours thresholds.

These credits also alter the relative gains of partnering because eligibility is based on family income. As a simple example, considering only Family Assistance and In-Work payments, suppose a couple with two children each earn $30,000 and work 20 hours per week. If living apart, the custodial parent in 2006 would have received $9,308 in benefits for two young children ($6,188 in family support plus $3,120 in In-Work payments) and this would not have been abated because individual earnings were less than the $35,000 threshold. The non-custodial parent would have received no benefits in this scenario. If the two were to partner, the combined household income of $60,000 would have caused an abatement of $5,000 (($60,000-$35,000)*0.2), and thus net benefits would have been only $4,308. Thus partnering results in a "partnering penalty" because the couple would have received a lower benefit compared to the aggregate benefit received by the parents living apart. As noted by Johnson (2005), we could just as well call it a "sole parent bonus". One can think of the partnering penalty as a coarse adjustment for economies of scale of the larger household (Johnson, 2005).

The expansion of benefits has made the partnering penalties larger. Consider the same example as above using the 2004 rules. The combined gross benefit for the custodial parent would have been $5,668 ($4,108 in family support, $1,560 in child tax credits), but abatement would have reduced this benefit to $3,662. If the pair were to partner, the combined income of $60,000 would have fully abated the benefit. Thus, this particular penalty for partnering was substantially lower in 2004 ($3,662) than in 2006 ($5,000).

A Family Assistance marriage bonus could occur in some circumstances. If a woman with children had less than twenty hours of work per week, she would not have qualified for the 2006 In-Work payment of $3,120 (annually) for up to three children. If she partnered with a man such that their total work hours exceeded 30 per week, they would have received the In-Work payment (although it would have been abated by the additional partner's income if joint income exceeded $35,000). In another example, if an unpartnered woman worked 20 hours per week but was ineligible for the In-Work payment because she received a part benefit from DPB, she could have an incentive to partner to get access to the In-Work payment even though she would forgo her part benefit. The sizes of the bonuses or penalties depend on incomes and number and ages of children.

Partnering incentives tell only part of the story. Couples decide to partner for many reasons. We argue that financial incentives can have an impact on propensity to partner for some, as is evident from the literature. As discussed below, other factors such as gains from economies of scale of the larger partnered family, labour supply adjustments, and income stability could also play roles in this decision. Increased income stability of sole parents may result in increased marriage rates in the future. The increase in family incomes of those on Family Assistance could potentially stabilize incomes and thus stabilize marriage (more on this possibility below). Our method is not able to tease apart these separate influences, and estimates only the total effect.

3. Background Literature

For labour supply, the impact of welfare and tax credit programmes in the US on work effort by low-income persons is well surveyed elsewhere (e.g., see Moffitt 1992, Hotz and Scholz 2003 and Moffitt 2003) and we discuss it only briefly here. One theme that emerges in this literature is that the work participation decision is more sensitive to policy than the hours of work decision (Meyer 2002). Our work focuses on employment outcomes as well as hours of work. The earned income tax credit, in particular, has been found to have a positive and substantial impact on labour participation of single mothers (Meyer and Rosenbaum 2001, Dickert, Houser et al. 1995). It also appears to have a modest negative effect on the hours worked by second earners in couples (Eissa and Hoynes 2004).

Two New Zealand studies have investigated the impact of the Working for Families Programme on work effort (Kalb and Scutella 2003 and Kalb, Cai et al. 2005). These studies employ a structural simulation methodology, predicting the impacts based on labour supply elasticities from tax changes in the 1990s. They predicted a small increase in labour force participation for sole parents by two percent over the next several years. Our methodology is quite different, using actual data from before and after these policy changes.

To gauge the potential impacts of partnering penalties from past literature, we can look at US studies of the Earned Income Tax Credit (EITC) which is also a tax credit for low-income working families. Since EITC benefits depend on combined family income, it also creates marriage penalties or bonuses in different situations. Single mothers with no earnings can reap a bonus by marrying a man with earnings so that the couple qualifies for the EITC. On the other hand, a single mother with moderate earnings who qualifies for the credit could lose the credit if she married a man with enough earnings to put the married couple beyond the earnings limit. The size of these penalties and bonuses have been documented by Dickert-Conlin and Houser (1998) and Holtzblatt and Rebelein (2000).

To help clarify this issue, consider a simple conceptual model. Suppose a single woman decides whether or not to partner by comparing her expected utility if married to her expected utility if she remains single. She chooses the higher valued option. The value of each option would depend on expected income, taking into account labour supply adjustments, leisure, government taxes and benefits, and tastes. The difference in utility between the married and unmarried states is generally modelled as a function of the incomes in the two states and demographic characteristics. A key issue is that couples are not usually observed in both the married and unmarried state and thus income differences must be predicted for the marital state not observed.

Within this framework, Eissa and Hoynes (1999) look at marriage by single mothers. They model incomes in each marital state based on current earnings and a tax/transfer function that calculates taxes and transfers including the EITC based on assumptions about household makeup after a split. They find small or nonexistent effects on family formation. Dickert-Conlin and Houser (2002) also model the impact of EITC using this approach. They use panel data on individuals to get an initial distribution of earnings, and then compute benefits over time allowing the benefit rules to change but holding the distribution of earnings fixed. They conclude that the EITC expansions of the 1990s had little impact on marriage. Overall, the EITC literature from the US suggests that marriage responses are likely to be either nonexistent or small in magnitude.

Studies of the U.S. experience are not directly applicable to NZ for several reasons. Firstly, the EITC benefits in the US are structured differently than Family Assistance. The EITC has an initial earnings subsidy component that increases net wages. It then abates away, but the EITC does not have hours-of-work thresholds like the NZ In-Work payment. Secondly, the social stigma of unwed motherhood and marriage customs vary between the countries. Cohabitation is less stigmatized in NZ and benefit rules explicitly allow for partner benefits as long as there is a marriage-like relationship. In our study, we use a broad definition of partnering that includes reported cohabitation and marriage. Furthermore, non-working low-income married couples in the US do not receive cash benefits apart from unemployment insurance (if applicable), but rather in-kind aid such as food stamps, housing assistance, energy assistance, and subsidised child care. The broader availability of cash aid in NZ complicates any comparison.

4. Empirical Models: Reduced Form Difference-in-Differences Approach

With the availability of annual cross sectional data on individuals from before and after the expansion of Family Assistance, we develop empirical models of the partnering and employment behaviour of women. We begin with a descriptive analysis of the trends in the proportions partnered and employed for various groups. We then jointly estimate the propensity to partner and the propensity to work. Joint estimation by bivariate probit allows for correlation in the unobservables across the equations and should improve precision of parameter estimates. Women with a high unobserved propensity to work (more productive in the market), for example, may be more sensitive to financial marriage incentives. This model is similar in form to that of Buffeteau and Echevin (2003). We estimate hours worked for those employed and the probability that a family’s hours of work will exceed the hours threshold for the In-Work payment.

Our model attempts to isolate the impact of the Family Assistance changes that took effect beginning in 2005. To control for other time varying factors in the economy that confound with changes in benefits and tax credits, our model includes the local unemployment rate, a measure of the strength of the labour market which varies by region over time. Annual time dummies or a linear time trend are also included to absorb other general trends in partnering and employment outcomes that are unrelated to policy changes.

To better control for non-policy influences we use a difference-in-differences approach. As one example, part of the variation in Family Assistance is due to the presence (and ages) of children because families without children are not eligible. The decisions of childless individuals to partner will be affected by other changes in the economic and social environment over time. Thus we use childless women as a control group that experiences these other changes. The treatment group will be women with children who are eligible for the tax credits. The policy change is captured by the variation in Family Assistance tax credits over time. Even though this approach based on children has been used extensively in studies of the labour supply impacts of the EITC in the U.S. (Hotz and Scholz, 2003) and of tax reform in general (Moffitt and Willhelm, 2000), it has limitations. In particular, Family Assistance could affect some women's partnering choices today if they expect to have children in the near future. Consequently, we do some specification testing on this issue. Similar concerns are less likely to arise in the case of employment which is more likely based on current incomes.

Our treatment is more general than the example above, because we also exploit potential differences in eligibility based on income as proxied by educational qualifications or hourly wages. We cannot directly condition on income because of its endogeneity with respect to both the partnering and employment decisions. Instead, we assume that low education or (predicted) low hourly wage women are more likely eligible for Family Assistance, and therefore more likely affected by the policy change. We use predicted wages to avoid potential endogeneity of actual wages. As explained below, we also use the child/no child identifying strategy with samples restricted to low education or low wage women.

To explain further consider a sample that includes married and unmarried women, both working and non-working. Since the Working for Families reforms only apply to families with children, let those without children be the control group and those with children be the treatment group. Let a woman's propensity to marry be written as:

$$m_{it} = \alpha_0 + \alpha_1 T_i + \alpha_2 R_t + \alpha_3 T_i R_t + \alpha_z z_{it} + \alpha_t t + \varepsilon_{it}$$

and her propensity to work be expressed as:

$$p_{it} = \beta_0 + \beta_1 T_i + \beta_2 R_t + \beta_3 T_i R_t + \beta_z z_{it} + \beta_t t + \eta_{it}$$

where $T_i = 1$ indicates that the woman is in the treatment group (with children) and $T_i = 0$ indicates that she is in the control group (no children) prior to the reforms. The dummy variable R_t equals zero prior to the treatment year (2005) and one thereafter, and z_{it} includes background covariates such as age, education, ethnicity, and environmental variables such as region of residence and local unemployment rates. We include a simple time trend t to control for general non-treatment trends in partnering and employment that are common across the treatment and control groups. In some specifications we make this time trend more flexible by including year dummies. We assume ε_{it} and η_{it} are bivariate normal disturbance terms. We observe marriage if $m_{it} \geq 0$ or $M_{it} = 1$; $M_{it} = 0$ otherwise. We observe work if $p_{it} \geq 0$ or $P_{it} = 1$; $P_{it} = 0$ otherwise.

The coefficients α_1 and β_1 allow for differences between families with and without children in marriage or employment propensities that are not related to the reforms. The coefficients α_2 and β_2 show the time variation in marriage and employment that is not due to treatment, but rather due to other secular trends at the time of the policy change. The difference-in-differences coefficients α_3 and β_3 tell us the change in marriage or employment probabilities due to the reforms, that is, the impact of treatment on the treated. The childless women provide information on how marriage and employment are changing for women unaffected by the reforms, and the differences between them and the women with children show the impacts of the reforms.[1]

The essence of the approach is that we allow for non-policy-related differences in outcomes for those with and without children and for

[1] When using nonlinear predicted probabilities, as we do later, Norton et al. (2004) show that the interaction term itself will not estimate the correct difference-in-differences impact. Ignoring time trend t and subscripts, let the probability of employment be $F(\beta_z z + \beta_1 T + \beta_2 R + \beta_3 TR)$. The correct impact (which we use) is estimated by $F(\beta_z \bar{z} + \beta_1 + \beta_2 + \beta_3) - F(\beta_z \bar{z} + \beta_1) - [F(\beta_z \bar{z} + \beta_2) - F(\beta_z \bar{z})]$ where $\bar{z}$ is the sample mean. This differs from the simple change in probabilities due to the interaction which would be typically calculated as $F(\beta_z \bar{z} + \beta_1 \overline{T} + \beta_2 \overline{R} + \beta_3) - F(\beta_z \bar{z} + \beta_1 \overline{T} + \beta_2 \overline{R})$ where $\overline{T}$ and $\overline{R}$ are the sample means. We thank an anonymous referee for pointing this out.

common time effects, but identify the policy effects of the *changes* in behaviour between the two groups after the policy is implemented. We condition on background characteristics to make the treatment and controls more similar, conditional on z_{it}. These background characteristics can include child variables (like age of youngest child) as long as these effects are constant over time (in the absence of policy change) after conditioning on other variables (for further discussion on this point, see Hotz and Scholz (2003) or Moffitt and Willhelm (2000)).

Furthermore, we estimate the impact of the reforms on hours worked for the subsample of women who work. We estimate a linear regression of weekly hours of work on background characteristics including the time and treatment indicators and interactions. We estimate these hours equations separately for single and partnered women. In each case, we must control for selection into the working sample, as well as selection into the partnered or single sample. A bivariate selection model allows for separate but correlated treatment of the two selections, based on computing Heckman-type selection correction terms from the estimated bivariate probit coefficients (Ham, 1982). The double-selection model assumes a correlation between the regression error and the errors in the two selection equations, and results in the addition of two selection-correction terms in a two-step correction procedure. For the selected sample of those employed and partnered, we estimate the following:

$$E\left(Hours_{it} \middle| x_{it}, P_{it} = 1, M_{it} = 1\right) = \gamma_x x_{it} + \lambda_1 s_1 + \lambda_2 s_2$$

where s_1 and s_2 are the selection correction terms from Ham, which are computed using output from the bivariate probit estimation. We calculate robust standard errors to allow for possible heteroskedasticity from the selection model.[2]

Finally, as noted earlier, the In-Work payment sets up a jump in benefits if the family meets the work hours threshold of at least 20 hours per week if unpartnered, and 30 hours per week (combined) if partnered. To investigate whether families increase hours enough to qualify for this In-Work payment, we ran selection-corrected probits for the probability of meeting the hours threshold separately for partnered and unpartnered women. For partnered women, the husband's hours were summed with the partner's hours to get a family total. The probits are corrected for sample

[2] We do not control for sample design effects which may impact standard errors in the results that follow (in Section 6) as the data available to us did not contain replicate weights.

selection due to partner status. That is, we estimate the probability of exceeding the hours threshold conditional on partner status, and partner status is estimated jointly as a selection equation. This allows for correlation in unobservables between the equations, with identification coming from additional age-related variables on children in the partnering probit.[3] Nevertheless, the probability of exceeding the hours threshold should provide complementary information to the hours regression.

5. Data

A sample of both partnered and unpartnered adult women are used for this analysis. The women are aged 22 to 50. We exclude younger women to avoid those still completing education, and exclude older woman who may retire early. We use the repeated cross sectional annual Income Supplements to the Household Labour Force Survey (HLFS) for years 1997 to 2007, which span the recent Family Assistance changes as well as some changes in the Domestic Purposes Benefit. The pooled sample size of adult women in this age range is over 80,000.

Each Income Supplement to the HLFS provides individual data for approximately 15,000 households, and includes background characteristics on age, ethnicity ("prioritised" ethnicity codes), gender, educational qualifications, region of residence (12 regions), as well as wage and hours data. Earnings data come from the most recent pay period prior to the June Income Supplement. Labour force information is taken from the week prior to this survey. We merged in a measure of regional unemployment rates by year. The HLFS identifies family groups which allow us to match child age records with the records of the parent or parents.

One dependent variable is partner status. We identify couples using the family code: two adults living together listed as family parents. We exclude same-sex partners because their labour supply and partnering behaviour could be quite different from that of opposite-sex partners. The survey also asks about marital status independently of family grouping. We count a woman as partnered if she was either identified as a part of a couple from the family code or responded as married/cohabiting based on the marital status variable. Thus, we use a broad definition of partnering

[3] We included number of children aged 5 or less, number of children aged 6 to 12 and number of children aged 13+. In general, the age-of-children variables are significant in the selection probits. This comment applies to all of our underlying selection models that use additional age-of-children variables.

including both cohabitation and possibly some married women living apart from their spouses.

The second dependent variable is employment. We define a woman as employed if her usual hours worked per week in the survey period are positive. We use usual hours of work both as the dependent variable in the hours regressions and to define a binary variable for whether the In-Work payment hours threshold was exceeded.

Part of the analysis requires forming groups based on predicted wages. We predicted wages using a selection-corrected regression, with selection into employment allowing for different coefficients between partnered and unpartnered women. The models used covariates of age, age squared, educational qualification indicators, ethnicity indicators, region of residence, regional unemployment rate, and year indicators. The probit model for probability of selection additionally included several variables for number of children of various ages to aid in identification. A wage was predicted for each woman in the sample, including an adjustment for sample selection. For the employed women we estimated:

$$E\left(Wage_{it} \middle| x_{it}, P_{it} = 1\right) = \beta_x x_{it} + E\left(\varepsilon_{it} \middle| x_{it}, P_{it} = 1\right)$$

where the last term is a Heckman selection term (Mills ratio) for employment. For those not employed we estimated:

$$E\left(Wage_{it} \middle| x_{it}, P_{it} = 0\right) = \beta_x x_{it} + E\left(\varepsilon_{it} \middle| x_{it}, P_{it} = 0\right)$$

using a different Heckman selection term for the non-employed. The wage variable was adjusted to 2006 dollars using the June CPI in each year.

Table 1 shows the means of key variables separately for partnered and unpartnered women. Partnered women are on average more than three years older, more likely to have children and more likely to be European/Pakeha. They are more likely to have school or post-school qualifications. More of the partnered women work, but those employed report somewhat fewer work hours. All of these statistics are weighted by sample weights provided in the HLFS which correct for non-proportional sampling.

Table 1: Sample Means for Partnered and Non Partnered Women

Variable	**Partnered**	**Not Partnered**
Age	37.4	34.3
Has child (18 or younger) %	68.5	44.8
Education: No Qualifications %	17.3	23.1
Education: Bachelors or Higher %	17.7	17.4
Employed %	71.9	65.0
Average Hours for those Employed	32.5	35.1
Ethnicity: %		
European/Pakeha	75.0	67.1
Maori	9.3	18.8
Pacific Islander	5.1	6.6
Asian	5.9	4.2
Other	4.7	3.3
Sample Size	57,066	24,561

Notes: Merged HLFS-IS data 1997-2007 for women aged 22 to 50 weighted by Statistics New Zealand sample weights.

6. Empirical Results

In this section, we begin with time trend plots for both partnering and employment. The results from probit models that allow us to condition on covariates and make formal hypothesis tests on the policy impacts are then discussed. We next present estimates for hours of work, corrected for bivariate selection on both partnering and employment. Finally, the results on the probabilities of exceeding the hours thresholds are presented separately for partnered and unpartnered women correcting for the selection on partnering. We work through all of these specifications for each of our three difference-in-differences identification strategies. That is, we look separately for changes in partnering or employment levels that occur following the Family Assistance changes in 2005 for both our policy treatment and control groups. Our treatment groups are those most likely to be eligible: those with children, low education levels, or low wages. We begin by considering women with children versus those without children.

Table 2: Difference-in-Differences Estimates of WFF Impact on Partnering

A. Probability of Partnering Probit: Column labels are group identifiers

Variable	**Has Child**	**Has Child**	**Low Education**	**Low Education**	**Low Wage**	**Low Wage**
Year	-0.001	-0.003***	0.002	0.000	-0.005***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
D05	-0.000	0.007	0.025**	0.016	0.003	0.023***
	(0.007)	(0.007)	(0.010)	(0.011)	(0.008)	(0.007)
Identifier	0.193***	0.114***	-0.049***	-0.081***	-0.294***	-0.357***
	(0.004)	(0.004)	(0.006)	(0.007)	(0.005)	(0.008)
Identifier • D05	**0.007**	**0.003**	**-0.042*****	**-0.025****	**0.025****	**0.014**
	(0.008)	**(0.007)**	**(0.011)**	**(0.012)**	**(0.010)**	**(0.010)**
Has Covariates?	No	Yes	No	Yes	No	Yes
Log Likelihood	-45,274.4	-42,427.2	-18,199.2	-16,534.5	-28,206.3	-25,907.8
Sample Size	81,627	81,627	30,236	30,236	54,415	54,415

Notes: Merged HLFS-IS data 1997-2007 for women aged 22 to 50 weighted by Statistics New Zealand sample weights. Covariates include age, age-squared, ethnicity indicators, region indicators, age of youngest child. The 'Has Child' specification includes education qualification indicators. Coefficients are partial derivatives of probabilities, and standard errors are included in parentheses. The reported results on the interaction terms (Identifier • D05) are corrected difference-in-difference probabilities as derived in Norton et al. (2004) evaluated at the mean value of regressors, and the standard errors are computed using the appropriate delta method (Corneliβen and Sonderhof, 2008). Significance levels: *** 1%, ** 5% and * 10%

Table 2 (Continued): Difference-in-Differences Estimates of WFF Impact on Employment

B. Probability of Employment Probit ─ Partnered: Column labels are group identifiers

Variable	**Has Child**	**Has Child**	**Low Education**	**Low Education**	**Low Wage**	**Low Wage**
Year	0.005*** (0.001)	-0.002 (0.001)	0.006*** (0.002)	-0.001 (0.002)	0.004*** (0.001)	-0.004** (0.002)
D05	-0.005 (0.010)	0.002 (0.010)	-0.019 (0.013)	-0.007 (0.014)	0.024*** (0.009)	0.012 (0.009)
Identifier	-0.186*** (0.004)	-0.083*** (0.005)	-0.154*** (0.008)	-0.191*** (0.009)	-0.044*** (0.006)	-0.003 (0.011)
Identifier • D05	**0.020** (0.008)**	**0.016* (0.009)**	**0.036*** (0.015)**	**0.041*** (0.015)**	**-0.026** (0.012)**	**0.025** (0.012)**
Has Covariates?	No	Yes	No	Yes	No	Yes
Log Likelihood	-32,768.3	-29,776.4	-12,183.1	-10,850.6	-22,809.9	-20,179.2
Sample Size	57,066	57,066	20,249	20,249	37,937	37,937

C. Probability of Employment Probit ─ Unpartnered: Column labels are group identifiers

Variable	Has Child	Has Child	Low Education	Low Education	Low Wage	Low Wage
Year	0.007***	0.002	0.004	0.007*	0.003	0.003
	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)
D05	-0.014	0.0003	0.018	0.022	0.047***	0.028
	(0.013)	(0.014)	(0.023)	(0.024)	(0.009)	(0.020)
Identifier	-0.306***	-0.169***	-0.425***	-0.350***	-0.142***	0.045**
	(0.007)	(0.009)	(0.010)	(0.013)	(0.009)	(0.021)
Identifier • D05	**No**	**0.044*** **	**0.020**	**0.008**	**-0.058*** **	**-0.022**
	Convergence	**(0.014)**	**(0.020)**	**(0.022)**	**(0.017)**	**(0.021)**
Has Covariates?	No	Yes	No	Yes	No	Yes
Log Likelihood	-14,706.1	-13,123.1	-5,646.8	-5,140.7	-10,495.8	-8,882.3
Sample Size	24,561	24,561	9,987	9,987	16,478	16,478

Notes: Merged HLFS-IS data 1997-2007 for women aged 22 to 50 weighted by Statistics New Zealand sample weights. Covariates include age, age-squared, ethnicity indicators, region indicators, age of youngest child and a selection correction term for partnering. The 'Has Child' specification includes education qualification indicators. Coefficients are partial derivatives of probabilities, and standard errors are included in parentheses The reported results on the interaction terms (Identifier • D05) are corrected difference-in-difference probabilities as derived in Norton et al. (2004) evaluated at the mean value of regressors, and the standard errors are computed using the appropriate delta method (Corneliβen and Sonderhof, 2008). Significance levels: *** 1%, ** 5% and * 10%

6.1 A. Presence of Children as a Policy Identifier Partnering

Figure A1 in the Appendix plots the proportion of women partnered between 1997 and 2007. There appears to be a slight dip in this propensity after 2005. When we plot this proportion separately for women with and without children, we can see some differences. While childless women show a slight dip, those with children show a slight rise in the proportion partnered. This suggests that the Family Assistance expansion did not reduce partnering in absolute levels.

To test this hypothesis, we ran a probit model for partnering, which included a time trend, an indicator for presence of children, an indicator for the policy change years 2005, 2006 and 2007 (D05), and the interaction between presence of children and this policy dummy. The interaction term tells the tale. These results are shown in the first two columns in Panel A of Table 2.

The first column of Table 2 shows that the partnering change is not statistically significant at conventional levels. The second column reports on a specification that adds several background covariates (estimated coefficients not shown). This makes the groups more comparable and controls for potential changes in partnering caused by modifications in the composition of the samples over time. The second column results condition on qualifications (and other things) so that any change in education will not drive the partnering results. The results in this second column tell the same story as the first. We conclude that adding additional covariates does not matter and the policy does not appear to have influenced partnering over the period from 2005 to 2007.

6.2 Employment and Hours of Work

Figures A2 and A3 in the Appendix display the separate time trends for the employment of partnered and unpartnered women. For both partnered and unpartnered women, employment has been rising primarily among women with children. The employment propensities for women without children have been relatively stable over this time period, suggesting that the policy has had a positive impact on employment of both partnered and unpartnered mothers. The employment probits in Table 2 confirm the conclusion derived from these figures. There is a significant increase in employment for both partnered and unpartnered women with children after the policy change.[4]

[4] Attempts to compute the policy effect on this interacted variable for unpartnered women resulted in non-convergence when no other covariates were included in the regression (first column of Panel C in Table 2). However, when other covariates

We ran a joint model of marriage and employment that allows correlation of the error terms for the two equations shown in Section 4. The advantage of the joint model is that unobservable influences likely correlate across the two dependent variables and the bivariate probit uses that correlation to improve the precision of the estimated coefficients. All models include the background characteristics listed in the notes at the bottom of each table. Results from the full model are shown in Panel A of Table A1 in the Appendix. The estimated correlation in unobservables between the partnering and employment equations is 0.234 and statistically significant at better than a 1% level. This positive correlation indicates that women who have unobservable traits making them more likely to be employed are also women with unobservables that make them more likely to be partnered. This argues against the Becker notion that women with comparative advantage in the market are less likely to marry, and instead argues that some traits may encourage both marriage and work (Nakosteen, Westerlund et al. 2004; Blackaby, Carlin et al. 2007). These bivariate probits are used to construct the Heckman correction terms included in the hours regressions. Table 3 displays only the abbreviated results for the policy interactions.

Table 3: Hours Regressions: With and Without Children
Interaction Coefficients for Difference-in-Differences

A. Hours of Work for Employed Women: Selection Corrected Regression

	Partnered Women		Unpartnered Women	
Variable	**Hours Coefficients**	**Standard Errors**	**Hours Coefficients**	**Standard Errors**
Year	0.004	0.046	-0.004	0.074
D05	-1.317***	0.292	-0.023	0.410
Has Kids	-5.591***	0.210	-8.565***	0.381
D05 • Has Kids	**1.002*****	**0.299**	**1.001****	**0.509**
Root MSE	13.625		13.061	
R Squared	0.113		0.134	
F for all Zero Coefficients	180.72		77.16	
Sample Size	40,596		15,207	

were included in the estimation, which is our preferred specification, convergence was reached and the marginal effect was positive and significant (second column of Panel C in Table 2).

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education qualifications, unemployment rate and two selection correction terms. Robust Standard Errors are reported. Bivariate probit estimation on partnering and employment was used to produce Heckman correction terms for these regressions with additional age of children variables as identifiers. These bivariate probit results are not reported.

B. Probits for Hours Thresholds: Selection Corrected for Partner Status

	Partnered Women 30 or More Combined Work Hours for Couple		Unpartnered Women 20 or More Work Hours	
Variable	**Coefficients**	**Standard Errors**	**Coefficients**	**Standard Errors**
Year	-0.008	0.005	0.0003	0.006
D05	-0.007	0.038	0.014	0.038
Has Kids	0.083**	0.032	-0.308***	0.044
D05 • Has Kids	**0.035*****	**0.012**	**0.028****	**0.011**
Rho	0.789	0.052	-0.702	0.058
Chi Squared	2,747.9		3,398	
Sample Size (uncensored)	57,066		24,561	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education qualifications, and unemployment rate (for employment probit). Selection on partnering model adds number of children less than age 6, number of children 6-12 and number of children 13-15 to the regression. The reported results on the interaction terms (D05 • Has Kids) are corrected difference-in-difference probabilities as derived in Norton et al. (2004) evaluated at the mean value of regressors, and the standard errors are computed using the appropriate delta method (Corneliβen and Sonderhof, 2008).

Significance levels: *** 1%, ** 5% and * 10%

Panel A in Table 3 shows the impact on hours of work for those employed. Even though we see policy effects on the employment decision, the same policy could have quite different effects on the hours worked for those employed. For both partnered and unpartnered women, those with children worked on average slightly more than one hour per week after the policy change relative to women without children. These policy effects are significantly different from zero at a 1% level for partnered women and 5% level for unpartnered women. The magnitude of these policy responses can be interpreted relative to the sample means for the number of hours worked per week in Table 1. They amount to approximately 3% increases in the length of the workweeks.

Panel B in Table 3 displays the hours threshold probits. Like the results in Table 2, these regressions are corrected for selection on partnering. We find a sizable and statistically significant increase in the probability that couples work 30 or more hours after 2005 for those with children compared to those without children. This policy effect could be the result of more hours worked by either the woman or her partner. The rho coefficient indicates a positive correlation in unobservables for being partnered and hours beyond the threshold, consistent with the correlation in the bivariate probit discussed above. For unpartnered women, we estimate a positive coefficient on the interaction for those with children, and this effect is also statistically significant. We find that the policy had positive effects on working beyond the hours threshold for both couples and unpartnered women.

In short, using children to define the group most likely to be affected by the policy suggests that the Family Assistance changes may have had positive effects on employment and hours of work, but not on partnering.

6.3 B. Low Education as a Policy Identifier

Women with low levels of education are more likely to be eligible for Family Assistance. We conduct the same analysis as above using 'low education women' (no qualifications) as our treatment group and 'high education women' (bachelor's degree or better) as our control group. We eliminate the middle group because it would include more of a mix of eligibles and ineligibles; thus we expect a clearer contrast without the middle group who have a qualification below a bachelor's degree. The results differ in some respects from those when the presence of children was used as a policy identifier.

The figures in the Appendix show the basic trends. Partnering displays a positive trend for the more educated, and no trend for the less educated. The difference-in-differences logic suggests that partnering by the low education group would have been higher in the absence of the policy. Results in Table 2 confirm that there is a statistically significant reduction in partnering by more than 4 percentage points for that group beginning in 2005. Adding covariates reduces the magnitude of this effect to less than 3 percentage points, but it remains statistically significant. Thus there is some evidence that recent policy reduced partnering, but only for this specification. We discount the result because it flows from a *rise* in partnering among more educated, with no obvious explanation since they are not affected by the policy, rather than a fall for the less educated, and because low education is clearly a coarse proxy for eligibility.

Turning to employment propensities, we see quite different policy effects for partnered and unpartnered women. These differences highlight the advantages of estimating employment responses for the two groups. The employment trends in Figures A2 and A3 show an increase in employment for the less educated women relative to the more educated women after 2005. These effects are statistically significant in both sets of probit results in Panel B of Table 2. Using our preferred specification that includes covariates, this point estimate implies a 4.1 percentage-point increase in the employment propensity of partnered women without qualifications. This represents a 5.7% increase in the employment propensity at the sample mean. Yet, there is no similar increase in the employment of unpartnered women with low education. This is true in both Figure A3 and the two sets of probit results in Panel B of Table 2. The estimated coefficients are both positive, but statistically insignificant.

For hours of work, we find positive and statistically significant effects for both less educated partnered and unpartnered women relative to their more educated counterparts (Panel A of Table 4). However, the estimated effect for partnered women (3.129) is somewhat larger than that for unpartnered women (1.920). This suggests that the policy is inducing unpartnered working women with low education to work more hours per week, even though there is no statistically significant impact on the proportion employed. It also suggested that for those without qualifications, partnered women are relatively more responsive to this policy change in both their employment propensity and hours of work than their unpartnered counterparts. The bottom panel shows that we observe no increase in the probability that work hours exceed the thresholds for the low education women compared to high education.

Table 4: Hours Regressions: Low and High Education
Interaction Coefficients for Difference-in-Differences

A. Hours of Work for Employed Women: Selection Corrected Regression

	Partnered Women		Unpartnered Women	
Variable	**Hours Coefficients**	**Standard Errors**	**Hours Coefficients**	**Standard Errors**
Year	-0.160*	0.083	-0.109	0.125
D05	-1.858***	0.466	0.576	0.697
Low Education	-0.459	0.580	-5.970***	0.670
D05 • Low Education	**3.129*** **	**0.524**	**1.920** **	**0.783**
Root MSE	14.04		13.39	
R Squared	0.094		0.123	
F for all Zero Coefficients	58.06		32.44	
Sample Size	13,687		5,639	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, unemployment rate and two selection correction terms. Robust Standard Errors are reported. Bivariate probit estimation on partnering and employment was used to produce Heckman correction terms for these regressions with additional age of children variables as identifiers. These bivariate probit results are not reported.

B. Probits for Hours Thresholds: Selection Corrected for Partner Status

	Partnered Women 30 or More Combined Work Hours for couple		Unpartnered Women 20 or More Work Hours	
Variable	**Coefficients**	**Standard Errors**	**Coefficients**	**Standard Errors**
Year	-0.004	0.009	0.011	0.008
D05	0.089	0.057	0.096*	0.060
Low Education	-0.507***	0.031	-0.869***	0.039
D05 • Low Education	**0.001**	**0.015**	**0.012**	**0.017**
Rho	0.649	0.077	-0.851	0.025
Chi Squared	1,363.78		1,621.34	
Sample Size (uncensored)	20,249		9,987	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education qualifications, and unemployment rate (for employment probit). Selection on partnering model adds number of children less than age 6, number of children 6-12 and number of children 13-15 to the regression. The reported results on the interaction terms (D05 • Low Education) are corrected difference-in-difference probabilities as derived in Norton et al. (2004) evaluated at the mean value of regressors, and the standard errors are computed using the appropriate delta method (Corneliβen and Sonderhof, 2008).

Significance levels: *** 1%, ** 5% and * 10%

6.4 C. Low wage as policy identifier

Our final method divides the sample into three equally-sized groups based on predicted hourly wages. In a sense, this is very similar to the low versus high education grouping. However, predicted wages are based on age, region, ethnicity and unemployment rates in addition to education. To make the contrast between the groups more clear, we keep those in the bottom and top thirds of predicted wages and delete the middle third. Note that the sample size is larger for the wage groups than for the education groups.

The results in Figure A1 in the Appendix and Table 2 show few differences in partnering trends. The estimated coefficient on the low wage group is positive and significant in the regression without covariates, but this effect disappears when covariates are added to the model (Panel A of Table 2). There is a negative employment effect for the low wage group relative to the high wage group after the policy change among partnered women (Panel B of Table 2). Once covariates are added to the regression, however, the policy is found to have the expected positive effect on employment status of these women. The negative and significant employment effect for the low wage group among unpartnered women becomes insignificant once covariates are added to the regression (Panel C of Table 2).

For hours of work, we find results that are qualitatively similar to those found earlier for the low and high education groups. Positive and statistically significant effects for both low wage partnered and unpartnered women relative to their high wage counterparts are shown in Panel A of Table 5. However, the estimated effect for partnered women (1.529) is somewhat larger than that for unpartnered women (1.227). The bottom panel shows that low wage partnered women have a higher relative probability of exceeding the work hours threshold after this policy change in 2005. The same is not true of low wage unpartnered women.

Table 5: Hours Regressions: Low and High Wages
Interaction Coefficients for Difference-in-Differences

A. Hours of Work for Employed Women: Selection Corrected Regression

	Partnered Women		Unpartnered Women	
Variable	**Hours Coefficients**	**Standard Errors**	**Hours Coefficients**	**Standard Errors**
Year	0.098*	0.059	-0.232**	0.092
D05	-1.791***	0.316	-0.064	0.614
Low Wage	5.831***	0.393	-0.635	0.827
D05 • Low Wage	**1.529*** **	**0.407**	**1.227* **	**0.638**
Root MSE	13.755		13.402	
R Squared	0.114		0.089	
F for all Zero Coefficients	115.17		35.66	
Sample Size	26,456		10,125	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education indicators, unemployment rate and two selection correction terms. Robust Standard Errors are reported. Bivariate probit estimation on partnering and employment was used to produce Heckman correction terms for these regressions with additional age of children variables as identifiers. These bivariate probit results are not reported.

B. Probits for Hours Thresholds: Selection Corrected for Partner Status

	Partnered Women 30 or More Combined Work Hours for couple		Unpartnered Women 20 or More Work Hours	
Variable	**Coefficients**	**Standard Errors**	**Coefficients**	**Standard Errors**
Year	-0.007	0.007	-0.012*	0.007
D05	0.063	0.042	0.111**	0.053
Low Wage	-0.277***	0.081	-0.551***	0.060
D05 • Low Wage	**0.089* **	**0.051**	**-0.041**	**0.055**
Rho	0.379	0.107	-0.776	0.028
Chi Squared	2,272.78		2,351.18	
Sample Size (uncensored)	37,937		16,478	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education qualifications and unemployment rate (for employment probit). Selection on partnering model adds number of children less than age 6, number of children 6-12 and number of children 13-15 to the regression The reported results on the interaction terms (D05 • Low Wage) are corrected difference-in-difference probabilities as derived in Norton et al. (2004) evaluated at the mean value of regressors, and the standard errors are computed using the appropriate delta method (Corneliβen and Sonderhof, 2008).

Significance levels: *** 1%, ** 5% and * 10%

6.5 D. Additional Specification Tests

To sharpen contrasts further, we also explored the use of children/no children as our policy identifier, but with samples restricted to those likely to be eligible for Family Assistance based on low education or low predicted wages. These restricted samples lead to estimates with less precision than our previous models. When we restricted the sample to women with low wages and used the child policy identifier, we do not observe significant policy effects in employment, partnering or hours worked (see Table 6). For the low education subgroup, we do not observe a statistically significant policy impact on partnering or employment between those with and without children, but we do observe a positive hours response (Table 7). For the less educated, there is an increase of slightly less than 2.5 hours per week for working partnered women with children and about 3.5 hours per week for working unpartnered women with children compared to women in this group without children. As mentioned earlier, we observe a significant increase after 2005 in the probability that couples with children have work hours that exceed the 30 hour threshold compared to those with no children. We conclude that using the restricted samples produces similar but weaker evidence on these policy effects relative to those shown before.

Table 6: Hours Regressions
Low Wage Subsample With and Without Children
Interaction Coefficients for Difference-in-Differences

A. Hours of Work for Employed Women: Selection Corrected Regression

	Partnered Women		Unpartnered Women	
Variable	**Hours Coefficients**	**Standard Errors**	**Hours Coefficients**	**Standard Errors**
Year	0.239**	0.104	-0.242*	0.125
D05	-0.870	0.667	1.476**	0.735
Has Kids	-3.563***	0.467	-8.208***	0.529
D05 • Has Kids	**-0.219**	**0.711**	**0.407**	**1.008**
Root MSE	13.057		13.051	
R Squared	0.071		0.107	
Sample Size	9,885		7,033	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education indicators, unemployment rate and two selection correction terms. Robust Standard Errors are reported. Bivariate probit estimation on partnering and employment was used to produce Heckman correction terms for these regressions with additional age of children variables as identifiers. These bivariate probit results are not reported.

B. Probits for Hours Thresholds: Selection Corrected for Partner Status

	Partnered Women 30 or More Combined Work Hours for couple		Unpartnered Women 20 or More Work Hours	
Variable	**Coefficients**	**Standard Errors**	**Coefficients**	**Standard Errors**
Year	-0.019*	0.010	-0.010	0.010
D05	0.142**	0.071	0.095	0.061
Has Kids	-0.020	0.044	-0.461***	0.050
D05 • Has Kids	**0.092**	**0.075**	**0.036**	**0.071**
Rho	0.519	0.119	-0.379	0.107
Chi Squared	310.32		813.99	
Sample Size (uncensored)	14,922		12,287	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education qualifications and unemployment rate (for employment probit). Selection on partnering model adds number of children less than age 6, number of children 6-12 and number of children 13-15 to the regression The reported results on the interaction terms (D05 • Has Kids) are corrected difference-in-difference probabilities as derived in Norton et al. (2004) evaluated at the mean value of regressors, and the standard errors are computed using the appropriate delta method (Corneliβen and Sonderhof, 2008).

Significance levels: *** 1%, ** 5% and * 10%

Table 7: Hours Regressions: Low Education Subsample With and Without Children
Interaction Coefficients for Difference-in-Differences

A. Hours of Work for Employed Women: Selection Corrected Regression

	Partnered Women		Unpartnered Women	
Variable	**Hours Coefficients**	**Standard Errors**	**Hours Coefficients**	**Standard Errors**
Year	-0.133	0.137	0.057	0.213
D05	-2.773***	0.962	-0.477	1.665
Has Kids	-4.873***	0.564	-6.910***	1.280
D05 • Has Kids	**2.457*****	**0.955**	**3.511****	**1.729**
Root MSE	13.956		13.72	
R Squared	0.093		0.126	
Sample Size	6,941		2,631	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education indicators, unemployment rate and two selection correction terms. Robust Standard Errors are reported. Bivariate probit estimation on partnering and employment was used to produce Heckman correction terms for these regressions with additional age of children variables as identifiers. These bivariate probit results are not reported.

B. Probits for Hours Thresholds: Selection Corrected for Partner Status

	Partnered Women 30 or More Combined Work Hours for couple		Unpartnered Women 20 or More Work Hours	
Variable	**Coefficients**	**Standard Errors**	**Coefficients**	**Standard Errors**
Year	0.0004	0.008	0.026**	0.011
D05	0.021	0.066	-0.041	0.074
Has Kids	0.213***	0.036	-0.178*	0.096
D05 • Has Kids	**0.038**	**0.065**	**0.092**	**0.076**
Rho	0.906	0.033	-0.790	0.121
Chi Squared	783.51		-	
Sample Size (uncensored)	11,518		6,472	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education qualifications, and unemployment rate (for employment probit). Selection on partnering model adds number of children less than age 6, number of children 6-12 and number of children 13-15 to the regression. The reported results on the interaction terms (D05 • Has Kids) are corrected difference-in-difference probabilities as derived in Norton et al. (2004) evaluated at the mean value of regressors, and the standard errors are computed using the appropriate delta method (Corneliβen and Sonderhof, 2008).

Significance levels: *** 1%, ** 5% and * 10%

We also experimented with restricting the age range of the sample to 22 to 40 years of age, the more relevant partnering years. The results are qualitatively the same, with some stronger and more precisely estimated employment effects, but no substantial differences in partnering effects.

We also checked alternate specifications regarding the treatment of children. Since we deal largely with families whose children were born prior to the policy change in 2005, we doubt endogeneity due to fertility effects of the policy change is a problem. To check this, we excluded women whose youngest child was less than 2 years old. This eliminates women who might have had a child in an effort to become eligible for these benefits during the two-year policy period we observe. The pattern of significant results using the sample with no young children is the same as before. Hours of work changes are larger in this restricted sample, but smaller in some specifications. We conclude that there is no qualitative difference in the results. In addition, we re-estimated the initial education group and wage group models excluding all children variables as potentially endogenous. Again, the results did not qualitatively change.

7. Discussion and Future Directions

We conducted difference-in-differences analyses using three alternative identifying assumptions. The results from our different assumptions gave somewhat mixed results. These results are summarised in Table 8. We favour the comparison of women with and without children as the best policy identifier. For partnering, we found no significant policy effects from changes in Family Assistance for the groups with children or facing low predicted wages. For the low education group there was a possible partnering effect in the hypothesised direction, but we discount it for reasons given above.

Employment rose significantly for women with children relative to childless women. This effect was somewhat larger among unpartnered women, so the policy may be producing a larger employment response among sole mothers. When low education and low predicted wages were used as policy indentifies, the employment responses appear to be concentrated among partnered women. We consistently found positive effects on hours worked among those employed in almost all specifications. In addition, the proportion of couples with combined hours of work that exceed the In-Work payment threshold of 30 hours per week increased in the policy period for those with children. A similar effect is found for unpartnered women with children whose hours of work exceeded the 20-hour threshold for the In-Work payment. There is little evidence of these hours threshold effects in other specifications. Overall, our analysis finds little evidence to suggest that these policies influenced partnering, but we do find evidence of positive effects on employment and hours of work. Yet, several caveats should be borne in mind[5].

[5] One such caveat to note is that the difference-in-differences methodology is unable to distinguish policy impacts from group-specific time trends. For example, hours of work may have increased for groups targeted by WFF policies relative to other groups because of the recent economic expansion even without these policy changes. However, there is no obvious reason why this would have occurred.

Table 8: Summary of Interaction Effects for Difference-in-Differences Estimation

				For Employed Women			
		Probability of Employment		Hours of Work		Hours Thresholds	
Policy Identifiers	**Probability of Partnering**	**Partnered**	**Unpartnered**	**Partnered**	**Unpartnered**	**Partnered 30 or more**	**Unpartnered 20 or More**
Has Child	0.003	0.016*	0.044***	1.002***	1.001***	0.035***	0.028**
Low Education	-0.025**	0.041**	0.008	3.129***	1.920**	0.001	0.012
Low Wage	0.014	0.025**	-0.022	1.529***	1.227*	0.089*	-0.041
Low Wage/Has Child	---	---	---	-0.219	0.407	0.092	0.036
Low Education/Has Child	---	---	---	2.457***	3.511**	0.038	0.092

Notes: Results reported in the first three columns are taken from Table 2 and pertain to our preferred regressions with covariates. The results in the last four columns are taken from Tables 3 through 7. See the notes at the bottom of these tables for further information on these regression results.

Significance levels: *** 1%, ** 5% and * 10%

Firstly, we have a relatively short period over which to observe any behavioural adjustments to these policy changes. This is particularly true for partnering decisions. Families have to learn about the new rules and learn about how they would be personally affected. Families in our data have not had long to react to the Family Assistance changes. We might expect larger impacts over a longer time horizon, consistent with evidence on marriage from experiments (Gassman-Pines and Yoshikawa 2006). On the other hand, we expect that labour supply changes might occur more easily and quickly, and we do observe some increases in employment propensities and hours of work for those with children.

Secondly, our analysis has been restricted to population proportions or levels. *Transition rates* from unpartnered to partnered or not-employed to employed states would show responses to policy more quickly that the overall stocks of partnered or employed women. But we did not have access to longitudinal data that spanned the period of the policy change. In future years, the Longitudinal Survey of Families, Income and Employment will provide better evidence on changes in these transition rates.

Thirdly, our difference-in-differences type of analysis is only as good as the identifying assumptions. We are forced to assume that 2005 is a good start date for the policy. The policy environment in NZ is more fluid than that. Some changes to Family Assistance were made in 2004, but we treat these as minor. Some people may have responded in anticipation of these policy changes, while others may have responded with a considerable lag. Furthermore, other policy changes occurred over the time period and the lagged responses to these changes, to the extent that they occurred, may confound our results.

Fourthly, we assume that our identifying groups are sharply divided enough to distinguish likely eligibility, but not so different that one group will not serve as a valid control. We hope that by comparing three different identifying assumptions and including additional tests on subsamples that we allay some of these fears. Moreover, we estimate policy effects for potential eligibles. The impacts for those who participate in the programme are likely to be somewhat larger.

Future work could include adding additional years of HLFS and Income Supplement data to lengthen the time period for the observation of these effects or using longitudinal data.[6] We have not examined the impact

[6] Future work could also make use of propensity score matching techniques as an additional form of policy analysis. However, matching would be better suited to

of Family Assistance on labour force participation and unemployment. Since it could be argued that these policies were designed partly to encourage unemployed women into work, this issue might merit further analysis. Another avenue would be to move beyond the descriptive difference-in-differences approach, and develop a joint structural estimation of discrete labour supply and partnering status wherein a woman chooses both her partner status and her work hours, based on income expectations that depend on tax and benefit policies. Such a model could estimate responses to changes in benefit amounts.

At this point, subject to the caveats above, we provide some evidence of employment increases and more solid evidence of work hours increases for those working due to the Family Assistance policy changes. Evidence on partnering is more elusive but there are certainly no large impacts currently.

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Appendix – Table A1
Bivariate Probits and Hours Regression: With and Without Children
Full Set of Coefficients

A. Bivariate Probit Coefficients for Partnered and Employed

	Partnered Equation		Employment Equation	
Variable	Coefficients	Standard Errors	Coefficients	Standard Errors
Year	-0.007***	0.002	0.002	0.003
D05	0.020	0.022	-0.013	0.024
Has kids	0.359***	0.013	-0.279***	0.014
D05 • Has Kids	0.012	0.022	0.068***	0.024
Age	0.104***	0.006	0.067***	0.007
Age Squared	-0.001***	0.000	-0.001**	0.000
Maori	-0.499***	0.014	-0.296***	0.014
Pacific Island	-0.178***	0.021	-0.219***	0.021
Asian	0.317***	0.027	-0.513***	0.024
Other	0.150***	0.027	-0.599***	0.025
None	0.023	0.160	-0.336***	0.153
Num. Kids < age 6	0.275***	0.009	-0.447***	0.008
Region 2	-0.085***	0.026	-0.038	0.029
Region 3	0.003	0.028	-0.005	0.031
Region 4	0.014	0.030	0.051*	0.031
Region 5	-0.021	0.029	-0.006	0.032
Region 6	-0.016	0.032	0.025	0.036
Region 7	-0.043	0.030	-0.049	0.033
Region 8	-0.073***	0.027	0.050	0.032
Region 9	0.086***	0.030	-0.005	0.036
Region 10	-0.045*	0.027	0.031	0.031
Region 11	0.012	0.031	0.040	0.034
Region 12	0.122***	0.033	0.002	0.039
Primary/School Cert	0.237***	0.017	0.377***	0.017
Sixth Form/Bursary	0.312***	0.017	0.410***	0.017

Vocational	0.186***	0.013	0.532***	0.013
Bachelors	0.309***	0.019	0.704***	0.019
Post Grad Degree	0.195***	0.027	0.801***	0.029
Unemployment Rate	---	---	-0.028***	0.005
Constant	12.281***	4.739	-5.233	6.711
Rho	0.234***	0.006		
Chi Square		20,012.92		
Sample Size		81,627		

Notes: Models also include age, age squared, ethnicity indicators, region indicators, presence of children, education qualifications, number of children less than age 6 and unemployment rate (for employment probit). Robust Standard Errors are reported.

Significance levels: *** 1%, ** 5% and * 10%

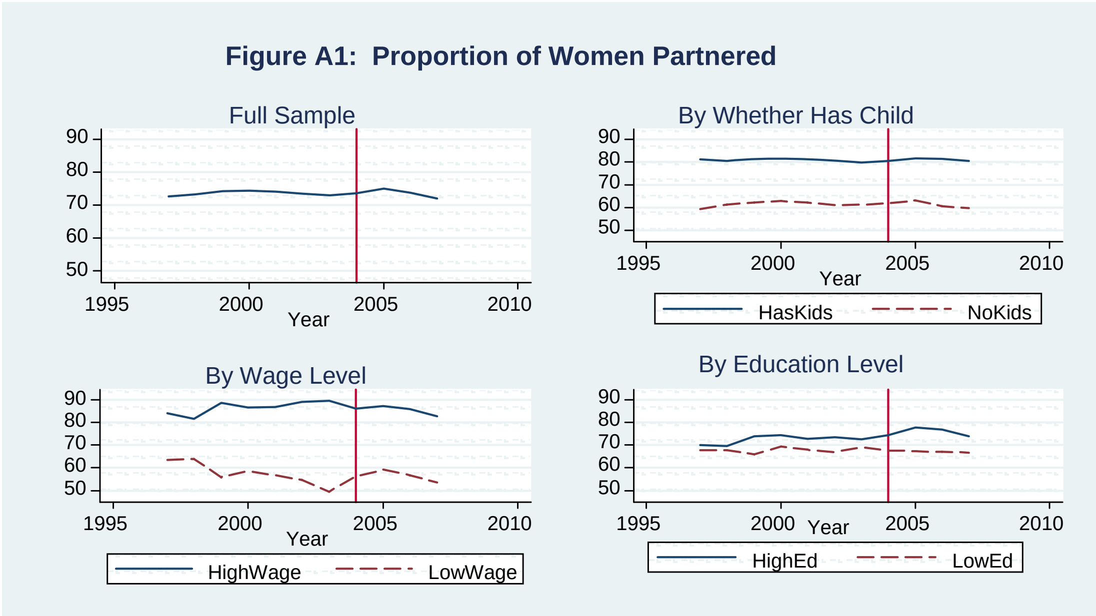
B. Hours of Work for Employed Women: Selection Corrected Regression

	Partnered Women		Unpartnered Women	
Variable	Coefficients	Standard Errors	Coefficients	Standard Errors
Year	0.004	0.046	-0.004	0.074
D05	-1.317***	0.292	-0.023	0.410
Has kids	-5.591***	0.210	-8.565***	0.381
D05 • Has Kids	1.002***	0.299	1.001**	0.509
Age	-0.399***	0.106	0.852***	0.149
Age Squared	0.006***	0.001	-0.010***	0.002
Maori	4.096***	0.251	-0.812**	0.375
Pacific Island	5.132***	0.297	1.277***	0.444
Asian	6.847***	0.437	-1.889***	0.793
Other	3.713***	0.432	-2.404***	0.792
None	4.942***	1.795	-0.538	2.666
Region 2	1.630***	0.461	2.871***	0.725
Region 3	0.454	0.491	0.595	0.783
Region 4	-0.012	0.489	1.661**	0.784

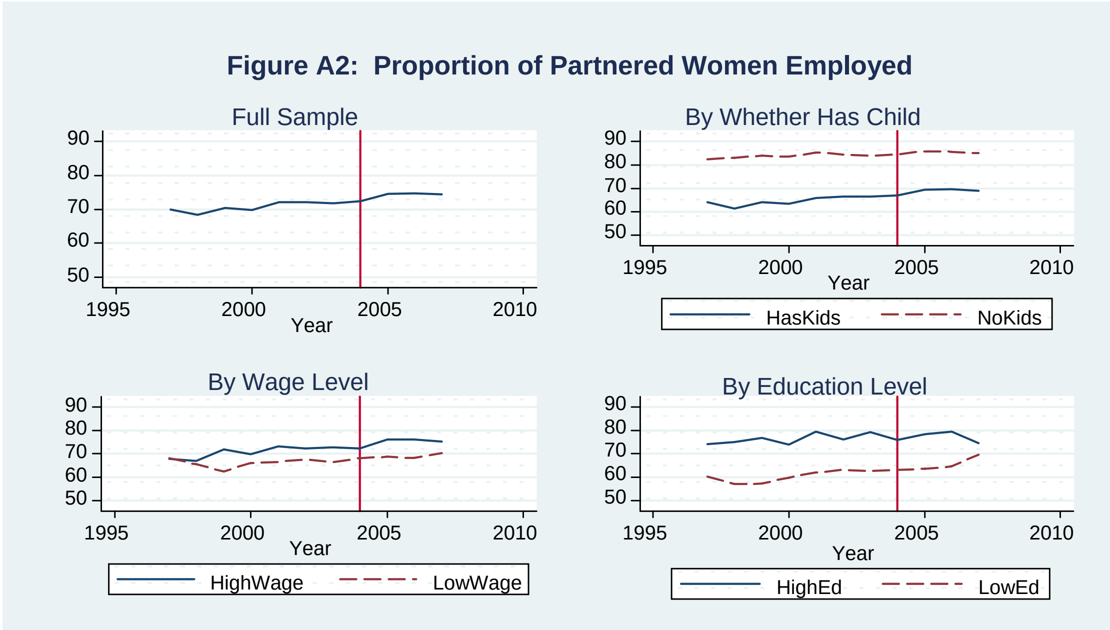
Region 5	-0.315	0.504	0.952	0.800
Region 6	-0.225	0.552	-0.650	0.871
Region 7	0.062	0.505	-0.733	0.829
Region 8	0.904*	0.480	1.663**	0.764
Region 9	-0.321	0.547	0.550	0.883
Region 10	-1.470***	0.479	0.302	0.762
Region 11	-1.110**	0.507	-1.801**	0.806
Region 12	-0.456	0.590	0.478	0.993
Primary/School Cert	-0.452	0.263	1.949***	0.417
Sixth Form/Bursary	0.175	0.278	1.757***	0.419
Vocational	-0.346	0.237	2.687***	0.357
Bachelors	1.172***	0.366	5.856***	0.506
Post Grad Degree	0.234	0.501	6.899***	0.889
Unemployment Rate	0.050	0.075	-0.030	0.118
Lambda Partnered	0.105***	0.007	-0.003	0.008
Lambda Employed	-0.039***	0.005	0.104***	0.027
Constant	32.759	93.349	23.383	147.717
Root MSE	13.625		13.061	
R Squared	0.113		0.134	
F for all Zero Coefficients	180.72		77.16	
Sample Size	40,596		15,207	

Notes: Models also include age, age squared, ethnicity indicators, region indicators, education qualifications, unemployment rate, and two selection correction terms. Robust Standard Errors are reported.

Significance levels: *** 1%, ** 5% and * 10%



Notes: Merged HLFS-IS data 1997-2007 for women aged 22 to 50 weighted by Statistics New Zealand sample weights.



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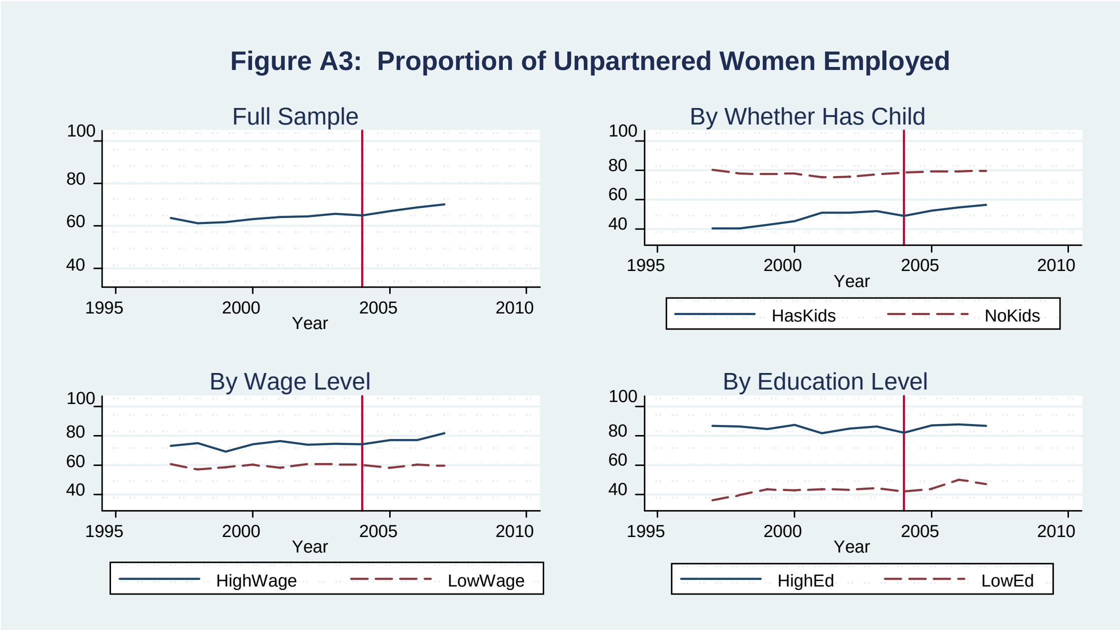

Notes: Merged HLFS-IS data 1997-2007 for women aged 22 to 50 weighted by Statistics New Zealand sample weights.