European Economic Review 119 (2019) 97–113

Contents lists available at ScienceDirect

European Economic Review

journal homepage: www.elsevier.com/locate/euroecorev

The ant or the grasshopper? The long-term consequences of Unilateral Divorce Laws on savings of European households

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ARTICLE INFO

Article history:
Received 27 July 2018
Accepted 1 July 2019
Available online 12 July 2019

JEL classification:
G11
J12
J22
J32

Keywords:
Divorce risk
Household savings
Europe

ABSTRACT

Unilateral Divorce Laws (UDLs) allow people to obtain divorce without the consent of their spouse. Using the staggered introduction of UDLs across European countries, we show that households exposed to UDLs for a longer period of time accumulate more savings. This effect holds for both financial and total wealth and is stronger at higher quantiles of the wealth distribution. Consistent with a precautionary motive for savings, we also find that exposure to UDLs increases female labour supply, numeracy, trust in others and dispositional optimism.

1. Introduction

In the second half of the twentieth century, a wave of liberal reforms to divorce law 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 and changed the allocation of bargaining power between partners. 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 several competing channels through which UDLs may affect marriage-specific investments and assets accumulation. Cubeddu and Rios-Rull (1997) argue that the increased divorce risk induced by UDLs may encourage households' saving behaviour by a standard precautionary motive: as divorce is costly (both due to the legal fees and because of the loss in economies of scale and risk-sharing opportunities) and households cannot hedge against this negative shock on the market, a higher risk of separation induces married couples to save more. Voena (2015) claims that, under a regime of equal division of property upon divorce, UDLs can affect saving behaviour throughout the marriage

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https://doi.org/10.1016/j.euroecorev.2019.07.002
0014-2921/

period, by allowing partners to exercise a credible divorce threat. This should increase the bargaining power of the member of the couple with a lower share in household resources – usually the wife – with positive effects on savings, as husbands will save more to self-insure against the loss of half of their wealth upon divorce. However, Mazzocco et al. (2014) stress that an increase in the probability of divorce may adversely affect saving propensity while married, as asset division laws impose a division of marital properties within the couple. In their model, this channel creates 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 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 buying a house or supporting a spouse through school. In addition, little attention has been paid to the longer-run effects of UDLs on the savings of couples around retirement. This is somewhat 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 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; Hsu, 2016).

In this paper, we explore the long-term consequences of exposure to UDLs on the wealth of married couples around retirement age in Europe. 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) find that the introduction of UDLs has permanently increased divorce rates in Europe by about 0.6 annual divorces per 1000 people, a large effect considering that the European average annual divorce rate in 2002 was 2 per 1000 people.[1]

Our analysis uses cross-sectional and life-history data from the Survey of Health, Ageing and Retirement in Europe (SHARE). Our final sample consists of close to 2700 couples whose head is between 50 and 70 years old, 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). With the exception of Austria and the Spanish region of Catalonia – where separation of property upon divorce holds – equal sharing of property upon divorce is the default in these countries.[2]

Our research design exploits the staggered timing of the introduction of UDLs across these countries to identify their reduced-form effect on household savings. Since the distribution of wealth is very skewed, we rely on both median and mean wealth regressions. To understand the effect of UDLs on the entire distribution of savings, we also use a set of unconditional quantile regressions (see Firpo et al., 2009). Our empirical analysis pays special attention to deal with several threats to internal validity, related to selection into and outside of marriage induced by the introduction of UDLs, omitted variables bias, concurring shocks, and anticipation effects.

We find that an additional year of exposure to UDLs increases median net financial wealth by 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 first decile of the wealth distribution, and increases at higher percentiles. Our estimates are smaller in magnitude and a bit less precise when we use total wealth (i.e., the sum of real and financial assets) as an alternative measure of the dependent variable. This finding is consistent with the idea that real assets represent the wealth component that is most difficult to change.

In the second part of our analysis, we exploit the breadth of our survey data to better understand the potential mechanisms through which exposure to UDLs may affect savings. We show that exposure to UDLs leads to higher female labour supply, numeracy, trust in others and dispositional optimism. Overall, these findings are consistent with a precautionary saving explanation, in which the wife self-insures against the risk of negative shocks associated with divorce. Instead, an increase in the bargaining power of the woman seems less likely to be an explanation for our results since this would imply a decrease in female labour supply, as shown by Voena (2015).

Our contribution to the literature is threefold. First, we focus on the long-term effects of exposure to UDLs on the whole distribution of households' financial and total wealth around the retirement age. This differentiates our work from previous studies that estimate the short-run impact of UDLs on mean household saving or on the saving rate (González and Özcan, 2013; Voena, 2015). Second, by exploiting data and quasi-experimental variation across countries, we are able to provide causal estimates that are valid for several European 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 exposure to UDLs and household savings. Overall, our results provide support for the precautionary motive for saving.

The remainder of this paper is organised 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

[1] This is in line with the evidence provided by Kneip et al. (2014) and Kneip and Bauer (2009) who estimate that the introduction of UDLs accounts for about one quarter of the total rise in divorce rates in Europe between 1960 and 2000. 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% 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 lower, at only 0.2 to 0.3 divorces per 1000 persons per year, and that the effects are transitory and fade out within a decade.

[2] Using data for the first wave of SHARE, we find that 79% of Austrian couples and 90% of Catalonian couples nonetheless report to have agreed upon a joint property regime.

Table 1
Year of introduction of Unilateral Divorce Laws (UDLs) and exposure to UDLs by country.

	(1) Austria	(2) Belgium	(3) Denmark	(4) France	(5) Germany	(6) Netherlands	(7) Spain
Year of UDL introduction	1978	1975	1970	1976	1977	1971	1981
Couples married before UDL	150	416	160	314	323	255	431
Couples married after UDL	16	92	209	73	48	174	29
Total number of couples	166	508	369	387	371	429	460
Years of exposure to UDLs							
Mean	28.5	30.9	31.3	29.0	28.9	33.6	25.5
Std. dev	1.5	2.7	7.6	3.4	3.1	4.1	2.0
Min	18	5	0	2	2	7	5
Max	29	32	37	31	30	36	26

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

are reported in Section 4, which also includes a set of robustness checks. We discuss the potential mechanisms through which exposure to UDLs may affect household savings in Section 5. Conclusions follow.

2. Data and institutional context

We use 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 allows us to conduct a cross-country study without having to worry about data comparability. 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 UDLs 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), the number of couples married before and after the change in divorce laws in the sample, and descriptive statistics for years of exposure to UDLs across the seven countries included in the sample.

In addition to the seven countries that we consider, two other countries covered in SHARE – Switzerland and Sweden – have also introduced UDLs by 2010. However, we are forced to exclude them because the switch to unilateral divorce occurred either too early (1915 in Sweden) or too late (2000 in Switzerland) 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 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 for SHARE in that country (Mazzonna and Peracchi, 2017).

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 it allows 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 and for whom it would be hard to understand the connection between divorce risk and wealth accumulation.[3] 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.[4]

SHARELIFE also includes information on early life conditions that we summarise using two indicators. As a proxy for parental investment in skill development early in life, we follow Brunello et al. (2017) and construct an indicator variable taking value one if the respondent had more than 10 books in the place where she or he 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 of SHARE contains detailed information on household finances, which is 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

[3] González and Özcan (2013) and Voena (2015) also use married couples in their first marriage. Within our selected countries and age range, 79% of the respondents that were ever married are still in their first marriage at the time of the SHARELIFE interview.

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

and corporate bonds, stocks, mutual funds, individual retirement accounts, contractual savings for housing and the face value of life insurance policies) minus financial liabilities.[5] 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.[6] We deflate all the wealth components using PPP exchange rates and CPI measures into 2006 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.[7]

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. 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. On the basis of the number of correct answers to the four arithmetic questions, Dewey and Prince (2005) construct a numeracy index that ranges from one to five.[8] The numeracy index has no cardinal interpretation, and we can attach to it an ordinal meaning at best. Hence, analysing this outcome variable with a linear regression would not be a good approach. An alternative could be to use an ordered probit model. However, Bond and Lang (2019) show that any finding from ordered probit models can be reversed by log-normal transformations of the index function. We therefore refrain from using these approaches, and construct a positional indicator taking value one (and zero otherwise) if the respondent has a numeracy score higher than the median. As shown by Christelis et al. (2010) and Lusardi and Mitchell (2014), numeracy is a relevant component of financial literacy, and is a strong predictor of individual portfolio choices. From SHARE wave 2, we also obtain information on trust on others, measured on a Likert scale going from 0 to 10, which we use to construct a dummy equal to one if the level of trust of the respondent is above the median. Following Puri and Robinson (2007), we measure dispositional optimism as the difference between self-assessed survival probabilities and that obtained from actuarial life tables by gender, country and year of birth provided by the Human Mortality Database (see http://www.mortality.org).[9] Both trust and optimism are correlates of financial development and savings. Finally, we use information on working histories to gather information on labour supply throughout the life course. Information on labour supply, numeracy, trust and optimism enables us to shed light on the potential mechanisms through which exposure to UDLs may affect financial wealth.

In line with the literature in this area (González and Viitanen, 2009; González and Özcan, 2013), 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.[10] 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.[11]

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., labour supply, numeracy, trust and optimism. 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 2690 couples for the household-level analysis on savings and 4959 individuals for the individual-level analysis.[12]

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.[13] 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

[5] Unfortunately, no comprehensive measure of pension wealth is available in SHARE for the waves that we consider.

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

[7] The information on wealth in SHARE is self-reported and therefore subject to measurement error. However, using individual social security numbers, Bingley and Martinello (2017) match the Danish subsample of SHARE with administrative data 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.

[8] The following four questions are asked to SHARE respondents. "*1. If the chance of getting a disease is 10%, 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% interest each year. How much would you have in the account at the end 2 years?*". 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.

[9] As in Angelini and Cavapozzi (2017), we elicit respondents' self-assessed survival probabilities from the question in SHARE: "*What are the chances that you will live to be age T or more?*". The target age *T* depends on the age of the respondent at the time of the interview. It is equal to 75 for respondents aged 50–65, 80 for those aged 66–70, 85 for those aged 71–75, 90 for those aged 76–80, 95 for those aged 81–85, 100 for those aged 86–95, 105 for those aged 96–100, and 110 for those aged 101–105. We then use the information from life tables by gender, country and year of birth to compute actuarial probabilities of survival for the same target age.

[10] 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. On average, married women are two years younger than their husbands.

[11] This age interval has been considered in several studies that focus on retirement, including Mazzonna and Peracchi (2017). In a sensitivity analysis, we show that our estimates are qualitatively similar when we consider couples aged 50 to 80 at the time of the interview. We also directly test for UDL-induced mortality in the 50–70 sample, but found no effect of UDL exposure on the likelihood of widowhood at the time of the wave 2 SHARE interview.

[12] The size of the individual sample is not equal to twice the size of the household sample because we drop individuals with missing information on the variables used in the analysis. Our results for wealth and for the mechanisms still hold even when we restrict our sample to couples for whom we observe all variables we use as mechanisms for both members. Due to the smaller sample size, however, our estimates for optimism are less precise.

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

Table 2
Descriptive statistics.

Variable	Mean	Std. dev.
Panel A: Sample of households. Sample size: 2690		
Household net financial wealth (€)	65,510	123,226
Household net total wealth (€)	344,105	378,634
Years of exposure to UDLs	29.75	4.716
Age (financial respondent)	59.81	5.818
Female (financial respondent)	0.454	0.498
Year of marriage	1970.9	7.546
Marriage duration (years)	35.86	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
Panel B: Sample of individuals. Sample size: 4959		
High numeracy score	0.557	0.497
High trust	0.565	0.496
Dispositional optimism	−0.038	0.259
Years of exposure to UDLs	29.783	4.685
Age	59.772	6.310
Year of marriage	1970.8	7.538
Marriage duration (years)	35.927	7.525
High school diploma	0.342	0.474
College degree	0.265	0.441
Several books at age 10	0.612	0.487
Good housing conditions at age 10	0.347	0.476

Notes: Both samples consider households (Panel A) and individuals (Panel B) aged 50 to 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. “Several books at home at age 10” is an indicator variable for having 10 or more books at home at age 10. “Good housing conditions at age 10” is an indicator variable for having at least one room per person in the accommodation where living at age 10. “High numeracy score” and “High trust” are indicator variables for numeracy score and trust above the median.

€ 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. Individuals are 60 years old on average at the time of the interview, 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.

3. Empirical methodology

3.1. Model specification

To examine how exposure to UDLs affects the savings of married couples, we estimate the following linear regression model:

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

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 to UDLs. 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 binary 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: current savings are the result of wealth accumulation over the life-cycle. For instance, a couple that was 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 a longer period of time.

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. Given that the variation in UDL exposure in our data is mostly concentrated between 25 and 35 years of exposure, our estimated effect shall be interpreted as the average marginal effect on the stock of savings around retirement age of a one-year change in UDL exposure within this range.

The model in Eq. (1) controls for year of marriage (YoM_{ijk}) and birth cohort fixed effects (μ_k) to account for possible trends in wealth accumulation.[14] 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 over time. 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.[15] Finally, ε_{ijk} represents a disturbance term.

One concern related to our specification could be that, once year of marriage, cohort and country fixed effects, as well as country-specific quadratic cohort trends and additional controls are included, there is not enough remaining variation in the years of exposure to UDLs to identify its effect on wealth. However, the R-squared of a regression of UDL exposure on all these covariates is equal to 0.83 (i.e., well below 1), thereby suggesting that there is substantial remaining variation.

3.2. Identification

Identification of the coefficient β 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 the introduction of UDLs across countries. Our first identifying assumption is 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 scattered introduction of UDLs shall provide a source of variation that is not related to predetermined observable or unobservable characteristics of couples that may explain their saving behaviour.

Second, since we focus on the sub-sample of couples who are still in their first marriage at the time of the SHARE interview, we also require 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.

Focusing on this latter assumption, we follow Kneip et al. (2014) and investigate whether the effects of UDL exposure on the hazard of divorce is heterogeneous with respect to a set of predetermined observable characteristics correlated to saving propensity. To do so, we estimate a Cox proportional hazard model on individual level data. We follow the same approach as in Eq. (1), but in this case the treatment is an indicator for being in a time period after the UDL introduction. We also interact this indicator with a set of pre-determined variables included in vector X_{ijk}, described above, as well as a broader set of pre-marital covariates related to family background (see, e.g., Gould et al., 2011), which may be listed among the determinants of wealth accumulation.[16] The results reported in Table 3 show that the effect of UDL introduction on the hazard of divorce does not vary with these characteristics, as the interactions terms are not statistically significant.[17] By increasing the risk of divorce, UDLs have induced dynamic selection among married couples, but we do not detect any pattern of endogenous selection with respect to the comprehensive set of determinants of savings that we analysed.

We also provide supportive evidence about the joint validity of the first two identification assumptions stated above by showing a set of balancing tests, aimed at verifying that married couples in our final sample who have been exposed to UDLs for different time periods are similar with respect to these characteristics.

Table 4 reports the estimates of "reverse regressions" of each of these predetermined covariates – and of two match-specific variables measuring the age and education gaps between partners[18] – on our treatment variable, year of marriage, country and cohort dummies, as well as country-specific quadratic cohort trends.[19] As we are verifying balancing on multiple covariates, for this analysis we report both the standard p-values and the ones adjusted for the problem of multiple hypothesis testing using the step-down method proposed by Romano and Wolf (2005) and implemented, among others, by Heckman et al. (2013). We find that the effects of UDL exposure on predetermined covariates are very close to zero and in most cases not statistically significant, especially once we take into account the problem of multiple testing.[20]

In addition, to further support the validity of our findings, in the sensitivity analysis (see Section 4.2) we also consider an imputation strategy akin to Olivetti and Petrongolo (2008). In this exercise, we also keep in our sample divorced individuals

[14] We include year of marriage in Eq. (1), as exposure to UDLs is measured in years. If we do not control for year of marriage, then exposure to UDLs will also pick up the effect of marriage duration. Since the timing of marriage could be endogenous to UDL introduction, in Table A.1 in the Appendix we show that there is no response of the timing of marriage to UDL introduction. To further alleviate endogeneity concerns, we also estimate Eq. (1) after excluding year of marriage from the controls and measuring exposure to UDLs as a share of marriage duration, thereby weakening the correlation between UDL exposure and marriage duration. All results are quantitatively comparable to our baseline specification.

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

[16] Details about these variables are reported in the notes to Table 3.

[17] The main effects are larger in magnitude but not statistically different from the one estimated by Kneip et al. (2014).

[18] These two variables are observed only for couples that are still living together. Therefore, we cannot include them in our duration analysis and in the analysis of selection into marriage.

[19] As suggested by Pei et al. (2019), 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.

[20] The results of the balancing tests hold also using the individual-level sample, both pooled and split by gender.

Table 3
The effect of UDL introduction on divorce – Cox proportional hazard model.

	(1) Hazard rate	(2) Hazard rate	(3) Hazard rate
After UDL introduction	1.826***	1.803***	1.991***
	(0.169)	(0.174)	(0.424)
After UDL introduction × …			
Experienced financial hardship before age 18			0.518*
High school diploma			1.059
College degree			0.995
Several books at age 10			0.977
Good at math			1.193
Good housing conditions at age 10			0.946
Poor housing sanitation at age 10			0.920
Parents drank, smoked or had mental health issues			0.838
Missed school for 1+ months in childhood			0.711
Had no serious childhood diseases			1.516
Parents had professional occupations			1.165
Did not live with mother at age 10			0.796
Did not live with father at age 10			1.148
Joint significance of all interactions terms (p-value)			0.354
Observations	8160	8160	8160
Covariates	No	Yes	Yes

Notes: The table reports the coefficients of "After UDL introduction" and its interaction with pre-determined covariates on the hazard rate of divorce. All models control for year of marriage, country and cohort fixed effects, and country-specific quadratic cohort trends. Additional covariates included in Columns (2) and (3) are the ones for whom the interaction with "After UDL introduction" are reported in Column (3). Good at math is an indicator variable equal to 1 if the respondent was better than average classmate in math at primary school, and to 0 otherwise. Good housing condition is an indicator variable equal to 1 if there were at least as many rooms as people in the accommodation where the respondent was living at age 10, and to 0 otherwise. Poor housing sanitation is an indicator variable equal to 1 if there was no cold running water or inside toilet in the accommodation where the respondent was living at age 10, and to 0 otherwise. Serious childhood diseases listed in the survey are: Infectious disease (e.g. measles, rubella, chickenpox, mumps, tuberculosis, diphtheria, scarlet fever); Polio; Asthma; Respiratory problems other than asthma; Allergies (other than asthma); Severe diarrhoea; Meningitis/encephalitis; Chronic ear problems. Standard errors clustered by country and year of marriage reported only for the main effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and couples where at least one member has divorced in the past, which are dropped from our main analysis.[21] For these groups, we use the observed level of wealth as an imputed value for the level of wealth that they would have experienced had they (or their current partners) not divorced. We then estimate the effects of UDLs on wealth using median regressions in this larger sample. As highlighted by Olivetti and Petrongolo (2008), median regressions have a very appealing feature for imputation methods. In fact, the estimates solely depend on the relative standing of observations with respect to the median one, and not on the specific imputed wealth value. Thus, as long as households keep on being on the same side of the median of the wealth distribution both in case of marriage continuation and in case of separation, imputations and median regression allow us to overcome dynamic selection.[22]

Another concern for our identification strategy would be the presence of an effect of UDLs on selection into marriage if different types of individuals decide to get married as a result of the introduction of UDLs, thereby generating selection bias. To verify whether this is the case, we use SHARE data on all individuals who ever got married in our chosen countries and age range, and employ a difference-in-differences model, in which we regress the pre-determined characteristics used in the previous analysis on an indicator variable equal to 1 if the individual married for the first time after the introduction of UDLs, and to 0 otherwise, country fixed effects, year of first marriage fixed effects, country-specific quadratic trends in the year of first marriage and gender.[23] The results are reported in Table 5: since we do not find any strong evidence of changes in characteristics of spouses at their first marriage as UDLs are approved, we conclude that it is unlikely that UDLs affected selection into marriage. We also approach this issue more directly in Table A.1 in the Appendix. In this analysis, we consider the sample of all individuals aged 50–70 and residing in one of the seven countries covered in this study, and we estimate the effects of exposure to UDLs since the age of 16 (the minimum marriage age in our sample) on being ever married (columns 1 and 2) and on age at first marriage conditional on marriage (columns 3 and 4). In columns 2 and 4, we also interact UDL exposure with the individual covariates, in order to assess potential heterogeneous effects. The regressions are conditional on cohort and country fixed effects, country-specific quadratic trends and individual covariates (excluding year of marriage). The empirical results show that UDL exposure since 16 has no impact on the probability of

21 We consider each of these individuals as a couple – as each individual is the survivor of a marriage that either (a) is still in place but is not the first for the other member of the couple, or (b) ended up in a divorce.

22 Bertoni et al. (2015) is another application of median regression and imputation to correct for dynamic selection.

23 Even in this case, we report both the standard p-values and the ones adjusted for the problem of multiple hypothesis testing using the step-down method proposed by Romano and Wolf (2005).

Table 4
Balancing tests.

	(1) Exposure to UDLs		
	Coefficient	Standard p-value	Corrected p-value
Experienced financial hardship before age 18	−0.001	0.343	0.888
High school diploma	0.003	0.400	0.984
College degree	0.012	0.006	0.083
Partners have the same educational level	−0.009	0.087	0.581
Absolute age gap between partners	−0.009	0.788	0.992
Several books at age 10	0.004	0.348	0.984
Good at math	0.010	0.055	0.470
Good housing conditions at age 10	−0.001	0.810	0.992
Poor housing sanitation at age 10	−0.004	0.310	0.976
Parents drank, smoked or had mental health issues	0.004	0.411	0.984
Missed school for 1+ months in childhood	−0.000	0.964	0.992
Had no serious childhood diseases	−0.001	0.809	0.992
Parents had professional occupations	−0.003	0.248	0.920
Did not live with mother at age 10	−0.001	0.376	0.984
Did not live with father at age 10	−0.006	0.027	0.290
Observations		2690	

Notes: The table reports the coefficient of exposure to UDLs derived by reverse regressions of the pre-determined 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. Standard p-values as well as p-values corrected for multiple hypothesis testing using the stepdown procedure of Romano and Wolf (2005) are reported. We use bootstrap clustered by country and year of marriage, based on 250 iterations of the stepdown procedure.

Table 5
Selection into marriage.

	(1) Married after UDL introduction		
	Coefficient	Standard p-value	Corrected p-value
Experienced financial hardship before age 18	0.001	0.847	0.988
High school diploma	0.002	0.910	0.988
College degree	−0.006	0.784	0.988
Several books at age 10	0.005	0.800	0.988
Good at math	−0.002	0.946	0.988
Good housing conditions at age 10	0.001	0.955	0.988
Poor housing sanitation at age 10	−0.017	0.433	0.988
Parents drank, smoked or had mental health issues	−0.003	0.988	
Missed school for 1+months in childhood	0.004	0.672	0.988
Had no serious childhood diseases	0.018	0.140	0.912
Parents had professional occupations	0.030	0.049	0.474
Did not live with mother at age 10	−0.014	0.062	0.772
Did not live with father at age 10	0.004	0.730	0.988
Observations		7723	

Notes: The table reports the difference-in-differences effects of marriage after UDL introduction on the pre-determined covariates listed in in each row on a indicator variable for being married after UDL introduction. All models control for year of marriage fixed effects, gender and cohort fixed effects, and country-specific quadratic trends in year of marriage. The sample includes all SHARE respondents in our countries and age range that have ever been married. Year of marriage refers to the first marriage. Standard p-values as well as p-values corrected for multiple hypothesis testing using the stepdown procedure of Romano and Wolf (2005) are reported. We use bootstrap clustered by country and year of marriage, based on 250 iterations of the stepdown procedure.

being ever married or on age at first marriage. In addition, these zero average effects do not mask any heterogeneity along the observable dimensions that we consider, since the interactions with the individual covariates are generally small and jointly insignificant, as confirmed by the p-value of the F tests reported at the bottom of columns 2 and 4.

At this stage, it is worth remarking that we cannot exclude that forces other than those observed are important to determine selection effects. However, although we wish to provide evidence on the absence of selection with respect to preferences and other social norms that led to the introduction of UDLs across countries, we are constrained by data availability, and have to focus only on variables for which at least some rough proxy that pre-dates the treatment is observed. Still, we believe that the breadth of our survey data helps us to alleviate this concern.

A further assumption required to attribute a causal interpretation to our estimated effects is that there is no other country-specific unobserved shock that affects saving behaviour and whose timing coincides with that of the adoption of UDLs – generating omitted variables bias. In Section 4.2, we provide support for this assumption in three ways. First,

Table 6
Effects of UDLs on financial wealth. Median and mean regressions.

Dep. var.: financial wealth (€)	Median		Mean	
	(1)	(2)	(3)	(4)
Exposure to UDLs	1857***	1493***	3309***	2581**
	(537)	(536)	(1,096)	(1,024)
Observations	2690	2690	2690	2690
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. The covariates included in Columns (2) and (4) are indicator variables for having a high school diploma, a college degree, several books at age 10, and good housing conditions at age 10. Household-level sample. Standard errors clustered by country and year of marriage are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

we show that our results still hold when we exclude from our sample one country at a time, allowing us to rule out the possibility that potential concurring shocks happening in single countries are the main drivers of our findings. Second, we estimate a placebo regression in which we switch the year of introduction of UDLs across countries. Third, we run a sensitivity analysis in which we replace the country-specific quadratic trends with quadratic trends in GDP per capita.

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 the 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 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 and inference

We estimate Eq. (1) by OLS when focusing on mean effects, and using Recentered Influence Function (RIF) unconditional quantile regressions (Firpo et al., 2009) to recover 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 6 reports estimates of the long-term effects of exposure to UDLs on the median (see columns 1 and 2) and the mean (see columns 3 and 4) of financial assets of married couples. The results in columns 1 and 3 control for the 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 € 1857 to € 1493, 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 € 3309 to € 2581, depending on the specification. These effects correspond to approximately 5% to 4% of mean financial wealth, respectively.

4.2. Robustness

In this section we describe how our estimates change when we use different samples or specifications.

First, we notice that the slight changes in coefficients due to the inclusion of covariates in Table 6 may signal a problem of omitted variables bias. To verify whether this is indeed the case, we apply the procedure suggested by Oster (2019), and estimate how important selection on omitted variables should be with respect to selection on the included ones (the ratio between the two being called δ) to explain away the estimated treatment effect. For example, $\delta = 2$ would imply that the omitted variables would need to be twice as important as the included ones to produce a treatment effect of zero. According to Oster (2019) and to the previous work on the topic by Altonji et al. (2005), a value of δ above 1 is sufficient to claim

Table 7
Effects of UDLs on financial wealth. Median regressions. Robustness tests.

Dep. var.: financial wealth (€)	Median (1)
Panel A. Including as controls all the variables used in the balancing tests.	
Exposure to UDLs	1705***
	(596)
Panel B. Cluster by country (Wild boostrap p-values reported in brackets).	
Exposure to UDLs	1493
	[0.10]
Panel C. Including Sweden and Switzerland.	
Exposure to UDLs	1293**
	(557)
Panel D. Drop Catalonia and Austria, where by default asset property regime is separated.	
Exposure to UDLs	1442***
	(576)
Panel E. Drop couples married +1/−1 years around UDL introduction.	
Exposure to UDLs	1337**
	(581)
Panel F. Age range: 50 to 80.	
Exposure to UDLs	1050***
	(446)
Panel G. Placebo test: switched order of UDL year of introduction by country.	
Exposure to UDLs	822
	(601)
Panel H. Including quadratic trends in GDP per capita at birth by country and cohort instead of country-specific quadratic cohort trends	
Exposure to UDLs	1704***
	(517)
Covariates	Yes

Notes: Unless otherwise stated, the table reports the effects of exposure to UDLs on median financial wealth, estimated 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, and indicator variables for having a high school diploma, a college degree, several books at age 10, and good housing conditions at age 10. Household-level sample. The additional covariates included in Panel A are listed in Table 4. Sample sizes: Panel A: 2181; Panels B, G, H: 2690; Panel C: 3149; Panel D: 2490; Panel E: 2343; Panel F: 3357. Standard errors clustered by country and year of marriage are reported in parentheses. In Panel B, wild-bootstrap p-values are obtained as in Cameron et al. (2008) (the number of countries is equal to 7). In Panel G, the placebo years of UDL introduction by country are the following: Austria: 1971; Belgium: 1977; Denmark: 1981; France: 1976; Germany: 1975; Netherlands: 1978; Spain: 1970. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

robustness of the empirical results to omitted variables bias.[24] We estimate a value of δ equal to 2.71 for median regression and 2.67 for mean regression. These results indicate that, in both cases, selection on omitted variables should be about 2.7 times stronger than selection on the included ones to drive our estimated treatment effect to zero, thus alleviating concerns related to coefficient instability and omitted variables bias.

In what follows, we present other robustness tests. For simplicity, we mostly focus on the specification in column 2 of Table 6 but results hold also for the other specifications.

In Table A.2 in the Appendix we report results of our imputation strategy to address the issue of dynamic selection described in Section 3.2 and based on median regressions estimated in a sample that includes also divorced individuals. For them, we use observed wealth as a measure of the potential wealth that they would have experienced had they not divorced. Following Olivetti and Petrongolo (2008), the assumption for consistent estimation of the effects of UDL exposure in this full sample is that the position of these couples with respect to the median of the wealth distribution would have been the same, had they not divorced. The result still shows a positive and significant effect of UDL exposure on savings, confirming that dynamic self-selection into marriage due to UDL exposure is not a major source of concern for our analysis.

Additional robustness tests are reported in Table 7. First, in Panel A we show that our main effect is qualitatively similar when we control for all the covariates included in the balancing tests, which are strongly related with the need (or the possibility) to save and vary by cohort, and thus exposure to UDL. This result further mitigates concerns related to the omitted variables bias discussed above.[25] Second, Panel B shows that the significance of our main effect is not strongly

[24] The procedure requires to choose a value of Rmax, the maximum attainable level of R-squared. As argued by Oster (2019), a value of R-squared=1 is too conservative, while a reasonable choice is to set Rmax = 1.3 times the R-squared of the model with all controls. In our case, this is equal to 0.23 for the median regression and to 0.16 for the mean regression.

[25] In fact, our main result is also robust and still pass the tests proposed by Oster (2019), when we include among the covariates a set of post-treatment "bad" controls, that includes the number of children, a dummy for currently living in a rural area, indicators for being currently employed, retired, or other for both members of the couple, and labour market experience of both members of the couple. The estimates are available from the authors.

affected when we cluster standard errors by country instead of country and year of marriage. As there are only seven countries in our data, we obtain p-values that are robust to clustering at the country level using wild bootstrap.

Third, in Panel C we show that the main result is not affected by the inclusion of Sweden and Switzerland, where UDLs have been adopted either too early (1915 in Sweden) or too late (2000 in Switzerland) to observe enough couples married both before and after the introduction of UDLs. In Panel D, we show that our main result still holds when we drop from the sample Austria and Catalonia, where asset property is separated by default.

Our identification strategy requires the absence of sorting into marriage on the basis of saving propensity before or after the introduction of UDLs. To verify that our main result is not only driven by potential violations of this no-sorting condition, we show in Panel E that our effect is qualitatively similar when we estimate Eq. (1) and exclude 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). To validate our findings on a wider age range, we also include households whose head is between 71 and 80 and show in Panel F that our main result still holds.

A remaining issue about our identification strategy concerns the possibility that, if our model was mis-specified, UDL exposure could be picking up some concurring country-specific trend. We deal with this concern in two ways. First, we run a placebo exercise, where we switch the order of the years of introduction of UDLs across countries by maximising the distance with respect to the original distribution.[26] If our model is correctly specified, we should find no significant effect of UDLs on wealth under this placebo assignment. The result reported in Panel G supports this view.[27] Second, as a specification test, we also substitute the country-specific quadratic time trends with quadratic trends in GDP per capita at birth by cohort and country (the data are taken from the Maddison tables). The effect presented in Panel H is similar to the one obtained with our main specification. Given the concern raised by Wolfers (2006) that country-specific cohort trends might inadvertently pick up some dynamics induced by the policy change and lead to biased results, we present additional sensitivities to the specification of trends in Table A.3 in the Appendix. Results for our main specification are always stable irrespective of whether we omit trends, include linear or quadratic country-specific cohort trends, or linear or quadratic trends in GDP per capita at birth by cohort and country. In addition, goodness-of-fit tests (i.e., AIC and adjusted R-squared) suggest that the specification with quadratic country-specific cohort trends – that we chose as our baseline – has the best fit.

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, Table A.4 in the Appendix reports the estimated effects on median wealth when we drop one country at a time from our sample: the estimated coefficients on the exposure to UDLs remain fairly stable, ranging from € 925 to € 2131.

A final concern with our empirical specification for the household-level regressions could be related to our choice of indexing all covariates – including birth cohort – to the household head. If old husbands respond in wealthy couples and younger wives in less wealthy couples this might bias the estimates. We have carried out two tests in this regard. First, we repeat our individual-level analysis on the balanced sample of couples for whom both members are interviewed, this time considering as dependent variable a binary variable taking value 1 if the individual is the financial respondent and 0 otherwise. We carry out this analysis separately for women and men. This can be interpreted as a test for selection of financial respondents by gender as a function of exposure to UDLs, allowing for gender-specific trends and fixed effects. The results (not reported to save space but available from the authors) suggest that there is no relationship between differential exposure to UDLs and the probability that a woman or a man is selected as the financial respondent. Second, we repeat our main analysis for savings at the household level including fixed effects and trends for the cohort of both members of the couple. Clearly, this can only be done for the balanced sample for whom information is available for both partners. The estimated UDL effect remains qualitatively unaltered with respect to our baseline, although it becomes slightly larger in magnitude.

4.3. Quantile treatment effects and heterogeneity by education

To investigate the heterogeneous effects across the distribution of household savings, we report in Table 8 the estimates of unconditional quantile treatment effects obtained by RIF regressions (Firpo et al., 2009) and the specification used in column 2 of Table 6. The effect of exposure to UDLs goes up from € 607 to € 4257 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 exposure to UDLs are larger for richer households. To reconcile our results with those obtained in the literature, we note that using US data, Voena (2015) also finds an increase in household assets in response to the introduction of UDLs in community property states. In particular, the author uncovers that the coefficients estimated using median regression are substantially smaller than those obtained from the OLS, thereby suggesting that

[26] The first three countries to introduce the UDLs are assigned the years of the three most recent countries and all other countries are switched back by three positions: Denmark is assigned year 1977, the Netherlands 1978, Belgium 1981, France 1970, Germany 1971, Austria 1975 and Spain 1976.

[27] To gain further credibility into this placebo, following Chetty et al. (2009), we have also estimated our model after carrying out 500 random permutations of years of exposure across the seven countries considered in the analysis. This provides an estimate of the empirical distribution of the effect under the null hypothesis of no effect. Our baseline estimate is well above the 95th percentile of this distribution.

Table 8
Unconditional quantile treatment effects of UDLs on financial wealth.

Dep. var.: financial wealth (€)	(1)
Quantile 10	
Exposure to UDLs	−134
	(206)
Quantile 25	
Exposure to UDLs	607**
	(294)
Quantile 50	
Exposure to UDLs	1493***
	(536)
Quantile 75	
Exposure to UDLs	4528***
	(1438)
Quantile 90	
Exposure to UDLs	6143**
	(2921)
Observations	2690
Covariates	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, country-specific quadratic cohort trends, and indicator variables for having a high school diploma, a college degree, several books at age 10, and good housing conditions at age 10. Household-level sample. Standard errors clustered by country and year of marriage are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9
Heterogeneous effects of UDLs on financial wealth by education. Median regressions.

Dep. var.: financial wealth (€)	Median	
	(1)	(2)
	High education	Low education
Exposure to UDLs	5817***	1125**
	(1756)	(487)
Observations	757	1933
Covariates	Yes	Yes
Median dep. var.	55,312	16,576

Notes: The table reports the heterogeneous effect of exposure to UDLs on median financial wealth by education, estimated via Recentered Influence Function (RIF) unconditional quantile regression. The model controls for year of marriage, country and cohort fixed effects, country-specific quadratic cohort trends, and indicator variables for having a high school diploma, a college degree, several books at age 10, and good housing conditions at age 10. High education is for household heads with tertiary degree, low education for secondary or below. Household-level sample. Standard errors clustered by country and year of marriage are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

richer households exhibit a greater response to the UDLs.[28] Importantly, this result confirms our findings that the effect of UDLs is particularly pronounced among more affluent households.[29]

In a similar fashion, in Table 9 we report the heterogeneous effects on median wealth by the level of education. We distinguish between households whose head has a tertiary degree and those with a secondary degree or lower. Results show that the effect of UDL exposure on wealth is much larger among the highly educated than among the low educated (€ 5817 vs. € 1125). This result is helpful for two reasons. First, education is a major determinant of late-life wealth, and therefore,

[28] Yet, differently from our study, Voena (2015) does not dig deeper into the heterogeneous impacts of UDLs across the distribution of household savings. We believe that our paper fills this gap, by analysing the effects to the UDL introduction at different points of the wealth distribution. We are not aware of other studies that conducted a similar analysis.

[29] To put the magnitude of our results into perspective, we used the publicly available data by Voena (2015), and calculated that in her analysis the increase in total assets is equal to about € 3500 per year of exposure to UDLs, compared to about € 5000 in our study. We believe that the larger magnitude of our estimated effect can be explained by two reasons. First, we consider a sample of older people, who are on average richer. Second, the individuals in our sample are exposed to UDLs for a much longer period of time (29.7 years in our case vs. about 17.58 years in Voena's, 2015 analysis). Given that the effects obtained by Voena (2015) increase with the years of introduction of unilateral divorce (see Panel A of Fig. 1, p. 2315), we would expect that these effects should become larger the longer people are exposed to UDLs.

Table 10
Effects of UDLs on total wealth. Median regressions.

Dep. var.: total wealth (€)	Median (1)
Exposure to UDLs	4988**
	(2423)
Observations	2690
Covariates	Yes
Median dep. var.	259,610

Notes: The table reports the effect of exposure to UDLs on median total wealth (the sum of real and financial wealth), estimated via Recentered Influence Function (RIF) unconditional quantile regression. The model controls for year of marriage, country and cohort fixed effects, country-specific quadratic cohort trends, and indicator variables for having a high school diploma, a college degree, several books at age 10, and good housing conditions at age 10. Household-level sample. Standard errors clustered by country and year of marriage are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

detecting heterogeneous effects by education may help to explain the differential effects uncovered at various points of the wealth distribution. Second, it is reasonable to assume that better educated people were more informed about the introduction of UDLs as well as more knowledgeable about their consequences, and therefore, exhibited a greater response to the change in the divorce regime.

4.4. Effects on total wealth

As a further extension, we also verify whether our main findings 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 effect for median wealth from a specification identical to the one in column 2 of Table 6 is displayed in Table 10, and is in line with the one reported for financial wealth (see Table 6), whereby longer exposure to UDLs increases household savings. The estimated effect is slightly smaller in relative terms with respect to the one for financial wealth only, as it ranges around 2–2.5% of the median total wealth in the sample. As reported in Table A.5 in the Appendix, even in this case we estimate larger effects at the top of the total wealth distribution. This suggests that our results are mostly driven by financial wealth, and are consistent with the idea that real assets represent the wealth component that is most difficult to change.

5. Potential mechanisms

What could be the mechanisms underlying our results? In what follows, we show that women in particular responded to the risk of unilateral divorce by increasing their labour supply, improving their numeracy, displaying a higher level of trust in others and dispositional optimism. By working, women earn a salary, which allows them to increase their saving potential. In addition, 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. Guiso et al. (2008) highlight how social capital and trust help financial development, and Jiang and Lim (2016) show that trust has a causal positive effect on household net worth, which is more pronounced for women than for men. Related to this literature, Puri and Robinson (2007) show that dispositional optimism, defined as having positive expectations about future events, is also related to savings: optimists save more. 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 exposure to UDLs increases labour supply, numeracy, trust and dispositional optimism is still indicative about the relevance of these potential mechanisms.[30]

We proceed in two steps. First, the availability of retrospective individual-level life histories data on labour supply by year allows us to estimate difference-in-differences models for the effects of UDL introduction on labour supply. These models allow us to enhance internal validity and provide tests for parallel trends before the introduction of the laws, to make sure that the effect we pick up is not due to rising trends in female labour supply but is a genuine treatment effect.

In this analysis, we consider the years between 1965 and 1980. We start in 1965, 5 years before the first introduction of UDLs in the countries of interest (in Denmark in 1970) to have a meaningful pre-treatment period in all countries, and stop in 1980 because from 1981 onwards all countries are in a UDL regime, and we do not have a control group anymore. To avoid issues related with selection into marriage, we only consider individuals in our sample that got married before 1970,

[30] We have also tested for potential fertility effects of UDLs as a mechanism, but we found no evidence that couples reacted to the UDL introduction by modifying their number of children.

Table 11
Effects of UDLs on labour supply.

Dep. var.: working in year t	Females (1)	Males (2)	Females (3)	Males (4)	Females (5)	Males (6)
After UDL	0.014*	0.001	0.019**	−0.002	0.004	−0.003
	(0.008)	(0.003)	(0.008)	(0.003)	(0.008)	(0.004)
2 years before UDL			−0.009	−0.001		
			(0.047)	(0.020)		
3 years before UDL			−0.009	0.005		
			(0.042)	(0.018)		
4 years before UDL			−0.002	0.008		
			(0.037)	(0.015)		
5 years before UDL			−0.009	0.011		
			(0.032)	(0.013)		
6 years before UDL			0.007	0.009		
			(0.026)	(0.010)		
7 years before UDL			0.014	0.007		
			(0.021)	(0.008)		
8 years before UDL			0.016	−0.006		
			(0.017)	(0.006)		
9 years before UDL			0.004	0.002		
			(0.012)	(0.004)		
10+ years before UDL			0.001	−0.000		
			(0.007)	(0.002)		
Observations	31,124	31,193	31,124	31,193	31,124	31,193
Individuals	1947	1950	1947	1950	1947	1950
Mean outcome 1965–69	0.433	0.942	0.433	0.942	0.433	0.942
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific quadratic trends	Yes	Yes	Yes	Yes	Yes	Yes
UDL introduction order	Original	Original	Original	Original	Placebo	Placebo

Notes: The table reports the effects of being in a UDL regime on labour supply. The dependent variable is a dummy for whether individual i is employed in year t. Individual-by-year level observations for years between 1965 and 1980 and individuals married up to 1969, the year before the first UDL introduction. Columns (3) and (4) include lagged effects for 2, 3,..., 10+ years before UDL introduction, omitting the year just before as the reference category. Columns (5) and (6) repeat the analysis in columns (1) and (2) after switching the order of introduction of UDLs across countries. Standard errors clustered by individual are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the first year of UDL introduction. This leads us to select 62,317 individual-year observations for 3897 individuals (31,124 observations for 1947 women and 31,193 observations for 1950 men). We then estimate the following OLS difference-in-differences regression:

$$Working_{ict} = \beta UDL_{ct} + \alpha_i + \gamma_t + \delta_c^1 t + \delta_c^2 t^2 + \varepsilon_{ict} \tag{2}$$

where the dependent variable, $Working_{ict}$, is a dummy for whether the individual i living in country c is working in year t. The main regressor, UDL_{ct}, is an indicator for whether a UDL is in place in year t in country c. α_i, γ_t and $\delta_c^1 t + \delta_c^2 t^2$ are, respectively, individual and time fixed effects, and country specific quadratic time trends. Standard errors are clustered by individual.

The parameter of interest, β, measures the impact of being in a UDL regime on the probability of being at work and is identified by comparing changes in working status over time of individuals who reside in countries with and without a UDL regime and exploiting the scattered introduction of UDLs for identification.

This framework allows us to carry out a test for parallel trends in an event study framework, that is, by introducing lags for introduction of UDLs in 2, 3, 4,..., years (omitting the year before the introduction as the reference category). If these terms were significant, the parallel trend assumption would fail. Similarly, in this framework we can also use placebo tests to further verify that the effects we are estimating are not due to the rising trends in female labour supply.

To analyse the potential gender differences in the reaction to increased unilateral divorce risk, we perform the analysis after splitting the sample by gender. The results are reported in Table 11. In particular, in column (1) we find that being in a UDL regime increases the probability that a woman is at work by 1.4 percentage points, while column (2) reports no effect for males. The effect for women is equivalent to a 3.3% increase with respect to the pre-UDL introduction mean employment in 1965–1969.[31] Columns (3) and (4) introduce lagged effects for 2, 3,..., 10+ years before the UDL introduction, omitting the year just before as the reference category. The main policy effect is still positive and significant for women, and of comparable magnitude, while the lags are never significant. Finally, columns (5) and (6) repeat the analysis in columns

[31] A potential concern about this analysis stems from the fact that in Germany, the UDL introduction coincided with a law change that abolished the husband's control over his wife's labour force participation. Similarly, in Belgium the UDL introduction also made it possible for the spouse of a mentally ill person to obtain a divorce after ten years of separation. To verify that these country-specific concurring policies are not driving our findings, we have replicated our analysis in Table 11 after dropping Germany and Belgium. Our results are wholly unchanged.

Table 12
Effects of UDLs on numeracy, trust, and dispositional optimism.

Dep. var.:	High numeracy		Trust in others		Dispositional optimism	
	Females (1)	Males (2)	Females (3)	Males (4)	Females (5)	Males (6)
Exposure to UDLs (original)	0.009*	0.006	0.010*	0.005	0.005*	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Exposure to UDLs (placebo)	0.006	0.007	0.006	0.005	0.004	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Observations	2497	2462	2471	2419	2333	2298
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.486	0.630	0.565	0.566	−0.103	0.028

Notes: The table reports the effects of exposure to UDLs on numeracy, trust and optimism. Numeracy and trust are indicator variables for having a score above the median. Dispositional optimism is equal to the difference between the self-assessed probability of survival and that obtained from the actuarial life tables by gender, cohort and country. Mean effects estimated via OLS regressions. All models control for year of marriage, country and cohort fixed effects, country-specific quadratic cohort trends, and indicator variables for having a high school diploma, a college degree, several books at age 10, and good housing conditions at age 10. Individual-level sample. We drop from the individual-level analysis individuals for whom missing values in the dependent variables are present. Standard errors clustered by country and year of marriage are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

(1) and (2) after switching the order of introduction of UDLs across countries. As expected, we find no significant effects for females or males.

Second, as information on the other potential mechanisms - numeracy, trust and dispositional optimism – is only available at the time of the SHARE interview, for this analysis we estimate the same specification as in Eq. (1), with the main difference being that the unit of observation i is now an individual in her first marriage (instead of a couple). For numeracy and trust, we construct dummies for having a score above the median, while we measure dispositional optimism as the difference between self-assessed survival probabilities and that obtained from actuarial life tables by gender, country and year of birth. Even in this case, we carry out the analysis after splitting the sample by gender. In each regression, we drop individuals with missing information on the respective outcome.

We report the results of this analysis in the upper panel of Table 12, while in the bottom panel we show the results of placebo tests for each of our measures. Columns 1 and 2 report the OLS estimates of the effect of exposure to UDLs on numeracy. We find that the coefficient on exposure to UDLs is statistically significant at the 10% level for women but not for men. An additional year of exposure to UDLs implies an increase in the probability of having high numeracy by 0.9 percentage points for women. Columns (3) and (4) report qualitatively and quantitatively similar results for trust in other, a well-known determinant of participation in financial markets and savings. In this case, an additional year of exposure to UDLs increases women's trust by 1.0 percentage point with respect to the mean. Finally, columns (5) and (6) show that exposure to UDLs significantly increases dispositional optimism, again only for women.

Overall, these results lend support to the precautionary motive for saving, suggesting that – especially for women – exposure to unilateral divorce risk leads to the adoption of self-insuring behaviours – such as increasing labour supply and investment in numeracy – and enhances levels of dispositional optimism and trust, which are key determinants of savings. Instead, an increase in the bargaining power of the woman seems less likely to be an explanation for our results because this would imply a decrease in female labour supply, as shown by Voena (2015).

A concern related to the effect on female labour supply is that our finding on savings could be simply due to the mechanical effect of higher labour supply on income, without altering households' propensity to save. To dispel this concern, we consider as our dependent variable the asset over income ratio, defined as financial wealth divided by total current net household income, and re-estimate our model using OLS and including the same covariates as in column 2 of Table 6. Reassuringly, the effect reported in Table A.6 in the Appendix is consistent with our main results (see Table 6) and continues to show a positive effect of UDL exposure on savings.

6. Conclusion

In this paper, we use European data on married couples around retirement age to analyse the long-term impact of exposure to UDLs on household wealth accumulation. Our results show that households accumulate more savings following the adoption of UDLs. According to our estimates, an additional year of exposure to UDLs increases median household savings by € 1493, 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 potential mechanisms underlying the relationship between the risk of unilateral divorce and household savings, we demonstrate that women in particular increase labour supply, numeracy, trust in others and optimism. These mechanisms have been identified by the literature as being positively associated with savings. Our findings lend support to the precautionary motive for saving, in which wives self-insure against the risk of a negative shock associated with divorce.

We believe it is less likely that a change in bargaining power between husbands and wives constitutes a mechanism behind our findings, as this would imply a decrease in female labour supply, as shown by Voena (2015).

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 the only resource to finance consumption and insure against negative shocks during retirement. In addition, 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 at the top of the wealth distribution, we believe that this effect is of second order, as it is unlikely that it led couples to reduce consumption below a minimum acceptable level.

Acknowledgements

We are indebted to the Editor, an Associate Editor, and two Reviewers for their comments on the paper. We thank Rob Alessie, Axel Börsch-Supan, Giorgio Brunello, Tabea Bucher-Koenen, Chiara Dal Bianco, Giulio Fella, Antonia Grohmann, Thorsten Kneip, Elena Lucchese, Francesca Marino, Alessandro Martinello, Gianluca Mazzarella, Claudia Olivetti, Daniele Paserman, Lorenzo Rocco, Mariacristina Rossi, Eduard Suari Andreu and Guglielmo Weber for helpful discussions. We also thank participants at the ESPE conference in Berlin, the IZA Workshop on Gender and Family Economics in Bonn, the EALE conference in Ghent, the SIEP conference in Padova, the Workshop on Household Finance and Economic Behaviour in Turin, the Luxembourg SHARE user conference and at seminars at DIW Berlin, the University of Cagliari, the Munich Center for the Economics of Ageing (MEA) and the University of Siena for comments and suggestions. This paper uses data from SHARE Waves 2 and 3 (SHARELIFE) (DOIs: 10.6103/SHARE.w2.500, 10.6103/SHARE.w3.500), see Börsch-Supan et al. (2013) (2017) for methodological details. The SHARE data collection has been primarily funded by the European Commission through FP5 (QLK6-CT-2001-00360), FP6 (SHARE-I3: RII-CT-2006-062193, COMPARE: CIT5-CT-2005-028857, SHARELIFE: CIT4-CT-2006-028812) and FP7 (SHARE-PREP:No 211909, SHARE-LEAP:No 227822, SHARE M4: No 261982). Additional funding from the German Ministry of Education and Research, the U.S. National Institute on Aging, (U01_AG09740-13S2, P01_AG005842, P01_AG08291, P30_AG12815, R21_AG025169, Y1-AG-4553-01, IAG_BSR06-11, OGHA_04-064) and from various national funding sources is gratefully acknowledged (see www.share-project.org). Marco Bertoni acknowledges funding from a CARIPARO foundation “Starting Grant” BERT_START16_01. The view expressed in this paper are those of the authors and do not necessarily reflect those of the European Investment Bank.

Declarations of interest

None.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.euroecorev.2019.07.002.

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