

Kapelle, Nicole

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Why time cannot heal all wounds: Personal wealth trajectories of divorced and married men and women

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SOEP papers
on Multidisciplinary Panel Data Research

**Why time cannot heal all wounds:
Personal wealth trajectories of
divorced and married
men and women**

Nicole Kapelle

SOEPPapers on Multidisciplinary Panel Data Research at DIW Berlin

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German Socio-Economic Panel (SOEP)

DIW Berlin

Mohrenstrasse 58

10117 Berlin, Germany

Contact: soeppapers@diw.de



Why time cannot heal all wounds: Personal wealth trajectories of divorced and married men and women

Nicole Kapelle ^{a,b} nicole.kapelle@sociology.ox.ac.uk

^a *University of Oxford, Department of Sociology, 42-43 Park End Street, Oxford OX1 1JD, England*

^b *University of Oxford, Nuffield College, New Road, Oxford, OX1 1NF, England*

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Abstract: Amid concerns of long-term economic consequences of divorce, cross-sectional research illustrated that ever-divorce men but particularly women hold less per capita wealth than continuously married spouses in older age. Using a longitudinal approach and unique personal-level wealth data from the German Socio-Economic Panel Study, the present study aims to understand *how* divorce stratifies men's and women's wealth trajectories. To this end, I apply a novel doubly robust estimation approach that combines propensity score and coarsened exact matching with random-effects growth models to provide causal comparisons of wealth trajectories. Results show that wealth differences between ever-divorce and continuously married individuals predominantly stem from persistent disadvantage generated immediately around divorce rather than a scarring of divorcees' wealth accumulation over time, although remarriage is a relevant moderator of post-divorce wealth accumulation. Divorced women's wealth disadvantage compared to men's likely stems from a range of sources including the maintenance of within-couple wealth inequalities, biased property division processes, and lower wealth accumulation potentials after divorce. Comparatively, married women benefit from marital protection to compensate their lower wealth accumulation potential. Finally, selection into divorce is a relevant although secondary factor that needs to be considered in the explanation of divorce-related wealth stratification.

Keywords: Divorce, Wealth stratification, Gender, Life course, Matching

Amid historically high divorce rates across OECD countries and a rising relevance of individuals' access to sufficient private wealth to secure living standards throughout the life course (Eurostat 2018, Keister and Moller 2000), concerns have been raised about potential long-term economic consequences of marital dissolution for men's and women's wealth (i.e., assets minus liabilities). Following these concerns, a small body of predominantly US-based cross-sectional research unequivocally found that ever-divorced men but particularly women held substantially lower wealth in late working age compared to continuously married men and women (e.g., Wilmoth and Koso 2002, Zissimopoulos, Karney and Rauer 2015). Thus, divorce likely contributes to rising between-household wealth inequalities.

Previous research is limited in two important ways that hamper our understanding of *how* divorce stratifies wealth of men and women. First, previous studies had to rely overly on cross-sectional data and methods. In line with notions from the life course framework, previous studies theoretically discussed older-age wealth disparities between the married and ever-divorced as a result of divorce-related immediate wealth penalties, long-term wealth accumulation disparities, and potentially wealth-related selection into divorce. However, an empirical exploration of these mechanisms was methodologically unfeasible. Thus, I argue that a longitudinal empirical strategy is needed to understand *how* divorce is linked to a disruption of wealth trajectories over time and to what degree inherent pre-divorce differences matter. A thorough understanding of divorce-related wealth stratification processes is relevant to researchers and policymakers alike to discuss how wealth disparities across marital states can be minimized to ensure economic self-sufficiency amid growing family complexities.

Second, due to data limitations, previous studies predominantly focused on household-level wealth. To compare between continuously married and ever-divorced remarried or unmarried respondents, researchers divided household wealth by two for couples (i.e., per capita wealth). Studies thereby implicitly assumed that household wealth is owned equally during the marriage

and split equally in the case of divorce. Although long-term, stably married spouses share and pool a substantial proportion of their resources, wives commonly hold less personal wealth than their husbands with independent money management particularly prevalent in higher-order marriages (Burgoyne and Morison 1997, Kapelle and Lersch 2020). Additionally, not all resources are split equally at divorce with pre-marital wealth or inheritances and gifts received during the marriage regularly excluded from the division process (Kapelle and Baxter 2021). Thus, the assumption of equal ownership and division of all available household resources is flawed and has hampered gender-sensitive research. Ideally, personal-level wealth data should be considered to fully understand how divorce stratifies wealth for men and women.

In the present study, I use unique longitudinal, personal-level wealth data from the German Socio-Economic Panel Study (SOEP v35; 1984-2018) to address these gaps about the mechanisms behind divorce-related wealth stratification for men and women. More precisely, I answer three research questions: (a) How does divorce disrupt wealth trajectories compared to trajectories of continuously married individuals? (b) To what degree does selection into divorce matter in the comparison of wealth trajectories between divorcees and the married? And (c) are these processes gendered? Defining wealth trajectories as unique wealth accumulation pathways that unfold over time, I utilize random-effects growth models to empirically explore how both immediate divorce penalties and potentially deteriorated wealth accumulation up until 30 years after divorce explain previously highlighted wealth differences between the continuously married and ever-divorced in late working age. Matching divorced and continuously married respondents on pre-divorce differences using a combination of (coarsened) exact and nearest neighbor propensity score matching prior to the outcome regression, allows me to better account for selection into divorce in the subsequent regression. Thus, regression results provide compelling and causal evidence on wealth trajectory differences between divorcees and the married. Re-running the outcome regression without

accounting for pre-divorce differences and comparing results across the two approaches, additionally provides evidence on the relevance of selection effects in the estimation of wealth trajectory differences. Using a matching approach has the added methodological benefit that I can assign divorcees' date of divorce to their married matches and thus generate a common time scale (i.e., time since (assigned) divorce) for ease of comparison (see van Scheppingen and Leopold (2020) for a similar approach). Finally, the German data are uniquely equipped to enable a gender-sensitive analyses of wealth trajectories because the data provide comprehensive wealth information at the personal level including each adult household members' share of jointly owned wealth over four survey waves (2002, 2007, 2012 and 2017).

Next to the unique methodological feature of German data, Germany is an intriguing setting to study divorce-related wealth stratification through a gender sensitive lens. Compared to the U.S., where state policies encourage married women's employment, German policies commonly favor a traditional division of labor within marriage leading to substantial within-couple economic inequalities (Aisenbrey and Fasang 2017, Grabka, Marcus and Sierminska 2015, Musick, Bea and Gonalons-Pons 2020, Trappe and Sørensen 2006). Contrasting the financial dependence between partners during marriage, German family law emphasizes financial self-reliance of ex-spouses in case of divorce. Future needs are not considered in the division of property at divorce and access to spousal alimony is restricted. Thus, German women's income situation – compared to German men – has been shown to be particularly volatile immediately at divorce and in years after while gender differences have been found to be less severe in other countries such as the U.S. (Andreß et al. 2006, Bayaz-Ozturk et al. 2018).

THE RELEVANCE OF PRIVATE WEALTH AND ITS ACCUMULATION

A range of saving motives that highlight the different functions of wealth have been discussed in the literature (e.g., Keister and Moller 2000, Pfeffer 2011, Spilerman 2000). First and

foremost, individuals are expected to save out of precautionary interests. Access to sufficient wealth can provide a real and subjective private safety net that may be drawn from to smooth expected and unexpected earning shocks (e.g., retirement, unemployment, care-related employment interruptions, etc.). Next to precautionary savings, individuals are also assumed to save out of motives related to social prestige, status and power, or the transfer of resources and their advantages to important others (e.g., children, spouse). Individual saving is thus driven by the desire to generate, maintain or improve one's own or others subjective wellbeing and financial security over the life course.

The wealth accumulation occurs through three pathways: *surplus income*, *financial transfers*, and *wealth appreciation* (e.g., Keister and Moller 2000, Killewald, Pfeffer and Schachner 2017, Spilerman 2000). First, surplus income may be saved instead of consumed, which is a particularly relevant channel of wealth accumulation early in life. Second, wealth may be obtained through financial transfers such as *inter vivos* or inheritances. Whereas *inter vivos* are particularly likely to occur at socially meaningful life course events (e.g., childbirth, marriage, etc.), inheritances naturally matter most later in life. Finally, wealth itself generates exponentially more wealth through capital appreciation and compounded interest effects. Thus, as wealth increases and is diversified, relevance of the last wealth accumulation channel raises. Whereas wealth levels are commonly low or even characterized by indebtedness right after education is completed, wealth levels are expected to increase throughout the working life until levels reach their peak in preparation for retirement. This may be considered the normative wealth accumulation pathway.

HOW DIVORCE MAY DISRUPT WEALTH ACCUMULATION TRAJECTORIES

Wealth trajectories naturally differ between individuals based on unique opportunity structures and constraints of financial decision-making based, *inter alia*, on educational achievements,

occupations, race, or family of origin characteristics (Killewald, Pfeffer and Schachner 2017). Additionally, the anticipated wealth trajectory (i.e., on average rising wealth levels throughout the working life) can be substantially disrupted and altered through certain life course events, often denoted ‘turning points’ (Abbott 1997, Abbott 2001). Marital dissolution may be considered such an event, or shock, that has not only the potential to immediately and drastically change the wealth levels around the event, but to also permanently alter the conditions of gaining or maintaining wealth in the future compared to continuously married individuals. Figure 1 graphically depicts this theoretical idea, which will be discussed in more detail in the following sections.

Initial level effect: Immediate wealth level disruptions associated with divorce

Marital dissolution is likely associated with distinct, immediate changes in wealth levels due to increased financial demand and a range of wealth-relevant burdens (Kapelle and Baxter 2021, Zagorsky 2005). First, direct expenses of the divorce proceedings relate to administrative divorce costs (i.e., court fees and solicitor fees) which commonly increase with the complexity of the divorce case and value in dispute. These costs can be substantial. In the US, for instance, administrative divorce costs can easily exceed the yearly household income of the former couple (Henry et al. 2011). Second, legal divorce requires the division of marital assets (i.e., wealth accumulated during the marriage excluding personal inheritances and gifts). Whereas some assets may be easily divided, such as savings in a bank account, other assets can be indivisible and liquidation may be necessary. This is particularly likely for the family home, which is predominately jointly owned and constitutes the major share of the marital wealth portfolio (Thomas and Mulder 2016). As spouses often lack sufficient cash collateral to buy out the other partner or are unable to qualify for a mortgage by themselves, housing property is regularly sold when spouses divorce (Mikolai et al. 2019). Property sales incur direct costs such as notary and real estate fees, or early repayment charges for premature terminations of

mortgage contracts. Additionally, property sales – but also sales of other assets such as shares – may be associated with indirect costs of wealth depreciation if assets need to be sold under time pressure and in a market unfavorable to the seller. Third, requirements for legal divorce often force at least one spouse to relocate before the divorce proceeding can commence to demonstrate the physical separation of spouses.ⁱ As living in the formerly marital home without the partner can be costly, budget constraints often force both spouses to eventually relocate to a more affordable dwelling (Mikolai, Kulu and Mulder 2020). Relocation not only generates additional costs, it also restricts the access to the partner's resources and sharing of costs.

Although divorcing spouses likely receive financial support from their parents, if parents are financially capable to help (Leopold and Schneider 2011), *inter vivos* transfers in combination with divorcing individuals' personal incomes are unlikely to fully compensate all divorce-related costs and prevent wealth declines. Indeed, previous research showed that marital dissolution is associated with a relatively abrupt and substantial decline in wealth levels compared to pre-divorce levels (Kapelle and Baxter 2021, Zagorsky 2005). As continuously married individuals do not experience similar financial burdens, I expect that *divorcees hold substantially less personal wealth than otherwise comparable, continuously married individuals in the year of divorce (Initial level hypothesis)*.

Long-term development: Wealth accumulation after divorce

The unfavorable wealth position of divorcees right after divorce compared to the position of continuously married individuals may pose a relative disadvantage to divorcees. On average more affluent married individuals would over-proportionally benefit from exponential wealth growth over time based on compounded interest effects or asset appreciation – the third channel of wealth accumulation. Thus, initial divorce-induced wealth inequalities themselves become a

detriment that theoretically lead to a systematic divergence of divorcees' wealth accumulation trajectories compared with those of continuously married individuals.

Furthermore, divorce may lead to restricted exposure to certain economic advantages, which may additionally inhibit divorcees' wealth accumulation over time. While continuously married couples benefit from marital wealth premiums, including economies of scale or long-term joint saving incentives (Lersch 2017, Wilmoth and Koso 2002), divorcees lack these benefits of first marriage. Furthermore, divorcees may be bound to their ex-spouse through financial ties (e.g., child and spousal alimony), which reduce divorcees surplus income that can be saved. Thus, I hypothesize that *divorcees' yearly wealth accumulation rate is lower than the rate of continuously married respondents (Growth rate hypothesis)*, leading to a growing gap between divorcees and first-time married spouses.

First empirical support for the idea of a growing wealth divide between divorcees and the married was provided by Zagorsky (2005), who found that un-partnered divorcees had lower yearly saving rates compared to continuously married respondents – 14 and 16 percent, respectively. However, considering only un-partnered divorcees' wealth accumulation rates, his study relied on a selective sample of divorcees and neglected potential advantages associated with remarriage. Remarriage likely restores some marriage advantages. For instance, remarried divorcees can benefit from improved economies of scale which may increase surplus income that can be saved. However, weaker beliefs about the longevity of higher-order relationships and previous experiences of a divorce are linked to more financial independence within these higher order partnerships, which decreases the likelihood of – commonly more efficient – joint investments (Burgoyne and Morison 1997, Kan and Laurie 2014). Thus, even remarried divorcees may not close the initial wealth gap generated as a result of their previous divorce. Nevertheless, it can be expected that divorcees' ability and motivation to save is

moderated by remarriage with *remarried divorcees experiencing higher wealth accumulation rates than unmarried divorcees (Remarriage growth rate hypothesis)*.

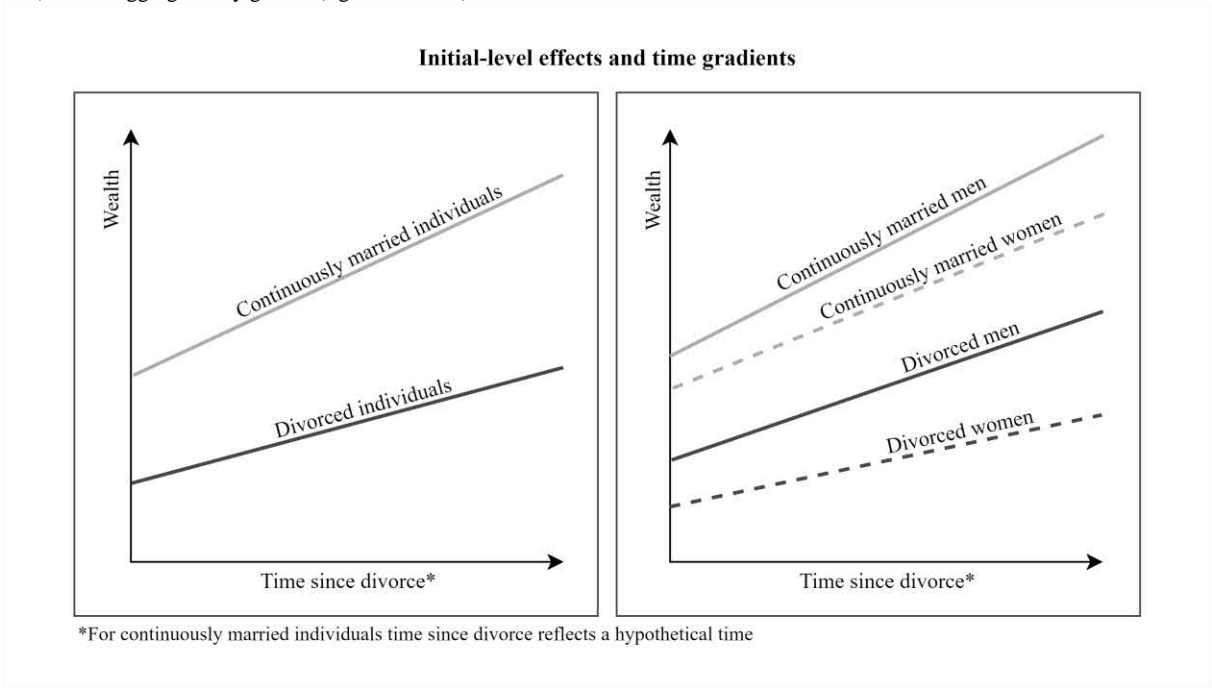
Gender differences in the initial level effect and long-term development

A growing body of research has highlighted substantial within-marriage wealth inequalities to the disadvantage of wives (Grabka, Marcus and Sierminska 2015, Kapelle and Lersch 2020). These inequalities stem for instance from a pervasive gender pay gap and women's lower access to wealth-relevant fringe benefits, which is exacerbated through marriage due to persistently traditional arrangements of paid and unpaid labor during marriage (Budig and England 2001, Chang 2010, Cheng 2016, Killewald and Gough 2013). Additionally, research has highlighted age differences (i.e., men's longer time spent in the labor market compared to women prior to the marriage) as well as gender differences in investment and spending as relevant contributors to the within-marriage wealth gap (Alesina, Lotti and Mistrulli 2013, Fisher 2010, Pahl 2005). Finally, *inter vivos* transfers, which are legally not regulated, may also over-proportionally favor husbands (i.e., sons) to endorse men's normative entitlement to relevant family-of-origin assets (e.g., property or businesses) (Bessièrè 2019, Deindl and Isengard 2011).

Depending on the origin of within-couple wealth inequalities, divorce maintains or potentially exacerbates these inequalities. In the majority of Western societies, pre-marital wealth as well as personal inheritances and *inter vivos* received during the marriage commonly remain (largely) untouched in the divorce-related equalization process meaning that inequalities in these wealth components are maintained. Although marital wealth (i.e., wealth accumulated during the marriage excluding personal inheritances or gifts) should *de jure* be divided equally or equitably – depending on the county –, *de facto* arrangements may disadvantage women. Husbands are commonly perceived to be entitled to a larger share of marital wealth due to their, on average, higher economic contribution and an overall undervaluation of women's unpaid

labor (Hersch and Shinall 2020). These ideas have been found to be reflected in gender-biased practices of family courts and divorce lawyers with endorsements of these practices by divorcees themselves across different country-contexts (For France: Bessi re 2019, For the USA: Wenig 1990). As a result of a potentially gender-biased division of marital wealth, it can be anticipated that *the initial wealth gap between men and women is larger for divorcees than the married (Gendered initial level hypothesis).*

Figure 1 Hypothetical average wealth trajectories of continuously married and divorcees – pooled for men and women (left-hand side) and disaggregated by gender (right-hand side).



Gender is likely also a relevant moderator of wealth accumulation differences over time. Within marriage, wives’ lower wealth accumulation potentials may to some degree – but not fully – be compensated as the majority of married spouses pool and share a substantial proportion of their resources (Amuedo-Dorantes, Bonke and Grossbard 2011). However, after divorce, voluntary financial cooperation between ex-spouses likely ceases and disparities in wealth accumulation potentials are no longer compensated. This is exacerbated by the fact that children commonly stay with their mother after divorce. Although post-divorce alimony and child support – if children are present – may cover some of the economic disadvantages, these payments are often considered insufficient with under-payment or non-payment common issues (Skinner et al.

2017). Although remarriage can restore some of the economic advantages of marriage (Jansen, Mortelmans and Snoeckx 2009), men are more likely to remarry and do so quicker than women (Coleman, Ganong and Fine 2000, Di Nallo 2018). Overall, it can thus be expected that *wealth accumulation disparities are more severe between divorced men and women than between married men and women (Gendered growth rate hypothesis)*.

WEALTH-RELATED SELECTION INTO DIVORCE

Initial wealth level differences between divorcees and the continuously married as well as differences in wealth accumulation rates between the two groups may substantially be determined by inherent differences between the two groups (i.e., selection into divorce or continuous marriage). Previous research on the determinants of marital stability highlighted a range of predictive couple characteristics including, *inter alia*, the prevalence of financial issues, and spouses' socio-economic background including parental separation (Amato 2010). An incipient body of research has also examined how couples' ownership and level of different wealth components act as predictors of divorce. While ownership of liquid and illiquid assets as well as secured debts was associated with a decreasing likelihood of separation and divorce (Eads and Tach 2016), large unsecured debt and consumer debt increased couples' likelihood to experience a marital dissolution (Dew 2011, Eads and Tach 2016).

Overall, the likelihood to experience a divorce is not randomly distributed amongst the married, but partially determined by wealth-relevant characteristics and couples' wealth levels and portfolio. Thus, pre-divorce differences may determine the magnitude of initial wealth differences between divorcees and the married as well as differences in their wealth accumulation over time. I hypothesize that *the omission of effects of selection into divorce from the analysis of wealth disparities between divorcees and the married leads to an over-estimation of these differences (Selection hypothesis)*.

THE GERMAN CONTEXT

Given that this study draws on German data, it is important for the interpretation of results to understand the specific German context that may influence wealth stratification processes associated with divorce and gender.

Economic inequalities. Although formal measures of economic wellbeing and security have placed Germany on average on a secure footing, economic inequalities have been soaring in recent decades with Germany ranking amongst the EU countries with the highest wealth inequalities (European Central Bank 2020, Piketty 2014). At the same time, households' and individuals' ability to accumulate sufficient wealth has become a critical issue as Germany's government has increasingly emphasized personal responsibility to ensure reasonable living standards throughout the life course amid an ageing population and rising economic pressure on the government (Ebbinghaus 2015).

Marriage premium. Amongst other factors, wealth accumulation in Germany is strongly linked to marriage. Strong normative expectations around joint savings within marriage are endorsed through institutional structures and privileges for the married compared to non-married (i.e., singles or cohabiters). Married spouses, but not cohabiters, can financially benefit from favorable taxation (i.e., joint taxation with income splitting) or joint insurances (e.g., health care insurance) and pensions (Bach, Haan and Ochmann 2013, Härtel 2001).

Economic gender differences. Whereas structural privileges for the married increase their surplus income and savings potential, they also rest on the notion of traditional gender roles and specialization within marriage. This has provided strong incentives for German wives to reduce their work hours and resulted in comparatively low rates of full-time employment amongst women (Aisenbrey and Fasang 2017, Trappe, Pollmann-Schult and Schmitt 2015). In combination with occupational segregation and undervaluing of jobs within female-dominated

industries and occupations, traditional family arrangements have carried a significant penalty for German wives' relative earnings and wealth (Grabka, Marcus and Sierminska 2015, Statistisches Bundesamt 2020, Trappe and Sørensen 2006).

Divorce the German way. Germany's divorce rate per 100 marriages (2019: 37) is comparable to the US rate (CDC 2018, Eurostat 2018). To legally divorce, Germany requires spouses to live separately for at least 12 months prior to divorce. At divorce, the default German matrimonial property regime emphasizes a *de jure* equal division of marital property through an equalization of accrued gains. This means that the wealthier spouse is required to make an equalization payment to the less wealthy spouse amounting to half the difference in accrued gains. The divorce proceeding itself incurs substantial, although capped and regulated, administrative cost (i.e., court fees and solicitors' fees) that increase with the complexity of the case and the level of financial value of goods and property in dispute.

The strong institutional support for women's economic reliance on husbands during marriage stands in contrast to Germany's legal emphasis on independence between spouses after divorce. For instance, the division of marital property does not consider the future need of the economically less advantaged spouses (i.e., commonly the wife) as is common in some US states (Voena 2015). Furthermore, post-divorce spousal alimony regulations emphasize the principle of financial self-sufficiency with alimony temporarily limited – if granted at all. For divorces that involve dependent children, monetary child support must be paid by the non-residential parent – commonly the father. Nevertheless, only a minority of residential parents receive child support from their ex-partner and only half of all payments are sufficient (Bröckel and Andreß 2015). Overall, German women have been found to experience high and lasting financial volatility after divorce compared to German men partially due to inequalities generated during marriage. German women also fare worse compared to women in other countries such as the US or the UK (Andreß et al. 2006, Bayaz-Ozturk et al. 2018).

DATA AND METHODS

Data

I used longitudinal panel data from the German Socio-Economic Panel (SOEP, v35 <http://doi.org/10.5684/soep-core.v35>; Goebel et al. (2019)). The SOEP is a representative panel study of German households that commenced in 1984 and has since been extended and refreshed several times. The data are well suited for the analysis of wealth trajectories of divorcees and continuously married including a gender sensitive approach, as they include (a) retrospective marital biographies that are updated yearly with prospective data, (b) comprehensive measures of *personal* wealth in four survey waves – 2002, 2007, 2012, and 2017, and (c) a wide range of other relevant covariates.

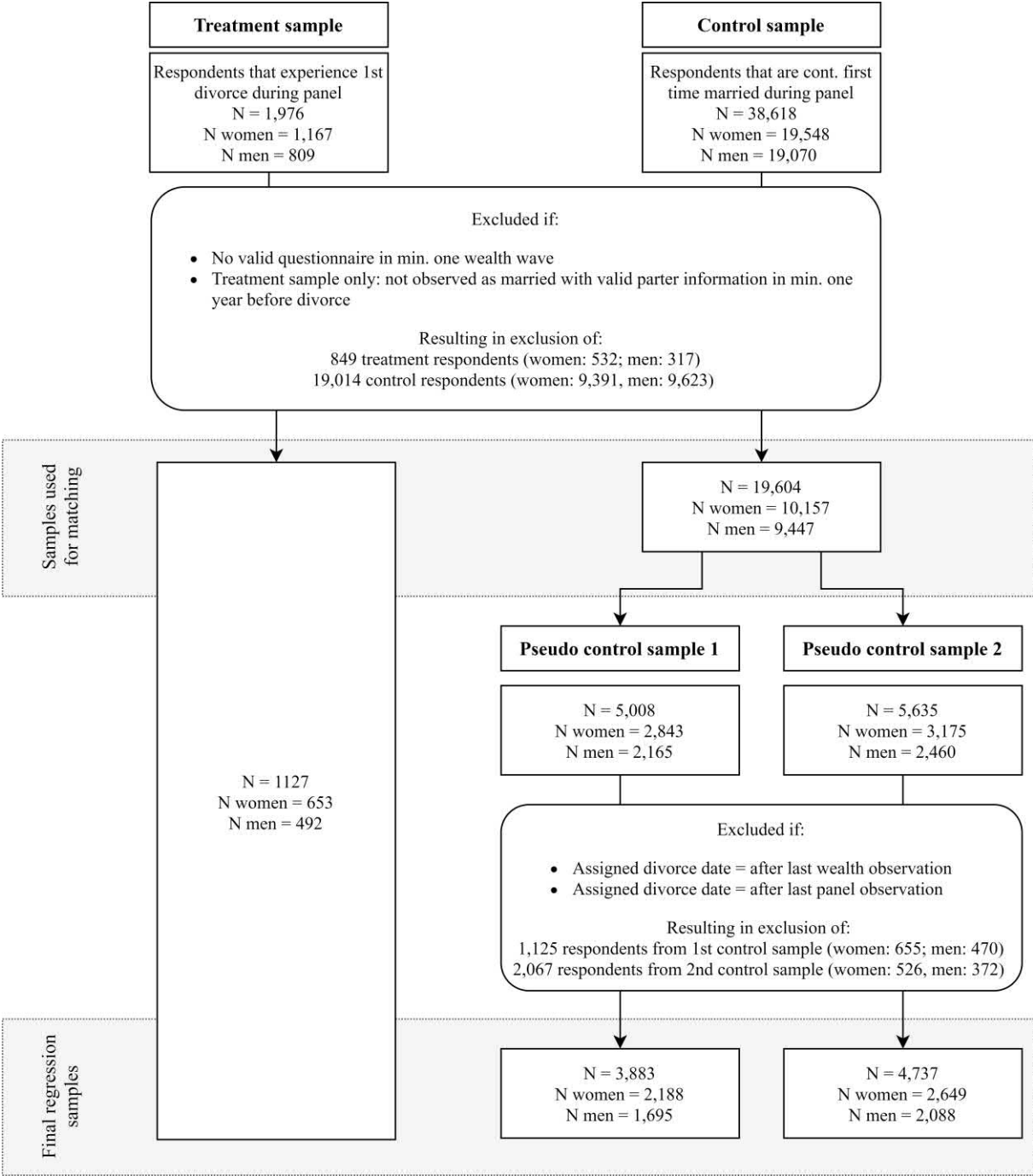
Around 39 percent of wealth data were edited and/or imputed by the SOEP survey team using a multi-step process including the logical and computational imputation of missing values (Grabka and Westermeier 2015).ⁱⁱ To deal with missing values on other relevant variables, I built on the five imputation sets provided by the SOEP team and additionally multiply imputed analytical variables and auxiliary variables using Stata's *mi* command under the assumption of missing at random. A full list of variables considered in the imputation process including the number and percentage share of imputed cases for each variable is provided in Table A.1 in the Online Appendix. Compared to wealth data, the percentage share of missing values on relevant non-wealth variables was rather low and ranges between 0 to 3 percent for most variables. Only parental education and partner's parental education had a comparatively high share of missing values with 10 to 11 percent, respectively. Estimation results from each imputed set of data were combined using Rubin's rule (Rubin 1987).

Sample selection and matching

The following section will elaborate the sample selection and generation process including the matching of samples. As this process is rather complex Figure 2 provides a graphical representation of the process for ease of comprehensibility.

Initial sample selection. In the first step of the sample selection process, I generated two samples: the treatment sample and the preliminary control sample. For the treatment sample, respondents were selected if they experienced a divorce from their first marriage during panel participation, were observed as being married in at least one survey year before their divorce with valid partner information in this year, and provided a valid questionnaire in at least one wealth survey year (i.e., 2002, 2007, 2012, 2017) after their divorce. In years after first divorce, respondents might have been partnered or un-partnered. The preliminary control sample included respondents that had been observed as continuously first-time married during their panel participation and provided valid information in at least one wealth-relevant survey year. Respondents of both groups were either married when first observed within the SOEP or entered their first marriage during their panel participation. To connect the present study to the previously discussed studies that focused on wealth of ever-divorced individuals in late working age (e.g., Zissimopoulos, Karney and Rauer 2015) and reduce the influence of wealth-declines associated with retirement, individuals were no longer followed once they were aged 61 years or older. Additionally, respondents were no longer followed once they experienced the death of their spouse as widowhood has a profoundly different impact on the financial standing of the surviving spouse compared to divorce (Zick and Holden 2000). These first sample selection criteria resulted in a treatment sample of 1,127 individuals (635 women and 492 men) and a preliminary control sample of 19,604 individuals (10,157 women and 9,447 men).

Figure 2 Sample selection and generation process diagram.



Generation of a pseudo control group. The second step of the sample selection process involved the generation of a pseudo control group using a matching approach. This means that from the initial 19,604 continuously married respondents those respondents were selected that resembled treatment group respondents on relevant pre-divorce characteristics. To this end, I used nearest neighbor propensity score matching in combination with (coarsened) exact matching. I first

predicted the likelihood to experience a divorce (i.e., propensity score) by regressing 33 covariates on the binary treatment variable ‘divorce’.ⁱⁱⁱ The predictors of divorce referred to respondents’ basic demographics, family of origin characteristics, household living arrangements and health status of members, the financial situation of respondents, respondents’ partner characteristics, and a range of annually collected household-level wealth ownership indicators and a categorical indicator of the household’s value of capital gains (see Table A.2 in the Online Appendix for the full list of measures). I identified three variables that seemed particularly relevant for the purpose of the present study and subsequently included these variables into the matching process through (coarsened) exact matching: the year of marriage^{iv}, age bracket^v, and gender. Each divorcee was matched to up to five best matches (i.e., nearest neighbor algorithm) of the control group based on the logit of the propensity score using calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score with (coarsened) exact matching on the above mentioned three covariates (Austin 2011, King and Nielsen 2019). Nearest neighbor matching was conducted with replacement, meaning that respondents in the control sample were included more than once. This guaranteed that each divorcee could be matched to the most appropriate nearest controls, even if these control respondents were already included in a previous match (Dehejia and Wahba 2002). In total, the 1,127 divorcees were matched to 5,008 continuously married control sample individuals. Due to the matching with replacement, 2,568 control respondents were matched to one divorcee, 1,394 were matched to two divorcees, 639 were matched to three divorcees and 407 were matched to four or more divorcees. This matching approach generated a pseudo control sample that largely mirrored the treatment sample in a wide range of covariates (see the Online Appendix for further detail on the quality of the matching). For the subsequent outcome regression this had the advantage that baseline differences between the two groups were adjusted for providing more accurate and causal predictions of wealth differences in the year of divorce and in years after divorce.^{vi}

To understand to what degree selection into divorce potentially mattered for a comparison of divorcees and continuously married respondents' wealth trajectories, I generated a second pseudo control group that deliberately did not account for baseline differences between the treatment and control group. To this end, I solely used (coarsened) exact matching on the year of marriage and gender^{vii}, and randomly selected up to five control group respondents for every treatment respondent resulting in a second pseudo control group of 5,635 individuals. As for the first pseudo control group, each preliminary control group respondent could be selected as a match several times. Thus, for the second pseudo control group 3,891 control respondents were matched to one divorcee, 1,404 were matched to two divorcees, 312 were matched to three divorcees and 28 were matched to four divorcees. Re-running regressions with this second pseudo control group and comparing results to regression results that used the first pseudo control group provided an indication of whether selection into divorce explains some of the wealth disparities between the continuously married and divorced.

Assignment of divorce dates to pseudo control group respondents and final regression sample restrictions. For the subsequent growth model, respondents in the two pseudo control groups were assigned the divorce date of their treatment matches. Using respondents' (assigned) divorce date, I generated a continuous variable that tracked years since divorce (see van Scheppingen and Leopold (2020) for a similar approach). Years prior to first (assigned) divorce were dropped, as the subsequent outcome analysis focused on wealth trajectories after divorce. Additionally, the matched sample had to be restricted to survey years that contained wealth information (i.e., years 2002, 2007, 2012, and 2017). In some cases, the assigned divorce date was after the last wealth observation or the last valid panel observation, which resulted in a loss of 1,125 respondents in the first pseudo control group and a loss of 898 respondents in the second pseudo control group.^{viii} Overall, the regression sample consisted of the initial 1,127 divorcees (635 women and 492 men) with 2,067 individual-year observations, a first pseudo

control group with 3,883 (2,188 women and 1,695 men) respondents with 7,708 individual-year observations, and a second pseudo control group with 4,737 (2,649 women and 2,088 men) respondents with 7,718 individual-year observations. The regression sample was unbalanced, with 50.53 percent of respondents providing valid information in at least two out of the possible four wealth waves (28.02 percent of respondents are observed twice, 13.82 percent three times, and 8.68 percent four times).^{ix} Table A.3 and A.4 in the Online Appendix provide descriptions of the samples using imputed and non-imputed data.

Outcome regression measurements

Outcome variable. My outcome measure, *personal net wealth*, was defined as the sum of all personally owned assets minus personally owned liabilities. Assets included the personal asset value held in real estate, financial assets (e.g., savings balance, shares, or bonds), life insurance, private pension plans, business assets, and valuable assets (e.g., gold, jewelry). Liabilities covered mortgage debt and consumer credits. SOEP wealth data have been collected separately for each household member aged 17 and older in a three-step process: (1) a filter question is used to assess whether a respondent holds a certain wealth component; (2) the market value of held wealth components is recorded; and (3) for wealth components that may be held jointly (e.g., real estate), respondents are asked to indicate whether they hold these wealth components solely or jointly and – in the case of joint ownership – provide the share they co-owned. My outcome measure thus explicitly included the personal share of any assets and liabilities that were owned with other individuals.

Just like income data, wealth data are highly skewed, but common transformation such as the natural log are not-suitable as wealth measures contain zero and negative values that would be dropped by a log-transformation (Killewald, Pfeffer and Schachner 2017). Two alternative transformations have been suggested in the literature: an inverse-hyperbolic sine transformation

(IHS) or rank-based transformation (e.g., Friedline, Masa and Chowa 2015, Killewald, Pfeffer and Schachner 2017). For the current study, a rank-based transformation was most appropriate as it efficiently accounts for periodic changes (e.g., financial crisis), which was relevant for the time-related focus of the current study. The rank measure was calculated separately for each survey year, but jointly for men and women and ranges from 0 to 100. For the interpretation of results regarding the growth rate, it needs to be noted, that a one rank point increase refers to larger absolute wealth differences at the top of the distribution while a one rank point difference at the bottom of the wealth distribution refers to smaller absolute wealth changes. Similar specifications apply to other common transformations. Results using the IHS-transformation, which were consistent with rank-transformed results, are provided in the Online Appendix (Table A.5).^x

Explanatory variables. To model wealth growth trajectories over time after divorce, I first generated a continuous variable to measure *time since (assigned) divorce*. This variable started with 0, representing the year of divorce, and increased by 1 for each year since first divorce. For the two pseudo control groups, this variable represented an artificial count since their assigned divorce date. Time since divorce covered up to 30 years, although the sample size was reduced during later years after divorce (see Table A.6 in the Online Appendix for an overview of cell sizes for years since divorce).^{xi} For the regression analyses, time since divorce was included as a linear term.^{xii} To distinguish and assess wealth trajectory differences between the control and treatment group, I generated a dummy variable to tag respondents with and without an actual *divorce experience* (0 = control, 1 = treated). To examine whether *remarriage* moderates wealth trajectories of divorcees, I used a dummy variable (0 = not observed as remarried, 1 = remarried). Finally, for the assessment of potential gender differences, I generated a *gender* dummy (0 = male, 1 = female).

Control variables. The multivariable regression models were controlled for two time-changing covariates. To account for potential under-reporting of personal wealth in the first observed wealth wave (see Fisher (2019) for a discussion on under-reporting of income measures in panel data), I included a flag for respondents' first wealth observation. Additionally, I flagged imputed wealth data using a dummy variable. Other covariates were not included as baseline differences between the treatment and control groups were adjusted for in the matching approach for the first pseudo control group or were deliberately not adjusted for within the generation of the second pseudo control group.

Multivariable random-effects growth model

After the selection of the treatment group and generation of appropriate pseudo control groups, I empirically test my hypotheses. To briefly recall all empirical expectations, Table 1 provides a summary of the hypotheses.

I used random-effects growth models with random intercept and random slope to predict initial-level personal wealth and yearly personal wealth increases over time for divorcees and the continuously married (Singer and Willett 2003). These models were most suitable as they can deal with the nested structure of the data, but can also handle unbalanced data and unequal spacing or numbering of measurements across respondents (Singer and Willett 2003:140ff).

I commenced the analysis using the first pseudo control group and pooled data across men and women. I specified the following general model:

$$WEALTH_{it} = [\gamma_{00} + \gamma_{10}DIVTIME_{it} + \gamma_{01}DIV_i + \gamma_{11}(DIV_i * DIVTIME_{it}) + \gamma_{0k}C_{it}] + [\zeta_{0i} + \zeta_{1i}DIVTIME_{it} + \varepsilon_{it}]$$

The first parenthesis contains the structural component of the model, while the stochastic component is represented within the second parenthesis. $WEALTH_{it}$ is the personal wealth of

respondent i at time t . The average intercept is captured by γ_{00} with the random component ζ_{0i} . The random component represents individual-specific variation in the intercept that is unexplained due to unobserved characteristics of individuals. $DIVTIME_{it}$ represents the years since (assigned) divorce. The related average growth slope over time is denoted γ_{10} , which may vary across individuals and is captured by ζ_{1i} . I allowed the random components, ζ_{0i} and ζ_{1i} , to be correlated. This means that time-constant respondents' characteristics could simultaneously modify the intercept (i.e., initial level) and slope (i.e., growth rate) of personal wealth. I further included a dummy, DIV , that identifies whether respondents belong to the pseudo control group (i.e., continuously married) or the treatment group (i.e., divorced) with the corresponding coefficient γ_{01} . This means that the term γ_{00} relates to the average intercept of the control group, whereas γ_{01} describes the treatment group's variation from the average intercept providing an indication for my *H1: Initial level hypothesis*. Additionally, I included an interaction between years since divorce and the treatment dummy, $DIV_i * DIVTIME_{it}$, with the corresponding coefficient γ_{11} ; this subsequently relates to the treatment group's slope variation from the control group's intercept, γ_{10} . The inclusion of the interaction thus allowed me to test *H2a: Growth rate hypothesis* (i.e., yearly wealth increases). Finally, C_{it} is the set of k control variables.

To test my *H2b: Remarriage growth rate hypothesis*, I included a three-way interaction between time since divorce, the divorce dummy, and an indicator for ever remarried after divorce into my initial model. Note that continuously married respondents are naturally never remarried and thus some of the interaction predictions fell out of the model.

Next, I induced a three-way interaction between time since divorce, the divorce dummy, and gender into my initial model to address how gender moderates wealth trajectories of divorcees and the married. Results of this model in combination with Wald tests enabled an empirical

assessment of *H3a: Gendered initial level hypothesis* and *H3b: Gendered growth rate hypothesis*.

In a last step of my analysis, I re-ran the initial model and gender-interaction model using the second pseudo control group, which did not adjust for baseline differences (i.e., selection) between divorcees and the married. Comparing results from this last set of analyses to the main analyses, allowed me to address *H4: Selection hypothesis* and provide an indication to what degree selection into divorce mattered for disparities in wealth trajectories between married and ever-divorced respondents.

Table 1 Overview of hypotheses

<i>Initial level effect: Immediate wealth level disruptions associated with divorce</i>	
<i>Initial level hypothesis</i>	In the year of divorce, divorcees hold substantially less personal wealth than otherwise comparable, continuously married individuals.
<i>Long-term development: Wealth accumulation after divorce</i>	
<i>Growth rate hypothesis</i>	In years after divorce, divorcees accumulate wealth at a lower rate compared to continuously married individuals.
<i>Remarriage growth rate hypothesis</i>	Remarried divorcees experiencing higher wealth accumulation rates than unmarried divorcees.
<i>Gender differences in the initial level effect and long-term development</i>	
<i>Gendered initial level hypothesis</i>	The initial wealth gap between men and women is larger for divorcees than for the married.
<i>Gendered growth rate hypothesis</i>	Disparities in wealth accumulation rates are more severe between divorced men and women than between married men and women
<i>Wealth-related selection into divorce</i>	
<i>Selection hypothesis</i>	The omission of selection effects leads to an over-estimation of disparities in men's and women's wealth trajectories between the married and divorced

Estimation results for all random-effects growth curve models are provided in Table 2 and will be discussed in detail in the following results section.

RESULTS

Initial wealth disparities and differences in yearly wealth accumulation rates

First, I hypothesized that in the year of divorce, divorcees hold substantially less personal wealth than otherwise comparable, continuously married individuals due to divorce-related wealth depletions (*Initial level hypothesis*). In line with these expectations, I find that divorcees rank 13.53 points lower in the overall wealth distribution in the year of divorce compared to married respondents. Adjusted for covariates included in the regression, married respondents have an average predicted initial personal wealth rank of 49.94. The difference between wealth levels of the married and divorced at the time of (assigned) divorce is substantial and statistically significant.

Further, I expected that divorcees accumulate wealth at lower yearly rates compared to their continuously married counterparts due to divorcees' restricted access to wealth-accumulation related benefits (*Growth rate hypothesis*). Lower yearly wealth increases amongst divorcees would lead to a growing gap between the married and divorced over time. Whereas the married increase their personal wealth by approximately half a rank point per year (0.55), which is a statistically significant increase, divorcees' yearly personal wealth increase of 0.71 (0.55+0.16) rank points is slightly higher at the average compared to continuously married respondents. Statistically, differences in growth rates between divorcees and the married are not significant and considering the large initial gap, the higher wealth increase for divorcees at the average is unlikely sufficient to close the initial gap over time. Additionally, the previously mentioned specifics of the rank transformation should be acknowledged when interpreting these rank-based wealth accumulation differences particularly since the two groups have substantially different starting values (i.e., initial wealth levels at divorce). Thus, it is unlikely that the small

rank differences in yearly rank wealth increases translate into substantial differences in untransformed wealth. Thus, my results do not seem to support the *Growth rate hypothesis*.

The first model considered divorcees as a homogeneous group although divorcees likely differ substantially in their wealth accumulation depending on whether they remarry or stay unmarried (*Remarriage growth rate hypothesis*). Including an interaction between time since divorce, the divorce dummy, and an indicator for ever remarried after divorce into the regression model highlights that remarried divorcees indeed show substantially and statistically significant higher yearly rank wealth increases after divorce than unmarried divorcees. Precisely, remarried divorcees increase their wealth rank by 0.95 points per year ($0.55+(-0.17)+0.57$), whereas unmarried divorcees experience yearly average rank increase of 0.38 ($0.55+(-0.17)$). Remarried divorcees even show higher yearly rank-transformed wealth increases than the continuously married. Thus, remarriage may be associated with a narrowing of the initial divorce-related wealth gap. For never-remarried divorcees, I find that the initial gap in fact would increase marginally over time – although growth rate differences to the married are statistically not significant. Thus, in line with my *Remarriage growth rate hypothesis*, remarriage moderates divorcees' post-divorce wealth growth rates.

Gender-specific effects in initial wealth disparities and differences in yearly wealth accumulation rates

Previous research has highlighted substantial gender differences in wealth disparities at older age between and within the groups of married and ever-divorced individuals (e.g., Lersch 2017, Wilmoth and Koso 2002, Zissimopoulos, Karney and Rauer 2015). Based on notions around the gender wealth gap and gender-biased practices during the marriage and in the case of divorce, it is relevant to explore potentially relevant gender differences in wealth trajectories of divorcees and the married.

Table 2 Linear random-effects growth curve models of personal net wealth (rank-transformed)

	Treatment & first pseudo control group: Accounted for selection into divorce			Treatment & second pseudo control group: Not accounted for selection	
	Base B/(SE)	Base + Remarriage B/(SE)	Base + Gender B/(SE)	Base B/(SE)	Base + Gender B/(SE)
Divorce duration (in years)	0.550*** (0.066)	0.546*** (0.066)	0.527*** (0.107)	0.389*** (0.062)	0.379** (0.104)
Divorced (Ref: cont. married)	-13.532*** (1.105)	-14.010*** (1.303)	-12.581*** (1.986)	-17.255*** (1.204)	-17.109*** (2.045)
Divorced X divorce duration	0.159 (0.128)	-0.169 (0.167)	0.136 (0.217)	0.333* (0.129)	0.305 (0.211)
Remarried (Ref: unmarried)		2.667 (2.281)			
Remarried X divorce duration		0.569* (0.231)			
Female (Ref: Male)			-4.723** (1.389)		-6.145*** (1.290)
Female X divorce duration			0.012 (0.123)		0.005 (0.135)
Female X divorced			-1.784 (2.703)		-0.318 (2.851)
Female X divorced X divorce duration			0.042 (0.281)		0.041 (0.288)
Intercept	50.337*** (0.778)	50.407*** (0.778)	53.130*** (1.236)	53.807*** (0.793)	57.357*** (1.157)
<i>Variance components:</i>					
Slope	0.585 (0.275)	0.574*** (0.276)	0.585*** (0.275)	0.323*** (0.305)	0.314 (2.351)
Intercept	20.836 (0.742)	20.829*** (0.745)	20.677*** (0.727)	21.653*** (0.748)	21.444*** (0.771)
Covariance	-16.464 (19.105)	-17.494 (18.771)	-16.374 (19.510)	-9.112 (44.354)	-6.687 (77.522)
Residuals	18.479 (0.394)	18.476*** (0.394)	18.478*** (0.395)	18.513*** (0.292)	18.511*** (0.293)
N Observations	9775	9775	9775	9785	9785
N Individuals	5010	5010	5010	5864	5864

Notes: Data are from the Socio-Economic Panel Survey v35 (2002, 2007, 2012, 2017), imputed and unweighted.

All linear random-effects models include the following control variables: a dummy to indicate whether wealth data were imputed and a dummy for the first observed wealth survey year. * p<.05, ** p<.01, *** p<.001

I argued that divorce-related wealth declines would be more considerable for women than men due to potentially gender-biased practices in the division of property at divorce. Thus, the initial wealth gap between men and women could be expected to be larger for divorcees than for the married (*Gendered initial level hypothesis*). Overall, I find substantial and significant gender gaps for both divorcees and the married. Whereas married women rank 4.72 points lower in their year of assigned divorce compared to men, who have an average predicted rank of 52.71, divorced women rank 6.50 points $(-4.72+(-1.78))$ lower in the personal wealth distribution than divorced men, who have an average predicted rank of 40.12 $(52.71+(-12.58))$. As suggested in the *Gendered initial level hypothesis*, I find a gender gap in initial wealth rank levels that is around 2 rank points higher for divorcees than the married. This translates into a 38 percent higher gap for divorcees than the married. Although this difference is statistically not significant according to a Wald test, it should still be considered substantial. Nevertheless, results suggest that gender differences between divorced men and women stem predominantly from disparities that were already present during the marriage while a potentially gender-biased division of marital wealth seems less relevant.

Regarding the wealth accumulation over time, I expected to find larger gender disparities for divorcees than for the married due to a compensation of women's lower wealth accumulation potentials within marriage and a lack of such compensation after divorce (*Gendered growth rate hypothesis*). Thus, initial gender differences should widen more for divorcees than the married. Regression results show that divorced women experience yearly personal wealth rank increases of 0.58 points $(0.53+0.01+0.04)$ while divorced men's wealth rank increases by 0.67 $(0.53+0.14)$ points on average. Whereas this difference of 0.09 rank points is in line with theoretical expectations, it is statistically not significant according to a Wald test. It should be noted that a lack of statistical significance may be due to the desired uncertainty introduced by the multiple imputation of the utilized data. Further, the implications of the rank transformation

should be considered. In comparison and as theoretically expected, married men and women experience similar rank wealth increases with 0.53 point and 0.54 (0.53+0.01) points per year on average, respectively. Thus, my results suggest that wealth accumulation disparities are more pronounced between divorced men and women than between married men and women as expected in my *Gendered growth rate hypothesis*.

The relevance of pre-divorce baseline differences

As previous research has illustrated that the likelihood to experience a divorce is not randomly distributed amongst the married with economically less successful spouses more likely to experience a divorce, I expected that differences in wealth trajectories between divorcees and the continuously married are larger if baseline differences between the two groups are not accounted for (*Selection hypothesis*). To assess this expectation, I re-specified the pooled and gender-specific models using the second pseudo control group that does not account for pre-divorce differences and compare the results of these models to the main models that more appropriately account for selection.

Results from the non-selection adjusted regression compared to the selection-adjusted regression show that predicted initial levels for the married are 3.40 rank points higher (53.34 compared to 49.94) if selection is not adjusted for. Disaggregating these results by gender, it becomes evident that both married men and women have an on average higher initial wealth rank if selection is not accounted for based on predicted initial wealth ranks in the year of (assigned) divorce although differences are more pronounced for men (men: 4.15 rank point higher; women: 2.72 rank point higher). However, contrary to my expectation, growth rates of married respondents overall and disaggregated by gender are around 0.15 to 0.17 rank points lower in regressions models that do not account for selection compared to the main, selection-accounted models.^{xiii} To some degree, this may again be driven by the previously discussed

specifics of the transformed data (i.e., higher overall levels for the second compared to the first pseudo control group translate into smaller rank increases for the second compared to first control groups despite potentially higher absolute increases for the first control group). Overall, results highlight that a failure to account for selection can lead to an overestimation of initial gaps between the married and divorced in the year of (assigned) divorce but potentially an underestimation of wealth growth rates between the groups. The latter should be interpreted with caution as previously mentioned. Thus, my *Selection hypothesis*, and the idea that selection into divorce is a mechanism that can explain previously found wealth disparities in older age, is only partially supported by the results.

DISCUSSION AND CONCLUSION

Against the scenario of historically high divorce rates and a rising relevance of access to sufficient private wealth during a time of soaring wealth inequalities, an incipient body of previous research highlighted substantial wealth disparities between pre-retirement ever-divorced men and women and continuously married men and women (e.g., Wilmoth and Koso 2002, Zissimopoulos, Karney and Rauer 2015). However, research has left the question *how* divorce is linked to the stratification of men's and women's wealth unaddressed. The present study filled this gap by theoretically and empirically disentangle the mechanisms behind divorce-related wealth stratification overall and taking a gender-sensitive approach.

My theoretical ideas scrutinized wealth disparities between ever-divorced individuals and continuously married individuals as a result of three main mechanisms: immediate wealth declines associated with divorce, potentially deteriorated wealth accumulation potentials of divorcees compared to the married after divorce, and selection of economically less advantaged individuals into divorce (i.e., baseline differences between the married and divorced). Further, I expected to find substantial gender differences in wealth trajectories due to gendered wealth

accumulation processes within marriage or after divorce and a potentially gender biased division of property in the case of divorce.

To test my expectations, I used longitudinal data from the German SOEP and applied a novel doubly robust estimation approach that combined propensity score and (coarsened) exact matching with an outcome regression to provide causal estimates of wealth trajectories (i.e., initial differences in wealth levels and differences in wealth growth rates) of divorced and married respondents while accounting for selection into divorce. Comparing these outcome regression results with results that did not account for selection allowed an assessment of the degree to which selection into divorce mattered.

Overall, this study is the first to provide both a theoretical and empirical understanding of the mechanisms around how divorce has the potential to contribute to soaring wealth inequalities and gender inequalities. This knowledge is critical for policymakers and practitioners alike to commence a discussion about whether and how interventions (e.g., subsidized legal aid, capped court costs) could minimize wealth-repercussions associated with divorce for men and women and improve divorcees' capabilities of economic self-sufficiency throughout their life course.

My study presents several original and relevant findings. First and foremost, results illustrate that initial differences generated around divorce are likely the main driver of lasting wealth disadvantages for ever-divorce compared to continuously married individuals. Compared to the substantial differences in wealth levels at divorce between divorcees and otherwise similar, continuously married respondents (in line with *Initial level hypothesis*), I find that divorcees do not show substantially lower wealth accumulation rates (after divorce) compared to the married in contrast to my *Growth rate hypothesis*. This indicates that discussions on how divorcees can be supported in their economic self-reliance should focus on how immediate costs associated with divorce may be reduced or how divorcees could achieve saving rates that outperform the

married – although the latter seems particularly inaccessible. Additionally, couples’ awareness of the substantial immediate wealth penalties should be raised to ensure that couples can make financially sound decisions during their marriage.

While the lack of a wealth accumulation scarring effect is surprising, several explanations can be discussed. First, to benefit from compounded interest effects, individuals need to have a substantial amount of wealth invested in assets that yield returns of investments (e.g., rental property, shares). This may not be the case for the majority of married or divorced individuals at least in Germany, as only a small share of Germans hold wealth in such assets (Grabka and Westermeier 2014). Thus, it is possible that the “average” divorced and married individuals may not have substantially different access to the benefits of compounded interest effects. Second, continuously married individuals are more likely to hold personal wealth in housing wealth, which has often been associated with wealth building advantages. Nevertheless, Lersch and Dewilde (2018) show that although Germans increase their financial wealth substantially leading up to the entry into homeownership (i.e., goal-oriented saving), once they are homeowners they reduce their probability to save and the rate at which they save. Thus, higher homeownership amongst the married is not necessarily associated with higher saving rates at least in the context of the current study. Finally, while divorcees accumulate wealth at similar rates as the married at the average, this average effect may obscure substantial underlying heterogeneity. Indeed, I find that remarriage importantly moderates wealth growth rates of divorcees in line with my *Remarriage growth rate hypothesis* that postulated that remarried divorcees accumulate wealth at higher rates than unmarried divorcees.

Second, my study highlights substantial gender differences in wealth accumulation trajectories. Both married and divorced women were found to rank substantially lower in the overall personal wealth distribution than men in the corresponding groups at the time of (assigned) divorce. Initial gaps were slightly larger for divorcees in line with my *Gendered initial level*

hypothesis although the difference in the gender gaps was statistically not significant, which may be due to the desired uncertainty introduced by the imputed data. For married spouses, this result supports previously found gender wealth gaps within marriage (Grabka, Marcus and Sierminska 2015, Kapelle and Lersch 2020). For divorcees, results highlight that the common assumption of an equal division of *all* available resources at divorce is unlikely to hold as also suggested by Kapelle and Baxter (2021). Rather divorce has the potential to widen gender wealth gaps – at least slightly. Thus, personal-level wealth measures reveal gender-specific effects in the immediate personal wealth penalty associated with marital dissolution. However, it needs to be acknowledged that previous research – which assumed equality in the division of all household wealth due to data restrictions (e.g., Zagorsky 2005) – referred to the US context. In the US, an equal division of wealth is often considered desirable and future needs of spouses are regularly considered in the property division. In this context, judges have more discretion in divorce cases than in any other field of private law. This may indeed lead to lower gender inequalities in post-divorce wealth in the US than in Germany, the context of the current study.

Even for wealth growth rates, gender differences were visible in my study. As expected, married men and women were found to accumulate wealth at similar rates in line with ideas about economic compensations of women's lower wealth accumulation potential within marriage. Potentially due to the lack of such compensation and higher per capita costs for divorced women than divorced men particularly if dependent children are present, my results highlight lower wealth accumulation rates for divorced women compared to divorced men. This supports theoretical ideas expressed in my *Gendered growth rate hypothesis*. Overall, gender differences that were previously found between marital groups in older age likely stem from a range of pervasive gender norms and institutional structures. This highlights the importance to improve gender equality throughout the life course rather than solely focused on time after divorce.

Finally, my study also provides an indication about the last mechanisms that could – at least partially – explain previously found wealth disparities between divorcees and the married: the selection of economically less advantaged individuals into divorce. Overall, selection effects seem to be less important compared to the wealth differences that are generated immediately at divorce itself. However, particularly for the initial wealth level differences, I find support for my *Selection hypothesis* as initial differences are over-estimated if selection is not considered. This over-estimation is more pronounced for men than women. This is in line with traditional norms of the male-breadwinner model, which attributes higher social relevance to husbands' compared to wives' earnings (Zelizer 1989). Thus, particularly men's job loss or economic struggle has been associated with a higher likelihood for the marriage to dissolve (Jalovaara 2003, Killewald 2016). For wealth growth rates, my analyses show contrasting results regarding the influence of selection into divorce as the failure to account for selection into divorce seemed to under-estimate wealth growth rate differences. However, these results should be interpreted with caution due to the applied wealth transformation. In models that do not account for selection, the married have substantially higher wealth levels compared to the models that do account for selection. Similar absolute wealth increases for the two groups would however translate into larger transformed wealth increases for the group with lower wealth levels. Overall, my results and theoretical discussion still highlight that selection into divorce should be considered when interpreting differences between ever-divorced and the married.

While the present study is the first to explore wealth trajectory differences between divorcees and the married, my analyses were limited in two ways. First, although the SOEP data are exceptional in the way that wealth is measured longitudinally at the personal level, the statistical analyses of the present study were restricted by the limited number of waves currently available. This has limited the time after divorce that could be covered and led to a limited sample size particularly in later years after divorce. The latter may have increased the studies' sensitivity to

outliers in these years although robustness checks that reduced the years after divorce in a stepwise manner were consistent with the main results. Further, the limited number of wealth waves also meant that the number of respondents with repeated observations was limited. Although the applied methods can deal with the unbalanced panel predictions particularly for the yearly wealth increases over time may have been more stable with a higher number of reoccurring respondents' observations.

Second, my analyses share a further limitation with other wealth studies: the reliance on self-reported personal wealth. While the collection of survey-based wealth data requires a high level of financial awareness and knowledge on the part of respondents, the collection of personal wealth within the SOEP additionally requires respondents to make judgment about their share of jointly owned assets. Nevertheless, it needs to be acknowledged that the data are currently unique in their provision of fully disaggregated wealth. As access to individual-level administrative wealth data are limited, no research has compared self-reported personal wealth to administrative data yet. Thus, the SOEP data remain the most reliable source of comprehensive personal-level wealth over several survey waves.

Under the assumption that the availability of high-quality, longitudinal wealth data expands, several directions for future research can be identified from the current study. First, using latent class growth analyses, future research should explore the potentially underlying heterogeneity in wealth trajectories not just for divorcees but across different marital pathways and identify salient groups that differ in wealth trajectories. Describing the identified groups would provide relevant evidence on factors that are associated with resilience or risks of lasting disadvantage. Second, as the life course framework highlights the importance of cohort differences based on the embeddedness of life courses within historical time, it seems relevant to continue monitoring the association of marital dissolution and wealth to better understand how cohort-specific differences matter. Third, research on the association between marital dissolution and

wealth would highly benefit from cross-country comparisons to further explore how different policies, and legal regulations and practices matter for the wealth of divorced men and women. Addressing the suggested directions for future research would provide additional policy-relevant evidence on the nexus between family dynamics and wealth inequalities.

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NOTES

ⁱ To commence the divorce proceeding, spouses in Germany and the Netherlands, for instance, commonly need to be separated for at least 12 months, UK spouses are required to separate for at least two years, and US law required a separation period of at least 6 to 12 months depending on the state.

ⁱⁱ Incidences of item non-response are relatively low for the data and range between zero percent for debts on other property and up to 14 percent for measures on private insurance (Grabka and Westermeier 2015).

ⁱⁱⁱ As multiple imputed data were used, I followed the suggestions by Mitra and Reiter (2016) and averaged the m propensity scores for each respondent across the completed datasets, then performed the matching with these averaged scores.

^{iv} While a sufficient number of respondents in the divorce sample got married between 1978 and 2009, divorcees that entered their marriage prior to 1977 or after 2009 had to be grouped together systematically due to low sample sizes: 1963-1969, 1970-1971, 1972-1973, 1974-1975, 1976-1977, 2008-2009, 2010-2011 and 2012-2016.

^v Age was disaggregated, as follows: (1) aged 34 and younger; (2) aged 35 to 41; (3) aged 42 to 49; (4) aged 50 and older.

^{vi} This assumed the correct specification of the propensity score. However, it should be acknowledged that only observable characteristics measured on a yearly basis within the SOEP could be considered in the matching process. Thus, unobserved factors might have still biased the current study.

^{vii} I excluded age from the list of variables used in the generation of the second pseudo control sample because age – in combination with the year of marriage – is a relevant predictor of divorce (i.e., early age at marriage has been associated with a higher divorce risk) (Lehrer 2008).

^{viii} Supplementary checks confirmed that dropped respondents did not substantially differ from control respondents that remained in the sample.

^{ix} The applied multi-level model for change can be fitted to unbalanced data. As respondents with only one or two waves of data provide no or little information about within-person variation over time, they do not contribute to the variance component estimation. Nevertheless, these respondents still contribute to the estimation of fixed effects (Singer and Willett 2003:148).

^x For the IHS-transformed measure, wealth was adjusted for inflation using the consumer price index and the extreme 0.1 percent of reported wealth measures were top- and bottom-coded prior to the transformation.

^{xi} As single outliers in later years with only few sample respondents in those years can influence regression results, I conducted robustness checks by first excluding post-divorce years larger than 20 and second excluding years larger than 15 years from the analyses. Results stay consistent between the different analyses.

^{xii} Including a quadratic term for time since (assigned) divorce did not improve the model fit or change results. This is likely because retirement-related wealth accumulation changes were not captured in the present study due to the focus until late working age.

^{xiii} Supplementary random-effects growth curve models that estimate wealth trajectories solely for the two pseudo control groups (i.e., including a dummy that indicates which control group observations belong to instead of a treatment dummy) confirm substantial and significant differences in initial wealth levels and growth rates between the first and second pseudo control group overall and disaggregated by gender. Precisely, the models confirm higher initial wealth levels but lower yearly rank wealth increases for the second compared to the first pseudo control group.

Online Appendix

Why time cannot heal all wounds:
Personal wealth trajectories of divorced and
married men and women

Diagnostics of the quality of the matching procedure

A range of numerical and graphical matching diagnostics can provide an insight into the quality of the applied matching algorithm.

To provide a numerical indication of the quality of the matching, I calculated the standardized mean differences (Cohens' d) between the treatment and control group before the matching and after the matching for all variables used in the generation of the propensity score (see Table A.7 in this appendix for standardized mean differences across variables). For comparison reasons, I also supply the mean differences for the second pseudo control group that was constructed using only coarsened exact matching on two variables (i.e., marriage year brackets and gender) without additionally considering the propensity score. Standardized differences ranged between .00 and .76 before the matching. After the nearest neighbor propensity score and (coarsened) exact matching on three selected covariates, the standardized differences ranged between .00 and .11, indicating that the balance of observable characteristics was substantially improved. As expected, the second matching approach led to no improved overlap between covariates as standardized mean differences ranged between .00 and .80.

Additionally, a graphical representation of the overlap of the propensity score provided an indication how well the control sample matched the treatment sample in the distribution of the propensity score before and after the matching. This is illustrated in Figure A.1. Although I did not use the propensity score for the generation of the second pseudo control group, I calculated the score for illustrative purposes and graphed results for the second pseudo control groups as well. As can be seen in Figure A.1. the matching substantially improved the overlap of the distribution of the propensity scores between the treatment and first pseudo control groups. As expected, the overlap was not improved by using the second pseudo control group.

Table A.1 Overview of variables used for the multiple imputation including number and share of missing values

		Number of missing values	Percentage
Basic demographics	Gender	0	0.00
	Divorce sample	0	0.00
	Age	0	0.00
	Cohort	0	0.00
	SOEP sample	0	0.00
	Migration background	0	0.00
Family of origin	Number of siblings	375	0.19
	Parents' highest education	18,822	9.51
Household characteristics	Number of children in household	58	0.03
	Number of adults in household	67	0.03
	Living in Eastern Germany	0	0.00
	Residential area	2,583	1.31
Health status	Household member needs assistance	249	0.13
Human capital	Education	1,736	0.88
	Household income (log)	2,011	1.02
	Household income (log)	2,099	1.06
	Employment status	3	0.00
	Full-time work exp. (yrs)	360	0.18
	Satisfaction with household income	1,096	0.55
	Financial concerns	758	0.38
	Homeowner	116	0.06
	Savings account	5,821	2.94
	Business assets	5,821	2.94
	Building loan	6,781	3.43
	Life insurance	5,821	2.94
	Shares	5,821	2.94
	Capital gains	6,162	3.11
Partner characteristics (first marriage only)	Partner's age	3,732	1.89
	Partner's migration background	3,732	1.89
	Partner's siblings	4,333	2.19
	Partner's parents' education	21,300	10.77
	Partner's education	6,162	3.11
	Partner's earnings	5,682	2.87
	Partner's employment status	4,697	2.37
	Partner's full-time work exp.	5,232	2.64
	Partner's HH income sat.	5,703	2.88
Partner's financial concerns	5,366	2.71	

Notes: Although imputations were conducted separately for each of the 35 available survey years, the table displays pooled results to provide a general overview of missing data.

Table A.2 List of covariates used for propensity score and (coarsened) exact matching

Basic demographics	<ul style="list-style-type: none"> - Female [yes/no]⁺ - Age [continuous for propensity score, categorical for coarsened exact matching]⁺ - Cohorts [<1946, 1946-1955, 1956-1965, 1966-1975, >1976] - Migration background [yes/no]
Family of origin	<ul style="list-style-type: none"> - Number of siblings [continuous] - Parental education [low, intermediate, high]
Marital status	<ul style="list-style-type: none"> - Year of marriage [categorical]⁺
Living arrangements	<ul style="list-style-type: none"> - Number of household members aged 0 to 17 years [continuous][*] - Number of household members aged 18 years and over [continuous][*] - Currently living in Eastern German federal state [yes/no][*]
Health status	<ul style="list-style-type: none"> - Someone in the household needs care/assistance on a constant basis due to age, sickness, or medical treatment [yes/no][*]
Human capital and financial situation	<ul style="list-style-type: none"> - Educational achievement [low, intermediate, high] - Personal earnings (log) [continuous] - Equalized household post-government income (log) [continuous][*] - Employment status [full-time, part-time, not in employment] - Full-time labor market experience since entry into labor market [continuous] - Satisfaction with household income [10 point Likert scale] - Worries about own financial situation [very concerned, somewhat concerned, no financial concerns]
Wealth	<ul style="list-style-type: none"> - Homeownership [yes/no][*] - Savings account ownership [yes/no][*] - Ownership of business assets [yes/no][*] - Holding building loan [yes/no][*] - Life insurance [yes/no][*] - Ownership of shares [yes/no][*] - Capital gains [none, under 250 Euro, 250 to under 1000 Euro, 1000 and more][*]
Partner-level characteristics	<ul style="list-style-type: none"> - Age [continuous] - Migration background [yes/no] - Number of siblings [continuous] - Parental education [low, intermediate, high] - Educational achievement [low, intermediate, high] - Employment status [full-time, part-time, not in employment] - Full-time labor market experience since entry into labor market [continuous] - Satisfaction with household income [10 point Likert scale] - Worries about own financial situation [very concerned, somewhat concerned, no financial concerns]

Notes: ^{*}Variables measured at the household level. ⁺ Variables used for (coarsened) exact matching

Table A.3 Descriptive statistics for **imputed** data

	Imputed data								
	Overall	Divorced		Married: 1 st control group			Married: 2 nd control group		
	mean/ (SE)	Men mean/ (SE)	Women mean/ (SE)	Overall mean/ (SE)	Men mean/ (SE)	Women mean/ (SE)	Overall mean/ (SE)	Men mean/ (SE)	Women mean/ (SE)
Basic demographics									
Age	46.33 (7.91)	47.18 (7.29)	45.37 (8.45)	45.41 (7.77)	46.17 (7.61)	44.71 (7.86)	48.01 (7.64)	48.74 (7.34)	47.38 (7.84)
Migration background	0.14	0.13	0.16	0.17	0.17	0.17	0.23	0.21	0.25
Marital status									
Age at first marriage	25.85 (5.54)	27.17 (5.57)	24.38 (5.14)	24.66 (4.45)	25.66 (4.32)	23.73 (4.38)	25.66 (4.91)	26.87 (4.81)	24.63 (4.75)
Age at divorce	39.18 (7.54)	40.22 (6.75)	38.02 (8.20)	37.59 (7.13)	38.02 (7.00)	37.18 (7.23)	39.07 (7.29)	39.64 (7.08)	38.59 (7.43)
Ever remarried after divorce	0.29	0.30	0.29	0.00	0.00	0.00	0.00	0.00	0.00
Family of origin									
Number of siblings	1.86 (1.91)	1.97 (1.94)	1.73 (1.87)	2.03 (1.74)	2.07 (1.76)	1.99 (1.72)	2.08 (1.85)	2.10 (1.91)	2.07 (1.80)
Parents' educational level (based on ISCED97)									
low	0.21	0.21	0.21	0.20	0.23	0.18	0.23	0.23	0.23
intermediate	0.69	0.69	0.68	0.70	0.68	0.73	0.68	0.70	0.67
high	0.11	0.10	0.11	0.09	0.10	0.09	0.09	0.08	0.10
Living arrangements									
Number of household members aged 0-17	0.49 (0.82)	0.26 (0.57)	0.74 (0.97)	0.92 (0.99)	0.93 (1.00)	0.92 (0.97)	0.77 (0.97)	0.81 (0.98)	0.73 (0.96)
Number of household members age >17	1.53 (0.72)	1.45 (0.65)	1.63 (0.79)	2.41 (0.65)	2.43 (0.67)	2.40 (0.64)	2.49 (0.69)	2.51 (0.70)	2.48 (0.68)
Currently in East Germany	0.22	0.21	0.23	0.21	0.20	0.23	0.16	0.16	0.16
Health status									
Household member requires help/care	0.03	0.01	0.05	0.02	0.01	0.02	0.03	0.03	0.03
Human capital & financial situation									
Educational level (based on ISCED97)									
low	0.14	0.14	0.14	0.11	0.10	0.13	0.12	0.11	0.13

intermediate	0.62	0.61	0.64	0.63	0.56	0.70	0.59	0.54	0.64
high	0.24	0.26	0.23	0.26	0.34	0.18	0.29	0.35	0.23
Individual Earnings (LOG)	8.49	9.06	7.85	8.69	9.87	7.58	8.61	9.99	7.43
	(3.79)	(3.48)	(4.01)	(3.41)	(2.47)	(3.77)	(3.57)	(2.39)	(3.97)
Annual Equiv. HH Income (LOG)	9.88	9.95	9.80	10.04	10.05	10.04	10.11	10.12	10.10
	(0.55)	(0.55)	(0.53)	(0.41)	(0.41)	(0.41)	(0.45)	(0.44)	(0.46)
Employment status									
full-time	0.60	0.78	0.40	0.54	0.88	0.23	0.53	0.88	0.24
part-time	0.15	0.02	0.28	0.21	0.02	0.40	0.21	0.02	0.37
other	0.25	0.19	0.31	0.24	0.10	0.38	0.26	0.10	0.39
Full-time work experience (in years)	17.82	22.99	12.06	16.42	22.85	10.41	17.69	25.27	11.19
	(10.47)	(8.52)	(9.38)	(10.63)	(8.79)	(8.48)	(11.46)	(8.45)	(9.55)
Satisfaction with household income	5.60	5.76	5.42	6.41	6.22	6.59	6.68	6.54	6.80
	(2.56)	(2.44)	(2.67)	(2.09)	(2.11)	(2.06)	(2.11)	(2.07)	(2.15)
Financial concerns	0.90	0.96	0.84	0.99	0.99	0.99	1.09	1.10	1.09
	(0.71)	(0.70)	(0.71)	(0.63)	(0.63)	(0.64)	(0.67)	(0.68)	(0.66)
Wealth									
Personal wealth (1'000 EUR)	52.95	58.68	46.57	84.61	92.70	77.06	103.27	120.40	88.56
	(126.08)	(141.69)	(105.75)	(145.98)	(155.47)	(136.06)	(192.94)	(222.20)	(162.26)
Personal wealth rank	39.46	41.23	37.49	53.23	55.52	51.09	55.51	59.10	52.44
	(27.85)	(28.71)	(26.73)	(27.37)	(27.14)	(27.41)	(27.95)	(27.18)	(28.22)
Homeowner	0.33	0.32	0.34	0.66	0.67	0.64	0.66	0.70	0.63
Owned savings account in last year	0.55	0.54	0.56	0.72	0.72	0.72	0.68	0.69	0.67
Held business assets in last year	0.04	0.06	0.02	0.07	0.05	0.09	0.07	0.06	0.07
Held building loan contract in last year	0.34	0.37	0.31	0.54	0.52	0.55	0.51	0.53	0.49
Owned life insurance in last year	0.44	0.48	0.40	0.67	0.67	0.67	0.64	0.67	0.61
Held shares in last year	0.06	0.05	0.06	0.12	0.12	0.12	0.12	0.11	0.12
Capital gains									
none	0.35	0.31	0.40	0.18	0.19	0.18	0.21	0.20	0.22
under 250 EUR	0.44	0.45	0.43	0.47	0.47	0.48	0.48	0.48	0.48
250 EUR to <1000 EUR	0.14	0.15	0.12	0.23	0.24	0.21	0.19	0.20	0.18
1000 EUR and more	0.07	0.09	0.05	0.11	0.09	0.13	0.12	0.11	0.12
Observations	2067	936	1131	7708	3438	4270	7718	3394	4324
Individuals	1127	492	635	3883	1695	2188	4737	2088	2649

Notes: Data are from the Socio-Economic Panel Survey (v35), weighted.

Table A.4 Descriptive statistics for **non-imputed** data

	Non-imputed data								
	Overall	Divorced		Married: 1 st control group			Married: 2 nd control group		
	mean/ (SE)	Men mean/ (SE)	Women mean/ (SE)	Overall mean/ (SE)	Men mean/ (SE)	Women mean/ (SE)	Overall mean/ (SE)	Men mean/ (SE)	Women mean/ (SE)
Basic demographics									
Age	46.38 (8.04)	47.40 (7.27)	45.07 (8.76)	45.73 (7.60)	46.53 (7.36)	44.95 (7.74)	48.31 (7.57)	48.81 (7.28)	47.89 (7.79)
Migration background	0.15	0.13	0.16	0.17	0.17	0.17	0.24	0.21	0.26
Marital status									
Age at first marriage	25.72 (5.48)	26.92 (5.56)	24.20 (4.96)	24.65 (4.37)	25.61 (4.18)	23.72 (4.36)	25.62 (4.99)	26.86 (4.86)	24.58 (4.85)
Age at divorce	39.07 (7.64)	40.16 (6.63)	37.67 (8.55)	37.60 (7.07)	38.04 (6.80)	37.16 (7.30)	39.27 (7.28)	39.67 (7.13)	38.92 (7.40)
Ever remarried after divorce	0.27	0.27	0.27	0.00	0.00	0.00	0.00	0.00	0.00
Family of origin									
Number of siblings	1.84 (1.76)	1.95 (1.76)	1.71 (1.75)	2.09 (1.74)	2.08 (1.72)	2.10 (1.77)	2.16 (1.82)	2.17 (1.89)	2.16 (1.76)
Parents' educational level (based on ISCED97)									
low	0.18	0.17	0.20	0.17	0.18	0.15	0.20	0.21	0.20
intermediate	0.61	0.61	0.62	0.65	0.64	0.67	0.65	0.67	0.64
high	0.10	0.10	0.10	0.09	0.09	0.09	0.08	0.07	0.09
Living arrangements									
Number of household members aged 0-17	0.42 (0.77)	0.20 (0.49)	0.71 (0.94)	0.89 (0.99)	0.88 (0.99)	0.90 (0.99)	0.73 (0.96)	0.77 (0.99)	0.68 (0.94)
Number of household members age >17	1.53 (0.73)	1.44 (0.66)	1.64 (0.81)	2.40 (0.64)	2.42 (0.66)	2.39 (0.62)	2.49 (0.68)	2.50 (0.69)	2.47 (0.67)
Currently in East Germany	0.21	0.21	0.21	0.21	0.20	0.23	0.16	0.15	0.17
Health status									
Household member requires help/care	0.03	0.01	0.07	0.02	0.02	0.01	0.03	0.03	0.03
Human capital & financial situation									
Educational level (based on ISCED97)									
low	0.14	0.14	0.15	0.12	0.11	0.13	0.12	0.11	0.13

intermediate	0.63	0.63	0.62	0.62	0.55	0.68	0.59	0.53	0.63
high	0.22	0.21	0.23	0.26	0.34	0.17	0.28	0.35	0.23
Individual Earnings (LOG)	8.13	8.81	7.26	8.71	9.86	7.57	8.55	9.95	7.36
	(4.08)	(3.76)	(4.29)	(3.40)	(2.49)	(3.78)	(3.62)	(2.49)	(3.99)
Annual Equiv. HH Income (LOG)	9.86	9.95	9.76	10.06	10.07	10.05	10.11	10.13	10.09
	(0.55)	(0.55)	(0.54)	(0.41)	(0.41)	(0.41)	(0.47)	(0.47)	(0.47)
Employment status									
full-time	0.58	0.76	0.35	0.55	0.88	0.23	0.52	0.87	0.22
part-time	0.12	0.02	0.25	0.20	0.02	0.38	0.21	0.02	0.37
other	0.30	0.22	0.40	0.25	0.10	0.40	0.27	0.11	0.40
Full-time work experience (in years)	17.67	23.19	10.66	16.68	23.15	10.33	17.66	25.07	11.36
	(10.53)	(8.62)	(8.34)	(10.80)	(8.77)	(8.62)	(11.42)	(8.42)	(9.73)
Satisfaction with household income	5.39	5.59	5.12	6.40	6.20	6.59	6.60	6.52	6.68
	(2.64)	(2.49)	(2.79)	(2.09)	(2.10)	(2.05)	(2.15)	(2.11)	(2.18)
Financial concerns	0.86	0.92	0.78	0.99	1.00	0.99	1.08	1.12	1.04
	(0.71)	(0.71)	(0.71)	(0.64)	(0.64)	(0.64)	(0.68)	(0.69)	(0.68)
Wealth									
Personal wealth (1'000 EUR)	46.13	53.28	37.05	82.22	86.65	77.87	103.18	122.19	87.03
	(120.11)	(132.56)	(101.54)	(133.51)	(137.41)	(129.45)	(186.41)	(227.88)	(140.05)
Personal wealth rank	37.36	39.18	35.04	53.47	55.81	51.17	55.52	59.93	51.78
	(27.87)	(28.58)	(26.78)	(27.06)	(26.71)	(27.21)	(28.49)	(27.55)	(28.75)
Homeowner	0.28	0.26	0.31	0.63	0.65	0.61	0.63	0.68	0.60
Owned savings account in last year	0.52	0.51	0.54	0.73	0.73	0.74	0.67	0.68	0.66
Held business assets in last year	0.04	0.05	0.02	0.07	0.05	0.09	0.07	0.06	0.07
Held building loan contract in last year	0.30	0.31	0.29	0.53	0.52	0.53	0.49	0.52	0.47
Owned life insurance in last year	0.39	0.43	0.34	0.65	0.66	0.64	0.62	0.65	0.59
Held shares in last year	0.04	0.04	0.05	0.13	0.13	0.12	0.11	0.11	0.12
Capital gains									
none	0.37	0.34	0.40	0.17	0.19	0.15	0.20	0.19	0.21
under 250 EUR	0.43	0.44	0.42	0.47	0.46	0.48	0.47	0.48	0.46
250 EUR to <1000 EUR	0.13	0.14	0.12	0.24	0.26	0.22	0.20	0.22	0.19
1000 EUR and more	0.07	0.08	0.06	0.12	0.09	0.14	0.12	0.12	0.13
Observations	1411	659	752	5065	2334	2731	4542	2027	2515
Individuals	860	390	470	2928	1297	1631	3024	1338	1686

Notes: Data are from the Socio-Economic Panel Survey (v35), weighted.

Table A.5 Linear random-effects growth curve models of personal net wealth (IHS-transformed)

	Treatment & 1 st pseudo control group: Accounted for selection into divorce			Treatment & 2 nd pseudo control group: Not accounted for selection	
	Base B/(SE)	Base + Remarriage B/(SE)	Base + Gender B/(SE)	Base B/(SE)	Base + Gender B/(SE)
Divorce duration (in years)	0.08*** (0.02)	0.08*** (0.02)	0.07** (0.02)	0.06*** (0.01)	0.07** (0.02)
Divorced (Ref: cont. married)	-3.43*** (0.30)	-3.68*** (0.37)	-2.98*** (0.43)	-3.75*** (0.31)	-3.31*** (0.43)
Divorced X divorce duration	0.05 (0.04)	-0.02 (0.05)	0.02 (0.05)	0.07 (0.03)	0.02 (0.05)
Remarried (Ref: unmarried)		1.16 (0.65)			
Remarried X divorce duration		0.09 (0.07)			
Female (Ref: Male)			-0.65* (0.31)		-0.68* (0.32)
Female X divorce duration			0.01 (0.03)		-0.01 (0.03)
Female X divorced			-0.80 (0.57)		-0.78 (0.59)
Female X divorced X divorce duration			0.06 (0.07)		0.08 (0.07)
Intercept	7.65*** (0.19)	7.68*** (0.19)	8.04*** (0.28)	7.98*** (0.18)	8.37*** (0.26)
<i>Variance components:</i>					
Slope	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Intercept	4.43 (0.11)	4.42 (0.11)	4.42 (0.11)	4.38 (0.11)	4.36 (0.11)
Covariance	5.28 (0.09)	5.28 (0.09)	5.28 (0.09)	5.29 (0.10)	5.29 (0.10)
N Observations	9775	9775	9775	9785	9785
N Individuals	5010	5010	5010	5864	5864

Notes: Data are from the Socio-Economic Panel Survey (v35; 2002, 2007, 2012, 2017), imputed and unweighted.

All linear random-effects models include the following control variables: a dummy to indicate whether wealth data were imputed and a dummy for the first observed wealth survey year. * p<.05, ** p<.01, *** p<.001

Table A.6 Cell sizes across years since divorce with 0 being the year of divorce

Years since divorce	Treatment group		1 st pseudo control group		2 nd pseudo control	
	Women	Men	Women	Men	Women	Men
0	125	92	362	214	292	189
1	125	94	353	308	286	289
2	99	76	384	270	332	193
3	91	79	284	206	291	186
4	101	66	273	220	234	215
5	69	74	322	220	272	196
6	68	60	282	270	253	268
7	60	47	252	209	281	208
8	53	44	199	162	227	150
9	45	37	194	170	197	145
10	38	39	187	136	186	164
11	40	42	196	177	206	205
12	29	37	146	138	203	166
13	42	25	136	136	165	145
14	26	22	111	105	156	104
15	22	24	115	76	138	102
16	14	21	111	89	119	104
17	8	15	78	69	97	73
18	20	5	58	71	78	60
19	14	10	34	49	74	50
20	14	9	49	34	59	50
21	5	7	42	32	47	40
22	3	2	26	20	27	24
23	8	2	25	19	32	23
24	6	4	13	15	29	9
25	3	0	10	7	12	15
26	1	2	13	8	12	7
27	0	0	5	4	3	3
28	0	0	7	3	6	4
29	1	1	0	1	4	3
30	1	0	3	0	0	0

Notes: Data are from the Socio-Economic Panel Survey (v35)

Table A.7 Means and standard deviations of covariates measured in the matching year of the divorce sample and the control sample. Before and after matching

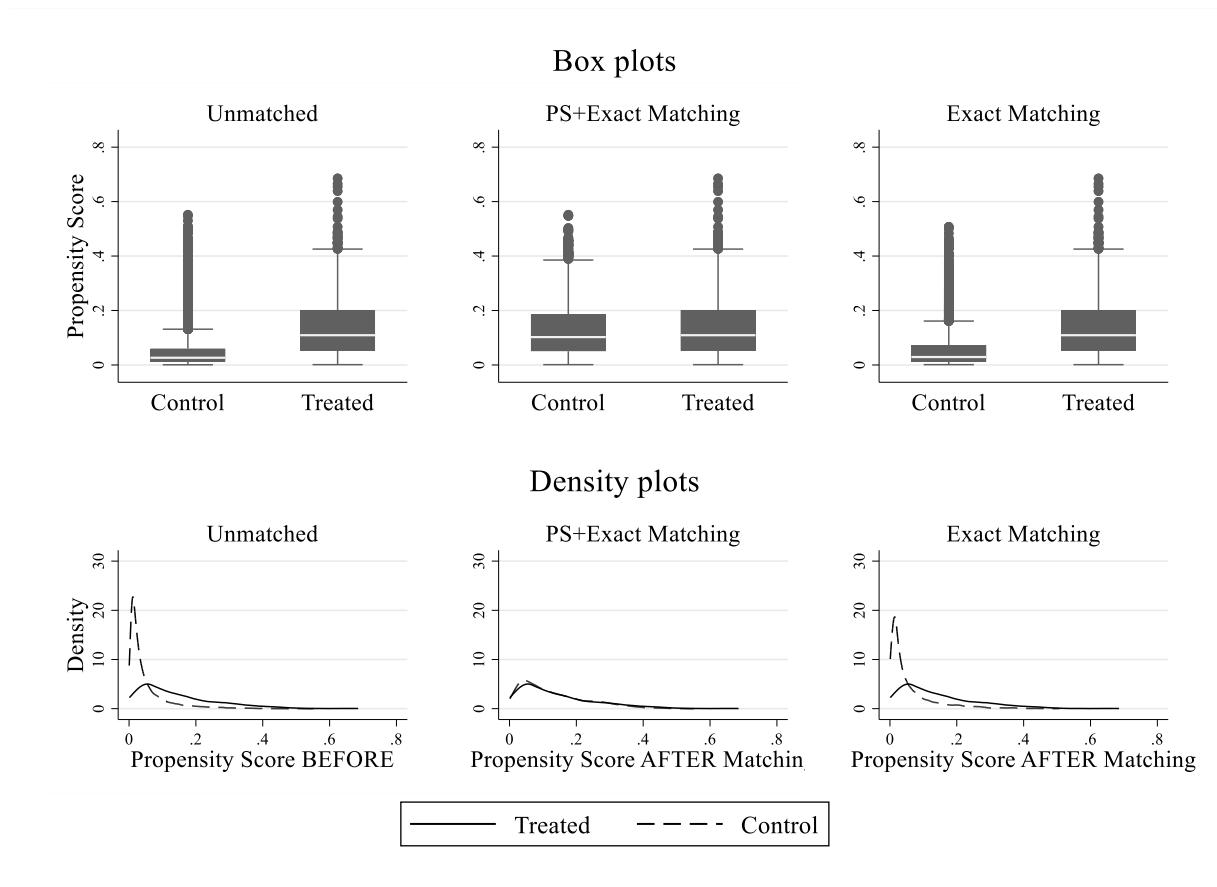
Covariates	Divorce sample (n = 1,127)		Unmatched control sample (n = 19,604)		Standardized difference in means (Cohen's <i>d</i>)	1 st pseudo control group (n = 3,883)		Standardized difference in means (Cohen's <i>d</i>)	2 nd pseudo control group (n = 4,737)		Standardized difference in means (Cohen's <i>d</i>)
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>		<i>M</i>	<i>SD</i>		<i>M</i>	<i>SD</i>	
Basic demographics											
Female	0.56	0.50	0.52	0.50	-0.09	0.57	0.49	0.01	0.56	0.50	0.00
Age	31.59	7.30	38.77	9.56	0.76	31.38	7.15	-0.03	38.96	9.53	0.80
Cohort	2.44	0.93	2.49	1.13	0.05	2.48	0.89	0.05	2.38	0.95	-0.06
Migration background	0.16	0.36	0.27	0.44	0.25	0.18	0.38	0.05	0.26	0.44	0.23
Family of origin											
Number of siblings	1.91	1.89	1.92	1.85	0.00	1.94	1.77	0.01	1.97	1.85	0.03
Parents' highest level of education	0.93	0.55	0.96	0.58	0.04	0.92	0.55	-0.02	0.93	0.56	0.00
Living arrangements											
Number of household members aged 0 to 17 years*	1.11	1.06	1.25	1.15	0.12	1.12	1.06	0.01	1.31	1.17	0.18
Number of household members aged 18 years and over*	2.11	0.41	2.24	0.55	0.23	2.12	0.41	0.01	2.26	0.57	0.27
Currently living in Eastern German federal state*	0.23	0.42	0.20	0.40	-0.09	0.23	0.42	-0.01	0.18	0.39	-0.14
Health status											
Household member needs care/assistance*	0.01	0.10	0.02	0.15	0.08	0.01	0.11	0.01	0.03	0.16	0.10
Human capital & financial situation											
Educational level	1.08	0.59	1.20	0.63	0.19	1.11	0.59	0.05	1.19	0.63	0.18
Personal earnings (log)	8.21	3.77	8.22	3.96	0.00	8.08	3.88	-0.03	8.11	4.02	-0.03
Equalized household post-government income (log)	9.87	0.47	9.99	0.51	0.23	9.90	0.43	0.06	10.00	0.50	0.26

Employment status	0.74	0.91	0.73	0.89	-0.01	0.77	0.91	0.03	0.78	0.89	0.04
Number of years in full-time work	7.84	6.79	12.13	9.79	0.44	7.60	6.55	-0.04	12.03	9.80	0.45
Satisfaction with household income	5.98	2.53	6.76	2.26	0.34	6.21	2.32	0.10	6.74	2.27	0.33
Worries about own financial situation	0.96	0.69	1.10	0.70	0.21	1.00	0.69	0.05	1.09	0.69	0.20
Wealth											
Homeownership*	0.26	0.44	0.48	0.50	0.44	0.29	0.46	0.07	0.50	0.50	0.49
Savings account ownership*	0.74	0.44	0.73	0.45	-0.03	0.76	0.43	0.04	0.73	0.44	-0.02
Ownership of business assets*	0.05	0.22	0.07	0.26	0.07	0.05	0.22	0.00	0.08	0.27	0.10
Holding building loan*	0.52	0.50	0.52	0.50	0.01	0.54	0.50	0.03	0.53	0.50	0.02
Life insurance*	0.65	0.48	0.64	0.48	-0.01	0.66	0.47	0.02	0.67	0.47	0.04
Ownership of shares*	0.16	0.37	0.18	0.38	0.05	0.18	0.38	0.06	0.20	0.40	0.09
Capital gains*	1.00	0.84	1.21	0.95	0.23	1.06	0.86	0.07	1.24	0.95	0.26
Partner-level characteristics											
Partner's age	32.19	7.77	39.31	10.09	0.71	32.22	7.70	0.00	39.73	9.97	0.78
Partner's migration background	0.17	0.37	0.27	0.44	0.22	0.19	0.39	0.06	0.25	0.43	0.20
Partner's number of siblings	1.64	1.85	1.91	1.85	0.15	1.72	1.57	0.05	1.94	1.87	0.16
Partner's parents' highest level of education	0.93	0.55	0.96	0.57	0.06	0.91	0.56	-0.03	0.93	0.55	0.00
Partner's educational level	1.06	0.58	1.20	0.63	0.22	1.10	0.58	0.07	1.19	0.63	0.21
Partner's employment status	0.63	0.89	0.73	0.89	0.12	0.58	0.87	-0.05	0.65	0.87	0.03
Partner's number of years in full-time work	9.01	7.63	12.98	10.55	0.38	9.08	7.41	0.01	13.64	10.36	0.46

Partner's satisfaction with household income	5.94	2.47	6.74	2.26	0.35	6.18	2.31	0.11	6.72	2.27	0.34
Partner's worries about own financial situation	0.97	0.68	1.10	0.70	0.19	1.01	0.68	0.05	1.09	0.70	0.18

Notes: Data are from the Socio-Economic Panel Survey (v35; imputed and unweighted). *Variables measured at the household level. See Table A.2 for information on the variable type and the categories of discrete variables.

Figure A.1 Distribution of propensity scores in treatment and pseudo control groups before and after matching.



Notes: Data are from the Socio-Economic Panel Survey (v35; unweighted; multiply imputed). Propensity scores for the second pseudo control sample were calculated for the purpose of this illustration but were not considered in the construction of this pseudo control group.