2006

Divorce Laws and the Structure of the American Family

Stéphane Mechoulan, *Dalhousie University*

Divorce Laws and the Structure of the American Family

Stéphane Mechoulan

ABSTRACT

This paper investigates the impact of no-fault divorce laws on marriage and divorce in the United States. I propose a theory that captures the key stylized facts of the rising then declining divorce rates and the apparent convergence of divorce rates across the different divorce regimes. The empirical results suggest that a shift from fault to no-fault divorce increased the odds of divorcing for those couples who married before the shift. The analysis further suggests that those couples who marry after the shift to a no-fault regime, in turn, sort themselves better upon marriage, which offsets the direct effect of the law on divorce rates. Consistent with that selectivity argument, after a switch to a no-fault divorce regime, women get married later in life. These results hold for the law that governs property division and spousal support. The law that governs divorce grounds does not seem to matter significantly.

INTRODUCTION

In the 1970s and early 1980s, the United States underwent what is commonly referred to as a "divorce revolution" (Weitzman 1985). Prior
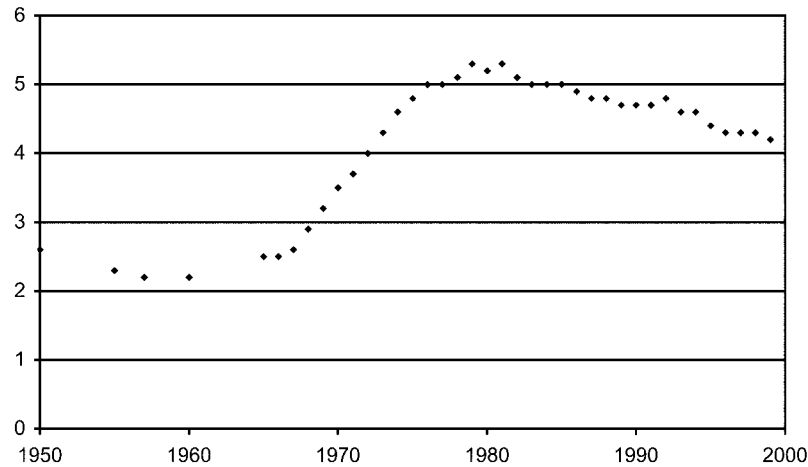
Figure 1. U.S. divorce rate per 1,000 population (National Center for Health Statistics 1950–2000).

to 1970, spouses typically either had to wait for a long period of time or had to prove that a fault had been committed in order to get a divorce. The reforms authorized spouses in most states to obtain a divorce by simply alleging a specific no-fault ground such as irreconcilable differences or irretrievable breakdown of the marriage. During the same period, about half the states adopted no-fault rules for property division and spousal support (henceforth referred to as "property").

Simultaneously, starting from the late 1960s, the divorce rate increased dramatically. It peaked in the late 1970s and early 1980s and has decreased slowly but continuously ever since (see Figures 1 and 2). In almost every state, divorce rates started to rise before the legal changes occurred, which has cast much doubt on the impact of these laws. The key is to address the potential endogeneity of divorce laws themselves and to disentangle the causation process: Were the new divorce laws at least partially a cause of the surging divorce rate? Did they accelerate the trend, if they did not create it? Or did they merely involve an upgrading of the legal texts, an adjustment to the common practice of courts and new attitudes regarding marriage?[1]

Divorce laws have received much attention lately. Gardiner et al.

1. In particular, there has been a growing acceptance of divorce within the U.S. population, especially in the presence of children; see, for example, Glenn (1991).
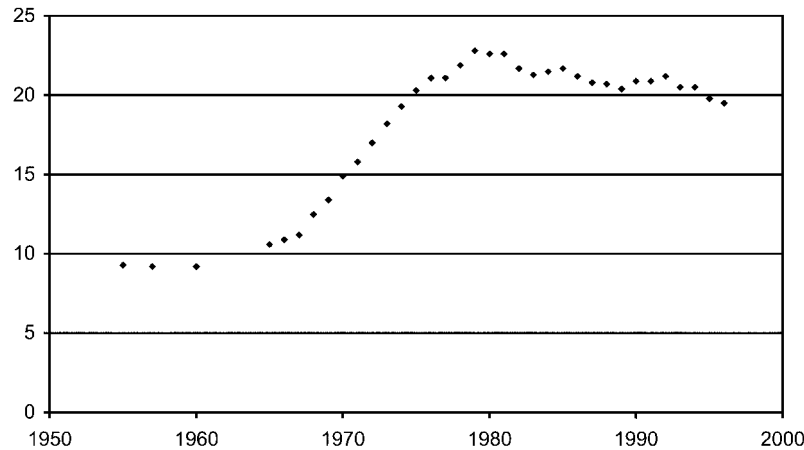
Figure 2. Divorce rate per 1,000 married women (National Center for Health Statistics 1950–2000).

(2002) provide an excellent overview of a wide range of state-level pro-family policies recently introduced in the United States, including the reintroduction of covenant marriages and the removal of marriage penalties in tax codes and Medicaid programs. A commonly expressed claim in support of a return to a fault rule is that the move to no fault caused the divorce rate to rise. One can add the arguments put forward by the fathers' rights movements, which take a strong position against no-fault divorce and its impact on custody of children (grandparents' groups make similar contentions) or the "supermom burnout" assertion that no-fault divorce has forced mothers to increase hours of work as a form of insurance against divorce. There is also a growing literature on the effect that no-fault divorce has had on the "culture" of divorce and the diminished social value placed on the institution of marriage (Dnes and Rowthorn 2002; Grossbard-Shechtman 2003).

Since the mid-1970s, a vast body of literature has sought to understand the effects of these divorce laws. After Peters's (1986, 1992; see also Allen 1992) seminal contributions, research focused on aggregate, state-level panel data to detect the impact of divorce laws changes on divorce rates.[2] There appears to be a consensus among scholars that no-

2. See Brinig and Buckley (1998); Ellman and Lohr (1998); Gray (1998, which also uses Current Population Survey and Panel Study of Income Dynamics data); Friedberg (1998); Gruber (2000); and Wolfers (2003) to cite only recent studies that have appeared in the economics literature.
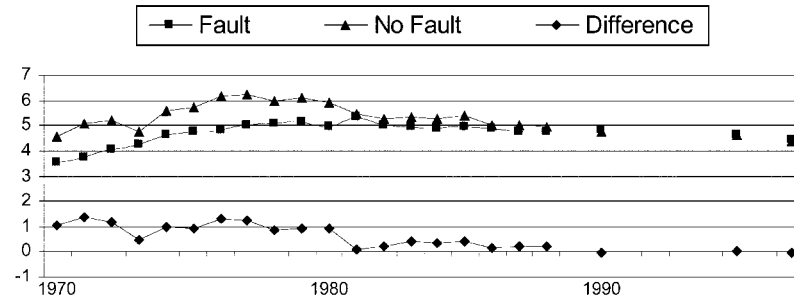
Figure 3. U.S. divorce rates by property regime (National Center for Health Statistics 1950–2000; U.S. Census Bureau 1999).

fault divorce produced a sudden burst in the divorce rate after the new laws were enacted. Yet there is still controversy around whether the changes were short-term or structural.[3]

The starting point of this analysis rests on the following observations: not only have aggregate divorce rates decreased since most of the legal changes were passed, but the average difference in divorce rates across different divorce regimes has been narrowing (Figure 3).[4] One should then also investigate why, since the early 1980s, divorce rates have decreased faster, on average, in states where fault is not considered for property.

To this end, the present contribution provides an empirical analysis of the underlying causation process between divorce laws, marriage, and divorce: the key extension on past literature is to consider that the changes in the laws may have different consequences depending on when marriages were contracted, that is, before or after the legal changes. In particular, this approach allows us to tell whether the divorce rate in any given regime is different for those married under different divorce laws.

The paper explores the hypothesis that spouses take the law into account and sort themselves differently according to which rule governs their possible future divorce. The main consequence is that the changes

3. Friedberg (1998) found permanent, structural effects. However, using the same data, Ellman (2000) and Wolfers (2003) questioned the robustness of those results.

4. Friedberg (1998) and Wolfers (2003) analyze this difference in difference, but their aggregate series stops in 1988, and therefore the bulk of the last divorces they analyze are for marriages contracted in the late 1970s and early 1980s.

in divorce laws did not necessarily translate into structural differences in divorce rates over time but rather more deeply into differences regarding match quality in marriage. Match quality, in turn, would affect the divorce rate and may thereby offset the effects of the change in the law. While the direction of the sorting is theoretically ambiguous, this study shows why a better match quality at the marriage stage is a consistent explanation for the patterns in the data described above. Such sorting would become apparent in the timing of marriage as well.

Using cross-sectional micro data from the June supplements of the Current Population Survey (CPS) (U.S. Department of Labor 1971–98), the findings first confirm that for couples who married before the changes in the law, there was a significant impact of no fault for property on divorce odds: this is referred to as the "pipeline effect" (that is, the increased divorce rate resulting from the divorces of couples whose marriages were falling apart but who did not divorce until the new law took effect). Most important, among individuals who have not experienced a change in property law since their marriage, the odds of divorce are found to not differ significantly between the two regimes: my interpretation is that the direct effect and the indirect offsetting effect (that is, better selection at marriage) cancel out. The law defining divorce grounds, in contrast, has a more limited impact on divorce probabilities. Further, there is evidence of a delay in marriage for women when fault is irrelevant for property decisions: ceteris paribus, a longer search also points toward better matching.

Consistent with the idea of adaptive behavior, I investigate both the overall average effect of no-fault divorce as well as its effect over time: the results suggest that those changes in the law that took place well after the first changes of the early 1970s still had some effect for those who married before those late changes. On the other hand, the selection effect is on par with other recent findings (Rasul 2003) and provides a clear economic rationale for the statistical argument, based on aggregate data, that no-fault divorce had only a short-run effect on divorce rates (Wolfers 2003).

As a caveat, it should be stressed that the use of CPS data is not without its problems: one does not know the migration history of the sample respondents, yet the American population is extremely mobile. Further, in most waves of the survey, divorcees do not report when they got divorced. Therefore, this study limits itself to individuals who married for the first time within a short time interval before their interview: this option bounds the error probability in assuming that respondents
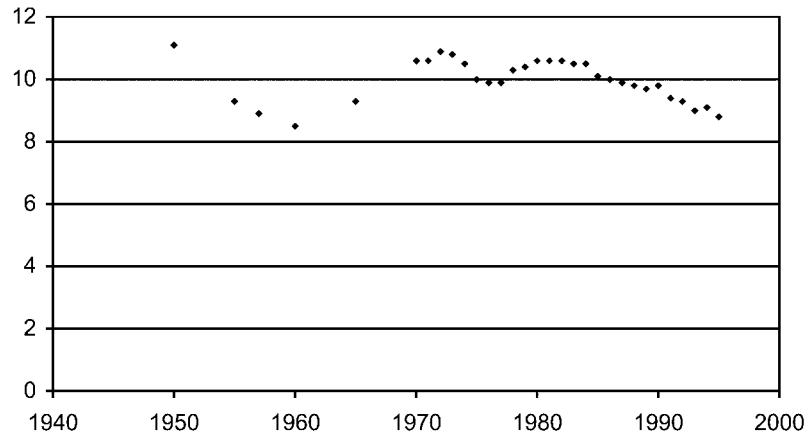
Figure 4. Marriage rates per 1,000 population (National Center for Health Statistics 1950–2000; U.S. Census Bureau 1999).

had not moved since their marriage and, most important, in classifying divorcees into one legal regime or the other. The divorce estimates obtained in the regressions cannot therefore be directly compared to those provided in Figure 3, which aggregate divorces per 1,000 population in each state and year. However, they should be seen as different angles of the same global picture. Put differently, this paper does not attempt to estimate the impact of divorce laws on divorce rates per 1,000 population. While it has been the object of past empirical research, this statistic is problematic: it aggregates divorces from marriages that were contracted under different legal regimes—thus mixing people with different incentive structures at marriage, as well as divorces from first marriages and remarriages. Therefore, these rates also indirectly capture the incentive effect of the legal changes on marriage and remarriage, making the identification of the impact of the laws on divorce per se very difficult. In particular, note that while overall marriage rates appear constant in the 1970s and 1980s (Figure 4), first-marriage rates have steadily decreased since the early 1970s (Figure 5): this is explained by the high number of remarriages, naturally following the increasing number of divorces over the same period.

Finally, it is important to emphasize that marriage vintage and endogenous selection effects in the "marriage market" by no means tell the whole story underlying divorce rate trends in the United States. That
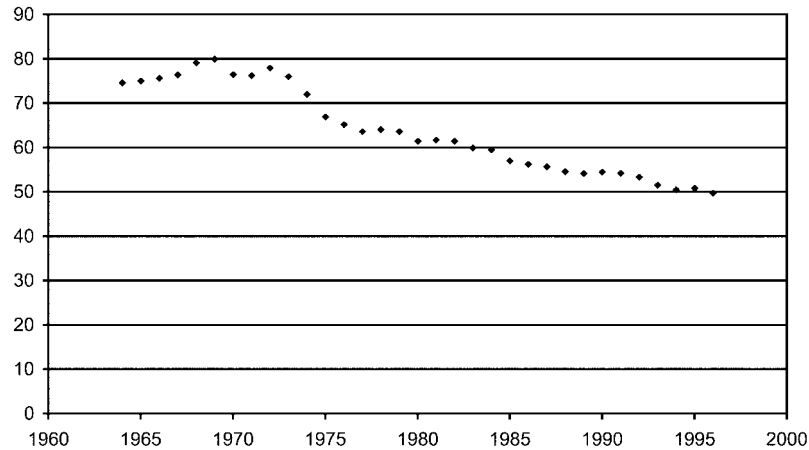
Figure 5. First-marriage rate per 1,000 unmarried women, 15 years and older (National Center for Health Statistics 1950–2000; U.S. Census Bureau 1999).

being said, any explanation must take into account the key empirical facts of rising then declining divorce rates and the apparent convergence of those rates across different divorce regimes.

The remainder of the paper is organized as follows. Section 2 provides further motivation for this research with a discussion of how to categorize divorce laws and with an overview of the existing literature. Section 3 describes the data; Section 4 presents the empirical methodology and sets out the results and interprets them; and Section 5 concludes the paper.

MOTIVATIONS AND LITERATURE ANALYSIS

Background

The concept of the divorce revolution covers a complex, multifaceted sequence of events, and there are detailed, excellent accounts of that story that focus on the now well understood causes of the movement and describe the actual changes that took place in the courts (see, for example, Jacob 1988; Parkman 1992).

In short, in the old system, proof of marital fault—defined differently in each state, but usually comprising adultery, desertion, or physical abuse, for example—or a long separation between spouses was required

before a court would grant a divorce decree. Therefore, it was not uncommon for one spouse to endorse a bogus fault in court and to have friends commit perjury in order for a couple to be granted a divorce.

Several reasons for the divorce reform movement initiated in the 1970s can be advanced: to save the U.S. judicial system from a procedure that validated hypocrisy and led to condoning perjury, to allow obviously separated couples to formalize their status, and to grant divorces to permit remarriage. Also, it was felt that spouses could not reach efficient bargains under the old rules. Finally, a long-forgotten, original motivation was to encourage reconciliation through a less conflict-oriented procedure.

This reform movement resulted in various rules that made it possible for one spouse to obtain a divorce without citing a fault by the other spouse or having the other spouse's consent, for that matter. Some states abolished all fault grounds, whereas others added a no-fault provision (incompatibility, irreconcilable differences, or irretrievable breakdown of the marriage) to traditional fault grounds. This distinction will be not pursued in this work since fault is rarely used in practice when no-fault grounds have been added.[5] Finally, in states in which no such grounds were introduced, separation requirements were usually shortened. In the following, I define as having no-fault grounds only those states that have enacted specific no-fault statutes, even though there are other states that have not generally specified that a no-fault divorce may be granted after the spouses live separate and apart for some minimum period of time.[6]

Simultaneously, legislatures changed rules concerning the financial part of the procedure. However, it is important to note that property regimes changed in more than one way. The first one dealt with what was to be considered marital assets. Each spouse was now allowed to share in what had been available only in community-property states:

5. Fault still retains a role in bargaining and is still used about 10 percent of the time in states where both fault and no-fault divorce are available. I thank an anonymous referee for pointing this out.

6. All states now have some form of no-fault divorce. While divorce can be instantaneous for many fault grounds, a small number of states recognize a long waiting period of separation as the only no-fault ground for divorce. Other states require separation in addition to other no-fault grounds, or if the divorce is contested, one spouse can obtain a postponement. I do not count as having fault those states in which a no-fault divorce may not be granted if it is not agreed upon by both spouses and where separation alone may not be grounds for a no-fault divorce. This decision is disputable, however estimation results were not found to be sensitive to the classification of these states.

everything earned by either of them. This was a set of changes that theoretically benefited women at divorce. At the same time, many states also eliminated or greatly reduced permanent alimony. This was a set of changes that theoretically benefited men at divorce (see Fineman 1983, 1991). Almost simultaneously, many states barred the consideration of fault in asset division and spousal support settlements.[7] With regard to property regimes, this work focuses on that no-fault dimension.

An analysis of all the theoretical and empirical work on this issue would require a lengthy survey paper, and it is still a very intensive area of research. From a theoretical standpoint, a strict application of the Coase theorem implies that divorce laws should not influence divorce rates: theoretically, spouses could write a binding prenuptial agreement that would bypass any legal change.[8] Yet it seems fair to say that the assumptions of perfectly transferable utility and no transaction costs, on which the Coase theorem rests, are not realistic in the context of marriage and divorce.[9] In the empirical literature, the conclusions on whether a change to no-fault divorce resulted in higher divorce rates have been going back and forth—even though we observe a recent convergence. There is a consensus about the existence of a short-term increase in divorce rates immediately after the enactment of the no-fault regimes. I will advance here that there was also a structural behavioral effect and characterize which dimension of the divorce revolution was responsible for it.

Trends

Another point that needs to be emphasized is that divorce rates per 1,000 population have been decreasing since the early 1980s, as can be seen from Figure 1. The time series I prefer to focus on is the divorce rate per married woman, shown in Figure 2. But they exhibit similar trending

7. Almost always, the statutes making the first set of changes outlined above were also the ones that changed from fault to a no-fault regime for property. Also, those states that eliminated or reduced permanent alimony were usually those moving to a pure no-fault regime rather than adding a no-fault ground to the fault ones. I thank an anonymous referee for these remarks.

8. However, spouses in the United States cannot contract around grounds for divorce. An exception is the covenant marriage states, which allow spouses to choose between two state-sanctioned sets of grounds. I thank an anonymous referee for this point.

9. Grossbard-Shechtman and Lemennicier (1999) provide a comprehensive discussion of the transaction cost issue. Lundberg and Pollak (2001) and Murphy (2002) present models of household behavior in which limits on marital contracting lead to inefficient outcomes. Zelder (2002) provides a very good overview of the theoretical literature on household bargaining and the empirical evidence on the efficiency of household decisions.

patterns, so looking at the former alone is not too misleading, provided the full time series is considered.

More puzzlingly, divorce rates have dramatically converged across regimes. Figure 3 depicts the divorce rates by property regime across time (note that rates for Nevada—the migratory divorce state—have been eliminated, as they are outliers, but adding them would actually reinforce the convergence pattern).[10] The number of states included in each series (fault and no-fault property) varied each time a state moved from a fault to a no-fault regime (the reverse never happened). A state that enacted no-fault laws in a given year was excluded when computing the average divorce rate of no-fault states that year. Thus, this figure is a picture without (most of) the so-called pipeline effect defined earlier. Excluding a state for more than 1 year to account for separation requirements would produce an analogous pattern. With a similar methodology, Rasul (2003) graphs divorce rates per married women using CPS data, separating those states that adopted "unilateral divorce" (in his terminology)[11] from those that did not, and obtains a very comparable picture.

This is a recent finding. However, we should concentrate on the trend and not on the magnitude of the divorce rates reported in Figure 3. Recall that this figure picks up divorce rates per 1,000 population,[12] and most important, as in Figure 2, it aggregates divorces from marriages that were contracted under different legal regimes. Therefore, the interpretation of those rates in isolation could be deceptive. But we know that the stock of marriages at high risk for divorce is composed of marriages with short tenure (the first 10 years of marriage, roughly speaking); so the divorces observed in no-fault states are increasingly coming from marriages contracted under no-fault rules, as we move forward in time.

Although standard demographic explanations can account, to some extent, for the patterns found in Figures 1 and 2,[13] the qualitative pattern

10. In Figure 3, the average divorce rates under each regime are calculated following Table A1.

11. "Unilateral" and "mutual consent" divorce are the terms fellow economists in this literature prefer to use. Sometimes they spell out the distinction between grounds and property, sometimes not.

12. The divorce rate per married woman is not available at the state level in the *Vital Statistics of the United States* (National Center for Health Statistics 1960–88).

13. For more on the aggregate compositional age effect on the marriage market, see, for example, Heer and Grossbard-Shechtman (1981), Grossbard-Shechtman and Granger (1998).

observed in Figure 3 is enough to tease out the main intuition that there are sorting, or selection, effects at the marriage stage that compete with the pure effect of no-fault divorce, and that intuition provides the key motivation of this work.

Interpretative Framework

In the Appendix, I construct a very simple interpretative framework that focuses on the change in the incentives to create high-quality matches and their consequences on divorce trends. It goes without saying that the issue is extremely complex, and it is very difficult to capture all the pieces of the puzzle; therefore, the framework abstracts from many interesting features that would make it appear more realistic.[14]

The model yields the following conclusions. (1) Ceteris paribus, people who married under a (broadly defined) fault regime are more likely to divorce after a shift to no-fault rules. (2) Given the pattern described in Figure 3, however, the model also implies that the match quality of those who marry under a new no-fault law should be higher than that of those who married under a previous, fault-based regime: this is a purely mechanical result. (3) Still, it is impossible to determine a priori if the higher selectivity of those married after a no-fault rule is passed would make the long-run equilibrium divorce rate lower under the no-fault regime than under the fault regime. It could be that, although more selective, the partners would not be selective enough to compensate for the higher probability of divorce generated by a no-fault rule. These three points are the subject of the empirical part of this work.

DATA

Sources

I use the June files of the CPS compiled as of 1971.[15] These data have the advantage that they provide some information on the marital history of the respondents. The number of observations is large, and this made the June CPS files a better candidate than the Panel Study of Income Dynamics.

Specifically, I use those waves that have information regarding the

---

14. A more sophisticated model of marriage markets that specifically examines the effects of divorce law liberalization on marriage market outcomes using the tools of search theory can be found in Rasul (2003).

15. From 1971 to 1998, years 1978, 1989, 1993, 1996, and 1997 are missing.

state location of the respondent (most of them have such information). Almost no wave specifies the date of divorce, however, which makes impossible the use of a hazard regression model. Further, for questions pertaining to marriage duration, the universe was composed only of women in most waves. Unfortunately, no migration information is available in this data set, so one needs to work as if the respondent stayed in her state over the entire course of her history.[16] We also miss other information, such as who filed for divorce, whether both parties agreed to the divorce, and so on.[17]

For the divorce and marriage rates, I use *Vital Statistics* (National Center for Health Statistics 1950–2000) and the *Statistical Abstract of the United States* (U.S. Census Bureau 1999). Summary statistics are presented in Table 1. The legal dummy variables (no fault for grounds, no fault for property) are constructed on the basis of Table A1. It should be noted that there has been substantial controversy on how to categorize divorce laws, as can be seen from Ashbaugh Vlosky and Monroe's (2002) piece devoted to that single question. Certain states are inherently controversial when it comes to classifying them into a fault or no-fault category, especially because of separation requirements. The present paper does not claim to present a criticism-proof classification: what matters most is whether the results are robust to changes in those few states where a particular classification is debatable and for which different views have been held in the literature.

In this paper, I classify as having no-fault grounds only those states that have enacted specific no-fault statutes. For the law defining divorce grounds, I rely on Kay's (1987) scholarship, and for the law defining fault in property and alimony, I rely on a slightly modified version of Ellman's (1996) classification, itself very close to that used in Brinig and

16. There is a way to get a back-of-the-envelope calculation of the error probability in assuming an event in the respondent's history happened in the same location in which the respondent lives at the time of the interview. On the basis of calculations from the Panel Study of Income Dynamics on women with the same characteristics as those in the present work, 9 percent of the sample can be expected to migrate between states each year. So any event occurring x years before the interview can be assigned a crude $9 \times x$ percent error. Furthermore, given that the regional migration rate is much smaller (roughly half) than the state migration rate, and given that regions are quite homogenous regarding divorce and property laws, this error term is really an upper bound of the probability that the respondent moved from one regime to another.

17. Historically, women have filed for divorce in the majority of cases, and there has been no discernible change in this trend with divorce law liberalization (Brinig and Allen 2000). What this tells us about who initiated the divorce is of course less clear-cut.

Table 1. Summary Statistics

	Mean	SD	Min	Max
Table 2:				
Divorced by interview time (everyone)	.042	. . .	0	1
No-Fault Ground	.69	. . .	0	1
No-Fault Property	.38	. . .	0	1
Education	12.94	2.37	0	18
Time elapsed since marriage (months)	24.71	14.23	0	48
Marriage year[a]	79.34	5.5	67	90
Table 3:				
Divorced by interview time				
(nonsurprised)	.041	. . .	0	1
No-Fault Ground	.68	. . .	0	1
No-Fault Property	.37	. . .	0	1
Education	12.95	2.38	0	48
Time elapsed since marriage (months)	24.05	14.14	0	48
Marriage year[a]	79.54	5.4	67	90
Table 4:				
Age at first marriage (months)	275.26	58	166	977
No-Fault Ground	.69	. . .	0	1
No-Fault Property	.38	. . .	0	1
Education	12.9	2.38	0	18

[a] Marriage year dummies are included in the regressions.

Buckley (1998) and Friedberg (1998).[18] I have also used an alternative specification for the grounds dimension using Friedberg's classification.[19] Finally, as mentioned before, the no-fault property variable may capture other changes in property and alimony regimes that occured at the same time as the enactment of the no-fault statutes. Only the net effect is identifiable. Therefore, to be perfectly accurate, one may interpret this variable as the combination of these multiple changes.

18. The main differences are no irretrievable breakdown and no no-fault property statute found for Arkansas, no fault for property in New York in practice after 1980 (fault is excluded from consideration in equitable distribution except for egregious cases that shock the conscience of the court; see, for example, *Havell v Islam,* 301 A.D.2d 339 [2002]), and fault for property in Utah after 1987. I thank an anonymous referee for those last two corrections.

19. Specifically, I have used columns 1 and 4 in Friedberg (1998, pp. 612–13). Those states where a "no" appears in column 1 are uniformly coded as zero even if a date appears in column 4. Estimation results using this alternative classification are available upon request.

Constructing the Sample

The sample pools cross sections of adult white female respondents between 1971 and 1990. The years 1975, 1984, 1991, and beyond were excluded because critical information was missing. Small states in year 1971, and to a larger extent in years 1973 and 1974, were identified only by their regions, so I dropped observations from them. Finally, my concern was to minimize the odds that a respondent might have moved to another state and, more important, become subject to a different divorce regime between her marriage and the time of the interview. A short marriage duration ensures that the location error will not matter too much. Therefore, I selected recently married women—specifically, I present here results for those who had married in the last 48 months before the interview: thus, on average, women are observed 2 years after marriage. The choice of 48 months is somewhat arbitrary, yet allows me to work with a large sample size with enough divorced respondents.

EMPIRICAL METHODOLOGY AND RESULTS

The econometric identification of the legal effects comes from the fact that many states changed their divorce laws during the period 1971–90 (see Table A1). More precisely, the identification comes from states in which one component of the law changed but not the other and, in states where both components changed, from those states in which the changes were not simultaneous. Table 1 shows that there are enough observations under different regimes for adequate identification. State dummy variables control for state fixed effects, year dummies control for time fixed effects, and I account for trend effects at the state level by using state × trend regressors (see, for example, Friedberg 1998). These variables should capture several evolutions in society that are unrelated to divorce law changes (such as greater opportunities for women in the workplace) and that may have an impact on the outcomes of interest. In particular, they will take care of any convergence in the definition of marital property and property division, regardless of the laws as they appear in the books. In the tables, No-Fault Ground indicates whether a state has introduced new no-fault grounds for divorce (such as irretrievable breakdown or irreconcilable differences) in addition to or instead of fault grounds or in addition to or instead of separation requirements, allowing, in theory, for a faster procedure. No-Fault Property indicates

whether a state considers fault in the property settlement and support award.

I provide specifications with state controls only and state controls plus local trends at the state level. The comparison provides insight into the robustness of the parameters. The regressions that have local trend effects (columns 2 and 2′ in Tables 2, 3, and 4) are the most meaningful and thus the ones on which to focus. I do not show specifications with no state fixed effects for clarity purposes.

We have observations on women who married before and after the changes. Yet those women who married before the changes, who were sampled after the law changes, and who declared themselves divorced might have divorced before the law changes. Therefore, the estimate of the impact of the divorce law on them is biased against finding a "surprise" effect. But we have no choice since we do not have the divorce date for most of the observations.

There are, therefore, for each legal dimension, two variables of interest: one for the surprised (married before the year of enactment of the change in their state) and one for the others (married after). The no-fault dummy for the surprised takes a value of one if the person was married under a fault-based regime and was sampled under no fault and zero otherwise. Conversely, for the nonsurprised the dummy takes a value of one if the person was married after the law changed to no fault and zero otherwise. Overall, 4 percent of the sample was surprised by no-fault property laws, while 5 percent was surprised by no-fault grounds.

In both cases, I investigate the following: as we move forward in time, how does the impact of the law change? For that purpose, I constructed the variables No-Fault Grounds × trend and No-Fault Property × trend.[20] The origin of the trend is the year of enactment of the first change for each legal dimension for all states (see Table A1) for the surprised and, if any, the year of enactment in each state for the others (that is, in the nonsurprised's states). The trend takes values (year of marriage − origin) for each observation. This way, I can measure whether the surprise effect dies out in states that implemented their change late. As for the nonsurprised, I can measure if the adaptation is immediate or gradual. The idea is that the equilibrium under the new

---

20. When simply using a dummy for the law, the dummy measures the average impact of the law over time. When using a dummy and a dummy × trend, the dummy measures the impact of the law at the time of enactment (it is purely an intercept) and the dummy × trend measures the impact of the law as it changes over time.

law may not be reached immediately. In practice, we typically expect transition dynamics that are not purely a stock/flow composition effect—in other words, some kind of social learning. To be on the safe side, those who married or are sampled in the year of enactment of the law are dropped from the sample (but including them does not change the results considerably).

Formally, let $y_{i,s,t}$ for individual i in state s at time t take the value of one if an individual declares herself divorced and zero otherwise, $x_{l,i}$ are individual characteristics (education, marriage year, and time elapsed since marriage), and cov and so on are dummies that indicate whether the individual was surprised by the different legal changes.

In the regressions that investigate the evolution of the impact of the legal changes, $T_{\langle grounds/surprised\rangle i,s,t}$, $T_{\langle grounds/nonsurprised\rangle i,s,t}$, and so on, are the legal-change-specific trends described above, 1_s is a state s dummy, 1_t a year t dummy, $x_{grounds,s,t}$ and $x_{property,s,t}$ are the law dummies in state s at time t, and $\varepsilon_{i,s,t}$ is the error term that accounts for serial correlation within each state s and year t:

$$
\begin{aligned}
y_{ist} = 1 \quad &\text{if } (\alpha_1 d_{\langle grounds/surprised\rangle i,s,t} + \alpha_2 d_{\langle grounds/nonsurprised\rangle i,s,t} \\
&+ \alpha_3 d_{\langle grounds/surprised\rangle i,s,t} T_{\langle grounds/surprised\rangle i,s,t} \\
&+ \alpha_4 d_{\langle grounds/nonsurprised\rangle i,s,t} T_{\langle grounds/nonsurprised\rangle i,s,t}) x_{grounds,s,t} \\
&+ (\alpha_5 d_{\langle property/surprised\rangle i,s,t} + \alpha_6 d_{\langle property/nonsurprised\rangle i,s,t} \\
&+ \alpha_7 d_{\langle property/surprised\rangle i,s,t} T_{\langle property/surprised\rangle i,s,t} \\
&+ \alpha_8 d_{\langle property/nonsurprised\rangle i,s,t} T_{\langle property/nonsurprised\rangle i,s,t}) x_{x_{property,s,t}} \\
&+ \beta 1_s + \gamma 1_t + l\delta_l x_{l,i} + \varepsilon_{i,s,t} > 0,
\end{aligned}
$$

and $y_{i,s,t} = 0$ otherwise, where $\varepsilon_{i,s,t} \sim N(0, \sigma^2)$; $\forall\ i,\ s,\ t,\ i',\ s',\ t'$, $\text{cov}(\varepsilon_{i,s,t}, \varepsilon_{i',s',t'}) = 0$, and $\text{cov}(\varepsilon_{i,s,t}, \varepsilon_{i',s,t}) \neq 0$. In the regressions with local trends, an interaction term between state dummies and a time trend starting in 1971 (the first wave of the survey) is added.

Finally, in the divorce regressions (Tables 2 and 3), we should focus on the sign and significance of the estimates more than on their magnitudes. Recall that the observed divorces originate from marriages with short tenure. The driving hypothesis behind the estimations is that any effect found for such couples would hold for couples with longer marriage tenure, which cannot be studied here with sufficient precision because of the lack of migration history in the data. The dummy coefficients

Table 2. Probit Estimation: Change in the Law (June Current Population Survey Pooled Cross Sections)

	(1)	(1′)	(2)	(2′)
No-Fault Ground	−.002	−.018	−.002	−.002
	(.004)	(.016)	(.005)	(.014)
No-Fault Ground × trend		6×10^{-4}		6×10^{-4}
		(7×10^{-4})		(8×10^{-4})
No-Fault Property	.015	.067	.02	.069
	(.007)*	(.007)**	(.01)*	(.04)*
No-Fault Property × trend		-1.6×10^{-3}		−.001
		$(9 \times 10^{-4})^{+}$		(9×10^{-4})
Pseudo R^2	.067	.067	.073	.073

Note. The dependent variable is Divorced by Interview Time. The sample comprises white women married for the first time, for at most 48 months, between 1971 and 1990, for whom there was a change in the law between marriage and the interview (the surprised). Specifications: (1) No-Fault Ground, No-Fault Property, state dummies; (1′) No-Fault Ground, No-Fault Property, No-Fault Ground × trend, No-Fault Property × trend, state dummies; (2) No-Fault Ground, No-Fault Property, state dummies, state dummies × trend; (2′) No-Fault Ground, No-Fault Property, No-Fault Ground × trend, No-Fault Property × trend, state dummies, state dummies × trend. All specifications control for education, marriage year (dummies), time elapsed since marriage, and no-fault variables for both surprised and nonsurprised. For No-Fault Ground and No-Fault Property, no fault equals one, and fault equals zero; divorced equals one, zero otherwise. The coefficient reported is the discrete change dF/dx computed at the means of the data, where F is the cumulative distribution function of $N(0, 1)$. The t-test reported is that of the underlying probit coefficient being zero. See the text for details. Number of observations = 44,748. Robust standard errors that allow for arbitrary correlation within a state in any given year are in parentheses.

  [+] Significant at the 10% confidence level.
  [*] Significant at the 5% confidence level.
  [**] Significant at the 1% confidence level.

reported in the probit regressions represent the marginal effects of the law (that is, the discrete changes dF/dx, where F is the cumulative distribution function of $N(0, 1)$ and x the law dummy), restricted to the means of the data. The t-tests reported are those for the underlying probit coefficients being zero and are unrestricted. [21]

Analysis of the Surprise Effect

Although the estimation is done with both the surprised and the non-surprised, I present only the effects for the surprised here. The results

21. This explains a phenomenon familiar to Stata users, that in the case of dummy coefficients, the marginal effects can be reported as statistically significant despite seemingly too large standard errors.

Table 3. Probit Estimation: No Change in the Law (June Current Population Survey Pooled Cross Sections)

	(1)	(1′)	(2)	(2′)
No-Fault Ground	.05	.004	4×10^{-4}	−.005
	(.004)	(.005)	(.006)	(.007)
No-Fault Ground × trend		.001		−.003
		(5×10^{-4})		(.001)*
No-Fault Property	.006	.01	.011	.008
	(.005)	(.005)*	(.009)	(.009)
No-Fault Property × trend		-9×10^{-4}		-5×10^{-6}
		(4×10^{-4})*		(.001)
Pseudo R^2	.069	.07	.075	.076

Note. The dependent variable is Divorced by Interview Time. The sample comprises white women married once, for at most 48 months, sampled between1971 and 1990, for whom there was no change in the laws between marriage and the interview. Specifications: (1) No-Fault Ground, No-Fault Property, state dummies; (1′) No-Fault Ground, No-Fault Property, No-Fault Ground × trend, No-Fault Property × trend, state dummies; (2) No-Fault Ground, No-Fault Property, state dummies, state dummies × trend; (2′) No-Fault Ground, No-Fault Property, No-Fault Ground × trend, No-Fault Property × trend, state dummies, state dummies × trend. All specifications control for education, marriage year (dummies), and time elapsed since marriage. For No-Fault Ground, no fault equals one, and fault equals zero; for No Fault Property, no fault equals one, fault equals zero; divorced equals one, and zero otherwise. The coefficient reported is the discrete change dF/dx computed at the means of the data, where F is the cumulative distribution function of $N(0, 1)$. The t-test reported is that of the underlying probit coefficient being zero. See text for details. Number of observations = 42,474. Robust standard errors that allow for arbitrary correlation within a state in any given year are in parentheses.
  * Significant at the 5% confidence level.

in Table 2, column 2, support the idea that for those women who married under a fault regime for property, a change to a no-fault regime was responsible for a significant increase in divorce odds. Another interesting aspect of the result is that the trend aspect is insignificant, despite the expected negative sign (Table 2, column 2′). This suggests that women who married after the first changes of the early 1970s in states that still retained fault did not fully anticipate that, in some cases, changes were about to occur there as well. A comparison of the average effect and of the immediate effect reveals that, not surprisingly, the early changes of the 1970s had a bigger impact.

On the other hand, we see that adding no-fault grounds to the statutes (whether supplementing fault grounds or supplanting them) seems to be irrelevant. This result is robust: specifications with no-fault grounds alone (that is, without the property variable), using the present classification or Friedberg's, would yield the same conclusion. Therefore, we

can see that efforts to change that aspect of divorce alone bears little consequence. An interpretation is that collusion is not possible along the dimension of property: what one spouse gains is lost by the other; on the other hand, holding division of property constant, spouses who have reached an agreement have an interest in bypassing fault, with its higher transaction costs. Fault would then become ineffective, and, therefore, its change to no fault would become irrelevant.

Thus, these results offer an explanation for the sharp increase in divorce rates between the early 1970s and early 1980s. The second and probably most important piece of the puzzle is to determine what the reaction was of those who married after the legal changes.

Analysis of the Impact of the Divorce Laws on Couples Who Married after the Legal Changes

This analysis compares the divorce patterns of women who married before and after the legal changes, but the sample now excludes those women for whom the law changed between marriage and the time of the interview (the surprised). These results can be interpreted as the impact of the legal changes inasmuch as they were anticipated. The methodology is the same as above, except that there is now only one dummy for no-fault grounds and one for no-fault property.

Following the above notations,

$$y_{i,s,t} = 1 \quad \text{if } \alpha_1 x_{\text{grounds},s,t} + \alpha_2 x_{\text{property},s,t} + \alpha_3 T_{\text{grounds},i,s,t} x_{\text{grounds},s,t}$$

$$+ \ \alpha_4 T_{\text{property},i,s,t} x_{\text{property},s,t} + \beta 1_s + \gamma 1_t + l \sum \delta_l x_{l,i} + \varepsilon_{i,s,t} > 0,$$

and $y_{i,s,t} = 0$ otherwise, where $\varepsilon_{i,s,t} \sim N(0, \sigma^2)$; $\forall$ i, s, t, i', s', t', $\text{cov}(\varepsilon_{i,s,t}, \varepsilon_{i',s',t'}) = 0$ and $\text{cov}(\varepsilon_{i,s,t}, \varepsilon_{i',s,t}) \neq 0$.

Note that I present the results for the nonsurprised in a different estimation—after dropping out the surprised group. The reason for this is that some of the divorcees in the surprised group did in fact divorce before the legal change (since we do not know the divorce date); therefore, there is no point in carrying this measurement error in the analysis of the nonsurprised group.

The nonsignificant coefficient on No-Fault Property in Table 3, column 2, supports the selection hypothesis. In other words, an increase in selectivity by potential spouses leading to higher quality matches works as an indirect, offsetting effect of the divorce law changes. To account further for this idea, recall that I have selected respondents who had married in the last 48 months before the interview; the focus is on early

divorces, which result from the worst matches on average. The more ill sorted the spouses are, the earlier they will find out about it and divorce. Therefore, if there is a sorting effect that prevents, to some extent, bad matches from being created in no-fault states, it will most clearly be reflected in early divorces.

The only unexpected result is the significant coefficient on the trend dimension of No-Fault Grounds. This result is not robust to using Friedberg's (1998) classification. One may interpret this trend effect as, for example, spouses making more "cooperative relationship-specific investments" (investments that affect only the noninvesting spouse's marital utility, such as reducing domestic violence) because no-fault grounds encourage such investments (Wickelgren 2005). However, while interesting in theory, this argument is still speculative.

The selectivity effect is likely to take many forms, most of them unobservable to the researcher. In the following section, I seek to detect it from an analysis of marriage patterns.

Analysis of the Incentive Effects of the Law at the Marriage Stage

The impact of no-fault divorce on age at marriage can be shown to be a priori ambiguous. However, from the analysis of the trends in divorce rates across property regimes (Figure 3), one may conjecture that selectivity became higher under no-fault regimes for property. The goal of this section is therefore to find some evidence of a selection effect at the marriage stage.[22]

Interestingly, age at first marriage has dramatically increased in the last 3 decades (Figure 6); therefore, I check here whether divorce laws can account for this phenomenon to some extent in the pooled cross-section sample. Of course, there is much evidence to suggest that age at first marriage for women has risen for a host of reasons. The regressions control for educational attainment (see Goldin 1992), but other causes for delaying marriage include, for example, access to modern contraceptives (Goldin and Katz 2002). Furthermore, changes in the age-sex composition of the population caused by the baby-boom generation may also lead to rising age at marriage for women. Quantifying all the prob-

---

22. A growing number of people in the United States never marry; in other words, marriage is not just delayed. Though first-marriage rates have been plummeting in the United States since the early 1970s (Figures 4 and 5), I could not find a significant impact of either aspect of divorce law changes on first-marriage rates. In a study on how no-fault laws affect marriage strategy, Rasul (2003) finds a very small effect on first-marriage rates and a stronger one for remarriage, on which I concur.
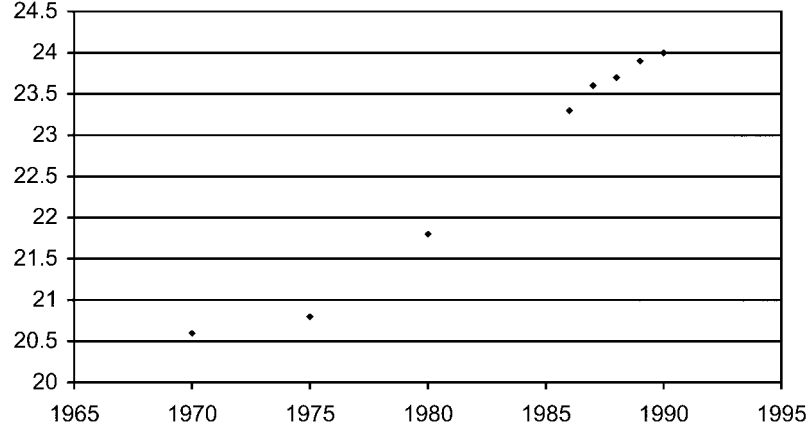
Figure 6. Median age at first marriage (women) (U.S. Census Bureau 1999)

able explanations for the rising age at first marriage is beyond the scope of this paper. Those trends are captured by the year dummies and state-specific trends in Table 4. In any case, they are independent of the adoption and timing of no-fault property regimes over the 1970s and 1980s.

Formally, defining A as being age at first marriage and following the notations defined above (I control only for education x now), I estimate

$$A_{i,s,t} = \alpha_1 x_{\text{ground},s,t} + \alpha_2 x_{\text{ground},s,t} T_{\text{ground},i,s,t}$$

$$+ \alpha_3 x_{\text{property},s,t} + \alpha_4 x_{\text{property},s,t} T_{\text{property},i,s,t}$$

$$+ \beta 1_s + \gamma 1_t + \delta x_i + \varepsilon_{i,s,t},$$

where $\varepsilon_{i,s,t} \sim N(0, \sigma^2)$; $\forall$ i, s, t, i', s', t', $\text{cov}(\varepsilon_{i,s,t}, \varepsilon_{i',s',t'}) = 0$ and $\text{cov}(\varepsilon_{i,s,t}, \varepsilon_{i',s,t}) \neq 0$.

From the empirical results in Table 4, we see that on average over the period, the impact of a no-fault regime for grounds regime is to decrease age at first marriage, although not statistically significantly,[23] while the effect of no fault for property is to significantly delay marriage.

23. This is also a robust result that holds using the law for grounds alone or using Friedberg's (1998) classification.

Table 4. Ordinary Least Squares Estimation (June Current Population Survey Pooled Cross Sections)

	(1)	(1′)	(2)	(2′)
No-Fault Ground	1.41	1.12	−1.39	−1.71
	(1.45)	(1.46)	(1.49)	(1.52)
No-Fault Ground × trend		.25		−.14
		(.16)		(.4)
No-Fault Property	3.79	3.78	4.86	5.16
	(1.44)**	(1.46)**	(2.04)*	(2)**
No-Fault Property × trend		.306		.734
		(.149)*		(.382)+
Pseudo R^2	.165	.165	.166	.167

Note. The dependent variable is Age at First Marriage (in months). The sample comprises white women married for the first time, for at most 48 months, sampled between 1971 and 1990. Specifications: (1) No-Fault Ground, No-Fault Property, state dummies; (1′) No-Fault Ground, No-Fault Property, No-Fault Ground × trend, No-Fault Property × trend, state dummies; (2) No-Fault Ground, No-Fault Property, state dummies, state dummies × trend; (2′) No-Fault Ground, No-Fault Property, No-Fault Ground × trend, No-Fault Property × trend, state dummies, state dummies × trend. All specifications control for education and marriage year (dummies). For No-Fault Ground, no fault equals one, and fault equals zero; for No-Fault Property, no fault equals one, and fault equals one. Number of observations = 44,758. Robust standard errors that allow for arbitrary correlation within a state in any given year are in parentheses.

+ Significant at the 10% confidence level.
* Significant at the 5% confidence level.
** Significant at the 1% confidence level.

Ceteris paribus, a no-fault rule for property should make marriage more risky for women.[24]

In the preferred estimation with state and state × trend controls (Table 4, column 2), the average effect of no fault for property laws is to delay first marriage by approximately 5 months—this result is very robust. The effect appears to be immediate since the trend effect is only weakly significant (Table 4, column 2′).

To summarize, my interpretation is that states provide little insurance against divorce when there is no fault for property: short of a third-party insurer (the state), women insure themselves by being more careful; hence, on average they put in a longer searching time. This follows the

24. For example, women are more at risk of falling below the poverty line if they get divorced, especially if they have children.

lines of Cohen's (1987) arguments.[25]

Finally, it can be verified that once education is controlled for, the impact of age at first marriage on divorce, although statistically significant, is small. Indirectly, this finding suggests that selectivity might operate at levels other than just age at marriage. Obviously, while we can measure searching time, we cannot observe search intensity.[26] As hinted previously, there are certainly more than just selectivity issues present in terms of people's strategic reaction to changes in divorce laws, not only at the marriage stage but also later. This deserves future research.

CONCLUSION

This paper has proposed a theory that intends to account for the patterns of divorce observed in the United States in the last 35 years. To summarize, this theory says that the divorce law changes introduced in the early 1970s affected the odds of divorce for those couples who married before these laws were passed. Such couples were more likely to divorce after a change in law from fault to no-fault divorce, and the key variable seems to be the law governing property division and spousal support. Once the first legal changes passed, many poorly matched couples who married before the changes in the law broke up, thus boosting the rate of divorce. The legal changes that appeared later still had some impact for those who had married under a fault regime. Most important, the effect of no-fault divorce was mitigated by those couples who reduced their probability of divorce through better sorting upon marriage. The main conclusion of the paper is that this better sorting decreased the probability of divorce by about as much as the institution of no-fault divorce increased it.

This selection effect is apparent since under no fault for property laws on average women marry when they are significantly older than are women in fault states. This work thus provides an explanation for the observed apparent convergence in divorce rates between fault and

25. Yet, I found that neither dimension of no-fault divorce significantly reduces first-marriage rates. This is based on a regression of first-marriage rates in states in which both bride and groom were residents of the state in which the marriage was registered, in at least 90 percent of cases, using the *Vital Statistics of the United States* (National Center for Health Statistics 1960–88). This regression is available from the author. Rasul (2003) finds a modest effect for the entire United States.

26. I also regressed race homogeneity and educational homogeneity within couples on the divorce laws, but the results were inconclusive.

no-fault states over the last 20 years. It presents a consistent interpretation for the argument that the effects of unilateral divorce laws on divorce rates died out a decade after their introduction (Wolfers 2003). The results also expand on Weiss and Willis (1997), who found that couples married under unilateral divorce regimes are less likely to divorce than those married under mutual consent regimes, all else equal, despite living in a state with a more liberal regime, which reinforces the theory of selection into marriage.

This theory awaits being connected with other branches of research studying the consequences of divorce laws at other levels. For example, has the nature of marriage contracts changed? Has the welfare of divorced women decreased? Johnson and Skinner (1986), Peters (1986), Parkman (1992, 1998), and Gray (1998) argue, in different ways, that the divorce law changes have affected female labor-force participation rates. A recent study (Stevenson and Wolfers 2005) finds a significant decline in domestic violence and suicide following enactment of unilateral divorce laws, which is again consistent with the general argument presented in this work of better sorting as a consequence of no-fault divorce.[27] More research can be done along the same lines, for example, with the analysis of household specialization and provision of public goods. Another promising line of research is to look for the impact of divorce laws on features that signal assortative matching, such as religious homogamy (see Call and Heaton 1997).[28] Finally, the impact of no-fault divorce on children's outcomes is also under scrutiny (Gruber 2000; Johnson and Mazingo 2000). Murphy (1999) finds evidence that the average difference in child outcomes between children of married and unmarried parents has increased since 1960. This may reflect a compositional effect, that is, that the pool of surviving marriages are better on average. This would also be consistent with better selection into marriage over time, although the study did not specifically relate this pattern to the liberalization of divorce laws.

Finally, from a policy perspective, and without taking a stand on the welfare implications of no-fault divorce, the conclusions of this study show that divorce rates alone do not provide a justification for shifting back to fault-based divorce. In any event, this work predicts that if a

27. An alternative argument might be that no-fault divorce makes it easier for battered women to exit marriage because of lower transaction costs (and less fear of being penalized for leaving the marriage if the domestic violence cannot be proved). I thank an anonymous referee for this insight.

28. I thank an anonymous referee for this suggestion.

no-fault state switched back to a fault regime, the impact on divorce rates would be a spectacular drop in the short run because a strict law governing divorce would be applied to spouses who sorted under a no-fault regime. However, this victory would likely be short-lived.

APPENDIX: INTERPRETATIVE FRAMEWORK

In the following, I explore the implications of nontransferable utility. Although it must be acknowledged that a partly transferable utility might be more suitable, this polar case is useful in analyzing the consequences of adding transaction costs and imperfect foresight to the Coasian benchmark.

Setup

In a stylized way, a single person maximizes the value of marriage net of search costs over match quality. To capture the main idea, I will assume here that each female chooses an effort level first—which I occasionally refer to as selectivity—and that the matching technology is such that she is exogenously paired with, that is, married to, a male who has made the same search effort. Marriage is divided into time periods (1, 2, 3, and so on). For simplicity, suppose that divorcees do not marry singles.

Call q the probability that, at the end of each period of the marriage, nature reveals to a spouse that the match is a bad one, and think of q as a measure of search effort to find someone really compatible (hence, a higher q means lower effort); q indexes this probability under a fault regime. A fault regime is here defined as one in which a spouse cannot unilaterally leave the match, say, because it is too costly.[29] In other words, the single person's problem is interpreted as one in which he or she chooses a probability of being in a good match and then nature draws.

If nature reveals to one spouse that the other spouse is not suitable (with probability q), he or she always wants to divorce. On the other hand, if nature reveals to one spouse that the other spouse is suitable (with probability $1 - q$), he or she always chooses to continue in marriage.

Similarly, let p denote the probability that one spouse is willing to divorce in the no-fault regime after nature reveals the relevant information. In the no-fault case, if nature reveals to one spouse that the other spouse is not suitable (with probability p), he or she always wants to divorce. On the other hand, if nature reveals to one spouse that the other spouse is suitable (with probability $1 - p$), he or she always chooses to continue in marriage.

Nature tells the spouses whether the match is good or not at the same time, independently, so one spouse can be told that the match is good while the other

---

29. Therefore, in this formulation, the different dimensions of the law are lumped together, which is a crude simplification.

is told that the match is bad. Nature discloses whether the match is suitable at each period t of their marriage for all married couples and under both regimes. For simplicity, and without loss of generality, p and q are time invariant.

Recall that willingness to divorce is independent across spouses. Therefore, upon the revelation of match quality by nature in the fault regime, the probability that divorce occurs in each period of the marriage is q^2, since both spouses need to agree for the divorce to be granted. The probability that divorce occurs in the no-fault regime is $1 - (1 - p)^2$ because now only one spouse's decision is sufficient to get a divorce (and no transfers are possible).

These divorce probabilities correspond to the steady state under each regime. Note, however, that matches of the same quality can dissolve at different times: let us now turn to the transition dynamics when there is a regime change.

Transition Dynamics

We will see here why the short-run impact of no-fault laws is unambiguous while only the data can tell whether a no-fault regime increases selectivity or not. Yet even this is not enough to predict the long-run effect of no-fault laws on divorce rates.

After a switch in the law from fault to no fault, the composition of marriages in which the spouses decide to divorce is a mixture of those who married before the switch and of those who married after it. According to the assumptions above, we can state the following:

Proposition 1. For those who married under fault and are not (yet) divorced, a no-fault regime increases the odds of divorce.

Proof. The increase in the odds of divorce is simply $(1 - (1 - q)^2) - q^2 > 0$ for all q in $(0, 1)$.

The intuition is straightforward: no-fault laws allow parties in bad matches to get out of marriages unilaterally.

It should be clear that getting an unambiguous result on the long-term effect of a change in the divorce regime would require strong assumptions. At an intuitive level, one should be able to compare the ex ante value of marriage under each rule. In other words, ideally one should be able to compare the value of remaining married if willing to divorce (when the other party is not) and the value of being divorced against one's will (that is, when one would have preferred to remain married). However, it turns out that a simple accounting exercise can provide useful answers. I now make use of the dynamics of the above framework.

Call the fraction of spouses at time t who decide whether or not to continue in marriage (that is, when nature reveals to each spouse the suitability of the other) and who married after the switch, $a(t)$. It is straightforward to show that

the weight of those who married after the law change is increasing with time, that is,

$$da(t)/dt > 0. \tag{A1}$$

Now the difference in divorce rates at any point in time after a switch from a fault to a no-fault regime is

$$\Delta(t) = [1 - a(t)][1 - (1 - q)^2] + a(t)[1 - (1 - p)^2] - q^2. \tag{A2}$$

This leads to the main proposition:

Proposition 2. Match quality is higher in no-fault states, $p < q$, if and only if the difference in divorce rates across regimes is decreasing over cohorts.

Proof. If the sign of the difference in difference is

$$\text{sign}\{d\Delta/dt\} = \text{sign}\{-[1 - (1 - q)^2] + [1 - (1 - p)^2]\} < 0, \tag{A3}$$

then since $f(x) = 1 - (1 - x)^2$ is increasing on $(0, 1)$, it must be that $p < q$.

Conversely, suppose $p < q$. Then, by the same argument, $\text{sign}\{-[1 - (1 - q)^2] + [1 - (1 - p)^2]\} < 0$. We can further state as an important corollary that the impact of no fault on divorce likelihood for those who married after the law was passed should be smaller than for those who married before its passage.

This simple framework provides a tentative explanation for the increase and then decline in the difference of divorce rates between fault and no-fault states over time and shows why looking at the effect of the law at a fixed point in time (or over a too-short period) would be misleading. Note, however, that this finding does not necessarily imply that divorce rates should equalize, nor does it rule out the possibility that divorce rates eventually will be lower in the no-fault regime, for that matter.

Table A1. Information Used to Construct Legal Dummy Variables

	Enactment of Specific No-Fault Provisions (Including Incompatibility)	No Fault for Property Division and Spousal Support
Alabama	1971	Fault
Alaska	1935	1974
Arizona	1973	1973
Arkansas	No (Ellman and Lohr: 1979)	Fault (Ellman and Lohr: 1979)
California	1970	1970
Colorado	1971	1971
Connecticut	1973	Fault
Delaware	1974 (Friedberg: no)	1974
DC	1977 (Friedberg: no)	Fault
Florida	1971	1986
Georgia	1973	Fault
Hawaii	1972	1960
Idaho	1971	1990
Illinois	1983 (Friedberg: no)	1977
Indiana	1973	1973
Iowa	1970	1972
Kansas	1969	1990
Kentucky	1972	Fault
Louisiana	No	Fault
Maine	1973	1985
Maryland	No	Fault
Massachusetts	1975	Fault
Michigan	1974	Fault
Minnesota	1974	1974
Mississippi	1976 (Friedberg: no)[a]	Fault
Missouri	1973 (Friedberg: no)	Fault
Montana	1975	1975
Nebraska	1972	1972
Nevada	1973 (Friedberg: no)	1973
New Hampshire	1971	Fault
New Jersey	No	1980
New Mexico	1973	1976
New York	No	1980 (Ellman and Lohr: fault)
North Carolina	No[b]	Fault
North Dakota	1971	Fault
Ohio	1974 (Friedberg: no)	Fault
Oklahoma	1953	1975
Oregon	1971	1971
Pennsylvania	1980 (Friedberg: no)[b]	Fault
Rhode Island	1975	Fault
South Carolina	No	Fault
South Dakota	1985	Fault
Tennessee	1977 (Friedberg: no)[a]	Fault
Texas	1969 (Friedberg: 1974)	Fault
Utah	1987 (Friedberg: no)	Fault (Ellman and Lohr: 1987)
Vermont	No	Fault

TABLE A1. *continued*

	Enactment of Specific No-Fault Provisions (Including Incompatibility)	No Fault for Property Division and Spousal Support
Virginia	No	Fault
Washington	1973	1973
West Virginia	1977 (Friedberg: no)	Fault
Wisconsin	1977 (Friedberg: no)[b]	1977
Wyoming	1977	Fault

Sources. Brinig and Buckely (1998); Ellman and Lohr (1998); Sepler (1981); Kay (1987); Freed and Foster (1977, 1979, 1981); Freed and Walker (1990); Friedberg (1998, pp. 612–13, cols. 1 and 4). Also Legal Research on LexisNexis (state codes and case law) and the expert scholarship of an anonymous referee. "Friedberg: no" may mean the state has no-fault grounds but includes or requires separation for a required length of time (compare column 2, pp. 612–13).

[a] Only if divorce is uncontested.

[b] Friedberg (1998) acknowledges ambiguity.

REFERENCES

Allen, Douglas W. 1992. Marriage and Divorce: Comment. *American Economic Review* 82:679–85.

Ashbaugh Vlosky, Denese, and Pamela A. Monroe. 2002. The Effective Dates of No-Fault Divorce Laws in the 50 States. *Family Relations* 51:317–24.

Brinig, Margaret F., and Douglas W. Allen. 2000. These Boots Are Made for Walking: Why Most Divorce Filers Are Women. *American Law and Economics Review* 2:126–69.

Brinig, Margaret F., and Frank H. Buckley. 1998. No-Fault Laws and At-Fault People. *International Review of Law and Economics* 18:325–40.

Call, Vaughn R. A., and Tim B. Heaton. 1997. Religious Influences on Marital Stability. *Journal for the Scientific Study of Religion* 36:382–92.

Cohen, Lloyd. 1987. Marriage, Divorce, and Quasi Rents; or, "I Gave Him the Best Years of My Life." *Journal of Legal Studies* 16:267–303.

Dnes, Antony, and Robert Rowthorn. 2002. *The Law and Economics of Marriage and Divorce*. Cambridge: Cambridge University Press.

Ellman, Ira M. 1996. The Place of Fault in Modern Divorce Law. *Arizona State Law Journal* 28:773–835.

———. 2000. Divorce. Working paper. University of California, Berkeley, School of Law.

Ellman, Ira Mark, and Sharon L. Lohr. 1998. Dissolving the Relationship between Divorce Laws and Divorce Rates. *International Review of Law and Economics* 18:341–59.

Fineman, Martha Albertson. 1983. Implementing Equality: Ideology, Contradiction and Social Change. *Wisconsin Law Review* 4:789–886.

———. 1991. *The Illusion of Equality: The Rhetoric and the Reality of Divorce Reform*. Chicago: University of Chicago Press.

Freed, Doris J., and Henry H. Foster. 1977. Divorce in the Fifty States: An Outline. *Family Law Quarterly* 11:297–313.

———. 1979. Divorce in the Fifty States: An Overview as of 1978. *Family Law Quarterly* 13:105–28.

———. 1981. Divorce in the Fifty States: An Overview. *Family Law Quarterly* 14:229–47.

Freed, Doris J., and Timothy B. Walker. 1990. Family Law in the Fifty States: An Overview. *Family Law Quarterly* 23:495–608.

Friedberg, Leora. 1998. Did Unilateral Divorce Raise Divorce Rates? Evidence from Panel Data. *American Economic Review* 88:608–27.

Gardiner, Karen N., Michael E. Fishman, Plamen Nikonov, Asaph Glosser, and Stephanie Laud. 2002. State Policies to Promote Marriage: Final Report. Unpublished manuscript. Lewin Group, Inc., Falls Church, Va.

Glenn, Norval D. 1991. The Recent Trend in Marital Success in the United States. *Journal of Marriage and the Family* 53:261–70.

Goldin, Claudia. 1992. The Meaning of College in the Lives of American Women: The Past One Hundred Years. Working Paper No. 4099. National Bureau of Economic Research, Cambridge, Mass.

Goldin, Claudia, and Lawrence F. Katz. 2002. The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions. *Journal of Political Economy* 110:730–70.

Gray, Jeffrey S. 1998. Divorce-Law Changes, Household Bargaining, and Married Women's Labor Supply. *American Economic Review* 88:628–42.

Grossbard-Shechtman, Shoshana. 2003. *Marriage and the Economy: Theory and Evidence from Advanced Industrial Societies*. Cambridge: Cambridge University Press.

Grossbard-Shechtman, Shoshana, and Clive W. J. Granger. 1998. Women's Jobs and Marriage, Baby-Boom versus Baby-Bust. *Population* 53:731–52.

Grossbard-Shechtman, Shoshana, and Bertrand Lemennicier. 1999. Marriage Contracts and the Law-and-Economics of Marriage: An Austrian Perspective. *Journal of Socio-economics* 28:665–90.

Gruber, Jonathan. 2000. Is Making Divorce Easier Bad for Children?: The Long Run Implications of Unilateral Divorce. Unpublished manuscript. Massachussetts Institute of Technology, Department of Economics.

Heer, David M., and Amyra Grossbard-Shechtman. 1981. The Impact of the Female Marriage Squeeze and the Contraceptive Revolution on Sex Roles and the Women's Liberation Movement in the United States, 1960–1975. *Journal of Marriage and the Family* 43:49–65.

Jacob, Herbert. 1988. *Silent Revolution: The Transformation of Divorce Law in the United States*. Chicago: University of Chicago Press.

Johnson, John H., and Christopher J. Mazingo. 2000. The Economic Consequences of Unilateral Divorce for Children. Unpublished manuscript. University of Illinois at Urbana-Champaign, Institute of Labor and Industrial Relations.

Johnson, William R., and Jonathan Skinner. 1986. Labor Supply and Marital Separation. *American Economic Review* 76:455–69.

Kay, Herma H. 1987. Equality and Difference: A Perspective on No-Fault Divorce and Its Aftermath. *University of Cincinnati Law Review* 56:1–90.

Lundberg, Shelley, and Robert A. Pollak. 2001. Efficiency in Marriage. Working Paper No. 8642. National Bureau of Economic Research, Cambridge, Mass.

Murphy, Russell D., Jr. 1999. Family Values and the Value of Families: Theory and Evidence of Marriage as an Institution. Unpublished manuscript. Virginia Polytechnic Institute and State University.

———. 2002. A Good Man Is Hard to Find: Marriage as an Institution. *Journal of Economic Behavior and Organization* 47:27–53.

National Center for Health Statistics. 1950–2000. *Vital Statistics of the United States*. Vol. 3, *Marriage and Divorce*. Washington, D.C.: U.S. Government Printing Office.

Parkman, Allen M. 1992. Unilateral Divorce and the Labor-Force Participation Rate of Married Women, Revisited. *American Economic Review* 82:671–78.

———. 1998. Why Are Married Women Working So Hard? *International Review of Law and Economics* 18:41–49.

Peters, H. Elizabeth. 1986. Marriage and Divorce: Informational Constraints and Private Contracting. *American Economic Review* 76:437–54.

———. 1992. Marriage and Divorce: Reply. *American Economic Review* 82:686–93.

Rasul, Imran. 2003. Marriage Markets and Social Change. Unpublished manuscript. University of Chicago, Graduate School of Business.

Sepler, Harvey. 1981. Measuring the Effects of No-Fault Divorce Laws across Fifty States: Quantifying a Zeitgeist. *Family Law Quarterly* 15:65–102.

Stevenson, Betsey, and Justin Wolfers. 2005. Bargaining in the Shadow of the Law: Divorce Laws and Family Distress. Unpublished manuscript. University of Pennsylvania, Wharton School.

U.S. Census Bureau. 1999. *Statistical Abstract of the United States*. Washington, D.C.: U.S. Government Printing Office.

U.S. Department of Labor. Bureau of Labor Statistics. June 1971–98 (not all inclusive). Current Population Survey. Supplement, Fertility and Marriage History. Conducted by the Bureau of the Census for the Bureau of Labor Statistics. Washington, D.C.: Bureau of the Census (producer and distributor) 1971–98; Santa Monica, Cal.: Unicon Research Corporation (production and distributor of CPS utilities), 1999.

Weiss, Yoram, and Robert. J. Willis. 1997. Match Quality, New Information, and Marital Dissolution. *Journal of Labor Economics* 15: S293–S329.

Weitzman, Lenore J. 1985. *The Divorce Revolution: The Unexpected Social and Economic Consequences for Women and Children in America.* New York: Free Press.

Wickelgren, Abraham. 2005. Why Divorce Laws Matter: Incentives for Non-contractible Marital Investments under Unilateral and Consent Divorce. Unpublished manuscript. University of Texas at Austin, Department of Economics.

Wolfers, Justin. 2003. Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results. Working Paper No. 10014. National Bureau of Economic Research, Cambridge, Mass.

Zelder, Martin. 2002. For Better or for Worse? Is Bargaining in Marriage and Divorce Efficient? Pp. 157–70 in *The Law and Economics of Marriage and Divorce,* edited by Anthony Dnes and Robert Rowthorn. Cambridge: Cambridge University Press.