

THE EFFECT OF CHANGING FINANCIAL INCENTIVES ON REPARTNERING*

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Short title: Financial incentives and repartnering

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 biweekly administrative data capturing separations occurring among low and middle income couples, we show that the policy reform significantly increased the speed of repartnering for affected separating mothers. The 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: J12, J18, H53

Keywords: Repartnering, lone parents, relationship breakdown

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Relationship breakdown is a common experience among families with children. More than a quarter of Australian children experience parental separation before age 18 (de Vaus and Gray 2003), and this figure is as high as 44 percent in the United States (Andersson, Thomson and Duntava 2017). Divorce and separation result in large, sudden reductions in household income, particularly for mothers, and can have persistent negative impacts, including poverty, higher rates of mental health problems, and lower well-being (Amato 2010; Osborne, Berger and Magnuson 2012). Over half of mothers experiencing relationship breakdown repartner within five years,¹ and this is an important recovery mechanism for household income (Fisher and Low 2016) and wellbeing (Amato 2010; Osborne, Berger and Magnuson 2012). However, the effect of financial incentives on the repartnering process is not well understood, and in particular there is a lack of consensus about the influence of welfare payments (Moffitt, Phelan and Winkler 2015).²

This paper examines how a welfare reform in Australia affected the repartnering rate of newly separated mothers. We use biweekly administrative data for 10,828 separating mothers, and estimate the effects of the reforms using a Regression Discontinuity Design (RDD). Welfare programmes are an important source of insurance for low-income mothers against the initial financial shock of separation; however little is known about how reducing that level of insurance affects how long mothers spend as lone parents. This is an important question as the amount of time a mother spends in lone-parenthood can affect her long-term wellbeing and that of her children (Amato 2010; Lee and McLanahan 2015; Zissimopoulos, Karney and Rauer 2015).

We show that a reduction in welfare payments increases the speed of repartnering for newly separated mothers. We exploit a natural experiment that unambiguously reduced government transfer payments for a subset of lone parents. In 2006, Australia implemented a suite of welfare reforms that included removing eligibility for the Parenting

¹Based on authors' calculations using HILDA Survey data.

²There is a broader body of research examining how policy affects partnering decisions. This includes how financial incentives in personal income tax systems affect marriage, cohabitation and divorce decisions (Whittington and Alm 1997; Dickert-Conlin 1999; Herbst 2011; Fisher 2013; Michelmore 2018), and how aspects of divorce law are linked to relationship formation and dissolution decisions (Matouschek and Rasul 2008; Halla 2013; Walker and Zhu 2006).

Payment Single (PPS) for newly separated parents with a dependent child aged eight to fifteen, offering a strictly lower payment in its place. This change reduced lone mothers' household income by up to 17 percent. Parents separating before the reform's implementation were grandfathered under the old eligibility rules, retaining access to the higher payment until all dependent children turned 16, and so are a natural comparison group.

Our RDD analysis compares the repartnering outcomes of mothers separating immediately before and immediately after 1 July 2006 when the reform was implemented. We find that the reform increased the six-month repartnering rates of affected mothers by six percentage points, representing a 64 percent increase. These effects are persistent, with repartnering rates higher by eight percentage points (25 percent) four years after separation. The increase in repartnering is highest for Australian-born mothers and those with a record of prior income support receipt.

These results contribute to a growing international literature examining the impact of welfare policy on relationship status. Much of this literature has evaluated the impact of US welfare reform on family structure, with mixed conclusions (Grogger and Karoly 2005; Moffitt, Phelan and Winkler 2015). These studies typically exploit variation in the timing and structure of welfare reform across states, capture the impact of a bundle of different welfare reforms, and focus on transitions to and from legal marriage³ (Acs and Nelson 2004; Bitler, Gelbach, Hoynes and Zavodny 2004; Fitzgerald and Ribar 2004; Bitler, Gelbach and Hoynes 2006; Dunifon, Hynes and Peters 2009). For the UK, Francesconi and Van der Klaauw (2007), Anderberg (2008) and Francesconi, Rainer and Van Der Klaauw (2009) show that changes in in-work benefits and income support significantly affected separation and partnering decisions. In other parts of Europe, González (2007) shows that benefit levels and single motherhood are positively correlated.

Our results advance this literature for three key reasons. First, we isolate the impact of changes in financial incentives. An important feature of the Australian reform is that

³In contrast to the US literature we consider marriage and informal cohabitation: remarriage is less likely in the short run due to the required legal arrangements, and cohabitation is an increasingly important household structure in Australia and elsewhere (Kennedy and Bumpass 2008; Buchler, Baxter, Haynes and Western 2009). This is in line with the UK and European literature.

it unambiguously increased the financial incentives to repartner. In comparison, previous studies tend to evaluate the combined effects of a package of reforms. For example, most US studies focus on the 1996 PRWORA reforms which included time-limits, earnings disregards, work search requirements, and sanctions. Similarly, the UK reforms from 1993 to 2003 covered changes to tax credits (in-work benefits) and to income support payments, and significantly increased work incentives for lone parents (Francesconi and Van der Klaauw 2007). Where these components have reinforcing or offsetting effects it is difficult to distinguish the effect of any one component (Blundell 2000; Anderberg 2008).⁴

Second, the natural experiment we study enables us to implement an RDD approach to identify causal effects. The Australian reform grandfathered a group of mothers under the old rules based on the date of separation, giving a control group of otherwise similar women. This means that unlike the US literature we do not rely on policy variation between states and over time, which could be endogenously determined. Our approach also improves upon the UK literature that uses childless women as a comparison group for affected mothers since the presence of children has been shown to be a barrier to repartnering (Wu and Schimmele 2005).

Finally, our biweekly administrative data allow us to observe transitions from separation to repartnering, including short-term transitions that would not be captured in annual survey data. We know that exits from income support payments are a result of repartnering as our data are not limited to income support recipients and instead includes all households receiving family payments. This includes both low- and middle-income households, and covers roughly 75 percent of Australian families with children (Bradbury and Zhu 2012).

The breadth and frequency of the data mean we can examine the impact of the reform on repartnering for different lengths of time since separation and so assess changes in

⁴A more recent literature has estimated the impacts of individual welfare components such as time-limits (Low, Meghir, Pistaferri and Voena 2018) or work participation requirements (Avram, Brewer and Salvatori 2018). We contribute to this more recent literature because we look at a different welfare component (changes to payment levels) and focus on its impact on the outcome of repartnering.

the speed of repartnering. In contrast, the existing literature typically examines effects either on the stock of lone parents and couples at a point in time, or on transitions into relationships from the stock of lone parents without reference to the length of time spent single.

The paper proceeds by first outlining key features of the Australian welfare system and the 2006 reforms, before outlining a conceptual framework of the repartnering process. Section 3 describes the administrative dataset used, and Section 4 outlines our main RDD empirical strategy. Sections 5 and 6 present our results from the administrative and survey datasets, and Section 7 concludes.

1 Institutional background

Australia has a suite of means-tested income support payments for working age adults. Here, we outline the key payments. Low-income parents with dependent children were, prior to 2006, paid either the Parenting Payment Single (PPS) or Parenting Payment Partnered (PPP) depending on their relationship status.⁵ Other important payments are Newstart Allowance (NSA), the primary payment for the unemployed, and Disability Support Pension (DSP) that is paid to those unable to work due to mental or physical disability.⁶ All payments are highly targeted, with spending on social protection at 28.2% of GDP in 2015 (OECD 2017).⁷

A major part of the 2005-06 budget was the ‘Welfare-to-Work’ reforms. These altered eligibility for some income support payments and introduced participation requirements for some recipients. The stated objective of the reforms was to increase labour force participation and to reduce welfare dependency. The budget was announced on 10 May

⁵A woman is not required to be married to be deemed ‘partnered’.

⁶Additional payments include Carer Payment, paid to those caring for a severely disabled/ill person or an adult who is frail and old.

⁷This compares to 20.8% for the United States, 38.4% for the United Kingdom, and 43.1% for Germany. The figures include spending on payments for the elderly and on housing, as well as payments to working age adults.

2005, with the required legislation⁸ introduced on 9 November, which eventually passed on 6 December 2005.⁹ The reforms were implemented on 1 July 2006. We now set out the key features of the reform for lone parents, partnered parents, and the disabled.

Parenting Payment Single (PPS)

Prior to 1 July 2006, lone parents with a youngest child aged 15 or under were eligible for the Parenting Payment Single (PPS). In 2006, the PPS maximum payment was \$499.70 (AUD) per two weeks, with a taper rate of 40% after an income of \$128.¹⁰ The reforms did not change the structure of PPS, but changed the eligibility criteria and introduced participation requirements. Eligibility was restricted to lone parents whose youngest child was aged under eight years old.¹¹ Lone parents with older children would instead be eligible for ‘enhanced’ Newstart Allowance (NSA). This payment was less generous, with a maximum payment of \$444.20 per fortnight, a lower allowable income of \$62, and higher taper rates of 50% for income between \$62 and \$250, and 60% for income over \$250 per fortnight.

Figure 1 illustrates the difference between these two payments, and shows that affected mothers were made unambiguously worse off by this change and experienced an increase in their effective marginal tax rate (Harding, Vu, Percival and Beer 2005). 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 per year saw disposable income fall by 17%.

The reforms also introduced part-time participation requirements for all lone parents with a youngest child aged six or more. These parents were required to engage in 15 hours of employment, education or job-seeking activities every week. Parents could refuse

⁸Employment and Workplace Relations Legislation Amendment (Welfare to Work and Other Measures) Bill 2005 (Cth).

⁹Details of the process are set out at https://www.aph.gov.au/Parliamentary_Business/Bills-Legislation/Bills_Search_Results/Result?bId=r2461.

¹⁰The allowable income, before the taper applied, increased by \$24.60 for each additional child.

¹¹The original budget announcement on 10 May stated this cut-off would be six years old, but this was amended in the Bill introduced on 9 November with no prior announcement.

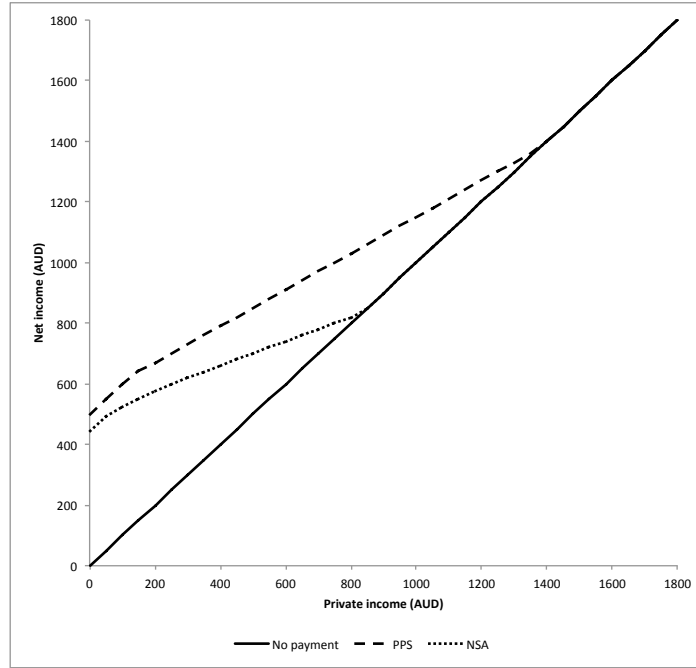


Fig. 1. *Structure of the Parenting Payment Single (PPS) and Newstart Allowance (NSA) on 1st July 2006*

Notes: Diagram shows PPS and NSA payment rates as a function of earned income for a lone mother with one child. Mothers separating before 1 July 2006 with a child aged 8-15 are eligible for the higher PPS payment; those separating after are strictly worse off receiving the lower NSA payment.

employment if the costs of required childcare made the job financially unviable or if the job required a commute of more than 60 minutes. Prior to the reforms, PPS recipients with children aged over 11 were required to engage in around 6 hours of activity per week.¹²

These reforms affected mothers separating from 1 July 2006.¹³ The eligibility and participation requirement changes together define three groups of new lone mothers according to the age of their youngest child. Mothers with a very young child were unaffected by the reform until their youngest child turned six; mothers with a child aged six or seven were subject to participation requirements but remained eligible for the higher PPS payment;

¹²We examine the effect of participation requirements in Section 5.4.2.

¹³A number of exemptions from the payment cuts and participation requirements were also introduced: foster carers and home-schooling mothers were exempt from participation requirements and paid NSA at the PPS rate. The criteria for eligibility for Carer Payment were also expanded to include parents with a severely disabled child, exempting these lone mothers from the effects of the reforms. Lone parents with 4 or more children were also exempt from participation requirements. Our data does not contain sufficient observations to examine the behaviour of these groups separately.

and mothers with a child aged 8 to 15 faced both the lower payment and participation requirements.

Those already receiving PPS before 1 July 2006 were grandfathered under the prior rules: they were eligible to receive the higher PPS payment until their youngest child reached the age of sixteen or when their eligibility stopped for some other reason (for example due to repartnering).¹⁴ These grandfathered lone parents were, however, subject to the new participation requirements from the later of 1 July 2007 or when their youngest child turned seven.

Parenting Payment Partnered (PPP)

PPP was also affected by the 2006 reforms. Eligibility was restricted to low-income parents with a youngest child aged under six, with partnered parents with older children instead being eligible for NSA. However, payment rates, income thresholds and taper rates are the same for PPP and NSA, so the reform did not change the financial resources of partnered parents differentially based on their child's age. In 2006 the maximum payment for a partnered mother was \$370.50. The reforms reduced the taper rate for both PPP and NSA from 70% to 60%, applying to all recipients from 1 July 2006 regardless of when receipt started. PPP recipients were subject to the same participation requirements as PPS recipients.

Disability Support Pension (DSP)

The 2006 reforms also affected eligibility for the DSP. Prior to 1 July 2006, the DSP was paid to those with an impairment preventing them from working for 30 hours per week. Post-reform, any applicants deemed able to work for 15 to 30 hours per week were instead eligible for the less generous NSA payment and expected to work for at least 15 hours per week. These changes applied to all new applicants from 1 July 2006, and were also implemented for post-announcement applicants at their first review, occurring from two

¹⁴In Section 5.4.1 we show that our results are not driven by grandfathered mothers being less likely to repartner.

to five years after application. Those receiving DSP before the reform’s announcement continued under the previous 30 hour rule.

In Section 5.2 we show that there is no evidence of programme substitution from PPS to DSP in response to these reforms. Our analysis therefore, focuses on the changes to eligibility for the Parenting Payment Single.

We are not the first to evaluate the 2006 Australian welfare reforms. Brady and 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 and Breunig (2014) demonstrate that lone mothers increased their labour force participation rates in response to these reforms. Fok and 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, 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 and Thapa 2008).

2 Financial incentives and repartnering decisions

The effect of welfare payments on the repartnering process can be captured in a theoretical framework where separated mothers search for a new partner, forming a new relationship when the expected payoff exceeds the expected return from continued search. In online Appendix A, we set out a dynamic model of relationship breakdown and repartnering that draws on the search model of marriage proposed in Rasul (2005) and the marital dissolution model of Weiss and Willis (1997). We assume efficient within-relationship behaviour in the spirit of Blundell, Chiappori and Meghir (2005), and that relationship

breakdown is efficient, occurring due to negative shocks to match quality.¹⁵ Here, we explain the model's key empirical predictions.

After relationship breakdown, a mother exerts effort in search for a new partner. She forms a new relationship if she meets a potential partner satisfying her reservation match quality, which is determined where the expected payoff to continued search is equal to the expected payoff from entering a relationship (including the risk of that relationship subsequently breaking down). Within this framework, a reduction in welfare payments affects the probability of repartnering through its effect on reservation match quality and search effort.

When welfare payments fall, a separated mother's reservation match quality also falls through two mechanisms. First, the value of continued search falls, conditional on not finding an acceptable partner. Second, the expected value of entering a relationship also falls. This is because the mother receives a lower within-relationship payoff due to her worse outside option, and faces a lower payoff if the relationship breaks down and she once again searches for a new partner. In general, the value of continuing to search falls by more than the expected value of entering a relationship, and so when welfare payments decrease, reservation match quality falls.

Search effort is determined by the mother's expected relative return from entering a relationship, which increases when welfare payments fall. In addition, as reservation match quality falls, a mother is more likely to meet an acceptable partner. Both mechanisms lead to an increase in the mother's expected gains from search, so when welfare payments decrease, search effort increases.

The reduction in reservation match quality makes meeting an acceptable partner more likely, and the increase in search effort increases the rate of meeting new partners. Together, this gives our main empirical prediction: *when welfare payments decrease, the probability of repartnering increases.*

¹⁵The model abstracts from household production, labour supply and fertility decisions to highlight the key empirical predictions.

The reduction in reservation match quality implies that the marginal new relationship will be of lower quality. However, it is not clear that this translates into a shorter expected relationship duration. In this framework, a relationship breaks down if there is a negative shock to match quality that results in no relationship surplus. Holding match quality constant, lower welfare payments imply a larger relationship surplus, and so a larger negative shock is needed to induce a breakdown. It therefore remains an empirical question whether relationships formed after welfare payments have fallen will be more or less stable than under the previous regime.

This framework is focused on how a reduction in welfare payments changes repartnering behaviour *for mothers who have separated*. These predictions are relevant for the empirical analysis presented below: our RDD analysis is based on the assumption that there is no change in selection into separation, and we present a range of tests of this assumption. However, the reduction in welfare payment is also expected to change the relationship breakdown process and so change selection into separation. We discuss the changing selection into separation in online Appendix A.

3 Administrative data

We use an administrative dataset of biweekly government payment records containing information on all income support and family payments received. The dataset also includes the demographic information required to administer these payments, including relationship status and the presence and age of all children in the household. All individuals and their partners have a unique identifier across the dataset, meaning we can observe relationship transitions on a biweekly basis.

Our analysis sample is drawn from an underlying dataset of government payment records for the mothers of a cohort of children born between October 1987 and March 1988.¹⁶

¹⁶This sample is drawn from the Transgenerational Data Set (TDS), which includes all contacts of this cohort of children and any adults receiving payments on their behalf with the social security system from

We observe full relationship transitions for these mothers when they are receiving Family Tax Benefit (FTB). 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 and Zhu 2012). This means that we capture the full relationship transition of mothers who were in both low and middle income households (receiving no income support, but receiving family tax 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 PPS when a lone mother and so will be affected by the 2006 reform. However, our data does not capture the relationship transitions of high income households.¹⁷

We select mothers who are observed separating from their partner between 1 July 2001 and 1 July 2011, and who are caring for a child aged eight to 15 years at the time of separation. For mothers who experience more than one separation over this period we select the first separation. This isolates the population potentially affected by the 2006 PPS reforms. Since the children in the underlying 1987-88 cohort dataset are aged 18-20 in 2006, it is almost always the presence of a younger sibling that means their mother satisfies the sample selection criteria.¹⁸ Online Appendix B provides a detailed description of the underlying dataset and our sample selection.

Our observation window extends to 1 January 2013.¹⁹ This means that we observe relationship status for between 18 months and 11.5 years post-separation. For example, a mother separating on 1 July 2011 is observed for 18 months after she separates; a mother separating on 1 July 2001 has 11.5 years of post-separation information. Mothers

1993 to early 2014. Over 98% of children born October 1987 to March 1988 are in the administrative data at some stage (Breunig, Cobb-Clark, Gørgens, Ryan and Sartbayeva 2009).

¹⁷For 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.

¹⁸The exception is for mothers separating before March 2004, where the dataset cohort child was aged 15 or under at the time of separation. These observations are not used in our preferred results.

¹⁹At this date, the grandfathering provisions for mothers separating before 1 July 2006 ceased; this again changed financial incentives for repartnering.

separating near the cut-off of 1 July 2006 are observed for 6 and a half years after separation.

The main analysis sample comprises 10,828 mothers who separated between 1 July 2001 and 1 July 2011 and whose youngest child was aged eight to 15 at the point of separation. Of these mothers 2,119 separated after the reform’s implementation. For robustness checks we also use samples of 8,038 mothers separating with a youngest child aged 16 or above, and 1,606 mothers separating with a youngest child aged six to seven and a half.

Our definition of relationship breakdown follows administrative guidelines. For our study period, the Social Security Act’s definition of an unmarried partner was a person in a ‘marriage-like relationship’ with another person. A couple (married or unmarried) was recognised as separated when the ‘couple are living separately and apart on a permanent or indefinite basis’ (Department of Social Security 2017). All mothers receiving family payments 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.²⁰

Table 1 shows descriptive statistics for mothers in our analysis sample. Column (1) displays averages for the whole sample, and Columns (2) and (3) show averages for mothers separating before and after the reform date respectively. On average, mothers were aged 42 at the point of separation and 44 in 2006. They were 27 when they gave birth to the cohort child, and their youngest child was aged 13 at the time of separation. The majority of mothers were born in Australia, and six percent are of indigenous (Aboriginal and/or Torres Strait Islander (ATSI)) descent. 19 percent of mothers were receiving

²⁰The 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): there are very high information and attention requirements to ‘game’ the system.

Parenting Payment Partnered one year before separation, a further nine percent received another income support payment, and the remainder were not income support recipients, demonstrating that our analysis includes mothers separating from middle- and low-income households.

Table 1
Descriptive Statistics: Administrative Sample

	All	Pre-reform	Post-reform	P-value
Mother				
Age (1 July 2006)	43.79	44.17	42.24	0.00
Age (separation)	41.74	41.15	44.16	0.00
Age (birth of youngest child)	26.61	25.83	29.83	0.00
Australian-born	0.75	0.75	0.76	0.82
Aboriginal and/or Torres Strait Islander	0.06	0.06	0.08	0.00
Youngest child				
Male	0.50	0.50	0.49	0.37
Age (separation)	12.63	12.59	12.79	0.00
Income support payments in year prior to separation				
Parenting Payment Partnered	0.19	0.20	0.13	0.00
Disability Support Pension	0.03	0.03	0.04	0.00
Newstart Allowance (unemployment)	0.04	0.03	0.05	0.00
Carers Payments	0.02	0.01	0.04	0.00
Observations	10828	8709	2119	

Notes: Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with youngest child aged 8-15 at the time of separation. Pre-reform mothers separated before 1 July 2006; post-reform mothers separated after 1 July 2006. P-value from test of difference in means for pre- and post-reform groups

Column (4) of Table 1 shows p-values for the test of the difference of average characteristics between mothers separating in the five years before and five years after 1 July 2006. Mothers affected by the reform are older at the time of separation and when their youngest child was born, are less likely to have received Parenting Payment Partnered, and are more likely to have received an alternative income support payment. These differences in observable characteristics motivate our Regression Discontinuity Design (RDD) empirical strategy.

4 Empirical strategy

We use a sharp RDD to estimate how the 2006 welfare reforms affected the repartnering behaviour of separating mothers. Let $R_{it}(1)$ be an indicator equal to 1 when a separating mother i subject to the post-reform welfare policy has repartnered by t months after separation, and $R_{it}(0)$ be the same repartnering outcome for the mother under the pre-reform welfare policy. Our objective is to estimate the impact of the reform on repartnering outcomes for various time points, t , after separation, that is $\alpha_t = E(R_{it}(1) - R_{it}(0))$.

In our RDD, the date a mother separates, $SepT_i$, is the running variable, and 1 July 2006 is the cut-off point after which separating mothers were subject to the new welfare rules. We normalise the separation date to $X_i = SepT_i - 01/06/2006$ so that it is centred around the cut-off.

Under the assumption that the regression functions $E(R_{it}(1)|X_i = x)$ and $E(R_{it}(0)|X_i = x)$ are continuous at $x = 0$, we estimate α_t^{RD} :

$$\alpha_t^{RD} = \mu_{t+} - \mu_{t-}, \quad \mu_{t+} = \lim_{x \downarrow 0} \mu_t(x), \quad \mu_{t-} = \lim_{x \uparrow 0} \mu_t(x), \quad \mu_t(x) = E(R_{it}|X_i = x).$$

That is, we estimate separate regressions for mothers separating before and after the cut-off and take the difference between those regressions at the boundary point.

For our main results, we implement the RDD using Local Linear Regressions (LLR). This fits independent linear regressions on either side of the cut-off using data from a chosen bandwidth, h , and kernel function $K(\cdot)$. Following Hahn, Todd and Van der Klaauw (2001) we estimate:

$$\begin{aligned}\hat{\alpha}_t^{RD} &= \hat{\mu}_{t+,1}(h) - \hat{\mu}_{t-,1}(h) \\ \left(\hat{\mu}_{t+,1}(h), \hat{\mu}_{t+,1}^{(1)}(h)\right)' &= \arg \min_{b_0, b_1 \in \mathbb{R}} \sum_{i=1}^n \mathbb{1}(X_i \geq 1) (R_{it} - b_0 - X_i b_1)^2 K(X_i/h) \\ \left(\hat{\mu}_{t-,1}(h), \hat{\mu}_{t-,1}^{(1)}(h)\right)' &= \arg \min_{b_0, b_1 \in \mathbb{R}} \sum_{i=1}^n \mathbb{1}(X_i < 1) (R_{it} - b_0 - X_i b_1)^2 K(X_i/h)\end{aligned}$$

We select optimal bandwidths based on the approach by Calonico, Cattaneo and Titiunik (2014) where we use one common MSE-optimal bandwidth selector. We refer to this as the CCT bandwidth.²¹ We use the triangular kernel function and robust standard errors.

Alongside the LLR approach, we also present estimates from a global polynomial approach using the full ten year range of separations (that is, a bandwidth of five years), and vary the polynomial order from one (linear) to four. We use the Akaike Information Criterion (AIC) to select the best model.

We begin by estimating ‘first-stage’ models of whether receipt of PPS and other welfare payments changed after the reform’s implementation, before presenting our preferred reduced-form results for repartnering outcomes from three months to four years after separation.

We also estimate the effect of (i) PPS receipt and (ii) the amount of income support received on repartnering using a fuzzy RDD approach. The fuzzy RDD scales our reduced-form estimates by the discontinuity in (i) the probability of receiving PPS, and (ii) the amount of income support received. It provides an estimate of the impact of PPS receipt or additional income support induced by the reform on repartnering. In each case, for our estimates to be valid we must make stronger assumptions. For (i) we must assume that the only way the 2006 reforms affect repartnering is through the change in PPS

²¹We also provide estimates varying this bandwidth. See online Appendix Table C2.

eligibility, and for (ii) we must assume that the only mechanism is through the change in the amount of income support received. In sections 5.2 and 5.4 we present evidence for these assumptions.

4.1 Identification assumptions

The key assumption of the RDD approach is that assignment of separating mothers around the cut-off is random: mothers did not precisely manipulate the timing of their separation around 1 July 2006. The concern is that some mothers may have responded to the policy announcement (made on 11 May 2005) by pulling forward their separation to occur before 1 July 2006 or being deterred from separating in the first place. Then any discontinuities in repartnering patterns around the cut-off could be driven by such non-random selection. We test this assumption in Section 5.1. We also include additional covariates such as prior income support receipt and pre-determined demographic characteristics as a robustness check.

We argue that we identify the impact of changes to *financial incentives* on the repartnering behaviour of separated mothers. This implies that it is the financial incentives that change on 1 July 2006 that drive our results, and that there are no other changes at this date that affect repartnering. There are five concerns with this assumption. First, there may be seasonal factors that differentially affect mothers separating in the first or second half of the calendar year. For example, it may be ‘easier’ to repartner if you separate in the (southern hemisphere) springtime. We provide evidence to rule out confounding seasonal effects in Section 5.4.²²

Second, the 2006 reform package also grandfathered existing PPS recipients into the old regime on 1 July 2006. Our results may therefore reflect these grandfathered mothers

²²An additional concern is that 1 July is the start of a new tax year in Australia. However, any aspect of the tax and benefit system that depends on partner status is prorated across the tax year based on the proportion of time in a relationship, so there is no discrete change in the incentive to be partnered around 1 July.

slowing their repartnering response to retain access to the payment. In Section 5.4.1 we estimate the impact of grandfathering on repartnering, and show no significant effects.

Third, participation requirements were immediately imposed for mothers separating in the post-reform period, but were introduced no earlier than 1 July 2007 for pre-reform separators. These participation requirements could explain our results, particularly over the short-term, if they affect the decision to repartner. In Section 5.4.2 we show that there is no change in the repartnering behaviour of mothers subject to only participation requirements, suggesting that participation requirements do not drive our results.

Fourth, the 2006 reforms also involved changes to the Disability Support Pension, meaning that our estimation approach may capture the effects of the restriction in DSP access as well as the restrictions to PPS. There are two dimensions to this concern. First, separating mothers who have work-limiting impairments who would have been previously eligible for DSP when separated will be diverted to NSA, reflecting a reduction in financial support and the imposition of participation requirements. Second, separating mothers who are no longer eligible for PPS may apply for DSP instead as the payment level is more generous than NSA. In Section 5.2 we show that there is no change in the rates of DSP receipt, or in the receipt of other income support payments, around 1 July 2006.

A further concern is that our estimated effects reflect changes in *reporting* behaviour and not genuine effects on repartnering. Mothers receive higher welfare payments when single compared to when partnered both pre- and post-reform, so there is an incentive to not report a new partner for mothers in both the pre- and post-reform groups.²³ The concern is that the financial incentive to not report repartnering is lower for mothers in the post-reform period. The policy changed the relative incentives to misreport, but importantly they did not change the dominant strategy to not report a new partner to

²³For example, before the reform, combined gross income for a mother with no private income and a man with an income of \$35,000 was 31% higher when reporting a separation 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. These numbers are based on a couple with one child aged eight, and incorporate Family Tax Benefits, Parenting Payments, and Newstart Allowance.

maximise welfare payments. Moreover, as previously discussed, there are heavy penalties for misreporting and regular compliance checks occur.

5 Results

We now present our RDD results showing that the 2006 reforms increased the probability of repartnering for affected mothers. We first present evidence that our analysis sample is balanced around the cut-off date, ruling out manipulation of the separation date and validating the RDD approach. We then present our main RDD results, along with evidence of heterogeneous effects and a series of robustness checks.

5.1 Manipulation

For our RDD approach to be valid we need to rule out manipulation of separation or selection into separation around the July 2006 cut-off date resulting in non-comparability of mothers separating before and after the reform. We first test for a change in the density of separations. If manipulation or selection are present, we would expect a discontinuity in this density around the cut-off date. We may expect more separations to occur just before 1 July 2006 as some mothers separate early to ensure access to higher welfare payments. After the reform mothers may not separate in response to the worse outside option. Figure 2 presents the McCrary density function and shows that the density is not discontinuous at the cut-off.²⁴ Note that the downward trend in separating mothers over time reflects the declining fraction of our sample with a youngest child aged eight to fifteen, and not a decline in the population-level hazard of separation.

We test the smoothness of the density at the cut-off using the test proposed by Cattaneo, Jansson and Ma (2017). The test is based on a local polynomial density estimator and does not require any pre-binning of the data. We do not reject the null hypothesis of

²⁴This figure is based on the automatic bandwidth selection. Online Appendix Figure C1 presents the McCrary density function when we manually restrict the bandwidth to be half the size of the automatically chosen bandwidth (0.5×1.656), as recommended by Lee and Lemieux (2010). Again, there is no evidence of any discontinuity in the density at the cut-off.

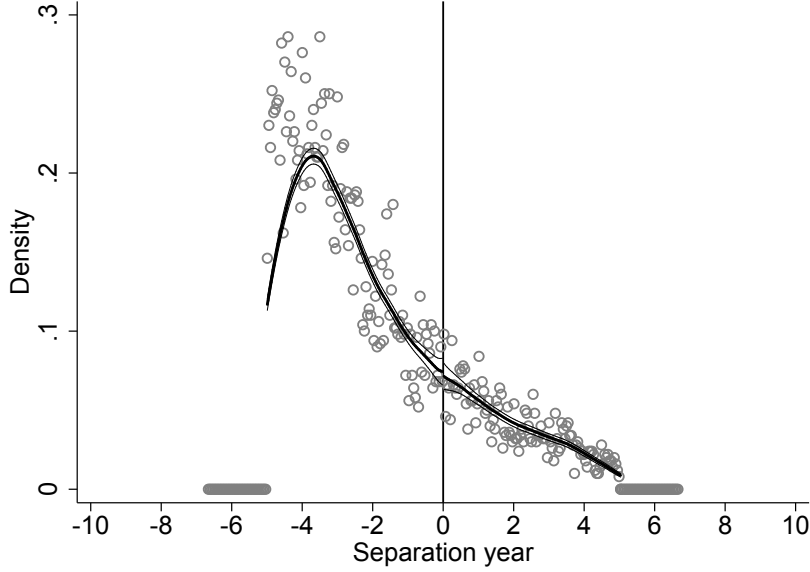


Fig. 2. *McCrary Density Test for Discontinuity at the Reform Date*

Notes: Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-16 years old at the time of separation (N=10,828). The separation date is normalised so that 1 July 2006 is equal to 0.

no discontinuity in the density at the cut-off according to the robust bias-corrected approach with a p-value of 0.42. We also apply this test separately to mothers who may have stronger motivations to manipulate the timing of separation such as mothers who stand to face the greatest degree of financial loss from the reform because their youngest child is relatively young (aged eight to 12) at separation or mothers who have greater levels of financial need as indicated by prior year receipt of income support. Again we find little evidence of any manipulation behaviour in mothers from these groups where the robust bias-corrected p-values for the null of no discontinuity are 0.15 and 0.84, respectively.

We also examine whether pre-determined characteristics of mothers are balanced at the cut-off. These characteristics cannot be influenced by the policy so any discontinuity may indicate selection based on observable characteristics. Figures 3 and 4 show how these characteristics vary by date of separation, and Table 2 shows results for tests of discontinuities in these characteristics using a LLR RDD approach. Both the graphical evidence and RDD results show no change in mother's age at 1 July 2006, at separation,

and at first birth. There is no change in the youngest child's age at the time of separation or their gender, and no evidence of differences in whether the mother is Australian-born or indigenous.

Table 2
Covariate Balance Tests

	Coefficient	SE
Mother		
Age (separation)	0.519	(0.550)
Age (birth of youngest child)	-0.307	(0.567)
Australian-born	0.054	(0.046)
Aboriginal and/or Torres Strait Islander mother	0.004	(0.016)
Youngest child		
Male	-0.071	(0.045)
Age (separation)	-0.199	(0.212)
Cohort child[†]		
Age (separation)	0.500	(0.551)
Income support payments in year prior to separation		
Parenting Payment Partnered	0.051*	(0.030)
Disability Support Pension	0.018	(0.017)
Newstart Allowance (unemployment)	-0.017	(0.014)
Carers Payment	-0.005	(0.015)

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation (N=10,828). Table presents LLR estimates for the smoothness of pre-determined variables at the 1 July 2006 cut-off. LLR bandwidths for each outcome vary and are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. The bandwidth sizes vary from 1 to 1.8 years.

[†] Cohort child is the child born between October 1987 and March 1988 which defines the set of mothers we observe. In most cases the mother's youngest child is the cohort child's younger sibling.

There is no change in the proportion of separating mothers who received disability, unemployment or carers payments one year before separating, though mothers separating post-reform are five percentage points more likely to have received Parenting Payment Partnered, statistically significant at the 10% level. We test for the joint significance of these pre-determined factors using a seemingly unrelated regression and find that the discontinuities are jointly insignificant (with a p-value of 0.133). In Section 5.3, we test the sensitivity of the regression results to the inclusion of these characteristics as controls.

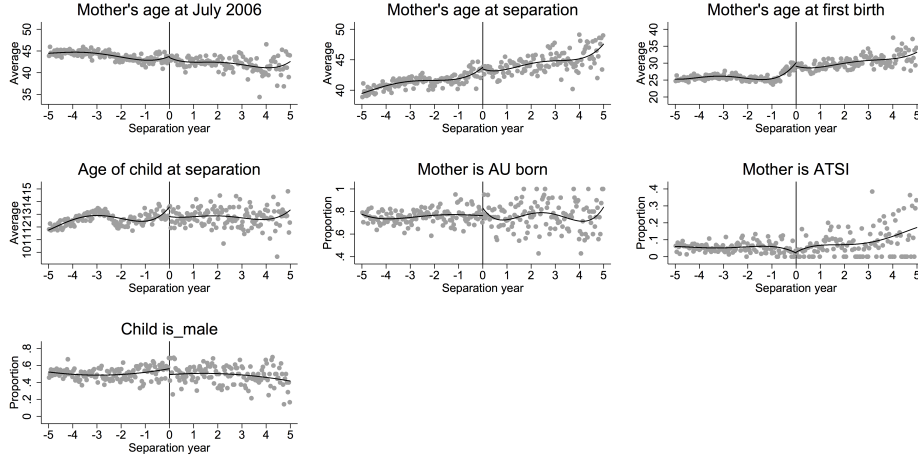


Fig. 3. *Demographic Characteristics*

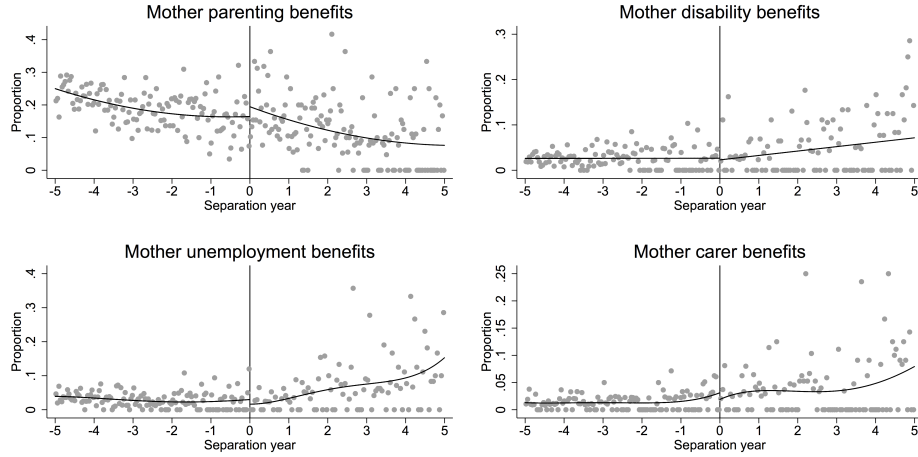


Fig. 4. *Previous Income Support Receipt*

Notes: Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation (N=10,828). The separation date is normalised so that 1 July 2006 is equal to 0. Income support benefits are measured in the year before the separation date. The figures have been generated based on data-driven bin-widths, where the number and size of the bin-widths vary on either side of the cut-off. Each point represents the average receipt rate within each of these bins (which vary in length from 12-19 days) and the solid line reflects the predicted receipt rates based on the Global Polynomial regressions. Polynomial orders for the predicted line have been chosen based on the model yielding the minimum Akaike Information Criterion (Lee and Lemieux, 2010).

There are a number of other factors that could vary between the pre- and post-reform separating mothers that are not reported in the administrative data. Factors such as income, education, risk aversion and attitudes can play a role in repartnering responses and may be associated with manipulation behaviour. We are unable to test for discontinuities in these unobserved characteristics. However, the smooth density and continuity of observable characteristics around the cut-off validate the RDD approach.

5.2 Policy implementation

We now show evidence of the implementation of the 2006 reforms. Figure 5 plots the proportion of separating mothers receiving PPS after separating (left figure) and the amount of income support payments received (right figure) by the timing of separation. For mothers separating after 1 July 2006, the probability of PPS receipt was close to zero.²⁵ Average income support receipt declined, reflecting mothers exiting the IS system or receiving the lower NSA payment.

Table 3 presents estimates of the magnitude of the changes in income support receipt. These are results from RDD estimations with either payment receipt or the amount of payment as the outcome. Based on the LLR results, PPS receipt declined by 32 percentage points post-reform. The proportion of mothers receiving any income support payment declined by 12 percentage points, with remaining mothers receiving the lower Newstart Allowance (NSA) payment (a 21 percentage point increase). Mothers separating after 1 July 2006 received AU\$70 less government assistance every two weeks.

Table 3 also shows that the reform did not cause any statistically significant changes in the proportion of separating mothers receiving the Disability Support Pension (DSP) or other income support payments, providing evidence that it is only the PPS changes that affect separating mothers in our sample.

²⁵A small percentage of mothers separating after 1 July 2006 were exempt from the new rules because they faced extraordinary hardships such as experiencing domestic violence or having four or more children.

Table 3
Policy Implementation

		LLR	GP			
	Average	BW: CCT	Order: 1st	Order: 2nd	Order: 3rd	Order: 4th
<i>PPS receipt</i>	0.409	-0.324*** (0.025)	-0.349*** (0.015) 13526	-0.344*** (0.022) 13530	-0.327*** (0.030) 13532	-0.321*** (0.039) 13535
<i>AIC</i>						
<i>IS receipt</i>	0.552	-0.115*** (0.037)	-0.162*** (0.022) 15138	-0.122*** (0.033) 15137	-0.108** (0.044) 15139	-0.129** (0.054) 15141
<i>AIC</i>						
<i>NSA receipt</i>	0.080	0.209*** (0.027)	0.190*** (0.017) 1410	0.206*** (0.024) 1412	0.221*** (0.032) 1415	0.194*** (0.039) 1417
<i>AIC</i>						
<i>DSP receipt</i>	0.038	-0.005 (0.018)	-0.010 (0.010) -5265	0.012 (0.014) -5272	-0.007 (0.018) -5272	-0.007 (0.023) -5269
<i>AIC</i>						
<i>Other receipt</i>	0.010	0.012 (0.013)	0.012* (0.007) -15254	0.008 (0.009) -15250	0.002 (0.012) -15247	-0.001 (0.014) -15252
<i>AIC</i>						
<i>IS dollars</i>	223.76	-70.43*** (22.977)	-81.04*** (11.633) 150140	-46.12*** (16.808) 150136	-40.79* (21.980) 150138	-55.39** (26.615) 150141
<i>AIC</i>						

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with youngest child aged 8-15 years old at the time of separation (N=10,828). Table presents LLR and GP estimates for the change in receipt rates at 1 July 2006 for the following payments: Parenting Payment Single (PPS), Newstart (Unemployment) Allowance (NSA), Disability Support Pension (DSP), Other (includes Carer payment and Parenting Payment Partnered (PPP)), and any Income Support receipt (IS), as well as changes in the amount of IS dollars received. As the Australian social assistance system delivers benefits to a broad cross-section of the working-age population, any IS payment includes: disabled individuals who can receive a Disability Support Pension, individuals with the responsibility of caring for someone with a severe disability who can receive the Carer Payment, low-income families with dependent children who can receive parenting benefits in the form of Parenting Payment Single (PPS) for single parents and Parenting Payment Partnered (PPP) for partnered parents, and unemployed individuals meeting certain activity tests can receive unemployment benefits in the form of Newstart Allowance. LLR bandwidths for each outcome vary and are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. The bandwidth sizes vary from 1 to 2.1 years. For each outcome, the GP model yielding the lowest AIC is highlighted. If there are two GP competing models, we choose the model with the higher polynomial order. All outcomes are estimated immediately after the policy was implemented.

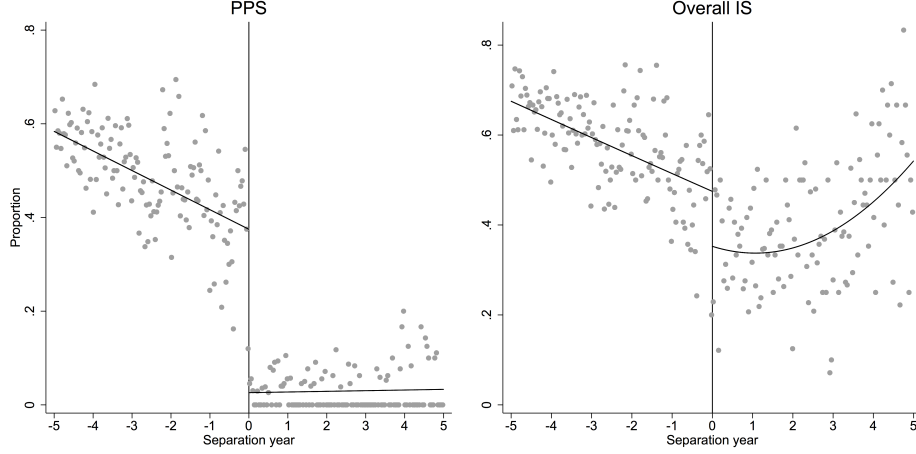


Fig. 5. *Policy Implementation*

Notes: Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation ($N=10,828$). The separation date is normalised so that 1 July 2006 is equal to 0. The figures have been generated based on data-driven bin-widths, where the number and size of the bin-widths vary on either side of the cut-off. Each point represents the average receipt rate within each of these bins (which vary in length from 14-16 days) and the solid line reflects the predicted receipt rates based on the Global Polynomial regressions. We have chosen polynomial order 1 for PPS receipt and order 2 for IS receipt based on the minimum Akaike Information Criterion (Lee and Lemieux, 2010).

5.3 Repartnering

We now present our main results. Figure 6 illustrates the effects of the 2006 reform on repartnering by 6, 12, 18, 24, 36 and 48 months after separation. Each graph shows how the proportion of separating mothers who have repartnered by this time varies around the date of separation. The proportion of mothers repartnering increases after the cut-off for all durations, and is most pronounced within six months of separation.

Table 4 presents the RDD regression results corresponding to these graphs. Column (1) shows the average repartnering rate for the sample. Column (2) displays the LLR results using CCT bandwidths, which vary from 1 to 1.5 years. Columns (3) to (6) display the results for the Global Polynomial regression for polynomial orders 1 to 4; the result with the lowest AIC is in bold.

These results show that the 2006 reform caused a statistically significant increase in the probability of repartnering. Six months after separation, post-reform separating

Table 4
Effect of Reform on Repartnering

		LLR	GP			
	Average	BW: CCT	Order: 1st	Order: 2nd	Order: 3rd	Order: 4th
<i>Repartnering (months from separation date)</i>						
<i>3 months</i>	0.056	0.003 (0.021)	0.002 (0.010)	0.002 (0.015)	0.018 (0.018)	0.024 (0.022)
<i>AIC</i>			-1051	-1050	-1048	-1048
<i>6 months</i>	0.098	0.063** (0.025)	0.022* (0.013)	0.029 (0.019)	0.058** (0.025)	0.079*** (0.030)
<i>AIC</i>			4442	4444	4444	4441
<i>12 months</i>	0.165	0.072** (0.032)	0.024 (0.016)	0.027 (0.024)	0.078*** (0.030)	0.086** (0.037)
<i>AIC</i>			9222	9225	9220	9219
<i>18 months</i>	0.209	0.069* (0.035)	0.029 (0.018)	0.019 (0.026)	0.067** (0.034)	0.084** (0.041)
<i>AIC</i>			11177	11178	11172	11173
<i>24 months</i>	0.243	0.055 (0.038)	0.015 (0.019)	-0.000 (0.028)	0.038 (0.036)	0.065 (0.044)
<i>AIC</i>			12333	12335	12331	12332
<i>36 months</i>	0.294	0.073* (0.039)	0.020 (0.020)	0.008 (0.029)	0.059 (0.039)	0.091* (0.048)
<i>AIC</i>			13622	13626	13620	13622
<i>48 months</i>	0.329	0.083** (0.042)	0.007 (0.021)	0.005 (0.030)	0.063 (0.040)	0.107** (0.050)
<i>AIC</i>			13622	13626	13620	13622

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation (N=10,828). Table presents LLR and GP estimates for the main outcomes. LLR bandwidths for each outcome are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. The bandwidth sizes (expressed in years) vary across outcome; specifically they are: 1 (3 months), 1.2 (6 months), 1.2 (12 months), 1.3 (18 months), 1.3 (24 months), 1.5 (36 months), and 1.4 (48 months). For each outcome, the GP model yielding the lowest AIC is highlighted. If there are two GP competing models, we choose the model with the higher polynomial order. The outcome is repartnering within a given number of months of the separation event.

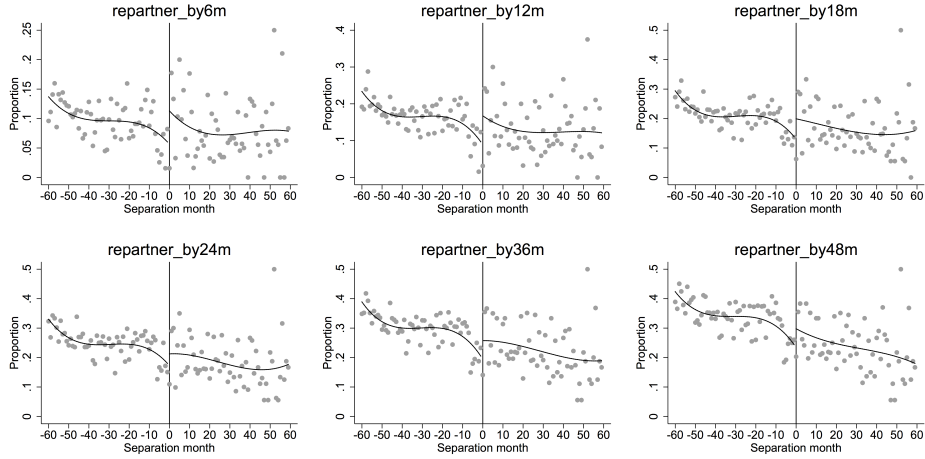


Fig. 6. *Repartnering*

Notes: Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation ($N=10,828$). The separation date is normalised so that 1 July 2006 is equal to 0. The figures have been generated based on data-driven bin-widths, where the number and size of the bin-widths vary on either side of the cut-off. Each point represents the average proportion within each of these bins (which vary in length from 11-18 days) and the solid line reflects the predicted values based on the Global Polynomial regressions. Polynomial orders for the predicted line have been chosen based on the model yielding the minimum Akaike Information Criterion (Lee and Lemieux, 2010).

mothers are six percentage points more likely to have repartnered than the pre-reform separating mothers, representing a 64 percent increase in the repartnering rate. The effect persists in the longer term with these mothers eight percentage points more likely to have repartnered four years after separation. Our empirical results are in line with the theoretical prediction of an increased probability of repartnering outlined in Section 2.

Table 4 also shows that the results are broadly consistent across the LLR and GP approaches. The LLR results are our preferred specification, particularly over the longer term, as all mothers used for the LLR estimation are observed for the full 48 month period after separation.²⁶ Furthermore, the LLR approach does not make any assumptions about the functional form of the relationship between the timing of separation and the outcomes for observations further away from the cut-off point.

²⁶See online Appendix B for more detail.

Fuzzy RDD estimates

Table 5 presents two sets of fuzzy RDD results. Column (2) shows the effect of PPS receipt on repartnering, and column (3) shows the effect of an AU\$1000 change in the annual amount of income support payments received. These estimates adjust the ‘reduced-form’ results presented above for the fact that not all separating mothers in the pre-reform group received PPS or faced a fall in income support receipt. We implement a Wald estimator, taking the ratio of the reduced-form effect to the first-stage effect of the reform, with the first-stage being the change in PPS receipt or change in amount of IS received as appropriate.

Table 5
Fuzzy RDD Results – Repartnering

	Reduced form	PPS receipt	IS amount (AU\$)
<i>Repartnering (months from separation date)</i>			
<i>3 months</i>	0.003 (0.021)	-0.003 (0.080)	-0.014 (0.014)
<i>6 months</i>	0.063** (0.025)	-0.216** (0.100)	-0.048* (0.025)
<i>12 months</i>	0.072** (0.032)	-0.260** (0.124)	-0.056* (0.030)
<i>18 months</i>	0.069* (0.035)	-0.248* (0.139)	-0.054* (0.032)
<i>24 months</i>	0.055 (0.038)	-0.211 (0.145)	-0.036 (0.029)
<i>36 months</i>	0.073* (0.039)	-0.273* (0.162)	-0.049 (0.034)
<i>48 months</i>	0.083** (0.042)	-0.314* (0.173)	-0.054 (0.037)

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation. Table presents LLR estimates. LLR bandwidths for each outcome vary and are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. The bandwidth sizes vary from 1 to 1.4 years for Column 2 and 1.8 to 1.9 for Column 3. For the Fuzzy regression results in Columns 2 and 3, the coefficient estimates are derived from the reduced form estimates in Column 1 deflated by the first-stage effects for any PPS receipt and total annual amount of income support receipt expressed in AU\$000.

The results in column (2) show that receiving PPS reduces the probability of having repartnered within six months of separation by 22 percentage points, and within four years by 31 percentage points. This interpretation rests on the assumption that the only way the 2006 reforms affected the way that separating mothers repartner is through the impact on PPS receipt. The results presented in Section 5.2 suggest that this is the case.

The results in column (3) are less precisely estimated as the first-stage effect on the amount of income support receipt is less precisely estimated. They show that a AU\$1000 increase in income support receipt (around eight percent of the maximum annual PPS payment in 2006) reduces the probability of repartnering within six months by 4.8 percentage points, with the magnitude of the estimate consistent over the longer term. To draw this conclusion, we must assume that the only way the 2006 reforms affect the repartnering of separating mothers is through the change in the amount of income support received. In Section 5.4 we provide evidence that other aspects of the reforms do not explain our main results, giving support to this assumption.

Our preferred estimates are the LLR RDD estimates showing the overall effect of the 2006 reform. This is because mothers who are not income support recipients may change their behaviour according to their potential eligibility changes. For example, a pre-reform separating mother may decide to take a high-risk job because she knows that she will be eligible for PPS in the event of job loss, and this may also affect her repartnering behaviour.

5.4 Robustness checks

In Section 5.1, we find little evidence that mothers manipulate the timing of separation near the cut-off, meaning that our RDD results reflect an unbiased estimate of the change in repartnering behaviour around 1 July 2006. However, there are a number of other potential mechanisms that may explain the changes we see around this time aside from the change in the amount of welfare payments received by separating mothers. In this section, we test the robustness of the main RDD results.

Table 6
Robustness Tests

	RDD-DD					
	RDD		Older children		Other years	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Repartnering (months from separation date)</i>						
<i>3 months</i>	0.003 (0.021)	-0.003 (0.021)	0.006 (0.009)	0.009 (0.014)	0.031* (0.017)	0.030* (0.017)
<i>6 months</i>	0.063** (0.025)	0.062** (0.025)	0.050*** (0.015)	0.052*** (0.015)	0.078*** (0.025)	0.074*** (0.025)
<i>12 months</i>	0.072** (0.032)	0.072** (0.032)	0.070*** (0.022)	0.078*** (0.023)	0.077*** (0.026)	0.076*** (0.026)
<i>18 months</i>	0.069* (0.035)	0.068* (0.035)	0.100*** (0.021)	0.107*** (0.025)	0.097*** (0.032)	0.073** (0.029)
<i>24 months</i>	0.055 (0.038)	0.053 (0.037)	0.097*** (0.026)	0.105*** (0.026)	0.061** (0.030)	0.060** (0.030)
<i>36 months</i>	0.073* (0.039)	0.069* (0.037)	0.116*** (0.028)	0.123*** (0.028)	0.082** (0.032)	0.079** (0.032)
<i>48 months</i>	0.083** (0.042)	0.081** (0.041)	0.132*** (0.029)	0.150*** (0.024)	0.087*** (0.034)	0.085** (0.033)
Controls	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample in Columns 1 and 2 consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation. Column 1 presents main LLR estimates. LLR bandwidths for each outcome vary and are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. Column 2 includes controls, including pre-determined demographic characteristics of age (mother and child), gender of child, ethnicity, and prior income support receipt (separate indicators for Parenting Payment Partnered (PPP), Unemployment benefit - NewStart Allowance (NSA), Disability Support Pension, and Carer Allowance). Columns 3, 4, 5 and 6 present RDD-DD models, which serves to eliminate any potential seasonality effects or manipulation effects on the main estimators. Columns 3 and 4 sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8 years or above at the time of separation (N=18,866). The additional comparison group are mothers whose youngest child was aged 16 or older at the point of separation. Column 4 includes controls. Columns 5 and 6 sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation. The additional comparison group are mothers who separated in 01 July of the years adjacent to 2006. Column 6 includes controls.

One direct way to test for the presence of bias in the estimated effects is to control for the characteristics of mothers and children in the RDD regressions (Frölich and Huber 2018). In Section 5.1, we showed that pre-determined characteristics of the mothers and child were both individually and jointly discontinuous at the 1 July 2006 cut-off. Here, we control for characteristics including the age of the mother and child, child gender, ethnicity, and prior income support receipt. Columns (1) and (2) of Table 6 present the LLR results, with and without controls. We see that the coefficient sizes are largely stable, although adding controls brings somewhat greater precision to the estimates.

We test for robustness to seasonal trends and other temporal effects on repartnering using a regression discontinuity-difference-in-difference approach (RDD-DD). Such mechanisms may give rise to differences in repartnering behaviour around 1 July 2006 for other groups that are unaffected by the policy or around the same date in other calendar years. We use these two additional comparison groups of (1) unaffected mothers and (2) other calendar years.

Our first additional comparison group is mothers with a youngest child aged 16 or older at the point of separation. All of these mothers were categorically ineligible for PPS payments upon separation both pre- and post-reform, so this group captures any independent time-varying influences on repartnering behaviour. Columns (3) and (4) in Table 6 present these results, with and without controls. Accounting for these time-varying effects strengthens our conclusions: separating mothers affected by the reform are 5 percentage points more likely to have repartnered within six months, 10 to 11 percentage points within 18 months, and 13 to 15 percentage points within four years after separation.

Our second additional set of comparison groups is mothers separating in other calendar years. During the pre-reform years of 2001 to 2005, all mothers in our sample were eligible for PPS payments upon separation. During the post-reform years of 2007 to 2011, all mothers with a youngest child aged 8 to 15 years were ineligible for PPS payments upon separation. Thus all mothers separating around the ‘1 July’ cutoffs in

these years face the same financial incentives and so should not exhibit any differences in repartnering behaviour. Columns (5) and (6) in Table 6 present these results, with and without controls. Again, our RDD-DD estimations using other years as comparison groups strengthens our conclusions: separating mothers affected by the reform (in 2006) are 7 to 8 percentage points more likely to have repartnered within six months, 8 percentage points within 36 months, and 9 percentage points within four years after separation. This shows that that our results are not reflecting a ‘1 July’ effect in repartnering.

In addition to the RDD-DD results in Table 6, we also include placebo tests using cutoffs in pre-reform calendar years and the cutoff related to the announcement date. This allows us to test for the continuity of the probability of repartnering around 1 July in other calendar years, and also to test for any evidence of a repartnering response around the announcement date of 10 May 2005. Our main analysis assumes that there would be continuity in the repartnering rate around 1 July 2006 in the absence of the welfare reforms. By testing for continuity in the repartnering rate away from the reform date we can gain some confidence in the continuity assumption. These results are presented in online Appendix Table C1. We examine cutoff dates 1, 1.5 and 2 years prior to the true implementation date, as well as at the announcement date.²⁷ There is no significant change in repartnering for mothers separating around 1 July in any other calendar year, and no evidence of discontinuities in repartnering at the policy announcement date.

Finally, we vary the bandwidth used in the LLR results away from the optimal bandwidths of 1.5 to 2 years used in our main results. Online Appendix Table C2 presents results with bandwidths of 1, 1.5, 2, 2.5, and 3 years. The results support our conclusions. In general as the bandwidth increases both point estimates and standard errors decrease, illustrating the trade-off between bias and precision.

²⁷Following Cattaneo, Idrobo and Titiunik (2018) our sample is restricted to mothers who separated before 1 July 2006 in order to avoid including any treatment observations in these placebo tests.

5.4.1 Grandfathering

As explained in Section 1, mothers receiving PPS before 1 July 2006 were grandfathered under the old eligibility rules so could continue to receive the higher payment until their youngest child turned 16 or they became ineligible for another reason (for example due to repartnering). Below we show the grandfathering provision does not explain our results, that is it did not cause mothers separating pre-reform to be more careful about their repartnering decisions.

Specifically, we test for the existence of grandfathering effects by isolating a group of separating mothers who were not affected by grandfathering or the 2006 reforms, and comparing their behaviour to that of grandfathered mothers. The unaffected mothers are those separating between 10 May and 10 November 2004. As the reforms were announced on 10 May 2005, these mothers did not know about the future reforms or the grandfathering provisions for at least six months after they separated and so they constitute our pre-announcement control group.

Our sample of grandfathered mothers are those separating after the reforms were announced on 10 May 2005 and so who could have known that they were grandfathered. We consider mothers separating between 10 May and 31 December 2005 as our post-announcement treatment group²⁸ and we compare their repartnering behaviour three and six months after separation with those of the control group. If mothers who are grandfathered are choosing not to repartner to retain their PPS eligibility, we would expect this treatment group to be significantly less likely to repartner compared to the control group.

Column (1) of Table 7 shows results from estimations of the difference in the repartnering probability at three and six months post-separation for our treatment group relative to the control group, controlling for pre-determined demographic characteristics.²⁹

²⁸Our results are consistent if the treatment group is all mothers separating between 10 May 2005 and 1 July 2006.

²⁹As this analysis omits mothers separating between 10 November 2004 and 10 May 2005 completely, we do not take an RDD approach.

We see no significant differences in the probability of repartnering, with point estimates very close to zero. These results imply that our main estimates are not being driven by the grandfathered mothers being less likely to repartner, but instead by the post-reform mothers repartnering more quickly.

Table 7
Tests of Grandfathering and Participation Requirements

	Grandfathering OLS results	Participation requirements RDD results
<i>Repartnering (months from separation date)</i>		
<i>3 months</i>	0.002 (0.009)	-0.044 (0.065)
<i>6 months</i>	0.003 (0.012)	-0.013 (0.083)
<i>N</i>	5,200	1,606

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample for grandfathering results consists of mothers who separated before the reform date (1 July 2006) with children 8-15 years old at the time of separation, and omitting mothers separating six months before the implementation date (of 1 July 2006) and those separating six months before the announcement date (of 10 May 2005). The coefficients represent a comparison of means after controlling for demographic (age of the mother and child at separation, age of mother at birth of first child, gender of child, ethnicity, and prior income support receipt). Sample for the participation results consists of mothers whose youngest child was 6 to 7.5 years old at the time of the mother's separation. The coefficients represent the RDD estimates comparing mothers who separated before and after 1 July 2006. LLR bandwidths for each outcome vary and are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. The bandwidths are 1.5 years for the outcome of repartnering within three months and 1.4 years for repartnering within six months.

5.4.2 Participation requirements

The estimated effects in Section 5.3 may reflect the participation requirements introduced by the 2006 reforms. Mothers separating after 1 July 2006 lost access to PPS and were also required to search for (or participate in) work or study-related activities lasting for at least 15 hours per week. Pre-reform separating mothers were subject to these participation requirements starting from one year later.³⁰

³⁰Fok and McVicar (2013) provide a detailed description of how the extension of the participation requirements to grandfathered mothers occurred.

We test for the effects of participation requirements using mothers separating with a youngest child aged six to seven and a half years old. Within this group, post-reform separating mothers were subject to participation requirements from the point of separation, but only affected by the reduced welfare payments when their child turned eight. The pre-reform separating mothers within this group, however, were only subject to participation requirements one year later – when their youngest child was aged seven years or older on 1 July 2007. We can therefore compare the repartnering behaviour of these two groups of mothers to isolate the effects of participation requirements. If the introduction of participation requirements explains our main results, we would expect to see an increase in the repartnering rate after the reform amongst the post-reform separating group of mothers.

Column (2) of Table 7 presents RDD results for separating mothers with children aged six to seven and a half at separation. The age restriction means we can look at the effect on repartnering by six months post-separation without post-reform mothers also receiving lower welfare payments. The results show no evidence of increased repartnering. The point estimates are negative for both time periods, though the much smaller sample size of 1,606 means the estimates are less precise. The results imply that the participation requirements do not explain our main results, with the reduction in the amount of welfare payments being the more important part of the reforms.

This conclusion is also supported by the fact that we see significant effects of the reform on repartnering as much as 48 months after separation compared to the repartnering effects within six months of separation. Within the 48 month-period since separation, pre- and post-reform separating mothers who separated near the cut-off point had both been exposed to the same participation requirements for nearly 75 per cent of the time. Second, anecdotal evidence suggests that participation requirements were less likely to have been enforced on post-reform mothers in the initial phase of implementation.³¹ This

³¹Anecdotally, Centrelink welfare officers were unprepared for the sudden increase in administrative duties associated with processing the participation requirements of newly affected mothers. Instead, officers activated a clause that has historically existed in the Centrelink rules: mothers who endured a difficult break-up were exempt from any participation requirements for 16 weeks.

further reduces the window in which pre- and post-reform groups of mothers were treated differently.

5.5 Treatment effect heterogeneity

We now examine whether there are heterogeneous treatment effects across various characteristics. Table 8 presents results from separate LLR regressions within a set of demographic subgroups. Columns (2) and (3) present results for mothers born overseas and Australian-born mothers. The increase in repartnering is driven by mothers born in Australia and is not seen for those born overseas. This may reflect their greater familiarity with the income support system or ‘thinner’ repartnering markets for overseas-born mothers. Columns (4) and (5) of Table 8 show that the repartnering responses are stronger for mothers whose youngest child is male, though we are unable to draw a strong conclusion about differential effects by child gender.

Columns (6) and (7) show that the repartnering effects are stronger for mothers aged below 42 years at the time of separation, whilst columns (8) and (9) show that short-term repartnering effects are driven by mothers with children aged 12 years or more, and the longer-term effects are driven by mothers with younger children. This may reflect changes in childcare responsibilities and also the parents of older children in the control group ‘aging out’ of PPS eligibility.

Finally, columns (10) and (11) show heterogeneous impacts by the income support status of mothers in the year before separation. The short-term repartnering effects are more pronounced for mothers who were previously receiving any income support payments. This may reflect the greater financial need of these mothers as well as their familiarity with the system.

Table 8
Repartnering: Heterogeneity Across Subgroups

	Mother birthplace		Sex of child		Mother's age		Child's age		Past IS receipt		
	Average (1)	Overseas (2)	AU (3)	Female (4)	Male (5)	≤ 42 (6)	>42 (7)	>12 (8)	≤ 12 (9)	On IS (10)	No IS (11)
<i>Repartnering (months from separation date)</i>											
3 months	0.056	-0.003 (0.043)	-0.009 (0.023)	0.003 (0.023)	0.018 (0.026)	-0.013 (0.021)	0.056 (0.037)	0.015 (0.021)	0.007 (0.041)	0.042 (0.043)	-0.014 (0.017)
6 months	0.098	0.003 (0.052)	0.073*** (0.028)	0.045 (0.034)	0.072* (0.037)	0.048 (0.031)	0.072 (0.050)	0.074*** (0.028)	0.007 (0.054)	0.139** (0.058)	0.012 (0.024)
12 months	0.165	0.029 (0.056)	0.087** (0.035)	0.060 (0.041)	0.082* (0.046)	0.042 (0.037)	0.125** (0.056)	0.054 (0.037)	0.096 (0.064)	0.086 (0.062)	0.020 (0.033)
18 months	0.209	0.021 (0.065)	0.079** (0.039)	0.060 (0.047)	0.080 (0.049)	0.042 (0.042)	0.113* (0.059)	0.059 (0.041)	0.060 (0.069)	0.097 (0.073)	0.042 (0.037)
24 months	0.243	-0.010 (0.072)	0.072* (0.042)	0.048 (0.047)	0.064 (0.054)	0.041 (0.044)	0.081 (0.065)	0.025 (0.039)	0.101 (0.071)	0.034 (0.074)	0.046 (0.040)
36 months	0.294	-0.013 (0.071)	0.097** (0.043)	0.038 (0.050)	0.098* (0.056)	0.038 (0.040)	0.105 (0.069)	0.020 (0.049)	0.163** (0.078)	0.056 (0.080)	0.062 (0.041)
48 months	0.329	0.011 (0.083)	0.103** (0.046)	0.067 (0.054)	0.086 (0.059)	0.051 (0.044)	0.108 (0.073)	0.041 (0.052)	0.155* (0.083)	0.068 (0.082)	0.070 (0.044)
N		2663	8165	5418	5410	4686	6142	6711	4117	3986	6842

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation. Table presents LLR estimates. LLR bandwidths for each outcome vary and are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. The bandwidth sizes vary from 0.8 to 1.5 years for the sub-group of overseas-born mothers; from 1.5 to 2.1 for the sub-group of Australian-born mothers; from 1.3 to 1.5 for the sub-group of mothers whose youngest child is female; and from 1 to 1.8 for the sub-group of mothers whose youngest child is male; from 1 to 1.4 years for the sub-group of mothers aged 42 or older at separation; from 1.1 to 2 for the sub-group of mothers aged less than or equal to 42 at separation; from 1 to 1.2 for the sub-group of mothers whose youngest child was aged 12 or older at separation; from 1.2 to 1.7 for the sub-group of mothers whose youngest child was aged less than or equal to 12 at separation; from 0.8 to 1.5 years for the sub-group of mothers previously receiving income support; and from 1.4 to 1.6 for the sub-group of mothers not previously receiving income support.

5.6 Relationship instability

We examine how the reform affected relationship instability by looking at the number of times a mother transitions in *and* out of a relationship within a given time period. For example, does the mother both repartner and subsequently separate within one (or more) year(s) of the initial relationship breakdown.

Figure 7 (top-left) shows that the reform increased the prevalence of relationship instability occurring within 1 year after separation. Over time, however, this effect weakens: pre-reform separating mothers who have repartnered are equally likely to experience a further separation.

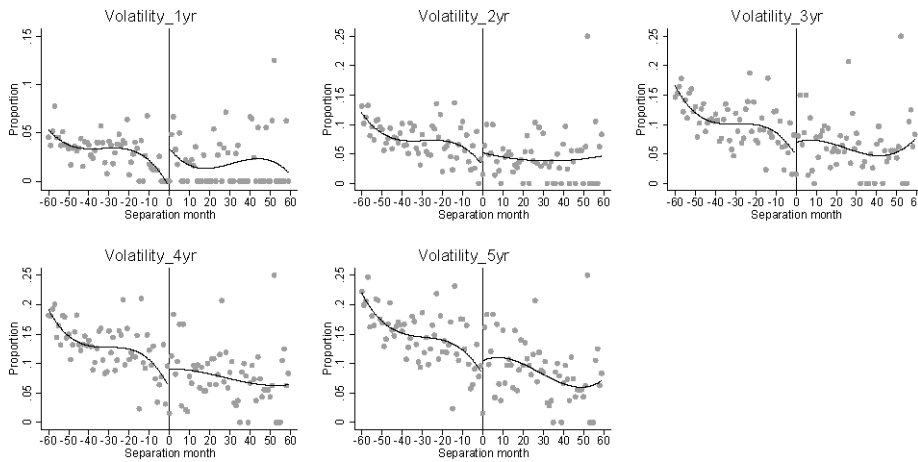


Fig. 7. *Relationship Stability*

Notes: Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation (N=10,828). The outcomes are the proportion of repartnerships that are transitory, which we define as those that subsequently break down. The separation date is normalised so that 1 July 2006 is equal to 0. The figures have been generated based on data-driven bin-widths, where the number and size of the bin-widths vary on either side of the cut-off. Each point represents the average proportion within each of these bins (which vary in length from 13-19 days) and the solid line reflects the predicted values based on the Global Polynomial regressions. Polynomial orders for the predicted line have been chosen based on the model yielding the minimum Akaike Information Criterion (Lee and Lemieux, 2010).

Table 9 presents results from the RDD analysis of this measure of relationship instability, and confirms the graphical analysis. Column (2) shows that post-reform mothers were 4 percentage points more likely to experience relationship instability in the year

Table 9
Relationship Instability

	LLR		GP			
	Average	BW: CCT	Order: 1st	Order: 2nd	Order: 3rd	Order: 4th
<i>Repartnering and subsequent separation in years since initial separation</i>						
<i>1 year</i>	0.031	0.037*** (0.012)	0.005 (0.007)	0.014 (0.010)	0.041*** (0.013)	0.044*** (0.015)
<i>AIC</i>			-7116	-7113	-7116	-7112
<i>2 years</i>	0.072	0.021 (0.018)	-0.001 (0.010)	-0.006 (0.015)	0.021 (0.020)	0.028 (0.023)
<i>AIC</i>			1359	1361	1358	1362
<i>3 years</i>	0.101	0.024 (0.022)	0.008 (0.012)	-0.002 (0.018)	0.020 (0.023)	0.038 (0.028)
<i>AIC</i>			4683	4684	4680	4683
<i>4 years</i>	0.125	0.031 (0.023)	0.007 (0.013)	-0.002 (0.020)	0.030 (0.026)	0.045 (0.031)
<i>AIC</i>			6669	6672	6669	6672
<i>5 years</i>	0.144	0.026 (0.024)	0.017 (0.014)	0.001 (0.021)	0.021 (0.027)	0.035 (0.033)
<i>AIC</i>			7964	7966	7964	7968

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample consists of mothers who separated within a five year window around the reform date (1 July 2006) with children 8-15 years old at the time of separation (N=10,828). Table presents LLR and GP estimates for relationship instability outcome. Outcome is equal to one if the mother repartners and separates again within time period after initial separation. LLR bandwidths for each outcome vary and are chosen based on the approach by Calonico, Cattaneo and Titiunik 2014. The bandwidth sizes vary from 1.2 to 1.4 years. For each outcome, the GP model yielding the lowest AIC is highlighted. If there are two GP competing models, we choose the model with the higher polynomial order.

after separation. We do not see any significant effect on relationship instability over the longer term.

The theoretical framework outlined in Section 2 does not give clear predictions for the change in relationship stability post-reform. In particular, any reduction in relationship quality may be offset by the mothers' poorer outside options, which reduces the probability of a further separation and may therefore, result in no change in stability. Our results suggest that there is a fall in relationship quality that is not fully offset by the worse outside option in the short run, and are suggestive of a reduction in average relationship quality over the longer run.

6 Annual panel survey data

The administrative data allows us to examine the short- to medium-term effects of the 2006 reforms on repartnering. However, it is limited in terms of the insight it can provide into the characteristics of the relationships formed. We complement the administrative data with richer survey data from waves 1-15 of the Household, Income and Labour Dynamics in Australia (HILDA) Survey, covering the period 2001-2015. This is a nationally representative longitudinal survey based on an initial sample of 7682 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.

We select women whose relationships break down during the sample period and who have at least one child aged 15 or below at the time of relationship breakdown. All women observed living with a partner are 'at risk' of relationship breakdown. Relationship breakdown is observed when the woman is no longer living with the same partner in the following wave, and either (a) the reason for the partner being absent is stated as 'separation or divorce' or (b) the woman is living with a different partner. The month the former partner left the home is used to allocate separations to the pre- or post-reform period. Since this is an annual panel survey, we do not observe short separations and

reconciliations that occur between surveys and short episodes of repartnering that do not include the survey date.

Our estimation sample includes separations occurring between 2004 and 2009, allowing us to follow all mothers for six waves following separation. We see 240 mothers separating and have a total of 1318 post-separation observations for these mothers.³² The smaller sample size available means that a regression discontinuity approach is not feasible; we instead use a difference-in-difference estimation strategy. The post-reform separations are those occurring from July 2006 onward, as identified by the month the partner left the household. The treatment group consists of mothers with a youngest child aged 8 to 15, who are directly affected by the reform at the time of relationship breakdown. The control group is mothers with younger children at the time of separation, who would still be eligible for PPS conditional on meeting income and assets tests.³³

Table 10 shows summary statistics for this sample the last time they are observed in a relationship. Mothers in the treatment group are older, from households with higher (pre-separation) household income, more likely to be married, home owners and are more likely to be employed full time. These characteristics reflect the construction of the treatment status, which requires having a youngest child aged eight or more and so leads to a sample of mothers who are older. We also see that mothers in the treatment group are, overall, less likely to have repartnered four years after relationship breakdown than the younger control group, though mothers in the treatment group are more likely to have repartnered post-reform.

³²This gives an average of 5.5 post-separation observations per mother, meaning that attrition is low. 83% of mothers have all six observations.

³³Children's ages are reported only on 30 June of each survey year; the month and year of birth are not provided. We therefore do not know with certainty whether a child aged seven or eight on 30 June was aged seven or eight at the time of separation. Results presented assume that if the child is aged seven on 30 June and the separation occurs later than this, the child is aged eight at the time of separation and the mother is in the treatment group. This is a conservative approach that may underestimate the effects; the results presented here are robust to alternative assumptions.

Table 10
Summary Statistics: HILDA

	Control		Treatment	
	Child aged <8		Child aged 8-15	
	Pre	Post	Pre	Post
Year of separation	2005 (0.75)	2008 (0.81)	2005 (0.82)	2008 (0.96)
Age of mother	30.63 (6.77)	30.73 (6.66)	42.97 (4.80)	41.26 (5.50)
Age of youngest child	2.75 (2.23)	2.33 (2.24)	11.54 (2.14)	11.58 (2.78)
Number of children	2.15 (1.08)	2.16 (1.21)	2.05 (0.81)	2.06 (1.00)
Household income (\$000)	82.22 (45.27)	103.08 (68.31)	96.82 (97.24)	132.63 (161.78)
Employed full time	0.15 (0.36)	0.22 (0.41)	0.32 (0.47)	0.45 (0.51)
Employed part time	0.33 (0.47)	0.35 (0.48)	0.38 (0.49)	0.23 (0.43)
Unmarried	0.46 (0.50)	0.42 (0.50)	0.05 (0.23)	0.19 (0.40)
Indigenous	0.06 (0.25)	0.02 (0.15)	0.00 (0.00)	0.03 (0.18)
Born overseas	0.20 (0.40)	0.12 (0.32)	0.22 (0.42)	0.13 (0.34)
Home owner	0.48 (0.50)	0.54 (0.50)	0.78 (0.42)	0.74 (0.44)
Repartnered by 4 years	0.37 (0.49)	0.28 (0.45)	0.16 (0.37)	0.26 (0.44)
Observations	79	93	37	31

Notes: Standard deviations in parentheses. All values reflect pre-separation characteristics.

6.1 Repartnering effects: HILDA

We begin by verifying that the repartnering effect found in the administrative data is present in the survey data. We estimate the following difference-in-difference specification:

$$R_{it} = \sum_{\tau=1}^6 (\alpha_{\tau} + \beta_{1\tau}Treat_i + \beta_{2\tau}Post_i + \beta_{3\tau}Treat_i * Post_i) + \gamma X_i \\ + \theta_s + f(w_{it}) + \varepsilon_{it}$$

Here, R_{it} is equal to one if mother i has repartnered by year t after separation. We pool the estimation for all six post-separation waves with a full set of intercepts (α_t), treatment indicators ($Treat_i$), post indicators ($Post_i$) and the interaction terms that give the treatment effect estimators ($Treat_i * Post_i$). These allow the probability of having repartnered by t years post-separation to vary flexibly between the treatment and control groups and pre- and post-reform. The coefficients of interest are the set $\beta_{3,t=1}$ to $\beta_{3,t=6}$.

X_i is a vector of control variables measured before relationship breakdown, including education, household income, employment status, a set of indicators for the age of the mother and a quadratic function in the age of the youngest child. State-specific differences are captured by state fixed effects θ_s and calendar-time variation in repartnering (as opposed to variations in time since separation) are captured by a survey wave time trend w_{it} , which is interacted with the treatment group in some specifications. Standard errors are clustered at the individual level.

Results from this estimation are presented in columns (1) to (3) of Table 11. We see significant effects on the probability of repartnering from year 4 onwards, with the treated group 25 to 30 percentage points more likely to have repartnered by four to six years after separation. The inclusion of group-specific survey wave trends in column

(3) leaves individual coefficients statistically insignificant, but does not change the point estimates substantially. The set of six coefficients are jointly significantly different from zero at the 5% level. Whilst the point estimates are larger than those found with the administrative data above, the small sample size and low precision of these estimates means that they are not statistically significantly different.

Table 11
Effect of Reform on Repartnering (HILDA)

	Repartnered			Transitory			Persistent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Year 1	-0.045 (0.050)	-0.039 (0.062)	0.024 (0.193)	-0.015 (0.027)	-0.009 (0.035)	0.001 (0.107)	-0.050 (0.050)	-0.045 (0.064)	0.009 (0.185)
Year 2	0.111 (0.105)	0.124 (0.104)	0.185 (0.203)	0.031 (0.052)	0.031 (0.049)	0.041 (0.101)	0.079 (0.095)	0.084 (0.097)	0.139 (0.192)
Year 3	0.051 (0.125)	0.054 (0.118)	0.114 (0.216)	-0.021 (0.060)	-0.018 (0.053)	-0.008 (0.120)	0.077 (0.113)	0.072 (0.110)	0.126 (0.197)
Year 4	0.234* (0.134)	0.246* (0.125)	0.308 (0.223)	-0.029 (0.081)	-0.022 (0.072)	-0.012 (0.127)	0.233* (0.124)	0.240** (0.118)	0.295 (0.204)
Year 5	0.237* (0.140)	0.240* (0.130)	0.301 (0.226)	0.050 (0.092)	0.054 (0.084)	0.064 (0.142)	0.232* (0.134)	0.238* (0.127)	0.293 (0.207)
Year 6	0.294** (0.146)	0.297** (0.137)	0.359 (0.234)	0.037 (0.103)	0.043 (0.094)	0.052 (0.149)	0.305** (0.148)	0.291** (0.141)	0.341 (0.217)
Controls									
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Group trends	No	No	Yes	No	No	Yes	No	No	Yes
F-statistic	2.051	2.166	2.176	0.648	0.525	0.524	1.854	1.757	1.754
p-value	0.060	0.047	0.046	0.692	0.789	0.790	0.090	0.109	0.110
Observations	1318	1318	1318	1297	1297	1297	1252	1252	1252
Individuals	240	240	240	229	229	229	222	222	222

Notes: Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$. Controls: All regressions include observation year as a quadratic trend and state of residence fixed effects. Demographic controls include household income and its square, indicators for being employed full time, part time and unemployed, age of youngest child and its square, indicators for the number of dependent children in the household, indicators for being unmarried, indigenous, born overseas, being a home owner and renting privately, and a set of indicators for five-year age groups of mothers (< 25 , $25-29$, $30-34$, $35-39$, > 40). All demographic characteristics are measured before separation occurs and do not vary over time. Columns (3), (6) and (9) include separate linear trends for the treatment and control group. F-statistic reports the F-statistic from a test that the set of treatment effects are jointly significantly different from zero; p-value reports the p-value from this test.

Results from the difference-in-difference estimation are subject to many of the same concerns as discussed in the context of the administrative data above: changing selection into separation (including manipulation of the timing of separation), changing reporting behaviour, and the potential additional effects of grandfathering and work requirements. Moreover, the results rely on a common trends assumption: that the change in repartnering propensity for mothers with younger children can be used to construct a counterfactual trend in repartnering for mothers with older children. The inclusion of a set of pre-separation demographic controls means we assume the common trends assumption holds conditional on these observable characteristics. The inclusion of group-specific trends relaxes this assumption, though the small sample size means these results are not individually statistically significant.

The difference-in-difference approach allows us to net out the effects of grandfathering, as mothers with younger children separating after June 2006 are also under the new regime. We are less concerned about reporting behaviour for the survey data as responses to the survey do not affect payment eligibility, and so there is no financial motivation to misreport relationship status.

The possible change in selection into separation is more concerning, and is more difficult to test for in the small sample difference-in-difference context. Online Appendix Table C4 reports difference-in-difference estimates for the pre-separation characteristics.³⁴ When we control for state of residence and a quadratic time trend, we do see that mothers in the treatment group post-reform are younger and less likely to be married, which may suggest some changing selection into separation. However, the coefficients are not jointly significantly different from zero (p-value 0.31). We control for these observable characteristics in our regressions.

³⁴We estimate $Characteristic_i = \alpha + \beta_1 Treat_i + \beta_2 Post_i + \beta_3 Treat_i * Post_i + \theta_s + f(w_i) + \varepsilon_i$, and report estimates β_3 . With no changing selection on observable characteristics, these estimated coefficients should be insignificantly different from zero.

6.2 Are the new relationships better or worse? Evidence from HILDA

The richer data available in HILDA allows us to examine the characteristics of the new relationships caused by the reduced financial resources available to lone parents. We begin by repeating the analysis of repartnering with redefined outcomes. We define ‘transitory’ repartnering as a new partner observed in only one wave followed by separation, and ‘persistent’ repartnering as a new relationship observed in at least three consecutive survey waves.³⁵ Results from this analysis are presented in columns (4) to (9) of Table 11.

The results show that the repartnering caused by the 2006 reforms is predominantly persistent repartnering, with insignificant effects on transitory repartnering shown in columns (4) to (6). Coefficient estimates of persistent in columns (7) to (9) are comparable in magnitude to those in columns (1) to (3). These results are consistent with the analysis of the administrative data presented above, which found that additional repartnering in the first year after separation was unstable. The short-term unstable repartnering is unlikely to be captured in the annual panel data as both relationship formation and dissolution can occur between waves. This additionally explains why we do not estimate significant repartnering effects in the survey data until the later years post-separation.

We next consider the quality of the new relationships formed, as measured by self-reported life satisfaction, self-reported health, and the characteristics of the new partner. We additionally evaluate the potential mechanisms separating mothers may have used to find their new partner, including employment, socialising and volunteering.

For each outcome we define a binary variable indicating a ‘good’ outcome, for example reported life satisfaction above median, reporting good or excellent health, reporting some

³⁵In these analyses the sample is restricted to observations where it is feasible to assess these outcomes. So, for transitory repartnering we discard repartnering observations occurring in the last wave we observe a mother as we cannot observe whether these were transitory or persistent; we must observe the mother for at least 2 additional waves after repartnering to include that observation as persistent repartnering.

time spent volunteering, or having a partner with income above median for men in their state at that time. Online Appendix D describes the construction of these variables.

We first look for any evidence of a direct effect of the reform on these outcomes, estimating the effect of the 2006 reform on the binary outcome of being in the ‘high’ or ‘low’ group for those outcomes that are measured. We estimate:

$$\begin{aligned} Outcome_{it} = & \alpha + \beta_1 Treat_i + \beta_2 Post_i + \beta_3 Treat_i * Post_i + \gamma X_i \\ & + \mu_j outcome_atsep_i + \phi_t + \theta_s + f(w_{it}) + \varepsilon_{it} \end{aligned}$$

Here $Outcome_{it}$ is the binary variable representing the ‘high’ or ‘low’ value of the outcome, $outcome_atsep_i$ is individual i ’s report of the outcome at the survey wave immediately before separation, ϕ_t is a set of time-since-separation fixed effects and other regressors are defined as in Section 6.1 above. As t refers to the number of years after separation, and separations occur in different survey waves for different individuals, we are able to separately identify the time-since-separation fixed effects and wave-of-observation trends $f(w_{it})$. Coefficient β_3 provides a reduced-form estimate of the direct effect of the reform on this outcome.

We then estimate Multinomial Logit (MNL) models to assess the characteristics of the repartnering induced by the 2006 reforms. Post-separation outcomes are divided into three mutually exclusive groups: mothers who have not repartnered; mothers who have repartnered and have the ‘bad’ outcome variable (eg. low life satisfaction); and mothers who have repartnered and have the ‘good’ outcome (eg. high life satisfaction). The multinomial logit model estimates the probability p_{ijt} that individual i is in category j at time t after separation as follows:

$$p_{ijt} = \frac{e^{x'_{it}\beta_j}}{\sum_{l=1}^3 e^{x'_{it}\beta_l}}$$

$$\begin{aligned} x'_{it}\beta_j = & \alpha_j + \beta_{1j}Treat_i + \beta_{2j}Post_i + \beta_{3j}Treat_i * Post_i + \gamma_j X_i \\ & + \mu_j outcome_atsep_i + \phi_{tj} + \theta_{sj} + f_j(w_{it}) \end{aligned}$$

In both the estimation of direct effects and the multinomial logit models we consider outcomes for years 4 to 6 post-separation, where the repartnering effects are observed, and pool observations. All results presented include treatment-group specific linear trends. The model is estimated by maximum likelihood with standard errors clustered by individual.

Average marginal effects for the treatment group, evaluated at $Treat_i = 1$ and $Post_i = 1$, are reported in columns (2) to (4) of Table 12. These effects sum to zero, and show how being affected by the reform affects the probability of being in each outcome group. These results should not be interpreted as causal estimates of the reform on the relevant outcome, but descriptive evidence of the characteristics of the new relationships formed as a result of the reforms.³⁶

Panel A of Table 12 considers measures of the mother's wellbeing, including self-reported satisfaction with life, finances and their relationship with their children, as well as self-reported good health. In all cases, an outcome of one reflects the 'good' state. Column (1) shows that there is no statistically significant direct effect of the reform on these outcomes. Columns (2) to (4) report the marginal effects from the multinomial logit estimation, illustrating whether the treatment group are more or less likely than the control group to either have no partner (column 2), have repartnered and have the

³⁶Note that a causal interpretation of the MNL results assumes the independence of irrelevant alternatives, which is unlikely to hold in this case. For example, if we remove the 'option' of repartnering and having high life satisfaction, it is unlikely that mothers would instead have been allocated proportionally to the other categories.

Table 12
Evidence on the Quality of New Relationships: HILDA

Outcome	Competing risks marginal effects				Obs. (5)	Mean (6)
	Reduced form (1)	No partner (2)	Outcome=0 (3)	Repartnered Outcome=1 (4)		
A: Mother's outcomes						
Satisfaction: life	-0.140 (0.150)	-0.301** (0.145)	0.186 (0.114)	0.114 (0.110)	642	0.57
Satisfaction: finances	0.119 (0.130)	-0.247 (0.166)	0.148 (0.154)	0.099 (0.081)	642	0.19
Satisfaction: children	-0.249 (0.152)	-0.318** (0.159)	0.052 (0.059)	0.267* (0.145)	489	0.72
Good health	-0.172 (0.194)	-0.339* (0.173)	0.197 (0.153)	0.142 (0.133)	475	0.50
B: Partner-related outcomes						
Satisfaction: partner		-0.316* (0.174)	0.237** (0.094)	0.080 (0.179)	522	0.69
Partner income > median		-0.240* (0.144)	0.052 (0.129)	0.187* (0.098)	543	0.48
Partner in good health		-0.349** (0.161)	0.051 (0.083)	0.297* (0.166)	434	0.37
C: Repartnering mechanisms						
Employed	0.081 (0.132)	-0.254 (0.166)	0.037 (0.157)	0.217 (0.160)	642	0.68
Volunteer	0.150 (0.173)	-0.540*** (0.136)	0.097 (0.135)	0.443*** (0.061)	401	0.21
See friends > once pw	0.060 (0.190)	-0.261 (0.179)	0.158 (0.121)	0.103 (0.161)	485	0.60

Notes: Standard errors in parentheses. *p<0.1; ** p<0.05. Column (1) reports the reduced form effect of the reform on the relevant outcome from a linear probability model. Columns (2), (3) and (4) show average marginal effects for the treatment group, and should be interpreted as how the reform is associated with the probability of being in the relevant group. Estimation sample is for years 4-6 post-separation. All estimations control for the set of demographic characteristics as in Table 11, state fixed effects, a quadratic time of separation trend, group-specific linear trends, as well as the relevant outcome in the pre-separation period.

low value of the outcome (column 3), or have repartnered and have the high value of the outcome (column 4). Estimated marginal effects in column (2) are consistently negative, showing that affected mothers are significantly less likely than the control group to have no partner: they are more likely to have repartnered, reflecting the results of Table 11. Columns (3) and (4) show that there is no evidence of mothers who repartner due to the reforms reporting higher or lower satisfaction with their life and finances, or being more or less likely to be in good health. There is some indication of repartnered mothers being more satisfied with their relationship with children.

In Panel B, we report results for the mother’s report of their satisfaction with their partner, as well as an indicator for the partner having income above median, and their partner’s own report of their health status.³⁷ Focusing on columns (3) and (4), we see that mothers in new relationships resulting from the reform are less satisfied with their partner, potentially reflecting lower match quality. On the other hand, the relationships are characterised by partners who are more likely to have high income and be in good health. This provides suggestive evidence that affected mothers may have targeted new partners with more resources, but gain a lower relationship surplus due to their worse outside option.

Panel C provides some suggestive evidence about these mothers’ search efforts by examining whether these mothers are more likely to be employed, to report spending time volunteering, and to report spending time with friends outside of the household at least once per week. The results suggest that affected mothers are no less likely to be employed than previously but are significantly more likely to spend time volunteering. We do not, however, find any evidence of affected mothers seeing friends outside of the home more frequently. This suggests that employment and repartnering may be complementary responses to recovering from the financial shock of relationship breakdown. One interpretation is that mothers meet their new partner due to the social connections made through employment and volunteering.

³⁷For partner outcomes we do not have reports when the mother has not repartnered, so we cannot examine a separate reduced-form treatment effect.

7 Conclusion

This paper examines the effects of the 2006 Australian welfare reforms which replaced the principal welfare payment for lone parents with an unemployment payment, reducing the financial resources available to a subset of separating parents. We use biweekly administrative data, and focus on how the reform affected family structure through its impact on the speed of repartnering. The high frequency of the administrative data enables us to implement a regression discontinuity design, to evaluate the effects over the short- and medium-term, and to examine important dimensions of heterogeneity.

We show that the reform significantly increased repartnering rates for affected separating mothers. Six months after separation, post-reform separating mothers are six percentage points more likely to have repartnered than the pre-reform separating mothers, representing a 64 percent increase in the repartnering rate. The effect is persistent, with higher repartnering rates remaining four years after separation. The impacts are concentrated among mothers who were born in Australia and who had prior attachment to the income support system. We find evidence that repartnering induced by the reforms in the first year after separation was less stable. We do not, however, see any increase in relationship instability over the longer term.

We confirm these results with evidence from an annual panel survey, which further allows us to perform a complementary analysis of the characteristics of policy-induced repartnering. We find no evidence of lower life satisfaction for mothers who repartnered as a result of the policy, though there is some suggestive evidence that these mothers are less satisfied with their partner despite their higher income.

Our results deepen the understanding of how the design of the welfare system affects family structure. In contrast to welfare reform in the US, the 2006 Australian reforms generated a clear reduction in the financial resources provided to affected mothers and did not include a range of other features such as time limits and family caps. Compared to the UK's reforms to income support and tax credits in the late 1990s, the Australian

reform (weakly) reduced financial resources for all affected mothers and so our results do not incorporate offsetting incentives from different policies. Moreover, the high frequency administrative data means we are able to follow separating mothers from the point of separation, and do not rely on women without children as a control group. We are therefore able to identify the effect of a reduction in financial incentives on the time it takes for separating mothers to repartner.

These empirical results have implications for the design of welfare payments in Australia and elsewhere. The reform we examine was part of a package known as ‘Welfare-to-Work’, intending to increase the labour force attachment of lone parents and people with disabilities to attenuate the effects of the forecast decline in the working age population and to reduce the number of jobless households. Our results therefore highlight potentially unintended consequences of the reforms: affected lone mothers smoothed the financial shock of separation by repartnering more quickly, and these effects were strongest for mothers with prior reliance on the welfare system. This alternative response to the reduction in financial resources offsets the reform’s effectiveness in increasing labour force participation.

Further research is required to understand the broader implications of the policy. First, we need to consider the cost of and potential barriers to greater search intensity, particularly for relatively time- or resource-constrained mothers. Second, we need to evaluate the impact of the policy on other dimensions of maternal well-being such as health or security. Third, we can consider the impact of the policy for other groups such as the children of affected mothers and mothers who remain single. Given the body of evidence establishing the negative effect of the Welfare-to-Work reforms on mothers who remain separated (Brady and Cook 2015) it is important to see these repartnering effects as one component of the broader impact of the 2006 Australian reforms.

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