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Channels of Labour Supply Responses of Lone Parents to Changed Work Incentives

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ABSTRACT

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In this paper, we investigate the response of female lone parents to two reforms to the welfare system in Australia. We look at changes to both hours and participation and focus on the channels of adjustment, in particular the role of job changes for adjustment in hours. We highlight the relationship between policy design and heterogeneous outcomes. Workers/non-workers and mothers with high/low education respond differently to different policies. We find evidence of within job rigidities as the adjustment of working hours happens primarily through changing jobs. Our findings also provide support for the importance of accounting for fixed costs of working.

JEL Classification: C23, H31, I38, J13, J22

Keywords: channel of labour supply adjustment, lone mothers, job changes, difference-in-differences

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1 Introduction

The focus of this paper is the means by which welfare recipients, in particular single mothers with children, respond to changed work incentives. The standard labour supply model assumes that workers can choose freely their utility-maximising hours of work at any given wage. Under this model, one would only observe changes in hours of work by an employee if she received a higher wage offer or if her working hour preferences changed. In particular, neither of these should be related to job changes. New welfare rules for lone mothers change their optimal working hours. Using panel data we look at the means by which single mothers realize their new preferred working hours. We find that changes in working hours are primarily achieved through changing jobs which we take as evidence for in-work rigidities in the Australian labour market.

A second contribution of this paper is to examine how different policies affect different sub-groups of targeted potential workers. We show that non-workers and those already working respond to different incentives. This illustrates that fixed costs of working may be an important element in modeling labour supply. Workers and potential workers with different education levels appear to have different channels of response to the reforms we consider. The presence of child care subsidies as part of the reform also interacts with education levels of lone parents.

As in many countries, labour supply of women with children in Australia, particularly lone mothers, is lower than other demographic groups. Figure 1 shows, however, that since about 2005, the employment rate of lone mothers has been increasing. At the same time, two sets of reforms were introduced to encourage labour supply through reducing work disincentives associated with transfer programs and assisting families with the cost of child care. Lone mothers were particularly targeted in these reforms. The first reform, introduced in 2004, reduced the rate at which benefits were reduced as income increases (the taper rate) for family tax credits in Australia. The second set of reforms, introduced in 2006, consisted of two policy changes: the rules for qualifying for the primary income support payment for single parents were tightened by restricting the age of the youngest child in the household and a new tax rebate for child care expenses took effect. These

reforms are described more fully below.

The paper is motivated by examining the following question: Is the observed increase in lone mothers' labour supply a coincidence or can it be attributed, at least in part, to those reforms? One stated purpose of the reform was to increase work incentives for lone mothers and our paper provides estimates of the impact and effectiveness of the reform. To answer this question we look separately at the changes in working hours for those already working and the effect on participation for non-workers and for all workers.

For our study of changes in working hours, we separate hours changes for those who stay in the same job and those who change jobs. In doing so, we hope to shed some light on the presence of possible rigidities in the Australian labour market. The literature has shown that workers' choices of hours within a job are limited and wage and hours are often 'packaged' together–see for example, Ham (1982), Moffitt (1984), Lundberg (1985), Altonji and Paxson (1988, 1992), Stewart and Swaffield (1997), and Euwals (2001).[1] As discussed in Altonji and Paxson (1988), when hours are constrained within job, a worker may be able to increase her utility by jumping to another job that is closer to her supply curve, even without any wage change. Altonji and Paxson (1992) show that hours changes (and changes in preferred hours) are significantly larger for quitters than non-quitters. Blundell, Brewer and Francesconi (2008), whose approach we largely follow in conducting our analysis, studied labour supply changes for single, British females in response to three reforms in the 1990s that affected work incentives. They conclude that the reforms led to a significant increase in single mothers' hours of work and that in Britain, hours of work are not very flexible and the adjustments were largely through job changes rather than hours changes with the same employer.

We use a quasi-experimental approach and the difference-in-differences estimator to evaluate the reforms. The reforms we study did not affect single, childless women who we use as a control group. Ex ante predictions of reforms on the basis of partial equilibrium, theoretical models fail to take into account indirect effects of reform. In our case, for example, child care providers may increase prices in response to increased subsidies and this would have the effect of dampening the effects of the reform we study.

[1] Blundell and MaCurdy (1999) have reviewed this literature.

Many studies have used similar approaches to evaluate reforms in the UK, the US, Canada and Australia. Eissa and Liebman (1996), Eissa and Hoynes (2004), Ellwood (2000), and Hotz, Mullin and Scholz (2002) studied the labour market impact of the Earn Income Tax Credit (EITC) reforms during the 1980s and 1990s in the US. These studies confirmed that EITC can explain a significant part of the rise in employment of women with children in the US over those periods (see Hotz and Scholz (2003) for more discussion). In the UK, the impacts of reforms related to Family Credit and Working Families' Tax Credit were investigated by Gregg and Harkness (2003), Francesconi and Klaauw (2004, 2007), Leigh (2005), Blundell, Brewer and Shephard (2005), Brewer, Duncan, Shephard and Suárez (2006), and Blundell et al. (2008). Card and Robins (1998) examined the 'Self-Sufficiency' experiment in Canada. A consensus among these studies is that those programs led to increases in employment of women with children. Yet, Blundell et al. (2008) seems to be the only one that studies the mechanism through which these effects are achieved. In Australia, Doiron (2004) uses repeated cross-sections of data from the Income Distribution Survey to evaluate the impact of the 1987 reform of Single Parent Pension. She finds that the reform increased lone mothers' labour force participation but their hours of work decreased. However, without longitudinal data, she was unable to investigate how adjustment occurred.

As noted in Blundell et al. (2008), it is essential to have long panel data to analyse the transition of labour supply over time. For our analysis, we use the first nine waves of the Household Income and Labour Dynamics in Australia Survey, which began in 2001.

We find evidence that the reforms increased working hours of workers and subsequent employment of non-workers. The probability of continuing to work for those already working was unaffected. The adjustment in hours of work was largely through changing employers providing evidence of labour market rigidities. This is similar to what was found in the U.K. in the 1990s. The two sets of reforms brought different results and the impacts were heterogenous. The 2004 reform had positive effects on working hours of lone mothers, but only through job changes. The effects were concentrated among lone mothers with lower levels of education and with fewer and older children. We do not find employment effects of the 2004 reform. In contrast, the 2006 reform affected

the employment probability of those who were not working prior to the reform. As the reform had both a work incentive aspect and a lowering of the cost of working (through the child care tax rebate) aspect, this conforms to our expectations. The increase in participation was particularly important for lone mothers with lower education levels and with fewer and older children. The 2006 reforms increased working hours of the employed, but primarily for women with higher levels of education. The tax offset nature of the child care reforms were such that the reform was more valuable for those with higher wages and incomes.

The rest of the paper is organised as follows, Section 2 summarises the principle government benefits paid to lone parents and the reforms to those payments introduced since 2004. The approach, the identifying assumptions, and model specification are discussed in section 3. We present the data in section 4. The results are summarised in section 5 and we discuss sensitivity analysis and robustness checks in sub-section 5.4. We conclude in Section 6.

2 Government support to lone parents in Australia

2.1 Transfer payments to lone parents before 2004

Prior to the 2004-05 Financial Year[2], lone parents in Australia were entitled to the following payments: Parenting Payment Single (PPS)[3]; Family Tax Benefit A (FTB-A) and Family Tax Benefit B (FTB-B); and if they used formal child care, Child Care Benefit (CCB)[4]. Lone parent families may be eligible for other payments depending upon their specific circumstances (such as disability support), but these three means-tested payments represent the main source of income support to lone parents before 2004.

PPS is a pension paid to low-income, single parents with children under the age of 16. One important contextual aspect of the Australian income support system is that 'pensions' are more generous than 'allowances' and this difference grows over time

[2]The Australian Financial Year runs from 1 July to 30 June.

[3]Low income *couples* were eligible for Parenting Payment Partnered (PPP), another type of Income Support Payment.

[4]This is a means-tested program which reduces the hourly cost of formal child care. The scheme remained largely the same over the last decade so it is not the focus of the analysis.

because pensions are indexed to Average Weekly Earnings (AWE) whereas allowances are indexed to the Consumer Price Index which rises less quickly than AWE. In the second set of reforms which we discuss below, some lone parents who received PPS were moved from a pension to an allowance which was both less generous at that point in time and which was going to grow more slowly over time. In 2003, PPS was paid at a maximum rate of $440.30 per fortnight (or $11,447.8 per annum) while the maximum amount of any allowance was $342.8 per fortnight (or $8,912.8 per annum).[5]

FTB-A and FTB-B are family assistance payments, excluded from taxable income, paid to families with children under 16 or full-time students under 19. FTB-B does not depend upon the number of children in the household nor upon employment status and is not means-tested for lone parents.[6] FTB-A, on the other hand, depends upon the age and number of children, and is means-tested. As illustrated by the broken line in Figure 2, in 2003, a lone parent with one child under 13 could get $4,001.8 of FTB-A per annum if her annual private income was below $31,755. After that, for each extra dollar she earns, her FTB-A entitlement was reduced by 30 cents (the 'taper rate') until reaching $1,695 per annum. Her FTB-A entitlement would stay at that level until her income reached $39,464 before it is again reduced by 30 cents for every additional dollar earned until reaching an entitlement of zero dollars at an income level of $82,052.[7]

2.2 Three reforms since 2004

There was one substantial reform introduced in the 2004-05 Financial Year and two reforms in the 2006-2007 financial year. Our analysis has two parts: the first reform and the combination of the second and third contemporaneous reforms.

The 2004-2005 reform was part of a legislative package entitled 'More help for families'. The Australian Government lowered the 'taper rates' of the means-tests for FTB-A

[5] All dollar figures in the paper are in Australian dollars. On June 30, 2004, the Australian dollar was equal to 0.57 Euro or 0.69 U.S. dollar. In June, 2013, the Australian dollar was equal to 0.71 Euro or 0.95 U.S. dollar.

[6] In 2003, a lone parent received $2,920 per annum if her youngest child was under age 5 and $2,037 otherwise.

[7] Lone parents who qualify for FTB-A also qualify for Rent Assistance if they are renting. Taper rates for rent assistance are the same as those for FTB-A because Rent Assistance is treated like a top-up of the FTB-A payment.

and FTB-B from 30 percent to 20 percent.[8] The reform is captured by the movement from the dotted line to the solid line in Figure 2. This change is equivalent to boosting the wage of a working lone mother by 10 percent if her annual income is in the tapering region. If the elasticity of labour supply with respect to own wage is positive, this reform should lead to an increase in hours worked of lone mothers. The effect of this reform is even larger for those receiving rent assistance because of its treatment as a top-up to FTB-A (see footnote 7).

The 2006-2007 reforms consisted of two policy changes. First, PPS eligibility rules were tightened.[9] Under the new rules, new single parent claimants were only eligible for PPS if their youngest child was under 8 years of age. Previously, single parents were eligible if their youngest child was under age 16. New income support claimants with children aged 8 or older no longer qualified for PPS (a pension) but instead could recieve New Start Allowance–a less generous unemployment benefit which also includes a more onerous training and job search requirement. Not all single parents were affected as those on the PPS program prior to the legislative changes were treated under the old rules provided that their relationship status remained unchanged and that they never had any payments cancelled. The PPS payments continued to these individuals as before, however, these individuals also faced a more onerous training and job search requirement once their youngest child turned 8. Importantly, single parents who were already working were unaffected by these changes.

The second reform in 2006-2007 was the introduction of the Child Care Tax Rebate (CCTR). Families of all types were able to claim 30 per cent of out-of-pocket costs (in excess of CCB payments) for approved child care up to a maximum of $4,000 per child per annum. Households were able to claim CCTR for two years prior to the reform back to the 2004-2005 Financial Year when they filed their tax return for the 2005-06 Financial Year (after 1 July 2006). Most households file their tax return between July and October, thus the first payment only reached families in late 2006. Initially, CCTR was introduced as a tax offset so only families with a tax liability could benefit. After

[8] Lone mothers, not subject to means testing for FTB-B, were not affected by this latter change.
[9] This was part of the legislative package 'Welfare to Work'.

the 2006-2007 Financial Year, it was changed into a transfer payment which households could access even in the absence of a tax liability. The labour market effects of this reform are ambiguous. Lowering child care costs lowers the costs of working so this may encourage people to work more. However, there is also an indirect income effect as the effective decrease in child care costs might result in a lowering of labour supply.

3 Approach

3.1 Identification

We use single, childless women as a comparison group for single mothers and the 'difference-in-differences' approach to identify the effects of the policy.[10] The first key identifying assumption of this approach is that single childless women are not affected by the reforms. Given that we are analyzing administrative rule changes which did not apply in any way to single, childless women, this assumption would appear to be met. The second key identifying assumption required is that no other factors affected the two groups differently over the same period. The period that we analyze was one of robust economic and job growth in Australia, the benefits of which seemed to be spread across most demographic groups. We can not find any reason why employment and hours changes, the variables we analyze, would have been affected differentially for these two groups apart from the reform. Many other studies use childless women as a control group for lone mothers (see Eissa and Liebman (1996), Gregg and Harkness (2003), Francesconi and Klaauw (2007) and Blundell et al. (2008)).[11]

We present regression estimates in what follows. We check the validity of our regression estimates and the validity of the comparison group in a number of ways. We compare the characteristics of our treatment and control group; we use nonparametric matching; and we restrict the sample in various ways all of which are described below in section 5. Our results are robust to these alternative approaches. In order to avoid the

[10]For discussions of the approach, see Ashenfelter (1978), Heckman and Robb (1985), Blundell and MaCurdy (1999), Meyer (1995) and Angrist and Kruger (1999).

[11]Doiron (2004) uses married mothers as a control group. This would be inappropriate in our case as married mothers were affected by the reforms we analyze such as changes in FTB-B. The legislative reform packages we analyze also had other changes which applied to couple-headed households.

confounding effect of changes in labour force status which are caused by the birth of a child or changes in relationship status, we restrict our estimation sample to those individuals who are lone mothers in both waves and those who are single, childless women in both waves. We thus do not analyze any impact which the reforms might have on fertility or relationship status which are likely to be very small.

One issue for comparability of our treatment and control groups is that the changing ages of children in the lone mother households across time will have labour supply effects which the single, childless women will not experience. In order to deal with this, we control for the changes in the number of children in different age ranges in the household. The age ranges are chosen to reflect schooling availability and differing care demands for children of different ages. We also, in the sensitivity tests presented below, restrict the sample to lone mothers whose children remain in the same age group before and after the policy change. Our results do not appear to be sensitive to this issue.

We specify three different models to analyse the effect of the reforms: (1) change in hours worked conditional on working before and after the reforms; (2) the probability of being employed conditional on not-working before the reform; (3) unconditional probability of employment.

3.2 Changes in working hours for workers

To investigate the possible channels through which hours adjustment occurs for lone mothers in response to the exogenous policy change, we specify an hours change model following Blundell et al. (2008), who also examine annual changes, as:

$$\begin{aligned}\Delta h_{it+1} &= \alpha_0 + \alpha_1 LP_{it} + \alpha_2 JC_{i,t+1} + \alpha_{21} JC_{i,t+1} I(2004 \leq t < 2006) \\ &\quad + \alpha_{22} JC_{i,t+1} I(2006 \leq t < 2009) + (\alpha_{31} + b_1 LP_{it}) I(2004 \leq t < 2006) \\ &\quad + (\alpha_{32} + b_2 LP_{it}) I(2006 \leq t < 2009) + \beta_1 LP_{it} JC_{i,t+1} I(2004 \leq t < 2006) \\ &\quad + \beta_2 LP_{it} JC_{i,t+1} I(2006 \leq t < 2009) + X'_{it}\gamma + \epsilon_{it}, \qquad (1)\end{aligned}$$

where LP_t indicates that the observation is a lone parent at time t, $I(w)$ is an indicator equal to one if condition w is true and JC_{t+1} is an indicator for a job change between t and $t+1$. Δh_{it+1} denotes the change in total weekly hours worked between year t

and $t+1$; X_{it} is a vector of observables including levels measured at t and the changes between t and $t+1$; and ϵ_{it} captures unobserved impacts on hours changes. $I(2004 \leq t < 2006)$ and $I(2006 \leq t < 2009)$ indicate the periods after the 2004 and 2006 reforms, respectively. Included in X_{it} are log wage, a quadratic polynomial in age, indicators for a stated desire to work more (under-employment) or to work less (over-employment), industry dummies, number and their changes between t and $t+1$ of children in four age groups (0 to 5; 6 to 12; 13 to 15; and 16 to 17), type of work contract (casual or fixed term) and dummies for state/territory and capital city. The variables 'underemployed' and 'over-employed' are included as indications of the deviation from the individual's preferred supply curve. Empirically, they also appear to have strong predictive power for hours changes. The model is estimated by OLS over nine years of data for all observations where an individual works in two consecutive years.[12]

The equation is specified to investigate the role of changing jobs on changing hours and its interaction with the impact of the reforms. There are two sets of parameters which capture the effects of the reform. b_1 and b_2 capture the effects of the two reforms for workers who stayed in the same job. β_1 and β_2, meanwhile, capture the additional effects of the two reforms on workers who changed jobs. If there were no within job hours restrictions, one would expect $\beta_1 = \beta_2 = 0$. If this were true, it would indicate that lone parents could change their hours in response to the reforms by staying in the same job or by changing jobs in an equal manner. On the other hand, if $\beta_1 > 0$ (and similarly for the 2006 reforms), it would indicate a within-job hours restriction.

These four parameters are the difference-in-difference estimators for the two reforms estimated separately for the group who change jobs and for those who stay in the same job. As noted by Blundell et al. (2008), equation (1) may suffer from an endogeneity problem if some omitted factor influences both the job change and the hours change. However, as they state, it helps to provide an 'indication of the possible presence of imperfections or technological rigidities' in the labour market. By controlling for an individual's expressed desire to work more or less we reduce this source of endogeneity.

[12] We also considered models where we included an interaction term for lone parents and job changes, but this was always insignificant. As there is no reason to believe that the probability of changing jobs differs for lone parents in the absence of the reform, we exclude this additional term.

Equation (1) is a flexible specification with group-specific discrete jumps after the reforms for job stayers (α_{31}, α_{32}, b_1 and b_2) and for job changers (α_{21}, α_{22}, β_1 and β_2).

3.3 Employment probability for non-workers

The model for employment probability in the subsequent year of those who did not work at time t ($L_{it} = 0$) is specified as

$$\begin{aligned} Prob\{L_{i,t+1} = 1 | L_{it} = 0\} &= F\{\eta_0 + \eta_1 LP_{it} + \eta_2 t + \eta_{31} I(2004 \leq t < 2006) \\ &\quad + \phi_1 LP_{it} I(2004 \leq t < 2006) + \eta_{32} I(2006 \leq t < 2009) \\ &\quad + \phi_2 LP_{it} I(2006 \leq t < 2009) + X'_{it}\mu\}. \end{aligned} \tag{2}$$

We estimate F as a linear probability model (we use a probit specification in sensitivity tests); η_2 reflects a linear time trend common to both lone mothers;[13] and η_{31} and η_{32} capture the shift in the employment probabilities after the reforms. X_{it} includes a quadratic polynomial in age, number and changes in the number between t and $t+1$ of children in four age groups (0 to 5; 6 to 12; 13 to 15; and 16 to 17), English-language ability and dummy variables for educational attainment and housing tenure (renter/owner).

The key policy parameters are ϕ_1 and ϕ_2 which capture the treatment effects of the 2004 and 2006 reforms, respectively.

3.4 Unconditional employment probability

Similar to equation (2), the probability of employment of all lone mothers and single childless women is given by

$$\begin{aligned} Prob\{L_{it} = 1\} &= G\{\eta_0 + \eta_1 LP_{it} + \eta_2 t + \eta_{31} I(2004 \leq t < 2006) \\ &\quad + \phi_1 LP_{it} I(2004 \leq t < 2006) + \eta_{32} I(2006 \leq t < 2009) \\ &\quad + \phi_2 LP_{it} I(2006 \leq t < 2009) + X'_{it}\mu\}, \end{aligned} \tag{3}$$

[13] As Francesconi and Klaauw (2004) point out, a more general specification would allow a different time trend for each group. However, given the limited number of waves, it would cause a collinearity problem and make the treatment effects impossible to identify. In our case, figure the time trend is not statistically significant and omitting it has no effect on our results.

where G is a linear probability function. The difference from equation (2) is that the dependent variable is contemporaneous with the right-hand side control variables. X_{it} contains the same set of control variables as equation (2) with the exception of the changes in the number of children in different age groups.

4 Data

Data for the analysis are drawn from the first nine waves of the Household Income and Labour Dynamics in Australia Survey (HILDA) which cover the period 2001 - 2009. The HILDA Survey is an annual panel survey of Australian households which was begun in 2001.[14] There are approximately 7,000 households and 13,000 individuals who respond in each wave.

We focus on the labour supply of 2,676 lone mothers and single, childless women of working age (between 15 and 64) excluding those who are students, permanently unable to work or self-employed. This number also excludes a handful of observations with missing data on key variables. We are left with 9,239 observations which we use to analyse the unconditional probability of employment (equation (3)). For the analysis of hours changes for workers (equation (1)), we restrict the sample to those whose status as lone parents or single, childless women is unchanged and who are are working for two consecutive waves. This provides 3,565 observations on 1,214 women. The sample used to estimate the employment probability conditional on not working in the previous year (equation (2)) consists of 2,148 observations on 760 women who remain in the same lone parents/single women status in two consecutive waves and are (not) working in the first of those two waves.

Sample statistics are presented in Table 1. From the table, it can be seen that the characteristics of lone mothers are different from those of single childless women. However, if we consider the subset of workers, they are more similar. Overall, as expected, lone mothers are less educated, younger and more likely to be renters. They are also less

[14] See Watson and Wooden (2002) for more details.

likely to participate in the labour force and, when they do work, work fewer hours. The biggest difference is between the non-working lone mothers and their single, childless counterparts. The latter group is much older (with average age of 53 years). This may invalidate one of our identification requirements. We check this by restricting the age of the comparison group to 50 or less in one of the sensitivity tests which we conduct and describe below in section 5.4. The lone parents and single, childless women who work are more comparable in their characteristics, and they are better educated than the non-workers. However, their labour supply differs. The lone mothers are more often casual workers, work fewer hours and are less likely to report being under- or over-employed.

Figures 3 through 7 compare the patterns of labour supply between lone mothers and single, childless women. From these pictures, we can see that labour supply differs by group. In Figures 3 and 4, we plot average hours of work and the change of hours for workers, respectively. Consistent with Figure 1, Figure 3 shows that lone mothers' hours of work increased in the second half of the last decade but those of single, childless women remained stable. Figure 4 shows that the change of hours for lone mothers are positive except in 2002 and are more volatile than the changes in hours of single, childless women. Figure 5 shows employment rates conditional on not working in the previous period. Future employment rates for non-working lone mothers are a bit higher since 2005 and the pattern is different from that of single, childless women. From Figure 6, however, we can see that the difference in the patterns of remaining employed for workers is less pronounced. Figure 7 confirms the overall increase in lone mothers' employment across our sample period.

5 Results

We estimate each model for the full sample and also for various sub-samples partitioned by mother's education, number of children and age of youngest child, to analyse potential heterogeneity of policy effects. For the sake of conciseness, we only report the main parameter estimates. Full regression results are available on request.

5.1 Channel of hours adjustment for workers

In Table 2, we present the parameter estimates of the treatment effects of the reforms on lone mothers' working hours. In the first column we present results from estimation on the full sample with the remaining columns providing estimates on selected sub-samples of interest.

For those who stay in their jobs we find no effect of the reforms on working hours– b_1 and b_2 are both insignificant. For those that change jobs, we find a positive and statistically significant effect of the 2004 reform. Working mothers who changed jobs increased their hours of work by 5.3 hours per week $(b_1 + \beta_1)$ in response to the reforms. Looking further across the table, we can see that the effect of the 2004 reform is not constant across all sub-populations. The effect appears stronger for women with fewer and older children. These women likely face lower costs to work additional hours.

For the 2006 reforms, we do not find a statistically significant effect of the reforms when we consider the entire sample. However, we do find a statistically significant effect on working hours for women with tertiary education of about 7.6 hours per week. This effect operates through the channel of changing jobs. This seems consistent with the CCTR reforms of 2006. Even before the reforms, women with tertiary education earn more and use more child care, CCTR is not means-tested and CCTR is only valuable when there is a tax liability to be offset. Thus the value of CCTR is higher for these women.[15] The changes to PPS eligibility were not expected to influence working hours for workers as women who were already working were not impacted by these reforms. So in the case of changes in working hours for those already working our evaluation of the 2006 reforms can be considered an evaluation of the introduction of CCTR.

We can also see from Table 2 that a self-reported desire to work more or less hours is highly predictive of future hour changes. Those who report wanting to work more increase their work hours by 3.5 hours per week on average relative to those who are satisfied with their hours whereas those who report wanting to work less decrease their work hours by 2.5 hours per week on average relative to those who are satisfied with

[15] This result is consistent with Gong and Breunig (2012) who simulated the ex ante effects of CCTR with a structural child care and labour supply model.

their hours.

Overall, the results provide evidence that there are important within-job hour restrictions in the Australian labour market. Changing jobs appears to be an important channel for all workers to respond to the 2004 policy reforms. It is also the primary channel by which higher educated workers respond to the 2006 reforms.

5.2 Subsequent Employment of Nonworkers

Table 3 summarises the key parameter estimates from the nonworkers' subsequent employment probability equation. The statistical significance of ϕ_2 and the insignificant ϕ_1 coefficient indicate that the 2006 reforms had a positive effect on the future employment probability of non-workers but that the 2004 reforms had no effect. This is consistent with our expectations. The 2006 reforms lowered the cost of working through the introduction of CCTR and also tightened rules and activity tests for receipt of PPS which had the effect of pushing people into a choice between a lower payment (New Start Allowance) or employment. The combination of these two should have a clear employment incentive which is particularly concentrated among lone mothers who are less educated and have older children as can be seen from the other columns of Table 3. The 2004 reform did not have a particularly strong employment incentive for those not already employed and our insignificant parameter estimate can be interpreted as an indication that the modest improvements to work incentives were outweighted to a great degree by fixed costs of working for non-workers.

We also estimated equation (2) for workers (that is, conditional on working at time t) but omit the results for conciseness. We find no effects of the reforms (ϕ_1 and ϕ_2 are both statistically insignificant) which is consistent with our expectation that neither reform should lead to a shift in employment probability for those already employed.

5.3 Employment of All Lone Mothers

In addition to analyzing employment effects conditional on previous employment status, we estimated the unconditional employment probability for all lone mothers and single childless women (equation (3)). The key parameters are presented in Table 4. The

estimates of ϕ_1 and ϕ_2 confirm the findings in section 5.2: while the 2004 reform did not have any employment effects, the 2006 reforms brought about an increase in the employment of lone mothers with lower education and fewer and older children.

5.4 Sensitivity Tests

To check the specification, the functional form, the validity of the common support assumption and the potential impact of other factors, we conducted a range of sensitivity tests for each of the estimated equations. First of all, for the two conditional equations, although we controlled both the level and the change in the number of children in each age group, aging of the children could still confound the estimated treatment effect as discussed above. To further reduce the impact of children's aging (although we can never completely remove it), we further restrict the sample to observations where the youngest child remains in the same age group at t and $t+1$. Secondly, to check whether there is a problem caused by non-random attrition, we restrict the sample to individuals who were observed in at least 6 waves. Thirdly, we estimate the model excluding the ninth wave. Because CCTR changed (it was increased from 30 to 50 percent as of July 2008) and the on-set of the Global Financial Crisis may have affected our two groups differently (although we think this is unlikely), the ninth wave may be quite different from other waves.

In addition, including many covariates in the models may make it harder to find over-lapping groups with the same characteristics in both the treatment and control groups. In the treatment literature this is called the common support problem. To see whether this affects our results, we combine the difference-in-difference estimator with propensity score matching. We re-estimated equations (2) and (3) with local linear regression matching (see for example, Heckman, Ichimura and Todd (1997) and Fan (1992)). For equation (1), we used linear regression matching as the number of observations in each cell is too small to undertake a non-parametric approach. Equation (2) was also estimated with the comparison group restricted to be 50 years of age or younger as discussed above. All equations are also estimated without controls, and where possible, using a probit functional form. Lastly, a natural way to check the condition that the

untreated response changes are the same across the treatment and control groups is to backtrack one period and examine the response changes in two pre-treatment periods. If the condition does not hold in the pre-treatment periods, then a pre-treatment gap may exist.

The key results of these sensitivity tests are summarised in Tables A1, A2 and A3, respectively. By and large, these results show that the estimates of the benchmark model (the first column of each table) are robust. In particular, the last column in each table shows that pre-treatment gaps do not exist so the assumption that the untreated response changes are the same across the treatment and control groups appears to hold.

6 Conclusions

The classical labour supply model predicts that changed welfare rules will alter women's optimal labour supply. Preferred hours will change for those who are working and the decision to participate for workers and non-workers will also be affected. That model assumes that workers can adjust hours of work at will within their present employment relationships.

Our paper illustrates the relationship between different policy designs and heterogenous labour supply outcomes for lone mothers. We find that two reforms which changed the work incentives for lone mothers in Australia increased working hours of those who were working and employment of non-workers, but that they had no effect on the continued probability of remaining in employment for workers. The adjustment in working hours was largely through changing employers in an environment where working hours are often constrained within jobs. The 2004 reform had positive effects on hours of work by working lone mothers, but only through job changes. The effects were concentrated among lone mothers with lower education and with fewer and older children. The 2006 reforms contributed to an increased probability of employment in subsequent periods for those who were not working pre-reform. Again, effects were concentrated among lone mothers with lower education and with fewer and older children. The 2006 reforms also increased hours of work for higher educated lone mothers who were already employed. Again, working hours changes occurred through the channel of changing employers.

These results highlight some caveats to the standard model. First, in-work rigidities appear to exist. The ability of policy changes to induce working hour changes therefore may be enhanced or diminished by the degree of dynamism in the labour market. Second, some reforms seem to have no effect on participation. This is consistent with important fixed costs of working which should be accounted for when modeling labour supply. Third, tightened welfare rules appear to have larger effects on those with lower education whereas increased child care tax rebates have a larger impact on those with higher education. This is consistent with the higher incomes of those with more education and the nature of the child care subsidy which is delivered through a tax rebate.

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Figures

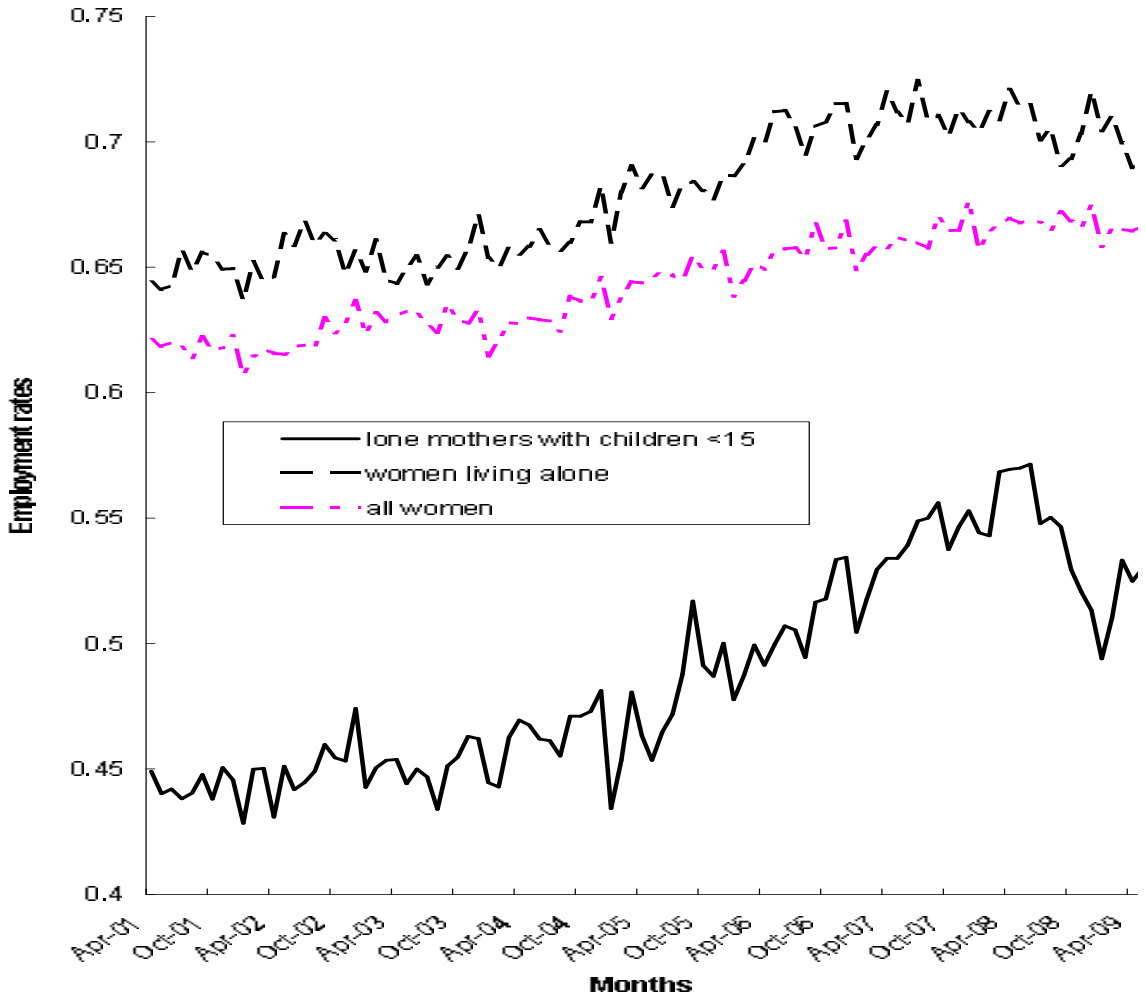

Figure 1. Employment rates of lone mothers vs. other groups of women (source: 6291.0.55.001 - Labour Force, Australia, Detailed - Electronic Delivery, Aug 2011)

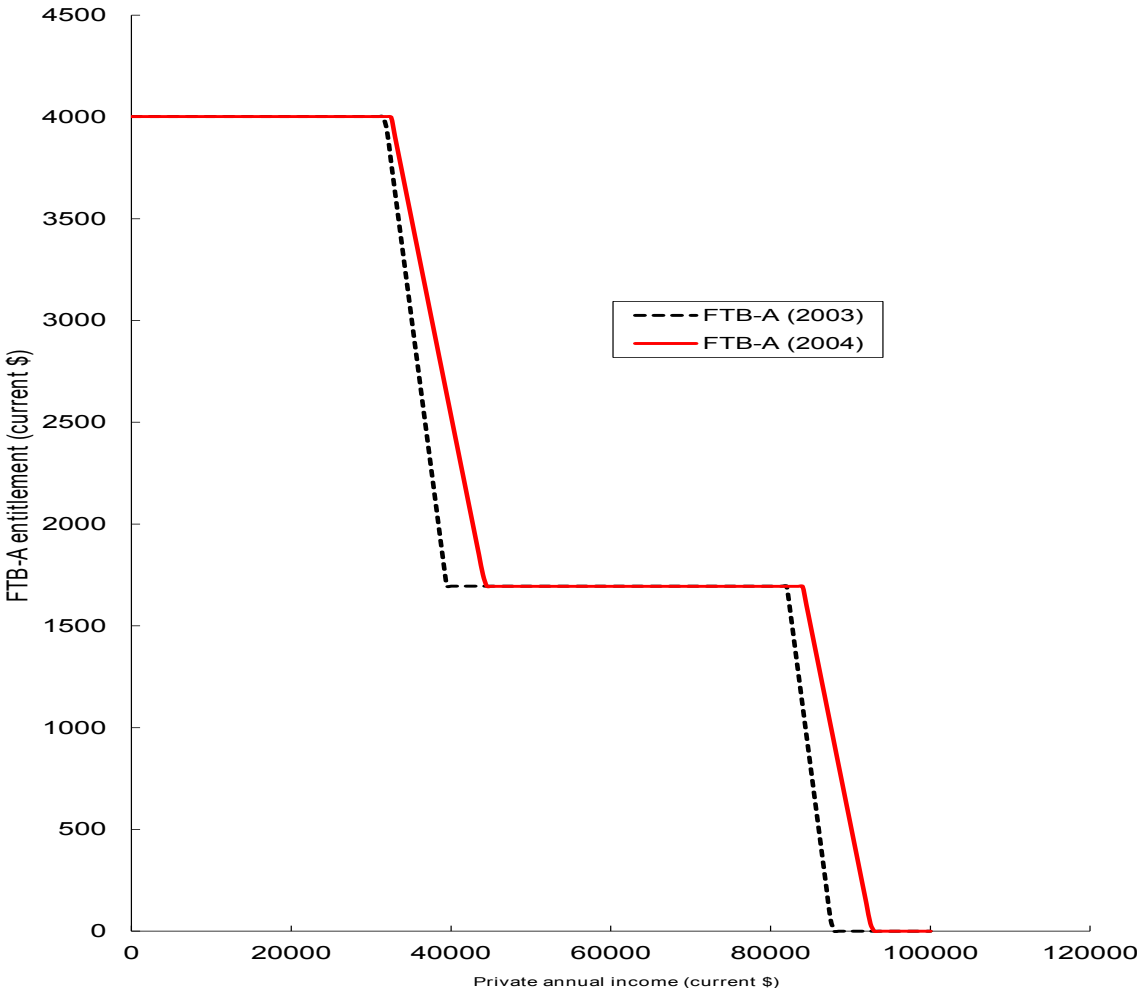

Figure 2. FTB-A Entitlement and annual income for a lone parent with one child under age 13

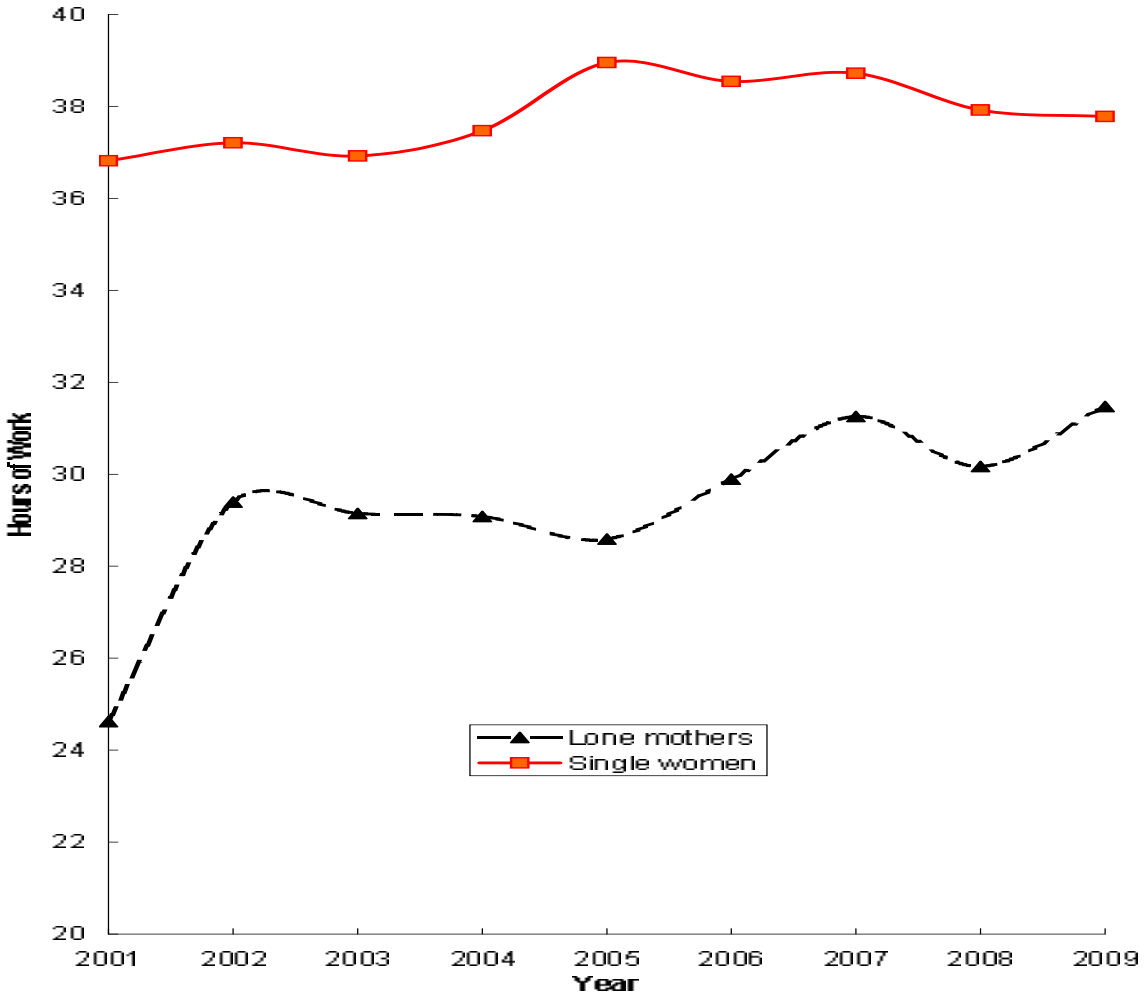

Figure 3. Hours of work (workers at both t and $t+1$)

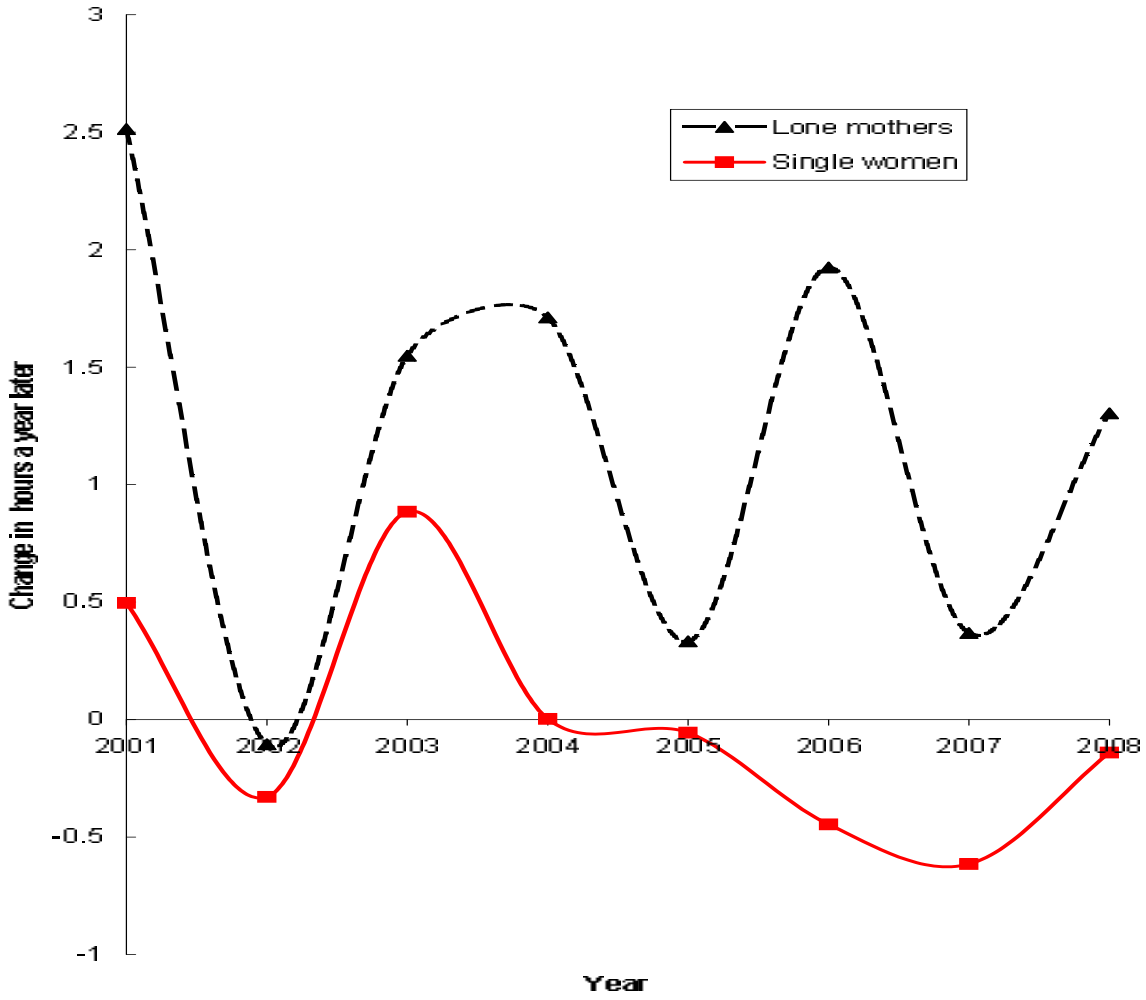

Figure 4. Changes in hours (workers at both t and $t+1$)

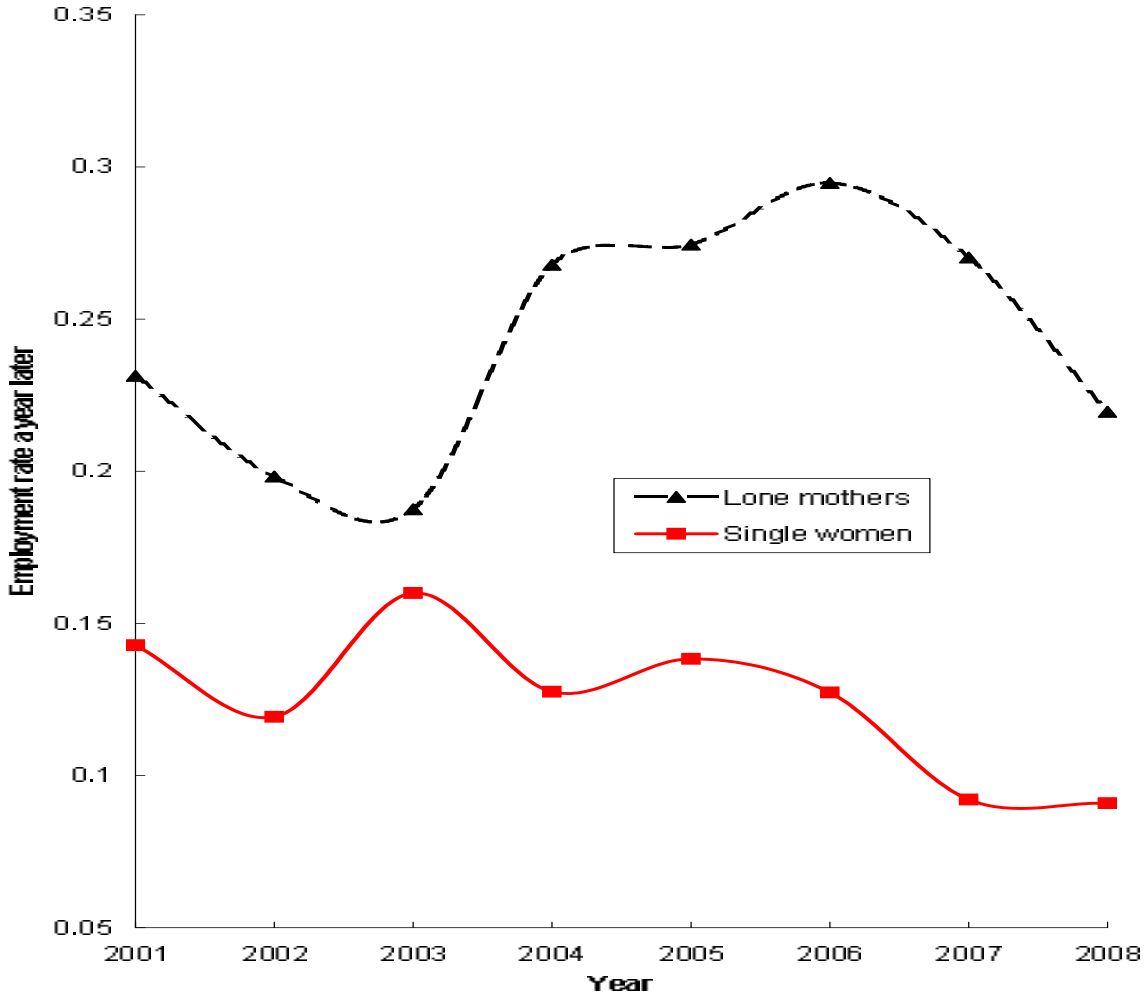

Figure 5. Employment rate at $t+1$ of nonworkers at t

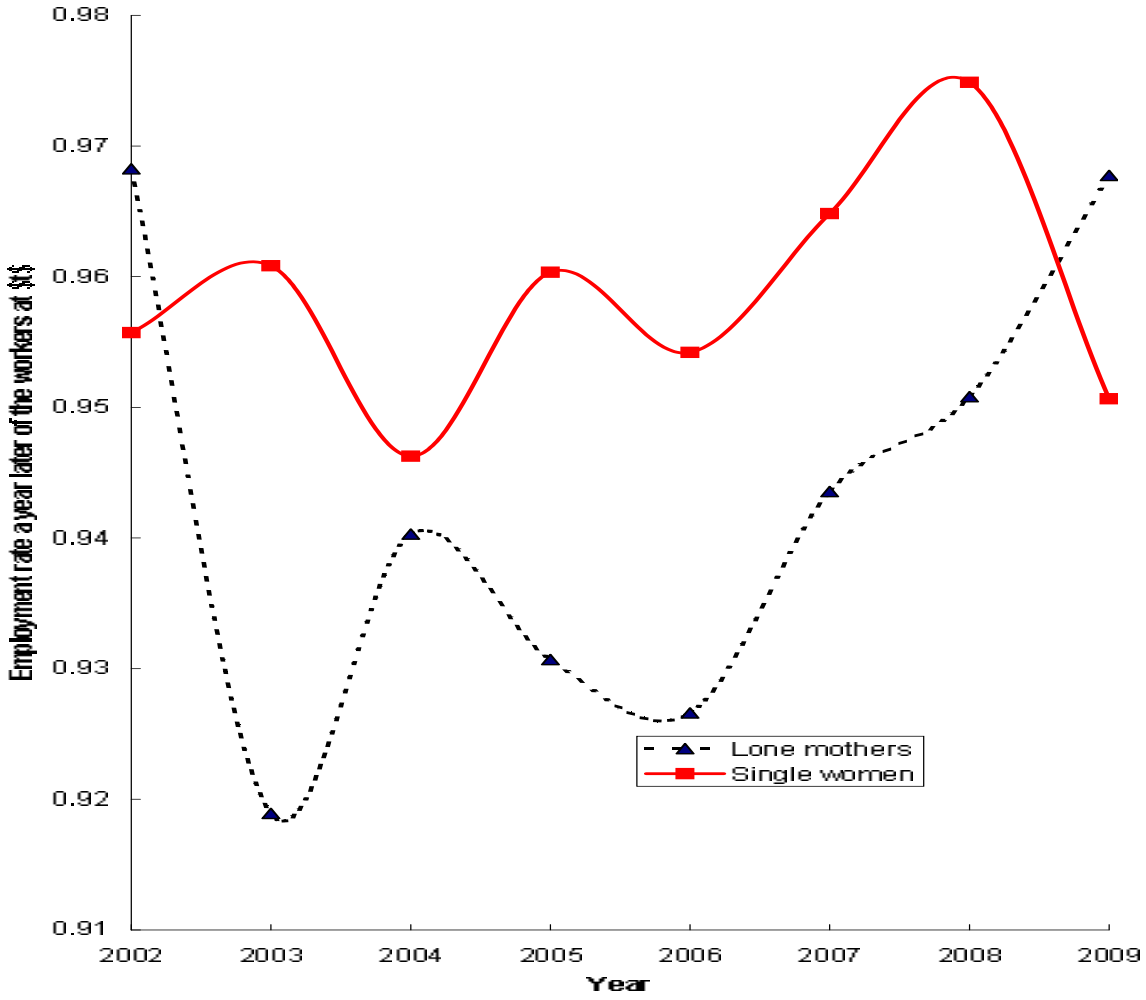

Figure 6. Employment rate at $t+1$ of workers at t

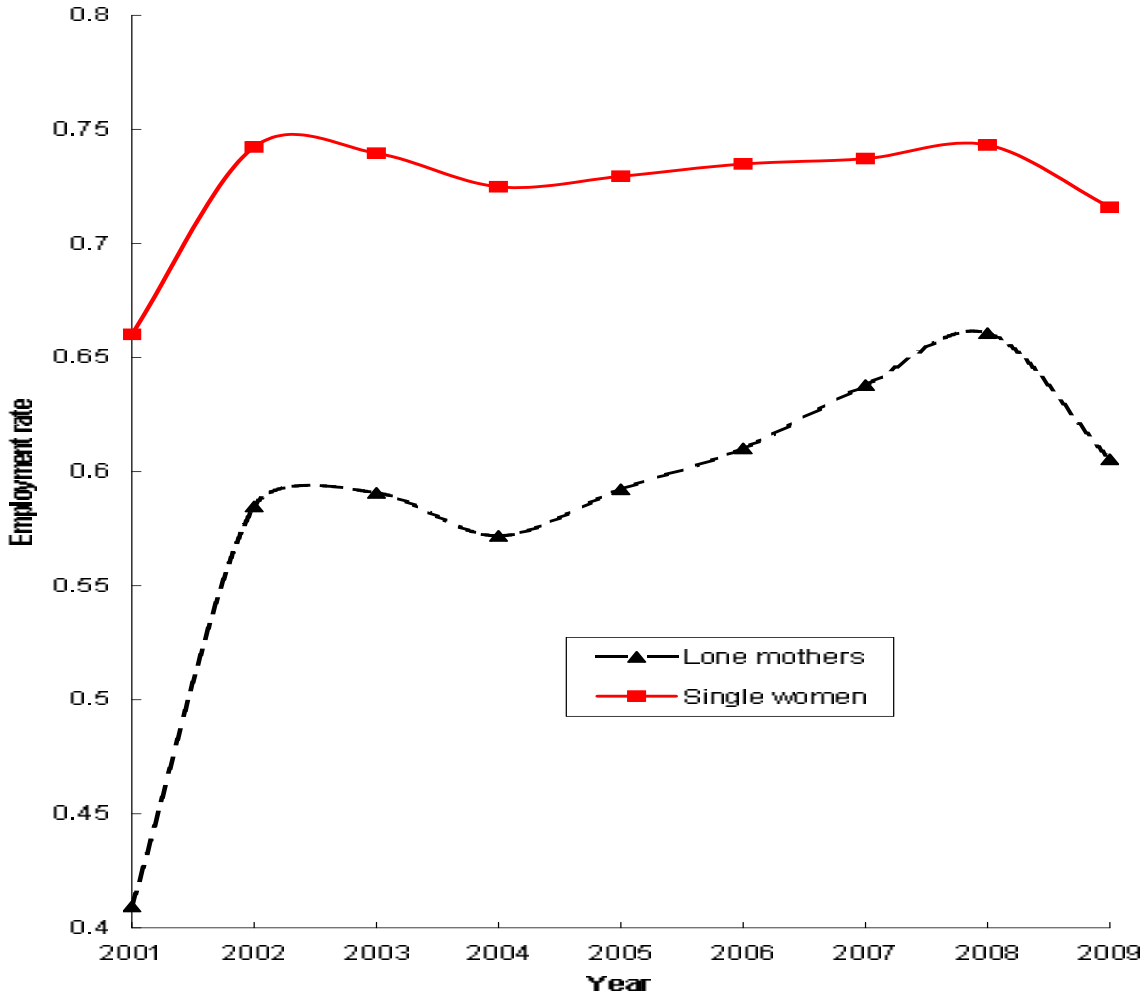

Figure 7. Employment rate of lone mothers and single women in our data

Tables

Table 1. Sample statistics

	Workers in t and $t+1$		Nonworkers in t		All	
Variables	Lone mothers	Single women	Lone mothers	Single women	Lone mothers	Single women
Age	38.7(7.5)	41.2(13.4)	35.7(8.9)	52.3(12.9)	36.7(8.8)	42.6(15.1)
Eng. 2nd tongue	.072	.069	.117	.116	.095	.084
Tertiary edu.	.263	.336	.058	.123	.146	.254
Vocational edu.	.326	.267	.291	.179	.302	.238
Year 12	.146	.170	.114	.100	.138	.161
<Year 12	.265	.227	.537	.598	.414	.347
Child. 0 and 5	.348(.55)		.749(.80)		.571(.74)	
Child. 6 and 12	.928(.76)		.968(.96)		.887(.87)	
Child. 13 and 15	.350(.55)		.378(.62)		.373(.588)	
Child. 16 and 17	.124(.35)		.110(.34)		.113(.33)	
Home owner	.021	.030	.021	.043	.025	.036
Home renter	.456	.452	.746	.506	.624	.508
Mortgage payer	.523	.518	.233	.451	.351	.455
Employed			.240	.126	.592	.729(.44)
Hours of work	29.4(13.1)	37.9(11.9)			29.2(13.8)	36.9(12.5)
ΔHour	1.077(7.79)	.055(8.29)				
ΔJob	.130	.164			.147	.188
Underemployed	.065	.075			.075	.090
Over-employed	.204	.292			.199	.275
Wage ($2001)	18.550(9.05)	18.788(7.88)			18.207(9.12)	18.345(7.67)
Permanent job	.623	.717			.587	.675
Fixed-term job	.083	.100			.085	.103
Casual job	.294	.183			.328	.222
Obs.	820	2,745	843	1,305	2,726	6,513

Standard deviations are in the parentheses; Job related characteristics are for workers only. Level variables are for t; changes are from t to $t+1$; and the rate of employment of the nonworkers are for $t+1$. 'Workers in t and $t+1$'/'Nonworkers' are conditional on being lone mothers/single women in both t and $t+1$.

Table 2. The Effects of the Reforms, Job Changes and Hours Constraints on Hours Changes

	All	By Education		By No. of Children		By youngest child	
		Tertiary	No Tertiary	One child	More	age < 6	age ≥ 6
α_1	.584	-1.243	.958	.309	.513	2.455	.463
	(0.77)	(-0.81)	(1.11)	(0.19)	(0.30)	(1.07)	(0.52)
α_2	.654	.538	.880	.738	.727	.955	.543
	(0.72)	(0.37)	(0.77)	(0.80)	(0.73)	(0.98)	(0.56)
Under	3.435**	3.976**	3.308**	3.533**	3.500**	3.643**	3.491**
	(5.89)	(2.58)	(5.12)	(5.68)	(5.60)	(5.52)	(5.85)
Over	-2.491**	-2.918**	-2.204**	-2.397**	-2.379**	-2.331**	-2.416**
	(-8.37)	(-5.32)	(-6.01)	(-7.68)	(-7.38)	(-7.03)	(-7.76)
Effect of 2004 (b_1) and 2006 reforms (b_2) for workers who stayed in same job:							
b_1	-.395	-.043	-.361	-.505	.274	-1.240	.080
	(-0.59)	(-0.03)	(-0.44)	(-0.58)	(0.29)	(-0.89)	(0.10)
b_2	.591	-.003	.969	1.022	.158	.779	.418
	(0.96)	(-0.00)	(1.32)	(1.25)	(0.18)	(0.60)	(0.60)
Effect of 2004 (β_1) and 2006 reforms (β_2) for workers who changed jobs:							
β_1	5.725**	5.397	5.813*	7.844**	3.311	-3.834	7.730**
	(2.18)	(1.21)	(1.82)	(2.11)	(1.08)	(-0.95)	(2.41)
β_2	-.830	6.405**	-3.261	-2.245	.833	-6.170	1.939
	(-0.38)	(2.37)	(-1.23)	(-0.80)	(0.27)	(-1.63)	(0.77)
R^2	0.088	0.125	0.092	0.090	0.093	0.094	0.090
Obs.	3,565	1,137	2,428	3,185	3,120	2,935	3,311

t-values calculated using robust standard errors clustered
at the level of the individual are in parentheses.
* Significant at 10% level. ** Significant at 5% level.

Table 3. The Effects of the Reforms on Participation of nonworkers

	All	By Education		By No. of Children		By youngest child	
		Tertiary	No Tertiary	One child	More	age < 6	age ≥ 6
η_1	.042	.530**	.025	.024	.023	.045	.045
	(1.03)	(2.24)	(0.59)	(0.32)	(0.42)	(0.85)	(0.68)
η_2	-.011	-.005	-.010	-.009	-.008	-.005	-.010
	(-1.10)	(-0.14)	(-0.96)	(-0.83)	(-0.81)	(-0.46)	(-0.97)
Effect of 2004 (ϕ_1) and 2006 reforms (ϕ_2)							
ϕ_1	.038	.010	.040	.061	.021	.009	.047
	(0.89)	(0.05)	(0.96)	(0.94)	(0.42)	(0.18)	(0.78)
ϕ_2	.077*	-.117	.092**	.109	.060	.048	.130**
	(1.89)	(-0.46)	(2.19)	(1.62)	(1.30)	(1.04)	(2.05)
R^2	0.151	0.319	0.143	0.190	0.161	0.165	0.187
Obs.	2,148	209	1,939	1,606	1,847	1,819	1,675

t-values calculated using robust standard errors are in the parentheses.
* Significant at 10% level. ** Significant at 5% level.

Table 4. The Effects of the Reforms on Participation of All Lone Mothers

	All	By Education		By No. of Children		By youngest child	
		Tertiary	No Tertiary	One child	More	age < 6	age ≥ 6
η_1	-.048	.131**	-.093**	-.041	-.098**	-.120**	-.028
	(-1.56)	(2.01)	(-2.67)	(-1.01)	(-1.99)	(-2.60)	(-0.61)
η_2	-.001	-.004	-.000	-.004	-.003	-.007	-.001
	(-0.35)	(-0.59)	(-0.06)	(-0.98)	(-0.62)	(-1.53)	(-0.15)
Effect of 2004 (ϕ_1) and 2006 reforms (ϕ_2)							
ϕ_1	.007	.006	.015	-.030	.038	-.030	.036
	(0.28)	(0.11)	(0.53)	(-0.86)	(1.14)	(-0.73)	(1.15)
ϕ_2	.047*	.042	.049	.032	.060*	-.014	.098**
	(1.71)	(0.81)	(1.56)	(0.86)	(1.72)	(-0.34)	(2.92)
R^2	0.256	0.153	0.234	0.246	0.275	0.281	0.2401
Obs.	9,239	2,053	7,186	7,758	7,994	7,714	8,038

t-values calculated using robust standard errors are in the parentheses.
* Significant at 10% level. ** Significant at 5% level.

Table A1. The Effects of the Reforms, Job Changes and Hours Constraints on Hours Changes—Alternative specifications

	Default	Child stays in age group	6 waves or more	No controls	Waves 1 to 8	Matching (linear)	Backtrack one period
α_1	.584	1.200	.652	.555	.225	.519	.518
	(0.77)	(1.48)	(0.76)	(1.20)	(0.30)	(0.65)	(.63)
α_2	.654	.735	.664	.961	.645	.949	-.127
	(0.72)	(0.79)	(0.65)	(1.05)	(0.71)	(1.09)	(.11)
Under	3.435**	3.516**	3.320**	3.485**	3.586	3.491**	3.468**
	(5.89)	(5.99)	(4.99)	(5.97)	(5.81)	(5.94)	(5.93)
Over	-2.491**	-2.527**	-2.597**	-2.692**	-2.606	-2.569**	-2.472
	(-8.37)	(-8.39)	(-8.26)	(-9.07)	(8.10)	(-8.73)	(8.26)
Effect of 2004 (b_1) and 2006 reforms (b_2) for workers who stayed in same job:							
b_1	-.395	-.233	-.309	-.455	-.378	-.554	.504
	(-0.59)	(-0.33)	(-0.45)	(-0.68)	(-0.56)	(-0.73)	(.57)
b_2	.591	.473	.770	.830	.396	0.851	.391
	(0.96)	(0.72)	(1.17)	(1.37)	(0.59)	(1.21)	(.50)
Effect of 2004 (β_1) and 2006 reforms (β_2) for workers who changed jobs:							
β_1	5.725**	5.226*	6.582**	5.501*	5.816**	5.667**	2.096
	(2.18)	(1.80)	(2.02)	(2.01)	(2.22)	(1.98)	(.74)
β_2	-.830	-.551	-1.259	-.577	.243	-.527	-.077
	(-0.38)	(-0.25)	(-0.52)	(-0.26)	(0.09)	(-0.24)	(.04)
R^2	0.088	0.088	0.094	0.047	0.097	0.049	0.086
Obs.	3,565	3,498	2,916	3,565	3,104	3,565	3,565

t-values calculated using robust standard errors clustered
at the level of the individual are in parentheses.
* Significant at 10% level. ** Significant at 5% level.

Table A2. The Effects of the Reforms on Participation of nonworkers—-Alternative specifications

	Default	Child same age group	6 waves or more	No controls	Probit	Waves 1 to 8	Compare Age≤50	Non-par. matching†	Backtrack one period
η_1	.042	.039	.074*	.065**	.061*	0.046	0.054		.061
	(1.03)	(0.92)	(1.68)	(2.36)	(1.76)	(1.06)	(1.07)		(1.37)
η_2	-.011	-.013	-.004	-.011	-.010	-0.004	-0.015		-.019**
	(-1.10)	(-1.29)	(-0.37)	(-1.10)	(-1.10)	(-0.37)	(-0.98)		(2.17)
	Effect of 2004 (ϕ_1) and 2006 reforms (ϕ_2)								
ϕ_1	.038	.053	.034	.072*	.012	0.037	-0.017	0.029	-.021
	(0.89)	(1.22)	(0.73)	(1.66)	(0.32)	(0.88)	(-0.25)	(0.40)	(-.51)
ϕ_2	.077*	.099**	.116**	.093**	.064	0.085*	0.102*	0.123*	.046
	(1.89)	(2.30)	(2.57)	(2.26)	(1.59)	(1.78)	(1.66)	(1.79)	(1.14)
R^2	0.151	0.152	0.133	0.261	0.170#	0.150	0.108		.150
Obs.	2,148	2,069	1,648	2,148	2,148	1,912	1,242	2,148	2,148

t-values calculated using robust standard errors are in the parentheses.
* Significant at 10% level. ** Significant at 5% level. # Pseudo R^2.
†Local linear regression matching with standard errors bootstrapped with 500 replications.

Table A3. The Effects of the Reforms on Participation of Lone mothers—-Alternative specifications

	Default	Observed in >5 waves	No other controls	Probit	Waves 1 to 8	Non-par. matching†	Backtrack one period
η_1	-.048	-.033	-.167**	-.058	.055*		-.051
	(-1.56	(-.88)	(-6.89)	(-1.51)	(1.76)		(1.58)
η_2	-.001	-.001	-.009*	-.002	.003		.003
	(-0.35)	(-0.21)	(-1.95)	(-0.53)	(0.60)		(.84)
	Effect of 2004 (ϕ_1) and 2006 reforms (ϕ_2)						
ϕ_1	.007	.009	.036	.002	.006	.020	.007
	(0.28)	(0.33)	(1.30)	(0.07)	(0.25)	(0.62)	(.029)
ϕ_2	.047*	.056*	.069**	.044	.042	.060*	.039
	(1.71)	(1.73)	(2.24)	(1.52)	(1.44)	(1.73)	(1.42)
R^2	0.256	0.250	0.020	0.226#	0.257		.255
Obs.	9,239	6,740	9,239	9,239	8,187	9,239	9,239

t-values calculated using robust standard errors are in the parentheses.
* Significant at 10% level. ** Significant at 5% level. # Pseudo R^2.
†Local linear regression matching with standard errors bootstrapped with 500 replications.