

Did Unilateral Divorce Laws Raise Divorce Rates in Western Europe?

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Abstract

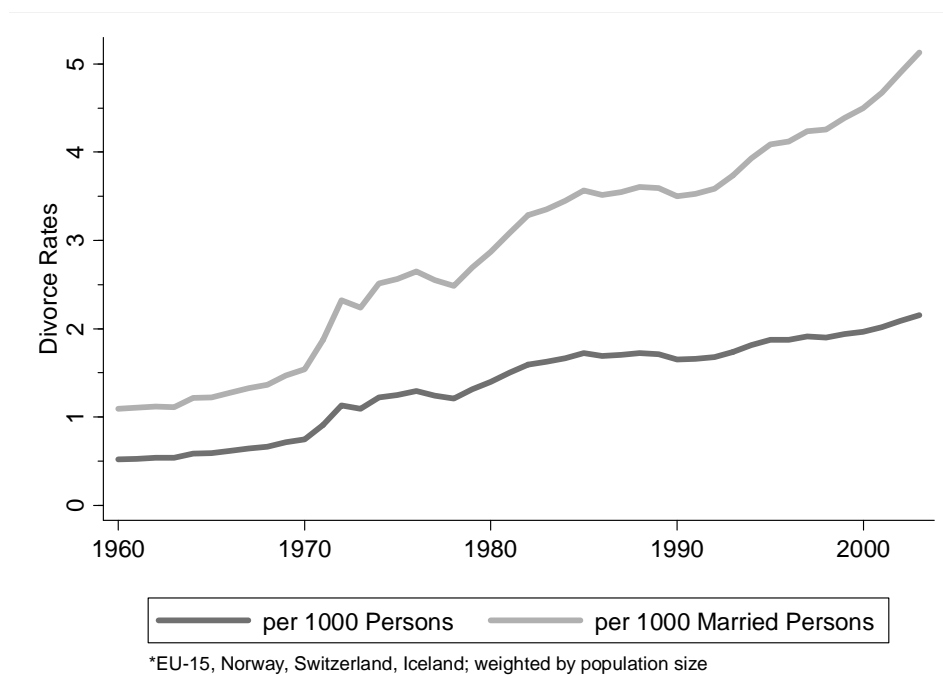
The increase in European divorce rates over the past decades was accompanied by several changes in divorce laws. Yet for European countries, research on the effects of divorce law on the divorce rate is scarce. Most of the existing studies are based on data from North America and provide numerous, but inconsistent, results. We use fixed-effects regression models to examine the impact of the introduction of unilateral divorce on the divorce rate in Western European countries. We find that de facto unilateral divorce practices led to a sustainable increase in the divorce rate, whereas legal rights to unilaterally divorce had no long-run effects.

Since the late 1960s, divorce rates have risen markedly across Europe and the industrialized world. It is not surprising that this rise, which is often interpreted as a serious societal change, has been intensively studied by sociologists, and that several correlates of marital instability could be identified (c.f., Kitson, Barbri, & Roach, 1985; Wagner & Weiss, 2006; White, 1990). The relevance of the topic also arises from possible consequences of the dramatic rise in divorce rates for other domains of human life, for example, changing patterns of fertility, child development, or the gendered division of labor (Furstenberg, 1990; Kitson & Morgan, 1990; Parkman, 1992; Peters, 1986; Seltzer, 1994). All in all, the explanation of rising divorce rates turns out to be a prominent and important issue in sociology and demography, as well as in economics.

The question we address here is whether changes in divorce legislation have had an impact on the development of divorce rates, particularly in Europe. This issue has not

gained much attention in the sociological literature so far, but has been more or less restricted to economic debate. That is somewhat surprising, as it concerns a genuinely sociological subject: showing how a collective phenomenon is socially determined (e.g., Wippler & Lindenberg, 1987).

*Figure 1: The Rise of the European Divorce Rate**



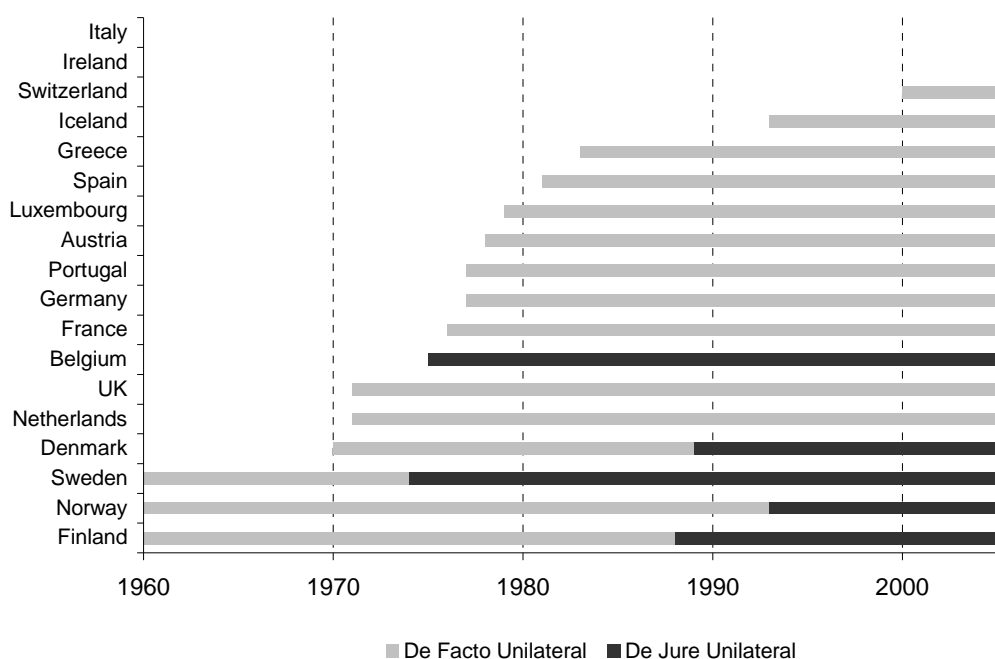
In all Western European countries divorce rates have risen over the last half century (Figure 1). Throughout this article, the term “Western Europe” applies, for matters of brevity, to a set of 18 countries that includes all EU-15 countries, as well as Norway, Iceland, and Switzerland. Family law in all of these countries has, during about the same time period, undergone major changes. How then have changes in divorce law – or institutional transformations –influenced the actual number of divorces?

The first, and maybe major, change was the introduction of divorce as a legal act. This adaptation occurred quite early in most European countries, aside from the laggards Portugal (1977), Spain (1981), and Ireland (1997). Prior to the introduction of divorce as an official opportunity to leave a marriage, the divorce rates in the governmental statistics were zero. A second change in divorce laws concerns the introduction of so-called “no-fault” divorce regimes. In ten out of 18 countries, no-fault divorce was already possible before 1960. All other countries had introduced no-fault grounds by 1997.

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These were sometimes intended as additional to fault grounds, but most of the countries we consider eventually installed them in replacement thereof. Moreover, where fault grounds have been kept, they are decreasingly used and do usually not affect the question of alimony payments (Goode, 1993, p. 32). This article focuses on more recent legal shifts, namely the introduction of unilateral divorce regimes. Unilateral divorce regimes are characterized by the possibility to obtain a divorce without the consent of one's spouse. In unilateral divorce regimes, the mutual consent necessary in countries and during periods with bilateral divorce laws is no longer a requirement. We can further distinguish between *de facto* and *de jure* unilateral divorce regimes. This is important because, in quite a large number of cases, courts had modified the legal practice before parliaments had formally passed explicit unilateral divorce laws. We argue that *de facto* and *de jure* unilateral divorce regimes do not just represent different operationalizations of the same theoretical construct, but that switching from *de facto* to *de jure* unilateral divorce merely reflects a simple reduction in the uncertainty whether a court will accept the unilateral filing or not.

Figure 2: Changes in Divorce Law 1960-2005



As will be shown in the next sections, the shift from mutual consent to unilateral divorce regimes (Figure 2) may affect a couple's bargaining situation when deciding about divorce. We present a simple bargaining model and illustrate how the law changes at

stake may influence the decision to stay married or to file for divorce. The literature review emphasizes the rather mixed empirical evidence gained so far – mainly from North America – and points to different analytic strategies used in the past. In our own analysis we chose a statistical model which is state of the art, further enhanced it, and applied it to the case of Western Europe.

Bargaining over Divorce

Applying bargaining models to pre divorce behavior may seem unusual to some social scientists, but they constitute a special case of what Coleman (1990) proposed as *sociological* explanation: A social phenomenon (the divorce rate) is traced back to individual behavior, which itself is contingent on pre existing macro structures (family law). Though a positive association between less restrictive divorce laws and the divorce rate intuitively seems plausible, theoretical positions on this issue are more complex, particularly when examining the introduction of the possibility to divorce unilaterally as the key feature of divorce law reforms. Applying the Coase theorem to analyzing marital relations, as proposed in the work of Gary Becker (1981), leads to the prediction that there should be no effect on divorce propensities when unilateral divorce laws are introduced. The Coase theorem implies that efficient marriages will not dissolve, irrespective of a given assignment of property rights: Whereas under mutual-consent law, the spouse who unilaterally wants to divorce must bribe the spouse who wants to stay married, in order to convince him or her to agree to the divorce, under unilateral divorce law the spouse who wants to stay in the marriage must bribe the spouse who wants to get out. In either case, divorce occurs only if it is efficient, that is, if there is no way to make both partners better off within the marriage through side payments (Becker, Landes, & Michael, 1977; Landes, 1978).

Why Bargaining Might Fail

The application of the Coase theorem to marital bargaining seems questionable, however, for several reasons. It requires costless bargaining, transferable utility, and well-defined property rights. Plausible reasons for its failure have already been discussed within a rather economic framework: Information asymmetries are in struggle with the notion of renegotiation and utility transferability (Peters, 1986); domestic violence clearly introduces costs of bargaining (Stevenson & Wolfers, 2006);

and the public-good character of marital assets, especially of children, violates the assumption of well-defined property rights (Zelder, 1993; see also Allen, 2002). Apart from that, non-economists have emphasized the processual nature of marital interaction, which eventually leads to divorce. Gottman (1993) for example, identifies a cascading process towards marital dissolution in which a state of serious consideration precedes the actual separation and divorce. This notion is also reflected in the construct of marital instability (or in the propensity to divorce) proposed by Booth and colleagues (1983, 1985). As a lack of opportunities outside the actual marriage may prevent the translating of instability into divorce, it is questionable whether spouses whose interaction style is characterized by “contempt” and “stonewalling” (Gottman, 1993) will start negotiating the arrangements of a common future once such opportunities arise.

Indirect effects

From a theoretical perspective, testing the impact of unilateral divorce law on the divorce rate can thus be seen as a direct test of the applicability of the Coase theorem. But also indirect effects of the divorce law on marital stability have to be taken into account. Unilateral law may lead to an underproduction of marital-specific capital in favor of higher labor market participation rates, particularly among women. That is because in the case of divorce, mutual-consent law enforces compensations for possible foregone investments in the labor market, as the affected spouse might otherwise withhold his or her consent. Under unilateral divorce law, however, wives should be more likely to invest in skills that yield returns outside the marriage, specifically in the labor market. This hypothesis is supported by studies by Peters (1986), and by Parkman (1992), who find similar effects of divorce law changes on female labor force participation in the United States. Kneip and Bauer (2007) also show that divorce law changes in Europe affected female labor force participation and fertility. As both female labor force participation (e.g., South, 2001; Rogers, 2004) and the absence of children (Brüderl & Kalter, 2001; Lillard & Waite, 1993) might promote marital instability, changes in divorce law that affect one or both will also lead to rising divorce rates.

Previous Findings

Previous research has mainly been carried out by economists as a direct test of the Coase Theorem and its applicability to marital bargaining. In the demographic, as well as

in the sociological literature the interest in these effects so far has been rather limited (Glenn, 1997, 1999; Nakonezny, Shull, & Rodgers, 1995). Furthermore, almost all empirical studies are based on datasets from North America. Evidence from European countries, on the other hand, is extremely limited (González & Viitanen, 2006).

Empirical evidence on whether divorce law affects the divorce rate is rather mixed. Peters (1986), using micro data from a special 1979 Current Population Survey that allowed her to analyze information on financial settlement at divorce, found that divorce law has no effect on divorce rates. Due to the advanced statistical modeling and the large dataset exploited, her results were widely accepted, as they confirmed the results of Becker's classical study (1981), which had already pointed out that trends in state-aggregate divorce rates are unaffected by changes in the divorce law. All earlier studies had been based more or less on anecdotal evidence (Gallagher, 1973; Goddard, 1972; Schoen, Greenblatt, & Mielke, 1975), as sufficient data were not available.

Most of the more recent studies, however, come to the opposite conclusion and agree that divorce law changes – the introduction of a no-fault and a unilateral divorce regime – do affect the divorce rate (Friedberg, 1998; see also Allen, 2002 for an overview). Those studies include that by Parkman (1992), who, using the same microdata as Peters, finds a divorce law effect and shows that Peters' results depend on a misclassification of data, namely on an inadequate coding of reform states. Note that in the existing literature, the shift from mutual-consent law to unilateral law is often confused with a shift from fault law to no-fault law, although no-fault divorce may still require mutual consent. This confusion may be grounded in the historical concurrence of the introduction of no-fault laws and mutual consent laws, and may thus lead to a misclassification of cases.

Weiss and Willis (1989) utilize individual-level panel data and reach the same conclusion as Parkman – that the introduction of unilateral divorce has had a positive effect on the divorce rate. Nakonezny, Shull and Rodgers (1995) analyzed U.S. state-level aggregate data, stating that the introduction of no-fault divorce positively affected the number of divorces occurring. This general result is confirmed by Friedberg (1998) using different statistical methods. She responds to concerns about the endogeneity of divorce reforms by controlling for state and year fixed effects, as well as for country-specific time trends. This approach enables her to rule out the possibility that the found

effect is due to higher probabilities of “high divorce risk states” to introduce divorce reforms (i.e., endogenous legislation).

Allen (1991) addresses the problem of endogeneity in a different way. He uses survey data from Canada, where the divorce law was reformed in 1968. All provinces then passed no-fault divorce laws at the same time. As Canada was among the first countries to adopt the new legal concept, the introduction can be seen as exogenous, this early law change being less likely to be a consequence of changing norms. In contrast, countries that adopted the new laws more recently can be seen as having been influenced by a new “divorce culture” that has evolved since the 1960s (Whitehead, 1997).

One recent study by Wolfers (2006) hints at a specific problem that may occur in regression models using flow-based aggregate measures like the divorce rate. Taking Friedberg’s (1998) specification as a starting point, Wolfers additionally models the sustainability of changes from mutual consent law to unilateral divorce law. This statistical approach is more appropriate than the one used by Friedberg a decade earlier, because a simple difference-in-difference approach may confound pre existing trends with the dynamic effects of a policy shock. Wolfers finds no persistent effects. The change in the law increases the divorce rate only immediately after the implementation, but in the long run the rate even falls below the trend. As we consider Wolfers’ model to be state of the art, we adopt his approach, enhance it by further distinguishing between *de facto* and *de jure* divorce law changes, and apply the model to Western European data.

Hypotheses

Based on the theoretical considerations outlined above several hypotheses can be derived and tested. The first, and rather general one, pertains to the total impact of divorce law. Replicating Friedberg (1998), we state that the *introduction of unilateral divorce laws overall increased the divorce rates* (H1). Yet, when difference-in-difference estimation strategies with aggregate measures are used, point estimates of such an overall effect may be misleading if a shift in divorce law has different short-run and long-run effects. Short-run effects might occur due to a pent-up demand for a facilitated divorce or to a preponement of divorces by couples who would probably have divorced anyway. In the long run, the divorce rate could converge to the pre existing trend or reach a new steady state. It is merely the relative level of the new

steady state that is of interest, because it represents the shift in the stock of the population that will ever divorce (Wolfers, 2006). We therefore specify the hypothesis that unilateral divorce affects the divorce rate. In this case we would expect that *the effect of introducing unilateral laws has led to a permanent rise in the European divorce rate* (H2). Furthermore, we regard the introduction of unilateral divorce regimes as a process that comprises a changing legal practice as well as shifts in the family laws themselves. As a unilateral practice is relevant for the choice to unilaterally file for divorce, we hypothesize that it should merely be *de facto changes in the legal courts' practices which have an effect on the divorce rate* (H3). Switching from *de facto* to *de jure* unilateral divorce then simply reflects a reduction in the uncertainty whether a court will accept the unilateral filing. We then expect only minor additional effects of a transition to a *de jure* unilateral regime. A final hypothesis refers to the gradual, rather smooth shift towards unilateral divorce. We take into account the possibility that the courts' decisions to declare a unilateral filing legitimate may be influenced by the divorce regimes in neighbor countries. Therefore, we expect *a positive effect of divorce law changes in adjoining states* (H4).

METHOD

Data

To empirically investigate the hypotheses presented above, we use longitudinal aggregate-level data on 18 Western European countries (EU-15, Norway, Iceland, and Switzerland). We restrict our analysis to these cases as they form a culturally quite homogeneous entity. Unlike the Eastern European countries, where divorce law changes and considerable fluctuations in divorce rates may coincide with the breakdown of the Communist system, the selected countries have not been affected by sudden political revolutions. The data cover a time span of 43 years, ranging from 1960 to 2003. Variables are predominantly derived from Eurostat sources (ec.europa.eu/eurostat/). For France, additional data from Rothenbacher's (2005) data collection were used.

Measures

In the models presented below, we use the following measures as dependent variables: *Crude Divorce Rate* (CDR), calculated on the basis of the number of divorces per 1,000 in the population and, as an alternative dependent variable, the *Divorce Rate per 1,000*

Married Persons. These data are partially missing for both France (1992-2003) and Ireland (1978-1999), for which reason data imputation was applied. The underlying regression model explained about 98% of the variation in the divorce rate.

As key explanatory variables, divorce laws and the legal practice have to be coded as bi- or as unilateral. Developments in family law have recently been studied by the European jurisprudence, mainly because knowledge about foreign legal systems is increasingly important as the process of European unification continues. Even though it is not the key interest of these legal scientists to describe historical developments in divorce law, an extensive expert survey among family law researchers (Boele-Woelki, Braat, & Sumner, 2003) serves well as the basis for our coding. González and Viitanen (2006) have already codified into numerical data the narrative description of the evolution of different legal aspects (Boele-Woelki et al., 2004; Dutoit, Arn, Sfondyla, & Taminelli, 2000; Smith, 2002). We adopted these data, but used material provided by the “Commission on European Family Law” (c.f., <http://www2.law.uu.nl/priv/cefl/>) to countercheck the coding and to add the missing dates of divorce law reforms that took place before 1950. This applies to the introduction of *de facto* unilateral practices in Norway (1909), Sweden (1915), and Finland (1948). Dummy variables indicate whether the divorce law regime is *bilateral divorce* (0) or *unilateral divorce* (1), differentiated into *de jure* and *de facto* divorce regimes: A *de facto* unilateral divorce regime is defined as one in which it is possible to file for divorce without the consent of one’s spouse. It is likely – though not automatic – that the desire for divorce will be granted by judicial verdict if certain requirements are fulfilled. In practice, such requirements entail adherence to a specified period of separation in order for the filing to be successful. In this analysis, regimes are coded as *de facto* unilateral if the period of separation required to get divorced is one year or less.

An example of a *de facto* divorce regime is Germany after 1977. Even though the law itself is not explicitly unilateral, judges *usually* dissolve a marriage in the event of irreconcilable differences, that is, if a couple has been separated for one year. In the time slot taken into account, only five out of 18 countries had adopted a *de jure* unilateral divorce regime: Sweden (1974), Belgium (1975), Finland (1988), Denmark (1989), and Norway (1993). By the year 2003, only Italy and Ireland continued *de facto* to require mutual consent.

Analytical Strategy

We calculate time-series models to analyze the causal relation between divorce regimes and divorce rates. We apply population-weighted fixed-effects regression models, controlling for country-specific time trends. These statistical control variables represent unobserved factors on the country level, as, for example, socioeconomic developments, women's labor force participation, changing norms and religious beliefs, or changing sex ratios (c.f., Trent & South, 1989). The country-specific trends are supposed to rule out the possibility of spurious findings due to a simultaneous variation of variables of time. The statistical model applied can be regarded as a conservative approach, because some variation caused by law changes may even be absorbed by the country-specific trend variables. It exceeds a model that controls only for a rather limited number of observed empirical trends. Moreover, adding such trends as female labor force participation rates, for example, would lead to additional estimates that cannot be meaningfully interpreted. We therefore adopt the conservative research strategy and take as our starting point the simple difference in difference estimators used by Friedberg (1998).

$$\begin{aligned}
 \text{Divorce Rate}_{c,t} &= \beta \text{ unilateral law}_{c,t} & (1) \\
 &+ \sum_c \text{country fixed effects}_c + \sum_t \text{year fixed effects}_t \\
 &+ \sum_c \text{country}_c \cdot \text{time}_t + \sum_c \text{country}_c \cdot \text{time}_t^2 + \sum_c \text{country}_c \cdot \text{time}_t^3 \\
 &+ \varepsilon_{c,t}
 \end{aligned}$$

Note that our model differs from Friedberg's with respect to the specification of country trends. Whereas she added a linear and quadratic term only, we also allow for a cubic trend. How to best model these trends is essentially an empirical matter, as they represent an amalgam of all sorts of processes occurring within the observed time span and with a potential impact on divorce propensities. These processes can be understood as diffusion processes, which usually are not linear (Rogers, 2003). If multiple processes are combined, however, they might possibly take any shape. The cubic specification allows sufficient variability and, as non parametric analyses of the trends have revealed, fits our data best.

Model (1) can be further enhanced to analyze the dynamic effects of divorce law changes in order to separate pre existing trends from the dynamic effects of a policy shock. We

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therefore add a set of dummy variables indicating how many years have passed since the new law was introduced:

$$\begin{aligned}
 \text{Divorce Rate}_{c,t} &= \sum_p \text{dynamic unilateral law effects}_p & (2) \\
 &+ \sum_c \text{country fixed effects}_c + \sum_t \text{year fixed effects}_t \\
 &+ \sum_c \text{country}_c \cdot \text{time}_t + \sum_c \text{country}_c \cdot \text{time}_t^2 + \sum_c \text{country}_c \cdot \text{time}_t^3 \\
 &+ \varepsilon_{c,t}
 \end{aligned}$$

In most European countries, the judiciary adopted a unilateral legal practice prior to the explicit amendment thereof by the legislative power. We take this into account by additionally modeling the dynamic effect of *de facto* divorce law changes: Effects of divorce law changes then split up into effects of explicitly unilateral law and unilateral practice.

$$\begin{aligned}
 \text{Divorce Rate}_{c,t} &= \sum_p \text{dynamic unilateral law effects}_p & (3) \\
 &+ \sum_p \text{dynamic unilateral practice effects}_p \\
 &+ \sum_c \text{country fixed effects}_c + \sum_t \text{year fixed effects}_t \\
 &+ \sum_c \text{country}_c \cdot \text{time}_t + \sum_c \text{country}_c \cdot \text{time}_t^2 + \sum_c \text{country}_c \cdot \text{time}_t^3 \\
 &+ \varepsilon_{c,t}
 \end{aligned}$$

In the final regression model, we control for what might be called the “European divorce climate”. As there usually is a considerable scope for judicial interpretation of any given law, it seems likely that legislative activism in reform countries created pressure for a more liberal divorce practice in other countries (Glenn, 1999; Rodgers, Nakonezny, & Shull, 1999). This should particularly apply to spillover effects between culturally coherent countries like those in Western Europe. Furthermore, the accretive adoption of unilateral divorce laws might reduce the social stigma associated with divorce, even in non reform countries. As suggested by Allen (2002), this process is operationalized as the percentage of countries that have already adopted new divorce laws. As for the law itself, we once again distinguish between the introduction of *de jure* and of *de facto* unilateral divorce regimes. Note that for technical reasons, this model no longer includes year fixed effects:

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$$\begin{aligned}
 \text{Divorce Rate}_{c,t} &= \sum_p \text{dynamic unilateral law effects}_p & (4) \\
 &+ \sum_p \text{dynamic unilateral practice effects}_p \\
 &+ \beta \text{ percentage of adjoining countries with unilateral law}_t \\
 &+ \beta \text{ percentage of adjoining countries with unilateral practice}_t \\
 &+ \sum_c \text{country fixed effects}_c \\
 &+ \sum_c \text{country}_c \cdot \text{time}_t + \sum_c \text{country}_c \cdot \text{time}_t^2 + \sum_c \text{country}_c \cdot \text{time}_t^3 \\
 &+ \varepsilon_{c,t}
 \end{aligned}$$

RESULTS

Discrete Effects

In a first attempt to test *H1*, we applied Friedberg's model to our data. Table 1 shows the results of four different specifications, where the dependent variable is the European *crude divorce rate*, as depicted in Figure 1. The key independent variable is the introduction of *explicitly unilateral divorce law*. The basic specification includes only country and year fixed effects as controls. Using this model, we identified a significant positive effect of a respective shift in divorce law. Yet such an estimation is likely to be biased because of the aforementioned endogeneity problems and because the rise of the divorce rates over time has a unit root ($\chi^2(36)=17.32$, $p>\chi^2=.996$; see Maddala & Wu, 1999). While using first-difference estimation would solve the latter problem, introducing a country-specific linear time trend – which allows unobserved country divorce propensities to trend over time – does the same ($\chi^2(36)=65.03$, $p>\chi^2=.002$). But it also led to a decrease in the effect, which is no longer significant. The third model allows a quadratic trend, as in Friedberg's final specification, which resulted in a rise in the estimated effect that is once again significant. Our final specification including cubic country-specific trends revealed a highly significant effect of .35. This suggests that, according to the presented model, the introduction of unilateral laws raised divorce rates by .35 percentage points on top of a specific trend in the introducing countries. That would correspond to about one quarter of the total rise in the divorce rate between 1960 and 2000. Though the results indicate a positive total effect of *de jure* unilateral divorce laws on the European divorce rate, they also show that point estimates are highly sensitive to the specification of country trends.

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Table 1: Effects of Adopting Unilateral Divorce Laws

	(1) Basic specification	(2) Country trend linear	(3) Country trend quadratic	(4) Country trend cubic
Unilateral	0.166* (0.081)	0.113 (0.106)	0.286*** (0.086)	0.350*** (0.087)
Country effects	F = 107.74***	F = 61.31***	F = 43.28***	F = 41.37***
Year effects	F = 217.80***	F = 228.10***	F = 197.12***	F = 161.40***
Country trend, linear	no	F = 19.83***	F = 95.58***	F = 44.97***
Country trend, quadratic	no	no	F = 101.93***	F = 41.60***
Country trend, cubic	no	no	no	F = 42.59***
adj. R^2	0.889	0.932	0.972	0.975

Note: $N = 792$; estimated using country population weights; standard errors in parentheses

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

Dynamic Effects

Table 2 shows the same four model specifications as in table 1 with one exception: We did not restrict the effect of unilateral divorce to be constant over time. The models report eight estimates for short time periods ranging from immediately to 15 years and longer after the transition. All models showed a positive significant effect immediately after the transition that is quite robust towards the different specifications. The effect was not sustainable, however. It fell below a significant level 3-4 years later and even turned negative, though not significantly, in the model specifications controlling for either a linear or a cubic country-specific time trend. Note that in the dynamic model estimates are less sensitive to the specification of country trends and that the substantial results are essentially the same across the models. Particularly the short-term effects are of comparable size, and all are significant.

Figure 3 gives a visual impression of the underlying process. It shows the time-demeaned divorce rate plotted against a cubic time trend. Subtracting the trend from the rate returns the change in the divorce rate *relative* to the trend. Whereas static models (Table 1) estimate a total effect by comparing the means before and after the transition, dynamic models can decompose this total effect and map the empirical process more closely.

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Table 2: Dynamic Effects of Adopting Unilateral Divorce Laws

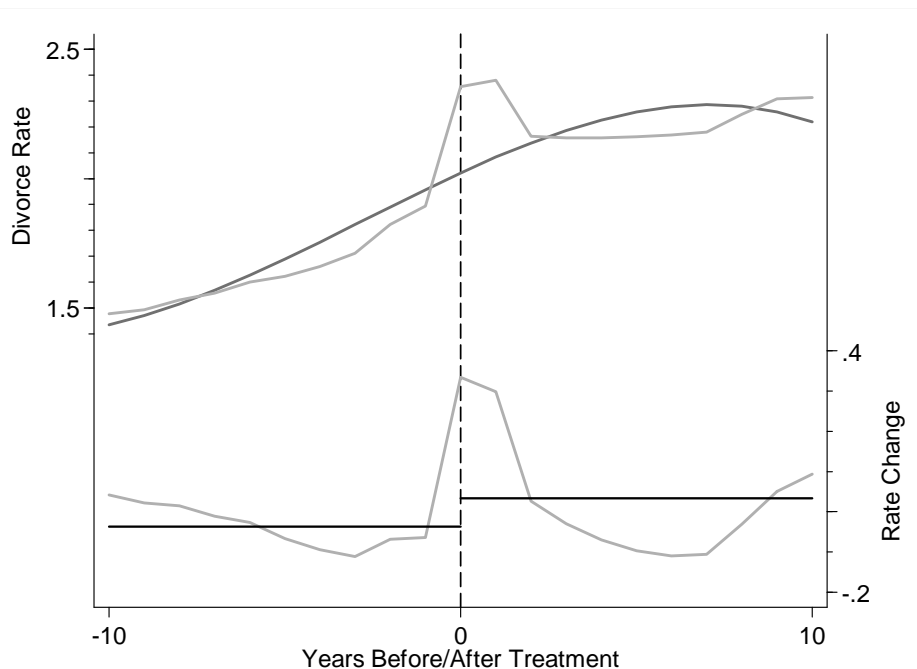
	(1) Basic specification	(2) Country trend linear	(3) Country trend quadratic	(4) Country trend cubic
First 2 years	0.444* (0.182)	0.359* (0.156)	0.430*** (0.108)	0.406*** (0.114)
Years 3-4	0.237 (0.182)	0.133 (0.161)	0.228* (0.115)	0.175 (0.135)
Years 5-6	0.166 (0.181)	0.044 (0.166)	0.165 (0.124)	0.076 (0.164)
Years 7-8	0.026 (0.181)	-0.114 (0.172)	0.036 (0.134)	-0.095 (0.199)
Years 9-10	0.032 (0.180)	-0.126 (0.178)	0.055 (0.145)	-0.123 (0.240)
Years 11-12	0.015 (0.186)	-0.158 (0.190)	0.057 (0.157)	-0.194 (0.283)
Years 13-14	0.007 (0.194)	-0.180 (0.204)	0.072 (0.172)	-0.279 (0.329)
Years 15+	0.231* (0.110)	-0.129 (0.224)	0.130 (0.187)	-0.276 (0.391)
Country effects	F = 107.76***	F = 61.95***	F = 41.00***	F = 42.31***
Year effects	F = 216.21***	F = 229.59***	F = 198.28***	F = 183.33***
Country trend, linear	no	F = 20.51***	F = 85.51***	F = 43.22***
Country trend, quadratic	no	No	F = 92.48***	F = 42.42***
Country trend, cubic	no	No	no	F = 44.08***
adj. R ²	0.889	0.932	0.973	0.975

Note: $N = 792$; estimated using country population weights; standard errors in parentheses

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

Drawing on these results, we had to reject hypothesis 2 with regard to the effect of *explicitly* unilateral divorce law. The effect was not sustainable, and thus seemed to be caused rather by a pent-up demand and by a preponement of divorces. The former is supported by the fall in the divorce rate against the trend before the reform. The latter is supported by the sharp increase in the year of the reform and by the subsequent fall below the trend.

Figure 3: The Dynamic Effect of the Introduction of Unilateral Divorce Law



De Jure versus De Facto Changes

The *de jure* implementation of unilateral divorce, as used in the previous models, is a rather strong criterion by which to consider a regime unilateral in terms of the underlying theoretical model. Moreover, only five countries in our sample ever introduced such laws. We therefore replicated the last model, using a different coding of the explanatory variable, where only a *de facto* possibility to obtain a divorce unilaterally before a court must be given. This operationalization is actually closer to the theoretical construct. Adding such a variable set for *de facto* unilateral law to our last model hardly altered the estimates of the *de jure* effect. Note that they now have to be interpreted as introducing *explicitly* unilateral law on top of a *de facto* possibility.

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Table 3: Dynamic Effects of Changes in European Divorce Jurisdiction

	(1) Crude Divorce Rate	(2) + % Adjoining countries	(3) Divorces per 1,000 married persons	(4) + % Adjoining countries
<u>Explicitly unilateral</u>				
First 2 years	0.386*** (0.103)	0.404*** (0.114)	0.861*** (0.217)	0.906*** (0.241)
Years 3-4	0.102 (0.122)	-0.004 (0.136)	0.214 (0.256)	0.001 (0.285)
Years 5-6	0.017 (0.147)	-0.184 (0.164)	0.019 (0.310)	-0.395 (0.346)
Years 7-8	-0.168 (0.178)	-0.318 (0.200)	-0.391 (0.376)	-0.698 (0.421)
Years 9-10	-0.178 (0.215)	-0.357 (0.241)	-0.419 (0.453)	-0.784 (0.507)
Years 11-12	-0.238 (0.254)	-0.451 (0.284)	-0.556 (0.535)	-0.990 (0.598)
Years 13-14	-0.286 (0.295)	-0.565 (0.330)	-0.700 (0.622)	-1.269 (0.696)
Years 15+	-0.270 (0.350)	-0.648 (0.391)	-0.686 (0.738)	-1.449 (0.824)
<u>De facto unilateral</u>				
First 2 years	0.087* (0.039)	0.120** (0.038)	0.204* (0.081)	0.276*** (0.081)
Years 3-4	0.291*** (0.044)	0.309*** (0.043)	0.635*** (0.093)	0.675*** (0.090)
Years 5-6	0.353*** (0.052)	0.431*** (0.048)	0.775*** (0.110)	0.937*** (0.101)
Years 7-8	0.470*** (0.062)	0.501*** (0.054)	1.030*** (0.130)	1.094*** (0.114)
Years 9-10	0.505*** (0.071)	0.545*** (0.059)	1.117*** (0.149)	1.199*** (0.125)
Years 11-12	0.509*** (0.079)	0.571*** (0.064)	1.139*** (0.167)	1.266*** (0.134)
Years 13-14	0.441*** (0.087)	0.498*** (0.069)	1.009*** (0.183)	1.124*** (0.146)
Years 15+	0.322** (0.101)	0.410*** (0.075)	0.774*** (0.213)	0.945*** (0.159)
% Explicitly unilateral		0.180 (0.186)		0.373 (0.392)
% De facto unilateral		0.550*** (0.093)		1.233*** (0.196)
Country effects	F = 59.80***	F = 124.15***	F = 57.21***	F = 136.86***
Year effects	F = 118.06***	-	F = 128.51***	-
Country trend, linear	F = 59.73***	F = 160.35***	F = 61.69***	F = 187.93***
Country trend, quadratic	F = 62.96***	F = 136.48***	F = 57.02***	F = 173.85***
Country trend, cubic	F = 64.87***	F = 125.74***	F = 52.97***	F = 167.08***
adj. R ²	0.980	0.974	0.982	0.977

Note: N = 792; estimated using country population weights; standard errors in parentheses

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

The coefficients for *de facto* unilateral law revealed a completely different picture than that for the *de jure* effects. They were sustainable over the observed time span. It even took some time for the effect to evolve. The effects remain virtually the same when not controlling for *de jure* unilateral law. Introducing year fixed effects into the model, as in the above specification, would result only in slightly smaller coefficients revealing the same pattern. In contrast to *de jure* unilateral divorce, we can accept H3 when considering *de facto* unilateral divorce regimes. At least two perspectives can be adopted when interpreting this result. The first one is rather methodological and refers to the sensitivity of estimates to the coding of the treatment variable. The second refers to different implications the legal setting has for couples deciding whether or not they want to divorce: Introducing the *possibility* to unilaterally divorce may lead to some inefficient divorces initiated single-handedly by one spouse; additionally introducing a *right* reduces only the expected costs for filing. This will lead to the described retentiveness of couples willing to divorce until the implementation of the new law, as well as to the occurrence of some divorces by couples who have been marginally efficient but who then turn inefficient due to the reduction of exit costs. Though the latter effect should be durable, it is likely to be very small.

Contamination

Model 2 from table 3 takes into account a possible contamination effect, represented by the two variables indicating the percentage of other Western European countries that have introduced either *de facto* or *de jure* unilateral divorce laws, respectively. The results show positive effects for both variables, though the first one is not significant (H4). The share of adjoining countries with unilateral divorce practices thus affected national divorce rates beyond a mediation by national legislation, for example, by inducing a more liberal interpretation of existing divorce laws. Taking this perspective, these variables reflect a gradual shift towards unilateral divorce, which is not captured by the variables indicating actual law changes. If this interpretation was true, one would expect that omitting those variables would lead to biased estimates of *de facto* law changes. As a comparison of the respective models shows, this is actually the case, although the bias is rather small. In fact, the omitted trend is then partly absorbed by the year fixed effects. As a technical consequence, a model including year fixed effects but no

contamination effects yields more conservative estimates of the effects of the introduction of unilateral divorce law.

Changes in the Population at Risk

The quantity and quality of marriage market matches may change in response to an introduction of unilateral divorce, particularly when spouses are unable to bargain efficiently. Easier divorce has two possible effects on marriage rates: It dilutes the value of marriage (and therefore reduces marriage rates) and it reduces the commitment cost of marriage (and therefore raises marriage rates). It is an empirical question which process dominates the other. Results of previous research on this topic are mixed. Rasul (2003) found that the observed decrease in US marriage rates is, to some extent, due to the introduction of unilateral divorce laws. In contrast, Alesina and Giuliano (2007) conclude that unilateral law raised marriage rates. Our data suggest that *de facto* unilateral divorce laws decrease the propensity to marry, whereas an *additional* legal claim increases marriage rates (results are not shown in this article).

This suggests that the crude divorce rate might be an inappropriate measure, and that analysis should focus on divorces with respect to married people only, who constitute the population at risk. As the results in table 3 show, replacing the dependent variable by divorces per 1,000 married persons makes hardly any difference. Note that this variable is scaled differently. The dependent variable has a mean of 2.91, compared to one of 1.36 for the crude divorce rate.

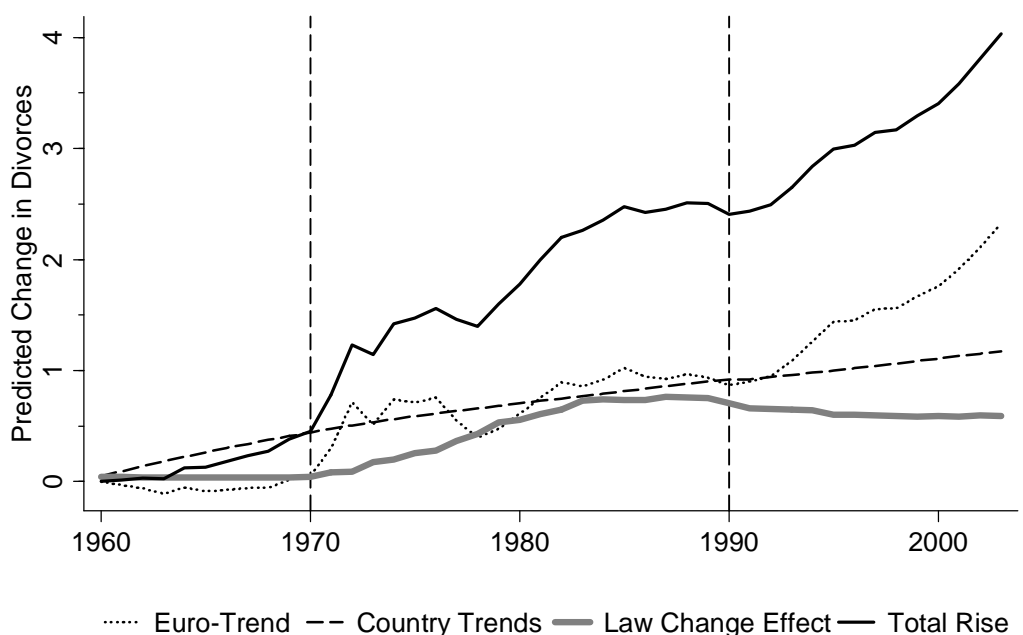
But there may be changes in the match quality of newlyweds that this quantity adjustment does not address. Reduced exit costs might lead to lower-quality matches, which would result in a rising divorce rate. On the other hand, benefits of marriage are reduced under unilateral law, as the spouses know they may be divorced against their will. Hence only couples that profit the most from marriage will actually marry, leading to higher-quality matches and a decline in the divorce rate.

We cannot rule out the possibility that the found long-run effect of introducing *de facto* unilateral law is partly amplified or understated by a changing composition of the population at risk. But this process is itself triggered by the change in divorce laws. Thus, the net effect of unilateral divorce on divorce rates is positive; we just cannot specify to what extent it is mediated by changes in match quality.

An Illustration

Figure 4 illustrates what actually happens to the divorce rate according to our model if the possibility of contamination effects is not considered explicitly (Model 2 from table 3). It shows the predicted rise in divorces between 1960 and 2003 (solid black line) decomposed into changes attributable to the introduction of (*de facto* or *de jure*) unilateral divorce (grey line), a broad trend (dotted line), and country-specific trends (dashed line). According to our model, changes in divorce law are accountable for about one third of the total rise in divorces between 1970 and 1990, when the European divorce rate climbed about two percentage points. But neither the increase before 1970 nor that after 1990 can be explained by unilateral divorce laws: The former seems to be driven by some countries with upward trends in divorce rates, the recent rise appears to be a global phenomenon, at least with respect to Western Europe, as reflected by the steep rise of the dotted line in figure 4.

Figure 4: The Decomposition of the Rising Divorce Rate



If contamination effects are also taken into account (Model 4 from table 3), the total impact of changing divorce practices on divorce rates roughly doubles. Almost 80% of the total rise between 1970 and 1990, and about 40% with respect to the whole period under observation, is due to the introduction of unilateral divorce in Western Europe. But again, the model cannot explain the actual rise before 1970 or that after 1990.

Robustness of Findings

Robustness of estimates is a critical issue, especially in time-series analyses. Results might not only be contingent on the exact operationalization of the treatment variable, but also on the set of countries selected, or on the time span under observation. We therefore ran several models to check the robustness of our general findings. Checks relating to the span of time considered and to the countries selected lead to robust results. Although there was some variation in point estimates, the general pattern remained stable. It might seem natural in the case of the United States to consider one common divorce rate and thus to estimate population-weighted models but this is not evident for Western Europe. One could argue that the units of analysis are actually countries and that population weights are inappropriate. At the worst, our findings might then be driven by a few influential (i.e., populous) countries. But again, results turn out to be robust when non weighted models are calculated. We report population-weighted models, as we attempt to estimate the size of divorce law effects on the European divorce rate.

DISCUSSION

In this paper we examined the impact of changes in Western European divorce laws on divorce rates. Effects of institutional change on a macro phenomenon have ever been a prominent sociological subject. According to Coleman's (1990) explanatory model, institutional change does not affect other macro phenomena directly, but mediated by social action. When institutions change, actors adapt their definition of the situation and are likely to make different choices. In this sense, social action can be traced back to subjective situational definitions. Reformed divorce laws have led to changes in the opportunity structures, as they offer the possibility to divorce without the partner's agreement. An established economic model (Becker, 1981) predicts that the bargaining outcome, to divorce or to stay married, is not substantially different under unilateral divorce law if certain requirements are fulfilled. We conclude that at least some assumptions fail. Therefore, bargaining over divorce is not well captured by classical economic theories.

Western Europe has faced an increase in its divorce rate that can in part be put down to new legal regulations. Our first hypothesis, stating that the *introduction of unilateral divorce laws overall increased the divorce rate*, could not be rejected. But modeling

dynamic effects of our key independent variables, we demonstrated that there is only a slight, short-lived effect of *explicitly unilateral divorce laws*: Even though a simple (static) estimator revealed a positive effect, the more sophisticated dynamic approach offered evidence for a short-run effect characterized by a pent-up demand and by a preponement of divorces. In other words, *de jure* divorce law changes affected the timing of divorces but not the probability that a Western European couple will ever divorce. When having a look at *de facto* unilateral divorce regimes, however, it turned out that these reforms in the legal practice had a sustainable impact. Our results are consistent with current findings for the United States (Wolfers, 2006) when a strong (*de jure*) criterion is applied to identify unilateral law regimes. They draw a different picture for Western Europe when this criterion is loosened to *de facto* unilateral divorce regimes.

These results suggest that the treatment variable used by Friedberg and Wolfers actually captured something comparable to the *de jure* implementation in our models. This would imply the existence of a *de facto* possibility to unilaterally divorce *before* the introduction of the cited law changes. Once the first U.S. states had adopted unilateral law, there was an opportunity to establish a (false) residence in such a state to file for divorce under this state's jurisdiction (Goode, 1993, p. 144). In this perspective, the introduction of unilateral law indeed can be interpreted as a mere switch from *de facto* to *de jure* unilateral divorce for the laggards in the reform process. It made unilateral divorce easier and quicker (as states impose a minimum time of residence to allow filing) resulting in a short-term rise in the divorce rate. In Europe, on the other hand, the responsible jurisdiction cannot be fled: A German couple, for example, may move abroad for a longer period of time but would still have to be divorced on the basis of German family law.

We have also shown that one has to be aware of the importance of a correct model specification. Dynamic effects have to be taken into account in order to disentangle short-run from long-run effects. Apart from that, a different coding of the key explanatory variable can lead to other results, as can be seen in varying effects of *de jure* and of *de facto* unilateral divorce laws. These variables, however, can also be interpreted as substantially different. It can then be argued that a *de facto* unilateral law is the theoretically important issue, as it allows unilateral divorce without side payments to bribe one's partner. Additionally introducing the right to individually divorce only

further reduces the probability that unilateral filing might fail. Taking this perspective would lead us to conclude that the introduction of unilateral divorce had a considerable impact on the divorce rate in Western Europe, but further reducing transaction costs for divorce by increasing the predictability of legal decisions had no long-run effects.

A more technical remark can be added concerning the interpretation of the presented results, especially the low, or even negative, long-run effects of *de jure* unilateral divorce. As the countries that introduced such explicit laws already exhibited high divorce rates at the time of the introduction, regression to the mean may explain why divorce rates rose faster in non-introducing countries, yielding negative coefficients (c.f., Wolfers, 2006).

We have argued that possible spillover effects of an adoption of unilateral divorce law in adjoining countries may be taken into account. In fact we find positive effects of measures for the number of countries that have already switched to unilateral divorce. Since in most European countries the new laws were brought to practice by the courts, our analyses suggest that judges may have been influenced by the appearance of unilateral divorce in other parts of Europe. Insofar as this was really the case, the transition to unilateral divorce might then have occurred rather gradually in most countries.

Another attendant result refers to the effect of country-specific trends. As depicted in the final figure, the common European time trend on the aggregated divorce rate gains importance as European unification proceeds. Whereas the rise in the 1960s was driven by a few countries only, common though also unobserved European trends explain a considerable part of the rising number of divorces in recent times. Shifts to unilateral divorce turn out to be a prominent explanation for a rising divorce rate, foremost between 1970 and 1990.

Our models suggest a sustainable effect for *de facto* law changes. But we have to admit the limitation with respect to long-run effects that go beyond the scope of the data used. Here, a change in the composition of the population at risk (i.e., the married population) might be of importance. The question whether effects of *de facto* law changes will persist in the future remains unsolved for now. We have argued that changes in the composition of the married population will likely affect the number of marital separations. But these compositional effects might be driven by the same underlying factors, that is, by the introduction of unilateral divorce. Future research should therefore take indirect effects

into account: A decline in marital stability might be caused by an underinvestment in marital-specific capital as well as by a reduced match quality. Completing the theoretical argument, both changing investment behavior and match quality might not just be causes for rising divorce rates, but at the same time consequences of a unilateral divorce law. The extent to which unilateral divorce laws affect other household decisions, like fertility and female labor force participation (Kneip & Bauer, 2007), offers a worthwhile opportunity for future research.

We finally conclude that the introduction of unilateral divorce law raised the European divorce rate persistently. This contradicts Becker's influential approach: Some underlying assumptions, information symmetry, costless bargaining, well-defined property rights and utility transferability, do not hold when the economic bargaining model is applied to marital separation. Further, detailed tests of these single assumptions are reasonable, but obviously, only micro data can be supportive in answering this question. Another unresolved issue concerns the share of direct and indirect effects. It is reasonable that within an existing partnership divorce law changes affect not only bargaining over divorce, but bargaining over marriage as well, that is, for example, negotiations over female labor force participation and fertility. The positive effect we found in Western Europe has to be interpreted as an overall effect. It comprises direct and indirect effects, adding up to a persistent rise in divorces.

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