Changing the Rules Midway: The Impact of Granting Alimony Rights on Existing and Newly-Formed Partnerships*

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Abstract

The paper analyzes the effect of a reform granting alimony rights to cohabiting couples in Canada, exploiting the fact that each province extended these rights in different years and required different cohabitation length. A theoretical analysis, based on a collective household model with a matching framework, predicts that changes in alimony laws would affect existing couples and couples-to-be differently. For existing couples, legislative changes aimed at favoring (wo)men do benefit them, especially if the match quality is low. However, for couples not yet formed, they generate offsetting intra-household transfers (in our model, of leisure) and lower intra-marital allocations for the spouses who are the intended beneficiary. Our empirical analysis confirms these predictions. Among cohabiting couples united long enough before the reform, obtaining the right to petition for alimony led women to lower their labor force participation. These results, however, do not hold — and, in some cases, are reversed — for newly formed cohabiting couples.

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1 Introduction

Since the seminal work of Becker (1973), economists have greatly progressed in their understanding of household behavior, from both theoretical and empirical perspectives. In particular, several empirical studies have established that factors affecting spousal bargaining power, such as laws governing divorce, alimony and abortion, targeted transfers or the sex ratios in the markets for marriage, all affect the allocation of household resources (Duflo 2000, Angrist, 2002, Chiappori et al., 2002, Oreffice 2007, Attanasio and Lechène 2009, to cite just a few). In order to identify the impact of public interventions on intra-household allocation of resources, several of these studies exploit particular policy changes as natural (or quasi-natural) experiments. Typically, however, they do not distinguish couples already together at the time of reforms from those couples that formed subsequently.

Theory, however, strongly suggests that the impact of policy on outcomes is conditional on *when* partnerships are formed vis-a-vis changes in policy. For existing couples, policy changes would typically affect respective bargaining powers (and possibly divorce probabilities) in a straightforward way. Switching the post-divorce allocation of household wealth from private property to equal division, for instance, would generally favor the poorer spouse. But the case of couples who unite after the reforms is potentially more complex. If individuals are forward-looking, changes that affect future bargaining would be taken into account in the initial matching phase, resulting in a different initial allocation of household resources, and possibly changes in matching patterns and even premarital behavior (as discussed in Lafortune, 2011). Moreover, such effects may vary both through time and by the type and degree of commitment available to couples. In a nutshell, one would expect that individuals who are "caught" by policy changes would behave differently than those who are able to react *before* entering in a relationship; and the specific nature of such responses deserve both a *theoretical* analysis and an empirical investigation.[1]

In this paper, we present an empirical investigation of a reform that significantly affected the respective bargaining position of cohabiting couples. The reform that took place in Canada allowed,

[1] Rasul (2006) discusses the different effects the unilateral divorce reform on currently-married and newly-married couples in a model with non-transferable utility and search frictions.

for the first time, such couples to petition for alimony upon separation. A theoretical analysis, based upon an *integrated collective household* model where the matching process as well as the prospect of separation are embedded into the collective analysis, suggests a totally different impact depending on when the partnership was formed. These predictions are tested and confirmed by the data.

The Theoretical Setting The main ingredients of our model are as follows: There is a continuum of men and women who live for two periods. Each agent is characterized by a single attribute, income (or human capital), with continuous distributions of incomes on both sides of the matching market, so that each agent has a close substitute. The economic gains from marriage arise from the joint consumption of public goods and a non-monetary common factor that is match specific. For each couple, this match quality is revealed ex post and those with poor matches may divorce. Finally, we rely on a 'Becker-Coase' framework, and assume transferable utility (from now on TU) both during the union and after separation.

Using this theoretical framework, we consider a reform that increases the wives' share of incomes *after separation*, such as a universal increase in the mandatory (share of) alimony payments. We show that the short- and long-term consequences of reform are different and generally opposite of one another. For *partnerships already in existence* at the time of the legislative change, an increase in mandatory alimony payments can only improve the wives' welfare at the husbands' expense. In our model, this generates a lower labor supply and more leisure for women and more market work and less leisure for men. While the exact scope of the reform depends on assumptions regarding commitment, either some or all women will strictly gain and no woman can lose (equivalently, no man can gain). Regarding couples who unite *after* the reform, the logic is quite different, because the new divorce settlement is taken into account at the matching stage, resulting in a different intertemporal allocation of resources and welfare. Specifically, a change in alimony settlement laws aimed at favoring women will typically generate offsetting intra-household transfers, eventually resulting in *lower* intra-marital allocations for all married women. In our model, this manifests itself in the form of more market work and less leisure time for women, particularly, at an early stage of the union.

It is well known that such changes in post-separation property rights cannot affect separation probabilities in a Becker-Coase environment with transferable utility. However, it may (and does) influence the allocation of resources within a household, both before and after divorce — even among couples who do not eventually separate. At the end of our theoretical section, we explain how, in the presence of limits to transferability in spousal utility, reforms may affect the dissolution rate, particularly for couples who were already together *before* the new laws were enacted. However, the impact on partnerships formed *after* the legislative changes should be much smaller.

The Empirical Investigation Our empirical strategy exploits legislative changes that granted the right to petition for alimony upon separation for cohabiting couples in Canada. These new laws, enacted between 1978 and 2000, were implemented at different times in different provinces with different eligibility rules — a feature that enables us to convincingly estimate the causal effect of these rules using a triple-difference strategy. Furthermore, one can easily distinguish between couples who started their relationships before and after the legislative changes. Our empirics thus compare the causal estimates of granting alimony rights to partnerships that were already in existence when the new rules were implemented, with those that potentially reflect how individuals responded to these changes before entering a union.

Finding the impact of such a legislation is not easy as there is an obvious endogeneity problem: regions that implemented such a rule may have been distinct from those that did not; there could also be self-selection in whether or not couples are subject to reforms. Furthermore, in the case of cohabiting couples, few countries have implemented rules with variations which allow the construction of a credible "control group" for the estimation of a causal impact (see Rangel, 2006, for a notable exception).

The context studied here is particularly interesting because not only were "common-law spouses" — as cohabiting couples are called in Canada — granted alimony rights at different time periods in different provinces, but also each province defined differently the length of cohabitation required to qualify for such rights. This provides a very rich source of variation for our analysis in which we employ a triple-difference strategy (based on province, time and relationship duration) in order

to identify the causal impact of the change in cohabitation laws. Furthermore, many of these legal changes were implemented not in response to a demand from cohabiting couples but as a way to offer homosexual couples — who, at the time, were unable to legally marry — the same legal protection as their heterosexual counterparts, thus diminishing the potential problem of endogenous adoption of the laws.

Using a model in which the intra-household distribution of resources are manifested in changes in spousal leisure and labor supply (something that has been employed previously[2] but mostly in the context of married couples[3]), we directly search test if alimony rights influence spousal bargaining power. Alimony rights are not gender-specific in the legislation but in practice were rarely granted to men who were usually the higher earning spouse. The results we obtained here suggest that, as cohabiting relationships were granted alimony rights, women were more likely to attend school and stop working and less likely to work full-time whereas men became more likely to work and less likely to study or have work interruptions. Leisure being a normal good, such findings strongly suggest that the reform triggered a change in intra-household allocations favoring women.[4] These results hold for a given relationship over time, but they do not apply to individuals who were *married* who were unaffected by the new laws.

Another advantage of this setting is that we can contrast how these laws impacted differentially couples that were already in a relationship that qualified for the new rules at the moment of the legal change (as in Voena, 2011) with those who formed after the reform was in place. We find contrasting outcomes for the new alimony rights' impact on the behavior of cohabiting couples who entered a union *after* the alimony rights were granted. Among such couples, the impact of the law is limited; and when observed, it is women — and not men — who were less likely to study and to have fewer work interruptions, while being more likely to have worked or worked full time. This is consistent with our empirical framework and does not seem to be driven by selection, as the characteristics of individuals or couples entering into such unions after the legislative change do not appear to have differed from those previously electing this form of relationship.

[2]See for instance Chiappori (1992), Chiappori et al. (2002) and Oreffice (2007).

[3]With the notable exception of Oreffice (2011).

[4]In this paper, we do we treat studying as a form of leisure. Thus leisure is defined broadly as any time use that directly benefits one of the spouses.

Our empirical results also suggest that the institution of alimony rights for cohabiting couples led to longer periods of cohabitation but also to fewer of these unions eventually evolving into marriage. This appears to be economically and statistically significant only for couples who were matched before the legislative changes occurred; we see no such effects among couples who entered a cohabiting union after the new laws were introduced.

The Related Literature These results contribute to our understanding of the dynamics between cohabitation and marriage, a topic that has been mostly the focus of sociologists and demographers (see Smock, 2000, for a review and Gemici and Laufer, 2010, for an exception in economics). Also, there are studies which document that children who live in cohabiting households perform worse in most measures (see Manning, 1995, 2001). Amador and Bernal (2008) attempt to correct for the obvious endogeneity problem in these comparisons, but still find that children with cohabiting parents have worse outcomes than those with married parents in Colombia (despite the fact that both types of households have the same rights there).

To the best of our knowledge, the only paper that has explored the impact of alimony rights on cohabiting households is Rangel (2006), who also finds that such a rule decreased female labor supply among existing unions. He obtains a causal estimate of granting alimony rights to cohabiting women in Brazil by using the fact that couples with children obtained such a right, but not those without. The identification assumption we use here has the advantage of relying on a much more similar control group through the use of a tripe-difference estimator. Most important, however, is the fact that we estimate the effects of changing spousal bargaining power in existing unions and compare them with those in relationships yet to be formed.[5] In doing so, we are able to theoretically identify and empirically document elements of policy neutrality in intra-household allocations, which is the novel contribution of what lies ahead.

An empirical application of our main 'marriage-market induced policy-neutrality' idea in an economic development context is provided by Ambrus et al. (2010). They document that *mehr,* a form of Islamic bride-price, which functions as a prenuptial agreement in Bangladesh due to

[5]Our results also complement studies that investigate the impact of changes in divorce laws on divorce rates in the United States, such as Peters (1986), Friedberg (1998, Chiappori et al. (2002), and Wolfers (2006).

the practice of it being only payable upon divorce, influences *dowries* positively in the marriage markets.

The rest of our paper is organized as follows: Section 2 presents our theoretical framework. Section 3 summarizes the legal setting and the data available for our analysis. Section 4 discusses our estimation methodology and our baseline empirical results. The following section then explores some other empirical implications of our framework. Our final section concludes.

2 The Model

We first present a stylized model of the 'cohabitation matching market' intended to illustrate why the impact of the granting of alimony rights should be different for couples already in a union at the time of the legislative change than for couples who unite after the law is enacted. Our model is based on a frictionless matching framework a la Becker-Shapley-Shubik; the reader is referred to Browning et al. (in progress) for a general presentation. Ours differs from alternative models such as that of Lundberg and Pollak (1993) in that commitment is not the driving force of our results but rather the existence of a competitive market that discipline the allocations between spouses. Also, in the theory section, we will not distinguish between marriage and cohabitation. And often times we shall use the terms 'husband' and 'wife' loosely to refer to the man and the woman in a cohabitation relationship, respectively.

2.1 Endowments

The economy is made up of individuals who live for two periods. They are characterized by their efficiency units of labor endowment, y for men and z for women. We assume that men's wage rate equals unity and we define women's wage rate as w, $w \leqslant 1$, so that w also represents a measure of the gender wage gap. There exists a continuum of men and a continuum of women. The measure of men is normalized to unity and, because there are typically more women than men in the "marriage market" among the young, we set the measure of women to r, where $r > 1$.

The endowments of men, y, are distributed over the support $[y_m, y_M]$, $0 < y_m < y_M$, according

to some distribution F. Similarly, the endowments of women, z, are distributed over the support $[z_m,\ z_M]$, $0 < z_m < z_M$ according to the distribution G. These endowments are fixed over time. The potential per-period incomes upon working full time are given by the endowments multiplied by the wage, y for men and wz for women.

Following separation, there can be income transfers (i.e., alimony payments) between the ex-spouses. We assume that these transfers are fully determined by law and no further voluntary transfers are made. Redistribution corresponds to a legal approach where property incomes or spousal earnings are treated as a common resource and each spouse has some claim on the income of the other. Specifically, if a man with income y marries a woman with income wz, her income following separation is $wz' = \beta(y + wz)$ and his income is $y' = (1 - \beta)(y + wz)$. Thus the net income of a 'divorced' person is generally different from what his or her income would have been had he or she not paired up. The special case in which all incomes are considered private, implying no redistribution via alimony payments, is represented by a β that is couple-specific, namely $\beta \equiv \frac{wz}{y+wz}$.

Given that we abstract from savings and the accumulation of human capital, the distinction between the post-divorce division of property and alimony payments is mostly semantic here. But one can interpret the variables y' and wz' as the stream of incomes generated from the (underlying) assets of the couple which were redistributed according to the alimony laws that apply in legal separation.[6]

2.2 Preferences

In each period, individuals derive utility from consumption of n private goods, $q^1, ...q^n$, N public goods $Q^1, ..., Q^N$ and leisure, l. Couples also derive satisfaction from the quality of their match, θ. For any couple, match quality θ is drawn from a fixed distribution Φ with a mean $\bar{\theta} \geq 0$. Upon union, both partners expect to derive the same non-monetary utility from cohabitation, $\bar{\theta}$. At the end of the first period, the match quality is revealed; a realized value of θ that is below the expected

[6] A more general model would endogeneize savings and human capital investments, as in Chiappori, Iyigun and Weiss (2009). In our context, however, endogenizing these decisions would significantly increase the complexity of the model without radically changing our main point. See also Mazzocco and Yamaguchi (2007) and Stevenson (2008).

level $\bar{\theta}$ constitutes a negative surprise that may trigger separation.

In order to remain within the standard 'Becker-Coase' framework, which relies on transferable utility, we assume that preferences of single individuals are *strictly quasi-linear*

$$u_i^s(q_i, Q) = l_i + B_i^s(Q, q_i), \tag{1}$$

while preferences of cohabiting or married partners are *generalized quasi-linear* (GQL)

$$u_i(q_i, Q) = A(Q) l_i + B_i^m(Q, q_i) + \theta. \tag{2}$$

These two assumptions are necessary to generate the Becker-Coase benchmark where, in a static context, divorce laws do not affect divorce probabilities.[7] Since one of our primary objective is to explore if and when alimony laws affect separation rates, we adopt these preference specifications as our benchmark.[8]

Having made these assumptions, we obtain a very simple framework with two basic features. First, singles' utility can be measured by their income following divorce and the total utility of cohabiting partners is a convex function of their total income. To see that, note, first, that, by strict quasi-linearity, the optimal levels of all consumption goods of singles $\left(\bar{Q}^1, ..., \bar{Q}^N, \bar{q}_i^1, ...\bar{q}_i^n\right)$ are independent of income. We can then choose units such that $B_i^s\left(\bar{Q}, \bar{q}_i\right) = \sum_{j=1}^N P^j \bar{Q}^j + \sum_{k=1}^n p^k \bar{q}_i^k$, $i = h, w$, implying that the indirect utility of a single person equals his or her income. Second, if a man with a y endowment is matched with a woman with a z endowment, they can pool their incomes. Under GQL there is a unique efficient level for the consumption of each of the public goods and each of the private goods and these levels depend only on the *total income* of the couple, $t \equiv y + wz$. The location of the Pareto frontier also depends only on total family income and is given by

$$u_h + u_w + 2\theta = \eta(t) + 2\theta \ ,$$

[7] See Bergstrom (1989), Clark (1999) and Chiappori, Iyigun, Weiss (2007).

[8] To simplify our analysis and to ensure positive assortative matching, we further assume that B_i^s, $i = h, w$, are increasing concave functions, with $B_i^s(0) = 0$ and that A and B_i^m, $i = h, w$, are positive, increasing, concave functions such that $A(0) = 1$ and $B_i^m(0) = 0$.

where u_h and u_w are the attainable utility levels that can be implemented by the allocations of leisure between the two spouses, given the efficient consumption levels of all other goods and $\eta(t)$ is *increasing and convex* in t.[9] It follows that the two individual traits (y and z) of a couple are *complements* within the household. This generates positive economic gains from marriage, $\eta(t) - t$. That is, the material output $\eta(t)$ the partners generate together exceeds the sum of the outputs that the partners can obtain separately. Moreover, the marital surplus rises with the total income of the partners, t.

A special feature of our model is that utility is transferred through changes in leisure rather than consumption. This assumption is natural in our context, if only because our data provide information on leisure-work choices but not on consumption. More importantly, any Transferable Utility (TU) model implies that the household demand for all commodities *except the transfer good* is exactly determined by efficiency considerations, and is therefore independent of power considerations (such as the share of income that spouses receive following divorce). Since we want to preserve the tractability of a TU framework, our assumption is in line with the empirical evidence (including the results presented below) which shows that labor supplies do respond to changes in power relationships.

2.3 Stable Matches and Expected Lifetime Utilities

Formation and dissolution of partnerships In the first period, all men and women wish to 'marry' because the expected economic and non-monetary gains from a union are positive and $\bar{\theta} \geq 0$. At the end of the first period, the true value of match quality is revealed and each partner of a couple (y, z) can decide whether or not to stay together based on the realization of θ. Because utility is transferable, the Becker-Coase theorem applies and separation occurs whenever the total surplus generated outside the relationship is larger than what can be achieved within it. Denoting

[9] By the envelope theorem, the derivative $\eta'(t)$ is equal to $A(Q)$. Therefore, η is increasing in t and, if $A(Q)$ is increasing in t as well, then η is convex. Note that a sufficient (but, by no means, necessary) condition is that public consumptions are all normal.

total income of the partners by $t = y + wz$, separation occurs whenever

$$\eta(t) + 2\theta < t \Leftrightarrow \theta < \hat{\theta}(t) = -\frac{1}{2}[\eta(t) - t] \ . \tag{3}$$

In words, a union dissolves if the sum of the outside options, here t, exceeds $\eta(t) + 2\theta$, implying that reservation utilities are outside the Pareto frontier if the partnership continues. On this basis, the ex-ante probability of divorce for a couple with endowments of y and z is

$$\alpha(t) \ \equiv \ \Phi[\hat{\theta}(t)] \ . \tag{4}$$

Note that the threshold $\hat{\theta}(t)$ decreases with the income of the couple, t, and consequently the probability of divorce $\alpha(t)$ declines.

The expected marital output (i.e. sum of utilities) generated over the two periods is

$$S(t) = \eta(t) + 2\bar{\theta} + [1 - \alpha(t)] \left\{ \eta(t) + 2E\left[\theta \mid \theta \geq \hat{\theta}(t)\right] \right\} + \alpha(t)\, t \ . \tag{5}$$

Note, first, that $S(t) > 2t$, because $\eta(t) \geq t$ and $E\left[\theta \mid \theta \geq \hat{\theta}(t)\right] > \bar{\theta} \geq 0$. Thus, all individuals prefer to get married rather than stay single. Secondly, $S(t)$ is increasing in t, hence in each partner's income. In particular, whenever women strictly outnumber men so that $r > 1$, women at the bottom of the female income distribution remain single. Finally, individuals sort positively into unions. Indeed, since the 'marriage' surplus only depends on total income t, the cross partial $\partial^2 S / \partial y \partial z$ is equal to $S''(t)$. One can readily prove that $S(t)$ is convex and therefore that the traits of the two partners are *complements* even after the risk of separation is taken into account.[10]

Who Matches with Whom? Given transferable utility and the complementarity of individual incomes in generating a surplus, a stable assignment must be characterized by *positive assortative matching*. That is, if a man with an endowment y is matched with a woman with an endowment z, then the mass of men with endowments above y must exactly equal the mass of women with endowments above z. This implies the following spousal matching functions:

[10] See Appendix A for the proof and Chiappori, Iyigun and Weiss (2008) for further details.

$$y = F^{-1}\left[1 - r\left(1 - G\left(z\right)\right)\right] \equiv \phi\left(z\right) \qquad \text{and} \qquad z = G^{-1}\left[1 - \frac{1}{r}\left(1 - F\left(y\right)\right)\right] \equiv \psi\left(y\right). \tag{6}$$

For $r > 1$, all men are married and women with endowments below $z_0 = G^{-1}\left(1 - 1/r\right)$ remain single. Women with endowments exceeding z_0 are then assigned to men according to $\psi(y)$ which indicates positive assortative matching.

Positive assortative matching has immediate implications for the analysis of separation. Because separation is less likely when a couple has higher total income and individuals sort into unions based on income, individuals with higher income are less likely to separate.[11]

Stability Conditions The allocations that support a stable assignment must be such that the implied expected lifetime utilities of the partners satisfy

$$U_h(y) + U_w(z) \ \geq S(t) \ ; \qquad \forall \ y, \ z \ , \tag{7}$$

where $U_h(y)$ and $U_w(z)$ respectively represent the expected lifetime utilities of the husband and the wife over the two periods. For any stable union, equation (7) is satisfied as an equality, whereas for a pair that is not matched, (7) would be satisfied as an inequality. In particular, we have

$$U_h\left(y\right) \ = \ \max_z \left[S\left(t\right) - U_w\left(z\right)\right] \qquad \text{and} \qquad U_w(z) \ = \ \max_y \left[S\left(t\right) - U_h\left(y\right)\right] \ . \tag{8}$$

It is important to note that the stability conditions above constrain the total (two-period) expected utilities U_h and U_w, but have no implication for the *intertemporal distribution* of utility over the two periods.

[11] Such a result is consistent with empirical findings on marriage and divorce patterns by schooling: individuals sort positively into marriage based on schooling and individuals with more schooling are less likely to divorce. See Browning, Chiappori, Weiss (in progress, Ch. 1).

2.3.1 Determination of Expected Lifetime Utilities

General Characterization Equation (8) leads to an explicit characterization of intra-household allocations. The envelope theorem applied to these conditions yields the differential equations :

$$U_h'(y) = S'[y + \psi(y)] \quad \text{and} \quad U_w'(z) = S'[\phi(z) + z] \; . \tag{9}$$

To derive the expected spousal allocations over the two periods and along the assortative order, we integrate the expressions in (9). Hence, surplus share of a *matched man* with the endowment y and that of a *matched woman* with the endowment z are, respectively,

$$U_h(y) = k^h + \int_{y_m}^{y} U_h'(x)\, dx \quad \text{and} \quad U_w(z) = k^w + \int_{z_m}^{z} U_w'(x)\, dx \; , \tag{10}$$

for some constants k^h and k^w which we determine below.

Pinning Down the Constants The constants k^h and k^w are pinned down by two conditions. First, for all couples, the total output is known as expressed by equation (10). Hence,

$$k^h + k^w = S[y + \psi(y)] - \int_{y_m}^{y} U_h'(x)\, dx - \int_{z_m}^{\psi(y)} U_w'(x)\, dx \; , \tag{11}$$

where the left-hand side, by construction, does not depend on y. Secondly, it must be the case that 'the last matched person' is just indifferent between a union and singlehood. In the case with more women than men, $r > 1$, we have

$$U_w(z_0) = 2z_0 \qquad \Leftrightarrow \qquad k^w = 2z_0 - \int_{z_m}^{z_0} U_w'(x)\, dx \; , \tag{12}$$

with $z_0 \equiv \Phi(1-r)$. Hence, $k^h = S[\phi(z_0) + z_0] - 2z$, $U_w(z) = 2z_0 + \int_{z_0}^{z} U_w'(x)\, dx$, and

$$U_h(y) = S[y + \psi(y)] - U_w[\psi(y)] = S[y + \psi(y)] - \left(2z_0 + \int_{z_0}^{\psi(y)} U_w'(x)\, dx \right). \tag{13}$$

It is important to stress that the stability conditions apply without any assumption regarding the level of commitment attainable by the spouses. The insight is that conditions on the marriage market determine the allocation of lifetime utilities between spouses: because of competition, a wife would not agree to marry a husband who would provide less than the equilibrium utility — since many perfect substitutes exist — and likewise for the husband.

2.4 The Intertemporal Allocations of Utility and the Role of Commitment

We now consider the allocation of lifetime utilities U_h and U_w between the two periods. At this point, commitment issues become crucial. While some degree of commitment is clearly achievable, there may be limits on the extent to which couples are able to commit, for instance, couples could not commit not to separate. Two broad views emerge from the existing literature. Some contributors argue that only short-term commitment is attainable and that long-term decisions are generally open to renegotiation at a further stage. Other authors point out that a set of instruments, including prenuptial agreements, are available to sustain commitment. They, therefore, claim that divorce is the only limitation on commitment.

For what lies ahead, we shall assume that couples can commit to their spousal allocations in their union ex ante, although the no-commitment case can be solved in a similar way.[12] No renegotiation can therefore take place unless separation is credible. Moreover, if renegotiation does occur, it results in the minimal change needed for a union to continue, if that is indeed optimal.

2.4.1 Second-period Utilities

Let $u_h^2(y)$ and $u_w^2(z)$ denote the *pecuniary* components of utility derived from the intra-marital allocations respectively of husband with endowment y and wife with endowment z in the second period should they continue with their partnership. Hence, the husband's (wife's) total second-period utility is $u_h^2(y) + \theta$ (resp. $u_w^2(z) + \theta$) if the union continues. Feasibility constraints require that $u_h^2(y) + u_w^2(z) = \eta(t)$.

[12] Please refer to Appendix B for a more detailed discussion on the topic.

Under unilateral separation, each spouse can walk away with the share of family income determined by law, βt for the wife and $(1-\beta)t$ for the husband, where $t = (y + wz)$ is family income. Individual rationality implies that these outside options cannot exceed the utility payoffs if the union continues. Therefore, it must be the case that

$$u_h^2(y) + \theta \geq (1-\beta)\, t \quad \text{and} \quad u_w^2(z) + \theta \geq \beta t, \tag{14}$$

which we shall hereafter refer as the *individual rationality constraints* (IR). Note that these conditions jointly imply that

$$u_h^2(y) + \quad u_w^2(z) + 2\theta = \eta(t) + 2\theta \geq t\ , \tag{15}$$

or equivalently that $\theta \geq \hat{\theta}(t)$, so that separation is not the efficient outcome.

Any allocation such that (14) is satisfied can be implemented as part of a feasible marital contract:

Proposition 1 *With commitment and unilateral separation, there exists exactly one allocation that is not θ-contingent and guarantees that all the constraints are satisfied for any realization of θ.*

Proof. *The key remark is that the individual rationality constraints (14) must be binding when $\theta = \hat{\theta}(t)$ since, for that value, the couple is indifferent between union and separation. Hence,*

$$u_h^2(y) = (1-\beta)\, t - \hat{\theta}(t) = \frac{1}{2}\left(\eta(t) + (1-2\beta)\, t\right), \tag{16}$$

$$u_w^2(z) = \beta t - \hat{\theta}(t) = \frac{1}{2}\left(\eta(t) - (1-2\beta)\, t\right). \tag{17}$$

Note that, for any realization of θ, either $\theta < \hat{\theta}(t)$ and separation takes place or $\theta \geq \hat{\theta}(t)$ and utilities are equal to $(1-\beta)\, t + \theta - \hat{\theta}(t)$ and $\beta t + \theta - \hat{\theta}(t)$ for the husband and the wife respectively, so that the time-consistency constraints are fulfilled for both spouses. ■

Interestingly, the second-period utilities in union exactly reflect the utilities if separated, with

the addition of the difference between the actual match quality θ and the threshold $\hat{\theta}$.[13] In particular, we have

Corollary 2 *Any increase of a spouse's utility in separation is exactly reflected in that spouse's second-period utility even if the couple does not separate.*

A natural question, however, is whether the material allocation $\left(u_h^2, u_w^2\right)$ can be contingent upon the realization of θ. Contingent allocations raise specific problems. For instance, depending on the enforcement mechanism, they may require that match quality be verifiable by a third party. Whether such verifiability is an acceptable assumption is not clear. It turns out, however, that under our assumption of common θ, verifiability is not an issue because there exists (exactly) one allocation that satisfies the incentive compatibility constraints for *all* θ.

2.4.2 First-period Utilities

The expected two-period utilities equal

$$U_h(y) = u_h^1(y) + \bar{\theta} + (1-\alpha(t))\left\{u_h^2(y) + E\left[\theta \mid \theta \geq \hat{\theta}(t)\right]\right\} + \alpha(t)(1-\beta)t\ , \tag{18}$$

$$U_w(z) = u_w^1(z) + \bar{\theta} + (1-\alpha(t))\left\{u_w^2(z) + E\left[\theta \mid \theta \geq \hat{\theta}(t)\right]\right\} + \alpha(t)\beta t\ , \tag{19}$$

where $\alpha(t) = \Pr\left(\theta < \hat{\theta}\right)$ is the separation (or divorce) probability. These utilities must coincide with the equilibrium values derived above. Therefore, for $r > 1$, the first period utilities are:

$$u_w^1(z) = z_0 + \int_{z_0}^{z} S'\left[\phi(x) + x\right]dx - (1-\alpha(t))\left\{u_w^2(z) + E\left[\theta \mid \theta \geq \hat{\theta}(t)\right]\right\} - \alpha(t)\beta t\ ,$$

$$\begin{aligned} u_h^1(y) \quad =& S[y+\psi(y)] - z_0 - \int_{z_0}^{\psi(y)} S'\left[\phi(x)+x\right]dx \\ &+ (1-\alpha(t))\left\{u_h^2(y) + E\left[\theta \mid \theta \geq \hat{\theta}(t)\right]\right\} - \alpha(t)(1-\beta)t\ . \end{aligned}$$

[13] If, one uses, instead, the Nash-bargaining solution, with post-separation utilities as threat points, the allocations will correspond exactly to the non θ-contingent allocations under commitment.

2.5 Reforming Alimony Laws

Consider now a change in alimony laws that raises the wives' share of household income from β to $\hat{\beta}$, something akin to the legal changes discussed in Section 2. This, of course, does not rule out the possibility that β's may be couple-specific (as it would be in a private-property regime). As long as utility is transferable, the Becker-Coase theorem applies and such a change does not affect separation probabilities. In particular, the threshold $\hat{\theta}(t)$ only depends on the surplus generated by a union, not on its post-separation division between (ex-) spouses; a couple splits if and only if its realized θ lies below the threshold, irrespective of the β in place. But, under unilateral divorce laws, changes in β typically result in a redistribution of the surplus between spouses during the union. Whether a wife would benefit from the new property division rules would depend on her income, her union match quality and the level of commitment achieved between the spouses.

Concerning the impact of the reform on the division of marital gains, it is crucial to distinguish between *existing couples*, who have already spent one period together when the change becomes effective, and *future couples* who start their relationship after the law is in place. For the former, unexpected legislative changes may trigger a renegotiation within the household and alter the original contract implemented. For the latter, the new legislation would be taken into account at the matching stage and reflected in the expected allocations entering the union. We now consider these two cases successively.

2.5.1 Existing Couples

Consider a couple with endowments y and z for the husband and wife, respectively, whose match quality θ strictly exceeds the threshold $\hat{\theta}(t)$. Since the intra-household allocations, as determined in the matching market, were individually rational, it must have been the case that neither spouse had an incentive to get separated with the original β in place.

In fact, if θ is large enough, the wife's individual rationality requirements given by (14) are satisfied for both β *and* $\hat{\beta}$. This occurs if $\theta \geq \hat{\beta}t - u_w^2(z)$,where $u_w^2(z)$ denotes the continuation utility of the wife under the current agreement. Then, due to commitment, the change in divorce

laws has no impact on intra-household allocations. If, on the contrary, θ is such that $\hat{\beta}t - u_w^2(z) > \theta \geq \beta t - u_w^2(z)$, then the initial agreement is no longer enforceable, since it would violate the wife's individual rationality. Hence, her second-period allocation must be adjusted upward to $\hat{u}_w^2(z) = \hat{\beta}t - \theta$, which requires an additional transfer equal to

$$T = \left(\hat{\beta} - \beta\right) t - \theta - \frac{\eta(t) - t}{2} \geq 0 \ . \tag{20}$$

From a comparative perspective, the probability of a renegotiation taking place depends on the distribution of θ. In the benchmark case where θ is more or less uniform over a 'large enough' support, the probability is proportional to $\left(\hat{\beta} - \beta\right) t$. When both β and $\hat{\beta}$ are identical across couples, the reform affects a larger proportion of higher-income couples. We conclude that the reform will affect intra-household allocations of some — but not all — couples. For couples with a low realized match quality, the second-period marital allocation of the wife may no longer be sustainable. As a result, there will be more recontracting in favor of women among such couples. And since first-period spousal allocations would have already been sunk for all existing unions at the time of the legislative change, a more generous settlement rule for the wives would imply more leisure and less work for them in the second period *and* over their lifetimes.

In the absence of commitment, the conclusions are identical except for one aspect — namely, that renegotiation takes place between *all* spouses, because the reform directly impacts the respective threat points of all couples, including the wealthy and happy ones.

2.5.2 Future Couples

Now consider a couple who is not yet together at the time of the legal change in alimony laws. The expected lifetime allocations of such a couple, as given by equations (18) and (19), can be decomposed into three parts: first-period utility, second-period utility if the union is continued, and second-period utility in case of separation. Unlike existing unions, however, future unions are formed taking the new laws into account by the agents in the matching phase and reflected in their equilibrium allocations. This has two consequences. First, the reform influences intra-household

allocation in *both* periods. This is because the allocations of *lifetime* utility, which involves first- and second-period welfare, is decided during the matching process, taking into account the new law. A second and more subtle implication is that the impact of the reform on a future union is the same whether or not agents are able to commit to specific intra-household allocations ex ante. Indeed, we have seen in subsection 3.4.1 that the (non-θ-contingent) allocation decided ex ante is the same in both contexts.

Using (16) and (17), we can compute the impact of a change in post-divorce allocations on individual utilities. Due to the change in β, the variations in individual utilities are given by:

$$\Delta u_h^1 = \Delta u_w^2 = \left(\hat{\beta} - \beta\right) t, \qquad \Delta u_h^2 = \Delta u_w^1 = -\left(\hat{\beta} - \beta\right) t. \tag{21}$$

Hence, an alimony reform that mandates more generous separation settlements for women increases their utility in the second period whether or not the couple separates. However, the reform also *lowers* their first-period allocations by the same amount. Implicit in the above argument is what we have already established: in unions not yet formed, a legislative change has no effect on expected *lifetime* allocations. But given that equilibrium spousal allocations need to be individually rational, more favorable separation rules may lead to a more rapidly rising allocation path for the wives-to-be in order to ensure that their marital commitments are time consistent; in practice, they get more at the end, therefore less at the beginning. In particular, all wives' expected intra-marital allocations *conditional on remaining together* are reduced and the reduction exactly offsets their gain in case of marriage.

Proposition 3 *A change in the rules governing property rights over the distribution of family assets has* no *impact on welfare as measured by expected lifetime utilities at the time of the union. To the extent that the policy raises the utility of women following separation, it must* reduce *their total utility while married.*

While the previous results have been derived within a convenient, TU framework, their scope is general. The main insight is that for couples formed *before* the reform, changes in the rules governing property rights over family assets redistribute expected lifetime utilities between spouses.

Regarding couples formed *after* the reform, however, the intra-couple allocation of total surplus, which determines individual lifetime utilities, is mainly governed by competition on the matching market. Consequently, any attempt by the government to redistribute income among agents without changing the situation in the matching market is completely undone by a redistribution over time *within* family units; in particular, if such a reform raises the utility of women following divorce, it must reduce their total utility while matched.[14]

None of these conclusions requires a TU framework. However, limits to intra-household transferability would complicate the model in two extra dimensions. First, the *size* of the surplus (and not only its distribution) would typically be affected by the reform. Such effects are quite specific, since they typically increase or decrease lifetime utilities of *both* partners simultaneously.[15] Secondly, separation probabilities would also be modified.[16] Ideally, one would want to test all such behavioral responses empirically, although our data allow us to only explore if spousal labor supplies vary in the fashion we exposited above as well as the extent to which separation probabilities react to changes in alimony laws.

3 Alimony Rights of Cohabiting Partners in Canada

Our empirical section relies on the variation in the legal rights of cohabiting partners upon separation in Canada. The variation is due to the fact that these rights have changed dramatically over the last 35 years. Before these new legislations were introduced, cohabiting couples had absolutely no recourse in case of separation. Each partner was entitled to what was purchased by him or her in the union and no economic compensation was granted to economically weaker partners. The revised laws, in contrast, allowed spouses the right to petition for alimony upon separation. They did not grant rights to an equal division of assets, which is still, in most jurisdictions, granted to

[14]These neutrality results relate to the literature on Ricardian equivalence (see Barro, 1974). Note, however, that our result relies on market forces rather than altruism to endogenize redistribution between spouses. Also related is Lazear's (1990) result on the neutrality of mandated severance payments in the context of worker-firm relationships.

[15]For instance, Chiappori, Iyigun and Weiss (2008), studying a model with divorce and remarriage, provide an example in which a policy that raises the share of wives in family income upon divorce ends up reducing social welfare in the Pareto sense.

[16]See for instance Chiappori, Iyigun and Weiss (2007).

married individuals.[17]

Table 1 presents a summary of the legislative changes studied in this paper. What sets the Canadian provinces apart from most other countries is that no "registration" of unions is required. Cohabitation, in itself, is the proof required by law for demonstrating one's commitment to the relationship, and the length of time required by law varied by jurisdiction. The legislative shifts analyzed here occurred between 1978 (in the province of Ontario) and 2000 (in Newfoundland). There appears to be no general trend for provinces close to one another to have coordinated their legislation. It also does not appear that provinces that were more liberal or with a higher proportion of common-law spouses adopted these legislations earlier than others. Actually, the province with the most common-law relationships (Quebec) is the only province that has continued not to offer any protection to partners in that form of unions.[18] Most of the shifts were brought upon by cases in provincial and national courts. The majority of recent changes in legislation was due to cases involving homosexual couples rather than heterosexuals,who were then granted these benefits on the grounds of equality. This should reduce the potential for endogeneity according to changes in cohabiting couples' behavior, although this is only true of most recent legislations.

What provides more source of variation for identification is that, as shown in Table 1, each province differed greatly on how it defined a common-law relationship in terms of year of cohabitation. The required duration of cohabitation ranged from one year in Nova Scotia to five years in Manitoba. Also, six provinces reduced the requirement in terms of cohabitation length for couples with children.

How are these laws enforced? It appears that it is left to the petitioner to prove that the relationship lasted the required amount of time. Evidence such as common leases and bank accounts are useful in this matter. However, since 1993, this is facilitated by a change in the federal tax code. As of that date, common-law partners having lived together for more than 12 months (or less but with a child) *must* file their income taxes jointly. This shift affected all couples in all

[17] Asset division was granted to common-law spouses in 2001 in Saskatchewan. We have excluded from our analysis the territories where, in general, spousal benefits and asset division rights were granted simultaneously.

[18] In July 2009, the Quebec Superior Court ruled that such a law was constitutional and allowed Quebec to continue with this policy in a case involving a famous tycoon and his cohabiting partner of many years who was seeking monthly alimony payments of CAN$56,000 in addition to a payout of CAN$50 million. In December 2010, the case was sent to the Supreme Court.

provinces at the same time. Moreover, cohabitation agreements are legal in all provinces and they could stipulate the financial exchanges that would be accepted if the couple ever separates. Signing such cohabitation agreements were not invalidated by the changes in the law. However, the courts have a record of refusing to enforce agreements that are judged to be "unfair". Furthermore, such cohabitation agreements are actually rarely signed.

Cohabitation in Canada is not uncommon and rising in popularity. According to Statistics Canada (2002), 16 percent of all couples were cohabiting. This is driven by the very large number of common-law unions in Quebec (where 30 percent of all unions are cohabitations) but the proportion of common-law relationships in the rest of Canada in 2001 (11.7 percent of all unions) is still larger than that in the United States (8.2 percent). Common-law relationships differ observationally from legal unions in many ways: they are shorter-lived, have lower fertility rates, involve younger, French-speaking and slightly more educated partners. For further details, see Statistics Canada (2002).

3.1 The Data

For our empirical analyses, we used *General Social Survey* (GSS) data of 2001, from *Statistics Canada*.[19] This is a cross-sectional sample but it includes very detailed retrospective questions on one's education, labor market activities, children and past relationships. The total sample covers 24,310 Canadians aged 15 and above in all provinces but not in the territories.[20] The survey is also retrospective and thus subject to recall bias. Nevertheless, as long as this bias is not altered by the new alimony rules, it should not affect our results.

The data collected from the GSS measure all the characteristics needed to classify a union as subject to the new alimony rules or not: the age at which the relationship began (which, taken together with the year of birth, identifies the year in which the relationship began), the age at which it ended (or whether it was still active at the moment of the survey) and the respondent's province

[19]Ultimately, we needed to obtain a source that provided us with all three key elements determining couples' treatment status. While the Census too had some relevant information, it only started distinguishing common-law couples in the 1990 wave and never recorder relationship length, making it impossible for us to use.

[20]Note that this is an "individual" survey which is why it does not include responses from two individuals of a same couple.

of birth (that of the spouse is unknown), which we will use to assign the legislative environment, in order to alleviate any concern regarding selective migration (pre- or post-separation).[21]

Our key outcomes of interest involve the relative welfare of each partner. The related literature has generally relied upon either gender-specific consumption goods, such as women's and men's clothing, or spousal leisure as measures of partner welfare (see Angrist, 2002, and Chiappori et. al., 2002, for example). Since the GSS data were lacking in the former, we used information on the labor supply as proxies of spousal leisure. While the GSS does not provide information regarding the number of hours or weeks worked, the respondents were asked to detail their full retrospective histories of work and education.[22] Periods of work, hiatus and education are then matched to each relationship based on the years of each event. While labor supply choices may not necessarily be perfect proxies of spousal welfare, we shall ensure in our empirical analysis that they are not contaminated by other life-events such as career progress and decisions related to childbearing.

The data also provide a variety of outcomes to measure the impact of the legislation on relationship stability. First, one can determine whether the cohabitation eventually led to marriage (which can be seen as a substitute for cohabitation). The data also allow us to measure the overall duration of the relationship (including the years of marriage when relevant) and whether or not the relationship had ended at the time of the 2001 survey. Finally, the survey only provides limited information on the spouses of the respondents. The data available include the age difference between the respondent and his or her partner as well as the marital status of the partner before the relationship.[23]

We do not use the full GSS sample. Immigrants and individuals born in the territories are excluded since their province of birth does not have a legislative rule to assign to it. Relationships that began before 1960 are also excluded so as to focus on relationships that are closer in timing

[21] Also, the number of cross-province migrations recorded in the data was censured to the two most recent ones which, in some cases, prevent us from tracking where individuals lived when they were cohabiting. About 80 percent of the sample lived in the province where they were born.

[22] All work episodes are described including the year they began, the year they ended and whether they involved mostly full-time or part-time work. All interruptions, which are defined as periods of more than 3 months where the individual was not working for a variety of reasons (including lack of work, sickness, maternity/paternity leaves, retirement, job switches, education, etc.) are also mentioned.

[23] Only for the current partner is there more information, but there is a severe selection bias in this, which prevents us from using these data.

to the legislative changes observed. This gives us a sample of 7,520 common-law relationships and 11,279 marriages (which we will use in placebo regressions). Since we will use the panel nature of the data for our baseline analysis, this translates into 65,445 year-relationship observations for cohabitations (39,066 of which formed prior to the legal changes) and 194,427 for marriages.

Table 2 presents the summary statistics for our panel regressions where each observation is a year-relationship, contrasting cohabiting relationships formed before and after the legal change. In both samples, about 11-12 percent of years included one where an individual studied, about three-quarters included work and about two-thirds, full time work. Around 12-13 percent included a work interruption and, while about 5 percent of years involved maternity leave, and a much smaller fraction included paternity leaves. Overall, the difference between the behavior of the two samples is relatively similar.

Table 3 contrasts common-law relationships formed before and after the legal change by various characteristics. In this table, every relationship is one observation and the summary statistics are computed using the person-specific weights provided by the survey. The first section documents the demographic characteristics of the respondents and highlights that relationships formed before a legislation was passed were formed by individuals who were more likely to be from Quebec, thus explaining the higher fraction of French-speakers and Catholics in the sample. Between 35 and 40 percent of cohabitations turned into a marriage. Durations are relatively short on average (3.5 years for relationships formed after the legislation and 5 years for those before) but longer if one includes the full-relationship duration. Three-quarters of relationships involve previously unmarried spouses and 30 percent of relationships involve partners with at least 5 years of age difference. Finally, 6 percent of the sample formed before the law was eventually subject to it, while 50 percent of relationships formed after lasted long enough to become subject to the rules.

4 Estimation Framework and Results

4.1 The Estimation Equation

We want to use the legislative framework presented above and the available data to estimate the impact of the extension of the right to petition for alimony in cohabitation on proxies of partners' welfare as argued in our theoretical framework. Specifically, we want to identify whether there is a redistribution between cohabiting partners after these rights are granted. Given the panel nature of the data, the conceptual question we would like to pose is what happens to a couple when they become eligible for these new rights. Naturally, the answer depends on various relationship characteristics, such as duration and the presence of children. However, since these partnership characteristics are also likely to influence labor supply choices, we need to compare a couple who lives in a province that has enacted the alimony law with an otherwise identical couple who lives in a province that has not, at a given point in time. Moreover, since there may be province-specific effects, we must subtract from this comparison the difference in those two provinces of the outcomes for couples who did not qualify for the benefits. Finally, since there could be time trends in female and male labor supply, we must also take a third difference, this time contrasting the eligible and non-eligible couples in the two provinces at a time when the law had not been enacted. Conceptually, this is what our triple-difference strategy computes.

Thus, we will estimate the effect of the law for an outcome L_{iptcj} of an individual in a relationship i, in province p, which began in year t and is observed in year j. Let c equal 1 if the couple has had a child by the observation date. For each province, define the year in which the new law is implemented as T_p, the required duration as D_p and an indicator for shortened duration when children are involved as C_p.

Define the variable Ω_{ptcj} as an indicator of whether the relationship was, in year j, subject to the new rules regarding alimony. That is:

$$\Omega_{ptcj} = \sum_{k=1}^{10} 1(p = k) * 1(j > T_k) * 1\left(1(j - t > D_k) + c * C_k\right) \ , \tag{22}$$

where 1(.) represents the indicator function. This simply means that a couple is 'treated' if the relationship is still active at the time of the legal change *and* it lasted more than the required amount of time, *or* if it had a child at that time.[24]

Given this, the triple difference strategy we propose translates into a regression equation which is given by:

$$\begin{aligned} L_{iptcj} = {} & \alpha\Omega_{ptcj} + \kappa X_{ij} \\ & + \sum_{k=1}^{10} (1(p=k) * (\delta_k 1(j > T_k) + \lambda_k 1(j - t > D_k) + \zeta_k c * C_k) + \tau_k 1(j > T_k) * c * C_k \\ & + \phi_k 1(j > T_k) * 1(j - t > D_k) + \theta_k 1(j > T_k) + \gamma_k 1(j - t > D_k) + \rho_k * c * C_k) + \mu_j + \nu_i + \varepsilon_{iptcj} \end{aligned} \tag{23}$$

What (23) does is to control for all the "ingredients" of Ω_{ptcj} with as much flexibility as possible, while isolating the role of the treatment Ω_{ptcj} on labor supplies. Ideally, this should be done with the fixed effects of all double interactions between p, t, c and j. However, our sample size isn't sufficient for the inclusion of all of these interactions. As an alternative, we include all of the double interactions between the single determinants of the treatment status, namely province of birth, whether the relationship started after a given legal change, whether the relationship lasted longer than the deadline in any province and whether the couple had children. We allow for the labor supply of men and women to change differentially in response to the threshold number of years in the relationship and the arrival of a child and differentially so by province and over time, as captured by the parameters δ_k, λ_k, ζ_k, ϕ_k and τ_k.[25] Fixed effects for the relationship and the year of observations are also included and individual controls X_{ij} for age and the square of age (and also their interaction with a male dummy) are added.[26]

The required identifying assumption here is that there was no other contemporaneous shock

[24]Since all provinces with special rules for children impose a relationship duration of at most one year, we will judge the presence of a child to be sufficient in itself.

[25]We will estimate α interacted with gender, as well as all the other terms interacted with the latter, except for year fixed effects, which was too costly to interact with the gender dummy in terms of degrees of freedom.

[26]Few other year-varying controls can be included as the survey is retrospective and the additional ones (such as the number of children, last year's labor supply or educational history, are likely to be endogenous).

affecting cohabiting couples who were living together for more than a certain period in provinces where the legislation was changed. This is robust to shocks occurring in a province at a given time or to couples with longer durations in a particular province being different than those in another one or to couples changing their behavior in response to the arrival of a child differently in different periods. Furthermore, as we specified above, in most of the recent legislative changes, the impetus for modifying the law was not a desire to provide legal rights to cohabiting partners, but more of a need to offer homosexual couples (who, at that time, were not allowed to legally marry) the same type of legal protection married heterosexual couples were afforded in case of separation. As we already noted, this helps to alleviate the concern for endogeneity of reforms. We further explore these assumptions in the robustness section of our results.

In order to allow for serial correlation, standard errors are clustered at the province level.[27] Since the number of clusters is small, we will also report the bootstrapped p-values estimated as in Cameron et. al (2008). Finally, many relationships are short-lived and the impact of alimony rights would likely impact both individuals still in a relationship and those who are not. In order to allow for this effect to be captured, we include the full length of the relationship or the first ten years after the beginning of the relationship for those that lasted less than this time.[28]

Our objective is to contrast the parameter α when estimated for couples who were united before the new law with those who united after. To obtain this parameter for couples "caught" by the legal change, we estimate equation (23) restricting the sample to relationships that began before couples could modify their behavior and avoid becoming subject to the new ruling (that is those that started more than x years before the legal change where x is the duration requirement in that province). In those regressions, the duration of the relationship is censored to the length of the relationship before the law changed so as to ignore potential endogenous responses to be included in the estimation. Furthermore, because of the way the sample is selected, all relationships that were still active when the laws changed had to have lasted long enough to be subject to the new alimony rules, which prevents the inclusion of the parameters δ_k in the estimation equation above.

[27]This will also allow for correlations across observations of a given individual who has more than one relationship in the sample, although this affects only a small number of observations.

[28]If a relationship becomes a marriage within 10 years of its start, the years as married will also be included for the same reason as the decision to marry may be driven by the rights to petition for alimony.

To measure the impact of the granting of alimony rights to couples formed after the legislative change, we perform two types of regressions. First, we take the full sample of relationship-years but censor all relationships that got "caught" by the new law at the date they became subject to it. So relationships that started before the laws were revised are still in the sample, but only in the years where they were not subject to the new rules. Our "treatment group" here includes only relationships that began after the reforms, but our "control group" includes those that began and terminated before the reforms as well as the pre-reform years of the relationships of couples who united before the reforms. We estimate equation (23) using this sample. Alternatively, we also restrict the sample only to new relationships and use a difference-in-difference model using the province and the duration of the relationship as the only variables determining treatment. In this case, the identifying assumption is that there were no other province-specific shocks that affected relationships lasting more than a given number of years. Those regressions will be estimated in a similar way to (23), except most controls will not be included as they would be colinear with the treatment.

4.2 Alimony Rights and Labor Supply

Alimony payments are usually made from the higher-earning partner to the lower-earning one. Since over the period in question, men were still more likely to earn higher incomes than their spouses, we assume that legally requiring alimony payments favored women. Thus, we expect that, when an existing relationship became eligible for these rules, female partners decreased their labor supply provided that leisure is a normal good. This section explores changes in spousal labor supply using these various outcome measures as proxies for labor supply.

The results of the estimation of equation (23), using the three samples detailed above, are presented in Table 4. Panel A focuses on couples formed before a legislative change, while those for all unions except those that were immediately subject to the new laws when the laws changed are found in Panel B. Panel C shows the results of a simple difference-in-difference regression focusing only on couples formed after the legislation had been passed.

The results imply that, when a relationship is granted the right to petition for alimony, women

are about 4.7 percent less likely to work full time (when about 51 percent of the control group worked) and 2 percent more likely to be studying (compared to 8 percent in the control group). They are also 5.3 percent more likely to have stopped working, when 23 percent of the control group worked. Their likelihood of having taken maternity leave increases by 2.4 percent, although not statistically so. Males, on the other hand, responded to the legislation in a statistically different manner. They appear to have reduced their likelihood of being in school and having suffered work interruptions but more likely to have worked, particularly full-time. They appear less likely to have taken paternity leave but the magnitude is very small and not significant. While one may worry that the inference performed with a small number of clusters would be erroneous, the results of the bootstrapped p-values based on the method of Cameron et. al (2008), if anything, strengthen our results where all are now significant at the 10 percent level and most are actually very precisely estimated.[29]

What is much more striking in Table 4 is the difference between Panel A and the following two panels. Once one measures the impact of a relationship being subject to alimony rules on relationships formed after the legal change, the conclusions are very much different. This is true both in Panel B and C, despite the difference in the estimation strategy in these two samples. The coefficients are usually smaller in magnitude for couples formed after the legal change and almost always of the opposite signs as the ones presented in Panel A. They are more rarely significant although, once more, the bootstrapped p-values generally increase the significance of the coefficients. These results appear to suggest that relationships formed after the "rules of the game" were changed responded very differently to being subject to the alimony rules than those that were formed before. In the bottom panel, we list formal tests of the equality of the coefficients across the three upper panels. As shown, we can reject that the two sets of coefficients are equal for work interruptions and full-time work (in all panels) and for work and maternity when Panel A is compared with Panel B.

[29]The method presented by Cameron et. al (2008) allows the estimation of a p-value for a single variable of interest in a regression. We extended this framework to our setting. In this case, the null hypothesis necessary to build the bootstrapped values is constructed as follows: For the main effect coefficient, the treatment effect is assumed to be null (that is both the main effect and the interaction effect are set to 0). For the interaction coefficient, the treatment effect is allowed to be positive but the interaction is constrained to be 0. We performed Monte Carlo analyses and found this method produced very good results in a simulated sample that had equal size and number of clusters equal to ours. The results presented here are generated with 500 iterations for each outcome.

This suggests that, when couples who united before the legal change became eligible to petition for alimony rights, women in these relationships generally reduced their labor supply while men increased theirs compared to similar individuals residing in provinces where the laws did not apply. On the other hand, comparing relationships formed after the legal change as they became eligible due to the arrival of a child or the length of their cohabitation with ones that crossed the same threshold but in a setting where such benefits were not granted, we find that, if anything, labor supply of women appears to have risen.

All of these empirical findings are new to the relevant literature. They suggest that the impact of a reform in alimony laws are conditional on the timing of the couples' union vis-a-vis the enactment of the reform.

4.3 Robustness checks

These findings are explored in more detail in Table 5, where we present various robustness checks.[30] First, we replicate Panels A and B of the previous table but this time excluding Quebec.[31] This is extremely costly in terms of sample size; particularly in Panel A, where we lose more than halve the number of observations. Despite this loss of power, the results of Panel A are very similar in terms of signs and magnitudes, except for the effect of reforms on maternity leave which is now negative. However, in the case of studying and work interruptions, the significance of the previous table is lost. The results in Panel B are also similar for the signs of the main effect, although less so for the interaction terms, with the precision of the estimates being lower than in Table 4. Overall, these estimates do not suggest that our previous conclusions are driven by the fact that we included Quebec (which is the only province that never introduced the policy and that may be different than others in various characteristics, such as language, religion, etc.).

In the next table, we further explore the robustness of the results of Panel A of Table 4 in various ways, focusing of three outcomes: whether the individual studied in a given year; whether

[30] Given the high time requirement of computing the bootstrapped p-values in the previous table and the fact that our results appear to be conservative when using the asymptotic clustering methods, we do not present the bootstrapped p-values in the rest of the tables.

[31] Panel C of Table 4 already excluded Quebec, as no relationship there was formed after the legal change.

the individual worked full time; and whether the respondent had work interruptions.[32] In the first column, the results are presented assuming that no exceptions are made for relationships with children and that the alimony rights are granted solely on the basis of the duration of cohabitation. The estimates are fairly consistent with those presented in Table 4. The next column uses all years when the relationship was active and, thus, excludes years after a short relationship terminated. Doing so alters the results but more in significance than in magnitude. This highlights the importance of having a comparison point for relationships that lasted longer which is impossible if we use only the active relationships in the sample. The next two columns compare the results restricting the sample to either older relationships, in column (3), or more recent ones, in column (4).[33] Overall, the measured impact appears to be larger and more significant when focusing on recent legal changes than older ones even if some significant impacts are measured for working full-time in the older sample. Column (5) repeats the exercise, but this time using married individuals as a placebo group. The coefficients in this case are much smaller in magnitude and rarely significant. When they are (as in the case of work interruptions), they are of the opposite sign for cohabitations. This suggests that the results obtained in the previous table are not driven by events contemporaneous to the legislative changes affecting all types of unions in a geographical location. The last column includes controls for forthcoming legislative changes. In all outcomes, the fact that an individual would become subject to alimony rights in 2 years has no significant effect on his or her contemporary labor supply. Furthermore, except in the case of work interruptions, the introduction of such an additional control does little to change the size and significance of the coefficients of interest. This seems to indicate that provinces which implemented these rules were not on a different trend before they did so, which validates the assumption required for the validity of our triple-difference estimate.

We also repeated these robustness exercises for Panel B and C of Table 4, when feasible. Although not presented here, they show that the difference between the impact of the new rule for

[32]The results of robustness checks performed on other outcomes are available upon request.

[33]We also explored the validity of our results using the 1993 Federal tax change as a cut-off threshold. The results using only observations prior to that point are similar to the ones presented above. The ones restricting the sample to relationships that began after that point are very noisy, in part because the number of provinces which changed their rules after that date is small, leaving a small sample to use in Panel A. All of these additional results are available upon request.

existing and new couples remained all through the period. Results are not as clear when we exclude the rules about children or include only years where the relationship was actually active. Married individuals usually do not show any difference in behavior around the time of the change in the law and when they do, it is almost always in the opposite direction as the results presented in Table 4. Finally, couples formed after the new legislation appear to change their behavior *before* the law actually enters into action as the coefficients on whether an individual would become subject to alimony rights in 2 years are often significant. This is consistent with our model through which couples who know they will be facing these new rules as their relationship last longer may be changing the allocation of resources even before the rules start applying.

So far, we assumed that women are the lower earning spouses and thus would be the ones benefitting from alimony payments. However, there are couples for whom this was not the case. While we have no information on the relative income of partners, we can use a crude proxy given by their age difference. Table 7 explores who is more likely to respond to these new rules concerning alimony. The same three outcomes as in the previous table are presented and results are fairly similar for other outcomes. The odd columns present the coefficients of the main effect of the law, and the even ones, the interaction between the legislative change and a dummy for the respondent being male. The results presented in the first panel of the table support the hypothesis that the granting of alimony rights appears to have lowered the labor supply of the lesser earning spouse, as captured by the age difference. The results we have found earlier appear to be concentrated among couples for whom the women was at least 5 years younger, thus making them more likely to obtain alimony in case of separation.[34] On the other hand, it appears to be men who lowered their labor supply (although results are rarely significant except for the probability of studying) when women were much older than their partners, which is again consistent with the hypothesis that the older partner was more likely to be the one making the alimony payments.

[34]This group represents about 25 percent of all relationships.

4.4 Alternative explanations

The model presented above suggests a reason to explain the difference between the two panels. However, another alternative explanation would be that couples formed after the reforms are different in some aspects which influences their labor supply behavior. To explore this, we contrast characteristics of the respondents who were in a cohabiting relationship before and after the legal change, controlling for fixed effects for the year the relationship began and the province of birth. We also contrast relationships that lasted the required time limit and were formed after the legal change with those that did not, controlling this time for whether the relationship would have qualified in any province and a dummy for being formed after the legal change, as well as a dummy for lasting the required time limit in one's province of birth. The results of these exercises are presented in Table 8, the first corresponding to the top of each panel and the second one, to the bottom panel. We find no evidence that either the characteristics of the individuals entering into cohabitation after the legal change or the characteristics of their partners were altered: the only two significant coefficients indicate that males were slightly more educated and that women were slightly more likely to match with an older partner. Once we also include the fact that only relationships that last a given number of years qualify for the policy, only one significant coefficient remains indicating now that men are more likely to have a high school degree but less likely to have attended university. While this could be driven by the fact that there are key characteristics that are unavailable in our survey, we found no consistent evidence that the pattern presented above could be linked to selection issues. This is also very consistent with our model: since the market regulates the expected utility each gender should obtain when entering a relationship, the fact that new rules are imposed should not alter one's expected utility making the type of individuals matching before and after the legal change most likely to be identical.

Another alternative explanation for the pattern we highlighted above is that alimony rights enabled couples to specialize; something they were unable to do in the absence of alimony laws because the spouse specializing in child-rearing or home production could not be compensated in case of separation. While it is clear that this could account for the results for couples "caught" by the new legislation, it is unclear why one would expect a different pattern for couples formed

after the legal change. If the granting of alimony rights helps to solve a problem of incomplete contracts within the household, it should solve it in both cases, unless the types of couples that are formed change substantially, although we previously found no indication of that. If one of the most important household-specific investment that is enabled by this policy is related to child-rearing, then, couples ought to respond to this policy by altering their fertility behavior. However, we find no significant effect on the number of children of couples who got together before the legislation was enacted (a negative effect is found for newly-formed couples).[35] Finally, the fact that there does not seem to be a marked difference in the response of couples formed by older and younger individuals (as we shall discuss below) also casts doubt on this theory, as one would expect couples of child-bearing age to be particularly sensitive if specialization was responsible for our findings.

5 Other Empirical Issues & Tests

The model we presented in Section 2 is consistent with our baseline empirical findings. It posits that intra-household transfers would occur through changes in spousal labor supply in response to an alimony-law reform, and it conditions couples' responses based on whether they got together before or after the reform. In what follows, we investigate some further implications of this model.

5.1 Intertemporal Allocation of Labor

First, our model predicts that, the expected utility of entering into a relationship of couples subject to the new laws compared to those who were never subject to them would remain identical but the intertemporal allocation of leisure would change. Thus, while the pattern we observed before for these new couples was consistent with our model, we can test more directly whether the behavior of men and women over time is consistent with our framework.

In particular, couples subject to the new laws should have women front-loading work more and enjoying leisure less and vice versa for men. This pattern is explored in Table 9 where the sample is the same as that in Panel B of Table 4: that is, all year-couple observations of a relationship

[35] Results on fertility are not presented but are available upon request.

(including at least 10 years following the beginning of a relationship if the relationship does not last long) but where relationships that were "caught" by the change in law are truncated at that point. The first panel simply compares the labor force participation of women and men contrasting relationships formed *before* and *after* the legal change. While the statistical significance of the results is limited, the pattern suggested is clearly in harmony with our model: women who enter common-law relationships *after* the legal change are more likely to work and less likely to study or have work interruptions. The difference in the probability of having a work interruption is of about 7 percent. Panel B explores this pattern more carefully by interacting whether the relationship started before or after the passing of the law with a linear indicator for the number of years since the relationship began. As years go by, women in the new regime should be able to benefit from alimony payments, either because the relationship ends at some point or because it stays active and they receive the payoff they were anticipating in the relationship. This is exactly what the results in the bottom panel indicate. We now see that, at the beginning of a relationship, men were more likely to study and less likely to work full-time after the new rules were implemented. Women were less likely to suffer work interruptions. However, with the passage of time, the pattern is reversed as men's labor force participation changes little (the interaction term is of the opposite sign and similar in magnitude as that of the main effect) and that of women decreases. The results are particularly marked for work interruptions where the probability of experiencing a work interruption would eventually favor women 11 years after the beginning of the relationship.

5.2 Heterogeneity in Responses

Our framework also suggests that matches with lower joint income would be more likely to respond to the policy change if there exists some capacity to commit to allocations. While the data do not offer information regarding the income of the respondent at the time of the relationship, we use two proxies: the education level and the age at which the relationship began. The results are presented in the bottom two panels of Table 7. Panel B contrasts the treatment effect by the education level of the respondent. In the case of whether the respondent worked full-time in a given year, the legislative change appears to have affected more directly individuals with higher

levels of education. While men with more education responded more strongly to the legislation in terms of work interruptions and studies (although not significantly so), it is women with lower levels of education who did so for work interruptions and women with a high school degree who did so for studying. It does appear that couples formed before age 21 responded less in terms of labor supply but more in terms of schooling. But, overall, there is no strong difference between individuals who were in their 20s at the beginning of the relationship and those who were older. This may be because our income proxies are rather coarse, but also because couples are relatively unable to commit to inter-temporal allocation of resources. Although not shown, we also find that most of the impact appears to influence couples who have been in a relationship for the longest periods before the legal change, except in the case of education where it is most visible for more recently united couples.

5.3 Alimony Rights and Relationship Stability

We have shown in our theoretical section that, when utility is transferable, the 'Becker-Coase' theorem applies resulting in no change in the separation likelihood following changes in alimony laws. However, this might not be the case when utility is not fully transferable. Hence, we investigate here whether granting alimony rights changes the duration and the stability of relationships. This closely relates to the debate surrounding the impact of no-fault divorce laws on the incidence of divorce in the United States (see, for example, Wolfers, 2006). The threat of separation may be sufficient to alter the way bargaining power is at play within the relationship, but this threat may need to be exercised in some cases when there are frictions in making compensating spousal transfers within the union.

We consider four related outcomes here. First, many cohabitation relationships eventually lead to a formal marriage. However, if cohabitation starts to resemble more traditional legal unions in the eyes of the law, there are less incentives for such a transformation. One may thus expect that granting alimony rights to cohabiting couples may lead fewer of them to "tie the knot". We use whether the relationship was still alive at the time of the survey and the length of the cohabitation as our measures of stability. Since there is a potential trade-off with marriage, the total length of

the relationship (including any subsequent marriage when relevant) is also included as an outcome.

Since we are not looking at changes in behavior over the duration of a relationship but rather at an outcome specific to the full relationship, we will not be able to use relationship fixed effects as in equation (23). Instead, we modify (23) such that $j - t$ is fixed to the relationship duration and c defined as an indicator of whether the couple ever had a child and relationship fixed effects are replaced with province fixed effects. In this case as well, the same three samples are used as described previously to allow a contrast between relationships formed before and after the legal change.

Table 10 presents the results for these measures. The top panel includes the results for unions formed before the laws were enacted, while Panel B presents those for unions except the ones that immediately became subject to the new laws once the laws were adopted. And Panel C presents the results of a simple difference-in-difference among unions formed after the legal change. The even columns represent the results when males and females are pooled, and the odd columns, one where all control variables as well as the treatment variable are interacted with a dummy for the respondent being male. The results suggest that the Becker-Coase theorem cannot be rejected in this sample: the likelihood of a couple being separated at the time of the survey in 2002 does not seem to be significantly related to the fact that the union became subject to alimony rights, at least in the case of existing couples. Similarly, being granted alimony rights does not appear to have changed the overall duration of relationships for couples formed before the legal change. However, it does appear to have significantly increased the duration of the cohabitation phase of the relationship by about 2 years, and reduced the likelihood of transforming them into marriage by about 14 percent. There is little indication that these impacts differed between male and female respondents. On the other hand, for unions that formed after the laws were changed, being eligible for alimony payments appears to have shortened the duration of cohabitation (and maybe even of the overall duration of the relationship, depending on the estimation strategy used). For none of the outcomes is the impact of being granted alimony rights among all relationships similar in size to the estimated impact in Panel A.

We then evaluated how robust these results are to some variations.[36] Six different tests were performed mirroring those of Tables 5 and 6. First, only rules regarding whether the relationship lasted the required number of years were used, ignoring the exceptions linked to the presence of children. The results were similar in magnitude in significance to the ones presented in Table 10. We then added a number of covariates including the mother tongue, religious background and educational attainment of the respondent. The results, once more, were unaltered by this modification. Next, we explored whether restricting the sample to relationships formed early or late influenced the estimates. When focusing on relationships that began before 1990, we found that granting alimony rights not only lengthened cohabitation but increased its stability, making it less likely to be over by 2001 and increasing its overall duration by about 1.7 years — a little less than half of the increase in cohabitation length. On the other hand, relationships formed after 1980 displayed a similar pattern as the one highlighted for the entire sample. This could be simply due to the fact that recent relationships were more likely to be censored and, thus, less likely to significantly demonstrate an impact on long-lasting measures such as relationship stability. We repeated the estimation this time excluding all relationships from Quebec. In this sample, the Becker-Coase theorem appeared to be violated again, as relationships that became subject to the alimony rights were 13 percent less likely to have ceased to exist at the time of the survey and would have lasted about 2.6 more years in total. Cohabitation length increased more significantly in this sample but the likelihood of it evolving into a marriage was not changed. Finally, we also introduced a lagged indicator which was equal to one if the couple was to become subject to alimony rules in 2 years. Comfortingly, the coefficient of that dummy variable was small and insignificant (except in the case of overall duration), suggesting that the results presented in the previous table were not simply capturing a time trend. Finally, we also used log duration as an outcome variable because of the likely problem of censoring in that variable and the results remained identical to the ones presented above.

As a final exercise, we explored whether these laws differentially affected individuals with distinct attributes. We found that the likelihood that a relationship was inactive by 2001 appears to have been influenced by the alimony rights more strongly for individuals with less education, those who

[36] All estimates discussed but not shown are available upon request.

started the relationship at an older age and those for whom the woman was much older than her partner.[37] In no subgroup was the total duration of the relationship significantly impacted by the legal change. However, the two variables that were significantly modified by the granting of alimony rights for the entire sample, the likelihood of eventual marriage and the duration of the cohabitation, did appear to respond more strongly in those groups where the labor supply responses were more visible — that is, individuals with more education, older couples and those among whom the man was much older than his partner.

6 Conclusion

We present in this paper empirical evidence from Canada that changes in alimony payment laws affected couples differently based on when their relationships began relative to the enactment of the new laws. For couples who were *already together* when the laws were enacted, changes in household income division rules appear to have lowered the labor supply of women. For other couples whose relationship started *after* the laws were enacted, however, we identified contrasting outcomes for the new alimony rights' impact on the behavior of cohabiting couples: among such couples, men — and not women — were more likely to study, have more work interruptions, whereas they were less likely to work or work full time.

We were able to rule out sample selection and household specialization as a likely cause of these findings and we argued that they may be driven by policy-neutrality in intra-household allocations induced by the existence of a competitive matching market. We demonstrated theoretically that such a mechanism would generate the above results and derived further predictions concerning selection into cohabitations. Since competition implies that the expected utility upon entering a relationship is unaffected by the rules governing alimony, the type of individuals matching before and after the legal change should be similar, as we demonstrated. Overall, our results suggest that changing the alimony rules for cohabiting partners could only improve the welfare of women in existing couples while for new couples, they simply lead to a inter-temporal shift in the timing of

[37] Several studies have found that early marriages and of uneducated partners tend to be less stable. See, for instance, Weiss and Willis (1997).

the payoffs without influencing the overall expected utility.

In a context where, all around the world, many couples choose to cohabitate before or in lieu of marriage, the issues we have explored here appear to be more and more relevant. Our results suggest that while government intervention in this "market" may have a short-run impact on existing couples, they are unlikely to be able to alter outcomes more than a simple inter-temporal shift. Furthermore, our results may also explain the limited number of cohabitation arrangements signed, even when they are legal and likely to be enforced, mirroring the limited number of pre-nuptial agreements (see Weiss and Willis, 1993). The theoretical framework presented highlights that they may only be able to influence the inter-temporal allocation resources, while expected utility is driven by market forces.

Finally, while our results indicate that more couples subject to these rules were likely to remain in a cohabitation union and not transform it into a marriage, our theoretical framework does not model the choice of marriage versus cohabitation. We leave this as future work.

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7 Tables and figures

Table 1: Summary of legislations granting alimony rights to cohabiting couples

Province	Legislation	Year	Length required	Special cases
Newfoundland	Family Law Act	2000	2 years	1 yr with child
PEI	Family Law Act	1995	3 years	1 yr with child
Nova Scotia	Maintenance and Custody Act	1989	1 year	
New Brunswick	Family Services Act	1980	3 years	
Quebec	No law			
Ontario	Family Law Act	1978	3 years	Auto. with child
Manitoba	Family Maintenance Act	1983	5 years	1 yr with child
Saskatchewan	Family Maintenance Act	1990	3 years	Auto. with child
Alberta	Domestic Relations Act	1999	3 years	1 yr with child
British Columbia	Family Relations Act	1979	2 years	

Table 2: Summary statistics for cohabitations (panel sample)

	Before the law		After the law	
	Mean	St. Dev.	Mean	St. Dev.
Subject to alimony rights	0.04	0.18	0.34	0.47
Subject to alimony rights-no child rules	0.04	0.17	0.26	0.44
Studied	0.11	0.32	0.12	0.32
Worked	0.76	0.43	0.74	0.44
Worked full time	0.66	0.47	0.62	0.48
Stopped working	0.13	0.34	0.12	0.33
Number of children	0.90	1.12	0.93	1.14
Had a maternity leave	0.06	0.23	0.05	0.21
Had a paternity leave	0.00	0.06	0.00	0.04
N	39066		26379	

All statistics weighted by person-specific weights.

Table 3: Summary statistics for cohabitations (cross-section sample)

	Before the law			After the law		
	N	mean	st.d.	N	mean	st.d.
Demographic characteristics						
Age relationship began	4025	25.98	8.46	3495	27.17	8.72
Male	4025	0.47	0.50	3495	0.48	0.50
English-speaking	4020	0.28	0.45	3484	0.88	0.32
French-speaking	4020	0.70	0.46	3484	0.07	0.25
Catholic	4001	0.70	0.46	3436	0.27	0.44
Atheist	4001	0.13	0.34	3436	0.35	0.48
Protestant	4001	0.14	0.35	3436	0.35	0.48
Attended rel. services at age 15	3990	0.42	0.49	3432	0.36	0.48
High school graduate	4024	0.81	0.39	3487	0.84	0.37
College graduate	4024	0.19	0.39	3487	0.17	0.38
Relationship characteristics						
Ended as marriage	4025	0.34	0.47	3495	0.40	0.49
Relationship has ended	4025	0.49	0.50	3495	0.39	0.49
Duration (years)	4025	5.07	5.46	3495	3.45	3.62
Duration (total in years)	4014	8.90	7.90	3495	6.40	5.91
Partner's characteristics						
Spouse was prev. unmarried	4019	0.76	0.43	3483	0.71	0.45
Age difference (own-spouse)	3573	-0.61	4.87	3201	-0.49	4.84
Women at least 5 years older	3573	0.06	0.24	3201	0.07	0.26
Male at least 5 years older	3573	0.24	0.43	3201	0.24	0.42
Legislative status						
Subject to alimony rights	4025	0.06	0.24	3495	0.53	0.50
... with no child rules	4025	0.06	0.24	3495	0.49	0.50

All statistics weighted by person-specific weights.

Table 4: Impact of alimony rights on labor supply

	Studied	Worked	Worked full time	Work interruptions	Maternity leave	Paternity leave
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Only relationships formed before a law was passed					
Subject to alimony rights	0.020†	-0.030	-0.047†	0.053†	0.024	
	(0.011)	(0.022)	(0.024)	(0.027)	(0.019)	
	[0.004]	[0.004]	[0.004]	[0.008]	[0.004]	
Subject to alimony rights	-0.040†	0.086*	0.104*	-0.070†		-0.002
*male	(0.018)	(0.035)	(0.034)	(0.036)		(0.003)
	[0.004]	[0.004]	[0.004]	[0.016]		[0.072]
R-square	0.475	0.701	0.722	0.445	0.375	0.101
N	39066	39066	39066	39066	22242	16824
	Panel B: All relationship-years except those "caught"					
Subject to alimony rights	0.009	0.030	0.058†	-0.059*	-0.054†	
	(0.013)	(0.021)	(0.030)	(0.025)	(0.026)	
	[0.164]	[0.096]	[0.004]	[0.004]	[0.004]	
Subject to alimony rights	0.019	-0.002	-0.018	0.038		-0.004
*male	(0.035)	(0.023)	(0.024)	(0.033)		(0.003)
	[0.610]	[0.986]	[0.476]	[0.028]		[0.004]
R-square	0.485	0.716	0.731	0.445	0.375	0.094
N	63305	63305	63305	63305	35957	27348
	Panel C: Only relationships formed after the legal change					
Subject to alimony rights	-0.003	0.008	0.032†	-0.010	-0.002	
	(0.010)	(0.020)	(0.017)	(0.010)	(0.017)	
	[0.638]	[0.510]	[0.004]	[0.024]	[0.788]	
Subject to alimony rights	0.015	0.030	0.011	-0.017		-0.002
*male	(0.022)	(0.021)	(0.014)	(0.015)		(0.003)
	[0.264]	[0.004]	[0.264]	[0.044]		[0.456]
R-square	0.498	0.738	0.741	0.434	0.343	0.123
N	26379	26379	26379	26379	14883	11496
	F-test of equality between the coefficients...					
Panel A vs. B (main effect)	0.48	4.16 †	8.01*	9.86**	6.33*	0.17
Panel A vs. B (interaction)	2.36	4.53*	8.88**	5.27*		
Panel A vs. C (main effect)	2.71	1.78	7.67*	5.24*	1.10	0.06
Panel A vs. C (interaction)	4.05 †	1.94	6.61*	1.99		

Equation (23) specifies the estimation equation for Panel B. Panel A excludes estimates of δ_k and Panel C includes only estimates of γ_k, ρ_k, μ_j and ν_i from that equation. All regressions are weighted using person-specific weights. The sample includes at least ten years following the beginning of any cohabitation relationship or all the years if the relationship lasted longer. In Panel A, only relationships formed before the legislation are included. In Panel B, all relationships except years in which relationships formed before the law were subject to alimony rights are included. In Panel C, only relationships formed after the legal change are included.

Standard errors clustered at the province level are in parenthesis. Bootstrapped p-values in brackets.

†: 10% significance, *: 5% significance, **: 1% significance

Table 5: Impact of alimony rights on labor supply-Excluding Quebec

	Studied	Worked	Worked full time	Work interruptions	Maternity leave	Paternity leave
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Only relationships formed before a law was passed					
Subject to alimony rights	0.013	-0.072†	-0.125**	0.021	-0.015	
	(0.012)	(0.034)	(0.031)	(0.031)	(0.019)	
Subject to alimony rights	-0.034	0.173*	0.229**	-0.054		-0.002
*male	(0.030)	(0.067)	(0.065)	(0.042)		(0.017)
R-square	0.373	0.698	0.703	0.439	0.402	0.343
N	14848	14848	14848	14848	9047	5801
	Panel B: All relationship-years except those "caught"					
Subject to alimony rights	-0.010	0.015	0.046*	-0.012	-0.009	
	(0.014)	(0.019)	(0.016)	(0.023)	(0.018)	
Subject to alimony rights	0.043	0.031	0.007	-0.023		-0.005
*male	(0.035)	(0.027)	(0.022)	(0.026)		(0.007)
R-square	0.471	0.728	0.733	0.452	0.402	0.113
N	39087	39087	39087	39087	22762	16325

Equation (23) specifies the estimation equation for Panel B. Panel A excludes estimates of δ_k from that equation. All regressions are weighted using person-specific weights. The sample includes at least ten years following the beginning of any cohabitation relationship or all the years if the relationship lasted longer. In Panel A, only relationships formed before the legislation are included. In Panel B, all relationships except years in which relationships formed before the law were subject to alimony rights are included. All relationships from respondents born in Quebec are excluded.

Standard errors are clustered at the province level.

†: 10% significance, *: 5% significance, **: 1% significance

Table 6: Impact of alimony rules on labor supply-robustness checks

	No child rules (1)	Only active rel. (2)	Before 1990 (3)	After 1980 (4)	Married (5)	Lagged (6)
			Panel A: Studied			
Subject to alimony rights	0.020†	0.022	0.008	0.038*	0.002	0.030†
	(0.011)	(0.013)	(0.014)	(0.012)	(0.004)	(0.016)
Subject to alimony rights	-0.038†	-0.034	-0.027	-0.075**	-0.003	-0.067*
*male	(0.017)	(0.020)	(0.021)	(0.016)	(0.005)	(0.023)
Will be subject to						-0.008
alimony in 2 years						(0.033)
Will be subject to						0.066
alimony in 2 years*male						(0.040)
R-square	0.475	0.574	0.450	0.477	0.429	0.476
N	38793	24286	29212	27795	150347	39066
			Panel B: Worked full time			
Subject to alimony rights	-0.044†	-0.028	-0.062†	-0.031	0.053**	-0.072**
	(0.021)	(0.022)	(0.030)	(0.027)	(0.007)	(0.018)
Subject to alimony rights	0.097*	0.076†	0.127**	0.108*	-0.006	0.134**
*male	(0.030)	(0.037)	(0.035)	(0.041)	(0.007)	(0.036)
Will be subject to						0.045
alimony in 2 years						(0.041)
Will be subject to						-0.079
alimony in 2 years*male						(0.061)
R-square	0.720	0.775	0.717	0.727	0.723	0.723
N	38793	24286	29212	27795	150347	39066
			Panel C: Work Interruption			
Subject to alimony rights	0.070*	0.054	0.060	0.068†	-0.035**	0.052
	(0.025)	(0.030)	(0.037)	(0.029)	(0.008)	(0.031)
Subject to alimony rights	-0.080*	-0.051	-0.082	-0.097*	0.054**	-0.070
*male	(0.032)	(0.044)	(0.048)	(0.033)	(0.014)	(0.039)
Will be subject to						-0.054
alimony in 2 years						(0.044)
Will be subject to						0.077
alimony in 2 years*male						(0.046)
R-square	0.435	0.471	0.423	0.443	0.523	0.446
N	38793	24286	29212	27795	150347	39066

All regressions include the same controls as in Panel A of the previous table. All regressions are weighted using person-specific weights. The sample includes at least ten years following the beginning of any cohabitation relationship, except in column (2) where it includes only years where the cohabitation relationship was active. The first column only includes, as subject to alimony rules, couples who qualified because of the duration of their relationship, not because they had children. The third restricts it to relationships that began before 1990, the next one, to couples formed before 1980. The fifth column only includes married couples and the last includes an indicator for becoming subject to the law in 2 years.

Standard errors are clustered at the province level.

†: 10% significance, *: 5% significance, **: 1% significance

Table 7: Impact of alimony rights on labor supply-heterogenous effects

	Studied		Worked full time		Work Interruptions	
	Main effect	Interaction	Main effect	Interaction	Main effect	Interaction
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: by age difference						
Fem. at least 5 yrs younger	0.063	-0.109**	-0.090*	0.187**	0.158*	-0.211*
	(0.035)	(0.017)	(0.035)	(0.049)	(0.054)	(0.075)
Within 5 years of spouse	-0.012	-0.010	-0.024	0.031	0.016	-0.005
	(0.024)	(0.036)	(0.025)	(0.032)	(0.055)	(0.069)
Fem. at least 5 yrs older	-0.003	0.062*	0.043	-0.107	-0.032	-0.030
	(0.013)	(0.025)	(0.034)	(0.060)	(0.056)	(0.057)
Panel B: by education						
Less than high school	0.021	0.000	0.029	0.014	0.096†	-0.109
	(0.022)	(0.031)	(0.036)	(0.037)	(0.044)	(0.062)
High school graduate	0.027*	-0.046	-0.053	0.107†	0.040	-0.045
	(0.010)	(0.030)	(0.044)	(0.054)	(0.039)	(0.051)
College graduate	-0.019	-0.210	-0.184*	0.335**	0.032	-0.147
	(0.048)	(0.127)	(0.073)	(0.091)	(0.078)	(0.136)
Panel C: by age						
Began rel. before age 21	0.063†	-0.025	0.034	0.043	-0.002	0.021
	(0.033)	(0.057)	(0.054)	(0.118)	(0.060)	(0.052)
Began rel. between 22-27	-0.005	-0.017	-0.086	0.142*	0.085	-0.111
	(0.017)	(0.026)	(0.047)	(0.047)	(0.064)	(0.069)
Began rel. after 28	0.008	-0.081	-0.079†	0.117	0.069	-0.100
	(0.044)	(0.061)	(0.037)	(0.067)	(0.073)	(0.078)

All regressions include the same controls as in Panel A of Table 4. All regressions are weighted using person-specific weights. The sample includes at least ten years following the beginning of any cohabitation relationship. Each set of columns and section of the table correspond to a regression. The table entries are the coefficients of an indicator for the couple being subject to alimony rules interacted with the characteristics as listed in the first column of the table (for columns (1), (3), (5)) and the interaction of that term with a dummy for the respondent being a male in columns (2), (4) and (6).

Standard errors are clustered at the province level.

†: 10% significance, *: 5% significance, **: 1% significance

Table 8: Change in respondent's or partner's characteristics in response to the new legislations

	Respondent's characteristics				Partner's characteristics		
	HS degree	College degree	Attended rel. serv. at 15	Age at beg. of relation	Male $>$ 5yrs older	Male $>$ 5yrs younger	Prev. unmarried
				Panel A: Males			
Formed after legal change	0.021	0.060*	0.084	-0.353	-0.032	-0.028	0.018
	(0.042)	(0.022)	(0.076)	(0.916)	(0.022)	(0.029)	(0.018)
N	3235	3235	3199	3240	2912	2912	3234
Formed after legal change*	0.069*	-0.053	-0.068	-1.019	0.029	-0.011	-0.050
Lasted long enough	(0.024)	(0.035)	(0.043)	(1.004)	(0.039)	(0.017)	(0.063)
N	3235	3235	3199	3240	2912	2912	3234
				Panel B: Females			
Formed after legal change	-0.010	0.020	0.056	0.336	0.017†	0.027	-0.000
	(0.020)	(0.031)	(0.053)	(0.588)	(0.008)	(0.031)	(0.033)
N	4276	4276	4223	4280	3862	3862	4268
Formed after legal change*	0.013	0.044	-0.026	-0.196	0.010	0.011	-0.001
Lasted long enough	(0.037)	(0.031)	(0.031)	(0.513)	(0.012)	(0.042)	(0.021)
N	4276	4276	4223	4280	3862	3862	4268

All regressions include fixed effects for the year in which the relationship began, the province of birth of the respondent. The bottom of each panel, in addition, includes dummies for the relationships lasting more than 2, 3 or 5 years, an indicator dummy for having been formed after the legal change and an indicator dummy for the relationships lasting the required amount of time in the province of birth, if that province ever had such a law. All regressions are weighted using person-specific weights.

Standard errors are clustered at the province level.

†: 10% significance, *: 5% significance, **: 1% significance

Table 9: Policy neutrality of alimony rights rules

	Studied	Worked	Worked Full-Time	Work Interruption
	(1)	(2)	(3)	(4)
Panel A: Comparing new and old relationships (N=63493)				
Formed after a legal change	-0.076	0.003	0.017	-0.076**
	(0.208)	(0.034)	(0.029)	(0.020)
Formed after a legal change*male	0.001	0.003	-0.053	0.071**
	(0.453)	(0.036)	(0.037)	(0.021)
R-square	0.021	0.046	0.055	0.037
Panel B: Comparing new and old relationships over time (N=63493)				
Formed after a legal change	-0.013	0.004	0.020	-0.091**
	(0.010)	(0.033)	(0.031)	(0.021)
Formed after a legal change*male	0.033**	-0.005	-0.061†	0.083**
	(0.010)	(0.032)	(0.033)	(0.024)
Formed after a legal change	-0.002	-0.004*	-0.005**	0.008**
time since beginning	(0.001)	(0.001)	(0.002)	(0.001)
Formed after a legal change	0.000	0.004	0.006*	-0.007**
time since beginning*male	(0.001)	(0.003)	(0.002)	(0.001)
R-square	0.032	0.047	0.056	0.039

All regressions include fixed effects for province, year of observation and their interactions with a male dummy. Regressions in Panel B also include controls for the time since the beginning of the relationship and its interaction with being male. The sample includes at least ten years following the beginning of any cohabitation relationship except for years where the couple was subject to alimony rights when they had entered into the relationship before the legal change. All regressions are weighted using person-specific weights.

Standard errors are clustered at the province level.

†: 10% significance, *: 5% significance, **: 1% significance

Table 10: Impact of alimony rules on relationship stability

	Relationship is over in 2001		Cohabitation led to marriage		Duration		Duration (total)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Only relationships formed before a law was passed							
Subj. to alimony rights	-0.109	-0.117	-0.140**	-0.177**	2.604*	2.125*	0.530	0.505
	(0.081)	(0.085)	(0.025)	(0.047)	(0.806)	(0.705)	(0.762)	(1.180)
Subj. to alimony rights		0.063		0.064		0.468		-1.122
male		(0.063)		(0.098)		(0.517)		(1.643)
R-square	0.294	0.308	0.262	0.275	0.917	0.921	0.480	0.495
N	4025	4025	4025	4025	4025	4025	4014	4014
	Panel B: All relationships except those "caught"							
Subj. to alimony rights	0.051	0.029	-0.007	0.017	-0.211*	-0.347*	-1.262†	-0.587
	(0.038)	(0.035)	(0.020)	(0.029)	(0.084)	(0.114)	(0.620)	(0.692)
Subj. to alimony rights		0.063		-0.045		0.230†		-1.806
*male		(0.043)		(0.060)		(0.108)		(1.010)
R-square	0.261	0.269	0.262	0.276	0.955	0.956	0.502	0.516
N	7163	7163	7163	7163	7163	7163	7147	7147
	Panel C: Only relationships formed after the legal change							
Subj. to alimony rights	-0.029	-0.059*	0.007	-0.036	-0.097†	-0.046	-0.210	-0.149
	(0.016)	(0.018)	(0.014)	(0.025)	(0.044)	(0.091)	(0.242)	(0.414)
Subj. to alimony rights		0.082†		0.092†		-0.108		-0.199
*male		(0.038)		(0.044)		(0.103)		(0.627)
R-square	0.210	0.224	0.259	0.279	0.948	0.948	0.555	0.578
N	3495	3495	3495	3495	3495	3495	3489	3489

Equation (23) specifies the estimation equation for Panel B. Panel A excludes estimates of δ_k and Panel C includes only estimates of γ_k, ρ_k and fixed effects for province from that equation. All regressions are weighted using person-specific weights. In Panel A, only relationships formed before the legislation are included. In Panel B, all relationships except years in which relationships formed before the law were subject to alimony rights are included. In Panel C, only relationships formed after the legal change are included.

Standard errors are clustered at the province level.

†: 10% significance, *: 5% significance, **: 1% significance

A Proof of the supermodularity of S

Claim:

$$S(t) = \eta(t) + 2\bar{\theta} + [1 - \alpha(t)] \left\{ \eta(t) + 2E\left[\theta \mid \theta \geq \hat{\theta}(t)\right] \right\} + \alpha(t)\, t \ . \tag{A.1}$$

is super modular.

Proof: Recall that $\eta(t)$ is super-modular and thus strictly convex.

Then,

$$S(t) = \eta(t) + \int_{\bar{\theta}}^{\infty} (\eta(t) + 2\theta) f(\theta) d\theta + t \int_{-\infty}^{\bar{\theta}} f(\theta) d\theta \tag{A.2}$$

and

$$\bar{\theta}(t) = -\frac{1}{2}(h(t) - t) \tag{A.3}$$

Therefore,

$$\begin{aligned} S'(t) = & \quad \eta'(t) + \int_{\bar{\theta}}^{\infty} \eta'(t) f(\theta) d\theta + \int_{-\infty}^{\bar{\theta}} f(\theta) d\theta + f(\bar{\theta})[-\eta(t) - 2\theta + t]\bar{\theta}'(t) \\ = & \quad \eta'(t) + \int_{\bar{\theta}}^{\infty} \eta'(t) f(\theta) d\theta + \int_{-\infty}^{\bar{\theta}} f(\theta) d\theta \end{aligned} \tag{A.4}$$

and

$$\begin{aligned} S''(t) = & \quad \eta''(t) + \int_{\bar{\theta}}^{\infty} \eta''(t) f(\theta) d\theta + f(\bar{\theta})[-\eta'(t) + 1]\bar{\theta}'(t) \\ = & \quad \eta''(t) + \int_{\bar{\theta}}^{\infty} \eta''(t) f(\theta) d\theta + f(\bar{\theta}) \frac{[-\eta'(t) + 1]^2}{2} > 0 \end{aligned} \tag{A.5}$$

Hence, $S(t)$ is convex in t, implying that z and y are complements.

B Intra-temporal Allocations & Commitment vs. Renegotiation

In our framework, the two alternative views about commitment have a natural translation. In the first case ('commitment'), couples can commit to their spousal allocations in both periods conditional on the continuation of their union; the corresponding contingent allocations are ex-ante efficient under the sole constraint that separation is unilateral. Therefore, the only constraint on intra-temporal allocations is that second-period utility should exceed singles' utility, at least insofar as separation is not an efficient outcome. Finally, should an unexpected event occur between the two periods, such as a reform of the alimony-payment laws, this would not trigger a renegotiation of the initial agreement, unless the new individual rationality constraint is violated for one spouse. In the latter case, such a spouse would receive an additional share of household resources so that she becomes just indifferent between marriage and singlehood under the new law.

In the alternative, polar case ('no commitment'), serious limits exist on the spouses' ability to commit. In this case, couples may be able to commit to the immediate (i.e. first-period) allocation of resources; but future allocations cannot be contracted upon and will therefore be determined by a bargaining mechanism at the beginning of the second period. This feature is known ex ante by the agents and it influences the decisions regarding first-period allocations. Finally, if a reform occurs between the two periods, the new situation is taken into account during second-period bargaining; i.e., bargaining always take place 'in the shadow of the law'.

As in standard contract theory, we assume in all cases that a minimal level of commitment, whereby agents are able to at least commit to *first-period* allocations when they get married, is attainable. Such contracts are actually (second-best) efficient under the constraint that agents cannot commit not to divorce. Similar ideas are used in different contexts, in particular risk sharing agreements under limited commitment (see Ligon et al., 2002, and Kocherlakota and Pistaferri, 2008).

Regardless of the extent and depth of commitment, however, cohabitation and marriage contracts should technically be seen as long-term efficient agreements under one constraint — namely that a person who wants to separate can always choose to do so. See Lundberg and Pollak (1993)

for alternative assumptions. Also see Lundberg and Pollak (1993) and Mazzocco (2007) for further discussions of commitment issues within marriage.