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Grandparenthood Related to Reduced Risk of Gray Divorce: Evidence From 15 Countries

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Abstract

Gray divorce (marital dissolution after age 50) has risen across most countries, yet relatively little is known about how grandparenthood may stabilize marriages in later life. Drawing on harmonized Generations and Gender Survey data from 15 countries spanning Europe, East Asia, and Latin America (N=30,187 parents still married at age 50), we examine whether the transition to grandparenthood is associated with a lower risk of marital dissolution. Using cause-specific Cox proportional hazards models with grandparent status specified as a time-varying covariate and widowhood treated as a competing risk, we find that grandparents face a substantially lower hazard of marital dissolution than those who have not yet become grandparents (adjusted HR=0.81, 95% CI 0.71-0.94). This protective association persists net of birth cohort and marriage duration and is broadly consistent across the 15 national contexts examined, though with substantial cross-national variation. The elevated raw hazard of widowhood among grandparents likely reflects cohort composition rather than a direct grandparenthood effect. The protective association is directionally consistent in 13 of the 15 national contexts examined, and is strongest in contexts where filial norms and divorce stigma are more pronounced, notably in Taiwan and several Eastern European countries.

Introduction and Background

Gray divorce, usually defined as divorce after age 50, has become an increasingly visible form of family change in later life. In the United States, the gray divorce rate doubled between 1990 and 2010, and later evidence indicates that this trend did not simply disappear in the period that followed. Although the increase in the divorce rate leveled off among adults aged 50 to 64, it continued to rise among adults aged 65 and older (Brown and Lin 2012, 2022). More broadly, later-life family change is no longer captured mainly by widowhood, as divorce, cohabitation, and re-partnering have become more common features of older adults' family lives (Brown and Wright 2017; Wang 2026).

Research has identified some of the main correlates of gray divorce, but while literature on divorce antecedents has explored numerous risk and protective factors for divorce in general (Lyngstad and Jalovaara 2010), much less is known about family transitions that tend to stabilize marriages in later life in particular. Longitudinal evidence from the United States shows that marital duration, marital quality, home ownership, and wealth are negatively associated with gray divorce, whereas later-life turning points, such as reaching the empty nest stage when children have left the parental home, transitioning to retirement, and experiencing poor health, are generally not associated with divorce risk once other factors are taken into account (Lin et al. 2018). This leaves open the question of whether other later-life family transitions matter for marital stability.

Grandparenthood as protective of marriage stability

One such transition is that of grandparenthood. Brown et al. (2021) argue that the arrival of a first grandchild may exert a "braking effect" on gray divorce, analogous to the stabilizing role that the transition to parenthood can play earlier in the life course. They suggest that entering grandparenthood is an important midlife transition that may improve marital satisfaction by creating a new shared point of orientation within the family. This could strengthen intergenerational ties and counteract typical challenges that often lead to gray divorce.

A related argument appears in the European literature on late union dissolution. Using SHARE data, Alderotti et al. (2022) suggest that grandchildren may "refill the nest" after a couple's children have left home, and thus that the arrival of grandchildren may inhibit gray divorce as the grandparents assume new familial and social responsibilities. Their results show that having

grandchildren is associated with a lower probability of late union dissolution in Europe, and that this association is even stronger when respondents also report frequent contact with their children (Alderotti et al. 2022).

This emphasis on intergenerational ties is consistent with earlier work treating grandparenthood not simply as an individual characteristic, but also as a relationship embedded in a wider kinship system. Matthews and Sprey (1984), in an exploratory study of grandparenthood after divorce, argue that grandparent-grandchild ties are shaped by relationships in the middle generation, and should therefore be understood within broader family structures, rather than as isolated individual roles. Although their study addresses how divorce in the middle generation shapes grandparenthood, rather than how grandparenthood shapes marital dynamics, it underlines the broader point that grandparenthood and marital dynamics are linked through intergenerational family processes.

Some support for the relevance of grandparenthood also comes from the well-being literature. Using European panel data, Sheppard and Monden (2019), as well as Tanskanen et al. (2019), find that the transition to first-time grandparenthood is associated with reduced depressive symptoms among women (Sheppard and Monden 2019), although the authors find no evidence for effects on life satisfaction. Tanskanen et al. (2019) also report improvements in life satisfaction and quality of life among grandmothers. Di Gessa et al. (2020) similarly show that positive associations are concentrated among women who become first-time grandmothers through their daughters and are strongest close to the transition itself, rather than appearing broadly across all grandparents. While these studies do not examine divorce directly, they suggest that the transition to grandparenthood can influence later-life well-being, which may be one pathway linking grandparenthood to marital stability.

Mechanisms

Several psychosocial and structural mechanisms link grandparenthood to reduced dissolution risk. Drawing on Erikson's (1950) framework of generativity, understood as the adult developmental concern for establishing and guiding the next generation, the transition to grandparenthood may reinvest the couple relationship with a renewed sense of shared purpose precisely at a life stage when post-childrearing couples frequently experience role contraction and reduced common activity. Life course theory similarly emphasizes that transitions in one family member's biography

can restructure the relational landscape of others through linked lives (Carr 2018; Elder 1998). In this case, the birth of a grandchild may encourage the couple to act as a cooperative unit engaged in intergenerational support, childcare coordination, and family rituals. This renewal of role coupling may counteract the relational drift that accumulates during the empty nest phase, a transition that has otherwise been found to have limited direct effects on gray divorce risk (Lin et al. 2018).

A second mechanism concerns the social and emotional costs of dissolution once grandchildren are present. Divorce disrupts the multigenerational family structure in ways that affect grandchildren's access to grandparental care and relationship quality. King (2003) has documented downstream negative effects of grandparental divorce on grandchild-grandparent ties, a finding that may be intuitively apprehended by grandparents and function as a deterrent. This normative mechanism is consistent with the null finding for step-grandparenthood in the U.S. analysis by Brown et al. (2021) and with the variation in effect size reported by Žilinčíková and Kreidl (2018) across institutional contexts, which suggests that when biological ties are weaker or normative family obligations are less enforceable, the deterrent force of grandchildren's presence attenuates.

Third, the psychological well-being benefits associated with grandparenthood may buffer marital dissatisfaction by elevating the overall quality of daily life. Sheppard and Monden (2019) using a fixed-effects design, Tanskanen et al. (2019) using longitudinal European data, and Hou (2025) using Korean longitudinal data all find that the transition to grandparenthood produces positive subjective well-being effects, at least in the period immediately following the transition. If grandparenthood increases the experienced valence of daily life, it may raise the threshold at which accumulated marital dissatisfaction motivates couples to divorce. Finally, active grandparenting is time-intensive; the logistical demands of childcare, transportation, and multigenerational coordination may reduce the cognitive and practical bandwidth available for initiating separation, a mechanism that may be expected to attenuate as grandchildren age into greater independence.

Current state of research

The study that is most similar to the present paper was conducted by Brown et al. (2021). Using the Health and Retirement Study data from the United States, they show that becoming a biological grandparent is associated with a substantially lower risk of gray divorce, whereas becoming a step-grandparent is not. This is the clearest direct test of the braking hypothesis to date.

At the same time, that study leaves important questions open. As it is limited to one national context, it cannot assess whether this association is generalizable across country contexts. Thus, it also does not address cross-national variation in either the magnitude or the timing of the association. Another related European study, by Alderotti et al. (2022), shows that grandchildren are negatively associated with late union dissolution, but it does not examine the transition to grandparenthood itself. Moreover, the authors do not distinguish between biological and step-grandchildren, and they do not perform country-specific analyses because the number of gray divorces is too small within individual countries (Alderotti et al. 2022).

Country context

A cross-national perspective is important because both the timing of grandparenthood and the prevalence and meaning of divorce differ across countries. Countries vary in terms of fertility timing, family formation patterns, intergenerational contact, and the broader social acceptability and institutional context of divorce (Di Gessa et al. 2016; Härkönen and Dronkers 2006; Sieben and Verbakel 2013). These differences may shape both whether people become grandparents during the period of gray divorce risk, and how strongly grandparenthood is linked to marital stability.

Alderotti et al. (2022) observe considerable variation in late union dissolution across European countries, even though country-specific analysis is not possible with their data. Their results also suggest that the association between grandchildren and late union dissolution is stronger when intergenerational contact is more frequent, which implies that the protective mechanism operates partly through active grandparental involvement, rather than through grandparent status alone.

Related evidence on post-divorce grandparenting points in the same direction. Using SHARE data from 18 European countries, Žilinčíková and Kreidl (2018) show that divorced grandparents are less likely than married grandparents to provide childcare, and that this association varies across countries. Although their focus is on the consequences of divorce for grandparental childcare rather than on divorce risk itself, their results indicate that the links between divorce and intergenerational involvement are context-dependent.

This comparative perspective is central to the present study. Using harmonized Generations and Gender Survey (GGS) data, we examine 15 countries that we group into four broad regional contexts: Western/Northern Europe (Belgium, France, Germany, Sweden), Eastern Europe and former

Soviet states (Belarus, Czech Republic, Estonia, Hungary, Kazakhstan, Lithuania, Moldova, Russia), East Asia (Taiwan), and Latin America (Argentina, Uruguay).

The present study

This paper builds on the existing literature by using harmonized GGS panel data from 15 countries to examine whether the risk of marital dissolution, operationalized as divorce or permanent separation, differs between those who have and have not yet become grandparents. In doing so, it extends the prior literature in three ways. First, it moves beyond single-country research to test whether the protective grandparenthood-divorce association identified by Brown et al. (2021) is generalizable across 15 countries spanning four world regions. Second, it models grandparent status as a time-varying covariate, which eliminates the immortal time bias that arises when grandparent status is treated as a fixed characteristic. Specifically, if respondents are classified as grandparents from the outset based on whether they ever become grandparents during the observation period, they must by definition remain married long enough to experience that transition, which can mechanically make grandparenthood appear protective even in the absence of a causal effect. This study also tests whether any association holds net of cohort composition and marriage duration, distinguishing it from the existing European evidence reported in Alderotti et al. (2022). Third, it explicitly treats widowhood as a competing risk, assessing whether an apparent protective association between grandparenthood and divorce could partly reflect differential mortality selection rather than marital dynamics.

Data and method

Data

We draw on the Generations and Gender Survey (GGS), a multi-country survey that combines cross-sectional baseline interviews with retrospective life-history calendars and is especially focused on family forms and transitions. The retrospective event-history component (harmonized across countries in the GGS Harmonized Histories) records month-precise dates of key life events including marriages, separations, divorces, and childbearing, enabling the reconstruction of continuous marital exposure histories despite the surveys being administered at a single point in time. We draw on GGS Rounds I and II, which together cover 20 countries across Eastern and Western Europe, East Asia, and Latin America.

Several countries are excluded from the analysis. Norway and Italy both participated in GGS Round I, but are absent from the harmonized event-history files used for this study and thus cannot be included. Three further countries are excluded on empirical grounds identified after an initial model run. Georgia contributes only seven post-50 dissolution events, yielding entirely uninformative estimates. Poland produces a model that fails to converge due to empty event cells within birth cohort strata. Bulgaria contributes 27 events and a point estimate that reverses direction relative to the pooled results, but with a confidence interval so wide that any substantive interpretation is precluded. The pooled results are robust to the inclusion or exclusion of these three countries. The final analytical sample covers 15 countries: Argentina, Belarus, Belgium, Czech Republic, Estonia, France, Germany, Hungary, Kazakhstan, Lithuania, Moldova, Russia, Sweden, Taiwan, and Uruguay (see Figure A1 in the Online Appendix for the full sample construction flowchart).

We restrict the sample to respondents in their first marriage who had at least one child before age 50. The focus on first marriages ensures cross-national comparability, as higher-order unions follow qualitatively different dissolution dynamics. The restriction to parents defines the population at risk of becoming grandparents, the exposure of central interest. We further require that respondents were married before age 50, remained married, were observed at or after age 50, and had not experienced marital dissolution before reaching that age. The observation window begins at age 50 and is capped at 20 years of follow-up. Respondents still married at the end of the follow-up period or at the survey interview date are censored.

Measures

Outcome

The outcome is marital dissolution after age 50, defined as the first recorded occurrence of either a legal divorce or a permanent separation, whichever comes first. This joint operationalization reflects the reality that in many countries, particularly across Eastern Europe, permanent separation without formal divorce is common and is functionally equivalent to marital breakdown. Widowhood is treated as a competing risk: respondents whose spouse dies during the follow-up window are censored for the divorce outcome at the date of spousal death, and a separate cause-specific model for widowhood is estimated to assess whether grandparenthood is associated with differential mortality exposure that could confound the primary result.

Grandparenthood

The central predictor is grandparent status, operationalized in two complementary ways corresponding to the two analytical stages. For the descriptive survival analysis, grandparent status is treated as a fixed characteristic: respondents are classified as grandparents if they become grandparents at any point during the 20-year observation window, and as non-grandparents otherwise. This classification allows for a straightforward comparison of the overall dissolution trajectories of people who do and do not experience the transition to grandparenthood during later life. For the Cox regression models, grandparent status is instead specified as a time-varying covariate that switches from zero to one at the month of the first grandchild's arrival, derived from the retrospective grandchild birth date information in the GGS country files. In cases in which the birth year is recorded but the calendar month is missing, the month is drawn at random from a uniform distribution to preserve the timing structure of the data. To assess whether this assumption affects the results, we have re-estimated the main model under two deterministic alternatives (January imputation and mid-year imputation), and find that the hazard ratios are highly similar across all three strategies, ranging from 0.815 to 0.821 (Table A2, Online Appendix).

An important measurement consideration concerns the identification of non-grandparents in several GGS Round I countries. The grandchild questions in the Round I questionnaire were administered only to respondents who had already confirmed that they have grandchildren, which means that a missing value on the grandchild count does not indicate unknown status, but rather that the question was not reached, effectively resulting in a confirmed response of zero grandchildren. This is corroborated empirically: in countries such as Belgium and Russia, the large majority of respondents carry a missing value on this variable, yet among those with a recorded response, virtually all are grandparents. Non-grandparents in these countries are accordingly classified as such rather than treated as cases of unknown status.

Covariates

We control for birth cohort, grouped into four approximately decade-wide bands (born before 1935, 1935-1944, 1945-1954, and 1955 or later), with the earliest cohort serving as the reference. The earliest cohort (born before 1935) includes a small share of respondents born before 1925 ($n=50$, 0.2%); the most recent cohort (born 1955 or later) includes respondents born as late as 1974, with 8.3% of the analytical sample ($n=2,514$) born after 1965. The full birth year distribution

is shown in Figure A8 of the Online Appendix. Cohort control is essential because older cohorts have structurally lower divorce rates and longer post-50 observation windows, and failing to account for this compositional variation would conflate cohort trends with the grandparenthood effect. Figure A5 of the Online Appendix shows gray divorce rates, grandparenthood prevalence, and mean age at the birth of the first grandchild by birth cohort, based on a restricted sample with a common 15-year observation window to ensure comparability across cohorts and covering the four birth cohort bands listed above (pre-1935 through 1955+). We additionally control for marriage duration at age 50 in years, reflecting the well-documented negative duration dependence in marital dissolution risk: couples who have remained married longer at age 50 face a lower baseline hazard of dissolution in the years that follow.

Analytical approach

We proceed in two steps.¹ First, we describe the raw association between grandparent status and later-life marital survival using Kaplan-Meier survival curves presented as country-specific panels. The Kaplan-Meier estimator treats widowhood as a censoring event rather than as a competing risk, and therefore slightly overestimates the absolute probability of dissolution; it is used here for descriptive purposes to characterize the gross difference in dissolution trajectories between grandparents and non-grandparents.

Second, we estimate cause-specific Cox proportional hazards models to assess the association between grandparenthood and the hazard of dissolution net of compositional differences. Grandparent status enters these models as a time-varying covariate specified in a counting process format, such that each respondent contributes separate risk intervals before and after the first grandchild's birth. This design eliminates immortal time bias, which occurs when respondents are treated as grandparents before they have actually become grandparents. In that case, people who eventually become grandparents must by definition remain married long enough to experience their grandchild's birth, so part of their pre-transition married time would be incorrectly attributed to the grandparent category. This would make grandparents appear less likely to dissolve their marriage even if grandparenthood itself had no protective association. Such misclassification would mechanically suppress the estimated dissolution hazard among grandparents relative to non-

¹ Replication code and output are available at: https://osf.io/3nb5z/overview?view_only=c46f034c8e2c4fd9a0a5e74c0e5842c6

grandparents by concentrating their exposure time in periods of marital stability that precede the grandchild's birth.

The baseline hazard is stratified by country, allowing it to vary freely across national contexts, while the grandparenthood coefficient is estimated as a common pooled effect. Robust standard errors are obtained using the sandwich variance estimator to account for the multi-episode structure of the data. We estimate both an unadjusted model retaining only grandparent status and an adjusted model adding birth cohort and marriage duration at age 50. A parallel set of models is estimated for the competing risk of widowhood to assess whether the effect of grandparenthood on divorce could be driven by differential mortality selection.

To examine cross-national heterogeneity in the grandparenthood effect, we additionally estimate country-specific Cox models under both the unadjusted and the adjusted specifications, and present the resulting hazard ratios alongside 95% confidence intervals. Because sparse event cells within birth cohort strata can cause non-convergence or extreme estimates in small national samples, country-specific results should be interpreted with appropriate caution regarding their precision.

Results

Table 1 presents descriptive characteristics of the analytical sample by grandparent status. The sample comprises 30,187 individuals (parents still in their first marriage at age 50), observed across 15 countries for up to 20 years following the age 50 entry point. Of these, 20,980 (69.9%) became grandparents at some point during the observation window, while 9,207 (30.1%) did not.

The two groups differ systematically in their demographic profiles, largely reflecting the structural prerequisites for grandparenthood. The grandparents were born, on average, in 1947 (SD=10.5), approximately seven years earlier than those without grandchildren (mean birth year 1955, SD=11.0). Birth years range from 1920 to 1974. Consistent with their earlier birth cohorts, the grandparents entered marriage at younger ages (mean 23.5 years, SD=4.3, versus 27.1 years, SD=5.8) and became parents earlier (mean age at first birth 24.5 versus 28.9 years). By age 50, the grandparents had been married for an average of 26.5 years (SD=4.3), compared to 22.9 years (SD=5.8) for those without grandchildren. Among the grandparents, just over half (53.8%) had already become grandparent by the age 50 entry point; the mean age at the birth of the first

grandchild was 49.9 years (SD=7.0), which indicates that the transition to grandparenthood frequently occurred close to the study entry age. The remaining grandparents in the sample made the transition during the post-50 follow-up period, and therefore contribute time-varying exposure in the event-history analyses.

Follow-up durations differed substantially between groups: grandparents accumulated an average of 154.4 months (SD=74.0) of observation after age 50, compared to 89.6 months (SD=71.2) for those without grandchildren. This difference reflects the older age composition of the grandparent group: older respondents were surveyed later relative to their age 50 entry point, resulting in more available follow-up time.

A total of 1,075 marriages dissolved through divorce or separation during the observation period. The crude event rate was markedly higher among those without grandchildren (6.2%; n=574) than among grandparents (2.4%; n=501). Widowhood, which is treated as a competing event in the survival analyses, occurred more frequently among grandparents (15.2%; n=3,193) than among those without grandchildren (8.8%; n=812), consistent with the older age composition of the grandparent group.

Table 1: Sample characteristics by grandparent status

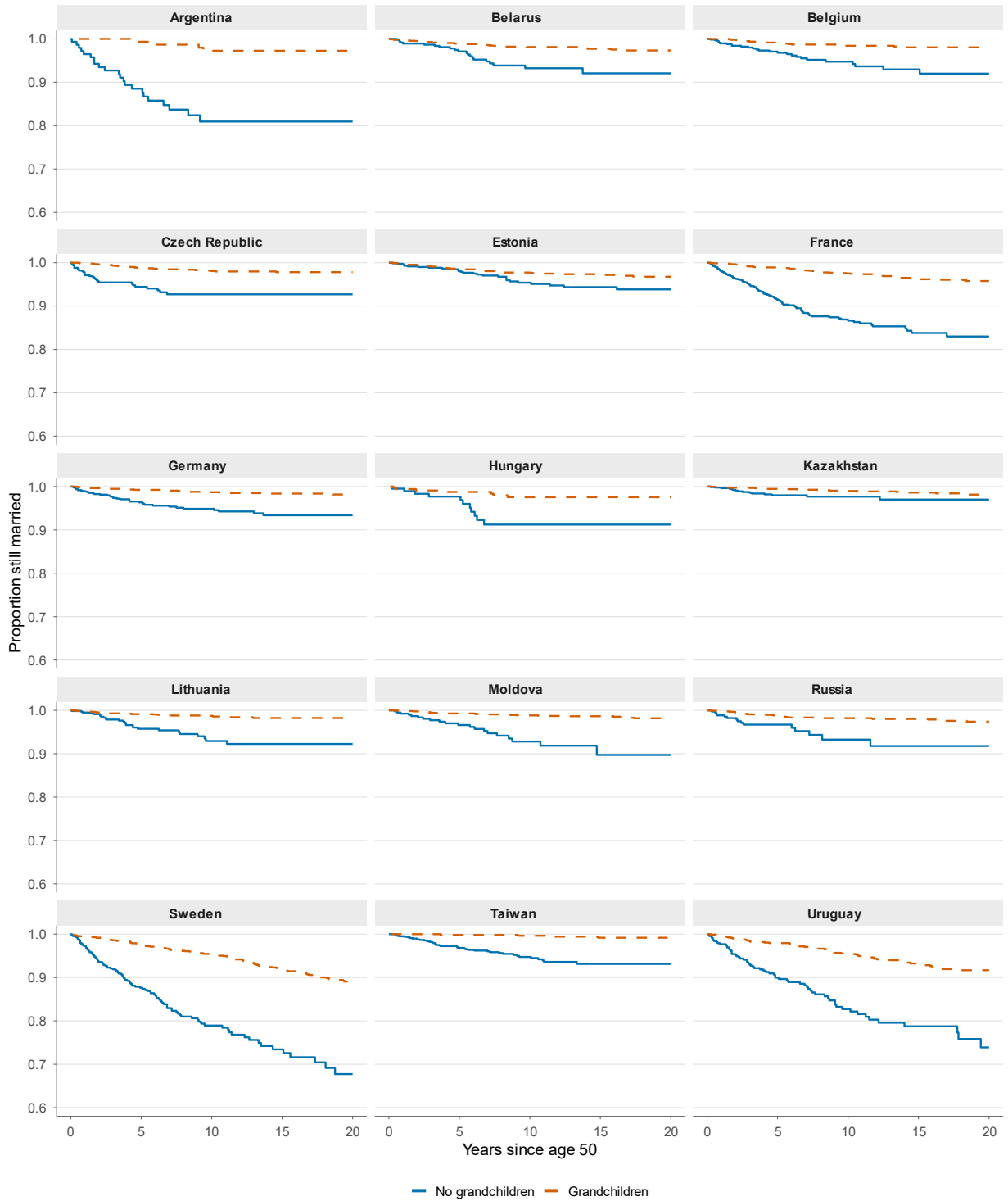
Characteristic	Parents, no grandchildren (n = 9,207)	Grandparents (n = 20,980)	Total (n = 30,187)
Birth cohort (%)			
pre-1935	5.6%	13.2%	10.9%
1935–1944	11.8%	25.4%	21.2%
1945–1954	33.2%	35%	34.4%
1955+	49.5%	26.4%	33.5%
Birth year, mean (SD)	1954.5 (11)	1947.4 (10.5)	1949.6 (11.2)
Age at marriage (years), mean (SD)	27.1 (5.8)	23.5 (4.3)	24.6 (5.1)
Marriage duration at age 50 (years), mean (SD)	22.9 (5.8)	26.5 (4.3)	25.4 (5.1)
Age at first child (years), mean (SD)	28.9 (5.6)	24.5 (4.1)	25.8 (5)
Already grandparent at age 50 (% among grandparents)	—	53.8%	—
Age at first grandchild (years), mean (SD)	—	49.9 (7)	—
Post-50 follow-up (months), mean (SD)	89.6 (71.2)	154.4 (74)	134.6 (79)
Events during follow-up			
Divorce or separation, N (%)	574 (6.2%)	501 (2.4%)	1,075 (3.6%)
Widowhood, N (%)	812 (8.8%)	3,193 (15.2%)	4,005 (13.3%)

Characteristic	Parents, no grandchil- dren (n = 9,207)	Grandparents (n = 20,980)	Total (n = 30,187)
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Note. Sample restricted to parents who had a child before age 50, 15 countries, observation window 20 years after age 50. Grandparent status is based on whether the respondent becomes a grandparent at any point during the observation window. Mean (SD) for continuous variables; % for categorical variables.

Figure 1 displays Kaplan-Meier survival curves for time to divorce or separation after age 50, estimated separately by grandparent status in each of the 15 countries. Grandparenthood prevalence varies markedly across countries, from 35% in Taiwan to 88% in Moldova (see Figure A4 of the Online Appendix; the figure refers to the 20-year post-50 observation window, i.e., ages 50–70, and counts respondents who either were already grandparents at the age 50 entry point or became grandparents during the follow-up period; of those ultimately classified as grandparents, 53.8% had already made the transition before age 50 (Table 1)). The pooled survival curves, combining all 15 countries, are presented in Figure A2 of the Online Appendix. A consistent pattern emerges across the great majority of national contexts: grandparents exhibit higher marital survival than those without grandchildren throughout the 20-year observation window. The divergence between the two groups is most pronounced in Taiwan and in several Eastern European countries, including Lithuania, the Czech Republic, and Moldova, where the gap between survival curves widens progressively over the follow-up period. In a smaller number of countries, most notably Kazakhstan, the curves are more closely aligned or partially overlapping, suggesting a weaker or absent protective association in that setting. On the whole, the cross-national consistency of the pattern supports the interpretation that the relationship between grandparenthood and marital stability is not an artifact of any particular national context, even as its magnitude varies.

Kaplan-Meier survival after age 50 (15 countries)



Note: Kaplan-Meier estimates. Sample: parents (first child born before age 50) still married at age 50; follow-up capped at 20 years. Grandparent status is time-varying: respondents enter the "Grandchildren" group at first grandchild's birth. Shaded bands are 95% confidence intervals. Widowhood is treated as censoring; the competing risk of spouse death is not modelled

Figure 1: Kaplan-Meier survival curves by country

Table 2 presents cause-specific Cox regression results from models pooled across all 15 countries, with grandparent status entered as a time-varying covariate. In the unadjusted model, grandparents face a roughly 28% lower instantaneous divorce hazard than those without grandchildren (HR=0.72, 95% CI: 0.63-0.83). After adjustment for birth cohort and marriage duration at age 50, the association attenuates modestly, but remains statistically significant and substantively meaningful (HR=0.81, 95% CI: 0.71-0.94): becoming a grandparent is associated with a reduction in the hazard of gray divorce of approximately 19%, net of the confounding introduced by cohort membership and marital duration.

The birth cohort coefficients reveal a pronounced secular increase in gray divorce rates. Relative to the pre-1935 cohort, the adjusted divorce hazard is 34% higher among those born in 1935-1944 (HR=1.34, 95% CI: 1.04-1.72), 55% higher among those born in 1945-1954 (HR=1.55, 95% CI: 1.19-2.01), and more than twice as high among those born in 1955 or later (HR=2.26, 95% CI: 1.67-3.07). This strong positive gradient is consistent with the well-documented historical rise in divorce across all countries in the sample over the second half of the 20th century. Marriage duration at age 50 is additionally and inversely associated with subsequent divorce hazard (HR=0.98, 95% CI: 0.96-0.99): each additional year of marital duration at entry is associated with a modest but reliable reduction in subsequent dissolution risk, consistent with habituation processes and negative duration dependence in late-life divorce rates.

Table 2 also reports models for the competing event of widowhood. The unadjusted widowhood hazard is substantially elevated among grandparents relative to those without grandchildren (HR=1.34, 95% CI: 1.23-1.45), a difference that narrows considerably after adjustment for birth cohort and marital duration (HR=1.11, 95% CI: 1.02-1.21). The residual adjusted elevation likely reflects age composition differences not fully absorbed by the categorical cohort indicators: older individuals within the grandparent group face higher spousal death rates that cohort controls alone cannot eliminate entirely.

Table 2: Pooled cause-specific Cox models

	Divorce or separation		Widowhood	
	Unadjusted	Adjusted	Unadjusted	Adjusted
Grandparent (ref.=no grandparent)	0.72 [0.63-0.83]***	0.81 [0.71-0.94]**	1.34 [1.23-1.45]***	1.11 [1.02-1.21]*

	Divorce or separation		Widowhood	
	Unadjusted	Adjusted	Unadjusted	Adjusted
Birth cohort (ref.=pre-1935)				
1935-1944		1.34 [1.04-1.72]*		1.07 [0.97-1.18]
1945-1954		1.55 [1.19-2.01]**		1 [0.88-1.14]
1955+		2.26 [1.67-3.07]***		0.84 [0.72-0.98]*
Marriage duration at 50 (yrs)		0.98 [0.96-0.99]***		1.06 [1.05-1.07]***

*Note. Cause-specific Cox proportional hazards models, 15 countries. Baseline hazard stratified by country (pooled models). Robust standard errors via sandwich estimator. Adjusted models include birth cohort and marriage duration at age 50. HR=hazard ratio; 95% confidence interval in brackets. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

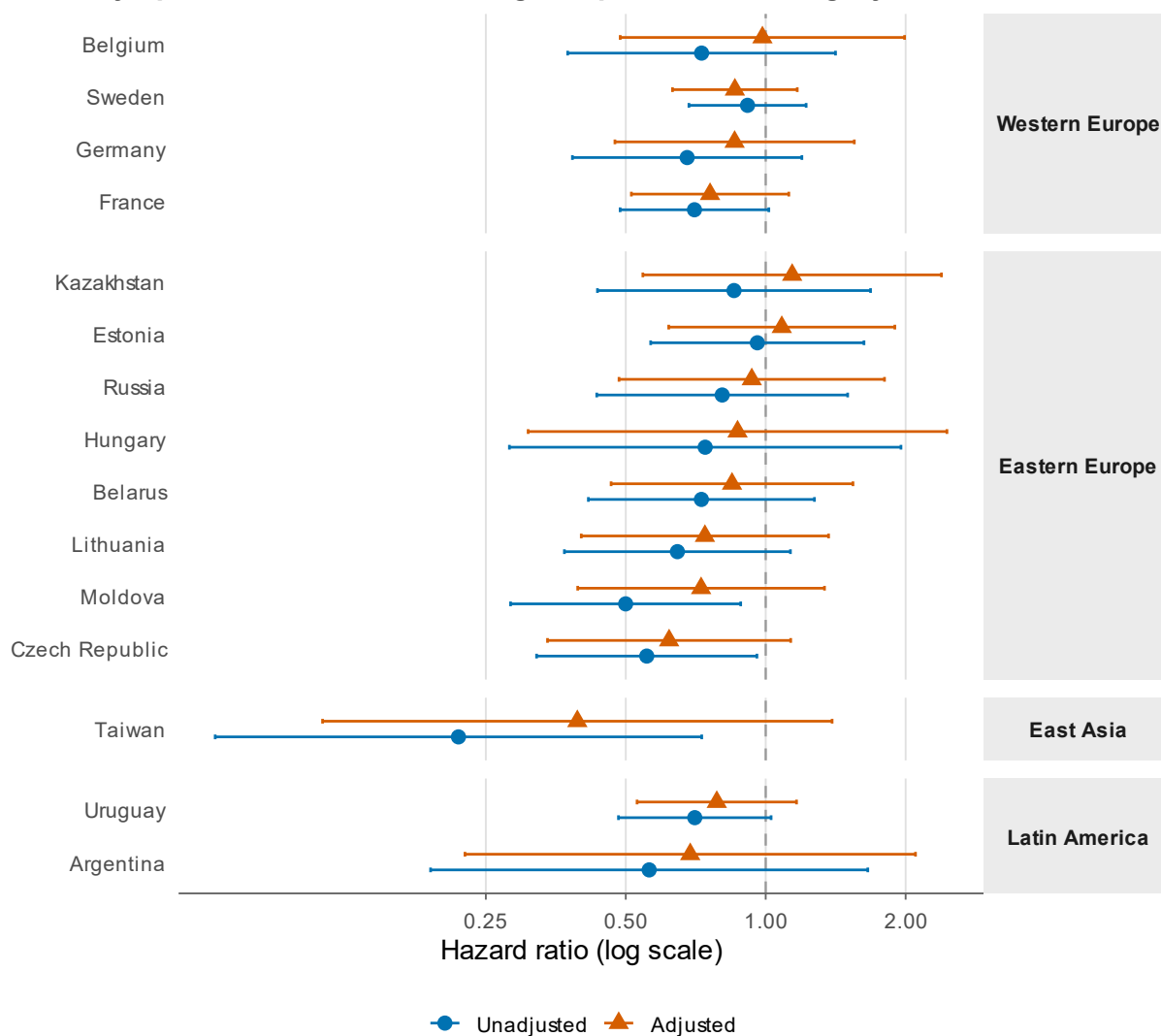
Figure 2 presents country-specific hazard ratios from the adjusted cause-specific Cox models, grouped by world region, with unadjusted estimates shown alongside for reference (the underlying hazard ratios and confidence intervals for all 15 countries are reported in Table A1 of the Online Appendix; Figure A3 presents the same estimates ordered by effect size rather than by region). Two prominent features of the cross-national distribution are apparent.

First, the direction of the grandparent-divorce association is consistently negative, i.e., protective, across the large majority of countries. Adjusted hazard ratios fall below 1.0 in 13 of the 15 countries included in the analysis. Confidence intervals are frequently asymmetric, with lower bounds further from the null than upper bounds. This indicates that, where uncertainty exists, it tends to concern the magnitude of the protective effect rather than its direction. The sole country for which the adjusted estimate clearly exceeds 1.0 is Kazakhstan, though the wide confidence interval renders this departure from the pooled pattern uncertain. Adjusted estimates for Belgium and Argentina are likewise near or slightly above 1.0, with intervals spanning the null value.

Second, the magnitude of the protective association varies considerably across national contexts. The most extreme association is observed in Taiwan, where both the unadjusted and the adjusted hazard ratios fall well below 0.5. This indicates a protective effect that greatly exceeds the pooled estimate and is robust to covariate adjustment. Among European countries, the Czech Republic and Lithuania show the lowest adjusted hazard ratios within the Eastern European cluster; within Western Europe, Germany and France exhibit somewhat stronger protective associations than Belgium and Sweden. In Latin America, Uruguay shows a more pronounced protective pattern than Argentina, though both country-specific estimates are individually imprecise.

Although Figure 2 suggests some degree of regional patterning, heterogeneity within regions, particularly within Eastern Europe, is substantial, and confident regional generalizations are not warranted. Consistent with the limited event counts available within individual countries, most country-specific confidence intervals include 1.0, and the within-country analyses lack power to establish statistical significance at the national level. The value of the country-level analysis lies not in establishing individual country significance, but in documenting the widespread directionality of the association and identifying the settings where the relationship between grandparenthood and marital stability is strongest. Regional pooled estimates, aggregating the country-specific results by inverse-variance weighting, are presented in Figure A6 of the Online Appendix and are broadly consistent with the pattern of widespread protective associations documented in Figure 2.

Country-specific hazard ratios for grandparenthood on gray divorce



Note: Cause-specific Cox models for divorce/separation after age 50. Sample: parents still married at age 50; grandparent status modelled as a time-varying covariate. Adjusted models control for birth cohort and marriage duration at age50. Horizontal lines are 95% confidence intervals. Dashed line marks HR = 1 (no association). Countries ordered by region, then by adjusted HR within region.

Figure 2: Country-specific hazard ratios of gray divorce

Discussion

This study provides the first multi-country population-level test of the braking hypothesis: the proposition that the transition to grandparenthood reduces the risk of gray divorce. Drawing on harmonized event-history data from 15 countries covering nearly half a century of marriages, and modelling grandparent status as a time-varying exposure with widowhood treated as a competing

risk, we find that becoming a grandparent is associated with a 19% reduction in the adjusted hazard of marital dissolution after age 50 (HR=0.81, 95% CI: 0.71–0.94). This association is directionally consistent in 13 of 15 countries and is robust to adjustment for birth cohort and marital duration. However, although the association is robust to these controls, given that the available harmonized data do not permit adjustment for the full set of socioeconomic and psychological characteristics known to predict divorce, the findings should be interpreted as descriptive evidence of a later-life family transition associated with marital stability, rather than as a causal analysis of the antecedents of gray divorce.

The magnitude of the protective effect is more modest than the 59% odds ratio reduction reported by Brown et al. (2021) in their foundational analysis of U.S. Health and Retirement Study data, although the metrics are not directly comparable as odds ratios and hazard ratios diverge, particularly when baseline event rates differ substantially across settings. The U.S. study also relied on couple-level panel data with a smaller analytic sample (1,045 couples) and controlled for a richer set of marital quality covariates, including remarriage status, spousal homogamy, and the empty nest phase. The attenuation observed here likely reflects both genuine cross-national variation in the strength of the effect and the conservative character of cause-specific Cox models relative to logistic specifications. The convergence on a protective direction is, nonetheless, striking: across geographically, culturally, and institutionally diverse settings (from East Asia to Latin America, from Western to Eastern Europe), the transition to grandparenthood consistently predicts a lower subsequent divorce hazard.

Cross-National Heterogeneity

The protective association is not uniform in magnitude. Country-specific adjusted hazard ratios range from 0.39 in Taiwan to 1.14 in Kazakhstan, reflecting a more than threefold variation in effect size (Figure A3, Table A1, Online Appendix). Taiwan stands apart from all other countries in terms of the strength of the estimated effect, a pattern that is consistent with the comparatively intense filial piety norms, high rates of multigenerational co-residence, and historically low divorce rates that characterize Taiwanese family culture. In contexts where divorce itself is normatively and legally less accessible, the marginal deterrent role of grandparenthood may be especially pronounced. In Europe, the Czech Republic and Lithuania show the most pronounced protective associations within the Eastern European cluster, while Sweden and Germany yield more modest

estimates, consistent with the comparatively liberal and individualized divorce culture of Northern and Central European societies (Härkönen and Dronkers 2006; Sieben and Verbakel 2013). In Latin America, Uruguay shows a somewhat stronger protective pattern than Argentina, although the country-specific estimates are imprecise in both cases.

Kazakhstan is the sole country where the adjusted point estimate exceeds 1.0, though the confidence interval is very wide (0.54–2.39) and the departure from the pooled estimate cannot be taken as reliable evidence of a genuinely distinct pattern. The sample is smaller and the event count is sparser in Kazakhstan than in most other countries, and the adjusted estimate may reflect compositional features not fully captured by the available covariates. Belgium and Estonia also show adjusted estimates near or above 1.0 with wide intervals. The most defensible interpretation of these findings is that the braking mechanism operates broadly but with culturally modulated intensity, and is strongest in contexts where family traditionalism, filial obligation, and divorce stigma remain comparatively strong.

Widowhood as a Competing Risk

The competing risk analysis reveals a second and substantively important pattern: grandparents face a modestly but significantly elevated widowhood hazard even after adjustment (HR=1.11, 95% CI: 1.02–1.21). This most likely reflects age composition differences within the grandparent group that categorical cohort indicators do not fully absorb: at any given interview wave, older individuals are more likely to have grandchildren and to experience spousal death. The implication is that the cause-specific approach adopted here, which appropriately treats widowhood as a censoring event rather than as a competing risk in the divorce equation, may nonetheless slightly understate divorce hazard differences by allowing the grandparent group to accumulate disproportionate spousal loss. A competing risks estimator would, if anything, further attenuate the grandparent-divorce association; the present estimates should therefore be regarded as conservative upper bounds on the protective effect. Gender-stratified models suggest that the association may be somewhat stronger for male respondents (HR=0.72, 95% CI [0.58–0.90]) than for female respondents (HR=0.87, 95% CI [0.72–1.05]), although the confidence intervals overlap substantially and the female estimate does not reach statistical significance (Table A3, Online Appendix).

The psychosocial and structural mechanisms through which grandparenthood may reduce dissolution risk are discussed in the theoretical framework above. Briefly, four candidate pathways are

identified: generativity-based renewal of shared purpose following Erikson (1950) and the linked-lives logic of life course theory (Elder 1998); the elevated social and emotional costs of dissolution once grandchildren are embedded in the family system (King 2003); the well-being benefits associated with the transition to grandparenthood (Hou 2025; Sheppard and Monden 2019; Tanskanen et al. 2019); and the time intensity of active grandparental involvement. These mechanisms are not mutually exclusive; their relative importance likely varies with the intensity of grandparental involvement, the normative salience of family obligations, and the legal accessibility of divorce. Their common implication is that the protective effect should be strongest in contexts where intergenerational obligations are most salient and divorce carries the greatest normative cost, a prediction that is broadly consistent with the cross-national pattern documented here.

Trends in Grandparenthood and Implications for Gray Divorce

The cohort data in our sample reveal a pronounced secular increase in gray divorce risk alongside a broadly stable pattern of grandparenthood prevalence. Relative to the pre-1935 cohort, the adjusted divorce hazard more than doubles for respondents born from 1955 onward (HR=2.26, 95% CI: 1.67–3.07), consistent with the well-documented historical liberalization of divorce norms and laws across all countries in the sample (Brown and Lin 2012, 2022; Zahl-Olsen 2022). When grandparenthood prevalence is assessed over a common 15-year observation window to remove truncation bias, it is shown to be broadly stable across cohorts, at approximately 45–47% (Figure A5, Online Appendix). The steep apparent decline visible in the unrestricted figures (from 84% in the oldest cohort to 55% among those born after 1955) largely reflects the shorter observation windows for younger cohorts rather than genuine cohort change. The mean age at the birth of the first grandchild among those who do become grandparents rises modestly across cohorts (from 55.3 years in the pre-1935 cohort to 57.2 in the 1955+ cohort), consistent with intergenerational transmission of fertility postponement (Steenhof and Liefbroer 2008). The mean age at gray divorce also declines modestly across successive birth cohorts, from approximately 56.5 years among those born before 1935 to 54.6 years among those born from 1955 onward, suggesting that dissolution events occur somewhat earlier in the post-50 life course in more recent cohorts (Figure A7, Online Appendix).

Simultaneously, grandparenthood prevalence within the 20-year post-50 observation window declines from 84% among the oldest cohorts to 55% among cohorts born after 1955 (Figure A5,

Online Appendix), a trend that is driven partly by observation window truncation in younger cohorts, but also reflects genuine fertility postponement and rising levels permanent childlessness (Herlofson and Hagestad 2011; Hoepflinger 2025; Leopold and Skopek 2015; Margolis 2016).

These patterns suggest that the structural brake of grandparenthood has not become markedly less available to successive cohorts within a comparable observation window, even as divorce risk has risen sharply. Whether grandparenthood prevalence will decline in younger cohorts as fertility postponement and permanent childlessness continue to spread remains an open question that the present data cannot resolve. Population register analyses with complete birth linkages, such as those available in the Nordic countries, are well positioned to address this issue. Kridahl and Kolk (2025) documented rising late-life divorce rates across Swedish cohorts, and Kridahl et al. (2026) further showed that late-life divorce risk in Swedish stepfamilies varies by multigenerational family complexity, defined by the presence and lineage of joint children, stepchildren, joint grandchildren, and step-grandchildren. Taken together with the present cross-national evidence, these findings point to a coherent intergenerational family mechanism underlying gray divorce trends.

Limitations

Several limitations temper these conclusions. First, the GGS data do not distinguish biological from step-grandparenthood. The U.S. evidence strongly suggests that only biological grandparenthood reduces gray divorce risk, with step-grandparenthood showing no protective effect (Brown et al. 2021; Yahirun et al. 2018). Conflating the two types in a single time-varying indicator attenuates the estimated biological grandparenthood effect toward zero. As stepfamily rates have risen across most countries in the sample, this attenuation may be more pronounced in later cohorts, potentially explaining some of the cohort variation in the protective association.

Second, the GGS event-history data do not include measures of marital quality, relationship satisfaction, financial stress, or grandparenting intensity. These omitted variables are plausible confounders, as couples with higher-quality marriages may both enter grandparenthood earlier and be less prone to divorce. Their absence therefore limits the interpretability of the estimated association as a causal effect. The time-varying specification mitigates but does not eliminate this concern. Future work incorporating survey measures of marital quality alongside administrative records of grandparenthood timing, as is possible in the Nordic countries, would substantially strengthen causal inference.

Third, country-specific estimates are individually imprecise due to sparse event counts. As noted in the results, the cross-national analysis is primarily descriptive with respect to individual country associations; the pooled estimate is the primary inferential target. Several countries show non-convergence warnings in adjusted models, and the estimate for Kazakhstan should be treated with particular caution. The 15-country sample, while broad, excludes populations among whom grandparenthood norms and divorce patterns may differ substantially, including most of sub-Saharan Africa, South and Southeast Asia, and the Middle East.

Fourth, right-censoring in younger cohorts affects both the grandparenthood prevalence estimates and the gray divorce rate estimates. To address this, Figure A5 is based on a restricted sample limited to individuals with at least 15 years of post-50 follow-up, ensuring that all cohorts share the same observation window. Under this restriction, grandparenthood prevalence proves broadly stable across cohorts (approximately 45–47% in each birth cohort), which suggests that the steep apparent decline in unrestricted figures is largely a truncation artifact rather than a genuine cohort change. A residual limitation remains: the 1955+ cohort contributes only 1,082 observations under this restriction, compared to approximately 3,900 for the two middle cohorts, hence estimates for this cohort carry greater uncertainty. A longer follow-up period will be needed to establish how grandparenthood prevalence and divorce risk are evolving in the most recent cohorts.

Fifth, the main analyses pool male and female respondents rather than presenting gender-stratified estimates throughout. Gender-stratified hazard ratios from the pooled adjusted model are reported in Table A3 of the Online Appendix and are discussed briefly in the Discussion; we refrain from stratifying the country-level and regional figures by gender because the resulting 30-country or eight-region forest plots would substantially reduce readability without materially altering the substantive conclusion, and because event counts within country-by-gender cells are too sparse to support reliable country-specific inference. The pooled specification also preserves statistical power for the primary test of the braking hypothesis, which concerns the average association across couples, rather than its gendered decomposition. A more detailed treatment of gender differences in the grandparent–divorce link, including one that addresses the question of whether the association is concentrated among grandmothers as predicted by the well-being literature (Di Gessa et al. 2020; Sheppard and Monden 2019), would require both larger national samples and richer measurement of grandparenting involvement than the GGS harmonized histories currently provide.

Sixth, the adjusted models include only birth cohort and marriage duration at age 50, and they do not account for a broader set of established correlates of divorce such as educational attainment, income, personality, or cognitive functioning (Lyngstad and Jalovaara 2010). This reflects the scope of the present study. Our aim is not to provide a comprehensive decomposition of gray divorce risk, but rather to examine whether the transition to grandparenthood is associated with subsequent marital dissolution across a large set of national contexts. At the same time, the absence of these covariates means that residual confounding cannot be ruled out. In particular, socioeconomic and psychological characteristics may shape both the likelihood and the timing of grandparenthood and the stability of later-life marriages. The cross-nationally harmonized GGS event-history files do not provide comparable measures of personality or cognition across countries, and measures such as education and income are not consistently available in a way that would permit harmonized inclusion across all 15 country samples without substantially reducing comparability and sample size. The results should therefore be interpreted as evidence of an association rather than a definitive causal effect. Future work using data with richer and more uniformly measured socioeconomic and psychological covariates should assess the extent to which these factors mediate or confound the observed grandparenthood gradient in gray divorce.

Conclusion

The transition to grandparenthood exerts a cross-nationally robust dampening effect on the risk of gray divorce. An adjusted 19% reduction in the dissolution hazard implies that intergenerational family ties function as a structural stabilizer of late-life marriages. As fewer individuals will experience grandparenthood over time due to declining fertility, overall divorce levels among older individuals could increase over time. As grandparenthood is delayed and becomes less universal in high-income societies, this stabilizing function will reach a shrinking share of the older married population, a compositional dynamic whose contribution to secular gray divorce trends warrants greater empirical attention. Hence, rising childlessness and fertility postponement will gradually reduce the share of older couples for whom grandparenthood functions as a marital stabilizer – a demographic consequence of fertility decline that has received little systematic attention alongside its more familiar effects on population size and age structure.

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Online Appendix

Grandparenthood Related to Reduced Risk of Gray Divorce: Evidence From 15 Countries

Contents

Figure A1. Sample Construction Flowchart	2
Figure A2. Kaplan-Meier Survival Curves (Pooled)	3
Figure A3. Country-Specific Hazard Ratios, Ordered by Effect Size	4
Figure A4. Grandparenthood Prevalence by Country (Ages 50–70)	5
Figure A5. Gray Divorce and Grandparenthood Trends by Birth Cohort	6
Figure A6. Regional Pooled Hazard Ratios for Grandparenthood on Gray Divorce	8
Table A1. Country-Specific Cause-Specific Cox Regression Results	9
Figure A7. Mean Age at Divorce by Birth Cohort	10
Table A2. Sensitivity Analysis: Random Month Imputation	11
Table A3. Gender-Stratified Hazard Ratios for Grandparenthood	12
Figure A8. Birth Year Distribution of the Analytical Sample	13

Figure A1. Sample Construction Flowchart

The flowchart illustrates the construction of the analytical sample from the raw GGS Harmonized Histories data. Starting from all respondents in first marriages who were observed at age 50 across the 20 countries included in GGS Rounds I and II ($N = 40,755$), the sample is restricted in two steps. First, four countries are excluded on empirical grounds: Bulgaria (27 events, estimate reverses direction), Georgia (7 events, uninformative estimate), Poland (model non-convergence due to empty cohort cells), and Norway (no harmonized event-history data). Italy participated in GGS Round I but produces zero rows in the processed data: no respondent identifiers in the Italian country file overlap with those in the GGS Harmonized Histories file (the ARID ranges do not intersect), so no observations survive the merge step and Italy is excluded. Second, respondents without a child born before age 50 are excluded, as they are not at risk of grandparenthood during the observation window. The final analytical sample of 30,187 parents is then divided into two groups according to grandparent status during the 20-year post-50 observation period.

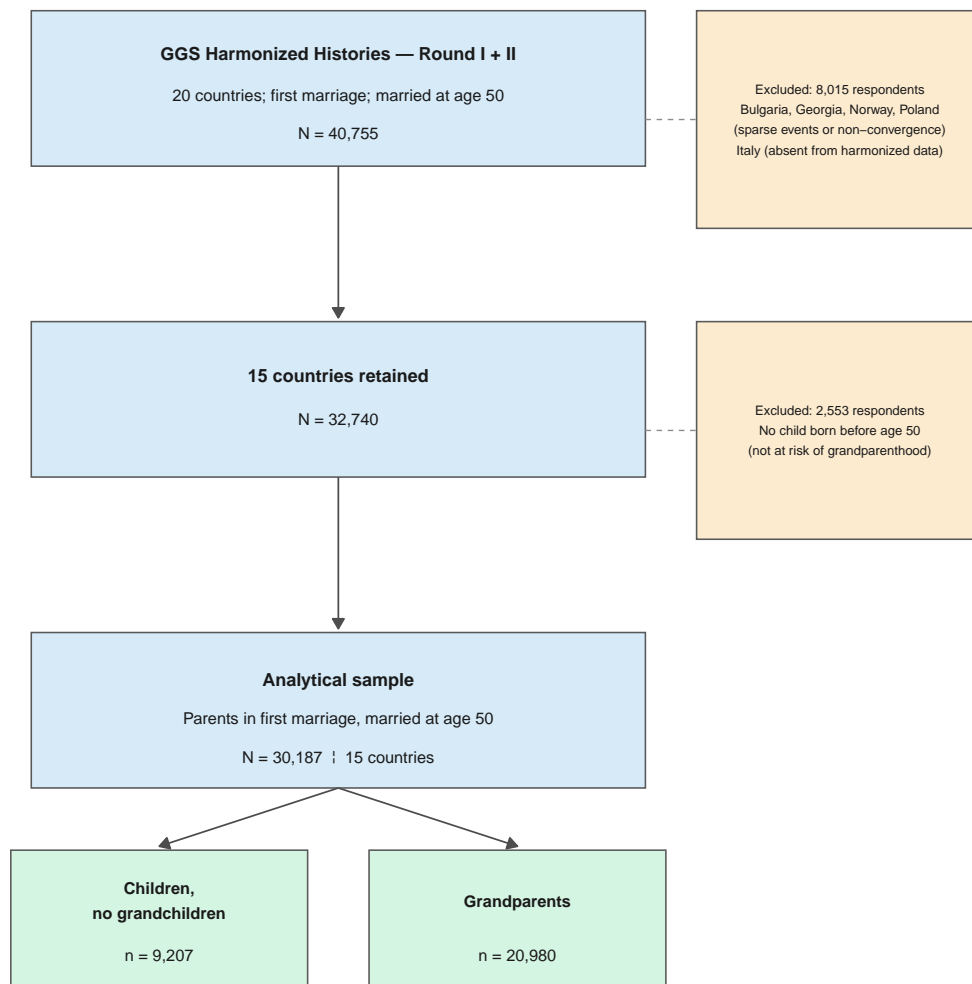
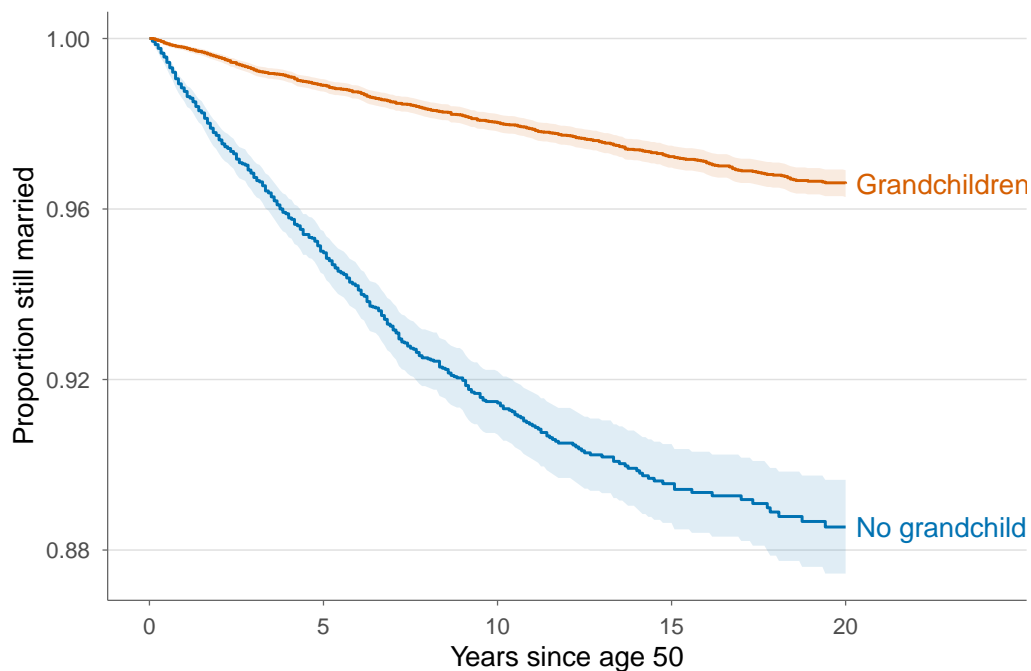


Figure A2. Kaplan-Meier Survival Curves (Pooled)

The figure presents pooled Kaplan-Meier estimates of marital survival after age 50, combining all 15 countries, by grandparent status. This complements Figure 1 in the main text (which shows country-specific panels) by displaying the overall average gap in dissolution trajectories across the full analytical sample. The shaded bands are 95% pointwise confidence intervals. Widowhood is treated as a censoring event; the Kaplan-Meier estimator therefore slightly overestimates the absolute probability of marital dissolution relative to a competing-risks estimator. The time axis runs from 0 to 20 years after age 50.

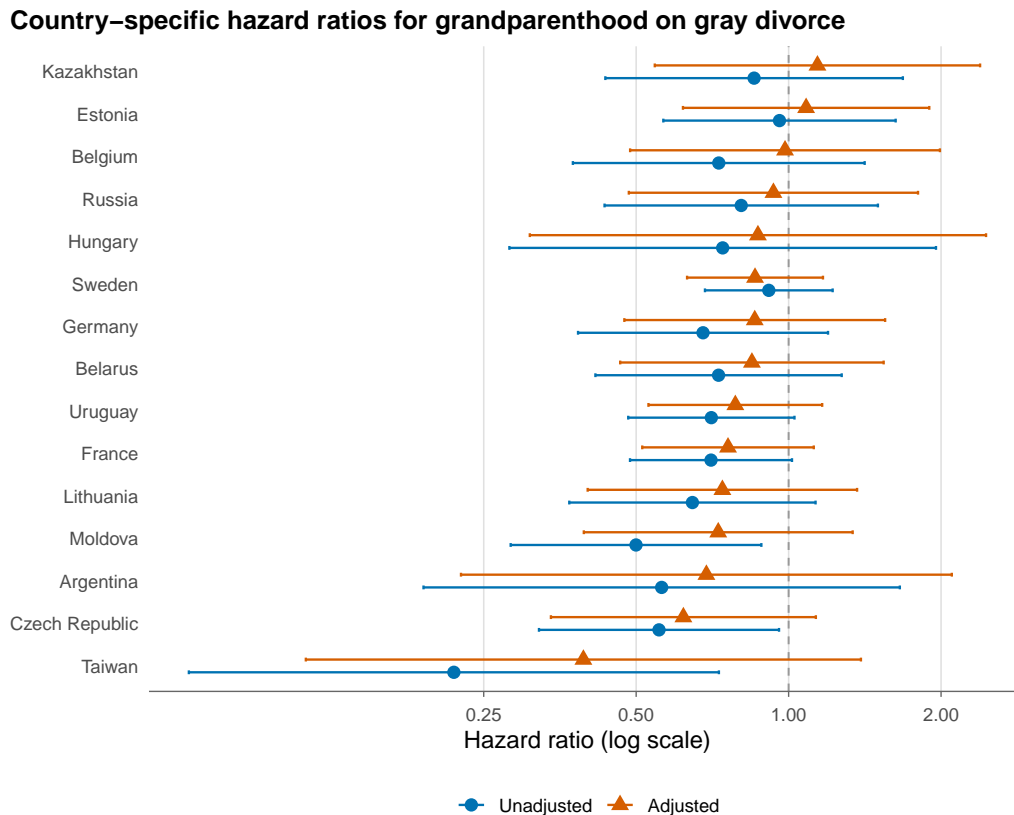
Kaplan–Meier survival after age 50 (pooled, 15 countries)



Note: Kaplan–Meier estimates. Sample: parents (first child born before age 50) still married at age 50; follow-up capped at 20 years. Grandparent status is time-varying: respondents enter the “Grandchildren” group at first grandchild’s birth. Shaded bands are 95% confidence intervals. Widowhood is treated as censoring; the competing risk of spouse death is not modelled. Countries: Argentina, Belarus, Belgium, Czech Republic, Estonia, France, Germany, Hungary, Kazakhstan, Lithuania, Moldova, Russia, Sweden, Taiwan, Uruguay.

Figure A3. Country-Specific Hazard Ratios, Ordered by Effect Size

The figure presents country-specific hazard ratios for the association between grandparenthood and the hazard of gray divorce, ordered from the smallest to the largest adjusted estimate. Both unadjusted and adjusted (for birth cohort and marriage duration at age 50) estimates are shown alongside 95% confidence intervals. This ordering allows direct visual comparison of effect magnitudes across countries. The corresponding figure in the main text (Figure 2) arranges the same estimates by world region. The dashed vertical line marks a hazard ratio of 1.0 (no association); the x-axis is on a log scale. Taiwan shows by far the strongest protective association; Kazakhstan is the only country where the adjusted estimate exceeds 1.0, though the confidence interval is wide. Numerical values underlying this figure are reported in Table A1.



Note: Cause-specific Cox models for divorce/separation after age 50. Sample: parents still married at age 50; grandparent status modelled as a time-varying covariate. Adjusted models control for birth cohort and marriage duration at age 50. Horizontal lines are 95% confidence intervals. Dashed line marks HR = 1 (no association).

Figure A4. Grandparenthood Prevalence by Country (Ages 50–70)

The figure shows the proportion of parents (first marriage, first child born before age 50, still married at age 50) who became grandparents at any point during the 20-year observation window from ages 50 to 70, separately for each of the 15 countries. This is a window-specific prevalence measure — it captures grandparenthood transitions occurring within the post-50 follow-up period — and should not be interpreted as lifetime grandparenthood prevalence. Individuals who had already entered grandparenthood before age 50 are included in the figure from age 50 onward and are not excluded; this already-grandparent group accounts for approximately 54% of all grandparents in the analytical sample, as shown in Table 1 of the main text. Data are drawn from GGS Rounds I (surveys conducted approximately 2003–2011) and Round II (approximately 2017–2022) across the 15 countries in the analysis; birth cohorts represented span from respondents born before 1935 through those born in the mid-1950s and later. Countries are ordered ascending by grandparenthood prevalence. Grandparenthood prevalence varies substantially across the sample, from approximately 35% in Taiwan to nearly 88% in Moldova. These differences reflect variation in fertility timing, childlessness rates, migration patterns, and the age composition of survey respondents across countries and GGS rounds.

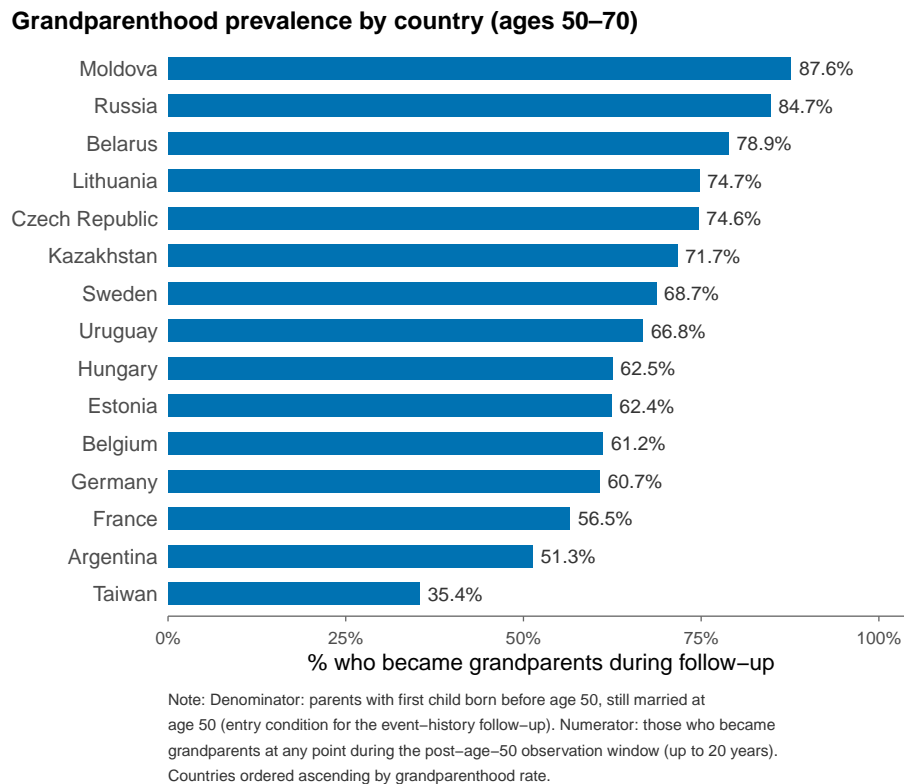


Figure A5. Gray Divorce and Grandparenthood Trends by Birth Cohort

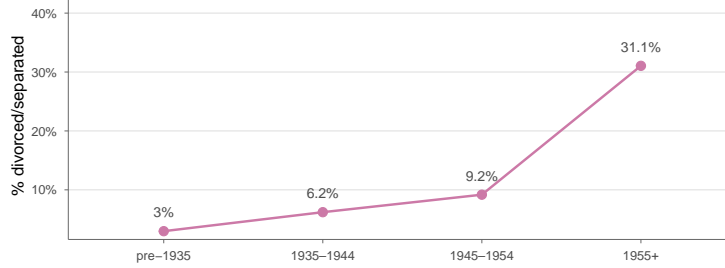
The three-panel figure describes variation in key outcomes and exposures across birth cohorts within the analytical sample (parents in first marriages, married at age 50, 15 countries). Data are drawn from GGS Rounds I (surveys conducted approximately 2003–2011) and Round II (approximately 2017–2022), covering birth cohorts from pre-1935 through the mid-1950s and later. Birth cohorts are grouped into four approximately decade-wide bands (born before 1935, 1935–1944, 1945–1954, 1955 or later). To ensure comparability across cohorts, all three panels use the same restricted sample and the same 15-year observation window: the sample is limited to individuals with at least 15 years of post-50 follow-up, or who divorced within 15 years, and all outcomes are capped at 15 years after age 50. This removes the truncation problem that arises because respondents in later cohorts were surveyed at younger ages and therefore have shorter observation windows.

Panel (a): Gray divorce/separation rate. The proportion of parents who divorced or separated within 15 years of age 50.

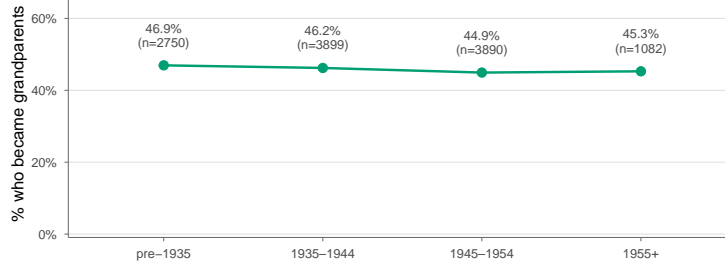
Panel (b): Grandparenthood prevalence. The proportion of parents who became grandparents within 15 years of age 50. Cohort sample sizes are shown alongside the prevalence estimates.

Panel (c): Mean age at first grandchild. Among those who became grandparents within the 15-year window, the mean age at first grandchild’s birth by birth cohort, consistent with fertility postponement across cohorts.

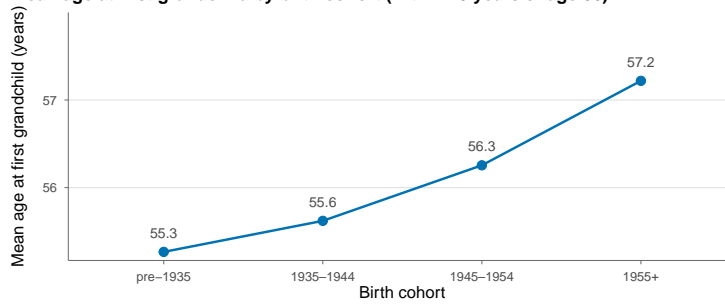
Gray divorce/separation rate by birth cohort (within 15 years of age 50)



Grandparenthood prevalence by birth cohort (within 15 years of age 50)



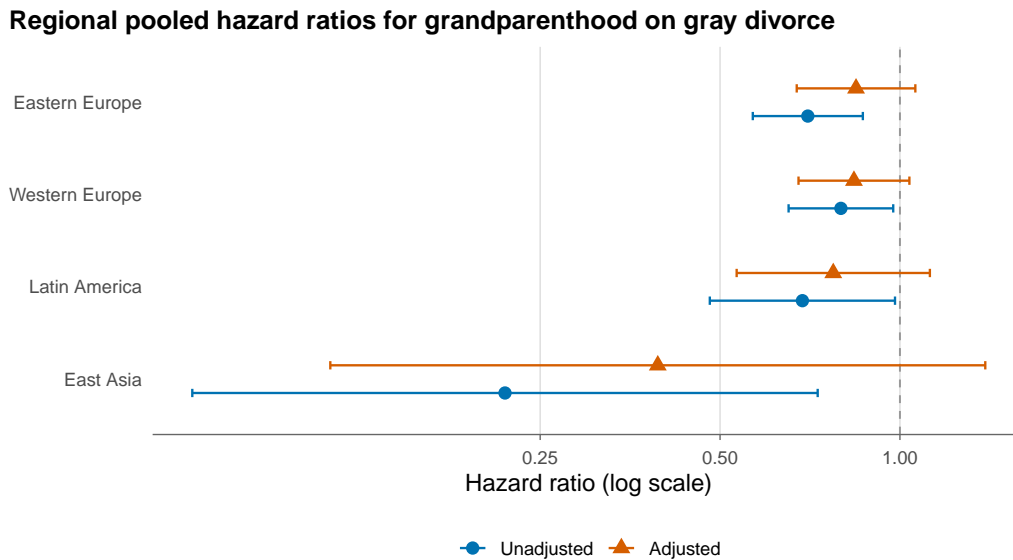
Mean age at first grandchild by birth cohort (within 15 years of age 50)



Note: All three panels use the same restricted sample and observation window. Sample: parents still married at age 50; 15 countries. Restricted to individuals with at least 15 years of post-50 follow-up, or who divorced within 15 years (ensuring early divorce events are retained). All outcomes are capped at 15 years after age 50. Panel (a) counts divorces within this window; Panel (b) counts grandparent transitions within this window; Panel (c) reports mean age at first grandchild among those who transitioned within the window. Sample sizes in Panel (b) refer to the restricted sample per cohort.

Figure A6. Regional Pooled Hazard Ratios for Grandparenthood on Gray Divorce

The figure presents inverse-variance weighted (IVW) pooled hazard ratios for the effect of grandparenthood on gray divorce, aggregated from the country-specific estimates to the regional level. This complements Figure 2 of the main text, which shows the same estimates at the country level grouped by region. For each region, the country-specific log hazard ratios are pooled using fixed-effects IVW weighting: each country's estimate is weighted by the inverse of its squared standard error (derived from the 95% confidence interval). Pooled hazard ratios and 95% confidence intervals are shown for both the unadjusted and the adjusted (birth cohort, marriage duration) models. The four regions are: Western Europe (Belgium, France, Germany, Sweden; 4 countries), Eastern Europe (Belarus, Czech Republic, Estonia, Hungary, Kazakhstan, Lithuania, Moldova, Russia; 8 countries), East Asia (Taiwan; 1 country), and Latin America (Argentina, Uruguay; 2 countries). The dashed vertical line marks a hazard ratio of 1.0 (no association); the x-axis is on a log scale. Fixed-effects pooling assumes a common underlying effect within each region, which is a simplifying assumption given the heterogeneity documented at the country level.



Note: Cause-specific Cox models for divorce/separation after age 50. Sample: parents still married at age 50; grandparent status modelled as a time-varying covariate. Adjusted models control for birth cohort and marriage duration at age 50. Horizontal lines are 95% confidence intervals. Dashed line marks HR = 1 (no association). Regional estimates are inverse-variance weighted pooled hazard ratios across the countries within each region (fixed-effects pooling)

Table A1. Country-Specific Cause-Specific Cox Regression Results

The table presents unadjusted and adjusted hazard ratios for the effect of grandparenthood (time-varying; reference: no grandparenthood) on the hazard of gray divorce, estimated separately for each of the 15 countries. Adjusted models control for birth cohort (four categories) and marriage duration at age 50 in years. Hazard ratios and 95% confidence intervals are reported; significance stars reflect Wald test p-values. Countries are ordered by ascending adjusted hazard ratio (most protective effect first). The same estimates are displayed graphically in Figure A3 (ordered by effect size) and Figure 2 of the main text (grouped by world region). Country-specific estimates should be interpreted cautiously: most confidence intervals are wide due to sparse event counts within individual national samples, and several countries show non-convergence warnings for the adjusted model. The pooled estimates (Table 2, main text) are the primary inferential target.

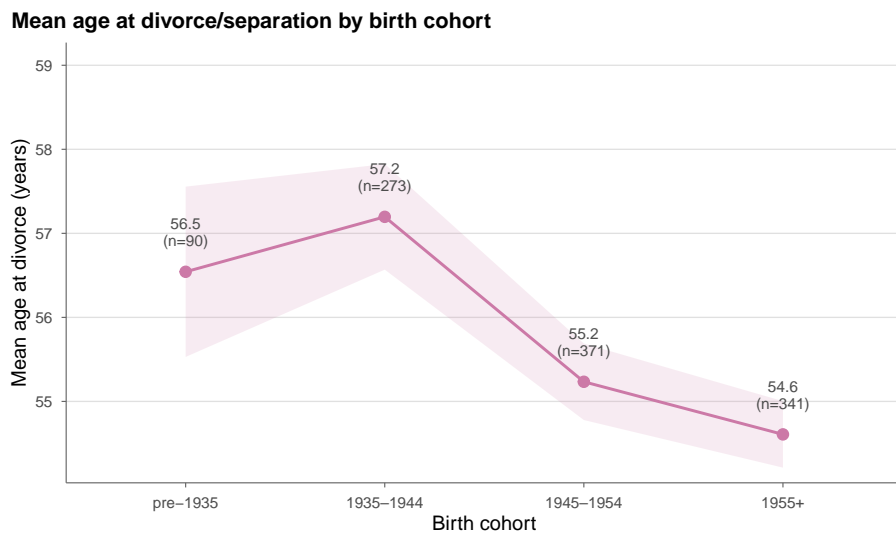
Table 1: Country-specific hazard ratios for grandparenthood on gray divorce

Country	Unadjusted HR [95% CI]	Adjusted HR [95% CI]
Taiwan	0.22 [0.07–0.73]*	0.39 [0.11–1.39]
Czech Republic	0.55 [0.32–0.96]*	0.62 [0.34–1.13]
Argentina	0.56 [0.19–1.66]	0.69 [0.23–2.1]
Moldova	0.5 [0.28–0.88]*	0.73 [0.39–1.34]
Lithuania	0.65 [0.37–1.13]	0.74 [0.4–1.37]
France	0.7 [0.49–1.02]	0.76 [0.51–1.12]
Uruguay	0.7 [0.48–1.03]	0.78 [0.53–1.16]
Belarus	0.73 [0.42–1.27]	0.85 [0.46–1.54]
Germany	0.68 [0.38–1.2]	0.86 [0.47–1.55]
Sweden	0.91 [0.68–1.22]	0.86 [0.63–1.17]
Hungary	0.74 [0.28–1.95]	0.87 [0.31–2.45]
Russia	0.81 [0.43–1.5]	0.93 [0.48–1.8]
Belgium	0.73 [0.37–1.41]	0.98 [0.49–1.99]
Estonia	0.96 [0.57–1.63]	1.08 [0.62–1.9]
Kazakhstan	0.85 [0.43–1.68]	1.14 [0.54–2.39]

Note. Cause-specific Cox proportional hazards models. Grandparent status is a time-varying covariate. Adjusted models control for birth cohort and marriage duration at age 50. HR = hazard ratio; 95% CI in brackets. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Countries ordered by ascending adjusted HR.

Figure A7. Mean Age at Divorce by Birth Cohort

The figure shows mean age at divorce or separation among respondents who dissolved their marriage within the post-50 observation window, by birth cohort. Each point summarises the cohort-specific mean; the shaded band shows ± 1.96 standard errors around the mean. The figure is restricted to the analytical sample (parents in first marriages, still married at age 50, 15 countries). It addresses the question of whether the timing of gray divorce (not just its prevalence) has shifted across cohorts. A rise in mean age at divorce across successive birth cohorts would indicate that dissolution events are occurring later in the life course, consistent with longer remaining life expectancy and longer marriages among more recent cohorts. Sample sizes per cohort (shown in parentheses) should be noted when interpreting cohort-level trends.



Note: Restricted to respondents who divorced or separated within the post-50 follow-up window (fail50 = 1). Sample: parents still married at age 50; 15 countries. Age at divorce is the respondent's calendar age at the dissolution event. Error band shows $\pm 1.96 \times SE$ around the cohort mean. Sample sizes show the number of divorces per cohort.

Table A2. Sensitivity Analysis: Random Month Imputation

The table presents the hazard ratio for grandparenthood on gray divorce under three alternative strategies for imputing missing grandchild birth months. When a grandchild’s birth year is recorded but the calendar month is missing or invalid, the main analysis assigns a random month drawn from a uniform distribution over 1–12. To assess whether the precision of grandparenthood transition timing affects the results, the same adjusted pooled Cox model is re-estimated under two deterministic alternatives: January imputation (month = 1, placing the transition at the earliest possible point within the birth year) and mid-year imputation (month = 7). The main analysis result (random imputation, seed 20260213) is shown for reference. Stability of the hazard ratio across all three strategies indicates that the results are not sensitive to the month imputation assumption.

Table 2: Sensitivity of HR(grandparenthood) to grandchild birth month imputation strategy

Strategy	N	Events	HR (95% CI)	p-value
Random / uniform (main analysis)	30071	1069	0.815 [0.707–0.939]	0.005
January (month = 1)	30071	1068	0.821 [0.713–0.946]	0.006
Mid-year (month = 7)	30071	1068	0.82 [0.712–0.945]	0.006

Note. Adjusted pooled cause-specific Cox model (grandparenthood + birth cohort + marriage duration at age 50 + country-stratified baseline hazard). The imputation strategy applies only to cases where the grandchild’s birth year is known but the calendar month is missing or invalid; cases with a valid recorded month are unaffected. HR = hazard ratio; 95% CI in brackets. The main analysis uses the random strategy (seed 20260213).

Table A3. Gender-Stratified Hazard Ratios for Grandparenthood

The table presents hazard ratios for the effect of grandparenthood on gray divorce estimated separately for male and female respondents, alongside the pooled estimate from Table 2 of the main text. All models use the same adjusted specification (grandparenthood + birth cohort + marriage duration at age 50 + country-stratified baseline hazard). The stratification is by respondent sex as recorded in the GGS Harmonized Histories (1 = Male, 2 = Female). A substantially stronger protective association among female respondents would be consistent with the theoretical expectation that the well-being benefits of the grandparent transition are concentrated among grandmothers.

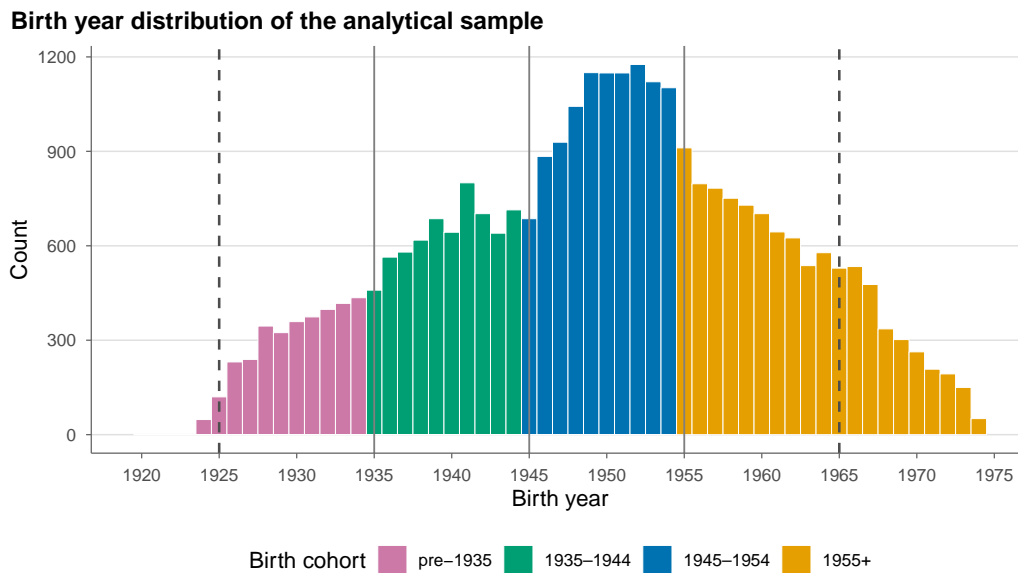
Table 3: Gender-stratified hazard ratios for grandparenthood on gray divorce

Sex	N	Events	HR (95% CI)
Male	13658	475	0.72 [0.58–0.9]**
Female	16413	594	0.87 [0.72–1.05]
Pooled	30071	1069	0.81 [0.71–0.94]**

Note. Cause-specific Cox proportional hazards models, adjusted for birth cohort and marriage duration at age 50; country-stratified baseline hazard. Respondent sex as coded in GGS Harmonized Histories (1 = Male, 2 = Female). The pooled estimate reproduces the adjusted model from Table 2. HR = hazard ratio; 95% CI in brackets. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure A8. Birth Year Distribution of the Analytical Sample

The figure shows the distribution of birth years across the full analytical sample (parents in first marriages, still married at age 50, 15 countries). Bars are coloured by birth cohort band (born before 1935, 1935–1944, 1945–1954, and 1955 or later), corresponding to the four groups used throughout the analysis. Solid grey vertical lines mark the cohort cut-points (1935, 1945, 1955); dashed vertical lines mark 1925 and 1965 to indicate the distribution tails. Birth years in the analytical sample range from 1920 to 1974. Respondents born before 1925 account for $n = 50$ (0.2%) of the sample; those born after 1965 account for $n = 2514$ (8.3%). The figure contextualises the cohort composition of the sample and the degree of representation at the extremes of the birth year distribution.



Note: Analytical sample (parents in first marriage, still married at age 50, 15 countries). Bars are coloured by birth cohort band. Solid grey lines mark the cohort cut-points (1935, 1945, 1955). Dashed lines mark 1925 and 1965 to indicate the distribution tails. Birth years range from 1920 to 1974. Respondents born before 1925: $n = 50$ (0.2%); born after 1965: $n = 2514$ (8.3%).