De facto marriage: when ending a cohabitation costs as much as a divorce.

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Abstract

I look at the effects of making the exit costs of cohabitation as high as divorce on new and existing partnerships. I exploit the Family Law Amendment Act, introduced in Australia in 2008, as an exogenous shock to the cost of exiting cohabitation. This law defines cohabiting partnerships as de facto relationships and makes the termination of a de facto relationship equivalent to a divorce. I hence exploit the time discontinuity produced by the reform to identify its effects on the stability of new and existing couples. I find that when terminating a cohabitation becomes as costly as getting divorced, (i) new unions are more stable (ii) existing cohabitors affected by the reform in their third year are more likely to split, while (iii) the probability of starting a cohabitation and the duration of premarital cohabitation do not change. This paper is the first to look at changes in the exit cost of cohabitation and it does it while disentangling the effect on new and existing partnerships.

1 Introduction

Despite the seminal work by Becker (1973) started a prolific research agenda on the economic aspects of marriage, the validity of applying methodologies conceived to study economic behaviour to family formation remains problematic. Already in the Nineteenth century, John Stuart Mill warned that while economics is solely interested in man's desire for wealth and its efficiency in obtaining it, there are situations in which such desire is in competition with other principles of human nature. Such non-economic motives, Mill maintained, act as *confounding causes*, potentially invalidating the deductive (*à priori*) results of economic models (Mill, 1994), as they do not take them into account. Mill's recommendations are still valid today. In particular, they justify the importance of empirically supporting or falsifying the theories that social scientists have constructed on how couples choose between informal cohabitation and marriage (see Cohen, 1987; Brinig and Crafton, 1994; Bishop, 1984). Indeed, the causal mechanisms at the heart of these theoretical models describing how and why we partner need further empirical testing (Matouschek and Rasul, 2008, being the only paper to my best knowledge addressing this issue). The scarcity of quasi-experimental evidence has left economists struggling to identify those causal links. This is critical, for instance, to understand what kind of legislation can promote the formation of stable unions.

This paper contributes to fill that gap in the literature, using a natural experiment to study how increasing the exit costs of cohabitation affects the formation and stability of couples. This is particularly relevant as the past decades have seen both household formation and dissolution changing dramatically in developed countries. As the demographic literature shows (see Perelli-Harris et al., 2017), while cohabitation has emerged as a new and trending way to live a romantic relationship, marriage has become less frequent and less stable. I look at the effects of making the exit costs of cohabitation as high as divorce and how this impacts new unions[1] and existing ones. Specifically, I exploit the Family Law Amendment Act, introduced in Australia in 2008, as an exogenous shock to the cost of exiting cohabitation.

The Family Law Amendment Act 2008 (Parliament of Australia, 2008) became active in 2009 and defined couples "living together on a genuine domestic basis"—i.e. acting as a married couple—as *de facto relationships*. Under the Family Law Amendment Act 2008, the termination of a de facto relationship carries the same consequences as getting divorced, for example giving to the spouses the right to seek a property settlement and spousal maintenance. I hence exploit the discontinuity in time produced by the reform to identify its effects on the stability of new and existing couples. I find that when terminating a cohabitation becomes as costly as getting divorced, (i) new unions are more stable (ii) existing cohabitors affected by the reform in their third year are more likely to split, while (iii) the probability of starting a cohabitation and the duration of premarital cohabitation do not change.

Previous research on the effects of changing the cost of ending a union has focused on marriage, particularly on reforms moving from mutual consent divorce to unilateral divorce regimes (Friedberg, 1998; Wolfers, 2006; Lee and Solon, 2011). In this paper, I add to that literature by focusing on changes in exit costs from cohabitation. This is particularly relevant as getting married after a period of cohabitation is increasingly becoming the norm. In countries like Norway, Spain (see Perelli-Harris et al., 2017) and Australia (Hewitt et al., 2005) this is already the case.

Furthermore, changing the cost of terminating a union affects new and existing couples in different ways. This has been shown analytically by Matouschek and Rasul (2008) in reference to divorce law reforms. In particular, on the one hand, new regimes will incentivise the formation of couples of a certain quality and disincentivise others, hence the composition of the couples starting a union before and after the reform will be different. They call this "selection effect". On the other hand, these policies change the incentives of already existing couples. They call this "incentive effect". While in their empirical analysis they are able to identify the incentive effect, they do not fully identify the selection effect.

[1] a couple that either cohabits or is married or both at different times

I test the hypothesis that a higher expected exit cost from cohabitation will deter low quality matches (couples) from entering cohabitation. This is a *selection effect* hypothesis. For these low quality couples, the expected probability of separation is high, hence their net benefit from cohabiting will decrease more given an increase in the exit costs. This implies that the average match quality is going to increase after the reform, observable as a lower probability to separate for new couples. I am able to separate the selection channel by comparing couples which started in the three years before the reform with those which started within three years from the reform and comparing them only for those periods in their relationships in which they were both under the reform. In particular, I can identify the selection effect by comparing couples that live under the same new legal regime but which started under different regimes.

On the incentive effect side, I improve on the identification the incentive channel by comparing couples that were in their j^{th} year of cohabitation just before the reform with the cohabiting couples that were in their j^{th} year just after. However, the timing and sign of the incentive effect is controversial ex-ante. Specifically, under the Family Law Amendment Act 2008, couples who decide to move in together are not automatically de facto relationships. They become de facto later on, but they do not know exactly when. Hence it is more challenging to make predictions on the incentive effect of the policy. Indeed, the reform applies only to cohabiting couples "living together under a genuine domestic basis". This concept is not well-defined, but it is more likely to apply the more the cohabitation lasts. Due to this difficulty in predicting the incentive effect, I take a more data-driven approach and estimate it using a flexible specification. I find that only cohabitors affected by the reform while in their third year of the relationship are more likely to separate that year. This might be a threshold emerging spontaneously, given the lack of a formal one. In other words, couples affected by the reform in their third year might see it as their last possibility to break up before being considered as married, causing lower quality couples to break up.

Other findings are more difficult to rationalise, particularly without departing from neoclassical assumptions, i.e. without introducing some behavioural assumptions. First, the duration of premarital cohabitation is not affected by the reform. On the contrary, once marriage and cohabitation are equalled and cohabitation loses its flexibility, we would expect premarital cohabitation time to significantly reduce for new couples. Secondly, the number of new cohabitations remains stable after the reform, while all standard models predict that it should change with the change in exit costs (Matouschek and Rasul, 2008).

This paper contributes to the literature in two ways. First, it is one of the first papers focusing on cohabitation regulations in a causal ways (Chiappori et al., 2017, is the only other one) and the first to look at exit costs from a cohabitation point of view. Secondly, it fully disentangles the selection and incentive channels, the two channels through which any family law reform affects unions.

The remainder of the paper is organised as follows. In Section 2 I give some definitions and present the reform of interest. I then present the data and summary statistics in Section 3. In Sections 4 and 5, I introduce the identification strategy for each of the two channels and then present the empirical specifications and the associated results and robustness checks. In Section 6, I study the transition from cohabitation to marriage and in Section 7 I estimate the changes in the probability of starting a new cohabitation. Section 8 concludes by summarising the findings and deriving some policy implications.

2 Background

2.1 Definitions

For the sake of clarity, it is important to define some concepts used throughout this paper. These are not new definitions. A *cohabitation* is defined as a romantic relationship in which the partners reside in the same dwelling. A *de facto relationship*, or *de facto*, is a cohabitation to which a legal recognition has been granted. Etymologically, the terms *de facto spouses* or *de facto marriage* indicate situations in which a couple is *in practice* living as if it were married. In the context of current Australian Commonwealth law, it indicates two individuals who "have a relationship as a couple living together on a genuine domestic basis" (Parliament of Australia, 2008).

Furthermore, the term *union* is used as a term including both cohabitation and marriage. This can be helpful, since in practice there is wide spectrum of long-term relationships, different in the meaning the couple gives to it. This continuum of union-types, can sometimes make categorisations arbitrary. For instance, a relationship that started as a cohabitation and then became a marriage is considered as a single union.

The term *separation* is both used in its legal meaning, as the moment in which a couple decides their marriage is over, and for indicating the termination of a cohabitation.

Lastly, the term 'period' is used when referring to the years of duration of a union (first, second, third, etc.), where confusion with calendar years might arise.

2.2 The 2008 Family Law Amendment

In 1984, the Parliament of New South Wales passed the De Facto Relationship Act (NSW Government, 1984), giving *de facto* partners virtually the same rights as married couples. Until recently, the Parliament of Australia could rule over married but not over *de facto* couples. It was only in November 2008 that the Constitution was modified to include within the power of the federal government the jurisdiction over *de facto* relationships matters. This allowed an amendment to the 1975 Family Law Act to be passed (Parliament of Australia, 2008) which effectively extended the De Facto Relationship Act NSW Government (1984) to the rest of Australia. This policy is interesting because (i) it grants de facto couples the same rights and duties as married couples and (ii) it does so through a loosely defined automatic mechanism. By this I mean that there is not a clear formal rule defining what constitutes a de facto relationship. Indeed, the Family Law Amendment (Parliament of

Australia, 2008) establishes that the circumstances defining a de facto "may include any or all of the following: (a) the duration of the relationship; (b) the nature and extent of their common residence; (c) whether a sexual relationship exists [...]" and other six criteria, whilst specifying that "no particular finding in relation to any circumstance is to be regarded as necessary in deciding whether the persons have a de facto relationship." I argue that this reform causes a behavioural change in those individuals starting a long-term relationship after its introduction.

3 Data

Household and individual data are taken from the *Household, Income and Labour Dynamics in Australia* (HILDA) Survey, which follows the lives of more than 17,000 Australians once a year, starting in 2001. Based on an initial sample of 7,682 households, it follows their lives over the generations, as the children of the initial families create new households. The sample was further extended in 2011, adding 2,153 responding households to counterbalance attrition. The Melbourne Institute designed and manages the study, which records information on a wide range of variables related to the economic life, the psychological well-being and the family dynamics of its participants.

3.1 New South Wales

As detailed in Section 2.2 New South Wales (NSW) was already under a legislation equivalent to the Family Law Amendment Act 2008, since 1984. This might make NSW seem suitable as a *control* state in a difference-in-differences setting. However, it is unclear whether the wave of media coverage of the 2008 reform constitutes for NSW a second treatment (after the 1984 one). If that were the case, the policy impact would be downward biased if not cancelled out. For this reason, I drop unions *started* in NSW when evaluating the selection effect and I drop unions *ongoing* in NSW when evaluating the incentive effect.

3.2 Descriptive Statistics

Table 1 summarises the composition of the unions in the selected sample. The sample includes only unions which formed outside of NSW and which began after the year 2000. It contains data on 3,963 unions of 5,350 individuals over 17 waves, between calendar years 2001-2017. The data is at the individual level, so that if both partners in a union are in the sample then the union is reported twice. The top part of the table reports the observed mean duration of unions, marriages, premarital cohabitation and cohabitation without marriage. Durations are short on average in part due to the right-censoring of the data. Furthermore, as shown in the the second part of the table, cohabitation makes up 60% of the unions. Their frequency, combined with their short average duration of 3 years, lowers the average duration of unions. In the *Partners' Characteristics* part of the table, the covariates used in the analysis are introduced. For categorical variables, the mode is reported rather than the mean. Birth cohort is an ordered categorical variable, grouping all the individuals born in the same decade. For instance, it is equal to 1970 if individual i was born between 1970-1979. The other covariates are only used in Section 4.6 to check if the results are robust to the introduction of divorce predictors common in the literature (see Hewitt et al., 2005). *Remoteness of area* takes integer values from 1 to 5, depending on how remote the area of domicile is. *Relative disadvantage* is a variable derived from one of the Australian Bureau of Statistics' socio-economic indicators for areas from the 2001 census (ABS, 2011). It takes integer values from 1 to 10, each representing which decile an area located in the index of socio-economic disadvantage is on. It is a summary of socio-economic characteristics. *Highest education* is a categorical variable on the highest level of education achieved, ranging from "Year 11 and below" to "Postgraduate". Finally, *Parents divorced* is a dummy variable equal to 1 if the parents are divorced, and 0 otherwise.

Table 1. Descriptive statistics

	Mean
Duration[1,2]	
Of unions	4.67 (4.52)
Of marriages	6.77 (4.47)
Of pure cohabitations[3]	3.87 (3.37)
Of premarital cohabitations	3.00 (2.10)
Unions[2]	
Union involves marriage	0.40
Marriage started with cohabitation	0.26
Union is purely cohabitational	0.60
Partners' Characteristics[2]	
Birth cohort	1980†
Remoteness of area	0=*major city*†
Relative disadvantage	4=4^{th} *decile*†
Highest education	5=*Certificate III or IV*†
Parents divorced	0.17
No. distinct unions	3,963
No. individuals	5,350

Note. The table includes only observations on couples whose relationship began outside of NSW and after the year 2000. The table reports mean and standard deviation (in parentheses) for the variables used in the analysis. The data is right-censored in year 2017. This implies that a union that is observed only cohabiting in the sample but will get married in 2021 is counted as purely cohabitational. Widowed single individuals are counted as married.

[1] Includes durations equal to zero, i.e. unions lasting less than a year.
[2] Unions are counted at the individual level. A couple counts as two unions.
[3] Cohabiting couples who never got married in the sample.
† Most frequent category (mode).

4 Selection Channel

4.1 Identification

The source of identification is the Family Law Amendment Act 2008, since it is exogenously passed in a particular moment in time. In an ideal setting, I would identify the selection effect by comparing unions formed just before the reform with unions formed just after. The unions in the two groups *live* under the same legal regime, that being the new one. They are only different in the legal regime under which their union *began*: the control group started under the old regime, the treated group under the new one. In the analysis, I keep all the unions that began in a 3-year window from either side of the reform's year 2009, i.e. between 2006–2011. The unions that began between 2006-2008 are assigned to the control group and the ones that began between 2009-2011 are assigned to the treatment group. The choice of a 3-year window is made in order to increase the sample size[2].

4.2 Sample Construction

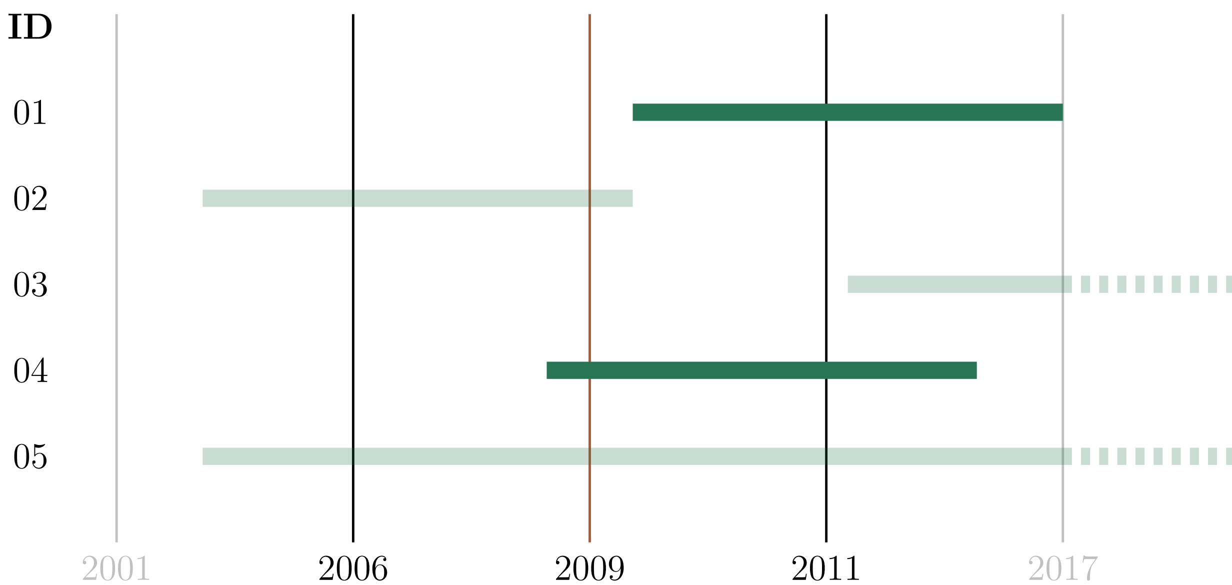


Figure 1. Visualisation of the selection channel sample

As visualised in Figure 1, the sample for separating the selection channel is constructed by only keeping unions that started in a 3-year window around the calendar year in which the reform became active (the darker lines). These unions are then followed over time. Those unions that started before 2009 are *always* in the control group, even after 2009, and those started during or after 2009 are always in the treatment groups, even after 2011.

[2] As shown is Section 4.6 results are robust to the use of a 2-years window

Furthermore, to better separate selection and incentive effect, the observations on the first two periods of all unions are dropped. Indeed, unions that began in 2007 remain in the old legal regime approximately two years before entering the new regime in 2009, during their third year. Unions started in 2006 remain for two years. In other words, those observations need to be dropped in order to exclusively compare unions *existing* under the same regal regime, which is the object of this section. Conversely, if those initial periods were kept, the comparison would include pre-reform unions during the old legal regime, thus introducing bias in the selection effect estimates.

In this sample, the unions that began as a marriage are dropped, as they are not affected by the reform on cohabitations. All the remaining unions start as a cohabitation, which then either becomes a marriage or does not. Each individual can only have one union at a time but more than one union over time.

4.3 Empirical Specification

To estimate the selection effect of an increase in the exit costs of cohabitation on union stability, I estimate the following regression equation using a linear probability model (LPM)[3]:

$$Pr[S_{j+1} = 1 | S_j = 0, D, \boldsymbol{X}] = \alpha_0(j) + \alpha_1(j)D + \boldsymbol{\beta X} \tag{1}$$

where S is a separation dummy, equal to 1 if the union ended at time j and 0 otherwise; D is a treatment dummy, equal to 1 if the union started strictly after 2008, 0 otherwise; it is flexibly interacted with period variable j, using a third degree polynomial, so that $\alpha_k(j) \equiv \gamma_{0k} + \gamma_{1k}j + \gamma_{2k}j^2 + \gamma_{3k}j^3, k \in \{0, 1\}$. Finally, $\boldsymbol{X}$ is a vector of birth cohort dummies (one per decade). Standard errors are clustered at the union level.

4.4 Results

Estimating Equation 1 on unions formed three years before and after the reform, I find that new unions are more stable (Figure 2). This is consistent with the hypothesis that the higher expected costs introduced by the reform deters the lowest quality matches from starting a cohabitation (see Section 2). Between the third and eighth year of their relationship, new unions are less likely to separate compared to the old unions formed in the three years before the reform. In particular, after the fifth year the effect is significant at 5% or less, with per-period point estimates around 2% (as can be calculated from Table 2). In other words, if we compare couples that lived both under the new legislation but formed under different ones, we find that the ones formed under laws imposing a higher cost of exit from cohabitation are more stable. This is consistent with the new law incentivising better matches on average.

[3]Results are virtually identical if a Logit model is used instead

Table 2. Selection effect: policy impact on new unions' per-period probability of separation

	(1) Separated
D	-0.0290
	(-0.14)
j	-0.129
	(-1.33)
$D \times j$	0.0180
	(0.14)
$j \times j$	0.0238
	(1.29)
$D \times j \times j$	-0.00432
	(-0.18)
$j \times j \times j$	-0.00143
	(-1.28)
$D \times j \times j \times j$	0.000273
	(0.19)
Birth cohort=1940	0.0445*
	(2.41)
Birth cohort=1950	0.0345***
	(3.73)
Birth cohort=1960	0.0372***
	(4.88)
Birth cohort=1970	0.0395***
	(5.88)
Birth cohort=1980	0.0402***
	(7.34)
Birth cohort=1990	0.0449***
	(4.50)
Constant	0.225
	(1.41)
Observations	7202

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note. This table presents the LPM estimates of the impact of the Family Law Amendment Act 2008 on the probability of separating for new unions. The interaction of the treatment D with a third degree polynomial of the union's duration j allows for a compact but flexible specification of the policy impact.

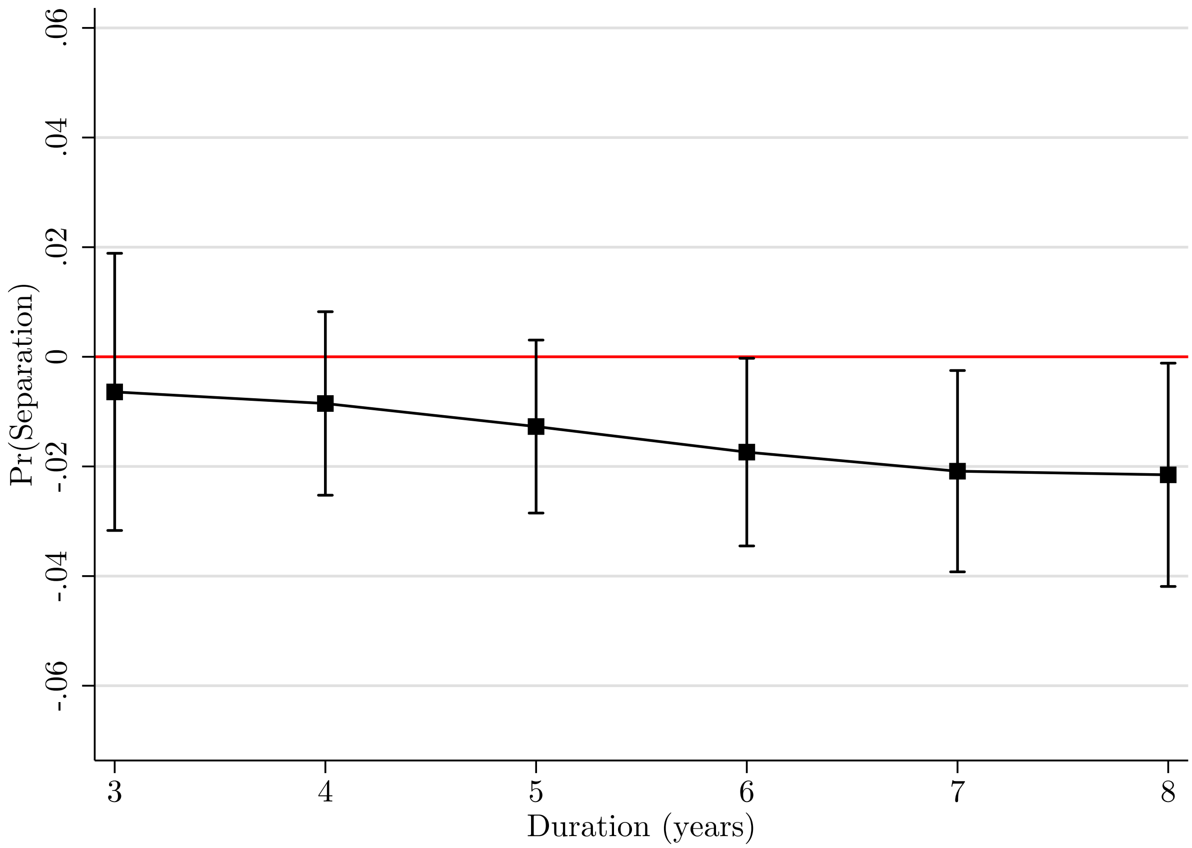


Figure 2. Selection effect: policy impact on new couples' per-period probability of separation

4.5 Separating the effect by union-type

This estimated effect on unions does not, by construction, separate between the impact of the reform on cohabitations-only unions and the impact on marriages that began as cohabitations. To do this, I estimate Equation 1, restricting the sample respectively (i) to cohabitation-only unions and (ii) to marriages that began as a cohabitation inside the 3-year window. When I look at unions which were pure cohabitations (Figure 3) I find a similar pattern as in the baseline analysis (Figure 2); after their fifth year, they are less likely to separate. The estimated effect is less precise but stronger, with new cohabitations 4% less likely to separate in period 6 compared to cohabitations that began before 2009, 5% less likely to separate in period 7 and 8% less likely to separate in period 8. Taken in isolation, these estimates are limited in that they only use information from cohabitations that either ended or that are censored, while discarding information from the cohabitations that became marriages.

If instead I restrict the baseline sample to marriages that began as cohabitations within the 3-year window, I still find a negative and significant 2% reduction in the probability of separation of new unions, but in their second and in third year of marriage (Figure 4). These estimates are again more limited than the baseline ones, particularly because I compare marriages that have premarital cohabitations of diverse lengths. This heterogeneity does not allow me to isolate the pure selection effect by dropping some initial periods, as done in Section 4.3. However, the sign and magnitude of these estimates suggests that the increase in match quality (as measured by stability) gained at the cohabitation stage has a positive impact on union stability also in the first years of marriage.

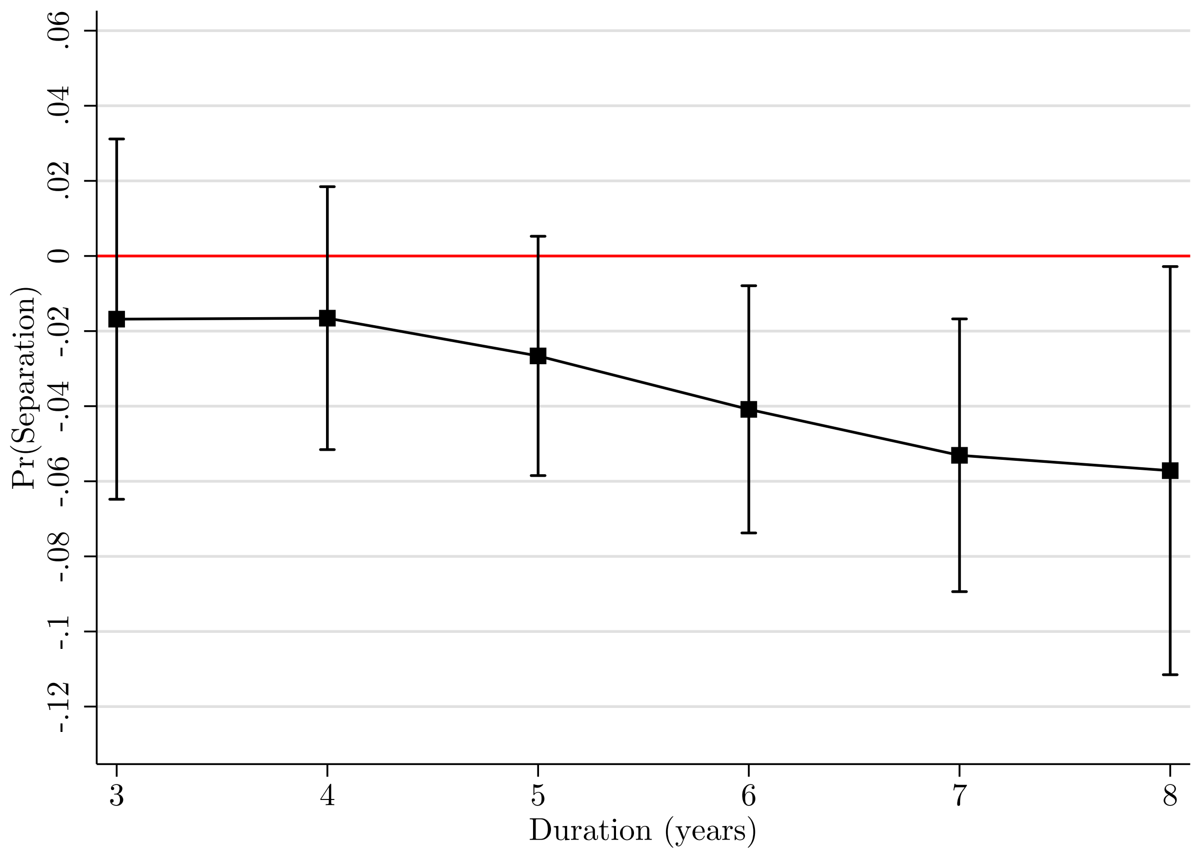


Figure 3. Selection effect: policy impact on purely cohabiting unions

4.6 Robustness Checks

Our policy of interest changes the legislation on cohabiting couples only, hence it should not affect unions which started as a marriage, without premarital cohabitation. Indeed, I find that the reform has no statistically significant selection effect on individuals who got married without cohabiting first (Figure 5). This is further evidence that the baseline results (Section 4.4) capture the causal effect of the policy of interest, as opposed to capturing some other general shock to relationship stability.

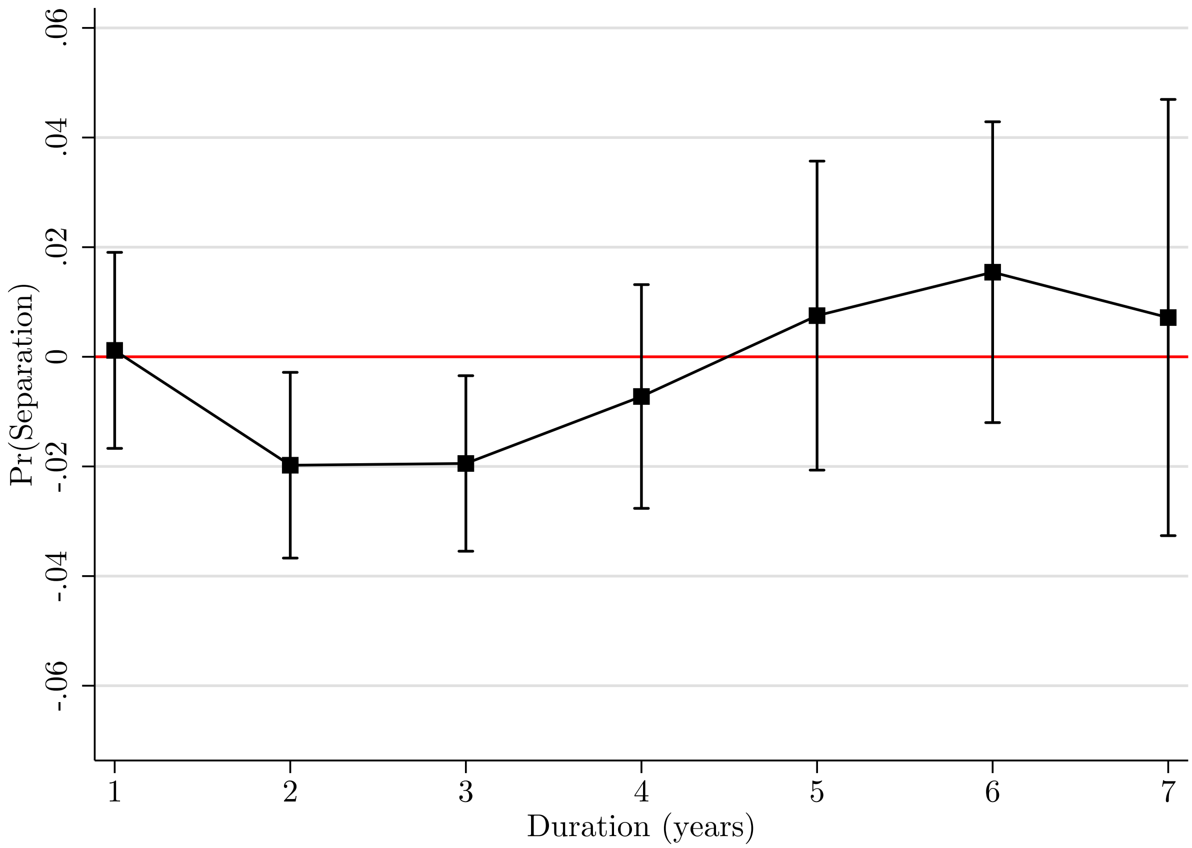


Figure 4. Selection effect: policy impact on marriages started as cohabitations

Adding covariates that are correlated with divorce can be useful for balancing the sample[4] (see the list in Section 3.2). I add them to Equation 1 and find that the estimated selection effect has a similar (per-period) magnitude compared to the baseline, but is now significant at 5% even in periods 4 and 5 (Figure 6). This suggests that the reform would produce more stable matches even if control and treatment group were identical in their observable characteristics predicting divorce.

Lastly, I test whether my findings can be replicated using placebo reform years (2003, 2004, 2005, 2006), in years antecedent to the 2008 reform. As expected, I find no selection effects in the previous years (7). This evidence suggests that the baseline estimates do capture the selection effect of the 2008 Family Law Amendment Act.

[4] Here I am using the full selection channel sample, which includes all the types of unions.

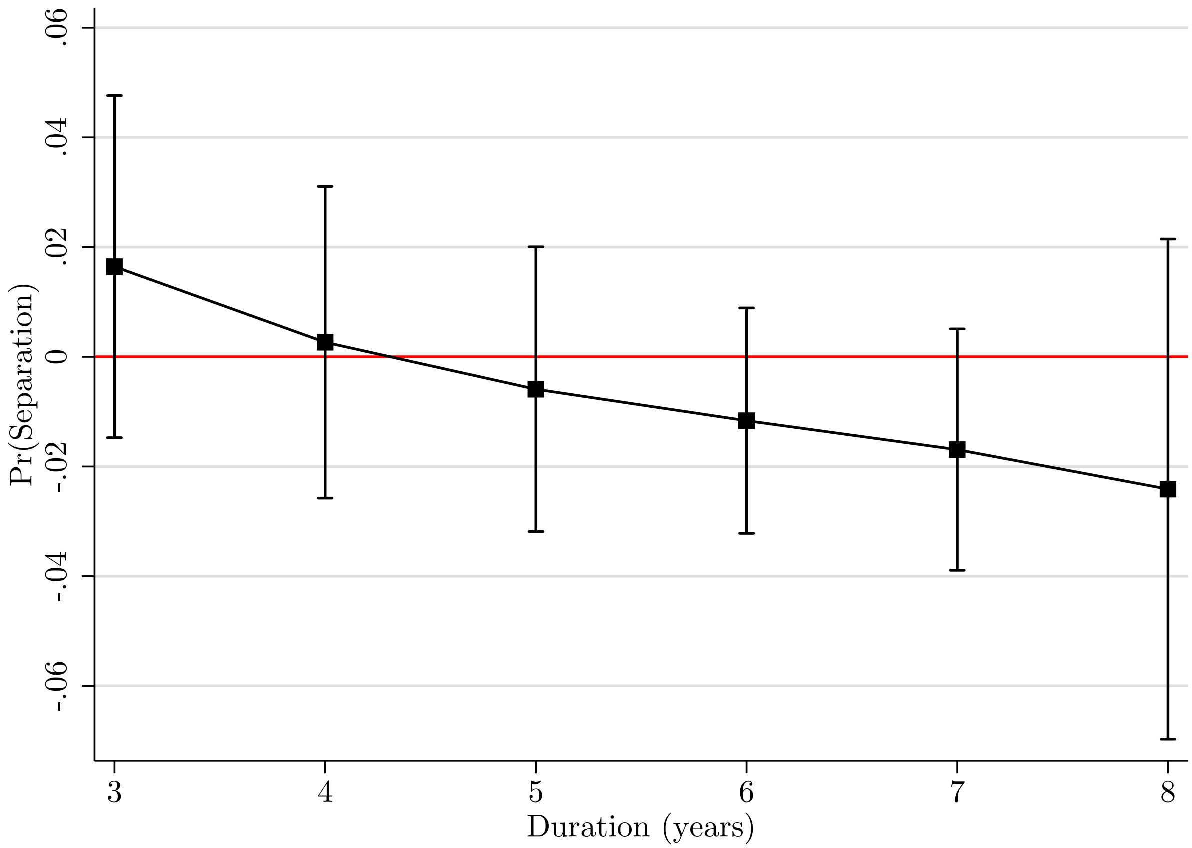


Figure 5. Selection effect: policy impact on purely marital unions

5 Incentive Channel

5.1 Identification

Imagine an experiment on cohabiting couples. All couples start cohabiting under a low-exit-cost regime — the old legal regime. After j years, a random group is assigned to a high-exit-cost regime: this is the treatment group. The remaining cohabitations form the control group and remain under the low-exit-cost regime. Estimand is then the effect of the reform on couples that are in their j^{th} period in 2009, when the reform becomes active. In other words, the aim is to compare unions that are affected by the reform since their j^{th} period with couples which were not affected in their j^{th} period. Both treated and control unions are already formed when the law changes, so they experience a change in their incentive structure *during* their relationship. Hence changes in their behaviour are attributed to changes in their incentive structure, in particular in their exit cost from cohabitation.

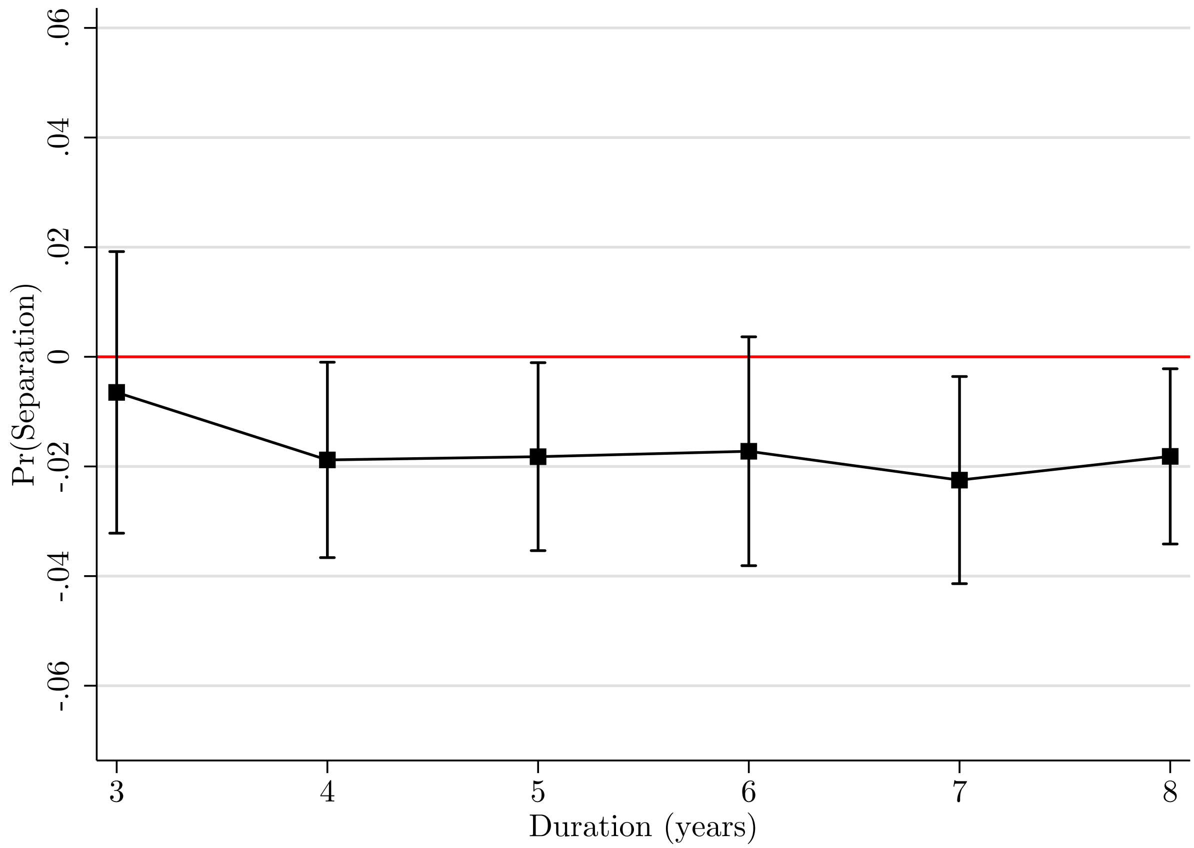
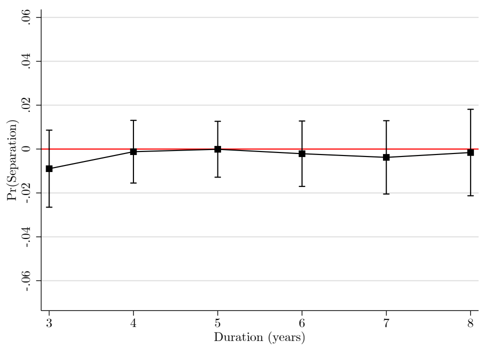


Figure 6. Selection effect: policy impact controlling for divorce predictors

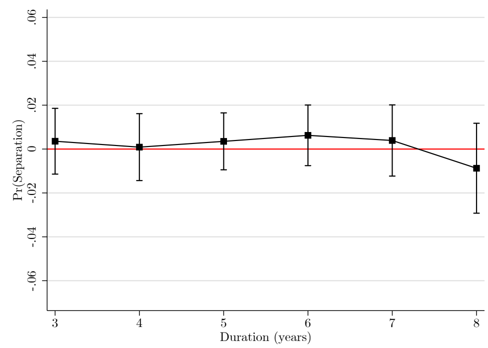
5.2 Sample Construction

My analysis in this section approximates such experiment by keeping only those unions that were in their j^{th} period within a 3-year window from the reform year, 2009. This is called a rolling window approach, because the 3-year window rolls back to "older" relationships[5] the higher the value of j. This can be visualised in Table 3, where, as the union's duration j increases, the coloured calendar years inside the window remain constant, while the years in which the union began decrease by one for each additional period of duration j. Notice that observations of cohabitations in period 1 and 2 are not used. Take period 2 as an example: it would be impossible create a subset of cohabitations that were in their second year in 2009 and that started in 3 different years (as necessary in order to construct a 3-year window). Similar issues apply to observations such that $j > 5$.

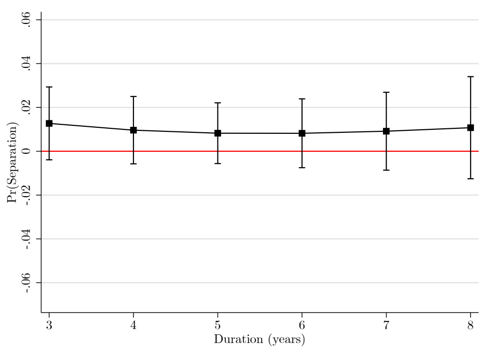
[5] Relationships that started before



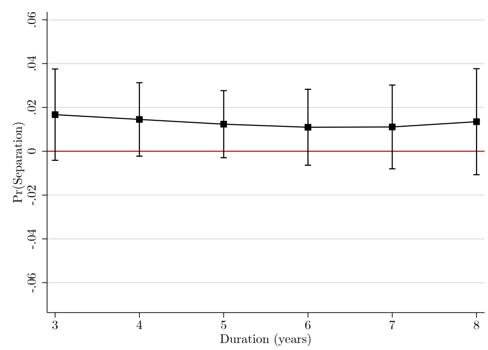
(a) 2003 placebo reform



(b) 2004 placebo reform



(c) 2005 placebo reform



(d) 2006 placebo reform

Figure 7. Selection channel: placebo effect

Table 3. Incentive channel: sample visualisation

year of start \ duration	3	4	5
2001	2004	2005	2006
2002	2005	2006	2007
2003	2006	2007	2008
2004	2007	2008	2009
2005	2008	2009	2010
2006	2009	2010	2011
2007	2010	2011	2012
2008	2011	2012	2013

Note. The table visualises how the sample was constructed. The values inside the table represent the current years. The 3-year window is coloured in grey for the control group and in brown for the treatment group. Each cell represent all cohabitations started in year of start s that in calendar year y were their j^{th} period of duration.

5.3 Empirical Specification

To estimate the effect of introducing the reform while an individual is in its j^{th} year of cohabitation, I estimate the following equation using the linear probability model (LPM)[6]:

$$Pr[S_{j+1} = 1 | S_j = 0, \bar{D}_j, M_j = M_{j+1} = 2, \boldsymbol{X}] = \beta_0(j) + \beta_1(j)\bar{D}_j + \boldsymbol{\beta_2 X} \tag{2}$$

where S is a separation dummy, equal to 1 if the cohabitation ended at time j and 0 otherwise; $\bar{D}_j$ is a treatment dummy, equal to 1 if the j^{th} period cohabitation takes place strictly after 2008, 0 otherwise; it is flexibly interacted with period variable j, using a third degree polynomial, so that $\beta_k(j) \equiv \gamma'_{0k} + \gamma'_{1k}j + \gamma'_{2k}j^2 + \gamma'_{3k}j^3, k \in \{0, 1\}$; M_j is a marital status categorical variable equal to 2 if a union is a cohabitation in period j. Finally, $\boldsymbol{X}$ is a vector of birth cohort dummies (one per decade). Notice also that unions cannot go from marital to cohabiting. Standard errors are clustered at the union level.

5.4 Results

Individuals who are in their third year of cohabitation in 2009 when the reform becomes active are 5% more likely to separate. Because the 2009 Family Law Amendment Act does

[6]Results are virtually identical if a Logit model is used instead

not define a cohabitation length after which it is classified a de facto relationship, it is impossible to predict a discontinuity around a specific threshold. However, it is also unlikely that a cohabitation that has lasted for several years would not be considered as a de facto. Given this lack of information, individuals might have relied on some rule of thumb, believing on average that any cohabitation longer than three years would not have been be considered a de facto relationship. If that were the case, an increase in separation rates in period 3 might come from partners in low quality matches who want to separate before they are treated as married. However, while New Zealand's Property (Relationships) Act 1976 sets the threshold for not being considered a de facto at the end of the third year of cohabitation, anecdotal evidence from Australia points towards a two-year one (Bryce, 2019). However, this is not part of the law and it could have emerged more recently. Indeed, if the two-year rule were followed immediately in 2009, we would have expected higher separation rates in the second year, close to the threshold for being considered a de facto.

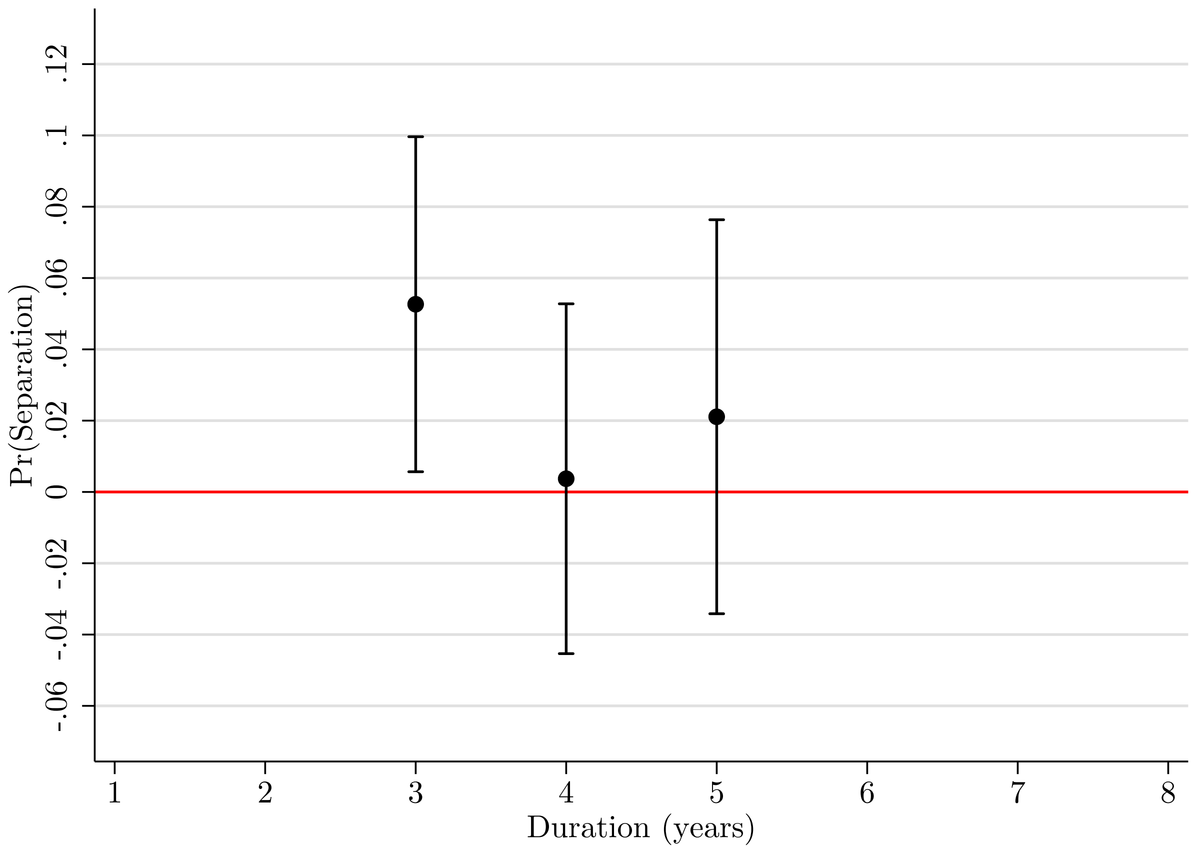


Figure 8. Incentive effect: policy impact on existing cohabitations' per-period probability of separation

5.5 Robustness Checks

To get closer to the ideal experiment, I restrict the window to 2 years, at the expense of the sample size. When the window is restricted to 2 years before and 2 years after the reform (2007-2010), the estimates hold, despite the loss in precision.

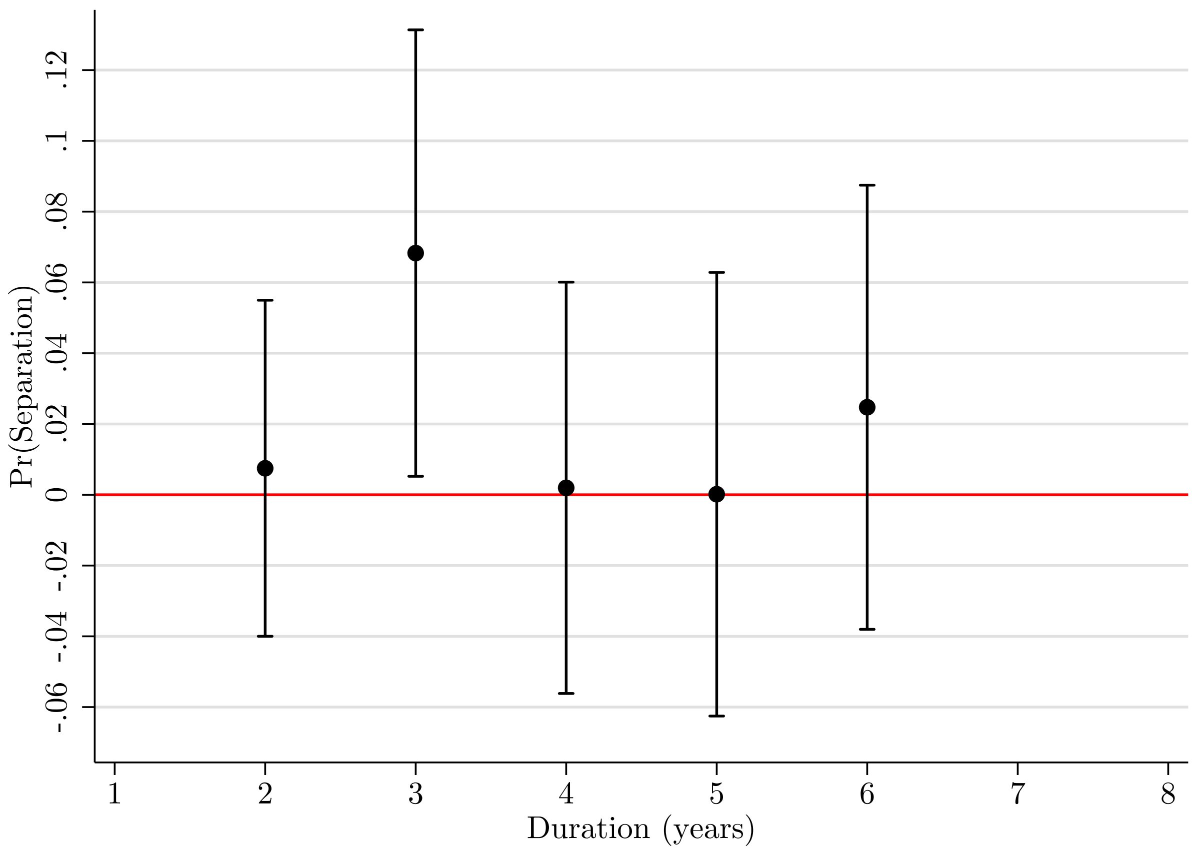


Figure 9. Incentive effect: policy impact on existing cohabitations using a 2-year window

Because the reform changes the incentives only for cohabiting couples, we should not observe any effect on married couples. To check if this is the case, I run a regression similar to Equation 2, but on a sample of married couples only:

$$Pr[S_{j+1} = 1 | S_j = 0, \bar{D}_j, M_j = M_{j+1} = 1, \boldsymbol{X}] = \beta_0'(j) + \beta_1'(j)\bar{D}_j + \boldsymbol{\beta_2'X} \tag{3}$$

where $M_j = 1$ for individuals married in period j of their union and the interpretation of the other parameters is unchanged from Equation 2. In line with the expected mechanism, I find that separation rates do not change for married couples when the reform becomes active in 2009 (Figure 10).

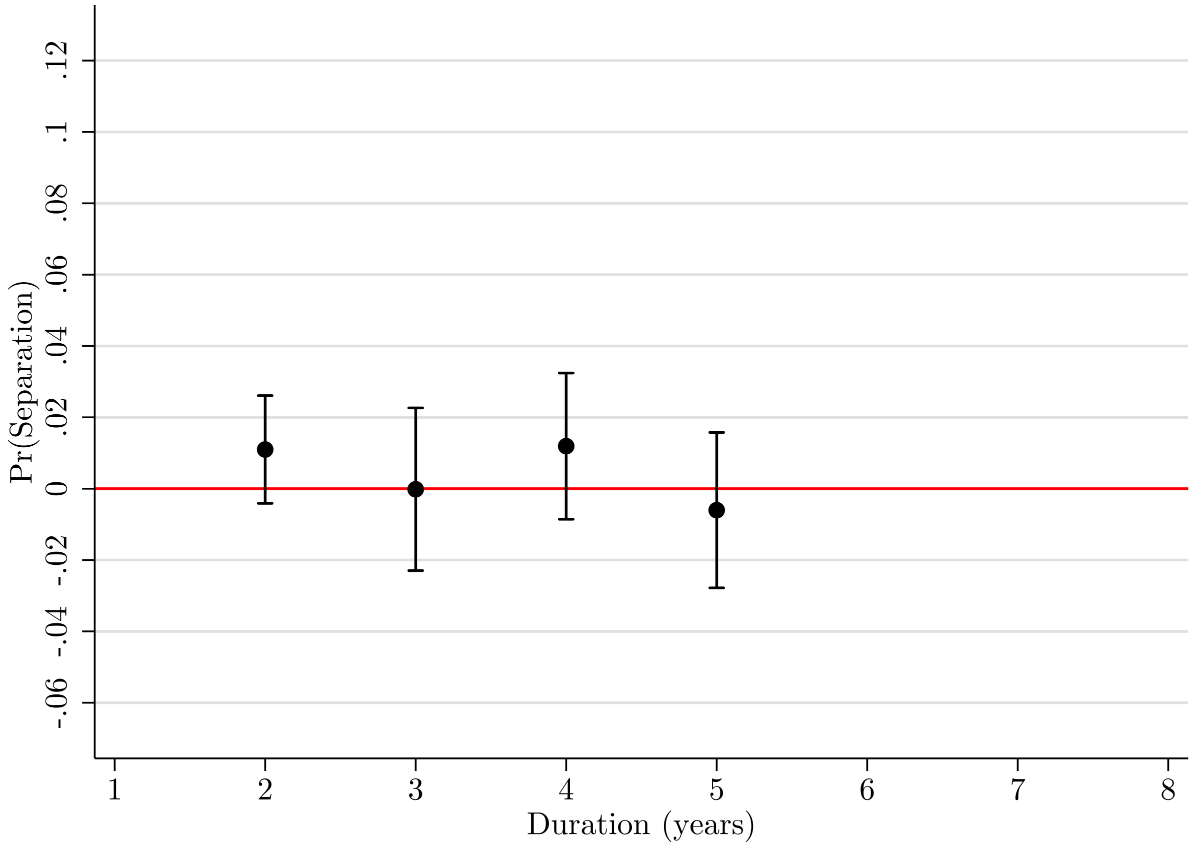


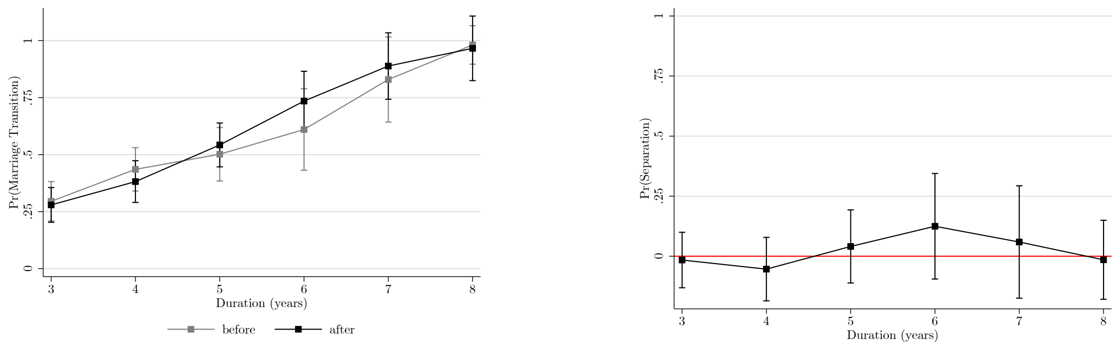
Figure 10. Incentive effect: policy impact on existing marriages' per-period probability of separation

6 Cohabitation-to-Marriage Transition

In this section, I test whether the reform has also shortened the phase of cohabitation for those partners who prefer to not get married immediately. Indeed, one might think that a reform that not only increases the cost of cohabitation, but makes it equal to marriage, could make shorter cohabitation phases preferable. Once the benefit of the flexibility provided by cohabitation is lost to the de facto status, it would be preferable to get married and enjoy the social benefits of it (Brien et al., 2006).

6.1 Sample Construction

To get some meaningful estimates on transition probabilities, I construct both a control and treatment group that include cohabitations that transitioned to marriage within the same number of periods. In particular, all cohabitations transition within their 8^{th} year of duration



(a) Transitioning before and after **(b) Policy effect on transitioning**

Figure 11. Probability of transitioning from cohabitation to marriage

and start between 2006-2011, with a 6-year time window, as in Section 4.3. This implies that by period 8 all cohabitation will have transitioned to marriage both in the treatment and in the control groups (by construction). Purely marital unions are dropped from the sample.

6.2 Empirical Specification

I use a specification identical to Equation 1 to test whether premarital cohabitation has shortened after the reform:

$$Pr[M_{j+1} = 3|M_j = 2, S_j = 0, Z = 1, \boldsymbol{X}] = \alpha_0''(j) + \alpha_1''(j)D + \boldsymbol{\beta''X} \tag{4}$$

where $M = 2$ if union is cohabitation and $M = 3$ if marriage; $Z = 1$ if a cohabitation eventually becomes marriage, while it is equal to 0 otherwise. The rest is identically defined as in Section 4.3.

6.3 Results

As Figure 11 shows, premarital cohabitations do not shorten after the reform. This means that the reform does not cause couples with a preference for a cohabitation phase to change the timing of their wedding. This result needs further study, as it can be easily shown that making terminating a cohabitation as expensive as divorce would induce a rational agent to prefer marriage to cohabitation in any model in which marriage provides an extra benefit relative to cohabitation. Restricting the window to 1 year, in order to estimate the effects for initial periods 1 and 2, provides further evidence of no effect (not shown).

7 New Cohabitations & Marriages

Given the claim that the observed increased stability found in Section 4 is due to the crowding out of low-quality matches[7], a decrease in new cohabitations is expected, or, similarly, a reduced probability for individuals (or dating couples) to start a cohabitation. A higher expected exit cost of cohabitation implies a higher match-quality threshold under which a non-cohabiting couple does not move-in. This implies that, ceteris paribus, finding a match good enough to justify starting a cohabitation should become rarer than before the reform.

The sample is constructed by using observations between 2003-2014 and keeping only the individuals aged between 16 and 60 in each year.

7.1 Empirical Specification

To estimate the probability of a non-cohabiting non-married individual to start a cohabitation, I estimate the following equation via LPM:

$$Pr[M_{j+1} = 2 | M_j = 1, \tilde{D}_t, \boldsymbol{X}] = \tilde{\delta}_0 + \tilde{\delta}_1 \tilde{D}_t + \tilde{\boldsymbol{\beta}} \boldsymbol{X} \tag{5}$$

where $M = 2$ if an individual cohabits and equals 1 if they single; $\tilde{D}_t$ is a treatment variable equal to 1 if $t \geqslant 2009$ and 0 otherwise, while $\boldsymbol{X}$ is again a vector of birth cohort dummies (one per decade), to control for generational differences. Standard errors are clustered at the individual level.

It can be noticed that this regression model is different from what seen in the previous sections. This is because here I am no longer looking at the expected duration before an event (separation) occurs. Instead, I am studying whether the yearly probability of an individual entering a union changes significantly after the reform is passed.

7.2 Results

Following the reform, the probability of starting a cohabitation does not change significantly (Table 4)[8]. This finding is inconsistent with most matching models, where higher exit costs crowd out the lower quality matches (see Matouschek and Rasul, 2008), which leads to a drop in new matches. To make these models consistent with my findings, one would have to make additional assumptions, for example higher exit costs making the search for a good match more efficient. In other words, assuming that high quality matches are scarcer than low quality matches, if the quality of new cohabitations increases but it remains as likely to start one, it means that the search for a match has improved on some level.

[7]Which is caused by the higher exit costs of cohabitation

[8]This result is robust to the use of a two-way fixed effect model.

Table 4. Probability of starting a cohabitation

	(1) New cohabitation
D	-0.00260
	(-1.73)
Birth cohort=1950	0.0101***
	(3.53)
Birth cohort=1960	0.0194***
	(6.68)
Birth cohort=1970	0.0372***
	(12.50)
Birth cohort=1980	0.0509***
	(17.91)
Birth cohort=1990	0.0340***
	(10.48)
Constant	0.0125***
	(5.16)
Observations	82145

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note. This table presents the LPM estimates of the impact of the Family Law Amendment Act 2008 on the probability of starting a new cohabitation.

8 Conclusion

As the way in which people partner changes, governments are called to respond to these changes. While politicians often introduce regulations aiming at simply ratifying them, we know that this action is hardly neutral to the outcomes. Indeed, I show that increasing the expected costs of terminating a cohabitation leads to more stable unions. The positive effects are not exclusive to cohabitation stability, but spill over into the following marital phase.

This is inconsistent with standard models of cohabitation and marriage, which assume that the agent can always rationally rank the different marital states (single, cohabiting, married) – even during a relationship. Hence standard models do not allow the possibility that cohabitation might create distortions to rationality by strengthening the romantic attachment of the partners, even in situations in which they do not form a good match. The psychological literature as put forward such "cohabitation inertia" hypothesis, as detailed in Stanley et al. (2006). Stanley et al. (2006) claims that cohabitation makes one more likely to marry her partner compared to a no-cohabitation scenario. My results are consistent with cohabitation inertia, providing evidence that policies improving the stability of cohabitation improve the stability of marriage too. This particularly applies to countries such as Australia where marriage is mostly preceded by a period of cohabitation.

Other findings are also difficult to reconcile without departing from neoclassical assumptions. First, the duration of premarital cohabitation is not affected by the reform. On the contrary, once marriage and cohabitation are equalled and cohabitation loses its flexibility, premarital cohabitation time for new couples is expected to drop. Secondly, the number of new cohabitations remains stable after the reform, while all standard models predicts that it should change with the change in exit costs (Matouschek and Rasul, 2008).

Finally, I find that existing cohabiting unions affected by the reform since their third year are 10% more likely to separate. This is consistent with the idea that the reform has an effect on match quality. The lowest quality existing cohabitors do not find worthwhile to maintain their union under the new exit cost regime, so they break up attempting to escape it.

From a policy perspective, my findings imply that governments should carefully consider how they decide to regulate cohabitation. In particular, making cohabitation a choice with important legal and economic ramifications can help to promote more stable households, to the benefit of all members.

From a research perspective, this paper shows how cohabitation laws can provide a precious opportunity to study the causal dynamics at the heart of household formation and dissolution. Establishing these causal links is the first step on the one hand to identify which models best explain these dynamics and on the other hand to update such models as to enable them to reproduce patterns observed in the data.

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