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Hayley Fisher and Anna Zhu

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Hayley Fisher[†] and Anna Zhu[‡]
[†] School of Economics, The University of Sydney
[‡] Melbourne Institute of Applied Economic and Social Research, The University of Melbourne; and ARC Centre of Excellence for Children and Families over the Life Course

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Melbourne Institute of Applied Economic and Social Research
The University of Melbourne
Victoria 3010 Australia
***Telephone* (03) 8344 2100**
***Fax* (03) 8344 2111**
***Email* melb-inst@unimelb.edu.au**
***WWW Address* http://www.melbourneinstitute.com**

Abstract

This paper examines how a reduction in the financial resources available to lone parents affects repartnering. We exploit an Australian natural experiment that reduced the financial resources available to a subset of separating parents. Using bi-weekly administrative data capturing separations occurring among low and middle income couples, we show that the policy reform significantly increased the repartnering hazard for affected separating mothers, especially those with low labour force attachment. Reconciliation with the woman's prior partner drives this result. Complementary analysis of an annual panel survey demonstrates that repartnering impacts are also present over the five years post-separation and that the impact on repartnering hazards is increasing in the extent of financial loss and the urgency of the impact. Together, these results demonstrate that one way that lone mothers respond to a reduction in financial resources available at the time of relationship breakdown is by repartnering more quickly.

JEL classification: J12, J18, H53

Keywords: Repartnering, lone parents, welfare reform

1 Introduction

Lone parenthood is an important risk factor for poverty across the world (Brady & Burroway 2012). Relationship breakdown is an important pathway to lone parenthood, and brings with it a significant financial shock, in particular for women (Duncan & Hoffman 1985, Jenkins 2008, de Vaus, Gray, Qu & Stanton 2014). Around half of women experiencing relationship breakdown repartner within five years (Wu & Schimmele 2005, Skew, Evans & Gray 2009), and this is a key mechanism by which these women's household income recovers (Fisher & Low 2016). However, little is known about what drives this repartnering process and, in particular, the scope for policy influence.

We exploit a natural experiment that unambiguously reduced government transfer payments for a subset of lone parents in Australia to estimate the effect of a reduction in financial resources available to lone parents on their rate of repartnering. We use biweekly administrative data for 11,895 separating mothers to estimate the short-run repartnering response to the reduction in financial support. We complement this analysis with nationally representative survey data from the Household, Income and Labour Dynamics in Australia (HILDA) survey to examine longer-run responses.

The Australian 2006 Welfare to Work reforms removed access to the Parenting Payment Single (PPS) for newly separated parents with a dependent children aged eight or older, offering a strictly lower payment in its place. This change reduced lone mothers' household income by up to 17%. Parents separating before this reform was implemented were grandfathered into the old eligibility rules, allowing access to PPS until all dependent children turned 16. Mothers separating before the reform are therefore a natural comparison group for estimating the impact of reduced financial support on repartnering.

Using administrative data, we find that this reform increased a mother's short-term repartnering hazard by 38%, equivalent to an increase from a 6% to an 8.4% probability of repartnering by 14 weeks after separation. The majority of this repartnering is reconciliation with the former partner, and the response is concentrated among mothers with low labour force attachment and those living in areas with high housing costs. We argue that this captures the short run response for mothers separating without knowledge of the reform, and excludes any longer term response driven by changing selection into separation.

Using annual survey data, we find evidence of changing selection into separation after the reform, though this does not happen immediately. Focusing on a sample of mothers separating in the three years around the reform when this differential selection is not observed, we again find that the reform significantly increased a mother's repartnering hazard.

We find that the repartnering effects vary by the intensity of treatment in both samples. Mothers whose youngest child is aged eight at the time of the reform face the highest treatment intensity: they are immediately affected and have the largest number of years of payments foregone. In general, the repartnering hazard increases as the number of years of PPS payments lost increases, and decreases with the number of years of payments remaining until the impact is felt.

These results contribute to a growing international literature examining the impact of policy on partnering decisions. A body of research has examined the impact of financial incentives in personal income tax systems on marriage, cohabitation and divorce decisions, typically finding robust but small effects (Whittington & Alm 1997, Dickert-Conlin 1999, Herbst 2011, Fisher 2013, Michelmore 2015). A range of aspects of divorce law have also been linked to relationship formation and dissolution decisions, including the shift from consent to unilateral divorce laws (Matouschek & Rasul 2008), the adoption of joint custody laws (Halla 2013), and changing levels of child support (Walker & Zhu 2006).

A large literature has evaluated the impact of US welfare reform on family structure, with mixed conclusions (Grogger & Karoly 2005, Moffitt, Phelan & Winkler 2015). These studies typically exploit variation in the timing and structure of welfare reform across states, and capture the impact of a bundle of reform aspects including time limits, family caps and earnings disregards (Acs & Nelson 2004, Bitler, Gelbach, Hoynes & Zavodny 2004, Fitzgerald & Ribar 2004, Bitler, Gelbach & Hoynes 2006, Dunifon, Hynes & Peters 2009). These studies mainly focus on transitions to and from legal marriage, excluding cohabitation, and do not specifically examine mothers' repartnering risk after relationship breakdown. Outside of the US, Anderberg (2008) and Francesconi, Rainer & Van Der Klaauw (2009) show that changes in in-work benefits in the UK significantly impact partnering and relationship breakdown decisions.

In comparison to this body of research, we make four key contributions. First, we use two different longitudinal data sources to examine the speed of transitions from entering lone motherhood after a relationship breakdown to repartnering. This captures separations from and transitions to marriage and informal cohabitation, and so captures broader effects than those studied by the previous literature. Informal cohabitation is more likely than marriage in the short run due to the legal arrangements required for marriage, and is an increasingly important household structure for lower income groups in Australia and elsewhere (Kennedy & Bumpass 2008, Buchler, Baxter, Haynes & Western 2009). Our larger sample size allows us to estimate effects for various demographic subgroups, and the use of a nationally representative panel survey allows us to examine the longer-term effects. Second, the data used for our short-term analysis allows us to identify reconciliations, so we can show that the short-term responses are overwhelmingly a return to the previous partner. Third, as we have biweekly observations, we can examine repartnerings that occur rapidly after separation, capturing an aspect of relationship volatility that would be missed with annual data. Finally, we exploit a simple policy reform that changes financial incentives for repartnering meaning that we identify the effect of the changing payment rate, rather than the combined effects of a spectrum of welfare policy variables such as time limits, family caps and variable earnings disregards. Moreover, we do not rely on state-by-time variation in policy that could be endogenously determined.

The paper proceeds by first setting out a conceptual model of how changing financial incentives affects repartnering decisions, before Section 3 describes the Australian Parenting Payment reform. Section 4 describes the two datasets used, and Section 5 explains our empirical strategy. Sections 6 and 7 present our results, and Section 8 concludes.

2 Financial incentives and repartnering decisions

Individuals make decisions about dissolving and forming cohabiting relationships based on a wide variety of factors, including the financial resources available as a single or couple. The following conceptual model, based on the model laid out in Becker, Landes & Michael (1977), sets out how changes to the financial resources available as a lone mother may change the decision to enter a new cohabiting relationship.

Consider a woman (f) and a man (m) who are currently single. Each has an individual utility $U_s^i(y_s^i, X_s^i)$, $i = m, f$, determined by their income when single, y_s^i and other characteristics when single X_s^i including, for example, the number of children the individual has and the social support networks available to them. We assume that these utilities are increasing in the level of income received:

$$\frac{\partial U_s^i}{\partial y_s^i} > 0, \quad i = m, f$$

If the woman and man form a cohabiting relationship, their individual utilities will be $U_p^f(y_p^f, y_p^m, X_p^f, X_p^m, \theta^{mf})$ and $U_p^m(y_p^f, y_p^m, X_p^f, X_p^m, \theta^{mf})$ respectively, where y_p^i and X_p^i are i's income and characteristics when partnered, and θ^{mf} is a couple-specific term representing match quality, which includes non-monetary aspects of household utility such as the presence of conflict or domestic violence. Match quality can evolve over time within a relationship and may be subject to shocks. Again, we assume that these utilities are increasing in incomes:

$$\frac{\partial U_p^i}{\partial y_p^f} > 0, \quad \frac{\partial U_p^i}{\partial y_p^m} > 0, \quad i = m, f$$

Second, we assume that utility is transferable within a cohabiting relationship: a couple can change the allocation of resources within their household to give any division of utility. Given these assumptions, relationship formation and dissolution decisions will be based on a comparison of the sum of utilities as a couple and as single. A man and woman will choose to form a cohabiting relationship when:

$$U_p^f + U_p^m \geq U_s^f + U_s^m \tag{1}$$

The assumption of transferable utility means that when this condition holds, it is always possible to allocate utility such that $U_p^f \geq U_s^f$ and $U_p^m \geq U_s^m$, implying individually rational efficient relationship formation decisions. When condition (1) does not hold, the two individuals will not form a cohabiting relationship.

Similarly, an intact couple will separate only when condition (1) does not hold: in the event of changes within or outside the relationship that result in $U_p^i < U_s^i$ for one partner, the allocation of utility within the relationship will adjust to satisfy individual rationality as long as a relationship surplus exists.

Given this framework, we can make predictions about the effect of a reduction in payments to lone parents on separation and partnering decisions. This is equivalent to a reduction in y_s^f, implying a reduction in U_s^f. All other things equal, this makes a lone mother more likely to choose to repartner, as it becomes more likely that condition (1) holds with any given prospective partner. We therefore expect a reduction in the level of welfare payments to lone parents to increase their rate of repartnering.

The second implication of this framework is that if U_s^f falls, a couple currently cohabiting is less likely to separate, implying a change in the population who choose to separate. Some couples will remain together as there are no longer utility gains from separating: these can be described as 'marginal' couples. Mothers in these marginal couples may be those who are most likely to repartner if they were to separate. This implies that a reduction in the level of welfare payments to lone parents could reduce lone mothers' rate of repartnering. A further consideration is that these marginal couples must be aware of the change to y_s^f to understand they are better off remaining together: it may take time for knowledge of the policy reform to spread among the affected group, meaning that this changing selection into separation takes time to occur.

The framework therefore suggests that a reduction in payments to lone mothers will have two opposing effects on the repartnering rate: first, conditional on separating, lone mothers will increase their repartnering rate, all other things equal. On the other hand, changing selection into separation is likely to reduce the repartnering propensity in the pool of those who separate. It is plausible that the changing selection into separation will take time to occur as marginal couples learn about the reduction in lone parent payments, whereas the reform will be immediately felt by lone mothers experiencing the lower payment rate. The analysis below identifies the short-run response of lone mothers who have already separated, before this changing selection has occurred.

A body of literature supports this approach to modelling relationship formation and dissolution decisions. Marriage penalties and subsidies in the US personal income tax system have been shown to influence both the decision and timing of marriage and divorce. Facing a marriage tax penalty makes an individual less likely to be married (Alm & Whittington 1995, 1999), more likely to delay marriage (Sjoquist & Walker 1995, Alm & Whittington 1997), and more likely to divorce (Whittington & Alm 1997, Dickert-Conlin 1999). The expansion of the Earned Income Tax Credit (EITC) in the US increased the financial penalty for marriage and reduced flows into marriage (Herbst 2011). These penalties and subsidies are determined by legal marriage, and have also been shown to influence the legal marital status of cohabiting couples in the expected direction (Fisher 2013, Michelmore 2015). There is less evidence from outside the US. Walker & Zhu (2006) show that an increase in the level of child support payments (reducing the financial resources available to the payee parent) in the UK reduced relationship breakdown rates. Couple penalties in the UK benefits system have been shown to significantly reduce the likelihood of partnering (Anderberg 2008). In addition, the introduction of the Working Families Tax Credit (which increased resources available to some lone mothers) has been shown to have reduced the chances of affected lone mothers repartnering (Francesconi & Van der Klaauw 2007) and increased the chances of the most affected couples separating (Francesconi, Rainer & Van Der Klaauw 2009).

This paper provides further support for the conclusion that financial incentives affect partnering decisions. However, it is distinct from the existing literature in a number of ways. First, it focuses explicitly on the decision to repartner after a stable relationship has broken down, and is not limited to formal marriage. Second, it does not rely on cross-jurisdiction variation in financial incentives that could be endogenously determined as is common in the US literature, and does not use childless women or higher income women as a control group as in the UK literature. Instead, our control group is separating mothers with younger children who are not immediately affected by the reform. Finally, it provides the first evidence for Australia.

3 The 2006 Parenting Payment Single reform

The Australian welfare system provides a number of payments to families. The income support payments targeted at low income parents are the Parenting Payments. The Parenting Payment Partnered (PPP) provides payments to low income couples with young children, and the Parenting Payment Single (PPS) is paid to low income lone parents.[1] In 2006, the PPS provided a maximum payment of \$499.70 (AUD) per fortnight to recipients, with a taper rate of 40% after income of \$128 per fortnight.[2] Lone parents with a dependent child aged less than sixteen years were eligible for the PPS.

The 2005 Australian budget introduced a range of reforms to welfare payments broadly described as 'Welfare to Work'. The objective of the reforms was to increase labour market activity among those receiving welfare pensions and allowances. The key change to the PPS was a change in the youngest child age eligibility criteria for new applicants. New applicants with a youngest child aged seven or less would continue to receive the PPS, whilst new applicants with older children were instead eligible for Newstart Allowance (NSA), a less generous payment. Figure 1 illustrates the difference between these two payments. NSA is less generous than PPS, with a maximum payment of \$444.20 per fortnight, a lower allowable income of \$62, and higher taper rates of 50% and 60% for income over \$250 per fortnight. New applicants with a youngest child aged eight or more were made unambiguously worse off by this change and their effective marginal tax rate was increased. A lone mother with one child aged eight earning no private income faced a reduction of 7% of disposable income, and a mother with private earnings of \$20,000 saw disposable income fall by 17%.

For this paper, the crucial feature of this reform is the grandfathering of existing PPS recipients. Those receiving PPS before 1 July 2006 were eligible to receive the payment

[1] There were age-eligibility changes to the PPP but these are not aligned with changes to the PPS and are not the focus of this paper.

[2] The allowable income before taper increased by \$24.60 for each additional child.

until their youngest child reached the age of sixteen. This creates a control group of plausibly otherwise-similar lone parents who are eligible for PPS to compare to the group separating after the reform and who are eligible for the lower NSA payment.

One concern is that the reduced payments may have changed a couple's optimal *reporting* choice to maximise their combined income – the question of whether it pays to lie about their relationship status. The income-maximising choice is unchanged after the reform: government transfer receipt is always higher when two individuals do not report a relationship than when they do. For example, before the reform, combined gross income for a lone mother with no private income and a man with an income of $35,000 was 31% higher than when admitting to being in a cohabiting relationship; after the reform, combined gross income when reporting a separation is 28% higher than when reporting a relationship.[3] Hence, a couple seeking to maximise their combined income through misreporting their relationship status will always report that they are separated, regardless of the policy regime.

In addition to the change to the child eligibility age, the reforms also changed activity requirements for recipients. All new PPS applicants with a child aged six or more were required to engage in 15 hours of paid work or work-related activity per week, in line with the activity requirements for NSA recipients. Note that this was not aligned to the child eligibility age: a mother separating with youngest child aged six would receive the higher PPS payment, but would face the same activity requirement as a mother with an eight year old child receiving NSA. She would also continue to face the activity requirement when partnered. The activity requirements were gradually extended to existing PPS recipients.

This is not the first study to evaluate the impact of this reform. Brady & Cook (2015) provide a review of qualitative and quantitative evidence on these and earlier reforms, highlighting the negative financial impact for lone parents and indicative evidence of reductions in subjective wellbeing and mental health. Gong & Breunig (2014) demonstrate that lone mothers increased labour force participation rates in response to these reforms. Fok & McVicar (2013) use administrative data for existing PPS recipients and find that women subject to work requirements were more likely to exit income support, although the data used did not allow the reason for leaving income support – which could be finding a job or repartnering, for example – to be identified. Previous studies of payments to lone parents in Australia for earlier reforms have also suffered from this limitation (Barrett 2002, Doiron 2004, Gregory, Klug & Thapa 2008).

[3] These numbers are based on a couple with one child aged eight, and incorporate Family Tax Benefit Parts A and B, Parenting Payments, and Newstart Allowance.

4 Data

We draw on two sources of data to understand the short and longer-term repartnering responses to the removal of eligibility for the welfare payment paid to single parents. First, we use high-frequency administrative data to look at short-term repartnering decisions. We also use data from the Household, Income and Labour Dynamics in Australia (HILDA) survey, a nationally representative annual panel survey, to examine the longer-term responses.

4.1 Administrative data

We use an administrative dataset drawn from biweekly records of adults receiving Family Tax Benefit (FTB) Payments. In Australia, FTB is paid to around three quarters of all families with dependent children, including all lone parents and all parents on any type of income support payment (Bradbury & Zhu 2012). For mothers in our sample, the dataset contains biweekly information on all family and income support payments received along with demographic information required to administer these payments, including the ages of all children and relationship status. All individuals and their partners, have a unique identifier across the dataset, meaning we can observe relationship transitions on a biweekly basis and distinguish reconciliations from finding a new partner.

We observe individuals whenever they are receiving FTB payments, expanding our observation criteria significantly beyond that used in previous studies such as Fok & McVicar (2013), which require income support receipt for sample inclusion. Studies based on data drawn from income support receipt records require a mother to be an income support recipient before separation, to receive income support payments whilst a lone mother, and to continue to receive income support payments if they repartner for a full relationship status transition to be observed. In contrast, our data captures the full relationship transition of mothers who were in middle income households (receiving no income support, but receiving family benefits) when partnered, throughout any separation, and any repartnering back into a middle income household. Many such women would be eligible for income support payments such as the PPS when a lone mother and so will be affected by the 2006 reform. However, our data does not capture high income households.[4]

[4]For 2006, a one child couple household with income above \$94,718 would be excluded, with the threshold increasing with the number of children. Median gross household income in 2005-2006 was \$54,080.

The analysis sample consists of women who were (a) partnered and receiving FTB on 10 December 2004, (b) observed in that same relationship throughout 2005, and (c) separated from that partner at some point during 2006.[5] We exclude families where the father was designated the primary carer at any point, where either parents was eligible for the Age Pension (aged above 65 for men and 60 for women), and those who die at some point during the analysis window. We restrict attention to mothers whose youngest child was aged between 2 and 16 throughout the analysis period to give a comparable treatment and control group. Our data follows these mothers through to the end of 2006 conditional on continued receipt of FTB. This yields a sample of 11,895 mothers who separate during 2006, with a total of 141,286 post-separation observations.

All FTB recipients are required to report any change in relationship status within 14 days of the change. There are financial incentives to report relationship breakdown, and severe penalties can apply to recipients who fail to report that they have repartnered.[6]

There are two important implications of this sample selection for our analysis. First, mothers separating earlier in 2006 are observed for longer than those separating towards the end of the year, creating endogenous censoring of observations related to the time of separation. Second, since all women in our sample are partnered consistently from December 2004 through to the observed separation, the post-reform separations require a longer period of relationship stability. Below, we present a number of robustness checks to ensure that these features are not driving our results.

A mother is observed to have repartnered if she ends her first spell of separation by reporting that she is partnered. She may be partnered with her previous partner of 10 December 2004 or a new partner. Mothers who stop receiving family payments drop out of our sample and are treated as censored observations, as are mothers who remain separated until the end of the analysis window. Further information about this data and how we deal with temporary missing observations and other anomalies is given in Appendix A.

[5]To be included in the base dataset, the woman also needed to be partnered and receiving FTB on 15 June 2001, though not necessarily to the same partner as in 2004. See Appendix A for a detailed description of the underlying dataset and our sample selection.

[6]The maximum penalty can range from imprisonment of between 12 months and 10 years depending upon the charge. Centrelink (the agency that administers all income support payments) reviews the eligibility of around two thirds of all income support recipients each year (usually by cross-verifying reported income with, for example, tax return information), makes adjustments as necessary and pursues prosecution in cases of fraud (Prenzler 2011). Qualitative evidence on the attitudes and behaviour of mothers receiving PPS and other income support payments indicates a desire to truthfully report circumstances coupled with a difficulty in navigating the disparate systems of family payments, income support and child support (Rawsthorne 2006). Complying with all systems truthfully is seen as a burden for these mothers; the information and attention requirements for 'gaming' the system are high.

Table 1 shows summary statistics for mothers in this sample as at 10 December 2014. At this point, before the event of separation, nearly a quarter of mothers in the sample are receiving income support payments. As our sample comprises of middle income households as well, we see that 24% own their home and 30% are in the private rental market. However, this captures the joint housing wealth and circumstance of the mother and her partner and may not reflect post-separation housing options. Furthermore, one-tenth of mothers are in receipt of child maintenance payments from a previous partnership. The demographic characteristics of mothers in the sample are as follows: 22% are living with a defacto partner; 22% are born overseas; 3% are Indigenous women, nearly a third have several children (three or more) where their youngest child is aged 8 on average; and nearly three-quarters of mothers in the sample are aged 35 years or above.

4.2 Annual panel survey data

We also use data from waves 1-14 of the HILDA survey, covering the period 2001-2014. This is a nationally representative longitudinal survey based on an initial sample of 7,682 households that records rich information regarding economic wellbeing, labour and family dynamics. This allows us to follow individuals over time as their relationships form and breakdown.

Relationship breakdowns are derived from the Household Form, which collects initial information about all household members and their relationship to each other. All women who are recorded as living with a spouse (including both married and de facto partners) are 'at risk' of relationship breakdown. We observe a relationship breakdown when we observe the woman with a partner in wave t, and without a partner at wave $t + 1$ (and never with the previous partner again), or when we observe a woman with a different partner at wave $t + 1$. We additionally include women observed with a partner at time t, with no partner at time $t + 1$, and with the previous partner again at a future wave if and only if the reason for the partner not being present at time $t + 1$ is stated to be 'separation or divorce'. Since the survey is conducted annually, this does not capture short separations and reconciliations that occur between surveys.

We select a sample of women aged between 18 and 60 who are observed experiencing relationship breakdown during the sample period, and use observations in the five waves following the breakdown. We further restrict our attention to women with dependent children and women who have had children in the past.

Of critical importance to our empirical strategy are the age of the woman's youngest child at the time of relationship breakdown, and whether the separation occurred before or after the reform was implemented in July 2006. Respondents report the month and year of a separation, so provided that these reports are accurate we can assign separations to the pre- and post-reform period with confidence. Children's ages, however, are only known for sure as at 30th June of each survey year – the month and year of birth are not provided. This means that we do not know for sure whether a child that is aged seven on 30th June was aged 7 or 8 if a separation occurred in October of that year, and conversely if a child is aged eight on 30th June, we do not know if they were aged 7 or 8 if the separation occurred earlier in the calendar year. Below, we present results assuming that all children aged 8 on 30th June were aged 7 at the time of separation if the separation occurred earlier in the calendar year, and that all children aged 7 on the 30th June were aged 8 at the time of separation if the separation occurred later in the calendar year. Since this almost certainly misclassifies some children between the treatment and control groups, we additionally present results excluding all children aged 7 or 8.

This results in a full sample of 708 individuals and 2788 post-separation observations. Table 2 shows summary statistics for this sample the last time they are observed in a relationship. On average, these mothers have 1.6 children and are 38 years old. Their youngest child is aged 8 on average, broadly comparable to the administrative data. Also comparable to the administrative sample are the proportion born overseas (19% vs. 22%) and the proportion in the private rental market. Around 17% of the sample have a high level of education, and household income was $87,600 on average when these women were last observed in a relationship.

We also consider a subsample of these women. This focused sample includes women separating between 2005 and 2007 to extract those women most likely to be affected by the reform without prior knowledge. In Section 6.2 we show that over this time range there is no significant change in the composition of separating mothers based on the age of their youngest child. This sample of women is broadly comparable, but is slightly more likely to have a high level of education and has, on average, older children.

5 Empirical strategy

Our aim is to estimate how a negative financial shock affects the speed of repartnering of single mothers. We exploit the natural experiment created by the change in eligibility age for the Parenting Payment Single from July 2006. The policy reform reduced the welfare payment paid to newly single parents with their youngest child aged eight or more, whilst grandfathering existing recipients under the old eligibility rules. We take a difference-in-difference approach: our treatment group is separating mothers whose youngest child was aged between 8 and 15 inclusive, who were immediately affected by the change in age-eligibility. The control group includes women who were not immediately affected by the age-eligibility change. Pre-reform separations occurred before the end of June 2006, with all separations observed from July 2006 onward being in the post-reform period.

Our first set of results estimate the single risk hazard rate of repartnering using a reduced form discrete-time hazard model. Each woman becomes 'at risk' of repartnering at the point of separation, and we observe their survival in this state until they repartner, exiting the state, or are censored. Each individual's survival history is broken down into a set of distinct observations, either biweekly or annual depending on the dataset. The dependent variable takes the value of one if the mother repartners or zero if she remains separated or is censored. We pool these observations and estimate a logistic regression model predicting whether a repartnering event occurred. $P_{i,t}$ – the conditional probability that mother i repartners at time t, given that she has not yet exited her first separation spell – is related to a set of covariates by a logistic regression equation:

$$log[P_{i,t}/(1-P_{i,t})] = \alpha_t + \beta_1 treated_i + \beta_2 post_i + \beta_3 treated_i * post_i + \gamma X_i + \theta Y_{i,t} + \varepsilon_{i,t}, \tag{2}$$

Here, α_t in equation (2) represents a full set of dummy variables for each of the possible time periods of repartnering. This allows for a flexible baseline hazard of repartnering, conditional on not having repartnered previously. We also control for the timing of separation: for the administrative data we include a third order polynomial of the date of reported separation and a set of month of separation indicators; and for the HILDA sample we include a set of wave of separation indicators. We also control for a vector X_i of socioeconomic and demographic characteristics of mothers measured in the period before their relationship breaks down, including whether the mother was in a defacto relationship, indigenous, overseas-born, age, number of children, child maintenance receipt and housing tenure. All of these variables may influence the baseline repartnering hazard: for example, a mother with more children may be less likely to find a new partner, and being in a de facto relationship rather than formal marriage may indicate attitudes towards partnering as well as differences in the costs of separation. In $Y_{i,t}$ we include measures capturing the age of the youngest child as it varies after relationship breakdown to capture any effects of the youngest child aging out of PPS eligibility.

The treatment effect of the policy reform is then estimated by β_3, where $treated_i$ is a binary indicator for mothers whose youngest child is aged 8 to 15 at the time of separation and $post_i$ is a binary indicator for the separation occurring from July 2006 onward. If the reform caused lone mothers with older children to repartner more quickly then β_3 should be significantly positive. This estimate should be interpreted as an Intention to Treat (ITT) effect of the policy change. Over the time after separation, some mothers will switch between treatment and control groups. For example, mothers who are initially categorised as a treatment case can become a control case after the separation date if they have another child. We do not examine this outcome, but note that there may be an incentive for a lone parent to have additional children to extend their period of eligibility for PPS.

The difference-in-difference approach identifies the impact of not being eligible for PPS receipt at the point of separation. Whilst the age of a woman's youngest child at the time of separation determines immediate eligibility for PPS, it does not capture the longer-term impact of the reform. For example, a woman with a seven year old child at the time of separation will be awarded PPS post-reform, but will only receive the payment until the child turns eight, reflecting a loss of eight years of PPS payments.[7] The intensity of the treatment, as measured by the number of years of PPS lost, is therefore of interest. All women with a child aged eight or below will lose eight years of payments as a result of the change. Women with a child aged over eight will lose fewer years of payments, for example a woman with a 12 year-old child will lose four years of previously-expected payments. Among women losing the maximum eight years of expected payments, however, the immediate impact is mediated by the number of years of PPS payments that can be received. For example, a woman with a four year old child loses the eight years of payments from ages 8-16, but will receive four years of payments until the child turns eight, allowing time for adjustment to the new regime. To capture this treatment intensity and urgency, we construct two variables to indicate the number of years lost due to the policy change ($years\_lost$), and the number of years of payments that will still be received under the new eligibility rules ($years\_rec$).

We estimate discrete proportional hazard models relating the probability of having repartnered by wave t after separation to covariates using the following logistic regression equation:

[7] Given that she would have received PPS until her youngest child turned 16 under the former policy regime.

$$
\begin{aligned}
\log[P_{i,t}/(1-P_{i,t})] = \alpha_t &+ \beta_1 post_i + \beta_2 years\_lost_i + \beta_3 years\_lost_i * post_i \\
&+ \beta_4 years\_rec_i + \beta_5 years\_rec_i * post_i \\
&+ \gamma X_i + \theta Y_{i,t} + \varepsilon_{i,t}
\end{aligned}
$$

Again, α_t is a set of dummy variables capturing a flexible baseline hazard, X_i are individual and household characteristics before relationship breakdown, including age, number of children and home ownership status, and Y_{it} includes the time-varying age of youngest child. The coefficients of interest are β_3 and β_5, which capture how the hazard of repartnering differs for women separating from July 2006 onward based on the number of years of payments that have been lost (β_3) and the number of years of payments that are retained (β_5). This identifies an intensity of treatment effect scaled for the extent of the financial loss caused by the policy change and by how far into the future the loss occurs.

5.1 Threats to identification

We will argue that our results isolate the impact of a reduction in financial resources on the speed of repartnering for those who have separated, and do not incorporate the longer-run effects of changing selection into separation. For this to be true, the policy change must be exogenous with respect to the decision to separate. It is not clear that this is the case. First, as outlined in Section 2, the choice to separate or remain in a relationship is likely to change. The policy change reduces the outside option of one partner and so may induce some couples to remain together when they would previously have separated, and the couples changing their decision will be those with the higher gains from their relationship who we would expect to be most likely to repartner. To the extent that mothers with the highest repartnering propensity will therefore be less likely to be present in the post-reform period, this form of selection will bias our causal estimates downwards. In Section 6 we look for evidence of changing selection in our data, and conclude that it took time for couples to learn about the policy change and so there is a sample of post-reform mothers who are not affected by this differential selection.

Second, some mothers may have responded to the announcement of the policy before implementation, leading to differential selection into separation between the policy announcement in May 2005 and the July 2006 implementation. The concern here is that mothers may be more likely to bring forward their separation in order to be grandfathered under the new rules when they expect to remain a lone parent for longer. As discussed below, we do not find evidence for these announcement effects.

A further threat to validity is that the policy change may induce a change in whether a lone parent *reports* that they have repartnered. If the payment available to the lone parent is reduced, it will reduce the financial loss from reporting repartnering. However, as explained in Section 3, a couple will always receive a higher level of combined income support payments and gross household income if they are each registered as single rather than as being in a relationship. So, while the policy change reduces the extent of the financial gain from reporting being separated, it does not change the dominant strategy for maximising welfare payments. We draw further confidence that changing reporting behaviour is not driving our results as there is no financial incentive to lie about repartnering in the HILDA survey.

6 Results

We begin by presenting results on short-term repartnering responses from the high-frequency administrative data, before examining the longer-term responses based on annual survey data. In both cases we show that the policy reform did indeed affect PPS receipt and increase the repartnering hazard for affected single mothers.

6.1 Short-term repartnering

Figure 2 displays empirical cumulative incidence functions for repartnering in the administrative data, split out by the treatment status and time period of the separation. The control group of women with younger children repartner more slowly in the post-reform period compared to previously. The repartnering rate in the treatment group of mothers with older children also falls in the post-reform period, but not as dramatically. In the first ten weeks after separation, the treated women repartner more quickly than in the pre-reform period, suggesting perhaps a shifting forward of some repartnering behaviour.

Table 3 presents regression estimates of the effect of the PPS reform on the repartnering hazard for the main administrative sample. Column (1) includes basic time controls and Column (2) includes the full set of controls for socioeconomic and demographic characteristics. The first three lines of Table 3 show a significant treatment effect of the policy: there is a significant increase in the repartnering hazard for women affected by the policy, with a coefficient estimate of 0.320. This is equivalent to a 38% increase in the repartnering hazard,[8] or an increase from a 6% to an 8.4% probability of repartnering at 14 weeks after separation.

Table 3 also shows that lower socioeconomic status mothers are more likely to repartner: mothers who were receiving income support whilst partnered (at 10 December 2004) are significantly more likely to repartner compared to those who were not receiving income support. Similarly, mothers who owned their own home whilst partnered (again at 10 December 2004) were significantly less likely to repartner. Older mothers were significantly less likely to repartner whereas mothers with more children were significantly more likely to repartner.

One concern with the interpretation of these estimates is that the reform may have been inconsistently implemented. Appendix Table 13 shows estimates from a linear probability model of the probability of receiving PPS for the main administrative sample. Column (1) includes basic time controls and column (2) includes a set of additional socioeconomic and demographic controls. The treatment group of women with children aged eight to fifteen are generally four percentage points less likely to be receiving PPS than women with younger children, reflecting a higher level of labour market engagement. The July 2006 reform caused a statistically significant 45 percentage point reduction in PPS receipt for this group, demonstrating that the reform did indeed remove access to PPS for lone mothers with a youngest child aged eight to fifteen.

Treatment effect heterogeneity

Given the large sample size available in the administrative data, we are able to explore whether the effect of the policy reform disproportionately increased the repartnering hazard of relatively disadvantaged mothers. We estimate separate regressions for seven sub-groups based on the mother's historical labour force attachment as measured by Family Tax Benefit Part B (FTB(B)) receipt at 10 December 2004,[9] her state of residence at separation,[10] whether she was housing poor, and whether she was housing poor and living in New South Wales. We consider a mother to be housing poor if she was not renting in the private rental market, she was not a home owner and she was not living in

[8] As the estimation is implemented using a logit model, given a coefficient estimate of β the proportional impact on the underlying hazard can be approximated by $\exp(\beta) - 1$.

[9] FTB(B) eligibility for partnered households is tested solely on the secondary earner's income and in most Australian families with children, this refers to the mother's income. However, the income threshold for FTB(B) eligibility is very low, which means that only mothers with no or very low levels of labour force engagement are eligible. For example, a mother who works a two-day week and earns at the minimum wage would be ineligible for the payment.

[10] We only explore this for the larger states of New South Wales (NSW), Victoria (VIC) and Queensland (QLD) due to sample size limitations with the smaller states and territories.

government housing, thus it includes mothers who are boarding, lodging, or not paying rent.[11] These categories are defined at the point of separation, capturing the mother's post-separation housing circumstances.[12] We further estimate the treatment effect for a sample of mothers with four or more children.

Table 4 presents these results. It clearly shows heterogenous treatment effects by the mother's socioeconomic status and state of residence. Table 3 above demonstrated that there is an overall increase in the repartnering hazards among the group of affected mothers, and Table 4 shows that this impact is stronger for mothers in more socioeconomically disadvantaged groups and particularly for those living in NSW, the highest housing cost state in Australia.

Column (1) shows that mothers who had extremely low or no labour market participation (and so received FTB(B) when partnered) have a 43% higher hazard of repartnering, perhaps reflecting that these women are less able to smooth an unexpected financial shock by altering their labour force activity, and so are more likely to repartner to recover their financial resources.

Columns (2) to (4) show that mothers residing in NSW are significantly more likely to respond to the reform by repartnering compared to mothers residing in the other large states. The treatment effect for mothers residing in NSW is a 114% increase in the repartnering hazard. This may reflect that NSW (and especially Sydney) residents on average face higher housing costs than those in the rest of Australia. Thus mothers in NSW may have additional financial incentives to turn to repartnering if affected by the reform.

Columns (5) and (6) show that mothers who had precarious housing arrangements after separation, particularly in NSW, have a 51% and 139% respectively, higher hazard of repartnering. This is likely to reflect that women with a lower stock of independent wealth or access to public housing, particularly those living in areas of high housing costs, are less able to smooth the housing instability associated with separation.

Finally, Column (7) shows that mothers with four or more children have a statistically insignificant 12% increase in the repartnering hazard. Whilst mothers with greater caring responsibilities are likely to find it more difficult to adjust labour market participation

[11] Homelessness was not a recorded status during the analysis time frame, however, there may have been a number of homeless mothers in our sample.

[12] Changes in the housing situation after separation may mediate some of the policy effect on repartnering, however, as the variables are measured at the point of separation (or soon after the event, allowing for lags in reporting), we minimise the extent of this issue.

and so encourage an increase in their repartnering hazards, this may be offset by the large caring responsibilities reducing the mother's appeal on the relationship market.

These estimated treatment effects are illustrated in Figure 3. The levels of the diamonds represent the treatment effect for mothers who separated before the introduction of the reform, that is, relative to the baseline hazard for women with younger children, and the squares represent the treatment effect for mothers who separated after the reform. The difference between the two points represents the treatment effect of the policy on the estimated logit hazard, and the stars indicate statistical significance. Overall, the conclusion is that relatively disadvantaged mothers were more affected by the policy reform. Mothers who have fewer opportunities in the labour market (either because of skills, discrimination, attitudes or greater caring responsibilities) or who have precarious housing situations may be more likely to turn to repartnering as channel of income recovery upon the event of separation.[13]

Who are these women repartnering with?

Our data allows us to distinguish between reconciliations with a women's prior partner and partnerships with a new man.[14] Previous descriptive analysis of this population suggests that 34% of separating lone mothers will reconcile at some stage, with the majority doing so within a year (Bradbury & Zhu 2012). We use a competing risks discrete time hazard model to estimate these two competing exits from lone parenthood. We also include the outcome of dropping out of the family payment system before the end of the sample, which can happen if the mother repartners with a high income man and so ceases receiving family payments. We implement this using a Multinomial Logit regression.

Table 5 presents the estimates of the treatment effect of the policy on the three types of exits, where the model estimated includes our full set of controls. The policy increased the hazard of a reconciliation by 34% (statistically significant at the 5% level). It also increased the hazard of a new partnership by 48% but this effect is not precisely estimated, reflecting the lower prevalence of finding a new partner in the short run for this population. There is no measurable effect on dropping out of the sample, so we can rule out an effect encouraging repartnering with new high income partners.

[13] Alternatively, unobservable characteristics may underlie a mother's propensity to exhibit these observable traits as well as her rapid repartnering behaviour.

[14] In 2006, Centrelink did not recognise same-sex relationships for the purposes of welfare payments, so this only captures opposite sex separations and repartnering.

It is also instructive to separate reconciliations into those where mother receive income support payments after repartnering, and those where women exit the income support system. When partnered, a mother's eligibility for income support depends on household income, so mothers who continue to receive income support payments have repartnered with a low income man and are in a low income household. We therefore estimate a competing risks model that has distinct exits from separation for: a reconciliation with receipt of income support, a reconciliation without any income support, a new partner,[15] and leaving the family payment system. Table 6 presents the estimates for a model including our full set of controls.

These estimates clearly show that the policy increased mothers' hazard of repartnering with low income men but had little effect to encourage mothers to repartner with higher income men. This reinforces the story that it is the most financially vulnerable mothers who respond to the reform by reconciling, and suggest that these reconciliations may not strongly improve mothers' wellbeing.

Robustness checks

As discussed above, there are a number of potential threats to our identification of the impact of reduced financial resources on repartnering for those who have separated. In this section, we perform a number of robustness checks and exploratory analyses. First, we look for evidence of changing selection into separation, and test for evidence of announcement effects. We then check whether endogenous censoring explains our results. Finally, we use a triple difference specification to eliminate concerns of seasonal trends influencing our results.

The results above reflect the total impact of the PPS reform on repartnering, incorporating any changing selection into separation, either due to equilibrium effects or anticipation effects. In this section, we examine the degree to which the policy induced mothers to manipulate the timing and/or incidence of separation. To begin, we investigate whether there are any patterns in observable characteristics by the timing of separation across 2006. Discontinuities in these characteristics at the time of the reform's implementation could indicate changes in selection into separation.

Figure 4 plots averages of individual characteristics by the timing of separation. The top-left figure shows the percentage of mothers in receipt of Parenting Payment Single (PPS). There is a steep decline in the PPS rates around the beginning of July 2006,

[15] Given the smaller number of new partners observed, dividing this group further by income support receipt is not informative.

reflecting the implementation of the reform.[16] However, for all the other variables, there are no significant discontinuities at the beginning of July 2006. The upward trends in age of mother and age of youngest child reflect the aging of our sample. This shows that there is no change in selection into separation visible in observable characteristics, and so our estimates are likely to capture the impact of reduced financial support for separated parents, and not the longer-run effects incorporating changing selection.

Anticipation effects may be part of our results: some mothers may have pulled forward their separation after the policy was announced in order to secure eligibility to PPS payments. As the package of reforms was announced in the Federal Budget on 10 May 2005,[17] potential announcement effects may give rise to selection into the pre-reform group (who we have defined as those separating between January and June 2006). If mothers who pull forward their separation are less likely to repartner then our main results will reflect this.

In order to assess whether announcement effects contribute to our results, we redefine our pre-reform group mothers to be those who separated before the announcement of the policy i.e. those who separated between January and 10 May 2005. Table 7, Column 1 shows that the reform also raises the repartnering hazard for the targeted mothers by 23%, compared to mothers who separated before the announcement of the policy. This effect is smaller than the estimated 38% effect from Table 3. This may reflect some anticipation effects lowering the repartnering hazard in the main pre-reform group. However, another explanation is that the duration between December 2004 (when all of the mothers in our sample are partnered) and the date of first separation (January - May 2005) is shorter than the pre-announcement period, and thus may be more likely to capture relatively unstable relationships – those that dissolve quickly but also reconcile quickly.

A further potential bias to our results may be due to differing potential post-separation observation periods, causing endogenous censoring. Our main results assume that the time of censoring is independent of the hazard rate of repartnering. To examine whether this censoring influences our results, we impose an artificial censor date of the end of June 2006 for mothers separating in the pre-reform period so that the observed post-separation period is equal for mothers who separated before and after the mid-July cut-off date. Table 7, Column 2 shows that the treatment effect estimate increases relative to the main results, reflecting an 86% increase in the baseline hazard, and supports the conclusion that the policy caused an increase in the repartnering hazard.

[16]The discontinuity is not sharp because some mothers (with four or more children, or those who were experiencing domestic violence for example, were accorded a grace period).

[17]Initially, the age of eligibility change was set for when the youngest child turned 6: this was subsequently changed to age 8.

There may be seasonal trends in separation or repartnering that explain out results. That is, women with older children separating in the second half of the calendar year (springtime, and the second half of the school year in Australia), may face different pressures due to these seasonal variations and repartner more quickly. We address these concerns using a triple difference specification (DDD), using separations occurring in 2005 as an additional comparison group.

In this specification, the treated group remains mothers with a child aged between 8 and 15 at the time of separation, with the control group being women with younger children. The post-reform group is now all separations taking place in the second half of the calendar year. The third difference is between the DiD estimates for the 2006 separations and the 2005 separations. Table 8 presents these estimates. The triple interaction term is positive and statistically significant, suggesting a 47% increase in the repartnering hazard. This gives further support to the conclusion that the fall in financial support for lone parents significantly increases the speed of repartnering.

6.2 Longer-term repartnering

We use data from HILDA to examine the longer-term effects of a reduction in lone parent welfare payments on repartnering. We begin by examining how selection into separation changes after the PPS reform: we expect that over time, as the impact of the policy becomes well-known, there will be changes in the population of those who separate which mean that the further away from the reform the separation occurs, the smaller the total impact on repartnering will be. From this analysis, we isolate a focused sample of separations that are not observably impacted by differential selection into relationship breakdown.

Selection into separation

To examine whether the change in PPS eligibility induced a change in the decision to separate and the timescale of any change, we run regressions to examine how the age of the youngest child at the time of separation (which determines the change in PPS eligibility), and the probability of being in the treatment group, change as the time since the reform increases. Unreported regression results show that the average age of youngest child at the time of separation in the post-reform period is 0.8 years lower (statistically significant) than in the pre-reform period, although the probability of being within the treatment group is not significantly different. This suggests that there may be a change in the decision to separate that could influence our results. We next examine whether this change in the average age of youngest child happens immediately after the reform's implementation, or takes longer to emerge, as well as checking for differential treatment probabilities over time.

We regress the age of the youngest child and treatment status on a series of indicators for the wave of post-reform separation. The comparison group is the average age of youngest child for pre-reform separations or probability of being in the treatment group in the pre-reform period. As the youngest child's age is censored at 18 (for observations where the separating mother had children but they are no longer dependent), we also include an indicator for the child's age being over 18 in the age of child regression. Table 9 presents results from these regressions, including results controlling for mother's education status and a quadratic in pre-separation household income. We find no evidence of a difference in the average age of youngest child in the two waves following the reform. However, the age of youngest child of separating mothers is significantly lower by 1.6 and 1.4 years in waves 8 and 9 respectively. This may reflect mothers with older children remaining in relationships as they know that they will not be eligible for PPS if they separate. Coefficients for later waves are negative, but apart from wave 13 are statistically insignificant. Controlling for mother's education and pre-separation household income strengthens these results: more educated women from higher income households become more likely to separate relative to less educated lower income women. Given that these women are less likely to be reliant on PPS when separating, this also reinforces the conclusion of changing selection into separation from wave 8 onward.

Columns (3) and (4) of Table 9 show comparable results from a linear probability model for the outcome of being in the treatment group. Here, we see that the probability of being in the treatment group is lower relative to the pre-reform period for those separating in waves 9 and 13, but otherwise find insignificant results, even controlling for education and household income. This is a weaker result, which may reflect that the treated/not treated division is not as clear cut when looking at longer term outcomes: a mother separating with a 7 year-old youngest child after the reform receives a maximum of one year of PPS payments and, anticipating this, may be less likely to separate than previously.

Based on this analysis, we define a focused sample of the 176 separations occurring in waves 5 to 7 which is not contaminated with changing selection into separation, based on the observable characteristic that determines the treatment intensity.

Results

Table 10 shows our main results for the two samples. Columns (1) to (3) are for the full sample, and columns (4) to (6) are for the focused sample. Columns (1) and (4) include the set of time-since-separation indicators, capturing the baseline repartnering hazard, and a set of wave of separation indicators. Columns (2) and (5) add a set of controls that broadly align with those in the short-run analysis presented in Table 3, and columns (3) and (6) add additional regressors available in HILDA but not the administrative data, including education and pre-separation household income.

Columns (1) to (3) of Table 10 show that we do not find statistically significant effects of the policy change on repartnering in the full sample. In contrast, the third line of the Table shows that in the focused sample, we do detect a statistically significant impact of the reduced welfare payments on the repartnering hazard over the five years post-separation. The treatment effect coefficient of 1.69 represents a four-fold increase in the baseline hazard of repartnering. In line with the short run results, having more children and being older reduce the repartnering hazard.[18]

One key concern with the HILDA sample is that the classification of mothers into treatment and control groups may be incorrect due to the level of detail available for the age of children. To gain some comfort that misclassification is not driving these results, we reestimate the results for the focused sample excluding all children reported to be aged 7 or 8 (these children are vulnerable to misclassification between treatment and control). The results are reported in Appendix Table 15: estimated coefficient magnitudes are similar and are significant for the focused sample, giving confidence that misclassification is not driving our results.

As mentioned above, we might also be concerned that individuals may have responded to the announcement of the PPS reform by separating earlier than they otherwise would have done to ensure they were grandfathered into the previous PPS scheme. To test for this, we include an additional term indicating separations occurring after the policy announcement, also interacted with the treatment group indicator. These results are reported in Appendix Table 16, and show that there is no evidence of any different repartnering behaviour for women separating in the period between announcement and implementation. The precision of the estimates of interest is reduced, but point esti-

[18]Appendix Table 14 shows results from a linear probability model of the receipt of PPS. The significant negative coefficients of the treatment effect of the policy change demonstrate that we are indeed capturing the implementation of the PPS reform with this identification strategy, with the reform reducing PPS receipt by 15 to 19 percentage points.

mates reinforce the conclusion that the reform increased the repartnering hazard over the medium term.

7 Intensity of treatment

Over the longer term, the impact of the policy reform is not restricted to the immediate effect of having access to PPS at the time of separation: the number of years of PPS payments lost due to the reform depends on the age of a mother's youngest child. Here we present results capturing the broader impact of the reform: the number of years lost, and the time remaining before the loss is felt. Tables 11 and 12 show estimates of the coefficients of interest using the administrative data and for the focused HILDA sample, respectively.

For Table 11, Column (1) includes basic controls for the timing of separation and the age of the youngest child as a time-varying variable, and Column (2) includes the full set of controls as in Table 3. For Table 12, Column (1) includes additional controls for the year of separation, and columns (2) and (3) add controls as in Table 10 above, including linear and quadratic terms in the age of the youngest child to capture the impact of the child's age on repartnering hazards independently of the impact of the age of the child at the time of separation.

We see that each year of PPS payments lost as a result of the reform is estimated to significantly increase the repartnering hazard of women separating after the reform. This is particularly true for the HILDA sample where the coefficient size is 0.337 (representing a 40% increase in the baseline hazard for every year of payments lost) and is statistically significant at the 5 percent level (Column 3). In the administrative sample, the coefficient size is 0.067 (representing a 7% increase in the baseline hazard for every year of payments lost) and the effect is also significant at the 5 percent level (Column 2).

At the same time, the estimates show that for women separating after the reform, the number of years of payments remaining reduces the repartnering hazard. In the administrative sample, the coefficient size is -0.149 (representing a 16% reduction in the baseline repartnering hazard) and where the effect is robust at the the 1 percent level. In the HILDA sample, the coefficient is -0.349 (representing a 42% reduction in the baseline repartnering hazard) and is statistically significant at the 5 percent level.[19]

[19]Furthermore, Appendix Table 17 shows that there is no comparable announcement effect, and that the coefficients of interest are robust to the inclusion of announcement effect indicators.

These results are illustrated in Figures 5 and 6 for the administrative and HILDA samples, respectively. The figures plot the estimated logit hazard of repartnering by the age of the mother's youngest child at the time of separation. The baseline case is for mothers separating in the pre-reform period. The diamond-shaped points represent the pre-reform repartnering hazards by age of youngest child at the time of separation. Having independently controlled for the time-varying age of youngest child, the hazard of repartnering is lower the younger the child at the time of separation. The vertical distance between the red square and blue diamond lines represents the treatment effect of the reform by the age of the youngest child.

The graphs show that the treatment effect on the repartnering hazard is not uniform by the age of the youngest child: it is largest for mothers with a youngest child aged six to ten. These are the mothers who face the largest financial impact and who experience the loss in income most immediately. The policy reform has the biggest impact for mothers whose youngest child is eight years old as they lose the most and do not benefit from any adjustment time when receiving PPS. For mothers with older children, the impact declines as the age of the child increases, which translates to a fall in the number of years of payments lost. On the other hand, women with younger children have some adjustment time, perhaps to undertake search for a new partner or to retrain and improve their labour market position, as they continue to receive PPS until their youngest child turns eight, thus the impact on their repartnering hazard increases as the age of the child increases.

Although the treatment intensity story is broadly similar across the two samples, the effect sizes on the coefficients of interest are larger (in absolute value) in the HILDA sample. A mother's response to the intensity of treatment depends on whether she understands the policy effect, the extent to which she can anticipate the longer-term economic effects of repartnering, and her planning horizon. More importantly, it depends on her financial capacity to respond to these calculations. Thus, one reason the effects are stronger in the HILDA sample is because it contains mothers that are, on average, better educated and likely to be more forward-looking. Furthermore, the results from the HILDA data may be more likely to capture learning and time effects since the longer time frame of these data captures mothers' responses several years after the policy was implemented and they begin to 'age out' of PPS eligibility, as opposed to the narrower six-month window used in the administrative sample.

8 Conclusion

This paper has examined how a reduction in the financial resources available to lone parents affects their repartnering behaviour. A body of research has established that repartnering is a key mechanism for separating women to recover financially, so it is hypothesised that a reduction in the financial resources available to lone parents will increase their rate of repartnering. We exploit a natural experiment generated by the 2006 reform to the main income support payment for lone parents in Australia. This reform changed the eligibility criteria for the principle welfare payment for lone parents, creating a reduction in the financial resources available to a subset of separating parents.

Using biweekly administrative data capturing separations occurring among low and middle income couples, we show that the reform significantly increased the repartnering hazard for affected separating mothers. This effect was concentrated among mothers with low labour force attachment, and was primarily a reconciliation with the woman's prior partner. We complement these results with evidence from an annual panel survey, which allows us to examine the longer-term repartnering effects. We show that this increased repartnering hazard is present over the five years post-separation. We also show that these results are unlikely to incorporate offsetting changes due to changing selection into separation.

With both the administrative and HILDA datasets, we also examine the effect of the intensity of the impact of the policy reform: how many years of welfare payments are lost, and how far into the future does the loss occur. We show that the repartnering hazard is increasing in the number of years of payments lost, and decreasing in the number of years of payments that are retained. Together, these results demonstrate that one way that lone mothers respond to a reduction in financial resources available at the time of relationship breakdown is by repartnering more quickly.

These results raise a number of questions for the wellbeing of separating couples and their children. The rapid repartnering effects observed in the high-frequency administrative data may reflect marginal couples reconciling as the reduced financial resources no longer make separating the efficient choice. On the other hand, the increased rate of reconciliation may reflect vulnerable women being forced back into abusive living arrangements with significant consequences for the wellbeing of women and their children. Due to the short window of observations available in the administrative data, we are unable to examine the duration of these reconciliations. Previous evidence suggests that 77% of reconciliations in this population are for more than 30 weeks (Bradbury & Zhu 2012), so it is not clear that these reconciliations will necessarily be shortlived. A broader understanding of the implications of this repartnering will inform policy decisions about the appropriate level of welfare payments for lone parents.

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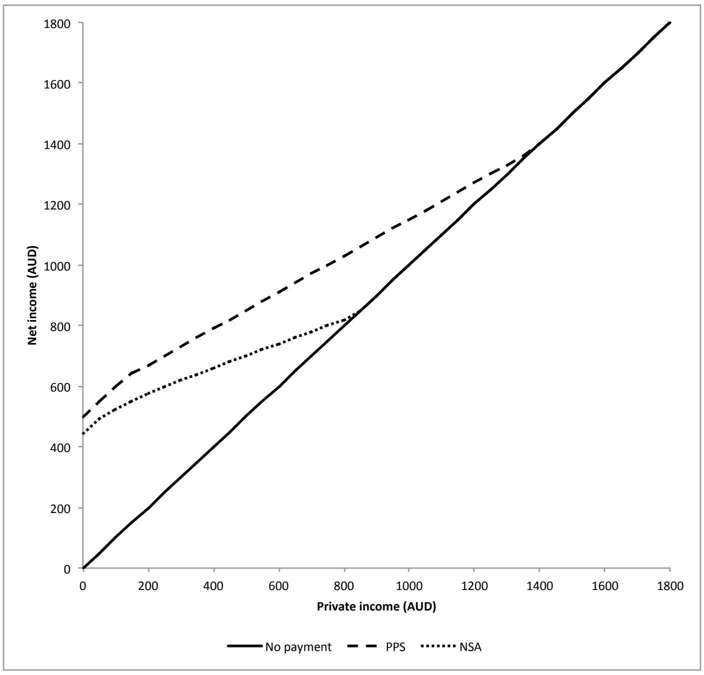

Figure 1: Structure of the Parenting Payment Single (PPS) and Newstart Allowance (NSA) on 1 July 2006

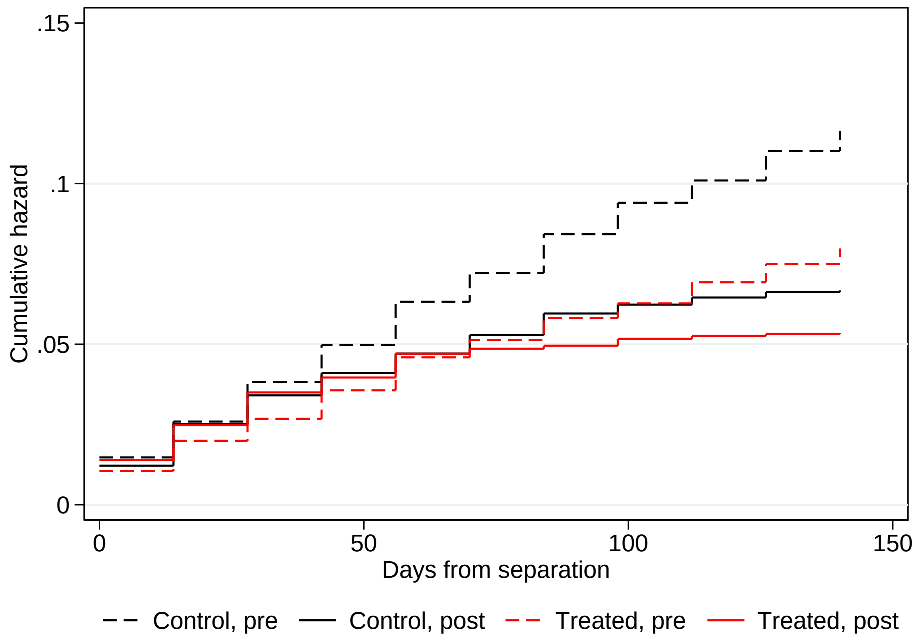

Figure 2: Cumulative Incidence Functions for repartnering

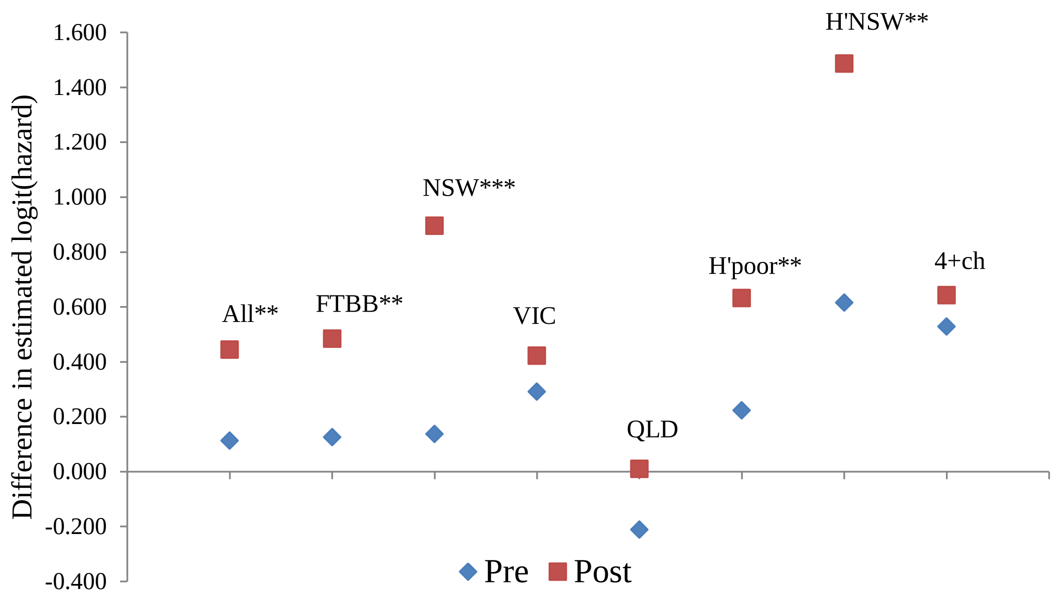

Figure 3: Treated effect, by post status, for various samples

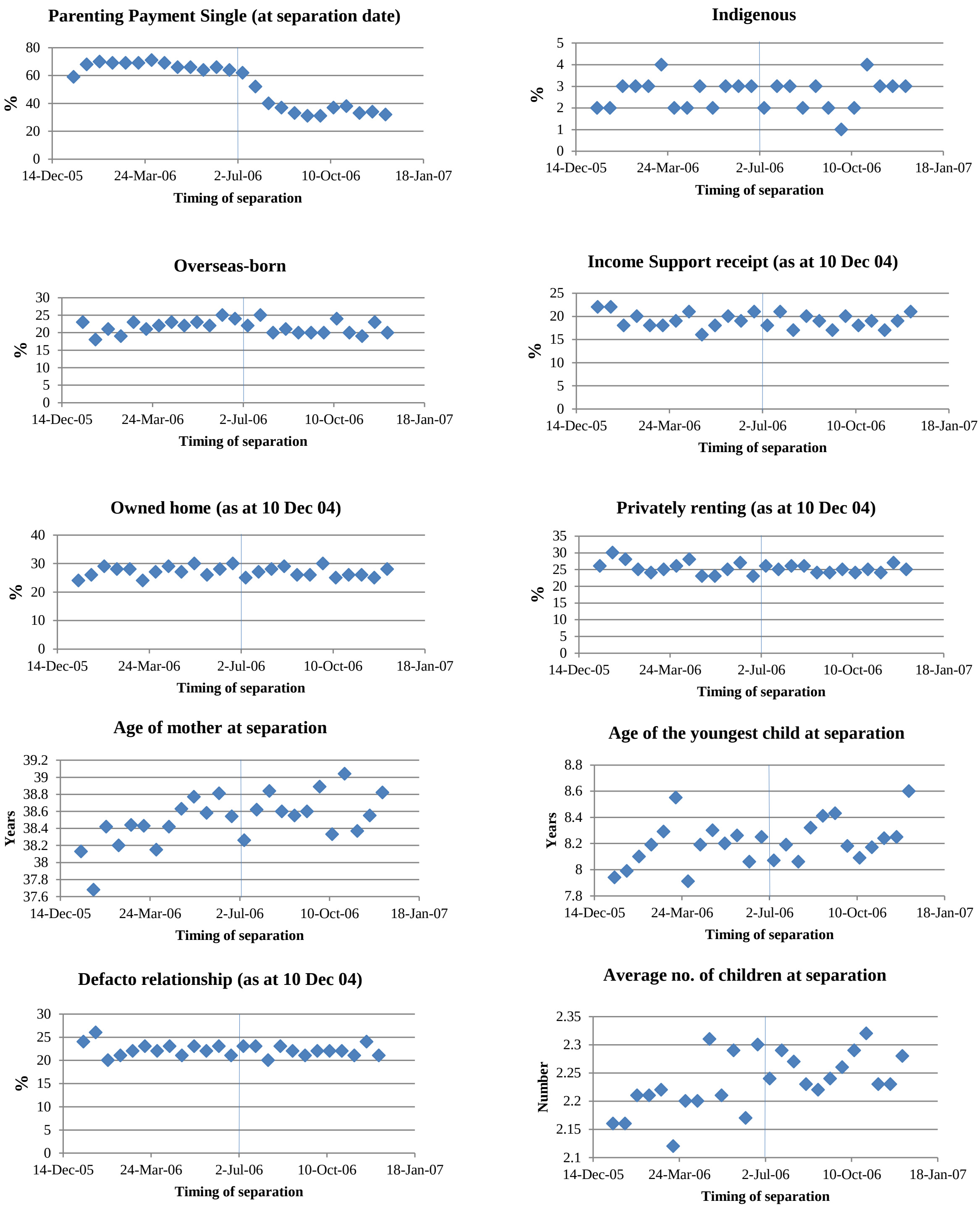

Figure 4: Variation in family and maternal characteristics, by timing of separation

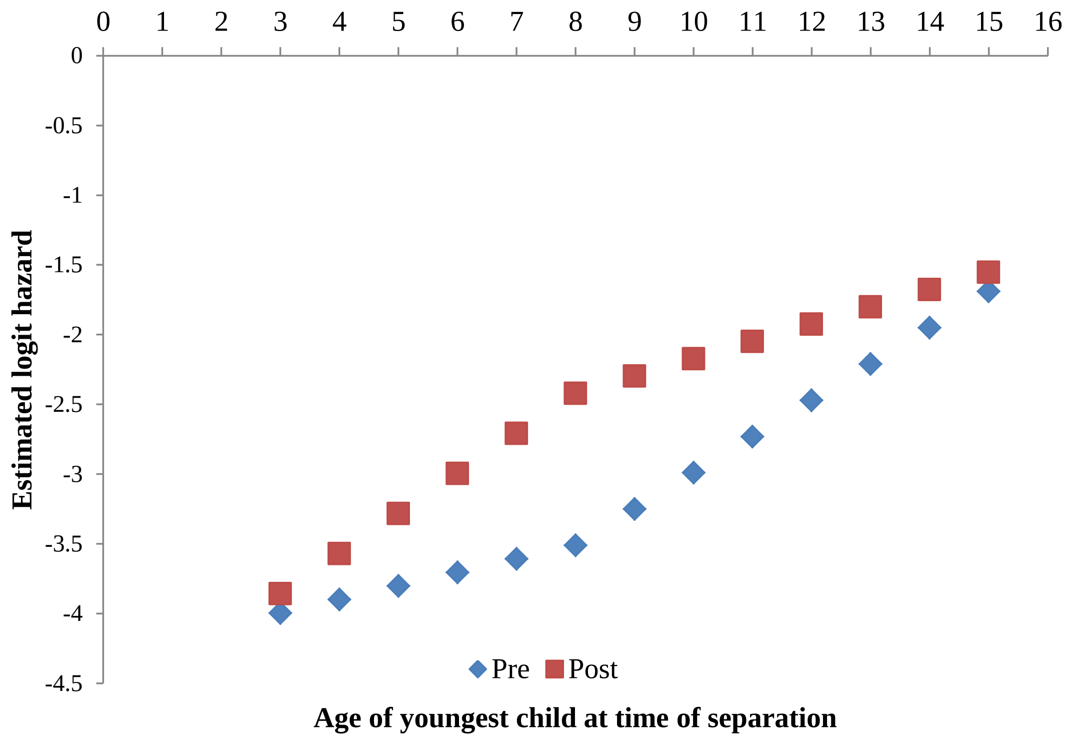

Figure 5: Estimated treatment effect, by age of youngest child at separation, (Admin)

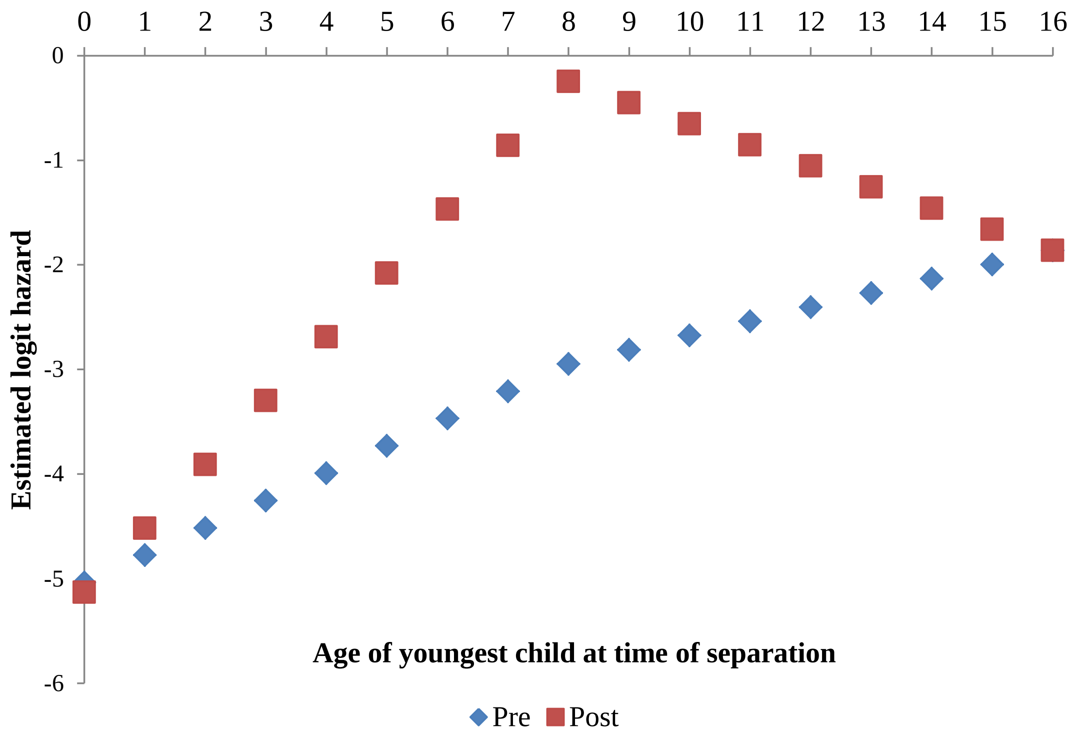

Figure 6: Estimated treatment effect, by age of youngest child at separation, (HILDA)

Table 1: Summary statistics before separation date: administrative sample

	Mean	StdDev
Defacto	0.22	0.42
Indigenous	0.03	0.16
Overseas born	0.22	0.41
Income support	0.24	0.42
Number of children		
one child	0.18	0.39
two children	0.49	0.50
three children	0.24	0.43
four or more children	0.08	0.28
Age of mother		
less than 30 years	0.07	0.25
30-34 years	0.20	0.40
35-39 years	0.31	0.46
above 40 years	0.42	0.49
Receives child maintenance	0.10	0.30
Owns home	0.24	0.36
Private rental market	0.30	0.46
Age of youngest child (years)	8.06	3.20
Observations	11,895	

Table 2: Summary statisitics before separation: HILDA samples

	Full Sample		Focused Sample	
	Mean	StdDev	Mean	StdDev
Defacto	0.35	0.48	0.33	0.47
Indigenous	0.05	0.21	0.06	0.23
Overseas born	0.19	0.39	0.16	0.37
Number of children	1.65	1.26	1.56	1.29
Age	38.19	10.55	39.78	10.28
Receives child maintenance	0.11	0.31	0.10	0.30
Owns home	0.66	0.47	0.64	0.48
Private rental market	0.29	0.45	0.31	0.46
Age of youngest child (years)	8.29	6.87	9.48	6.87
No children aged under 18	0.21	0.40	0.27	0.45
Gross household income ($000)	83.63	63.45	88.37	80.14
Bachelor degree or higher	0.17	0.37	0.22	0.41
Observations	708		176	

Table 3: Estimated Hazard of repartnering: administrative sample

	(1)	(2)
Treated	-0.347***	0.115
	(0.067)	(0.103)
Post	-0.342**	-0.027
	(0.167)	(0.837)
Treated*Post	0.296**	0.320**
	(0.133)	(0.133)
Characteristics before separation		
Children in household (base: one child)		
Two children		0.032
		(0.087)
Three children		0.198**
		(0.096)
Four or more children		0.428***
		(0.118)
Age category (base:less than 30 years)		0.000***
		(0.000)
30-34 years		-0.220**
		(0.111)
35-39 years		-0.325***
		(0.112)
Above 40 years		-0.456***
		(0.118)
Income support		0.282***
		(0.069)
Child maintenance		0.082
		(0.095)
Owns home		-0.177**
		(0.075)
Private rental market		-0.002
		(0.069)
Fixed characteristics		
Defacto		-0.041
		(0.072)
Indigenous		0.236
		(0.169)
Overseas born		0.092
		(0.072)
Intercept	-3.874***	-3.568***
	(0.131)	(0.212)
Mother observations	11,895	11,895
Mother-fortnight observations	140,468	140,468

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All the discrete hazard regressions include indicators for timing from separation (polynomial function of order three) and a flexible function for the baseline hazard (dummy variables for every fortnight). Column 2 presents regressions controlling for receipt of child maintenance, indicators for home ownership or being in the private rental market, being in a de facto relationship, indigenous status, being born overseas, age of the mother, income support status before separation, state of residence, month of separation, and time-varying regressors indicating the age of the youngest child.

Table 4: Heterogenous policy effects, by SES subgroups

	FTBB	NSW	VIC	QLD	H'poor	H'NSW	4+ ch
Treated	0.125	0.136	0.291	-0.211	0.223	0.615**	0.528*
	(0.121)	(0.192)	(0.215)	(0.214)	(0.146)	(0.274)	(0.319)
Post	-0.090	-1.936	1.157	-1.180	-0.218	-0.737	0.218
	(0.919)	(1.980)	(1.441)	(1.825)	(1.096)	(3.037)	(3.016)
Treated*Post	0.359**	0.759***	0.131	0.221	0.409**	0.872**	0.115
	(0.149)	(0.268)	(0.279)	(0.271)	(0.184)	(0.408)	(0.409)
Intercept	-3.367***	-3.183***	-3.164***	-3.731***	-3.402***	-2.650***	-3.210***
	(0.241)	(0.384)	(0.446)	(0.415)	(0.297)	(0.546)	(0.663)
Mother observations	6992	3986	3164	2760	6202	1761	1010
Mother-fortnight observations	76,913	43,851	34,807	30,363	66,561	19,320	11,106

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
FTBB refers to Family Tax Benefit - Part B and is an indicator of low to no labour market engagement of the mother; NSW, VIC and QLD refer to the States of New South Wales, Victoria and Queensland; H'poor refers to being housing poor (without an owned home, private rental or public housing); H'NSW refers to being housing poor in NSW and 4+ch refers to having four or more children. The discrete hazard regression includes indicators for timing from separation (polynomial function of order three), a flexible function for the baseline hazard (dummy variables for every fortnight), receipt of child maintenance, indicators for home ownership or being in the private rental market, being in a de facto relationship, indigenous status, being born overseas, age of the mother, income support status before separation, state of residence, month of separation, and time-varying regressors indicating the age of the youngest child.

Table 5: Estimated hazard of different exits from lone motherhood: administrative sample

		With controls
Post	Drop-out	0.903
		(2.116)
	Reconciliation	0.221
		(0.836)
	New Partner	-4.351
		(3.033)
Treated	Drop-out	0.196
		(0.200)
	Reconciliation	0.158
		(0.107)
	New Partner	0.290
		(0.317)
Treated*post	Drop-out	0.008
		(0.306)
	Reconciliation	0.295**
		(0.137)
	New Partner	0.390
		(0.664)
	Mother observations	11,895
	Mother-fortnight observations	140,468

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
The discrete hazard regression includes indicators for timing from separation (polynomial function of order three), a flexible function for the baseline hazard (dummy variables for every fortnight), receipt of child maintenance, indicators for home ownership or being in the private rental market, being in a de facto relationship, indigenous status, being born overseas, age of the mother, income support status before separation, state of residence, month of separation, and time-varying regressors indicating the age of the youngest child.

Table 6: Estimated hazard of different exits from lone motherhood: administrative sample

		With controls
Post	Drop-out	0.902
		(2.115)
	Reconciliation -on IS	-0.860
		(1.588)
	Reconciliation - not on IS	0.627
		(1.006)
	New Partner	-4.351**
		(3.033)
Treated	Drop-out	0.196*
		(0.200)
	Reconciliation -on IS	0.210***
		(0.184)
	Reconciliation - not on IS	0.136**
		(0.131)
	New Partner	0.290***
		(0.317)
Treated*post	Drop-out	0.007
		(0.306)
	Reconciliation -on IS	0.656***
		(0.249)
	Reconciliation - not on IS	0.128
		(0.164)
	New Partner	0.390
		(0.664)
	Mother observations	11,895
	Mother-fortnight observations	140,468

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
The discrete hazard regression includes indicators for timing from separation (polynomial function of order three), a flexible function for the baseline hazard (dummy variables for every fortnight), receipt of child maintenance, indicators for home ownership or being in the private rental market, being in a de facto relationship, indigenous status, being born overseas, age of the mother, income support status before separation, state of residence, month of separation, and time-varying regressors indicating the age of the youngest child.

Table 7: Sensitivity analysis: Hazard of repartnering (administrative sample)

	Pre: Jan - May 2005	Artificial censoring
Treated	0.245***	-0.334
	(0.078)	(0.298)
Post	2.691	6.043***
	(1.187)	(1.971)
Treated*Post	0.204*	0.619**
	(0.123)	(0.286)
Intercept	-4.037	-3.775***
	(0.188)	(0.405)
Mother observations	11,835	11,895
Mother-fortnight observations	277,121	74,264

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Column 1 applies an artificial censoring date for mothers who separated before July-2006 to be five months after the date of separation. Column 2 redefines control group mothers as separated between January-2005 and 10 May-2005. All columns include indicators for timing from separation (polynomial function of order three), a flexible function for the baseline hazard (dummy variables for every fortnight) and controls for receipt of child maintenance, indicators for home ownership or being in the private rental market, being in a de facto relationship, indigenous status, being born overseas, age of the mother, income support status before separation, state of residence, month of separation, and time-varying regressors indicating the age of the youngest child.

Table 8: Hazard of repartnering, triple difference specification

	With controls
Treated	0.159
	(0.099)
July	0.312
	(0.958)
Year 2006	-0.688
	(1.407)
July*Treated*Year2006	0.382***
	(0.128)
Intercept	-2.857
	(1.410)
Mother observations	34,302
Mother-fortnight observations	589,496

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
The discrete hazard regression includes indicators for timing from separation (polynomial function of order three), a flexible function for the baseline hazard (dummy variables for every fortnight), receipt of child maintenance, indicators for home ownership or being in the private rental market, being in a de facto relationship, indigenous status, being born overseas, age of the mother, income support status before separation, state of residence, month of separation, and time-varying regressors indicating the age of the youngest child.

Table 9: Changing selection into separation, HILDA sample

	Age of child		Treatment probability	
	(1)	(2)	(3)	(4)
Wave 6	0.363	-0.375	0.140	0.113
	(0.560)	(0.711)	(0.148)	(0.151)
Wave 7	0.400	-0.353	0.005	-0.019
	(0.731)	(0.797)	(0.065)	(0.069)
Wave 8	-1.616**	-2.065***	-0.044	-0.059
	(0.813)	(0.786)	(0.059)	(0.059)
Wave 9	-1.397**	-1.843***	-0.148***	-0.160***
	(0.662)	(0.666)	(0.043)	(0.044)
Wave 10	-0.327	-0.883	-0.011	-0.028
	(0.629)	(0.619)	(0.058)	(0.059)
Wave 11	-0.376	-0.950	-0.016	-0.034
	(0.709)	(0.704)	(0.061)	(0.060)
Wave 12	-0.793	-1.503**	-0.020	-0.039
	(0.799)	(0.760)	(0.062)	(0.062)
Wave 13	-1.951***	-2.709***	-0.120**	-0.142***
	(0.654)	(0.664)	(0.050)	(0.050)
Controls	No	Yes	No	Yes
Observations	708	708	708	708

Robust Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Indicators are for the wave of separation, conditional on the separation happening in the post-reform period. Columns (1) and (2) also include an indicator for no children. Controls are an indicator for high education, and a quadratic of pre-separation household income.

Table 10: Estimated hazard of repartnering, HILDA samples

	Full sample			Focused sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.244	0.305	-0.035	-1.447**	-0.885	-1.919***
	(0.265)	(0.346)	(0.375)	(0.628)	(0.750)	(0.706)
Post	-0.841	-1.011	-0.785	-1.363*	-1.237	-1.273
	(0.777)	(0.745)	(0.718)	(0.779)	(0.827)	(0.897)
Treated*Post	0.354	0.574	0.554	1.975**	1.709**	1.685*
	(0.405)	(0.403)	(0.409)	(0.822)	(0.848)	(0.902)
Characteristics before separation						
Children in household (base: all children have left household)						
One child		0.237			-0.208	
		(0.368)			(0.671)	
Two children		0.034			-0.705	
		(0.375)			(0.738)	
Three children		-0.182			-1.573*	
		(0.437)			(0.893)	
Four or more children		0.127			-0.609	
		(0.479)			(0.747)	
Number of children			-0.141			-0.309*
			(0.089)			(0.180)
Age category (base: less than 30 years)						
30-34 years		-0.590**			-0.593	
		(0.289)			(0.554)	
35-39 years		-0.581**			-0.330	
		(0.289)			(0.523)	
Above 40 years		-1.787***			-1.113	
		(0.363)			(0.818)	
Age			0.082			0.174
			(0.071)			(0.112)
Age^2			-0.002**			-0.003*
			(0.001)			(0.002)
Household income ($000)			0.003			0.002
			(0.004)			(0.008)
Household income^2 ($000)			-0.000			-0.000
			(0.000)			(0.000)
Bachelor degree or higher			-0.206			0.628*
			(0.255)			(0.347)
Time-varying characteristics						
Age of youngest child		-0.031	-0.127		-0.004	-0.383**
		(0.049)	(0.100)		(0.086)	(0.155)
Age of youngest child^2			0.008			0.026***
			(0.006)			(0.010)
Mother observations	708	708	708	176	176	176
Mother-wave observations	2463	2463	2463	706	706	706

Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All columns include a set of time-since-separation indicators to capture the baseline hazard. Columns (1) and (4) include controls for wave of separation only. Columns (2) and (5) also include controls comparable to the short-term analysis: indicators for de facto, indigenous, born overseas, child maintenance receipt, and housing tenure. Also included are time-varying indicators for being ineligible for PPS under the old and new rules. Columns (3) and (6) include the same demographic indicator variables.

Table 11: Estimated effect of the intensity of treatment on repartnering, administrative sample

	(1)	(2)
Post	-1.429	-0.541
	(0.679)	(0.857)
Years of receipt lost	-0.260	-0.210
	(0.012)	(0.015)
Years of receipt lost*Post	0.136**	0.067**
	(0.038)	(0.032)
Years of receipt remaining	-0.097	-0.150
	(0.019)	(0.027)
Years of receipt remaining*Post	-0.190**	-0.149***
	(0.046)	(0.048)
Mother observations	11,895	11,895
Mother-wave observations	140,468	140,468

Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All columns include a set of time-since-separation indicators to capture the baseline hazard. Other controls as in columns (1) to (2) of Table 3.

Table 12: Estimated effect of the intensity of treatment on repartnering, focused HILDA sample

	(1)	(2)	(3)
Post	-1.397	-1.737*	-1.862*
	(0.936)	(0.958)	(1.122)
Years of receipt lost	-0.036	-0.261	-0.136
	(0.076)	(0.179)	(0.162)
Years of receipt lost*Post	0.224*	0.349***	0.337**
	(0.120)	(0.134)	(0.138)
Years of receipt remaining	0.123	-0.121	-0.229
	(0.076)	(0.133)	(0.157)
Years of receipt remaining*Post	-0.296**	-0.396***	-0.349**
	(0.135)	(0.152)	(0.147)
Mother observations	176	176	176
Mother-wave observations	706	706	706

Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All columns include a set of time-since-separation indicators to capture the baseline hazard. Other controls as in columns (1) to (3) of Table 10.

A Administrative data description

Nearly all benefits in Australia are administered at the national level through one central agency known as Centrelink. The data used in this paper are based on biweekly Centrelink welfare and social benefit records from the period June 2001 to December 2006. The data are derived from payment records for Family Tax Benefit (FTB).

From the population of FTB recipients, our base sample extracts all mothers who were partnered (married or de facto) at 15 June 2001 and then subsequently reported a relationship separation to Centrelink between June 2001 and December 2006. For these mothers, we have information on their own as well as their partners' family and income support payments for every two-week period between June 2001 to December 2006. All mothers report their relationship status as this affects the calculation of the amount of FTB received. Within the FTB system there are financial incentives to report separation and so qualify for a higher level of FTB payments. Recipients are required to report any change of circumstances (such as to partnership or income) within 14 days of the change.

Given the timing of the reform, our main estimation sample consists of women who we observe separating at some point during 2006. We begin by restricting our sample to mothers who were partnered on 10 December 2004.[20] We exclude families where the father was the main recipient of FTB (i.e. the primary carer) at any point during this time, families with parents eligible for the Age Pension (aged above 65 for men and 60 for women), and those who die at some point during the analysis window. Also, we restrict attention to mothers who had at least one child aged under 16 in their care throughout the analysis period. Once the youngest child ages beyond 16, all mothers lose eligibility to PPS, thus it is important to eliminate any families who would have aged out of the eligibility requirements for PPS at any point during the analysis window. We further restrict our sample to exclude mothers with very young children (below the age of two at any point throughout the analysis window). This is to ensure that we have a more comparable control group. Together, these sample restrictions reduce the sample size to 37,945 partnered mothers as at 10 December 2004.

We focus on the mothers who separated between January 2006 and December 2006. This decision is driven by the timing of the reform relative to the data available. Since our data

[20] As mentioned above, the initial data extraction was based on mothers who were partnered on 15 June 2001. We redefine our starting sample to be mothers who were partnered at 10 December 2004, which is in keeping with the initial sample design. However, instead of solely analysing mothers who stayed continuously partnered (from 2001) until the end of 2005 (and subsequently separated in 2006), we also analyse mothers who experienced a spell of separation between 2001 and 2004 but had repartnered by 10 December 2004. This start date is an arbitrary choice and variations in the starting date bring little change to the results presented in the paper.

ends in December 2006 we have a maximum window of 6 months to observe those who separate after the reform is implemented: the pre-reform separations selected represent a comparable length of time. Moreover, comparing mothers who separate within a narrow time frame helps to minimise bias arising from endogenous entry into the first spell of separation (or subsequent repartnering) during different macroeconomic conditions. These restrictions result in a sample of 11,895 mother-observations and 141,286 mother-fortnight observations.

The linking of administrative records across time relies on the identity management procedures of Centrelink. Identity management is a central function of the income support payment system as it is required to ensure that people do not receive multiple payments, and that the income and asset tests associated with most payments can be accurately managed. Hence we are confident that the data here does contain accurate information on patterns of payment receipt over time. The administrative data, however, does not contain any information for periods where people are not receiving any payment.

Imputations for temporary drop outs

We impute values for missing information due to mothers temporarily dropping out of the system. This arises because mothers fail to report required information to Centrelink. For these periods, we do not observe any other information about the mother. To avoid misinterpreting these short drop outs as intentional changes in the mothers partnership status or as welfare exits, we impute values for these missing records.

For mothers who drop out from the Centrelink system for two fortnights or less while we observe a change in their partnership status, we impute the missing values for the partnership status variable with the partnership status proceeding the temporary drop out. For mothers who lose contact with the Centrelink system for two fortnights or less within a continuous spell of separation or partnership, we impute the missing values for the partnering status variable, assuming that these mothers are continuously separated or partnered. We also impute values using the above rule for the receipt of PPS, other income support payments as well as for all the other key variables used in subsequent regression analysis.

Attrition: Mothers who leave the family payment system

A group of mothers drop out of the family payment system for a period of time extending beyond the short spells described above (around 2.9 per cent of the sample). We do not impute values for these periods of missing data because they are unlikely to arise from administrative or reporting-related reasons.

All lone parents are eligible for FTB, but couples with high incomes are not. So one reason for leaving the family payment system is that the mother partners with a high-income man. Other explanations, however, can apply while the mother remains single. These include mothers losing custody of the child to either a grandparent or to the partner; the mother retaining custody but no longer being the family payment recipient (as might happen in new blended families); incarceration; moving overseas; or choosing to receive family payments on an annual rather than regular basis. Finally, these dropouts might be due to people not filling in the required paper work when their family status changes (particularly when they are only eligible for the lowest rate of payment).

These drop-out cases are treated as censored cases in the basic hazard model. When we conduct competing risk analysis, we treat these drop out observations as an exit (as a separate exit state).

B Tables

Table 13: Probability of PPS receipt: administrative sample

	(1)	(2)
Treated	-0.042***	-0.047***
	(0.009)	(0.014)
Post	0.122***	0.169*
	(0.020)	(0.086)
Treated*Post	-0.448***	-0.451***
	(0.015)	(0.015)
Mother observations	14,759	14,759
Mother-fortnight observations	191,762	191,762

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
All columns include indicators for timing from separation (polynomial function of order three). Column 2 also controls for receipt of child maintenance, indicators for home ownership or being in the private rental market, being in a de facto relationship, indigenous status, being born overseas, age of the mother, income support status before separation, state of residence, month of separation, and time-varying regressors indicating the age of the youngest child.

Table 14: Probability of PPS receipt: HILDA sample

	Full sample			Focused sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.084*	0.094**	0.091**	-0.111	0.045	0.070
	(0.043)	(0.043)	(0.039)	(0.069)	(0.067)	(0.059)
Post	-0.065	0.018	0.054	-0.128	-0.051	0.017
	(0.089)	(0.059)	(0.058)	(0.081)	(0.057)	(0.059)
Treated*Post	-0.195***	-0.190***	-0.191***	0.000	-0.099	-0.148**
	(0.054)	(0.047)	(0.046)	(0.089)	(0.073)	(0.074)
Controls						
Wave of separation	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	No	Yes	Yes
Income and education	No	No	Yes	No	No	Yes
Observations	2788	2788	2788	828	828	828
Individuals	708	708	708	176	176	176

Standard errors clustered at the individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Columns (1) and (4) include controls for wave of separation only. Columns (2) and (5) include controls comparable to the short-run analysis: indicators for de facto, indigenous, born overseas, and child maintenance receipt; a set of indicators for age categories, number of children and housing tenure. Also included are time-varying indicators for being ineligible for PPS under the old and new rules, and the age of the youngest child in the household. Columns (3) and (6) include the same demographic indicator variables, linear and quadratic terms in age at separation, number of children at separation, linear and quadratic terms in previous household income, and an indicator for holder a Bachelor degree. These columns also add a quadratic in the age of the youngest child to the time varying regressors.

Table 15: Estimated hazard of repartnering, excluding children aged 7 and 8. Focused HILDA sample

	(1)	(2)	(3)
Treated	-1.445**	-0.402	-1.536**
	(0.631)	(0.804)	(0.732)
Post	-1.822*	-1.826**	-1.826
	(1.038)	(0.857)	(1.162)
Treated*Post	1.990**	2.084**	1.756*
	(0.853)	(0.925)	(0.926)
Controls			
Wave of separation	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Income and education	No	No	Yes
Mother observations	162	162	162
Mother-wave observations	653	653	653

Standard errors clustered at the individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls as in columns (4) to (6) of Table 10.

Table 16: Estimated hazard of repartnering, including announcement effect. Focused HILDA sample

	(1)	(2)	(3)
Treated	-1.177	-0.668	-1.669**
	(0.783)	(0.883)	(0.804)
Post announcement	-13.231***	-14.266***	-13.717***
	(0.493)	(0.563)	(0.585)
Post implementation	-1.387*	-1.235	-1.275
	(0.777)	(0.825)	(0.896)
Treated*Post announcement	-0.642	-0.552	-0.624
	(1.325)	(1.319)	(1.317)
Treated*Post implementation	2.344**	2.028*	2.059
	(1.190)	(1.146)	(1.255)
Controls			
Wave of separation	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Income and education	No	No	Yes
Mother observations	176	176	176
Mother-wave observations	706	706	706

Standard errors clustered at the individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls as in columns (4) to (6) of Table 10.

Table 17: Estimated effect of the intensity of treatment, including announcement effect. Focused HILDA sample

	(1)	(2)	(3)
Years of receipt lost	0.035	-0.165	-0.057
	(0.091)	(0.195)	(0.179)
Years of receipt remaining	0.080	-0.153	-0.255
	(0.087)	(0.138)	(0.158)
Post announcement	-12.386***	-13.573***	-13.705***
	(0.785)	(0.824)	(0.863)
Years of receipt lost*Post Announcement	-0.225	-0.249	-0.231
	(0.176)	(0.185)	(0.196)
Years of receipt remaining*Post Announcement	0.181	0.161	0.138
	(0.169)	(0.186)	(0.180)
Post implementation	-1.648*	-2.000**	-2.107*
	(0.949)	(0.963)	(1.102)
Years of receipt lost*Post Implementation	0.378**	0.506***	0.485**
	(0.177)	(0.181)	(0.192)
Years of receipt remaining*Post Implementation	-0.433**	-0.532***	-0.466**
	(0.184)	(0.188)	(0.191)
Controls			
Wave of separation	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Income and education	No	No	Yes
Mother observations	176	176	176
Mother-wave observations	706	706	706

Standard errors clustered at the individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls as in columns (4) to (6) of Table 10.