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

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**Abstract**

By allowing people to obtain divorce without the consent of their spouse, Unilateral Divorce Laws (UDLs) increase the risk of divorce. 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. Longer exposure to UDLs also increases female labour market participation and financial literacy, contributing to uncover the mechanisms through which the risk of divorce may affect savings. Our results are consistent with a precautionary 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 law reforms took place across many developed countries. By allowing people to obtain divorce without the consent of their spouse, the newly-introduced Unilateral Divorce Laws (UDLs) raised the risk of divorce. The economic literature has investigated the short-term effects of the adoption 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 household saving behaviour (González and Özcan, 2013, Voena, 2015).

From an economic perspective, there are two competing channels through 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) argue that this may encourage households' saving behaviour by a standard precautionary motive: as divorce is costly and households cannot hedge against this negative shock on the market, a higher 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, as asset division laws impose a division of marital properties within the couple and may create incentives for spouses to increase current consumption and decrease marriage-specific investments.

To the best of our knowledge, only a few contributions have attempted to test which of these channels dominates in practice, and the resulting empirical evidence remains rather inconclusive. For instance, while Voena (2015), González and Özcan (2013) and Pericoli and Ventura (2012) provide support for the precautionary saving channel, 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, little attention has been paid to the longer-run effects of UDLs on the standards 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). The problem 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 (Hsu, 2016, Lusardi and Mitchell, 2007, 2008).

In this paper, we explore the long-term consequences of the increase in the risk of divorce induced by UDLs on the stock of wealth of European married couples around retirement age. Several papers already provide evidence about the stark increase in marital separation following the introduction of UDLs across European countries. For instance, González and Viitanen (2009) use aggregate data for several Euroepan countries and find that the introduction of UDLs has permanently increased divorce rates in Europe by about 0.6 annual divorces per 1,000 people, a large effect considering that the European average annual divorce rate in 2002 was 2 per 1,000 people. Kneip et al. (2014) and Kneip and Bauer (2009) provide comparable evidence using micro-level and aggregate data - respectively - as they estimate that the introduction of UDLs accounts for about one quarter of the total rise in divorce rates in Europe between 1960 and 2000.[1]

Our analysis uses cross-sectional and life-history data from the Survey of Health, Aging and Retirement in Europe (SHARE). There are several reasons why SHARE is well suited for our purposes. First, by collecting harmonised 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 across several European countries. Second, SHARE provides detailed information about households' financial wealth and total wealth - i.e. the sum of net financial and real wealth - at the time of the interview, and about individual relationship histories throughout the lives of the respondents. Third,

[1]Comparable evidence for the US is provided by Friedberg (1998), who finds that UDLs have permanently raised divorce rates by 0.4 divorces per 1000 people, accounting for almost 20 percent of the increase in divorce rates between 1968 and 1988 in the US. However, using data for a longer time span and accounting for dynamic effects, Wolfers (2006) shows that the actual increase in the US is only 0.2 to 0.3 divorces per 1,000 persons per year, and that the effects are transitory and fade out within a decade.

the survey also contains rich information on an individual's life, including family background, early life conditions, work histories and financial literacy, thereby enabling us to shed light on the potential mechanisms through which divorce risk may affect savings.

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 adopted UDLs in the second half of the twentieth 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 an increase in the risk of divorce on household savings.

In support of our identification strategy, we show that - conditional on country, year of marriage, year of birth, and country-specific quadratic cohort trends - married couples exposed to UDLs for a different number of years are similar with respect to their family background, early life conditions and education. This evidence suggests that our estimated effects of UDL exposure on savings later in life are unlikely due to endogenous dynamic selection outside of marriage and into divorce. For instance, if wealthier couples were more likely to survive into marriage when exposed to UDLs - generating reverse causality - we should have found imbalances with respect to observable background variables correlated to saving propensity. On the contrary, the balancing tests suggest that - even in our selected sample of stable couples - exposure to UDLs can be considered as exogenous, as it does not vary with individual characteristics determined before the adoption of UDLs. Therefore, we can use the variation across countries in the timing of introduction of UDLs to identify the causal impact of the increase in the risk of divorce induced by UDLs on household wealth accumulation. Since the distribution of wealth is very skewed, we mostly focus on median instead of mean wealth regressions. Nevertheless, to understand the effect of UDLs on the entire distribution of savings, we also use mean regressions and a set of unconditional quantile regressions (see Firpo et al., 2009).

Consistently with a precautionary motive for savings, we find that the higher risk of

divorce induced by UDLs: an additional year of exposure to UDLs increases median net financial wealth by €1,493, which corresponds to an increase of approximately 6%. This effect 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 reaches €6,143 per year of exposure at the 90th percentile. Reassuringly, our estimates are similar - albeit a bit less precise - when we use total wealth as an alternative outcome.

In the second part of our analysis, we exploit the breadth of our survey data to gain a better understanding of the potential mechanisms through which a higher risk of divorce may affect savings. In particular, we show that longer exposure to UDLs leads to higher female labour market participation over the life course and 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 an increase in the risk of divorce induced by the introduction of UDLs on households' financial and total wealth around retirement age. This crucially differentiates our work from previous studies that estimate the short-run impact of UDLs on household savings (González and Özcan, 2013, Voena, 2015). Second, by exploiting data and quasi-experimental variation for several countries we are able to provide causal estimates that are valid for several countries, thereby increasing the external validity of our study. 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. We then discuss some potential mechanisms through which the risk of divorce may affect household savings in Section 5. The last Section is a conclusion.

# 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) that were carried out between 2006-07 and 2009-10. The SHARE data have a number of unique features that make them particularly attractive for our analysis.

First, by gathering harmonised current and retrospective information on a representative sample of the population aged 50+ in several European countries, SHARE enables us to conduct a cross-country study without having to worry about data comparability. In this paper, we present evidence for seven European countries - Austria, Belgium, Denmark, France, Germany, the Netherlands and Spain - where UDLs have been adopted during the second half of the twentieth 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. In addition to these seven countries, three other countries covered in SHARE - 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 - 2010 - and Switzerland - 2000) or too early (Sweden - 1915) to obtain information on couples that were married both before and after the introduction of UDLs. We find that our estimates remain unchanged when we include these three countries in the analysis. We also have to 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 because we can then 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 and for whom it would be hard to understand the connection between divorce risk and wealth accumulation. This sub-sample of stable couples accounts for 79.06% of ever-married couples that are present in SHARE within our selected countries and age range.[2] The availability of retrospective life history data on housing trajectories allows us also to exclude 88 individuals who were married in a different country than the one where they live at the time of the interview, which could be endogenous to changes in divorce laws across countries.[3]

In addition, SHARELIFE includes information on early life conditions, that we summarise using two indicators. As a proxy for parental investment in skill formation early in life, we follow Brunello et al. (2016) and construct an indicator variable taking value one if 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), and zero otherwise. As a proxy for family wealth and good housing conditions early in life, we use an indicator variable taking value one if the number of rooms in the house where the respondent was living at age 10 was at least as high as the number of persons living in the household, and zero otherwise.

Third, the second wave contains detailed information on household finances, which is

[2]González and Özcan (2013) and Voena (2015) also use married couples in their first marriage.

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

available only at the time of the interview. One financial respondent per household is asked to answer several questions on household income and wealth. 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.[4] We deflate all the wealth components using PPP exchange rates and CPI measures into 2006 German Euro, so that the values are comparable across countries and over time. Information on PPP-adjusted exchange rates and CPI measures is obtained from the OECD and national sources.[5]

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 with numbers.[6] On the basis of the number of correct answers to the four arithmetic 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) and Lusardi and Mitchell (2014), numeracy is a relevant component of financial literacy, and is strongly predictive of portfolio choices of individuals. Therefore, we construct an indicator variable

[4]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).

[5]Needless to say, information about wealth in SHARE is self-reported, and therefore subject to measurement error. However, using individual social security numbers, Bingley and Martinello (2015) match the Danish subsample of SHARE with administrative data drawn from Danish civil registries and tax reports, and show that measurement error for monetary variables in SHARE data is classical, suggesting that SHARE is a reliable source for the analysis of socioeconomic data.

[6]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?*".

taking value one (and zero otherwise) if the respondent has a numeracy score higher than the median, as an indicator of high financial literacy.[7]

Fifth, we use information on working histories to construct an indicator for whether individuals in our sample have ever worked during their life. The variables on financial literacy and labour market participation enable us to shed some light on the potential mechanisms through which the increase in the risk of divorce induced by UDLs may affect financial wealth later in life.

Lastly, from the second wave of SHARE, we also obtain information on gender, year of birth, country of birth and of current residence, educational levels (primary, secondary or post-secondary qualifications) and number of children.

In line with the literature in this area (González and Özcan, 2013, González and Viitanen, 2009), we select couples in their first marriage and whose head (i.e. the financial respondent) is between 50 and 70 years old at the time of the interview of SHARE wave 2.[8] We choose this age interval to obtain a sample of couples who are around retirement and are not too old to be strongly affected by survival bias.[9]

We use data at the household level for the analysis on savings and data at the individual level for the analysis on the potential mechanisms, i.e., numeracy and labour force participation. The analysis at the individual level also allows us to shed light on gender differences in the impact of UDLs on these mediators. Our final sample contains 2,690 couples for the household analysis on savings and 4,540 individuals for the individual analysis on financial literacy and labour market participation.[10]

[7]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.

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

[9]This age interval has been considered in several studies that focus on retirement, including, e.g., Mazzonna and Peracchi (2016). In a sensitivity analysis, we show that ourestimates are qualitatively similar when we consider couples aged 50-75 or 50-80 at the time of the interview.

[10]The size of the individual sample is not equal to twice the size of the household sample because we drop couples with missing information on financial literacy or labour market participation for at least one partner.

Table 2 reports descriptive statistics on the main variables 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 individual level.[11] Average financial assets and total assets are respectively equal to €65,510 and €344,105, while the median values of these variables are 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 at least 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% were living at age 10 in an accommodation with at least one room per person.

**[Table 2 about here]**

# 3 Empirical Methodology

## 3.1 Model Specification

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

$$Y_{ijk} = \alpha + \beta UDL_{ijk} + \gamma YoM_{ijk} + \delta X_{ijk} + \mu_k + \eta_j + \lambda_j^1 k + \lambda_j^2 k^2 + \varepsilon_{ijk} \qquad (1)$$

where the index $ijk$ denotes a 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 $i$. Assets are measured in levels to include households with debt (negative assets).

Our variable of interest is $UDL_{ijk}$, defined as the number of years the couple was exposed

[11] In the household sample, the statistics on individual variables such as age and education refer to the household head (i.e., the financial respondent).

to UDLs. Thus, it is a semi-continuous treatment variable that measures the number of years of marriage for couple $i$ since the introduction of UDLs in country $j$. We prefer this specification to a dummy treatment variable for marriage before/after UDL introduction, because our specification allows us to consider the intensity of the exposure to the UDL-induced divorce risk as current savings are the result of wealth accumulation over the life-cycle. For instance, a couple that got married well before the introduction of a UDL may have been exposed only marginally to the UDL-induced risk of divorce. It may be incorrect to assume that the saving behaviour of this couple should be equivalent to that of a couple married in the same year but residing in a country where a UDL was introduced early on, as the latter was exposed to unilateral divorce risk for longer time. Therefore, instead of estimating the average difference in savings between couples exposed or not exposed to UDLs for *any* given period of time, our specification allows us to estimate an average effect per year of UDL exposure. Considering that our outcome is the stock of savings around retirement age, we believe that this parameter is informative to describe how exposure to divorce risk affects the path of wealth accumulation until this point in the life.

Model (1) controls for year of marriage ($YoM_{ijk}$) and birth cohort fixed effects ($\mu_k$) to account for possible trends in wealth accumulation. We also include a full set of country fixed-effects ($\eta_j$) 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 over time. In model (1), we also include a set of individual pre-marital covariates that may affect financial assets and correlate with UDL exposure, contained in the vector $X_{ijk}$ and described in the previous section.[12] Finally, $\varepsilon_{ijk}$ represents a disturbance term.

[12] All specifications also include an indicator equal to one if the financial respondent is female and to zero otherwise.

## 3.2 Identification

Identification of the coefficient $\beta$ as the average causal effect of one additional year of exposure to UDLs on cumulated savings is granted by the quasi-natural experiment provided by the staggered timing of introduction of UDLs across countries. Our first identifying assumption is therefore that conditional on year of marriage, country and cohort dummies as well as country-specific cohort trends, the variation in the number of years of exposure to UDLs is as good as randomly assigned. This means that the UDLs ought to provide a source of variation in divorce risk that is not related to predetermined observable or unobservable characteristics of couples that may explain their saving behaviour.

Second, since we focus on the subsample of couples who are still in their first marriage at the time of the SHARE interview, we require also the absence of endogenous dynamic selection outside of marriage and into divorce that takes place differentially with respect to exposure to UDLs. For instance, we need to rule out the possibility that wealthier couples are more likely to survive into marriage when exposed to UDLs, generating reverse causality.

We provide supportive evidence about the joint validity of these first two assumptions by showing a set of balancing tests, aimed at verifying that still-married couples in our final sample who have been exposed to UDLs for different time periods are similar with respect to a set of predetermined observable characteristics correlated to saving propensity. We consider variables included in vector $X_{ijk}$, described above, as well as a broader set of pre-marital covariates related to family background (see for instance Gould et al., 2011) and that may be listed among the determinants of wealth accumulation.

Table 3 reports the estimates of "reverse regressions" of each of these predetermined covariates on our treatment variable, year of marriage, country and cohort dummies, as well as country-specific quadratic cohort trends.[13] In carrying out this analysis, we adjust p-values for multiple hypothesis testing, using the stepdown method proposed by Romano

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

and Wolf (2005) and implemented - among others, by Heckman et al. (2013). Failure to do so would in fact lead to detect spuriously significant effects. We find that the effects of UDL exposure on predetermined covariates are very close to zero and never significant at the 95 percent confidence level, supporting the identifying assumptions stated above.

**[Table 3 about here]**

To attribute a causal interpretation to our estimated effects, we also require that there is no other country-specific unobserved shock that affects saving behaviour and whose timing is correlated with the adoption of UDLs - generating omitted variables bias. Reassuringly, our results still hold when we exclude from our sample one country at a time, allowing us to rule out that potential concurring shocks happening in single countries are the main driver of our findings.

Finally, while the country and time variation of UDLs 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 law reforms. As a result, the anticipation of the introduction of UDLs by spouses would violate the identifying assumptions described above. To verify that endogenous adjustments of the timing of marriage in response to the anticipation of UDL introduction is not responsible for our findings, we will show that our results still hold when we exclude couples married in a 1-year interval around the year of adoption of UDLs, when (potentially endogenous) sorting into marriage before/after the law changes is more likely to have taken place.

## 3.3 Estimation

We estimate model (1) by Ordinary Least Squares (OLS) when focusing on mean effects, and using 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. Throughout the analysis, we cluster standard errors by country and year of marriage, the level of variation of exposure to UDLs.

# 4 Empirical Results

## 4.1 Main Results

Table 4 reports estimates of the long-term effects of an increase in the risk of divorce induced by UDLs on the median (columns 1 and 2) and the mean (columns 3 and 4) of financial assets of married couples. The results in columns 1 and 3 control for year of marriage, cohort and country fixed-effects, as well as quadratic country-specific cohort trends, while columns 2 and 4 also control for pre-marital covariates in vector $X_{ijk}$. The estimates in columns 1 and 2 suggest that an additional year of exposure to UDLs leads to an increase in median financial wealth of €1,857 to €1,493, depending on the specification, which correspond to an increase of approximately 7.5% to 6% relative to median financial wealth, respectively. The OLS estimates reported in columns 3 and 4 portray a similar picture: we find that an additional year of exposure to UDLs increases mean household savings by €3,309 to €2,581, depending on the specification. These effects correspond to approximately 5% to 4% of mean financial wealth, respectively.

**[Table 4 about here]**

## 4.2 Robustness

To assess the robustness of our main results, in Table 5 we report how our estimates change when we use different samples or specifications. First, in Panel A we show that the estimates are qualitatively similar when we control for the number of children, which is a potentially endogenous variable but may be strongly related with the need (or the possibility) to save.

Second, in Panel B we show that the main results are not affected by the inclusion of Italy, Sweden and Switzerland, where UDLs have been adopted either too early (Sweden - 1915) or too late (Italy - 2010 - and Switzerland - 2000) to observe enough couples married both before and after UDL introduction. Third, our identification strategy requires also that couples with different saving propensity do not adjust their timing of marriage in response to expectations about the introduction of a UDL. sorting into marriage before or after UDL introduction on the basis of saving propensity. To verify that our results are not only driven by potential violations of this no-sorting condition, in Panel C we show that our results are unchanged when we estimate equation (1) excluding from the sample spouses who were married in the close vicinity of the divorce laws (i.e., one year before/after the change in the laws). Finally, to validate our findings on a wider age range, in Panel D and Panel E respecitvely, we include also households whose head is aged 71 to 75 and 71 to 80, and show that our results still hold.

**[Table 5 about here]**

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.1 in the Appendix we report the estimated effects on median and mean wealth when we drop one country at a time from our sample.[14] The estimated coefficients on the exposure to UDLs remain fairly stable, ranging from €925 to €2,131 for the median and from €2,248 to €3,163 for the mean.

## 4.3 Quantile Treatment Effects

To investigate heterogeneous effects across the distribution of household savings, we report in Table 6 the estimates of unconditional quantile treatment effects obtained by RIF regressions (Firpo et al., 2009). Again, the model in column 1 controls for year of marriage, cohort and

[14]We use the specification that includes all controls, but the results are equivalent in the more simple specification that excludes the observables in vector $X_{ijk}$.

country fixed-effects, as well as linear and quadratic country-specific cohort trends, while the model in column 2 also controls for pre-marital covariates in the vector $X_{ijk}$. The effects of the higer risk of divorce induced by UDLs are larger at higher quantiles of the financial wealth distribution. For instance, considering results in column 2, the effect of exposure to UDLs goes up from €607 to €4,257 as we move from the 25th to the 75th percentile, and the treatment effects are even more pronounced when we compare the 10th with the 90th percentile. In other words, the long-term effects of the higher risk of divorce induced by UDLs are larger for richer households.

**[Table 6 about here]**

## 4.4 Effects on Total Wealth

As a further robustness test, we also verify whether the results still hold when we consider a different definition of household savings. To this aim, we conduct a parallel analysis using total wealth, i.e., the sum of real and financial wealth, as the dependent variable. The coefficients of interest for median and mean total wealth are reported in Table 7, and show a pattern that is similar to the one reported for financial wealth (see Table 4), whereby longer exposure to UDLs increases household savings. The estimated effects are also slightly smaller in relative terms with respect to those using 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.2 in the Appendix, even in this case we estimate larger effects at the top of the total 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 argue that individuals in a married couple, and in particular women, respond to the higher risk of divorce by improving their financial literacy - proxied in our exercise by a high value of the numeracy score - and increasing labour market participation, and that this may help to explain the uncovered effects on savings. On the one hand, van Rooij et al. (2012) document a positive association between financial literacy and wealth accumulation, while Lusardi and Mitchell (2014), Christelis et al. (2010) and Banks et al. (2010), show that people with better numerical abilities are better prepared for retirement in terms of savings, make more sophisticated investment choices and de-cumulate assets at a faster pace after retirement - in accordance with the prediction of a standard life-cycle model. On the other hand, by participating in the labour market women earn a salary, which allows them to increase their saving potential. Therefore, although in our setup we are not able to provide causal estimates about the link between each of these potential mechanisms and savings, finding that longer exposure to UDLs increases numeracy or labour force participation is still indicative about the relevance of these potential mechanisms.

To this end, we estimate the same specification as in model (1), with the main difference being 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 zero otherwise; and ($b$) an indicator variable taking value one if the person has ever been employed during her/his career and zero otherwise. To analyse potential gender differences in the reaction to increased divorce risk, we carry out the analysis both in the full sample and splitting the sample by gender.[15]

[15]To maintain a balanced composition of the samples by gender, we consider in this analysis only 2,270

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 therefore 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.

**[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 increase lifetime savings is through higher women labour force participation - and therefore earnings. The estimates for labour supply throughout the life course reported in columns 4 to 6 of Table 8 suggest that an additional year of exposure to UDLs increases the probability that someone has ever worked by 0.7 percentage points. However, column 5 shows that the results are driven by women.[16] 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 for women - the higher risk of divorce leads to adopt self-insuring behaviours, such as increased investments in financial literacy and labour force participation.

of 2,690 couples in our initial sample for whom we observe the outcome variables for both members.

[16]These results are in line with Kneip et al. (2014) who also use SHARELIFE data and find a strong effect on female labor 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 accumulate more savings as a consequence of the higher risk of divorce following the adoption of UDLs. According to our estimates, an additional year of exposure to UDLs increases median household savings by €1,493, which corresponds to approximately 6% relative to the median. We also show that the effects are particularly pronounced at higher quantiles of the financial wealth distribution, i.e., among more affluent households.

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 raising their labour market participation. Both mechanisms have been identified by the literature as being positively associated with savings.

Overall, our findings lend support to the precautionary motive for saving, in which spouses - and wives in particular - self-insure against the risk of a negative shock associated with divorce.

Although a structural model would be needed to reach firmer conclusions, we believe that the overall implications about the long-term effects of UDLs for the welfare of couples around retirement age are positive. On the one hand, approaching retirement with higher savings is surely favourable, as it diminishes the threat of ending up with public pensions as only resource to finance consumption and insure against negative shocks during retirement. Additionally, increased female labour force participation and numeracy have positive consequences for the empowerment of women both in the economy and within the couple.

On the other hand, higher savings come at the cost of foregone consumption. However, since this estimated effect on savings is mainly concentrated among the wealthiest, we believe that this effect is second order, as it has unlikely led couples to reduce consumption below a minimum acceptable level.

# 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)<br>Exposure to UDLs |
|---|---|
| High school diploma | 0.003 |
| College degree | 0.012* |
| Several books at age 10 | 0.004 |
| Good housing conditions | -0.001 |
| Parents had Mental Health Issues | 0.002 |
| Parents had Drinking Issues | -0.005 |
| Parents smoked | 0.005 |
| Missed school for 1+months in childhood | -0.000 |
| Parents had middle-class occupations | 0.009 |
| Parents had professional occupations | -0.003 |
| Did not live with mother at age 10 | -0.001 |
| Poor home sanitation at age 10 | -0.004 |
| Observations | 2,690 |

Notes: The table reports the coefficient of exposure to UDLs derived by reverse regressions of the pre-determinded covariates listed in in each row 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. *** p<0.01, ** p<0.05, * p<0.1. Standard errors not computed. P-values corrected for multiple hypothesis testing using the stepdown procedure of Romano and Wolf (2005). Failure to do so for can lead to detect spuriously significant effects. We use bootstrap clustered by country and year of marriage, based on 2,000 iterations of the stepdown procedure.

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 | 1,857*** | 1,493*** | 3,309*** | 2,581** |
| | (594) | (573) | (1,096) | (1,024) |
| 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 year of marriage 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 controls. | | | | |
| Exposure to UDLs | - | 1,363** | - | 2,321** |
| | - | (568) | - | (1,031) |
| Observations | - | 2,690 | - | 2,690 |
| Panel B. Including IT, SE and CH. | | | | |
| Exposure to UDLs | 1,120*** | 860** | 2,479*** | 1,983** |
| | (423) | (408) | (875) | (837) |
| Observations | 3,783 | 3,783 | 3,783 | 3,783 |
| Panel C. Drop couples married +1/-1 years around UDL introduction. | | | | |
| Exposure to UDLs | 1,728*** | 1,337** | 3,200*** | 2,469** |
| | (622.19) | (606.47) | (1,034.40) | (996.62) |
| Observations | 2,343 | 2,343 | 2,343 | 2,343 |
| Panel D. Age range: 50-75. | | | | |
| Exposure to UDLs | 1,768*** | 1,505*** | 3,106*** | 2,533*** |
| | (533) | (520) | (991) | (943) |
| Observations | 3,098 | 3,098 | 3,098 | 3,098 |
| Panel E. Age range: 50-80. | | | | |
| Exposure to UDLs | 1,278** | 1,050** | 2,730*** | 2,225** |
| | (496) | (482) | (903) | (870) |
| Observations | 3,357 | 3,357 | 3,357 | 3,357 |
| 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 year of marriage 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 | -134 |
| | (224) | (224) |
| Quantile 25 | | |
| Exposure to UDLs | 741** | 607** |
| | (305) | (295) |
| Quantile 50 | | |
| Exposure to UDLs | 1,857*** | 1,493*** |
| | (594) | (573) |
| Quantile 75 | | |
| Exposure to UDLs | 5,486*** | 4,528*** |
| | (1,649) | (1,594) |
| Quantile 90 | | |
| Exposure to UDLs | 7,725** | 6,143** |
| | (3,227) | (3,094) |
| 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 year of marriage 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*** | 4,988** | 7,542** | 5,350 |
| | ( 2,500) | ( 2,413) | (3,368) | (3,275) |
| 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 year of marriage 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 | 0.48 | 0.62 | 0.94 | 0.91 | 0.98 |

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 year of marriage are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

# Appendix

Table A.1: 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 | 1,430** | 1,797*** | 1,619*** | 2,131*** | 1,505** | 981* | 9251* |
| | (592.84) | (562.48) | (556.54) | (726.33) | (627.79) | (594.29) | (525.41) |
| Mean Effects | | | | | | | |
| Exposure to UDLs | 2,410** | 2,566** | 3,163*** | 2,678*** | 2,508** | 2,248* | 2,469** |
| | (1,059) | (1,161) | (1,108) | (1,008) | (1,135) | (1,145) | (1,134) |
| 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 year of marriage 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.2: Unconditional Quantile Treatment Effects of UDLs on Total Wealth.

| Dep. Var.: Total Wealth (€) | (1) | (2) |
|---|---|---|
| Quantile 10 | | |
| Exposure to UDLs | 4,529** | 3,696* |
| | (1,952) | (1,991) |
| Quantile 25 | | |
| Exposure to UDLs | 7,823*** | 6,594*** |
| | (2,314) | (2,311) |
| Quantile 50 | | |
| Exposure to UDLs | 6,986*** | 4,988** |
| | (2,500) | (2,413) |
| Quantile 75 | | |
| Exposure to UDLs | 11,791*** | 8,934** |
| | (3,825) | (3,798) |
| Quantile 90 | | |
| Exposure to UDLs | 13,842 | 9,437 |
| | (8,498) | (8,553) |
| 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 year of marriage are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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