The Ant or the Grasshopper? The Long-term Consequences of Unilateral Divorce Laws on Savings of European Households*

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Abstract

Unilateral Divorce Laws (UDLs) shift the ground for divorce from mutual consent to unilateral choice, thereby increasing the risk of separation. Using the staggered introduction of UDLs across European countries, we show that households exposed to UDLs for longer time accumulate more savings. This effect holds for both financial and total wealth and is stronger at higher quantiles of the wealth distribution. Increased exposure to UDLs increases also female labour market participation and financial literacy, contributing to uncover the mechanisms underlying the relationship between divorce risk and savings. Our results are consistent with a precautionary (self-insurance) motive for saving.

Keywords: Divorce, Household savings, Financial literacy.
JEL classification: G11, J12, J22, J32

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1 Introduction

In the second half of the twentieth century, a wave of liberal divorce reforms took place across many developed countries. By allowing each spouse to seek for a divorce without the agreement of the partner, the newly-introduced Unilateral Divorce Laws (UDLs) increase the risk of marital dissolution.[1] A substantial body of research has investigated the short-term effects of the introduction of UDLs on a large array of household outcomes, including marital conflict (Stevenson and Wolfers, 2006, 2007), well-being of children (Gruber, 2004, Reinhold et al., 2013), women's labour supply decisions (Gray, 1998, Stevenson, 2007), and - most notably for economists - household saving behaviour (González and Özcan, 2013, Voena, 2015).

From an economic perspective, there are two competing channels by which an increase in the risk of divorce may affect marriage-specific investments such as assets accumulation. On the one hand, Cubeddu and Rios-Rull (1997) suggest that increases in marital dissolution may encourage households' saving behaviour by a standard precautionary motive: since divorce is costly and households cannot hedge against this negative shock, a greater risk of separation induces married couples to save more. On the other hand, Mazzocco and Yamaguchi (2013) stress that an increase in the probability of divorce may adversely affect saving while married, because asset division laws impose a division of marital properties within the couple, thereby creating incentives for spouses to increase current consumption.

To the best of our knowledge, only a few contributions have made attempts to test which of these channels dominates in practice, and the resulting empirical evidence remains rather inconclusive. For instance, Voena (2015), González and Özcan (2013) and Pericoli and Ventura (2012) provide support for the precautionary saving channel. On the contrary, Stevenson (2007) reports evidence of a decline in the propensity to undertake marriage-specific investments, such as supporting a spouse through school or buying a home. In addition,

[1] Evidence about a positive effect of the introduction of UDLs on the divorce rate is provided by Friedberg (1998) and Wolfers (2006) for the US, and by González and Viitanen (2009) and Kneip and Bauer (2009) for Europe.

little attention has been paid to the longer-run effects of UDLs on the standard of living of couples around retirement. This fact is rather surprising, given the increasing concerns that a large cohort of baby boomers is approaching retirement with little savings and virtually no assets other than their home (Lusardi and Mitchell, 2014). This issue is particularly serious for divorced women, who tend to live longer than men, have less attachment to the labour force, earn less, contribute less to pension plans and are less financially literate (Lusardi and Mitchell, 2007, 2008).

In this paper, we explore the long-term consequences of the UDLs-induced increase in the risk of divorce on the wealth accumulation of European married couples around retirement age. Several papers already provide evidence about the stark increase in the threat of separation induced by the introduction of the UDLs across European countries. For instance, according to the estimates provided by both González and Viitanen (2009) and Kneip et al. (2014) - respectively using using aggregate and individual data - the introduction of UDLs has raised divorce rates across Europe by 20 to 40 percent depending on the chosen specification, a stark increase. To carry out our analysis, we use cross-sectional and life-history data from the Survey of Health, Aging and Retirement in Europe (SHARE). There are several reasons why SHARE is particularly suited for this analysis. First, by collecting harmonized data on the socio-economic status of the European population aged 50+, it allows us to perform a multi-country analysis that exploits the quasi-natural variation in the timing of introduction of UDLs between several European countries. Second, SHARE provides detailed information about household financial and total wealth at the time of the interview and about individual relationship histories throughout the lives of the respondents, which is collected in a comparable way across countries, a crucial point for our study. Third, the survey is rich in information about several other aspects of life, including family background, early life conditions, work histories and financial literacy, thereby allowing us to shed light on several potential mechanisms behind the relationship between divorce risk and savings. In our analysis we focus on couples whose head is aged 50 to 70, who are still in their first

marriage at the time of the SHARE interview and reside in one of the seven European countries that have introduced UDLs in the second half of the 20th century (Austria, Belgium, Denmark, France, Germany, the Netherlands and Spain).

Our research design exploits the staggered timing of introduction of UDLs across these countries to identify the effect of a longer exposure to a UDL regime on households' savings. First, we show that - conditional on year of marriage, on country and cohort dummies and country-specific quadratic cohort trends - exposure to UDLs for a longer time span is not correlated with background characteristics of the individuals in our sample. This evidence suggests that, since still-married couples exposed to UDLs for a different period share similar background characteristics, we can hardly impute the estimated effects to differential selection into marriage vs. divorce due to UDL exposure, that can therefore be taken as exogenous. Hence, we can use the variation between countries in the timing of introduction of UDLs to identify the causal effect of a longer exposure to a UDL regime on households wealth accumulation. Since the distribution of wealth is very skewed, we mostly focus on median instead of mean wealth. However, to understand the effect of UDLs on the whole distribution of savings, we exploit also mean regressions and a set of unconditional quantile regressions (see Firpo et al., 2009).

Our results show that there is a large and significant long-term positive effect of increased separation risk on households' savings, lending strong support to the precautionary channel for saving proposed, for instance, by Cubeddu and Rios-Rull (1997). An additional year of exposure to UDLs increases median household net financial wealth by €1,493.32, which corresponds to an increase of approximately 6%. The uncovered pattern is particularly pronounced among more affluent households. We estimate that the effect of UDL exposure is close to zero at the 10th percentile of the wealth distribution, while it approaches €6,142.68 per year of exposure at the 90th percentile. Reassuringly, we find a comparable set of results also for total wealth, i.e. the sum of net financial and real wealth - although less precisely estimated.

In the second part of our analysis, we exploit the breadth of our survey data to gain a better understanding of the channels linking increased separation risk to higher savings. In particular, we show that longer exposure to UDLs leads to higher female labour market participation over the life course and to higher levels of financial literacy - especially for females. Both findings are again consistent with the precautionary saving explanation, in which spouses self-insure against the risk of negative shocks associated with divorce.

Our contribution to the literature is therefore threefold. First, we focus on the long-term effects of increased divorce risk on financial and total wealth. Second, by exploiting data and quasi-experimental variation for several countries we are able to provide causal estimates that are valid for several countries, increasing external validity. Third, the richness of our data allows us to dig deeper into the mechanisms underlying the relationship between the risk of divorce and household savings, considering in particular women's labour force participation and financial literacy. Overall, our results provide strong support for the precautionary motive for saving.

The remainder of this paper is organized as follows. Section 2 describes the data and provides background information on UDLs reforms in Europe. Section 3 discusses the identification strategy and empirical model. The main results of the paper are reported in Section 4, which also includes a set of robustness checks, while we discuss some potential mechanisms underlying the relationship between the risk of divorce and household savings in Section 5. Conclusions follow thereafter.

2 Data and institutional context

We draw our data from the second and third waves of the Survey of Health, Aging and Retirement in Europe, SHARE, respectively carried out between 2006-07 and 2009-10. The SHARE data have a number of unique features that make it particularly attractive for our analysis.

First, by gathering harmonized current and retrospective information on representative samples of the population aged 50+ of several European countries, SHARE allows us to conduct a cross-country study without worrying about data comparability. In this paper, we present evidence for 7 European countries - Austria, Belgium, Denmark, France, Germany, the Netherlands and Spain - where UDLs have been introduced during the second half of the 20th century. We obtain information on the timing of introduction of unilateral divorce laws from other recent studies exploiting these regime changes, including González and Viitanen (2009) and Kneip et al. (2014). Table 1 reports the year of the introduction of *de-facto* UDLs - that range from 1970 in Denmark to 1981 in Spain - and the number of couples married before and after the change in divorce laws across countries in our final sample (more details on sample selection follow in the next paragraph). In addition to these seven countries, three other countries - Switzerland, Sweden and Italy - have also introduced UDLs by 2010. However, we are forced to exclude them because the switch to unilateral divorce occurred either too late (Italy and Switzerland) or too early (Sweden) to obtain information on couples that married both before and after the introduction of UDLs.[2] We instead drop Greece because of unreliable data on economic variables and sample selection issues due to the use of the telephone directory as the sampling frame in that country (Mazzonna and Peracchi, 2016).

[Table 1 about here]

Second, the third wave of the survey (SHARELIFE) collects retrospective information on many dimensions of the life histories of respondents, including relationship histories. This information is crucial for our study, as it permits us to focus on couples who are still in their first marriage at the time of the survey interview, thereby mitigating the risk of selecting individuals who have been married more than once, for whom it would be hard to understand the connection between divorce risk and wealth accumulation.[3] Availability of retrospective

[2] In a robustness test we show that our results still hold when we include these countries.

[3] This sampling strategy is also used by González and Özcan (2013) and Voena (2015).

life history data on migrations allows us also to drop from our sample 88 individuals who have married in a different country with respect to the one where they live at the time of the interview, which could be endogenous to changes in divorce laws between countries.[4] The third wave of SHARE also allows us to collect information on the childhood conditions experienced by respondents, which we summarize with two indicators. As a proxy of parental investment in skill formation early in life, we follow Brunello et al. (2016) and construct a dummy for whether the respondent had more than 10 books in the place where he/she was living at age 10 (i.e. more than a shelf of books, excluding magazines, newspapers or school books), year and country of birth and residence, educational levels (primary, secondary or post-secondary qualifications). As a proxy for family wealth, we construct a dummy for whether the number of rooms in the house where the respondent was living at age 10 was as least as high as the number of persons living in the household, which indicates good housing conditions.[5]

Third, the second wave collects detailed information on household finances. One financial respondent per household is in fact asked to answer several questions on household income and wealth. From this information, we compute household net financial wealth, which consists of gross financial assets (bank accounts, government and corporate bonds, stocks, mutual funds, individual retirement accounts, contractual savings for housing and the face value of life insurance policies) minus financial liabilities. We also compute household net total wealth, which is defined as the sum of net financial and real wealth, where the latter is the sum of the value of the primary residence net of the mortgage, the value of other real estate, owned share of own business and owned cars. We consider these two variables as our main outcomes.[6] All the wealth components are transformed using PPP

[4] As noted by González and Viitanen (2009), concerns regarding divorce-driven migration are much more relevant in the US than in Europe.

[5] We show in Table A.1 in the Appendix that our treatment is uncorrelated to a larger set of pre-marital conditions, bringing even stronger support to the exogeneity assumption. Our results are wholly robust to the introduction of these additional controls.

[6] Whenever information about a components of wealth is missing, we rely on the imputed amounts reconstructed by the SHARE team. Imputations have been carried out using state-of-the-art multivariate fully conditional specification methods, as detailed by De Luca et al. (2015)

exchange rates and CPI measures into 2006 German Euro so that the values are comparable across countries and over time. PPP-adjusted exchange rates and CPI measures are taken from the OECD and national sources.

Fourth, the second wave of the survey also asks each respondent a set of four questions aimed at measuring their ability to perform basic operations on numbers.[7] On the basis of the number of correct answers to the four questions, Dewey and Prince (2005) construct a numeracy indicator that ranges from one to five. The median value in our sample is 3, the sample average is 3.57, and the standard deviation is 1.1. Less than 25% of the sample achieves the maximum score of 5. As shown by Christelis et al. (2010), numeracy is a relevant component of financial literacy, and is strongly predictive of portfolio choices of individuals. Therefore, in our analysis we consider a dummy variable for having a numeracy score higher than the median as an indicator of high financial literacy.[8] From the second wave of SHARE we also gain information on gender, year and country of birth and residence, educational levels (primary, secondary or post-secondary qualifications) and on the number of children. Lastly, the SHARE data also contain individual information on working history, which we use to construct indicators for whether individuals in our sample have ever worked in their lives. The variables on financial literacy and labour market participation allow us to shed some light on the potential mechanism underlying the relationship between the increase in divorce risk and financial wealth later in life.

For our analysis we select couples still in their first marriage and whose head (i.e. the financial respondent) is between 50 and 70 years old at the time of the SHARE wave 2

[7] In detail, the following four questions are asked to SHARE respondents. "*1. If the chance of getting a disease is 10 percent, how many people out of one thousand would be expected to get the disease?*"; "*2. In a sale, a shop is selling all items at half price. Before the sale a sofa costs €300. How much will it cost in the sale?*"; "*3. A second-hand car dealer is selling a car for €6000. This is two-thirds of what it costs new. How much did the car cost new?*"; "*4. Let us say you have €2000 in a saving account. The account earns 10 percent interest each year. How much would you have in the account at the end 2 years?*".

[8] Unlike Christelis et al. (2010), in generating the numeracy score we treat the few "Don't Know"s and "Refusal"s that are present in the data as wrong answers instead of dropping or imputing numeracy for individuals who use these answer modes. We thank Rob Alessie for suggesting us this solution.

interview.[9] The reason for choosing this age interval is to obtain a sample of couples who are around retirement and are not too old to be strongly affected by survival bias.[10]

We carry out the analysis on savings at the household level, and the analysis on the mechanisms, i.e. numeracy and labour force participation, on individual level data. This also allows us to shed light on gender differences in the effects of UDLs on these mediators. After dropping observations with missing data on the variables involved in the analysis, our final sample contains 2,690 couples for the household-level analysis on savings and 4,540 individuals for the individual-level analysis on financial literacy and labour market participation.[11]

Table 2 reports descriptive statistics for the main variables of interest used in the analysis. It consists of two panels, Panel A for the sample at the household level and Panel B for the corresponding sample at the level of individuals.[12] Average financial assets and total assets are respectively equal to €65,510 and €344,105, while the median values of these variables are respectively equal to €24,545 and €259,610, confirming the skewness of these distributions. On average, couples have been married for close to 36 years, have been exposed to UDLs for 30 years and have 2.3 children. Individuals are 60 years old on average at the time of the interview, only about 5% have never worked, approximately 25% have a college degree, and close to 35% have at most a high school diploma. About 60% report that they had more than 10 books in the place where they were living at age 10 and 35% had at least

[9]SHARE interviews partners of individuals in the sample irrespective of their age, and we also do not select couples on the basis of the age of the partner.

[10]This age band has been considered in several other studies that focus on retirement, including Mazzonna and Peracchi (2016). In a sensitivity analysis, we show that our main results also hold when we consider couples aged 50-75 or 50-80 at the time of the interview.

[11]This is not equal to twice the size of the households sample because we drop from the individual-level analysis households for which we do not have information on the outcome variable for at least one partner.

[12]On the one hand, in the household-level sample, individual variables like age and education refer to the household head (i.e. the financial respondent). On the other hand, in the individual-level sample we have dropped members of couples for whom information on the dependent variables is missing for at least one member of the couple.

one room per person in the place where they were living at age 10.

[Table 2 about here]

3 Empirical methodology

To examine how an increase in the risk of divorce generated by the introduction of UDLs affects the savings of married couples, we estimate the following linear regression model:

$$Y_{ijk} = \alpha + \beta UDL_{ijk} + \gamma YoM_{ijk} + \delta X_{ijk} + \mu_k + \eta_j + \lambda_j^1 k + \lambda_j^2 k^2 + \varepsilon_i \tag{1}$$

where the index ijk denotes couple i residing in country j and whose head is born in year k. The outcome variable Y_{ijk} represents financial (or total) assets of couple i. As described in the previous section, assets are measured in levels to include household with debt (negative assets). Our variable of interest is UDL_{ijk} and it is defined as the number of years the couple was exposed to UDLs. Thus, it is a semi-continuous variable that measures the number of years of marriage for couple i since the introduction of UDLs in country j. The rationale for using this variable is that a couple may have married before the introduction of UDLs but may have spent most of their marriage during the period in which the unilateral regime was in place. Our model controls for year of marriage (YoM_{ijk}) and birth cohort fixed effects (μ_k) to control for possible trends in wealth accumulation. We also include a full set of country fixed-effects (η_j) as well as a set of quadratic country-specific cohort trends ($\lambda_j^1 k + \lambda_j^2 k^2$). The former control for unobservable, time-invariant differences across countries that may influence the accumulation of households' financial asset, the latter for unobserved, cross-country differences in financial assets accumulation in different years. Additionally, we also include a set of individual pre-marital covariates that may affect financial assets and correlate with UDL exposure, included in vector X_{ijk} and described in the previous section. Finally, ε_{ij} represents a disturbance term. Throughout the analysis, we cluster standard

errors by country and year of marriage, the level of variation of exposure to UDLs. We estimate model (1) via OLS when focusing on mean effects, and via Recentered Influence Function (RIF) unconditional quantile regressions (see Firpo et al., 2009) when estimating treatment effects on the median or other quantiles of the wealth distribution.

Identification of the coefficient β as the causal effect of one additional year of exposure to UDLs is granted by the quasi-natural experiment provided by the staggered timing of introduction of UDLs across countries. Our identifying assumption is therefore that conditional on year of marriage, country and cohort dummies and country-specific cohort trends, the variation in the years of exposure to UDLs is as good as randomly assigned. For this assumption to hold in our case we need several conditions to be jointly verified. First, these laws ought to provide a source of variation in divorce risk that is unlikely to be related to pre-determined observable or unobservable characteristics of couples that may explain their saving behaviour around retirement. Second, since we focus on the subsample of couples that are still in their first marriage at the time of the SHARE interview, there needs to be non-differential selection into marriage vs. divorce with respect to UDL exposure. Third, while the country and time variation of unilateral divorce laws offer an appealing identification strategy for the estimation of the effect of divorce laws on wealth accumulation later in life, couples can adjust their year of marriage in response to expected changes in unilateral divorce laws. As a result, the anticipation of the introduction of UDLs by spouses would violate the identifying assumptions described above, thereby producing biased estimates. Additionally, we also need that there is no other country-specific unobserved shock that affects saving behaviour and whose timing is correlated with UDL introduction.

While this latter assumption is not testable, we will show that our results still hold when we drop couples married in a 1-year interval around UDL introduction, when (potentially endogenous) sorting into marriage before/after UDL introduction could have taken place. In addition, we provide supportive evidence about the joint validity of the first two assumptions by showing a set of balancing tests, aimed at verifying that - in our final sample - there is

no correlation between our treatment and a set of pre-determined observable characteristics of couples. Table 3 shows the results of "reverse regressions" of each of the covariates included in vector X_{ijk} on our treatment, year of marriage, country and cohort dummies and country-specific quadratic cohort trends.[13] We estimate zero effects of UDL exposure on pre-determined covariates, supporting the credibility of our identifying assumption. Consistently with this evidence, we will also show that all our estimates are stable whether we include or exclude vector X_{ijk} from our models, suggesting that, at least as far as observables and unobservables correlated with variables in X_{ijk} are concerned, bias due to omitted variables is not a relevant concern in our case. To bring further evidence in favour of this assumption, in Table A.1 in the Appendix we repeat the "reverse regression" balancing tests considering a broader set of pre-marital covariates related to family background (see for instance Gould et al. (2011)) and that may correlate with wealth accumulation. Again, we detect no significant relationship between pre-determined observables and exposure to UDLs.

[Table 3 about here]

4 Main results

Table 4 reports estimates of the long-term effects of an increase in the risk of divorce following the introduction of UDLs on the median (columns 1 and 2) and the mean (columns 3 and 4) of financial assets of married couples. Models in columns 1 and 3 control for year of marriage, cohort and country fixed-effects, as well as quadratic country-specific cohort trends, while models in columns 2 and 4 also control for pre-marital covariates in vector X_{ijk}.

Results in columns 1 and 2 suggest that an additional year of exposure to UDLs leads to an increase in median financial wealth of €1,856,65 to €1,493.32, depending on the specification, which respectively correspond to an increase of approximately 7.5% to 6% relative to median

[13] As suggested by Pischke and Schwandt (2015), this test is less subject to concerns of attenuation bias than a "balancing" regression of the treatment on covariates if the latter may be subject to measurement error.

financial wealth. Mean effects reported in columns 3 and 4 portray a similar picture: we find that an additional year of exposure to UDLs increases household savings by €3,308.57 to €2,580.59, depending on the chosen specification. These effects respectively correspond to approximately 5% to 4% of mean financial wealth.

[Table 4 about here]

To verify the robustness of our main results, we perform a battery of robustness tests, reported in Table 5. First, in Panel A we show that our main results still hold when we control for number of children, which is a potentially endogenous variable but may be strongly related with the need for saving. Second, in Panel B we show that our results are qualitatively unchanged when we include in our sample also Italy, Sweden and Switzerland, where UDLs have been introduced either too early (Sweden) or too late (Italy and Switzerland) to observe couples married both before and after UDL introduction. Third, for our identification strategy to hold there needs to be no differential sorting into marriage before or after UDL introduction on the basis of saving propensity. To overcome this selection problem, in Panel C we show that our results are unchanged when we estimate equation (1) excluding from the sample spouses who were married in close vicinity of the divorce laws (i.e., one year before the change in the laws). Finally, to validate our findings on a wider age range, in Panel D and Panel E we respectively include also households whose head is aged 71 to 75 and 71 to 80, and show that results still hold. An additional concern regards the sensitivity of our findings with respect to the countries included in the sample and to whether these are driven by a specific country. To dispel this concern, in Table A.2 in the Appendix we report the estimated effects on median and mean wealth when we drop one country at the time from our sample.[14] Results suggest that the estimated coefficients on the exposure to UDLs remain fairly stable, ranging from €925.21 to €2,130.66 for the median and from €2,248.14

[14]We focus on the specification including all controls, but results are equivalent in the restricted one that excludes the observables in vector X_{ijk}.

to €3,162.75 for the mean.

[Table 5 about here]

To investigate heterogeneous effects across the distribution of household savings, we report in Table 5 the estimates of unconditional quantile treatment effects obtained via RIF regressions (Firpo et al., 2009). Again, the model in column 1 controls for year of marriage, cohort and country fixed-effects, as well as linear and quadratic country-specific cohort trends, while the model in columns 2 also controls for pre-marital covariates in vector X_{ijk}. Estimation outcomes indicate that the effects of the increased risk of divorce following the introduction of UDLs are larger at higher quantiles of the distribution. For instance, considering results in column 2, the effect of exposure goes up from €607.15 to €4,257 as we move from the 25th to the 75th percentile, and the effects are even more diverse when we consider the 10th and 90th percentile. In other words, the long-term effects of the increased risk of divorce following the introduction of UDLs is larger for richer households.

[Table 6 about here]

To understand whether our results hold also when we consider a different definition of savings, that include also real wealth, we conduct a parallel analysis using total wealth, that is, the sum of real and financial wealth. Results for median and mean total savings are reported in Table 7, and show a pattern that is analogous to the one reported for financial wealth, whereby longer exposure to UDLs increases savings. The estimated effects are smaller in relative terms with respect to those considering financial wealth only, as they range around 2-2.5% of the median or mean total wealth in the sample. As reported in Table A.3 in the Appendix, even in this case we estimate larger effects at the top of the wealth distribution.

[Table 7 about here]

All in all, our results are consistent with the findings of Voena (2015) and support the precautionary saving motive, in which spouses self-insure against the risk of negative shocks associated with a divorce by increasing savings.

5 Potential mechanisms

What could be the mechanisms underlying our results? In what follows we show that individuals in a married couple, and in particular women, respond to the increased risk of divorce by improving their financial literacy and increasing their labour market participation. We estimate the same specification as in model (1), with the main difference that the unit of observation i is now an individual in her first marriage (instead of a couple). We use two different outcome variables: (a) an indicator variable taking value one if the person has high numeracy, i.e. a total score above the median value of 3 out of 5 points; and (b) an indicator variable taking value 1 if the person has ever been employed during his career and 0 otherwise. We will carry out the analysis both in the full sample and splitting by gender, to highlight potential gender differences in the reaction to increased divorce risk. Therefore, to maintain a balanced composition of the samples by gender we consider in this analysis only 2,270 of 2,690 couples in our initial sample for whom we observe the outcome variables for both members.

We report the results of this analysis in Table 8. Columns 1 to 3 reports the OLS estimates of the effect of exposure to UDLs on financial literacy as measured by our numeracy indicator: we find that the coefficient on exposure to UDLs is statistically significant at the 10 percent level for the pooled sample (see column 1) and for women (see column 2), but not for men (column 3). An additional year of exposure to UDLs implies an increase in the probability of having high numeracy by 0.6 percentage points in the pooled sample and of 0.9 percentage points for women. We believe that increased financial literacy (proxied by higher levels of numeracy) may be one channel through which the introduction of UDLs have

affected household savings. In fact, this would be consistent with a large literature arguing that numeracy is a good proxy of financial literacy, and that people with higher financial literacy save more and make more sophisticated investment choices (Christelis et al., 2010, Lusardi and Mitchell, 2014).

[Table 8 about here]

Women born between 1920 and 1956 have lower labour market participation than men: while 98% of men in our data report that they have ever worked, this is the case for 91% of women. Hence, a channel by which UDLs may have increase lifetime savings is by increasing women labour force participation - and therefore earnings. Results for labour supply throughout the life course are reported in Columns 4 to 6 of Table 7, and show that an additional year of exposure to UDLs increases the probability that someone has ever worked by 0.7 percentage point. However, the second column shows that the results are driven by women. In fact, in the sample of men, the estimated coefficient on the exposure to UDLs is not statistically significant and very close to zero.

Overall, these results lend support to the precautionary motive for saving, suggesting that - especially fo women - the additional threat of divorce leads to adopt self-insuring behaviours, such as increased investments in financial literacy and labour force participation.

6 Conclusion

In this paper we use European data on married couples around retirement age to analyse the long-term impact of an increase in the risk of divorce on household wealth accumulation. Our empirical strategy exploits the variation in the timing of introduction of Unilateral Divorce Laws (UDLs) across European countries as an exogenous shock to the risk of marital dissolution. Our results show that households tend to save more as a result of the increased risk of divorce following the introduction of UDLs. The effects are particularly pronounced

at higher quantiles of the financial wealth distribution, i.e. among more affluent households. An additional year of exposure to UDLs increases median household savings by €1,493.32, which corresponds to approximately 6% relative to the median. To uncover the mechanisms underlying the relationship between the risk of divorce and household savings, we show that married individuals, and women in particular, respond to an increase in the risk of divorce by improving their financial literacy and increasing their labour market participation. Overall, our findings lend support to the precautionary motive for saving, in which spouses self-insure against the risk of a negative shock associated with divorce.

Figures and Tables

Table 1: Unilateral Divorce Laws (UDLs) by Country

Country	(1) Year of UDL introduction	(2) No UDL at first marriage	(3) UDL at first marriage	(4) Total
Austria	1978	150	16	166
Belgium	1975	416	92	508
Denmark	1970	160	209	369
France	1976	314	73	387
Germany (FRG)	1977	323	48	371
Netherlands	1971	255	174	429
Spain	1981	431	29	460
Total		2,049	641	2,690

Notes: Column 1 shows the year when de facto unilateral, no-fault divorce was first allowed (González and Viitanen, 2009, Kneip and Bauer, 2009). All the samples contain households aged 50-70 who are in their first marriage at the time of the interview and for whom information on all observables is not missing.

Table 2: Descriptive Statistics

Variable	Mean	Std. Dev.
Panel A: Sample of Households. Observations: 2,690		
Household net financial wealth (€)	65,510	123,226
Household net total wealth (€)	344,105	378,634
Exposure to UDLs	29.752	4.716
Age (Financial Respondent)	59.805	5.818
Female (Financial Respondent)	0.454	0.498
Year of marriage	1970.9	7.546
Marriage duration	35.857	7.535
High school diploma (Financial Respondent)	0.343	0.475
College degree (Financial Respondent)	0.281	0.450
Several books at age 10 (Financial Respondent)	0.623	0.485
Good housing conditions at age 10 (Financial Respondent)	0.355	0.479
Number of children	2.273	1.166
Panel B: Sample of Individuals. Observations: 4,540		
Numeracy score	3.570	1.112
High numeracy score	0.555	0.497
Has ever worked	0.948	0.223
Exposure to UDLs	29.818	4.647
Age	59.832	6.359
Year of marriage	1970.7	7.519
Marriage duration	36.007	7.505
High school diploma	0.344	0.475
College degree	0.259	0.439
Several books at age 10	0.610	0.475
Good housing conditions at age 10	0.352	0.478
Number of children	2.196	0.986

Notes: Both samples consider households (Panel A) and individuals (Panel B) aged 50-70 who are still in their first marriage at the time of the SHARE interview and for whom information on all variables is not missing. We drop from the individual-level analysis couples for whom missing values in the dependent variables are present for at least one member of the couple. "Several books at home at age 10" is a dummy for having 10 or more books at home at age 10. "Good housing conditons at age 10" is a dummy for having at least one room per person in the accomodation where living at age 10. "High numeracy score" is a dummy for numeracy score above the median (3 out of 5).

Table 3: Balancing Evidence

	(1) High school diploma	(2) College degree	(3) Several books at age 10	(4) Good housing conditions at age 10
Exposure to UDLs	0.00	0.01***	0.00	0.00
	(0.146)	(0.00)	(0.00)	(0.00)
Observations	2,690	2,690	2,690	2,690
Covariates	No	No	No	No

Notes: The table reports balancing tests derived by reverse regressions of the pre-determinded covariates listed in the heading of each column on exposure to UDLs. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Household-level sample. Standard errors clustered by country and cohort are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of UDLs on Financial Wealth. Median and Mean Regressions

	Median		Mean	
Dep. Var.: Financial Wealth (€)	(1)	(2)	(3)	(4)
Exposure to UDLs	1,856.65***	1,493.32***	3,308.57***	2,580.59**
	(594.18)	(572.60)	(1,095.46)	(1,023.67)
Observations	2,690	2,690	2,690	2,690
Covariates	No	Yes	No	Yes
Median dep. var.	24,545	24,545	-	-
Mean dep. var.	-	-	65,510	65,510

Notes: The table reports the effects of exposure to UDLs on mean and median financial wealth. Mean effects estimated via OLS regressions, median effects via Recentered Influence Function (RIF) unconditional quantile regressions. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Covariates included in Columns (2) and (4) are dummies for having a high school diploma, a college degree, several books at age 10, good housing conditions at age 10. Household-level sample. Standard errors clustered by country and cohort are reported in parentheses. For RIF regressions, clustered standard errors are computed via bootstrap (400 replications). *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects of UDLs on Financial Wealth. Median and Mean Regressions. Robustness tests.

	Median		Mean	
Dep. Var.: Financial Wealth (€)	(1)	(2)	(3)	(4)
Panel A. Including number of children among the control. Obs.: 2,690.				
Exposure to UDLs	-	1,362.90**	-	2,321.14**
	-	(567.53)	-	(1,031.28)
Panel B. Including IT, SE and CH. Obs.: 3,783.				
Exposure to UDLs	1,120.26***	860.27**	2,479.47***	1,983.41**
	(423.40)	(408.47)	(875.11)	(837.25)
Panel C. Excluding coulpes married +1/-1 years around UDL introduction. Obs.: 2,343.				
Exposure to UDLs	1,728.13***	1,337.46**	3,200.02***	2,469.06**
	(622.19)	(606.47)	(1,034.40)	(996.62)
Panel D. Age range: 50-75. Obs.: 3,098.				
Exposure to UDLs	1,767.93***	1,505.07***	3,105.69***	2,533.31***
	(532.50)	(519.93)	(991.45)	(942.96)
Panel E. Age range: 50-80. Obs.: 3,357.				
Exposure to UDLs	1,278.41**	1,049.90**	2,729.63***	2,224.87**
	(496.46)	(482.33)	(902.80)	(870.08)
Covariates	No	Yes	No	Yes

Notes: The table reports the effects of exposure to UDLs on mean and median financial wealth. Mean effects estimated via OLS regressions, median effects via Recentered Influence Function (RIF) unconditional quantile regressions. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Covariates included in Columns (2) and (4) are dummies for having a high school diploma, a college degree, several books at age 10, good housing conditions at age 10. Household-level sample. Standard errors clustered by country and cohort are reported in parentheses. For RIF regressions, clustered standard errors are computed via bootstrap (400 replications). *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Unconditional Quantile Treatment Effects of UDLs on Financial Wealth.

Dep. Var.: Financial Wealth (€)	(1)	(2)
Quantile 10		
Exposure to UDLs	-77.35	-134.01
	(223.62)	(223.93)
Quantile 25		
Exposure to UDLs	740.78**	607.15**
	(304.87)	(294.55)
Quantile 50		
Exposure to UDLs	1,856.65***	1,493.32***
	(594.18)	(572.60)
Quantile 75		
Exposure to UDLs	5,486.23***	4,527.68***
	(1,648.73)	(1,593.95)
Quantile 90		
Exposure to UDLs	7,725.09**	6,142.68**
	(3,227.18)	(3,094.25)
Observations	2,690	2,690
Covariates	No	Yes

Notes: The table reports the unconditional quantile treatment effects of exposure to UDLs on financial wealth. Unconditional quantile treatment effects are estimated via Recentered Influence Function (RIF) regressions. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Covariates included in Column (2) are dummies for having a high school diploma, a college degree, several books at age 10, good housing conditions at age 10. Household-level sample. Bootstrap (400 replications) standard errors clustered by country and cohort are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effects of UDLs on Total Wealth. Median and Mean Regressions

	Median		Mean	
Dep. Var.: Total Wealth (€)	(1)	(2)	(3)	(4)
Exposure to UDLs	6,986.49***	4,988.19**	7,541.69**	5,350.26
	(2,499.68)	(2,412.86)	(3,368.18)	(3,275.26)
Observations	2,690	2,690	2,690	2,690
Covariates	No	Yes	No	Yes
Median dep. var.	259,610	259,610	-	-
Mean dep. var.	-	-	344,105	344,105

Notes: The table reports the effects of exposure to UDLs on mean and median total wealth (the sum of real and financial wealth). Mean effects estimated via OLS regressions, median effects via Recentered Influence Function (RIF) unconditional quantile regressions. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Covariates included in Columns (2) and (4) are dummies for having a high school diploma, a college degree, several books at age 10, good housing conditions at age 10. Household-level sample. Standard errors clustered by country and cohort are reported in parentheses. For RIF regressions, clustered standard errors are computed via bootstrap (400 replications). *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Mechanisms: Effects of UDLs on Numeracy and Labour Force Participation. Full sample and split by gender.

	High Numeracy			Ever Done Paid Work		
	Full sample	Females	Males	Full sample	Females	Males
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to UDLs	0.006*	0.009*	0.006	0.007***	0.009***	0.002
	(0.003)	(0.005)	(0.004)	(0.002)	(0.003)	(0.002)
Observations	4,540	2,270	2,270	4,540	2,270	2,270
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.55	0.62	0.48	0.94	0.98	0.91

Notes: The table reports the effects of exposure to UDLs on numeracy and labour force participation. Numeracy is a dummy for having a numeracy score above the median (High Numeracy). Labour force participation is a dummy for having ever dome paid work (Ever done paid work). Mean effects estimated via OLS regressions. All models control for year of marriage, country and cohort fixed effects, country-specific quadratic cohort trends, dummies for having a high school diploma, a college degree, several books at age 10, good housing conditions at age 10. Individual-level sample. We drop from the individual-level analysis couples for whom missing values in the dependent variables are present for at least one member of the couple. Standard errors clustered by country and cohort are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Table A.1: Additional Balancing evidence

	(1) Parents had Mental Health Issues	(2) Parents had Drinking Issues	(3) Parents Smoked	(4) Missed School for 1+ Months in Childhood	(5) Parents had Middle-Class Occupation	(6) Parents had Professional Occupation	(7) Did Not Live with Mother at Age 10	(8) Poor Home Sanitation at Age 10
Exposure to UDLs	0.00	-0.00*	0.01	-0.00	0.01**	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	2,689	2,689	2,689	2,688	2,609	2,609	2,662	2,688
Covariates	No	No	No	No	No	No	No	No

Notes: The table reports balancing tests derived by reverse regressions of the pre-determinded covariates listed in the heading of each column on exposure to UDLs. Parental occupation refers to the one of the main breadwinner in the parental household. Poor home sanitation refers to an accommodation without running water or an inside toilet. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Household-level sample. The number of observations varies by column to account for missing values in the covariates. Standard errors clustered by country and cohort are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Effects of UDLs on Financial Wealth. Median and Mean Regressions. Excluding one country at a time

	(1) No AT	(2) No DE	(3) No NL	(4) No ES	(5) No FR	(6) No DK	(7) No BE
Median Effects							
Exposure to UDLs	1,429.83**	1,797.48***	1,618.88***	2,130.66***	1,505.07**	981.42*	925.21*
	(592.84)	(562.48)	(556.54)	(726.33)	(627.79)	(594.29)	(525.41)
Mean Effects							
Exposure to UDLs	2,409.79**	2,566.01**	3,162.75***	2,677.82***	2,507.92**	2,248.14*	2,468.76**
	(1,058.87)	(1,161.26)	(1,108.31)	(1,007.83)	(1,134.78)	(1,145.15)	(1,133.89)
Observations	2,524	2,319	2,261	2,230	2,303	2,321	2,182
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the effects of exposure to UDLs on mean and median financial wealth. Mean effects estimated via OLS regressions, median effects via Recentered Influence Function (RIF) unconditional quantile regressions. All models control for year of marriage, country and cohort fixed effects, country-specific quadratic cohort trends, dummies for having a high school diploma, a college degree, several books at age 10, good housing conditions at age 10. Household-level sample. Standard errors clustered by country and cohort are reported in parentheses. For RIF regressions, clustered standard errors are computed via bootstrap (400 replications). *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Unconditional Quantile Treatment Effects of UDLs on Total Wealth.

Dep. Var.: Total Wealth (€)	(1)	(2)
Quantile 10		
Exposure to UDLs	4,529.02**	3,696.43*
	(1,951.56)	(1,990.80)
Quantile 25		
Exposure to UDLs	7,822.55***	6,593.67***
	(2,314.15)	(2,310.93)
Quantile 50		
Exposure to UDLs	6,986.49***	4,988.19**
	(2,499.68)	(2,412.86)
Quantile 75		
Exposure to UDLs	11,791.36***	8,934.44**
	(3,825.43)	(3,797.92)
Quantile 90		
Exposure to UDLs	13,842.30	9,437.01
	(8,497.89)	(8,553.47)
Observations	2,690	2,690
Covariates	No	Yes

Notes: The table reports the unconditional quantile treatment effects of exposure to UDLs on total wealth (the sum of financial and real welath). Unconditional quantile treatment effects are estimated via Recentered Influence Function (RIF) regressions. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Covariates included in Column (2) are dummies for having a high school diploma, a college degree, several books at age 10, good housing conditions at age 10. Household-level sample. Bootstrap (400 replications) standard errors clustered by country and cohort are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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