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248-2011

MEA DISCUSSION PAPERS

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Direct and Indirect Effects of Unilateral Divorce Law on Marital Stability*

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Abstract

Previous research on the impact of unilateral divorce law (UDL) on the prevalence of divorce has provided mixed and little cumulative results. Studies based on cross-sectional cross-country survey data have been criticized for not being able to account for unobserved country heterogeneity, whereas studies using country-level panel data fail to account for possible mediating mechanisms at the micro level. We seek to overcome these shortcomings by using event history data from 11 European countries and employing a difference-in-difference approach, thus combining the advantages of both approaches. We find that UDL in total increased the risk of marital breakdown by about 20%. This effect is, however, strongly moderated by selection into marriage, the direct effect being twice as large. Additionally, we find unilateral law effects on female labor force participation and transition to parenthood, although the latter is completely moderated by increased age at marriage. Neither labor force participation nor children have strong mediating effects but marital stability is found to be more sensitive to the legal setting in the presence of children.

* This paper uses data from SHARELIFE release 1, as of November 24th 2010 or SHARE release 2.5.0, as of May 24th 2011. The SHARE data collection has been primarily funded by the European Commission through the 5th framework programme (project QLK6-CT-2001- 00360 in the thematic programme Quality of Life), through the 6th framework programme (projects SHARE-I3, RII-CT- 2006-062193, COMPARE, CIT5-CT-2005-028857, and SHARELIFE, CIT4-CT-2006-028812) and through the 7th framework programme (SHARE-PREP, 211909 and SHARE-LEAP, 227822). Additional funding from the U.S. National Institute on Aging (U01 AG09740-13S2, P01 AG005842, P01 AG08291, P30 AG12815, Y1-AG-4553-01 and OGHA 04-064, IAG BSR06-11, R21 AG025169) as well as from various national sources is gratefully acknowledged (see http://www.share-project.org for a full list of funding institutions).

European countries, like the US and most other developed countries, have faced a persistent increase in divorce rates over the last four decades. The rise in divorce rates was accompanied by several changes in divorce law, particularly the introduction of unilateral divorce law which allows for divorce without the consent of the other spouse. The question whether unilateral divorce law has increased divorce rates is still disputed (see Allen, 2002 for an overview). The empirical evidence so far is based either on cross-sectional survey data or on country-level panel data. While both methods have their advantages, they have also some drawbacks. Whereas studies based on cross-sectional surveys, which exploit differences in divorce law across states or countries are not able to account for country heterogeneity, studies using country-level panel data cannot account for possible mediating mechanisms at the micro level. The aim of our study is to combine the advantages of both approaches by examining the impact of the introduction of unilateral divorce using event history data from 11 European countries. This allows us to account for unobserved time trends affecting all countries in Europe, country heterogeneity, and mechanisms at the micro level. Specifically, this paper addresses the question whether the establishment of unilateral divorce has had an impact on the risk of marital breakdown and whether such an effect, if existing, has been rather direct or mediated by changing patterns in marriage, female labor force participation or fertility.

Divorce laws have undergone several changes in Europe in about the last century. Historically, the first, and maybe most significant change was the introduction of divorce as a legal act, which occurred quite early in most countries. A second change concerns the introduction of "no-fault" grounds for divorce. By the mid of the 20th century those had been adopted by the majority of countries; the remainder followed during the second half of the century. No-fault grounds were sometimes intended as additional to fault grounds, but most

countries eventually installed them in replacement thereof. Moreover, where fault grounds have been kept, they have been decreasingly used and do usually not affect the question of alimony payments (Goode, 1993, p. 32). The third change has been a shift from a divorce legislation that requires mutual consent to one that allows for unilateral divorce. Apart from a few outriders and laggards this change mainly took place in the 70s and early 80s. Although the shift from mutual-consent to unilateral law is often confused with a shift from fault to no-fault law in the literature (perhaps due to the historical concurrence of their introduction in many legislations), they are conceptionally different. Theoretically, it is the shift to unilateral law that is of main interest here, as it affects the assignment of property rights (particularly the right to remarry) – and with this bargaining power – to spouses. Consequently, this specific time-varying characteristic constitutes the key explanatory variable of our analyses.

THEORETICAL BACKGROUND & PREVIOUS RESEARCH

Direct Effects: Bargaining in the shadow of the law

From a theoretical point of view, the question of whether the introduction of unilateral divorce laws (UDL) should affect divorce rates is often seen as concerning the applicability of the Coase Theorem (1960) to analyzing marital relations. If it applies as, for example, proposed in the work of Gary Becker (1981), there should be no (direct) effect. The rationale behind this hypothesis is as follows: If property rights in marriage are well defined, under mutual-consent law the spouse who 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 marriage must bribe the spouse who wants to get out. In either case, divorce only occurs if it is efficient, that is if there is no way to make

both partners better off within marriage through side payments (Becker et al. 1977; Landes, 1978).

The application of the Coase Theorem to marital bargaining has been questioned, though: First, information asymmetries with respect to spouses' divorce opportunities are in struggle with the notion of renegotiation and utility transferability. A possible solution to this, as suggested by Peters (1986), may be a contract that ex ante divides expected marital gains and prohibits ex post bargaining. In contrast to the prediction of Coasian bargaining this would allow for inefficient marriages under mutual consent law and for inefficient divorces under unilateral law. Second, the public-good character of marital assets, especially of children, violates the assumption of well-defined property rights (Zelder, 1993; Allen, 2002). Because of the joint consumption of public goods, gains to marriage cannot be fully transferred from one spouse to the other and some marriages will be dissolved under unilateral law, although not under mutual consent law. Chiappori et al. (2007) make a similar point by noting that divorce changes the way goods are consumed within the household. Formerly public goods such as children may become private goods for one party or the rate of substitution between public and private goods changes for the divorced couple, for instance because the relative value of the public good decreases. The Coase theorem would then only hold under strict quasi-linearity, and in general unilateral divorce laws affect divorce rates, though it is not clear in which direction. Third, although costs of bargaining should be relatively small in marital relations they may be not negligible, especially in the presence of domestic violence (Stevenson & Wolfers, 2006). These transaction costs may then impede an efficient redistribution of assets. Whereas the information asymmetry and transaction costs arguments are rather general objections and there is no argument why these assumptions should be *particularly* violated in marital relationships the opposite is

true for the public goods argument. This leads to the testable hypothesis that the divorce regime should for the most part affect marital stability in the presence of public goods like, for instance, children.

Indirect effects

Identifying the *direct* effect of UDL can thus be seen as a test of the applicability of the Coase Theorem to bargaining over divorce. But also *indirect* effects on marital stability have to be taken into account. Even if the Coase Theorem is applicable to existing marriages with given marital investments one might still find a significant impact of UDL as it could trigger changes in marital specific investment behavior, investments in marketable human capital, or the selection into marriages (with respect to match quality). Unilateral law may lead to an underproduction of marital-specific capital in favor of higher labor market participation rates, particularly among women. This may be due to a lack of compensation at divorce of marriage-specific investments (Peters, 1986) or a lack of compensation of reduced human capital (Parkman, 1992), respectively. Mutual-consent law, in contrast, enforces such compensations as the affected spouse might otherwise withhold his or her consent to divorce. As female labor force participation (e.g., South, 2001; Rogers, 2004) and the absence of children (Brüderl & Kalter, 2001; Lillard & Waite, 1993; Wagner & Weiß, 2006) have been found to be associated with marital instability, changes in divorce law that affect one or both will also affect divorce risks.

Finally, making divorce less restrictive may affect the selection into marriages and thus, in the medium run, divorce rates. Whether allowing unilateral separation leads to an increase in marriage rates or to a reduction is an open question, however. Rasul (2005) argues that gains from marriage are reduced under unilateral divorce regimes, resulting in a decline of

marriage rates. According to his findings, the observed decrease in U.S. marriage rates can to some extent be attributed to the introduction of UDL. If only couples marry that value marriage per se, the match quality within the married population will rise after the introduction of unilateral divorce. Alesina and Giuliano (2007) on the other hand come to conclude that UDL raised marriage rates. Their explanation is that reduced exit costs trigger rash marriages. Hence, unilateral divorce laws could likewise lead to a lower average match quality.

State of research

Previous research on the effect of unilateral law on divorce has lead to somewhat mixed results (e.g. Allen, 2002; Friedberg, 1998; Gallagher, 1973; Goddard, 1972; Gonzáles and Viitanen 2009, Kneip & Bauer 2009; Marvell 1989; Nakonezny et al. 1995, Peters, 1986; 1995; Schoen et al., 1975; Wolfers, 2006; Wright & Stetson, 1978). Moreover, due to the different empirical approaches used, findings are hardly cumulative. One of the first studies using advanced statistical modeling and large datasets was that of Peters (1986). Using US micro data from a special 1979 Current Population Survey, she found no effect of the presence of unilateral law on divorce probabilities. On the other hand, Allen (1992), using the same data as Peters, found an effect. Friedberg (1998) traced this discrepancy back to the fact that the studies differed in the consideration of controls for geographic differences in divorce propensities. She proposed a model exploiting state level panel data to control for state and year fixed effects, as well as for country-specific time trends. Her model thus controlled for various sources of state heterogeneity without being specific about them and revealed an effect of UDL on divorce. Wolfers (2006) introduced an extension to Friedberg's model by accounting for the potential dynamics of a policy shock and found no long-run

effect for the US. However, applying Wolfers' model, Kneip and Bauer (2009) as well as Gonzáles and Viitanen (2009) found a sustainable effect in Europe. These differences in results between Europe and the US could arise because of differences in the selection process into marriage, and hence average match quality, and how this process changed in response to changes in divorce laws. For the US, there is a mixed picture of whether unilateral divorce laws have improved match quality (Alesina & Giuliano 2007, Rasul 2005), and we provide first evidence on the development of match quality in Europe.

METHOD

Data

We use data from the SHARELIFE study which is part of the "Survey of Health, Ageing and Retirement in Europe" (SHARE) conducted in 13 European countries between October 2008 and September 2009. The use of this data has several advantages but also disadvantages. As compared to, for instance, using U.S. federal states a sample of European countries is likely to be less homogeneous. This is a large advantage at the same time. Because migration costs are higher within Europe, there is less concern that households take residence in countries with more liberal divorce laws than in the US. Still, in order to avoid to much cultural heterogeneity in the sample of countries considered we excluded the former communist countries Poland and the Czech Republic from our analyses (c.f. Kneip & Bauer 2009). For the same reason we also omitted persons that had lived in the GDR prior to 1989 from the German sample.

On the other hand, although not primarily designed to study divorce, these data are particularly appropriate for our research question: First, they contain the necessary biographic information on marriages, child births, job episodes, and separations over the life

course. Second, the cross-national design generates a variation in the timing of divorce law changes necessary to identify the effect of UDL. Third, with a target population of 50+, the cohort structure of the sample leads to a favorable distribution of respondents over applicable divorce laws during the course of marriage: We observe a substantial share of marriages that started under mutual consent law and did never experience a shift as long as they lasted (16.62%), marriages that outlasted the shift to a unilateral divorce law (60.27%), and finally marriages that already started under unilateral law (23.11%). The variation in distributions across countries is displayed in table 1.

– Table 1 about here –

Analytic Strategy

Our general analytic strategy comprises a difference-in-difference approach. With respect to marital breakdown, marriage and fertility as dependent variables we apply and estimate Cox duration models of the following form:

$$h_i(t) = h_0(t) \cdot \exp(\alpha \cdot uni + \sum_c \beta_c c + \sum_b \gamma_b b_i + \sum_c \delta_c l + \sum_c \zeta_c l^2 + \sum_k \eta_k x_i + \varepsilon_i),$$

where α gives the effect of the introduction of unilateral divorce (*uni*=1). We control for country fixed-effects (c), cohort fixed-effects (b) as well as linear (l) and quadratic (l^2) country-specific cohort trends. This eliminates unobserved heterogeneity in divorce (marriage, childbearing) propensities over countries and cohorts from the model without being specific about the driving factors. Such may comprise different levels or trends in, e.g., real income, education, religiosity, marriage market opportunities, or any other factors that may affect the outcome of interest. Essentially, the model fits country-specific trends in a transition risk and assesses whether there has been a significant discontinuity in the trend at

the time when unilateral law was installed. With respect to female labor force participation as the dependent variable we run a two-period fixed-effects OLS regression model, taking into account the periods shortly before and after marriage and considering the same controls as in the Cox duration models. Note that without further controls we obtain the total UDL effect. However, we are interested in the direct effect to assess the applicability of the Coase Theorem. To do so, we include possible individual level mediators x_i (age at marriage, labor force status, and presence of children) in the model that might otherwise be absorbed by the unilateral law effect.

Measures

Our main dependent variable is duration of marriage until marital breakdown. We consider separation rather than divorce because the time lag between separation and actual divorce is affected by uncontrolled features of family law appliance, e.g. by the mere duration of proceedings at courts and administration.

The main independent variable, *unilateral law* is a time-varying variable which is 0 during episode time that is prior to the date where de facto unilateral divorce is eventually introduced in a given country, from then on it takes the value 1. We apply the same coding of this variable as used by Kneip and Bauer (2009), focusing on a de facto unilateral divorce regime. Such a regime is defined as one in which it is possible to file for divorce without the consent of one's spouse. Divorce will then not follow automatically but can be expected to be granted by judicial verdict if certain requirements (like a specified period of separation) are fulfilled.[1]

[1] For a detailed discussion on unilateral divorce in Europe also see Boele-Woelki et al. 2003 and Boele-Woelki et al. 2004 as well as the national reports of the European expert group on family law at http://www.ceflonline.net/Field1.html.

Age at first marriage gives the age of the respondent when entering the first marriage (centered at the mean over all countries). It serves as an indicator of match quality because persons that marry at relatively young age would have greater search costs in order to obtain an optimal match and are thus more likely to accept less advantageous marriages. They are less informed about themselves, their mates, and the marriage market, all of which increases the probability of a mismatch (Becker et al. 1977; Oppenheimer, 1988). Empirically, age at marriage has been widely used as indicator for match quality and has been found to be one of the strongest predictors of marital stability (Brüderl & Kalter, 2001; White, 1990).

Employment is a time-varying categorical variable capturing the respondent's employment status as either fulltime employed, part-time employed, or not employed, where the latter serves as reference category. We interpret (female) labor force participation as indicator of an investment in individual bargaining power due to increased outside options. When employment is used as dependent variable it gives the average workload over the three consecutive years before and after marriage, respectively, where fulltime is coded as 1, partime is coded as 0.5, and not employed is coded as 0 in every year. The resulting variable thus ranges from 0 to 1 with a mean of 0.69.

Common child(ren) is a time-varying variable that takes the value 0 before the first common child is born and switches to 1 after the birth of the first common child. We interpret the existence of common children as indicator of marital specific investments.

Family intact when 10 is a dummy variable indicating whether the respondent was living together with both his parents when he was 10 years old (1) or not (0). This variable serves as a control for the stability of the parent's marriage which is known to affect divorce propensities of their offspring (Diekmann & Engelhardt, 1999; Wagner & Weiss 2006).

Female takes the value 1 if the respondent is female and 0 if he is male. This variable serves as a mere control variable which is, for example, necessary to interpret the age of marriage effect, given the average difference in this trait between men and women. However, it can also be interpreted substantially in interaction with labor force participation.

RESULTS

The results section structures as follows: We start with focusing on the model selection with respect to the parametrization of country-specific trends and report the total effect of UDL on the risk of marital breakdown. We would expect this to mirror previous findings based on country level analyses. Next, we strive to identify the *direct* effect of UDL by controlling age at marriage, female labor force participation and the presence of children as indicators for a selection into marriage, investments in marketable capital, and marital specific investment behavior, respectively. For each of these potential mediators we look at how they are influenced by UDL and how the UDL effect on marital stability is affected by their inclusion in the model. We conclude this section with a detailed note on conducted robustness checks.

Model setup & total effects

Table 2 shows the effects of the introduction of unilateral divorce laws on the risk of marital breakdown for different specifications of country specific cohort trends. Specification 1 includes only country and cohort fixed effects but no trend. Models 2 to 4 consecutively introduce linear, quadratic and cubic trends. The UDL effect is strikingly robust with respect to the different model specifications. This demonstrates that our estimates are not driven by the selected trend parametrization. It is about 19% in magnitude and significant on the 10% level when clustered standard errors are used (N=11 countries). For further analyses we use

specification 3 including linear and quadratic trends as it fits the data best. Further including a cubic trend does not significantly improve the model's explanatory power.

– Table 2 about here –

Besides demonstrating the robustness of our model with respect to calibration table 2 yields also a first substantial result. Based on the selected model specification the unilateral law coefficient can be interpreted as the *total effect* on the risk of marital breakdown. Thus, the divorce risk has risen by about 20% due to the introduction of *UDL* when country and cohort differences are accounted for. The above analysis thus mirrors what can be – and has been – done with country level data. It is noteworthy that our estimation is quite consistent with the findings from Kneip and Bauer (2009) who attribute about 20% of the rise in divorce rates between 1960 and 2003 to the introduction of *UDL*. This is the starting point for the following analyses where we try to decompose the found total effect according to possible mediating pathways.

Selection into Marriage

The first intervening mechanism we consider is a change in the selection into marriage. Particularly, we investigate whether the introduction of *UDL* has affected the chance and timing of marriage and whether controlling for age at marriage alters the estimated UDL effect. Table 3 reports effects on the transition to first marriage (panel a) as well as on marital disruption when controlling for age at marriage (panel b). Both models account for country differences and cohort trends as specified above. We look at the transition to first marriage in two ways: first, how the chance of marrying is affected by the introduction of

UDL and second, how the timing of marriage is shifted for those ever marrying. As the Cox model in the first column reveals, the chance of getting married is reduced by about 40 percent under unilateral law. The log-logistic model, on the other hand, shows that those who actually marry do so about 20% later. Thus, for example, if the average age at first marriage had been 25 years before the introduction of *UDL*, five years of the subsequent increase in age at marriage could be attributed to the change in divorce laws.

– Table 3 about here –

Panel b) looks at how age at first marriage may have mediated the unilateral law effect on marital instability. It shows that a higher age at marriage reduces divorce risks by roughly 4% per year delay. More interestingly, the UDL effect rises to about 1.4 and is now significant at the 1%-level. Hence, we can state that changes in age at first marriage *do* mediate the divorce law effect. *UDL* has triggered a postponement of first marriages which we interpret as an indicator for match quality. The resulting gradual quality shift in the sample of married couples compensated part of the effect which divorce law changes would have had if age at first marriage had remained constant. Flexible trends on the country level do not seem to sufficiently account for this process and the actual (direct) impact of unilateral law is strongly underestimated.

Female Labor Force Participation

We have further argued that a switch to unilateral divorce laws might affect investments in marketable human capital vis-à-vis marital specific assets. The next analyses scrutinize on female labor force participation that could possibly mediate the effect of UDL on the risk of

marital disruption. Again, we first have a look at how female labor force participation is affected by unilateral law.

The models shown in panel a) of table 4 compare individuals with same marital status within countries and cohorts, where marriage is either entered under mutual consent or unilateral law. For this, we employed OLS regression pooling the two periods shortly before and after marriage, where each period comprises workload averaged over three consecutive years. The basic model shows indeed that there is a significant effect of UDL on labor supply. As the dependent variable is normalized coefficients can be interpreted in percentages of fulltime work equivalence. Thus, overall labor supply rose by about 9% due to the introduction of *UDL*. However, as revealed by the interaction in column 2, only women are affected by the change in divorce laws. Furthermore, as shown in column 3, changes in women's labor force participation in reaction to unilateral divorce laws do not only occur after marriage (though the new law only affects married women) but mainly already in advance. However, given that labor supply largely depends on educational decisions made prior to marriage this is what one would expect to find. Nevertheless, the typical reduction of female labor supply subsequent to marriage has diminished under unilateral law, albeit only to a relatively small extent.

– Table 4 about here –

The reported effects are also illustrated in Figure 1 where men's and women's average workload shortly before and after marriage is depicted relative to the workload of unmarried men under mutual consent law. Note again that these effects are net of country-specific trends in labor force participation. The analysis replicates three well known findings: the

average workload of men is higher than that of women and men increase their workload after marriage whereas women reduce labor force participation – a pattern reflecting the gendered division of labor. The figure also shows that women strongly increase their workload under unilateral divorce from about 75% to 90% of the reference workload if they were unmarried at the time of introduction and from about 60% to 80% if they were married. In effect, despite the persistence of a gendered division of labor, the estimated labor force participation of married women under unilateral law even exceeds that of unmarried women under mutual consent law.

– Figure 1 about here –

Panel b) of table 4 shows how female labor force participation affects marital stability and to what extent it mediates the unilateral law effect. First, the analysis confirms what is well known from previous research: Fulltime employment of women raises divorce propensities, and so does part-time employment of men. When labor force participation is accounted for, the UDL coefficient slightly reduces to about 1.18. Although the effect is no longer significant, with respect to point estimates the reduction is only of small size. Similarly, when age at marriage is included in the model, there is hardly any difference (as compared to the estimated effect from table 3b). Therefore, the mediating role of increased female labor force participation can be considered rather weak after all. Or, put in another way, not including female labor force participation in the model does hardly alter the estimated effect of unilateral law when country differences and trends are accounted for.

Common children

The next analyses consider common children as a factor that could possibly mediate or even moderate the effect of unilateral divorce on divorce propensities. Panel a) of table 5 shows how the introduction of UDL has affected the transition to parenthood. According to the basic model it reduces the risk of getting a first child by about 20%. The negative UDL effect on fertility disappears, however, when age at marriage is taken into account. A higher age at marriage is not only an indicator of match quality but also reflects the reduced time at risk for begetting a child. The most plausible interpretation of this finding is that unilateral law has reduced the chances of entering parenthood by reducing fertile time in marriage.

With respect to marital stability panel b) of table 5 reveals a pattern quite similar to what we found for the impact of female labor force participation. The presence of children reduces the risk of marital breakdown by some 40%. Including this variable in the model leads to a reduction of the UDL effect to about 1.15, which is no longer significant. When age at first marriage is included, the estimated effect is 1.4 which is essentially not different to the effect when children are not included in the model.

The third column from table 5b) gives the test of the hypothesis that unilateral law affects marital stability particularly in the presence of children – because they constitute a public good. As the interaction effect reveals, we can confirm this hypothesis. As long as no children are present, the UDL effect reduces to about 16% and is no longer significant. This also means that children lose part of their stabilizing effect on marriage when unilateral divorce laws are installed.

– Table 5 about here –

Robustness of Findings

Robustness of findings is always a critical issue, especially when the statistical models applied include control terms for unspecified sources of heterogeneity (Lee & Solon, 2011) or if there is a possible selection bias in the analytic sample. We therefore ran a number of robustness checks to ensure that our findings are not mere coincidence. The first, and maybe most crucial, has already been presented at the beginning of the empirical section and concerns the model calibration with respect to the inclusion of control trends. While the ex ante decision on the highest polynomial degree included was based on baseline model fit, we also replicated the full models with respect to all dependent variables (final column in table 5b) including either linear trends only or up to a cubic trend. All models appear to be very robust. Like in the baseline models the estimated UDL effects hardly differ at all (see figure 2).

– Figure 2 about here –

A second robustness check conducted was the inclusion of leads. If this effected the results this could hint at the law transition being endogenous with respect to the dependent variable ("endogenous legislation") and thus to biased estimates. However, including leads into the model has no significant impact ($\chi^2(5)= 4.77$) and the coefficient for unilateral law remains stable and significant. Taking together these findings we would argue that the results presented above are reasonably robust as regards model calibration.

Another robustness check was necessary due to the way respondents are sampled in SHARELIFE. Respondents consist of a target person aged 50 or older and their current spouses, regardless of age. Thus, our analytic sample contains spouses who may, or may be

not, be in their first marriage. Although these "double counts" do not generate a problem of deflated standard errors when clustered standard errors are used, it may lead to some bias. However, the direction of this bias is not ex ante clear. On one hand, the sample may be biased towards persons with a reduced divorce risk (due to persistent first marriage); on the other hand, it may be biased towards divorced persons (due to higher order marriages). To eliminate double counts we ran again the full model on divorce with a sample of females only. Again, results are only slightly different. The UDL effect rises by 5 percentage point to 1.45, indicating that the overall sample may be slightly biased towards lower risk persons but the resulting bias is rather small.

Given that our analyses are based on retrospective life history information from elderly respondents recall bias is an issue one has to address, although a possible recall bias in the used data was shown to be only modest (Garrouste & Paccagnella 2011). We use an interviewer rating on the perceived reliability of answers to eliminate those respondents that were suggestive of *almost never* understanding questions correctly (N=318). It turned out that recall bias seems to be of no great importance here as all coefficients remain quite stable.

We have also to discuss the calculation method of the reported standard errors. Since the regressor of interest, *unilateral*, is only measured at the country-level there a good a priori reasons to cluster standard errors on the country level. However, as there are only 11 countries in our dataset we may run into problems because of the low number of clusters as the resulting standard errors may be biased downwards. We address this problem by calculating standard errors assuming no clustering with and without heteroskedasticity and compare the standard errors and pick the most conservative standard errors. By doing so, we use a similar rule-of-thumb that Angrist and Pischke (2009) have suggested when

deciding between standard errors under the assumption of homoskedasticity with heteroskedasticity-robust standard errors who find that this approach works well in their simulations. In our application we find that the clustered standard errors are the largest and therefore we stick with those.

We have argued in favor of using separation rather than divorce as outcome variable. It nevertheless seems reasonable to crosscheck our findings by looking at divorce. As in fact unilateral separation was also possible under mutual consent law, we might get downward biased estimates when looking at separation. As can be seen in figure 2, replacing separation by divorce does not alter the previous finding much. The point estimate is even slightly lower when looking at divorce.

A further issue to address is that we used a linear specification of age at marriage to capture match quality although the theoretical link could be u-shaped. This is because individuals (especially women) might settle with poorer matches as they hear their biological clock tick. Actually, Becker et al. (1977) and, more recently, Lehrer (2008) found support for a non-linearity. When including a quadratic term in the model we can replicate that finding. We have stuck with the linear term for matters of simplicity as this issue is not crucial for the argument and, as shown in figure 2, does not affect the unilateral law estimate.

A final aspect concerns the sensitivity of our findings with respect to the countries included in the sample and thus the degree to which of our findings can be generalized. We tested this by case by case omission of countries from the analytic sample. It turns out that the UDL coefficients are fairly stable, ranging from 1.32 to 1.47, and always remain significant (see figure 3). This suggests that, while the overall finding is robust, one has to be cautious in interpreting point estimates and making inferences on Europe as a whole.

– Figure 3 about here –

CONCLUSION

This paper has scrutinized the effects of the presence of unilateral divorce law on the risk of marital breakdown and possible mediating mechanisms. Effects were estimated using an event history approach exploiting between-country variation in the timing of law changes. Accounting for unobserved heterogeneity over countries and cohorts we find a huge direct unilateral law effect, increasing the divorce risk by about 40%. However, this effect is partly offset by an increased match quality of couples sorting into marriage as a response to the new divorce law: The chance of getting married is reduced by about 40% under unilateral law and, respectively, age at marriage increases by 20%. When age at marriage, female labor force participation and the presence of children as possible mediating factors are not controlled, we identify a total UDL effect amounting to a 20% increase in the risk to divorce. These findings turn out to be very robust against various model specifications.

Furthermore, we find that unilateral divorce law stimulated increased female labor force participation. This applies not only to reducing labor supply to a lesser extent subsequent to marriage but rather to increased labor force participation already prior to marriage. Although female labor force participation is affected by the introduction of unilateral divorce law and is also found to increase divorce risks its mediating effect is rather small. With respect to investments in children we find no direct UDL effect and thus no mediation of the effect on marital stability. However, children are found to moderate the unilateral law effect. A possible explanation is that children constitute public goods within marriage which impedes full utility transferability and by this Coasian bargaining.

How do our findings contribute to an assessment of the impact of the transition to unilateral divorce law on spousal welfare and child development? Our results suggest a positive sorting into marriage resulting in higher quality matches. There is also evidence from previous research that the introduction of unilateral law has reduced domestic violence (Stevenson & Wolfers, 2006). On the other hand, we find that couples postpone marriage or do not get married at all. To the extent that marriages provide additional benefits to couples, this results in welfare losses. In addition, we see more divorces both in absolute numbers and relative to the number of married couples (c.f. Kneip & Bauer 2009). If separation costs are higher among married couples, this also leads to welfare losses under the new divorce regime. The net effect is not clear, however, and we are thus unable to draw any conclusion on the effect on the total welfare of the adult population.

With respect to child development, there are also opposed effects. Experiencing divorce has been shown to be unfavorable for children (Amato & Keith, 1991; Amato, 2001). As unilateral law has raised the risk of marital breakdown, particularly in the presence of children, the likelihood of children experiencing parental divorce has increased under the new law. Moreover, match quality has risen in reaction of unilateral divorce law. Thus, children will more often experience the benefits of high quality marriages (including the reduced incidence of domestic violence), but will also more often be affected by divorce in relatively intact marriages. Whereas children's wellbeing might even increase following divorce at levels of high parental conflict, they suffer particularly when marital quality was rather high (Amato et al., 1995). Previous research has also shown that the introduction of unilateral law had adverse long-term effects on children by changing intra-family bargaining in intact marriages (Gruber, 2004; Reinhold et al., 2011). Additionally, children raised out of wedlock have to be taken into account and there is evidence that their share is increasing

while at the same time the stability of their parents' unions is decreasing (Bumpass & Lu 2000; Raley 2001).

Even though unilateral law itself is not at stake in Europe our findings bear some clear policy relevance. Clearly, the introduction of unilateral divorce has had some negative effects that were unintended by policymakers.

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TABLES & FIGURES

Table 1: The distribution of marriages over divorce laws per country

Country	Mutual consent	Switch to unilateral	Unilateral
Austria	10.61	79.84	9.55
Belgium	5.46	76.87	17.67
Denmark	4.62	50.66	44.72
France	7.45	74.60	17.95
Germany	7.60	78.34	14.06
Greece	3.70	89.85	6.45
Italy	100.00	0.00	0.00
Netherlands	2.15	59.73	38.12
Spain	2.19	91.58	6.24
Sweden	0.00	0.00	100.00
Switzerland	26.38	72.99	0.62
Total	16.62	60.27	23.11

Table 2: Different model specifications

	spec. 1	spec. 2	spec. 3	spec. 4
unilateral law	1.182^{+}	1.196^{+}	1.192^{+}	1.195^{+}
	(0.111)	*(0.125)*	*(0.127)*	*(0.126)*
country FE	+	+	+	+
cohort FE	+	+	+	+
linear trend	-	+	+	+
quadratic trend	-	-	+	+
cubic trend	-	-	-	+
Observations	19759	19759	19759	19759
LR χ^2		26.81**	21.95*	9.07

Hazard ratios; Clustered standard errors in parentheses
Likelihood ratio tests compare spec. 2 to spec. 1, spec. 3 to spec. 2, and spec. 4 to spec. 3
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Unilateral law, age at marriage, and marital disruption

	a) Transition into marriage		*b) Marital disruption*
	Cox	Log-logistic	+ age/marriage
unilateral law	0.582***	1.197***	1.391**
	(0.040)	*(0.029)*	*(0.146)*
age at first marriage			0.962***
			(0.007)
person is female	1.672***	0.895***	0.959
	(0.055)	*(0.010)*	*(0.040)*
family intact when 10	0.909***	1.021***	0.710***
	(0.022)	*(0.003)*	*(0.032)*
Observations	19977	17489	19759
Episodes	22465	19977	66378
LL0	-179442.59	6355.21	-26930.16
LL	-178259.36	8899.54	-26126.85

Hazard ratios/Exponentiated coefficients; Clustered standard errors in parentheses
Controlled for country FE, cohort FE, and country specific quadratic trends
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Unilateral law, female labor force participation, and marital disruption

	a) Labor force participation (pooled OLS)			*b) Marital disruption* (Cox-regression)	
	basic model	+ IE	+triple IE	+ LFP	+ age/marr.
unilateral law	0.094***	0.011	0.008	1.178	1.388**
	(0.020)	*(0.028)*	*(0.037)*	*(0.125)*	*(0.143)*
female	-0.354***	-0.393***	-0.254**	0.821	0.694*
	(0.048)	*(0.049)*	*(0.064)*	*(0.137)*	*(0.125)*
after marriage	-0.029	-0.030	0.128***		
	(0.023)	*(0.023)*	*(0.016)*		
uni x female		0.178**	0.161*		
		(0.041)	*(0.057)*		
uni x after marriage			-0.032		
			(0.023)		
female x after marriage			-0.292***		
			(0.048)		
uni x female x after marr.			0.088^{+}		
			(0.048)		
fulltime employed				1.026	0.965
				(0.154)	*(0.147)*
parttime employed				1.334^{+}	1.342^{+}
				(0.225)	*(0.207)*
fulltime x female				1.753**	1.913***
				(0.304)	*(0.343)*
parttime x female				0.819	0.833
				(0.147)	*(0.142)*
age at first marriage					0.959***
					(0.008)
family intact when 10	-0.008	-0.008	-0.008	0.704***	0.718***
	(0.008)	*(0.009)*	*(0.009)*	*(0.033)*	*(0.033)*
intercept	-0.578*	-0.725**	-0.826**		
	(0.238)	*(0.223)*	*(0.234)*		
Observations	39853	39853	39853	19759	19759
Episodes				66378	66378
R^2	0.212	0.219	0.245		
R^2 adjusted	0.210	0.217	0.243		
LL0				-26930.16	-26930.16
LL				-26103.85	-26064.61

Hazard ratios reported in panel b); Clustered standard errors in parentheses
Controlled for country FE, cohort FE, and country specific quadratic trends
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Unilateral law, common children, and marital disruption

	a) Transition to 1st child		*b) Marital disruption*			
	basic model	+ age/marr.	+ children	+ age/marr.	+ IE	final
unilateral law	0.795**	1.110	1.148	1.402**	1.160	1.400**
	(0.064)	*(0.086)*	*(0.122)*	*(0.149)*	*(0.143)*	*(0.146)*
person is female	1.032**	0.925***	1.060	0.947	0.949	0.725+
	(0.012)	*(0.011)*	*(0.045)*	*(0.039)*	*(0.039)*	*(0.130)*
family intact when 10	1.021	1.039	0.707***	0.721***	0.718***	0.727***
	(0.027)	*(0.025)*	*(0.031)*	*(0.032)*	*(0.033)*	*(0.033)*
age at first marriage		0.956***		0.951***	0.952***	0.950***
		(0.005)		*(0.008)*	*(0.008)*	*(0.009)*
child(ren)			0.573***	0.514***	0.421***	0.539***
			(0.047)	*(0.031)*	*(0.032)*	*(0.030)*
uni x child(ren)					1.339***	
					(0.087)	
fulltime						0.990
						(0.150)
parttime						1.380+
						(0.231)
fulltime x female						1.742**
						(0.312)
parttime x female						0.791
						(0.147)
Observations	19860	19860	19759	19759	19759	19759
Episodes	25359	25359	66378	66378	66378	66378
LL0	-159104.45	-159104.45	-26930.16	-26930.16	-26930.16	-26930.16
LL	-158845.92	-158489.89	-26096.36	-26038.05	-26033.03	-25988.30

Hazard ratios; Clustered standard errors in parentheses
Controlled for country FE, cohort FE, and country specific quadratic trends
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

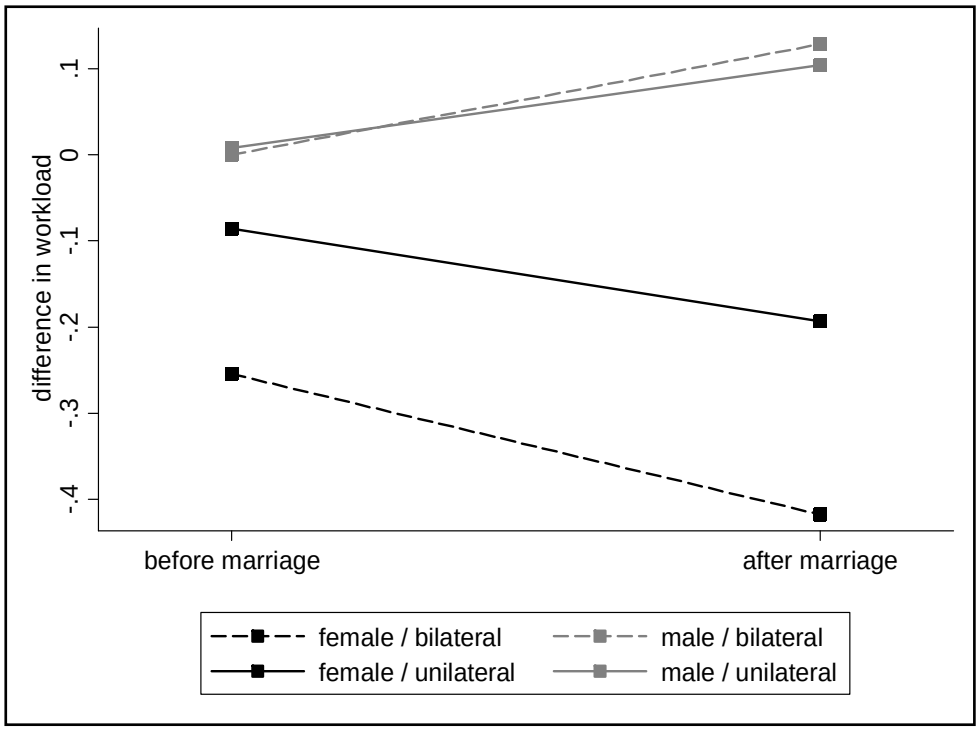

Figure 1: Divorce law and workload by gender and marital status (Predicted from full model in table 4, panel a)

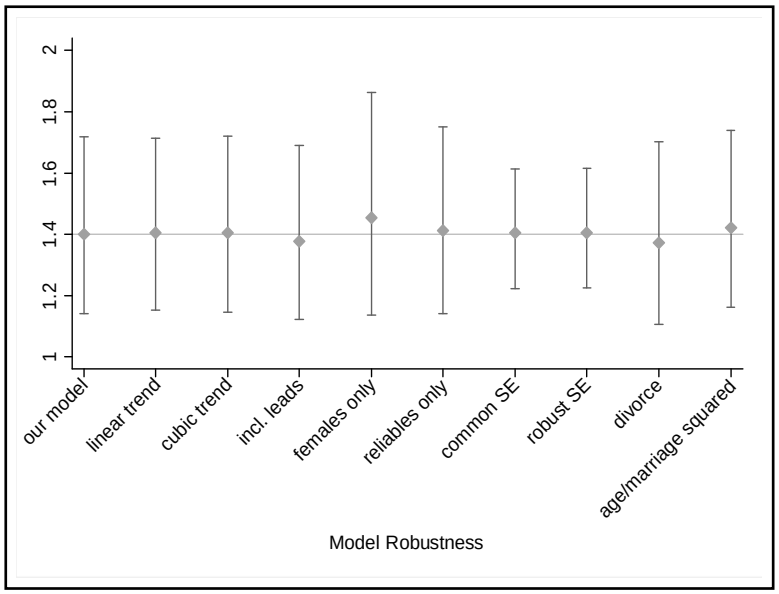

Figure 2: Robustness of Findings

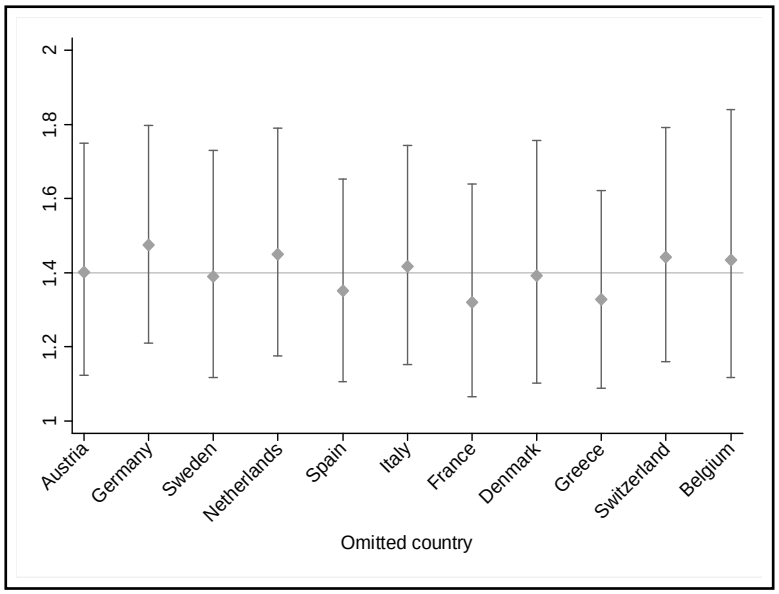

Figure 3: Sensitivity to country selection