



Consequences of Divorce in Germany

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Summary

This cumulative dissertation investigates the economic consequences of divorce in Germany. It includes a general introduction (Chapter 1), three research papers (Chapters 2, 3 and 4) and one article that validates the data quality (Chapter 5). The analysis is based on register data from the statutory German pension fund. The data provide not only large sample sizes but also detailed earnings, employment and marital histories of the divorcees. On the basis of these data, the (causal) effects of divorce on employment, health and earnings are examined by relying on the propensity score framework. However, because propensity scores are sensible to the estimation strategy and the observed covariates, robustness checks with alternative estimation frameworks are also performed. Finally, to address the issue of hidden bias, estimated results are also challenged by sensitivity analyses.

The results may be summarised as follows: Among other strategies, women can cope with economic strain connected with divorce by increasing their labor supply and/or by earning higher incomes (compared to their married counterparts). While the income effect is negligible, the employment effect is very pronounced. I find particularly strong negative employment effects with respect to marginal employment and strong positive effects with respect to regular employment. Furthermore, the lower the labour market attachment before separation, the more pronounced the employment effect. Aside from labour market outcomes, health (measured in days of work disability) also declined considerably around the time of separation. However, I demonstrate that a naïve descriptive investigation would overestimate the effect of divorce on health because there is substantial selection into divorce. Thus, poor health is not only a cause of divorce but also a pathway to divorce. Lastly, the data set allowed me to link former couples and examine their employment and earning trajectories across divorce. Results indicate that women's earnings increased in the years around divorce, whereas their ex-partner's earnings declined. Nevertheless, huge gender differences persist.

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Abbreviations

ATT – average treatment effect on the treated

ATT-DiD – average treatment effect on the treated combined with difference-in-difference

AKVS – Aktiv Versicherte (data set)

CART – classification and regression trees

CDR – crude divorce rate

CMR – crude marriage rate

DiD – difference-in-difference

FDZ-RV – Forschungsdatenzentrum der Deutschen Rentenversicherung Bund

FE – fixed effects

FEIS – fixed effects with individual slopes

GBM – general boosted models

GDR – German Democratic Republic

KS – Kolmogorov-Smirnov-Statistic

ND – normalized difference

OLS – ordinary least square

pp – percentage points

ps – propensity score

SOEP – Socio-Economic Panel (data set)

SUF – scientific use file

SUTVA – stable unit treatment value assumption

TDR – total divorce rate

VA – Versorgungsausgleichsstatistik (data set)

VSKT – Versicherungskontenstichprobe (data set)

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Chapter 1

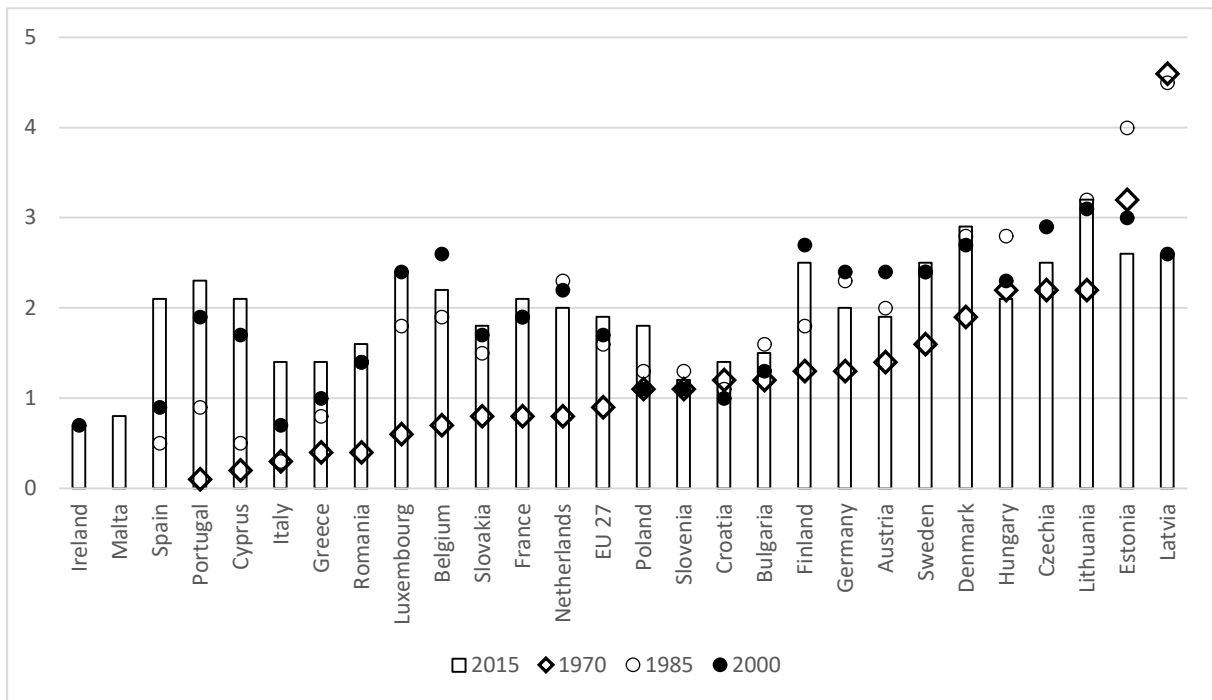
1 Introduction and overview

1.1 Divorce in Europe

Over the past few decades, divorce rates have increased dramatically in Europe or are stagnating at high levels. Figure 1-1 provides an overview of divorce rates, as measured by the crude divorce rate (CDR), in Europe (EU-27) for the period from 1970 to 2015. Except for the three outliers of Ireland, Malta, and Spain, divorce had been legalised (with various forms and requirements) in all European countries by 1970. Spain legalised divorce in 1981 after the restoration of democracy. Ireland removed the constitutional prohibition on divorce in 1996. Malta, as an extreme outlier, only recently introduced the option to divorce, in 2011. These outliers notwithstanding, the CDR in European countries in 1970 varied considerably, from a low of 0.1 in Portugal (where Catholic marriages could not be dissolved by divorce and the dissolution of civil marriages was still difficult under Portuguese law (de Oliveira, 2002)) to a high of 4.5 in Latvia. However, very recent CDR data (2015) show that divorce rates have increased, and that all European countries except three have CDRs that are higher currently than they were in 1970. In some countries, CDRs doubled or even quadrupled over the 1970-2015 period. In Germany, the CDR increased from 1.4 in 1970 to 2.0 in 2015. Nevertheless, it appears that the strong rising trend in divorce rates came to a halt around 2000, when CDRs started falling or at least plateauing in most European countries.¹

¹ Whether marital dissolution has plateaued or declined is still being debated (Chen and Yip, 2018; Bennett, 2017). This question cannot be answered using the CDR because the CDR is the ratio of the number of divorces during the year to the average population in that year. As it is expressed in terms of the total population rather than the married population, changes in the CDR may not reflect real changes in the divorce risk because the population and the family structures are not stable over time. Chen and Yip (2018) found that at least for some countries, the CDR misrepresents the divorce trends and underestimates the divorce risks among married people. Alternative measures like the total divorce rate, which is standardised for the contracted marriages, also shows that divorce intensities have declined somewhat in recent years (Kreyenfeld and Trappe, 2020). However, this measure may also be misleading, as it is affected by the growing selectivity of people who marry (see below).

Figure 1-1: Crude divorce rate in Europe (EU-27)

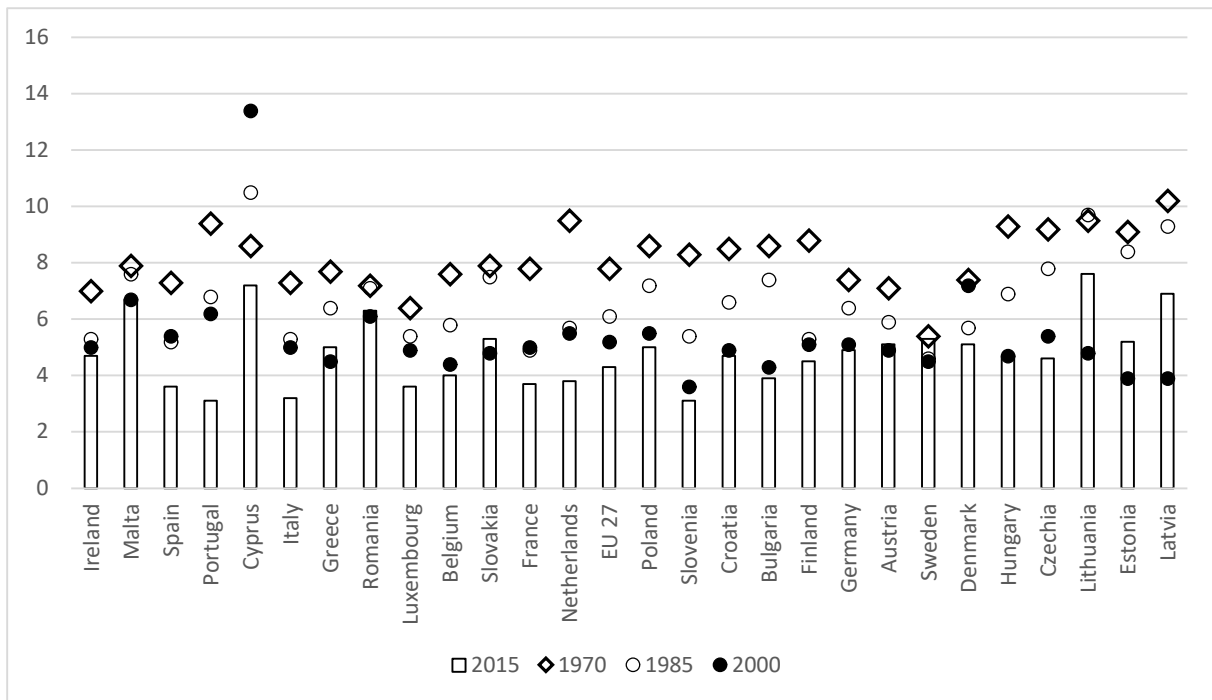


Source: https://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=demo_ndivind&lang=en, own representation, accessed September 2020

1.2 Divorce rates and their relationship to marriage behaviour

The increase in divorce rates was accompanied by a decrease in marriage rates (Figure 1-2). This decline was partly attributable to tempo effects, as individuals were marrying at older ages (Figure 1-3). Figure 1-2 shows that marriage rates, as measured by the crude marriage rate (CMR), declined in all European countries over the study period: while most EU-27 countries had CMRs somewhere between six and 10 marriages per 1000 people in 1970, these values had declined to between three and seven by 2015. While the decline in marriage rates seems to have started earlier in the Western European countries, in the Eastern European countries, marriage rates remained high until the fall of the Iron Curtain (Sobotka and Toulemon, 2008). As a result of societal changes and economic turmoil, these post-communist countries experienced a sharp decline in total first marriage rates over the 1990-2000 period. Marriage rates stabilised between 2000 and 2015 in some European countries, including in Germany. By contrast, in countries like Lithuania, Estonia, and Latvia, the decline in CMRs reversed over this period, and marriage rates were higher in 2015 than they were in 2000.

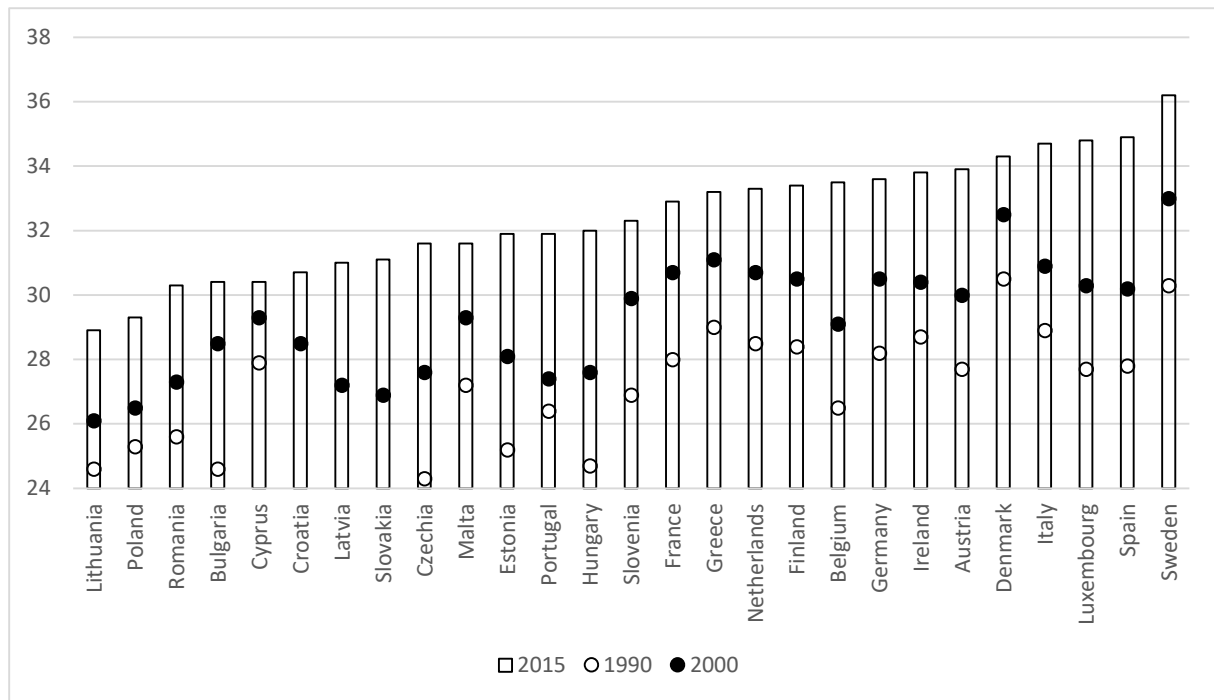
Figure 1-2: Crude marriage rate in Europe (EU-27)



Source: https://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=demo_nind&lang=en, own representation, accessed September 2020

Figure 1-3 shows that the average age at first marriage (for men) increased in all EU-27 countries. (The figure only displays the values for men. Women are, in general, 2.5 years younger than men at marriage.) However, despite the common historical trends in these countries, some differences in the average age at first marriage remain. The age is relatively high in the Nordic countries, but it is relatively low in Eastern European countries like Lithuania and Poland. In 1990, the mean age across Europe was between 24 and 30. By 2015, the average age had increased to 29 in Lithuania and to 36 in Sweden. In Germany, the mean age for men increased from 28.2 in 1990 to 33.6 in 2015. The increase in the age at first marriage was also accompanied by an increase in the proportion of younger cohorts who never married. Furthermore, the increase in the average age was also a consequence of the postponement of almost all transitions to adulthood – i.e., the completion of higher education, union formation, starting a first job, and leaving the parental home (Sobotka and Toulemon, 2008) – and by the de-coupling of marriage and childbearing (Sobotka, 2008).

Figure 1-3: Average age at first marriage for men in Europe (EU-27)



Source: https://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=demo_nind&lang=en, own representation, accessed September 2020

1.3 Family behaviour and divorce patterns in East and West Germany

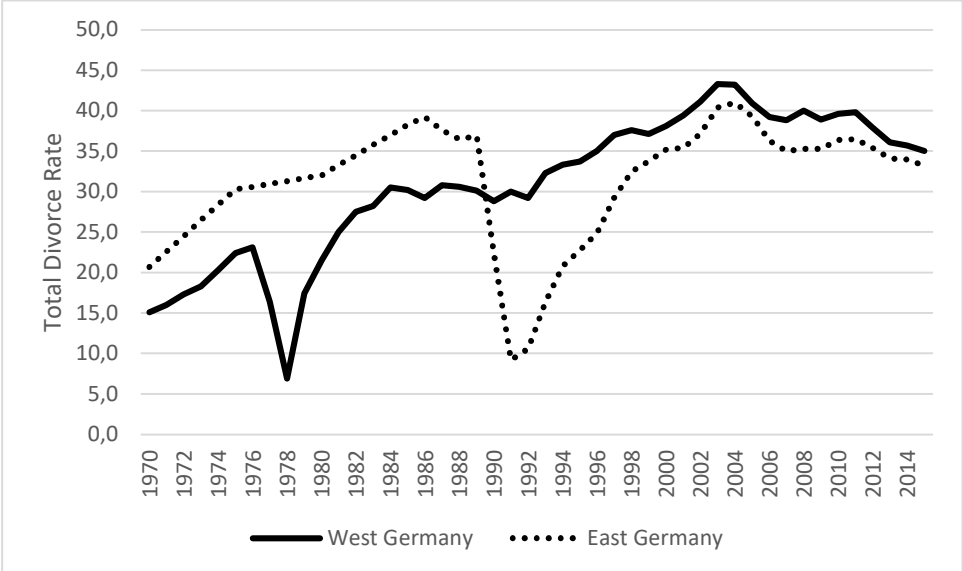
1.3.1 Divorce in East and West Germany

In Figure 1-4, the total divorce rate (TDR) is shown separately for East and West Germany. From 1970 onwards, the TDR in West Germany was characterised by an upward trend that peaked in 2003, and was followed by a decline. This upward trend in West Germany was interrupted in 1978, when a new divorce law went into effect that caused pending divorces to be delayed (Dorbritz, 2008). The previous divorce law was based on fault, or on evidence that the union had ceased to exist for at least three years. Under the new divorce law, the irrevocable breakdown of the marriage was the only grounds for divorce, and the spouses were required to live apart for at least one year before the divorce was finalised. The separation period was mandatory to prevent couples from making hasty decisions. The 1977 divorce law reform changed not only the grounds for divorce, but also the rules on maintenance for ex-spouses. Before the reform, maintenance was awarded based on fault, whereas after the reform, maintenance was awarded independent of fault, and broadly followed the principle of a lasting “post-divorce solidarity” between the spouses (*nacheheliche Solidarität*) and of a “marriage-created need” (*ehebedingte Bedürftigkeit*) (Martiny and Schwab, 2002). Maintenance could be denied, reduced, or granted for a limited period in a few situations only (hardship clauses), such

as in cases in which a maintenance award would be grossly unfair to the debtor (Martiny and Schwab, 2002).

In addition to changing the grounds for divorce and the maintenance rules, the 1977 reform also introduced an adjustment of the pension rights in divorce. Upon divorce, the earning points the spouses had accrued during marriage were added up and equally divided. This adjustment was made because lawmakers recognised that in West Germany, prior earnings and pension entitlements were closely linked, and there was usually an unequal division of paid and unpaid labour between the spouses. Thus, the aim of the equalisation of pension entitlements was to protect the rights of the “economically weaker” spouse, and to compensate for the unequal distribution of paid and unpaid tasks during the marriage.

Figure 1-4: Total divorce rate in West and East Germany



Source: <https://www.bib.bund.de/DE/Fakten/Lebensformen/Scheidungsziffern.html>, accessed September 2020

By 1970, the TDR was already higher in East than in West Germany, mainly because in East Germany, (Catholic) religious ties had loosened, the female labour market participation rate was higher (Dorbritz, 2008), and higher shares of the population were living in urban areas (Markovitz, 1971). In East Germany, the fall of the Iron Curtain led to substantial changes in social life and a sharp rise in economic uncertainty. In 1990, the TDR in East Germany fell drastically. This decline must be seen in relation to German reunification, and was partly attributable to the transfer of West German divorce regulations (in particular, the introduction of the separation year) to the former German Democratic Republic (GDR) (Engelhardt *et al.*,

2003), which affected the pending divorces (Dorbritz, 2008). Following the decline after reunification, the TDR increased, and, in line with trends in West Germany, peaked around 2003. After 2003, the TDR in East Germany plateaued, but at a lower level than that in West Germany.²

While the no-fault grounds for divorce were introduced in West Germany in 1977, they were introduced in East Germany as early as 1955 with the Decree of Marriage and Divorce (Eheverordnung) (Markovitz, 1971). From that point onwards, there was only one grounds for divorce: namely, the irretrievable breakdown of the marriage. In the GDR, the complete integration of women into the production process and the public takeover of former household functions released wives from their domestic tasks and put them on an equal footing with their husbands (Markovitz, 1971). Overall, family law in the GDR emphasised women's equality, and it provided a foundation for the country's family policy approach, which promoted women's labour market participation, and also while helping women reconcile their roles in the workplace and the family. Consequently, it may be argued that in East Germany, the earlier adoption of liberal divorce laws and family policies that supported greater economic independence for women made divorce easier and less costly, which may, in turn, have reduced the economic consequences and stigma associated with divorce (Engelhardt *et al.*, 2003).

To sum up, the two German states, which existed alongside each other from 1949 to 1990, followed very different ideological approaches to family policy. The GDR implemented more liberal and egalitarian divorce laws and changed the role of marriage earlier than West Germany. East German courts tended to view the family not as an "economic unit", but as a "moral unit"; and thus rejected early the view that marriage (particularly for the wife) is a kind of personal insurance, and that the wife's legal position is considerably weaker than that of her husband (Markovitz, 1971).

² Note that the fact that the TDR remains lower in East than in West Germany does not necessarily imply that partnership stability is greater in East Germany than in West Germany, because marriage-specific selection effects are not reflected by the TDR. The East-West gap in marriage behaviour is most evident in the non-marital birth ratio. For example, while the share of non-marital births in East Germany exceeded 58% from 2009 to 2014, this share increased only slightly in West Germany over this period, from 26% to 29%. These figures clearly show that East and West Germans have very different marriage patterns (BiB, 2019). According to Bastin *et al.* (2013), East German mothers are also less likely than West German mothers to marry after giving birth. The reasons for this difference are manifold. One explanation could be that the employment behaviour of East and West German women differs. Marriage is particularly beneficial if the earnings of the two partners are very unequal, or if one of the partners is not gainfully employed. As East German women are more likely than West German women to work full time, they are less likely to profit from marriage and the related benefits (such as "income splitting" and the coverage of the non-working spouse in the public health care system).

1.3.2 Societal change in East and West Germany

The decades-long division of Germany was in many respects a special case, as it led to a divergence of the family behaviour in East and West Germany. Until recently, family policy in West Germany was oriented towards ensuring monetary support for non-working spouses and promoting the male breadwinner model. The West German system of joint taxation and the coverage of non-working spouses in the public pension and health care systems were key characteristics of a regime that was never seriously interested in promoting the integration of mothers into the labour market (Kreyenfeld and Konietzka, 2017). Consequently, West German women had often to choose between family and work, and left the labour market after they had their first child. Moreover, relatively large shares of highly educated women in West Germany remained childless (Kreyenfeld and Konietzka, 2017). In sum, West German family policy made it difficult for women to combine work and family. Instead of promoting women's employment by ensuring the widespread availability of affordable child care, the West German family policy approach incentivised mothers to stay home to care for their children. Consequently, a West German woman was often dependent on her partner's income after she started a family (Engelhardt *et al.*, 2003).

While West Germany has been labelled "pro-traditional" by Gauthier (1996), the GDR could be characterised as having a "pro-natalist" family model with a focus on increasing the fertility rate through government interventions. These interventions were designed to reduce the obstacles to having children, and especially the challenges of combining employment and family responsibilities (Engelhardt *et al.*, 2003). In East Germany, public child care (including after-school care) was widely available and inexpensive, which helped to increase female labour participation rates (especially full-time employment), and to reduce the difficulties women faced in combining work and family. In addition, the GDR provided generous cash benefits (e.g., child allowance) and other forms of support for employed mothers (e.g., leave to care for a sick child, and reduced working hours for mothers of two or more children in full-time employment) (Engelhardt *et al.*, 2003).

However, with the incorporation of the GDR into the Federal Republic of Germany, the family patterns in East Germany changed dramatically, and the previous social welfare system was replaced by new regulations and institutional structures. For example, the "Babyjahr" (one year of paid parental leave) and the monthly "household day" for women in full-time employment were not retained, and the amount of time off women were allowed to care for sick children was reduced (Dennis, 1998).

These changes in family policies and the increase in economic uncertainty resulted in sudden and drastic declines in marriage, divorce, and birth rates in East Germany around the time of reunification (Dennis, 1998). Since then, these rates have recovered somewhat. While the divorce rate in East Germany has returned to its former level, it has remained lower than of the rate in West Germany (see Figure 1-4). In addition, the marriage rate in East Germany has increased, but has remained lower than the rate during the 1980s. In East Germany, the CMR decreased sharply from 8.2 in 1988 to 3.4 in 1992, and then rose again to reach 5.3 in 2018 (BiB, 2018). By contrast, the CMR in West Germany declined steadily over the same period, from 6.5 in 1988 to 6.2 in 1992, and to 5.5 by 2018 (BiB, 2018). Finally, in both regions of Germany, fertility has recovered from previous lows. The total fertility rate (TFR) in East Germany plummeted from 1.67 in 1988 to 0.85 in 1992, but recovered thereafter, and had risen to 1.62 by 2017 (HFD, 2017). In West Germany, the TFR was 1.42 in 1988 and in 1992, and had increased slightly to 1.58 by 2017 (HFD, 2017).³ In 2008, the TFR in East and West Germany converged (for details, see Goldstein and Kreyenfeld, 2011). However, childlessness levels are still lower in East Germany than in West Germany (Kreyenfeld and Konietzka, 2017). Moreover, compared to West Germans, East Germans tend to be younger when they have their first child, and are far more likely to have children in a cohabiting union or as a single parent (Huinink *et al.*, 2012).

To sum up, while the two parts of Germany seem to have converged in some respects, women (and men) in East and West Germany continue to differ in a number of ways. These differences can be attributed to the socialisation, values, and norms these two populations were exposed to prior to reunification. For example, East German women continue to be highly attached to both the labour market and to caring for their family, which clearly demonstrates that there are certain deeply embedded social norms that have proved highly resistant to change (Dennis, 1998). Likewise, East Germans are more likely than West Germans to use pre-school institutions and to place a high value on children (Dennis, 1998). These East-West differences in attitudes regarding lifestyle, marriage, and the labour market also affect the financial

³ Note, however, that the TFR is not an optimal statistic because the values can be substantially “biased” by tempo effects in childbearing. For example, an increase (decrease) in the average age at childbirth leads to a reduction (increase) in the TFR (Luy and Pötsch, 2010). The enormous size of those tempo effects in East Germany after reunification was largely caused the downward trend in the TFR. Luy and Pötsch (2010) were able to provide an estimate for as early as 1996 of a tempo-adjusted TFR, and showed that the magnitude of the divergence could be very large. While their TFR was 0.95 for 1996, their estimated tempo-adjusted TFR was 1.42 for East Germany (see their Annex 1 and Annex 2).

consequences of divorce. Thus, East and West Germany should not be combined in analyses of the effects of divorce, as homogenous effects across the regions cannot be assumed.

1.4 Determinants and consequences of divorce

1.4.1 Determinants of divorce

With respect to the determinants of divorce, fundamental societal changes, like increases in women's education and economic independence, shifts in social norms and values, decreased religious commitment, changing attitudes towards gender roles, and a decline in the stigma surrounding family dissolution have been cited as the main factors that have contributed to the upswing in divorce rates since the 1960s (Lesthaeghe, 2014). Likewise, the responses to these changes, like the increased legal availability of divorce and the simplification of divorce procedures (especially reforms that legalised no-fault divorce), have further simplified the divorce process, which has, in turn, led to higher divorce rates (Gonzales and Viitanen, 2006).

As Figures 1-1 and 1-2 show, the divorce and marriage patterns differ across European countries. This suggests that the social, cultural, political, and economic contexts must be taken into account when seeking to understand the changes in partnership patterns (Perelli-Harris and Lyons-Amos, 2016). However, it appears that in all European countries, the increase in female education has contributed to these changes (Perelli-Harris and Lyons-Amos, 2016). Until recently, it was widely believed that the increase in divorce and the decline in marriage were strongly associated with improvements in women's educational levels and labour market prospects. Women's increased financial independence has allowed them to live outside of marriage, and to leave unhappy marriages more easily than they could in the past. Thus, through their greater orientation towards autonomy and career, women have been able to reject patriarchal marriage in favour of more flexible arrangements, like cohabiting or being in a partnership with more egalitarian structures in which both partners contribute to the relationship (Perelli-Harris and Lyons-Amos, 2016).

In addition, men's decreasing employability and stagnating real income levels (especially in the lower income groups) might have contributed to the increase in divorce rates. Marriage tends to be less stable when the partners face financial strain and a high probability of unemployment. However, recent research has also shown that marriage is destabilised by female as well as by male unemployment (see, e.g., Solaz *et al.*, 2020).

1.4.2 Consequences of divorce

The increase in divorce has also triggered a resurgence of long-standing public policy debates over the “decay of the family” (Parsons and Bales, 1955), as divorce is clearly a disruptive event that can have large effects on the wellbeing of the adults and the children involved.

While the financial consequences of divorce are perceived as being less severe for men, they are seen as being more severe for women (and particularly for mothers). Research by Hauser *et al.* (2016) for Germany for the 1990-2006 period showed that, on average, women experienced a dramatic short-term drop in equalised household income of approximately 26% in the year following the dissolution of a marital or cohabiting union. Government taxes and transfers reduced this decline to 17%. While there was a significant drop in income for women, for men, the equalised household income before taxes and transfers increased by 4% after separation, and it declined by just 4% from pre-divorce levels after taxes and transfers were taken into account (Hauser *et al.*, 2016). It has, however, been pointed out that women may be able to mitigate the shock to their household income caused by divorce by re-partnering, moving in with their parents, and/or expanding their labour market attachment (Mortelmans, 2020).

Previous analyses indicated that divorced women in West Germany adjusted their labour market participation levels by increasing their employment rates eight percentage points to 74% in the 1990-2006 period (Hauser *et al.*, 2016), and six percentage points to 73% in the 2000-2012 period (Bröckel and Andreß, 2015). These studies also found that the average labour earnings of divorced women who were employed increased 36% to €17,775 in the first period, and 22% to €14,681 in the second period. However, less positive trends in the earnings of divorced women in Germany have also been reported. For example, DiPrete and McManus (2000) found for the 1984-96 period (based on a fixed-effects approach predicting the changes in income in the two years around the time of union dissolution) that divorce had a slight, non-significant negative impact on labour earnings in Germany. These diverging findings might be attributable to the use of different methods or to data availability. However, both analyses were done using data from the Socio-Economic Panel (SOEP). The SOEP is a representative study of private households in Germany that covers multiple characteristics like household composition and income, as well as subjective characteristics like health satisfaction. Unfortunately, the number of cases of divorce included in the SOEP data is low.

“... research on the consequences of partnership dissolution is often plagued by the small number of separations observed in population-wide survey samples. For example, one of the first comparative analyses for Germany (Burkhauser et al., 1990, 1991) is based on

56 women and 45 men. As a consequence, country rankings ... are not very stable and observed differences ... may as well be a result of unreliable estimates” (Andreß et al., 2006: 535).

Thus, instead of using the SOEP data or equivalent survey data, I use register data from the German Pension Fund for my dissertation. These register data have a high level accuracy, include a relatively large number of observations (even for a single year), and are not affected by attrition. In the following section, I give a brief overview of the main dataset I use to study divorce events after 1977.⁴

1.5 The pension registers

1.5.1 The data

The Statistics on the Equalization of Pension Entitlements after Divorce (Versorgungsausgleichsstatistik – VA) is a complete register of individuals in Germany who divorced from 1977 onwards, and whose pension entitlements were equalised after divorce. The process of equalising the pension entitlements of ex-spouses was first introduced as a mandatory element of the divorce proceeding in the Federal Republic of Germany in 1977. In 1992, this process was implemented in East Germany as well. After a divorce, the earning points each of the spouses accrued during marriage are added and equally divided. While the pension splitting process is a mandatory component of the divorce proceeding, couples can choose to opt out of it. However, a minority of couples opt out of the procedure (Langenfeld and Milzer 2015).

Until 2009, the statutory, occupational, and private pension entitlements of divorcing spouses were considered in the division of pension rights during the divorce proceeding. The ex-spouses’ pension entitlements were then adjusted through the pension insurance system. In 2009, a structural reform of the equalisation of pension entitlements came into force. The aim of this reform was to simplify this procedure and the ongoing adjustments of the ex-spouses’ pension entitlements after the divorce. Since the reform, the ex-partners’ private and occupational pension entitlements are no longer administered by the German pension insurance system (Reimann and Wiechmann, 2009). These legal changes simplified the calculations and limited the division of pension rights that had to be processed by the German pension insurance system. However, these changes in the regulations affected the comparability of the data across

⁴ The dataset includes only divorces that had resulted in pension point equalisation between the spouses.

time (FDZ-RV, 2018; Wagner, 2012). First, the pension adjustments no longer include private and occupational pension entitlements. Second, the share of divorces covered in the data was affected. Couples who had occupational or private pension entitlements, but no public pension insurance entitlements, were no longer recorded in the data (FDZ-RV, 2018). Another important feature of the reform affected marriages of short duration (less than three years). Following the reform, these marriages were, by default, exempted from an adjustment of the pension rights unless one spouse demanded the adjustment. Thus, because of this regulation, the data from before and after 2009 are not fully comparable.

Importantly, the VA dataset can be linked to other datasets provided by the German pension fund, including the sample of active pension accounts (Versicherungskontenstichprobe – VSKT) and the records of the actively insured (Aktiv Versicherte – AKVS). Using these datasets, additional demographic characteristics and labour market participation characteristics can be obtained for subsamples of the divorces recorded in the VA. The VSKT is a random sample of all individuals with a pension account in Germany. It provides detailed pension-relevant information, such as information on the individuals' employment and earnings histories and their fertility histories (Himmelreicher and Stegmann, 2008). The VSKT for 2018 contains approximately 800,000 employment histories starting at age 15. By contrast, the AKVS contains records of pension-relevant events for all actively insured individuals in Germany for the current year and the two preceding years. Individuals count as actively insured during a range of pension-relevant events, including employment, unemployment, temporary disability, and parental leave. The last version of the dataset from 2018 contains roughly 47 million individuals. Due to a unique identifier (first implemented in 2011), all yearly records of the AKVS can be linked to one large panel dataset covering the years 2011 to 2018, and consisting of hundreds of millions of person-years.

1.5.2 Descriptive results: Employment and earnings of divorcees in West and East Germany

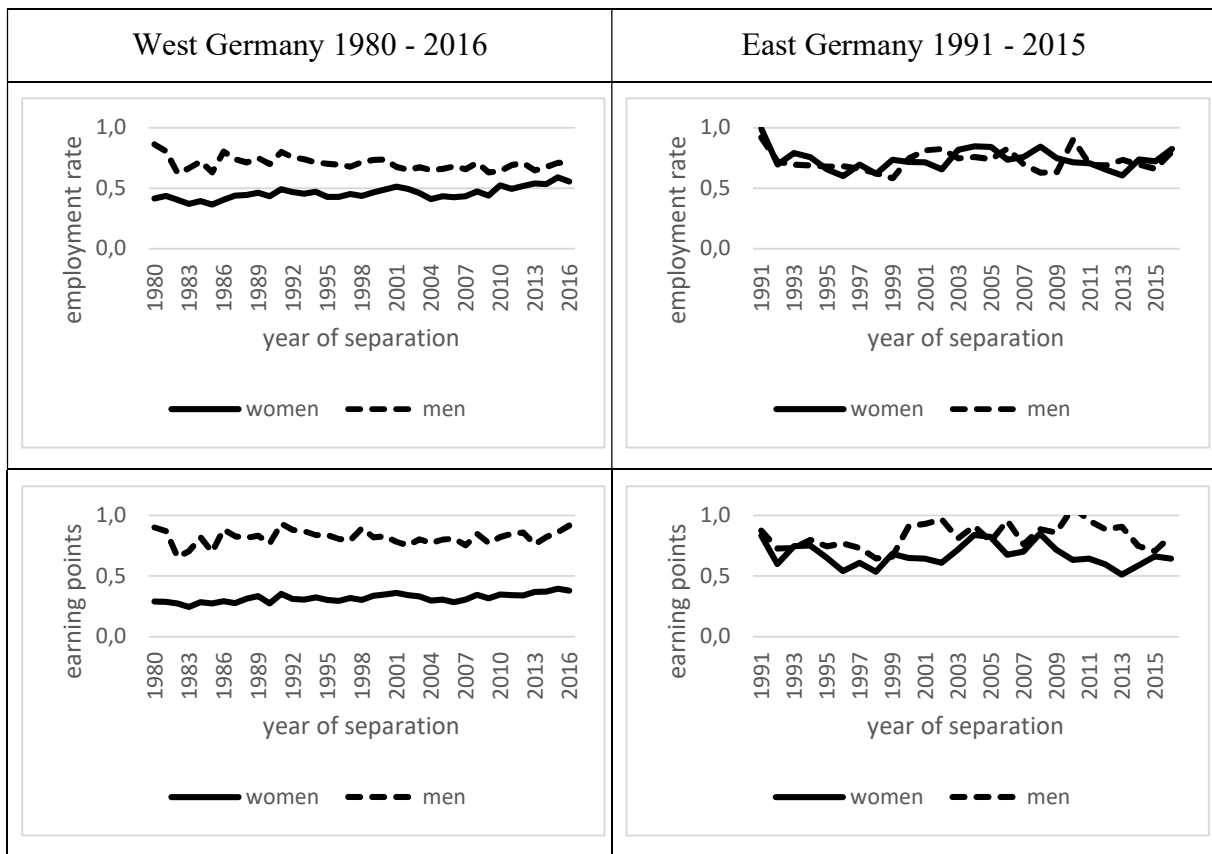
In the following, I will give a short overview of the employment and income situations of divorcees in West and East Germany based on the register data (a linkage of the VA and the VSKT datasets) introduced above. The analysis covers the first divorce, and the divorcees are aged 30 to 55. While the case numbers are acceptable for West Germany, the case numbers for East Germany are certainly too low (see Appendix Table A. 1-1). Hence, this overview should

be interpreted with caution. Employment rates (for employment with social security contributions) and income (as measured in pension points) are measured in the year of separation; i.e., when one spouse informs the other that s/he has filed for divorce.

In Figure 1-5, we observe for West Germany that the labour market participation rate of women increased steadily, but was generally constant for men (upper-left panel). While the gender gap in employment rates was roughly 45 percentage points (pp) in 1980, it had narrowed to 15pp by 2016. However, while the employment rates of female divorcees increased substantially (from 0.41 to 0.55, upper-left panel), their income levels increased only slightly, from 0.29 to 0.38 pension points (lower-left panel). While male and female employment rates were converging over the period, woman's earnings were still substantially lower than those of men. The gender difference in pension points was roughly 0.61 in 1980, and was still as high as 0.54 in 2016. While those numbers have been derived from a small dataset (resulting from a linkage of the VA statistics and the VSKT statistics; see Table A.1-1 in the Appendix for the case numbers), in Chapter 3, we will show that similar results (for West Germany and the years 2011 to 2015) can be derived from the large AKVS (panel) dataset.

This picture changes substantially if when we look at divorcees in East Germany. The employment rates of East German men and women were much more similar than those of their West German counterparts (upper-right panel). However, while the employment rates of East German men and women were similar in the year of separation, there was a substantial gender gap in their income levels (lower-right panel) (Trappe and Rosenfeld, 1998; Trappe and Rosenfeld, 2004).

Figure 1-5: Employment and earnings of the divorcees in Germany. Share of people employed out of all persons and average earning points. West and East Germany by year of separation and gender



Source: Linkage of Versorgungsausgleichsstatistik (2016) and Versicherungskontenstichprobe (2016), own representation

This overview has shown that the labour market outcomes of divorcees in East and West Germany differed substantially over the study period for reasons for that have already been discussed (see 1.3.2). However, the overview also demonstrated that in both East and West Germany, women earned less than men. It therefore appears that women have more to lose in terms of household income than men when they divorce (Bennett, 2017; Boll and Schüller, 2021). In West Germany, women contribute less to the household income than men. Thus, we might reasonably assume that the household income losses of West German women are greater than those of East German women. Therefore, it is likely that the consequences of divorce with respect to labour market outcomes (in order to compensate for those household income losses) are less severe for East than for West German women. If a woman is working full-time prior to her divorce, then the impact of the divorce on her economic wellbeing will be minimised (Roeper and Bennett, 2015).

1.5.3 Methods used in this dissertation

1.5.3.1 Causality

The easily accessible descriptive statistics mentioned above provide us with some initial insights into the economic ramifications of divorce. However, such descriptive analyses may be misleading. First, they only show the earning developments of divorcees, and they lack a comparison group of married individuals. Second, these analyses do not sufficiently account for the selection into divorce. Divorcees may be a select group whose baseline characteristics differed from those of married individuals before they divorced. In my dissertation, I focus on unravelling the “true” causal effects of divorce on economic outcomes.

In Chapter 2, I apply propensity score kernel matching. In Chapter 3, I use fixed effects (and propensity score weighting). In Chapter 4, I rely on nearest-neighbour matching and pooled OLS. While all of these approaches are superior to performing a simple descriptive analysis, they differ in how rigorously they seek to unravel causality. *Fixed effects (FE)* are commonly used as causal modelling strategies. However, there are some downsides to using this approach. As Ludwig and Brüderl (2018) argued, standard FE models are biased because of the different earning profiles of the treatment group and the control group. The divorcees (the treatment group) are very likely to have lower wage growth than their married counterparts. While conventional FE models can control for selection based on wage levels, they fail to control for selection on wage growth. Hence, for making causal inferences, it is possible to use modifications of FE models, like fixed effects with individual slopes (FEIS), FE models on first-differenced data, or matching techniques that do not rely on the parallel trend assumption.

Matching approaches (based on the propensity score) are another means of unravelling causal effects. In theory, the gold standard in estimating treatment effects is the use of randomised control trials. The random treatment allocation ensures that all characteristics (observed and unobserved) of the individuals involved are statistically similar. Thus, the effect of the treatment can be estimated by comparing the average of the outcome variable after treatment. However, in observational studies, the treated individuals and the controls are often systematically different in terms of observed and unobserved covariates. Thus, the researcher must account for those differences. The propensity score (ps) matching approach is one potential solution to account for the observed characteristics between the treated and the untreated subjects (Austin, 2011). The propensity score is defined as the probability of treatment assignment conditional on observed covariates (Rosenbaum and Rubin, 1983). The *ps* is typically estimated by a logit

or probit regression based on a binary treatment indicator and the observed pre-treatment covariates. The covariates do not need to be used in linear form, and interactions often improve the model fit. All in all, the ps is a balancing score; i.e., conditional on the ps , the distribution of the baseline covariates is similar between the treated and the untreated units, and thus mimics the features of a randomised control trial (for observed characteristics). Matching techniques are at the heart of the causal analysis of my thesis. I employ more conventional techniques, but also further developments in these techniques, which I detail below.

1.5.3.2 Matching techniques and their further developments

Propensity score matching approaches involve forming sets of treated and control units with similar ps values. The most basic of these approaches is nearest-neighbour matching, whereby for each treated unit a statistical twin is chosen from the control group based on the smallest available (absolute) difference in the ps . However, multiple variants of this approach exist, such as matching with replacement or matching with a definition of thresholds (caliper) to ensure that the nearest neighbour is not too far away (this also helps to reduce the bias due to differences in the applied observed covariates) (Imbens, 2015).

While an advantage of nearest-neighbour matching is that the bias (due to differences in observed covariates) is low, a disadvantage of this approach is that there is a high variance of the estimated treatment effect. Alternative methods like kernel matching use a much larger number of control units at the cost of increased bias, but reduced variance. In Chapter 2, I apply a kernel matching approach based on the Epanechnikov kernel. The kernel function derives weights W_i for each control unit (within a certain bandwidth h) in order to estimate the counterfactual outcome of a treated unit. Controls with (absolute) propensity score differences ($|ps_{it} - ps_{ic}|$) greater bandwidth h receive zero weight and do not contribute to the counterfactual estimate. The following equation shows the Epanechnikov kernel function with N_c for the number of selected control units, and ps_{it} and ps_{ic} for the propensity scores of a treated and a control unit.

$$W_i(ps_{it}, ps_{ic}) = \frac{1 - \left(\frac{ps_{it} - ps_{ic}}{h}\right)^2}{\sum_{ic=1; ic \in h}^{N_c} 1 - \left(\frac{ps_{it} - ps_{ic}}{h}\right)^2}$$

One major advantage of this method is that it has lower variance than nearest-neighbour matching, as it uses more control units, and thus more information. The main disadvantage of

this approach is that some of the controls might be too far away from the treated unit (expressed by the bandwidth h), and are thus poor matches. However, those control units are given small weights, and do not count as much as better matches. In addition, because the denominator is the sum of all of the transformed propensity score distances in the nominator, all the weights sum up to one, with the greatest weight being assigned to the control with the smallest distance (the best match). Hence, extraordinarily high weights, like in unnormalised inverse probability weighting estimators, are not possible.

So far, the matches and the respective weights I have discussed are based on the propensity score. However, estimating the propensity score is not as trivial as it may sound, as matches and weights react sensitively to variations in the propensity score estimation; i.e., in the choice of the covariates in the fitted regression model. It is, therefore, worth noting the advantages of propensity score estimations. Most of the models in the published literature rely on logistic regression models. Unfortunately, the alternatives offered by disciplines like machine learning seem to be dismissed, despite having several advantages. Moreover, the models that are based on logistic regressions are mostly built on linear predictors for treatment exposure (Westreich *et al.*, 2010; Lee *et al.*, 2010).

The underlying assumption that the recipient of the treatment is linear in its predictors might be too much of a stretch, and can ultimately result in a poor model fit and biased effect estimates. Westreich *et al.* (2010) called this way the naïve way, and pointed to the importance of including higher order and interactions terms among the covariates. “Failing to include these terms in a model is equivalent to assuming that they collectively contribute nothing to model fit, a strong assumption and one which is (likely) frequently inadvisable” (Westreich *et al.*, 2010: 7f.).

To control for higher order and interactions terms among the covariates, machine learning tools are a potential alternative to standard logistic regressions, because those techniques implicitly model interactions and non-linearities in their implementations. Lee *et al.* (2010) evaluated the performance of standard logistic regressions against a diverse set of machine learning algorithms, and found that in scenarios in which the treatment decisions are additive and linear, the standard approach is appropriate and sufficient. Moreover, Pirracchio *et al.* (2015) added that in such scenarios, the main-term logistic regressions will even outperform any data-adaptive method for modelling the propensity score. However, with increasing non-additivity and non-linearity, the performance of logistic regressions decreases, and the performance of machine learnings (like bagged CART, random forests and boosted CART) increases. After

comparing different scenarios, Lee *et al.* (2010) concluded that boosted CART and random forests techniques performed the best (in terms of balancing covariates and biases in effect estimates), which led them to recommend the use of those two variants in future propensity score estimations. I followed their suggestion, and added to the naïve way of estimating the propensity score (i.e., no interactions and higher order terms) in Chapter 2, using a boosted CART model as a robustness check.

1.6 Aim and structure of my dissertation

To sum up, I have noted in the introduction that divorce rates in Germany remain high. However, the consequences of divorce are not equally distributed, as women (and particularly mothers) are at greater risk than men of experiencing negative financial consequences following a divorce. In my dissertation, I study the consequences of divorce.

The dissertation consists of three analysis (chapters) that deal with the consequences of divorce with regard to labour market and health outcomes, and a fourth chapter that describes the data. The labour market and health outcomes are estimated for West Germany only, first because the case numbers are low for East Germany (especially if subgroups are formed), and second because East-West differences in household structures and labour market participation rates remain (see Figure 1-5). Since the income levels and the contributions to the household income of West German married women are generally lower than those of East German married women, I expect to find that the behavioural (labour market) responses to separation were larger (and more interesting) in West than in East Germany.

In Chapter 2, I assess the employment and income effects of divorce for women in West Germany between 2000 and 2005. Based on administrative data that allow me to adopt a causal approach, I find strong negative effects on marginal employment and strong positive effects on regular employment. However, I also observe that the overall employment rate (marginal and regular employment combined) was not affected. Furthermore, the lower a woman's labour market attachment was before the separation, the more pronounced the employment effects were. In addition, I also estimate the impact of divorce on the daily gross incomes of the divorcees. I find no convincing evidence of an income effect. I conclude that divorce might have a pure labour supply effect only.

In Chapter 3, I present collaborative work in which we estimate the changes in income for couples in West Germany around the time of separation. The separations included in this study

occurred in 2013, and the spouses' incomes are tracked between 2010 and 2016. In this chapter, we link three different datasets from the German pension fund in order to identify married and divorced couples. An advantage of identifying couples is that it enables us to analyse the household contexts in which the ex-spouses lived while married.

In Chapter 4, I use the administrative data to study the influence of divorce on the uptake of work disability in West Germany. Information on work disability is included in the data if the sick leave extends beyond 42 days of illness. Thus, the analysis focuses on long-term health issues. I compare divorced men and women over a period of 12 years, starting seven years before they filed for a divorce, and ending four years thereafter. The method I use is matching techniques, which enables me to compare the divorcees with a comparable control group of people who did not divorce. I show that the divorcees' health declined considerably around the time they separated and filed for divorce. Even four years after they filed for a divorce, the divorcees' risk of receiving work disability benefits was strongly elevated. However, a portion of the differences in the health status of the divorcees and the control group could be attributed to selection, because the health status of the divorcees had been lower than the health status of the control group seven years before they filed for a divorce. I conclude with a discussion on the societal consequences of divorce.

In Chapter 5, I present collaborative work in which we validate the administrative data from the German statutory pension fund. The aim of this work is to find out whether the divorcees observed in the administrative data are a selected subsample of all divorcees in Germany. Selection issues pose a severe problem in treatment evaluation studies, as the external validity might suffer. However, our results (based on comparisons with the Socio-Economic Panel (SOEP) data) show that for the West German sample, there were no severe deviations from the SOEP distributions in terms of basic indicators. However, for the East German sample, we find large differences, particularly for divorcees with employment biographies in both East and West Germany.

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Appendix

Table A. 1-1: Case numbers for employment and earnings in West and East Germany, Figure 1-5

year of separation	East Germany		West Germany	
	women	men	women	men
1980			104	34
1981			76	60
1982			113	75
1983			133	66
1984			129	80
1985			143	83
1986			148	88
1987			150	88
1988			160	98
1989			153	104
1990			187	127
1991	7	6	197	163
1992	21	22	241	191
1993	41	25	266	208
1994	40	35	281	214
1995	58	26	306	242
1996	62	22	324	229
1997	51	27	368	263
1998	49	35	408	263
1999	58	29	447	308
2000	58	33	509	349
2001	47	31	542	375
2002	56	34	538	372
2003	52	31	556	445
2004	50	32	577	428
2005	43	31	557	427
2006	42	41	598	480
2007	43	29	628	500
2008	52	27	689	531
2009	54	35	655	480
2010	38	31	590	444
2011	50	29	614	468
2012	44	24	611	445
2013	47	31	596	499
2014	35	34	596	496
2015	38	27	505	434
2016	9	5	107	86

Chapter 2

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2 Women's Employment, Income and Divorce in West Germany: A Causal Approach

2.1 Introduction

Divorce and separation rates have increased in most industrialized societies since the 1960s. In the European Union, for example, the crude divorce rate stood at 0.8 in 1965. This figure soared to 1.5 in 1980, to 1.8 in 2000 and 1.9 in 2015 (Eurostat, 2018). In response to this development, a large body of work has amassed that examines the impact of separation or divorce on either economic well-being or on changes in labour market activities (Hauser *et al.*, 2016; Bröckel and Andreß, 2015; Tamborini *et al.*, 2015; DiPrete and McManus, 2000; Van Damme *et al.*, 2009; Jenkins, 2008; Mueller, 2005; Raz-Yurovich, 2011; Tach and Eads, 2015; McKeever and Wolfinger, 2001). Research by Hauser *et al.* (2016) for Germany for the period between 1990 and 2006 has shown that women experience a dramatic short-term drop in equalized household income of approximately 26% in the year following the dissolution of a marital or cohabiting union. Government taxes and transfers reduce this decline to 17%. While there is a significant drop for women, the equalized household income before taxes and transfers of men increases by 4% after separation and it only drops by 4% from the pre-divorce income once taxes and transfers are taken into account (Hauser *et al.*, 2016).

In this paper, I add to the previous literature by using administrative data to examine the causal consequence of divorce on individual labour income and employment participation of women in West Germany. Previous research for Germany was regularly constrained by the low number of events available in social science surveys that were used to study the economic ramifications of divorce and separation. Thus, scholars often combined multiple survey years or even decades for their investigations (Hauser *et al.*, 2016; Bröckel and Andreß, 2015; DiPrete and McManus, 2000). In this paper, I overcome some of these limitations by focusing the analysis on women with a divorce file opening in the calendar year 2002 using administrative data of the German pension insurance. Apart from the overall employment rate (which is defined as being marginally and/or regularly employed) and the rate for regular employment, I also examine changes in marginal employment. In the context of the German system, a transition from marginal to regular employment is a significant process. Marginally employed persons face lower wages, are exempt from unemployment benefits, do not contribute to the statutory health insurance and, until 2013, were only voluntarily covered in the statutory pension system. As many married women are working marginally in Germany, it is important to understand whether divorce increases regular employment.

As a method, I primarily rely on propensity score matching (kernel matching). Matching techniques have become widely used to unravel causal effects. In a setting like divorce where the selection into divorce is not random, the “divorce effects” in conventional models are very likely biased. The matching approach is one possibility to address the selection bias. It removes selection into divorce by finding similar individuals in the treatment and control group (conditional on observed pre-treatment characteristics). Thus, based on observed covariates it mimics a randomized controlled trial.

As to the structure of the analysis, I first examine the employment effects for marginal⁵ employment, regular⁶ employment and then I estimate the overall employment rate as a combination of both. Since the plausibility of estimates relies heavily on the assumption of conditional independence (no hidden bias), I scrutinize the employment effects with respect to hidden bias from unobserved confounders (Mantel-Haenszel bounds). In a second step I analyze the impact of divorce on daily gross earnings (for regular employment only) by principal stratification (Zhang and Rubin, 2003; Zhang *et al*, 2008; Lee, 2009; Huber and Mellace, 2015). I decided on principal stratification because in the presence of sample selection (non-random selection into employment) naïve treatment-minus-control differences cannot be interpreted as impact estimates (Lee, 2009).

2.2 Institutional Background

For a long time, women in West Germany were treated primarily as housewives and caregivers instead of workers or breadwinners and various institutional features fostered the gendered or traditional division of labour between spouses.

In particular, the tax-splitting scheme provides strong incentives for both spouses to combine one large labour income with one small or zero labour income. The splitting advantage was as high as € 8,000 for high earner breadwinners and was close to € 3,000 for an average breadwinner (Steiner and Wrohlich, 2004). This tax advantage strongly inhibits women’s labour market participation due to the relatively high marginal tax rate for the “second earner”. If the wife were to increase her labour income, the splitting advantage would be reduced with each Euro additionally earned until both spouses earn the same.

⁵ Marginal employment (or equally called mini-jobs in Germany) are specific employment types with an earnings threshold of € 400 in 2003.

⁶ Regular employment refers to standard employment contracts for full-time or part-time jobs with social security contributions.

Apart from the tax system, availability of childcare influenced parents' ability to participate in the labour market (Uunk, 2004). Childcare provision has increased over time in West Germany, but public childcare was largely restricted to part-time care for children of pre-school age (age 3-6) (Wrohlich and Müller, 2014). Since 2005, the German government has initiated several reforms to increase the provision of day care for children under age three. However, for the period that I investigate (2000 to 2005), availability of full-time care and day care for children under age three was very restricted (Bröckel and Andreß, 2015). In addition, the long duration of parental leave was considered an obstacle for women's swift return into the labour market after childbirth and the low amount of benefits was regarded as a barrier for fathers' uptake (Spieß, 2011). It was only in 2007 that the German government initiated a major reform and introduced an income-related "*Elterngeld*". This reform is, however, not relevant for my investigation as it was enacted after the observation period.

As for divorce regulations, until 2008 German law offered the possibility of receiving support payments for the economically weaker spouse (§1361 BGB) and the amount of alimony was granted based on the living conditions before divorce. The lower earning partner (usually the woman) was, in addition, not expected to take up employment until the child entered primary school, and was not expected to work full-time before the youngest child reached age 16 (Bröckel and Andreß, 2015; Hummelsheim, 2009).

While family policies did not see significant shifts around 2002, there have been major labour market reforms since 2003, including the Hartz reforms. While the Hartz IV reform in 2005 involved a drastic cut in benefits for the long-term unemployed and stricter job search obligations, the Hartz II reform in 2003 provided incentives for the uptake of marginal employment by lifting the maximum income from € 325 to € 400 and exempting marginal employment (held as a secondary job) from social security contributions. In theory, the latter reform could partly affect my estimates and result in overestimating the true unbiased treatment effect of divorce as long as married women react stronger to the incentives than divorced women. With the approach applied here, I was not able to disentangle the reform effect from the divorce effect. However, the comparisons of treatment effects for marginal employment before the reform (2002), at the reform year (2003) and after the reform (2004 and 2005) show no strong deviations. I conclude that the likelihood of deviations from the true unbiased effect is rather low.

Overall, social policies in Germany supported, until very recently, the male breadwinner model where one partner reduced employment while married. Despite an increase in women's

employment rate over time, the large majority of women (especially with children) did not work full-time, but were employed part-time or marginally (Bröckel and Andreß, 2015; Engstler and Menning, 2004). Especially marginal employment is widely considered as ambivalent because being continuously employed in the marginal sector means a prolonged risk of de-qualification, wages at the lower end with limited access to in-house training and career advancement (Seifert, 2011). However, compared to non-employment, marginal employment might ameliorate the depreciation of human capital and serves as a stepping-stone into regular employment if employers use it as a screening mechanism (Caliendo *et al.*, 2012).

2.3 Prior Findings

A large body of literature has amassed that studies the social and economic consequences of separation and divorce on equivalent household income. In most instances these studies have found substantial declines (before and after government taxes and transfers) for separated women in the US (Hauser *et al.*, 2016; Tach and Eads, 2015; McKeever and Wolfinger, 2001; DiPrete and McManus, 2000), in Europe (Uunk, 2004), in the UK (Jenkins, 2008) and in Germany (Hauser *et al.*, 2016; Bröckel and Andreß, 2015; Burkhauser *et al.*, 1991). While the majority of empirical assessments have addressed changes in household income, others have investigated the effect of divorce on women's employment and earnings. Studies on the employment effect mostly show that women increased their labour supply after break-up. Raz-Yurovich (2011) analyzed the Israeli context, for example, and found that women increased their employment stability and the number of jobs held following divorce. Monthly salary increased only slightly and the effect was not significant.

Tamborini *et al.* (2015) studied women's employment and average earnings in 1970-74, 1980-84 and 1990-94 in the US. They found long-lasting employment and income increases. However, employment and income increases were substantially lower in the latter period. The decline in effect size is explained by the increased labour market activity while married because women who are already more involved in the labour market may be limited in how much they increase their employment.

While most studies found that divorce leads to an increase in women's employment, there are also studies finding the opposite (Mueller (2005) for Canada, Jenkins (2008) for the UK and Van Damme *et al.* (2009) for countries in Europe). Jenkins (2008), for example, found lower employment rates after divorce in the UK. In the period 1991-97, employment dropped by 5

percentage points (pp) and in 1998-2003 by 2pp. The most obvious reason for the lower drop in the second period were policy changes in 1998, which increased the incentives to work.

Van Damme *et al.* (2009) studied the employment effects in Europe for 13 countries in the period 1994-2001. They found a significant but small increase in participation rates after divorce. Overall, the increase was from 63% the year before separation to 68% one year after, but country variations were substantial. While in the Netherlands, Denmark and Italy the increase was more than 10pp, negative but not significant results were found for Finland and Greece. Employment in the UK dropped significantly by 4.9pp. Overall, increases in employment were greatest for those countries where women worked less before divorce. For Germany, where female employment rates are low, they found an overall increase of 7.3pp to 76%.

The German context was analyzed for example by Hauser *et al.* (2016) and Bröckel and Andreß (2015) based on before-after estimations. On average, divorced women in West Germany increased their employment rates by 8pp to 74% in the period 1990-2006 (Hauser *et al.*, 2016) and by 6pp to 73% in 2000-12 (Bröckel and Andreß, 2015). Average labour earnings (of those who were employed) increased by 36% to € 17,775 and by 22% to € 14,681. In contrast, DiPrete and McManus (2000) found for the period 1984-96 (based on a fixed-effect approach predicting the two-year change around union dissolution) a slight, non-significant negative impact of divorce on labour earnings.

I contribute to the existing literature in the following way. I estimate the “treatment effect” of divorce on the employment rate and on daily gross incomes. This means that I compare divorce effects to a well-defined control group. While for the employment rate the treatment-minus-control difference can be a valid estimate (if matching successfully randomizes the divorce status like random assignment would do), the analysis of incomes, however, might still be flawed. The reason is that earnings are only observed conditional on being employed. As Lee (2009) notes, even with the aid of a randomized experiment, the analysis of an outcome (income) which is dependent on another outcome (employment) is subject to the sample selection problem, if the first outcome (employment) is not randomly distributed after the impact of the treatment. It seems very plausible that for some women (i.e. those women with no children, with older children and women with better education) employment is easier to find. Thus, employment after the treatment is not random but a matter of children and education. Likewise, those women might also work more hours and thus, have higher daily earnings. Therefore, the simple gross daily income comparison between treated and controls might be

flawed by the characteristics that promote employment. To overcome this shortcoming in my analysis, I use the principal stratification framework (Zhang and Rubin, 2003; Zhang *et al.*, 2008; Lee, 2009; Huber and Mellace, 2015). To my knowledge, I am the first who applies this concept to the divorce literature.

2.4 Theoretical Considerations and Key Questions

Prior evidence has shown that employment effects vary by countries and time periods. It has also been shown that divorce may cause an increase or a drop in labour market participation. There are arguments for both effect directions.

On the one hand, the loss in economies of scale as well as the shock in household income should, *ceteris paribus*, increase financial pressure and reduce the reservation wage. One might also argue that the family is maximizing a joint family utility function (Killingsworth and Heckman, 1987) or is specializing in home and labour work (Becker, 1981) while married. As new information becomes available and marriage quality decreases, the value of specialization and the value of maximizing a joint family-utility might change and the focus turns to individual utility and the importance of women's loss in labour market skills. This again reduces the reservation wage because women gain from increasing their work effort in order to acquire work experience for the purpose of employability and income prospects after separation.

On the other hand, since divorcees might face time constraints (especially mothers), qualify for welfare payments or maintenance payments, or move into smaller homes, the reservation wage might also be unaffected or may even increase if women adapt to the new economic condition of reduced household income. Moreover, even if women (in particular mothers with young children) would like to work, there remains the obstacle of low public childcare availability for children under age 3. Although childcare availability has increased over time in West Germany, the share of children under 3 in day care was only 7.7% in 2005 (Bröckel and Andreß, 2015). Therefore, the non-availability of public childcare very likely hampered mothers' labour market entry.

Summing up, a theoretical assessment of the overall effect of divorce on employment is ambiguous. However, one can expect strong effect heterogeneity by whether the woman had been attached to the labour market prior to divorce. Women who were only working in marginal employment should face strong economic incentives to expand their labour market attachment by shifting to regular employment. Conversely, regularly employed women and those with a

strong labour market attachment before separation will not expand their employment to the same degree. Contrary, they might need to decrease it if the double burden of employment and childrearing increases.

Besides employment effects, I also study the impact of divorce on daily gross earnings. In contrast to married women, divorced women might be in need to upwardly adjust their daily income because financial strains are higher and household income is lower (to the extent that alimony and governmental payments are not counteracting those adjustments). On the other hand, due to the double burden of employment and childrearing (in the case of mothers) divorcees might be less able to participate in on-the-job training and might even be forced to change jobs to mother-friendly jobs and to trade higher earnings for flexibility (Gangl and Ziefle, 2009).

2.5 Data and Method

2.5.1 Data

In the present study, I used administrative data from the statutory German pension system. I linked the records of the Sample of Active Pension Accounts (VSKT) with the records of the Pension Rights Adjustments Statistic (VA). The VSKT is a one percent random sample of all individuals with a pension account in Germany. It provides detailed pension-relevant information, such as information on the individuals' employment and earnings history, spells of parental leave, and childbirths since age 15 (Stegmann and Himmelreicher, 2008). The VA contains the dates of separation and divorce of those individuals who have gotten divorced since 1977 and whose pension entitlements were equalized after divorce. The pension fund collects these data, because Germany has a system of "income splitting", whereby pension entitlements are split after divorce (for more details, see Keck *et al.*, 2019). The great advantage of using these data is first, that they provide a reasonably large sample size for a divorce event in a single year and second, the high accuracy of the data (because these data is the source for pension calculations). Furthermore, unlike prospective survey data, administrative data do not suffer from attrition, which is especially likely to occur after a separation or a divorce. However, there are other caveats that I need to mention. One limitation of the data is that the administrative data (the source data for the VSKT) do not include the full resident population, but cover only those who have a pension account. About 90% of the resident population in Germany are included in the data, but people in certain professions, such as civil servants and farmers, are

not included (Kruse, 2007).⁷ Furthermore, not all divorces are included in the VA because the data only contain information on divorces that result in pension splitting. Pension splitting is, in theory, mandatory, but certain couples – and particularly those with short marriages – can avoid pension splitting (Keck *et al.*, 2019). Thus, the observed divorcees might not be a representative subpopulation of all divorcees in Germany. This would limit the external validity of the study. For that reason, my results are limited to the population of women with pension right adjustments in the divorce. However, note that about two thirds of all divorces are included in the data (Keck *et al.*, 2019).

I have restricted the sample to persons with a divorce file opening in 2002. I have further restricted the sample to women who were 25 to 55 years old, were married at least five years before the file was opened, are of German citizenship and lived in West Germany (i.e. never earned any pension records in East Germany). The final analytical sample consist of 413 divorced women. Note that I dropped East German women from the analysis first, due to low case numbers, second, because of structural differences in childcare availability between West and East Germany and lastly, because of systematic differences between West and East German women in terms of labour market participation.

Separation (t_0) is defined as the 15th day of the month in 2002 that the divorce file was opened; i.e., the month when the defendant received the divorce petition. I have furthermore limited the investigation to the time window of 2189 days before the file was opened up to 1095 days thereafter. Employment and income effects are then estimated at file opening (t_0), one year after (t_{365}), two years after (t_{730}) and three years after (t_{1095}).

For my control group I used married women out of the same combined dataset who were still married in 2002 but experienced a divorce in the distant future (after 2008). Taking the women from the same dataset had the advantage that I indirectly controlled for variables that I usually cannot observe (like preferences to work, motivation or religiosity) but which are important for the selection into divorce and employment. To the extent that a woman who is married and who never gets divorced faces lower divorce risks, lower employment risk and follows more closely traditional family norms, my results would be upward biased if these women were chosen as the control group. A control group instead who eventually shares the same risk to divorce controls for such unobserved characteristics and reduces the risk of overestimation.

⁷ Some occupations are not fully covered by the German pension system because those occupations have their own pension institutions and are not obliged to contribute to the statutory pension system. Those occupations are for example architects, medics or self-employed individuals.

In total, the control group consists of 1,437 women who fit (at a randomly chosen month (15th day) in 2002) the same criteria as the treatment group except that they had no file opening in 2002. The control group consisted of 262 women with a file opening in 2008, 267 women with an opening in 2009, 219 women in 2010, 208 in 2011, 176 in 2012, 160 in 2013, 117 in 2014 and 28 in 2015.

I also split the main sample into four subsamples in order to derive employment and income effects for women with different labour market attachment while married. The subsamples were constructed first, by cumulating the days of regular employment between t_{-2189} to t_{-730} and second, by generating four quantiles.⁸ However, I display results only for the most extreme groups, i.e. the subsample of women with zero days of regular employment between t_{-2189} and t_{-730} (Group A; $N_{treated}=144$ and $N_{control}=654$) and the group of women with strong labour market attachment, i.e. days ≥ 967 (Group B; $N_{treated}=134$ and $N_{control}=328$).⁹ I focused on these subgroups because each presents an extreme part of women's labour market attachment while married, i.e. they represent the typical housewife or mother on one side with relatively low lifetime work commitment and, on the other side, the women with substantially more work commitment and fewer young children (see Tables A. 2-2 and A. 2-3 for selected demographic statistics).

A practical challenge is the causal direction of female labour supply and divorce, and addressing the competing perspectives, i.e. the "anticipation" or the "independence" perspective (for a detailed discussion see Özcan and Breen (2012)). I followed the practice in prior studies and implied anticipation of a divorce, i.e. all employment and income changes refer to the baseline day at t_{-730} instead of t_0 . However, I also addressed the independence perspective by the framework of matching and the chosen pre-treatment period (t_{-2189} to t_{-730}). Thus, I controlled for observed differences between divorcees and married women in the period t_{-2189} to t_{-730} (except childbirth).¹⁰ In addition, since higher education, occupational training and work experience are important determinants for employability, income prospects and marital stability (following the independence perspective) I also constructed lifetime measures. These measures

⁸ The cumulated days for regular employment within t_{-2189} and t_{-730} are zero days for the first group, are 2 to 129 days for the second group, are 131 to 960 days for the third and 967 to 1461 days for the fourth group.

⁹ Case numbers for the second quantile are $N_{treated}=29$ and $N_{control}=96$ and for the third quantile $N_{treated}=103$ and $N_{control}=359$.

¹⁰ I measure childbirth in the period t_{-729} to t_{-365} , since childbirth occurs with a time-lag of 9 months and the decision to become pregnant often lies well before t_{-730} . Note, marginal employment is not recorded before 1998, thus, t_{-2189} to t_{-1825} and t_{-1824} to t_{-1460} are excluded for marginal employment and income measures.

are cumulated days for the entire period of age 15 to t_{-730} . A full list of all covariates is presented in Table A. 2-1 (Appendix).

2.5.2 Method

The abovementioned covariates (Table A. 2-1, Appendix) were used in linear form in a logit regression to estimate the individual probability for a file opening in 2002. This is the propensity score.¹¹ In addition, I used a second model from machine learning as an alternative way to calculate the propensity score. This model is based on random trees and incorporates many higher order and interaction terms and thus acknowledges that the true functional form of the selection process was unknown. I used a general boosted model (GBM) for three reasons: first, because these models can handle large numbers of covariates, second, these models are immune to multicollinearity and third, because they often achieve better balance properties than simple logistic regressions (McCaffrey *et al.*, 2013).¹²

Because estimated propensity scores are highly sensitive to selected covariates and their interactions I expect strong differences between these two models. However, if both models come to similar point estimates for employment effects (regardless of strong differences in estimated propensity scores) I am confident that the model is robust against misspecification.¹³

These estimated propensity scores were used to derive weights by either kernel matching or weighting by the odds. To be precise, I combined the logit model with kernel matching and the GBM model with weighting by the odds.¹⁴

¹¹ The propensity score estimation is used only as a tool to get covariates balanced. The concern is not about the parameter estimation of the covariates, but the resulting balance property and thus, standard concerns about collinearity do not apply (Stuart, 2010).

¹² In particular, I use *TWANG* from the *R* library with the following parameters: Interaction depth (3); smoothing parameter (0.0001); iterations (1,000,000) and stopping rule (minimizing ND_{mean}).

¹³ Both models (logit with linear covariates and GBM with higher order covariates) come to very different propensity scores. The largest observed difference is 0.44 probability points (for one woman the logit-based propensity score is 0.70 and it is 0.26 for the same woman in the GBM model).

¹⁴ I extract the kernel weights from kernel matching (Epanechnikov) with *PSMATCH2* at a bandwidth $h=0.056$ for my main sample and 0.082 for group A and 0.038 for group B. Odd weights are derived by $w_{i,j} = D_i + (1 - D_j) \frac{ps_j}{1-ps_j}$ with $D \in (0,1)$ if treated or not and ps as the propensity score. Subscripts i for treated and j for control. Extreme weights can be a problem for odd weighting (if women from the control group have high propensity scores) because results are dominated by only a few cases. In my study, however, odd weights range between 0.033 and 1.18 and a mean of 0.42. The distribution of weights is therefore reasonable without extreme outliers.

Based on these derived weights, I estimated the average treatment effect on the treated (ATT), i.e. I estimated the effect of divorce on employment for those women with a file opening in 2002. In this set-up, the control group serves as a reflection of the outcome that the treated group would have experienced had they not filed for divorce. For my purpose, I combined matching with a difference-in-difference (DiD) approach, thereby considering the change in employment from the baseline day t_{-730} to the respective day at either t_0 , t_{365} , t_{730} and t_{1095} .¹⁵

The mean values of the outcome variable of the control group only serve as a reflection of the outcome that the treated group would have experienced had they not filed for a divorce, if the following assumptions are satisfied: Stable Unit Treatment Value Assumption (SUTVA), Conditional Independence Assumption (CIA) and common support.

The SUTVA assumption rules out that the treatment affects the control group, i.e. we need to assume that the job search effort of the divorcees does not affect the employment probability of married women. Otherwise, the outcome of the control women would not be the same as the one they would have experienced in a world without divorcees and the counterfactual outcome would be biased, leading to overestimated results. Since I have only micro-data I am not able to estimate such displacement effects on the macro-level and, thus, I am not able to verify that such effects do not exist. However, I assume that the labour market in Germany is large enough and can absorb all women (from the treatment and from the control group) without placing constraints on one group. This assumption might be reasonable because first, the entry into divorce is quite low in comparison to the number in unemployment. Second, a substantial part of divorcees is already employed while married and third, divorcees might aim for regular employment whereas married women are often marginal employed and stay marginal employed (thus, competition for the same jobs might be rather low).

It is in general difficult to claim that the CIA holds because it rules out the existence of unobserved covariates that simultaneously affect treatment and employment decisions. I therefore addressed this issue separately in the sensitivity analysis by scrutinizing the employment effect with respect to hidden bias from unobserved covariates.

¹⁵ Note, I applied DiD as a procedure to remove any pretreatment differences in the outcome of interest after matching, i.e. to remove the difference in outcome between treated and control group at t_{-730} from the simple ATT (i.e. the difference in outcome between treated and control at t_0 , t_{365} , t_{730} and t_{1095}). In other words, I did not rely on the common trend assumption for the identification of the treatment effect. Lechner (2011) showed that DiD and matching assumptions do not nest in each other and that the researcher has to decide on which identifying assumptions the analysis is based, i.e. either DiD assumptions or matching assumptions but not both. I relied on the matching assumptions.

Lastly, since I applied kernel matching with reasonably small bandwidths, I claim that the common support assumption is fulfilled automatically.

2.6 Summary Statistics

In Table 2-1, I compare my treatment and control group on some selected background characteristics (for subsamples see also Table A. 2-2 and A. 2-3, Appendix). The raw sample (column 1 and 2) shows that the characteristics of the women who did not undergo a divorce differed sharply from the characteristics of the divorcees. The most obvious differences are found in age, in marriage duration, in childbirth and the number of children, and the labour market outcomes of regular employment.¹⁶ Divorcees are on average older at t_0 , have been married longer, are less likely to have younger children, are more often regularly employed and have higher incomes (income ≥ 0). The low share of young children under six in the treated group might be a sign that young children reduce the risk of divorce or that opportunity costs of divorce are higher. A more formal analysis of the selection process for the main sample (before and after matching) is shown in the Appendix (Table A. 2-1, column 1 and 2).

¹⁶ The justification whether mean values differ is based on the Normalized Difference known from Rosenbaum and Rubin (1985).

Table 2-1: Selected baseline covariates used in logit estimation for the propensity score before and after matching (main sample)

	raw sample			matched sample		
	treated	control	ND	treated	control	ND
<i>at t₀</i>						
age	38.20	36.65	0.22	38.20	38.05	0.02
age difference to spouse	3.19	3.06	0.03	3.19	3.16	0.01
marriage duration	14.49	12.67	0.26	14.49	14.48	0.00
<i>in t₋₇₂₉ to t₋₃₆₅</i>						
childbirth (one if yes)	0.01	0.07	-0.99	0.01	0.01	-0.01
<i>at t₋₇₃₀</i>						
number of child age 0-2	0.11	0.29	-0.52	0.11	0.12	-0.03
number of child age 3-5	0.28	0.40	-0.22	0.28	0.29	-0.02
number of child age 6-15	0.84	0.83	0.02	0.84	0.84	0.00
number of child age 16 and older	0.38	0.25	0.17	0.38	0.37	0.01
<i>in t₋₁₀₉₄ to t₋₇₃₀</i>						
days schooling (higher)	4.53	2.14	0.07	4.53	3.81	0.02
days vocational training	2.88	1.45	0.05	2.88	3.98	-0.04
days care	8.07	5.88	0.04	8.07	7.82	0.01
days parental leave	176.93	242.38	-0.37	176.93	178.47	-0.01
days work disability	2.80	0.77	0.12	2.80	1.53	0.08
days unemployed	16.89	19.78	-0.05	16.89	16.48	0.01
days employed (marginal)	76.10	77.21	-0.01	76.10	75.81	0.00
days employed (regular)	160.55	112.38	0.28	160.55	158.67	0.01
daily income (marginal)	2.33	2.31	0.00	2.33	2.33	0.00
daily income (regular)	26.47	19.07	0.24	26.47	26.20	0.01
yearly income (marginal)	563	579	-0.02	563	560	0.00
yearly income (regular)	8260	5744	0.23	8260	8289	0.00
<i>from first record to t₋₇₃₀</i>						
lifetime days schooling (higher)	354.55	411.30	-0.08	354.55	351.15	0.01
lifetime days vocational training	481.20	492.29	-0.02	481.20	479.47	0.00
lifetime days tenure	2874.74	2427.77	0.19	2874.74	2846.32	0.01
lifetime days tenure (yearly)	156.55	144.35	0.12	156.55	155.88	0.01
case numbers	413	1437		410	1436	

Note: Mean values for selected covariates. ND is the normalized difference: $\frac{\bar{x}_t - \bar{x}_c}{\sqrt{\frac{V_{xt} + V_{xc}}{2}}}$ with $V_{xt} = \frac{1}{(N_t - 1)}$ *

$\sum_{i=1}^{N_t} (x_{it} - \bar{x}_t)^2$ (V_{xc} respectively) (Rosenbaum and Rubin, 1985). Matched sample is based on kernel matching (Epanechnikov) with $h=0.056$. *Lifetime days tenure (yearly)* is calculated by: $Lifetime\ days\ tenure / [age - 16 - (lifetime\ days\ school / 365) - (lifetime\ days\ training / 365)]$. T_0 is the day the divorce file was opened. Case numbers for the raw and matched sample deviate because some cases are lost by kernel matching under the chosen bandwidth.

After matching (Table 2-1, columns 4, 5 and 6) both groups are rather similar and the difference between the treated and matched married women is almost eliminated. The largest difference is in *days of work disability* with 8% of a standard deviation (column 6). The value, nevertheless, is low and does not show a serious bias. Following Sianesi (2004), the matching procedure succeeded in eliminating observed differences between treated and controls, as

indicated by the low Pseudo R^2 of 0.003 after matching (Table A. 2-1, column 2, last row, Appendix).^{17 18}

2.7 Empirical Findings – Employment Dynamic

For my analysis, I estimated the change in overall (i.e. marginal and/or regular), marginal and regular employment for the day of the file opening, one, two and three years after the file opening (t_0 , t_{365} , t_{730} , t_{1095}) to the baseline day at t_{-730} . The difference in the change (DiD) between the treated and the controls shows the effect of divorce for those women with a divorce file opening in 2002. To the extent that the CIA is satisfied, the outcome of the control group would be the outcome that the treated group would have experienced had they not divorced. For the moment, I assume that the CIA holds and assume that selection on unobservable confounders is irrelevant.

In the main sample (Table 2-2, panel 1), the overall divorce effect is significant and -9 percentage points (pp) for marginal employment and 8pp for regular employment in t_0 , i.e. marginal employment is 9pp lower and regular employment is 8pp higher than it would be without divorce. The effect on the overall employment rate is not significant, slightly decreases and shows that it might not be the best parameter to look at because important changes in employment types are hidden.

¹⁷ Note, Pseudo R^2 reduction due to matching are similar for group A (Pseudo R^2 reduced from 0.0673 to 0.0078) and group B (from 0.1383 to 0.0107).

¹⁸ Table A. 2-4 (Appendix) provides additional balance statistics for the subgroups. The test statistics (Normalized Difference and Kolmogorov-Smirnov) show no strong deviation from randomizing individuals into treated and control group for kernel matching. The GBM model (with odd weighting), however, performed more poorly but balance results are still sufficient and reliable.

Figure 2-1: Marginal employment rates in t_{-2189} to t_{1095} , main sample

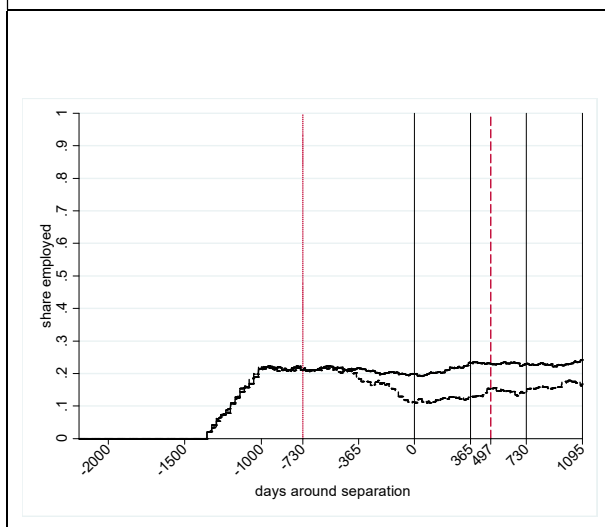
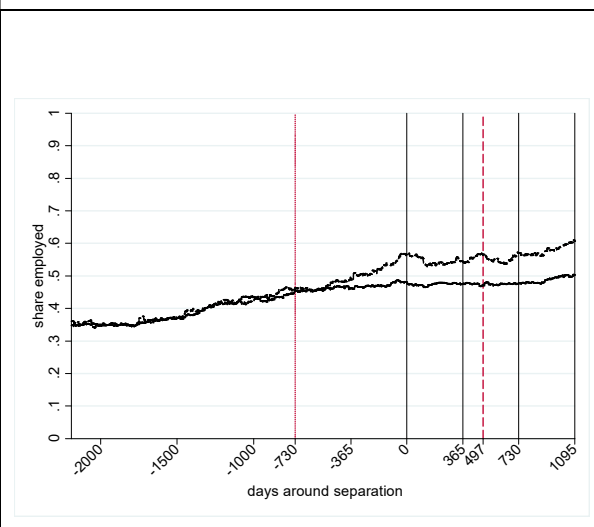


Figure 2-2: Regular employment rates in t_{-2189} to t_{1095} , main sample



Note: Treated sample dashed line, control sample solid line. T_0 is the day the divorce file was opened and the period t_{-2189} to t_{-730} represents the pre-treatment period for balancing observed covariates. Red dashed vertical line represents the average day of divorce. Marginal employment starts at zero because marginal employment was not recorded before 1998. Matched sample is constructed by propensity score kernel matching based on covariates listed in Table A. 2-1 (Appendix).

Figures 2-1 and 2-2 visualize the employment rates for treated and controls and show that the change in employment rates in marginal and regular employment is driven by the employment dynamic of the divorcees but not by the married women. While the labour market participation of women from the control group is fairly stable over time, I observe signs of anticipation in the treatment group, starting around one year before the divorce file was opened. (Figures A. 2-1 to A. 2-3 in the Appendix provide the effect sizes for overall, marginal and regular employment in the main sample.)

Table 2-2: ATT-DiD employment effects in percentage points for main sample, group A and group B at t_0 , t_{365} , t_{730} and t_{1095}

	Logit and kernel matching				GBM and odd weighting			
	h bandwidth	overall employment	marginal employment	regular employment	overall employment	marginal employment	regular employment	
<i>main sample</i>								
at day t_0	0.056	-0.02	-0.09 ***	0.08 **	-0.02	-0.10 ***	0.09 ***	
at day t_{365}	0.056	-0.04	-0.10 ***	0.06	-0.03	-0.10 ***	0.07 *	
at day t_{730}	0.056	-0.01	-0.09 **	0.08 *	-0.01	-0.09 ***	0.08 **	
at day t_{1095}	0.056	0.01	-0.07 *	0.09 **	0.00	-0.08 **	0.10 **	
<i>group A - women not regularly employed in t_{-2189} to t_{-730}</i>								
at day t_0	0.082	-0.10	-0.21 ***	0.13 **	-0.10 *	-0.22 ***	0.14 ***	
at day t_{365}	0.082	-0.08	-0.25 ***	0.17 ***	-0.06	-0.24 ***	0.19 ***	
at day t_{730}	0.082	-0.02	-0.23 ***	0.24 ***	-0.01	-0.23 ***	0.24 ***	
at day t_{1095}	0.082	0.00	-0.22 ***	0.25 ***	0.00	-0.22 ***	0.24 ***	
<i>group B - women with strong labour market attachment (regular employment) in t_{-2189} to t_{-730}</i>								
at day t_0	0.038	-0.03	-0.04	0.00	0.00	-0.04	0.03	
at day t_{365}	0.038	-0.06	-0.04	-0.03	-0.04	-0.03	-0.01	
at day t_{730}	0.038	-0.04	-0.04	-0.03	-0.02	-0.02	-0.01	
at day t_{1095}	0.038	-0.04	-0.01	-0.04	-0.01	-0.02	-0.01	

legend: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Note: Cumulative days of regular employment for group A in t_{-2189} to t_{-730} equals zero and for group B is between 967 to 1460 days. Bandwidth h for kernel matching was chosen by leave-one-out cross-validation. Overall employment is marginal and/or regular employment. T_0 is the day the divorce file was opened. Difference-in-Difference (DiD) estimation with respect to t_{-730} for all measure points. Matching (and odd weighting) was based on the propensity score. Propensity scores were estimated via a General Boosted Model (GBM) with higher order covariates and interaction terms (depth 3) as well as with a logit model based on covariates in linear form (see Table A. 2-1 in the Appendix for all applied covariates).

Table 2-2 breaks down the analysis by subgroups. Women from group A were not regularly employed before separation but were to a substantial part marginally employed at t_{-730} (treated: 44%; control: 40%; see Figure 2-3). The average divorce effect is higher and women exit marginal employment to a significant degree already before the divorce file was opened. Marginal employment is on average 21pp lower in t_0 than it would be without divorce. This effect does not fade out and stays rather constant even at the three subsequent measure points in t_{365} , t_{730} and t_{1095} (Figure 2-3). At the same time, regular employment increases by 13pp in t_0 due to divorce and even further to 25pp in t_{1095} (Figure 2-4). (See also Figures A. 2-4 to A. 2-6 in the Appendix for the effect size for all three employment types.)

Figure 2-3: Marginal employment rates in t_{-2189} to t_{1095} , group A (not regular employed before separation)

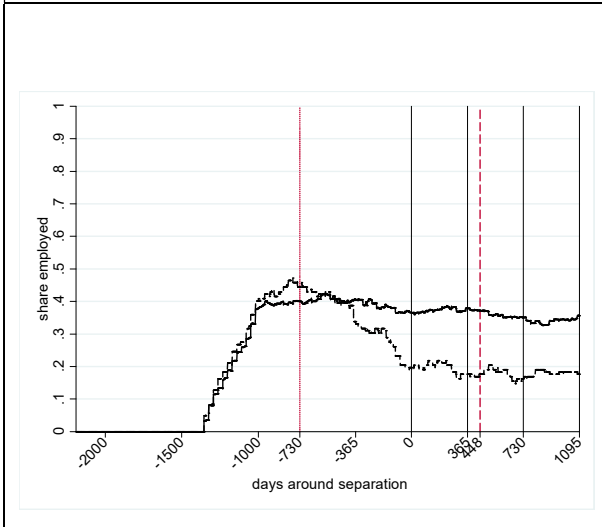
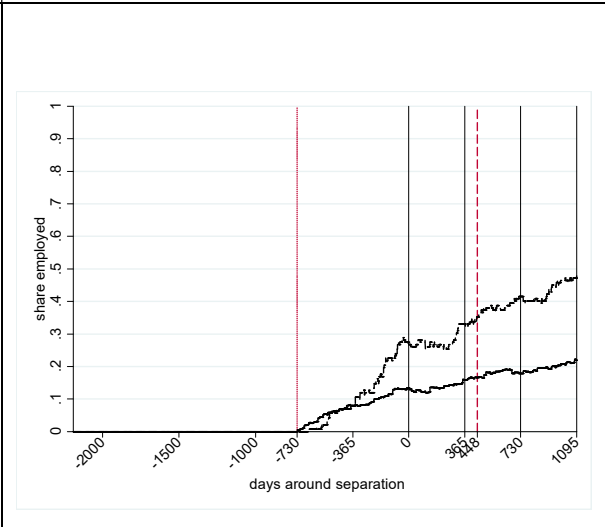


Figure 2-4: Regular employment rates in t_{-2189} to t_{1095} , group A (not regular employed before separation)



Note: Treated sample dashed line, control sample solid line. T_0 is the day the divorce file was opened and the period t_{-2189} to t_{-730} represents the pre-treatment period for balancing observed covariates. Red dashed vertical line represents the average day of divorce. Marginal employment starts at zero because marginal employment was not recorded before 1998. Matched sample is constructed by propensity score kernel matching based on covariates listed in Table A. 2-1 (Appendix).

In contrast, women from group B (with strong labour market participation in regular employment in t_{-2189} to t_{-730}) have no significant employment effects compared to the control group, i.e. the employment rates of divorcees and married women do not differ (Table 2-2 or Figure 2-5). That implies that divorce has neither improved nor worsened the employment status of those divorcees in our observation period. Regarding marginal employment, note that the case numbers in group B are very low for marginal employment, so that I do not discuss nor visualize these results. Likewise, I also skipped the visualization of the effect size.

Figure 2-5: Regular employment rates in t_{-2189} to t_{1095} , group B (strong regular labour force attachment before separation)



Note: Treated sample dashed line, control sample solid line. T_0 is the day the divorce file was opened and the period t_{-2189} to t_{-730} represents the pre-treatment period for balancing observed covariates. Red dashed vertical line represents the average day of divorce. Matched sample is constructed by propensity score kernel matching based on covariates listed in Table A. 2-1 (Appendix).

Finally, if I compare the logit model with the GBM model (Table 2-2), then I observe almost identical point estimates and similar signs in all estimations. I treat this as a strong sign that my results are robust to different analytical applications (logit model versus random trees) and weighting schemes (kernel matching versus odd weights), thus, robust to misspecification.

2.8 Empirical Findings – Income Dynamics with Special Emphasis on Sample Selection

In the presence of sample selection, i.e. non-random selection into employment, the treatment-minus-control difference in incomes might not represent the true causal effect of divorce as long as the non-employed differ systematically in important characteristics from the employed (Heckman, 1979). This is not trivial in my application and Table A. 2-1 (columns 3, 4, 5 and 6, Appendix) provides evidence that employed and non-employed women differ sharply. For example, significant predictors of employment are found in childbirth, in the number of toddlers, the education measures, in disability, in parental leave and prior labour market attachment.

In order to address this issue, I applied a procedure in which the causal treatment effect is not point estimated, but obtained by upper and lower bounds. I derived lower and upper bounds for the set of women who are “always observed”, i.e. the share of women who would be employed

under the treatment arm and the control arm (Zhang and Rubin, 2003; Zhang *et al*, 2008).¹⁹ Unfortunately, without assumptions, the bounds are usually very large and uninformative and I therefore assumed stochastic dominance, monotonicity and both combined in order to sharpen these bounds.²⁰

Stochastic dominance is very likely to hold in the divorce context because it implies that the average daily income of the “always observed” is no less than that of women who are employed under only one treatment arm, i.e. treatment *or* control but not as opposed to treatment *and* control (see footnote 19). To justify that assumption, I assumed that the “always observed” are very likely more motivated, talented or able. As long as these skills transform into higher daily incomes by higher wages and/or more hours worked, this assumption seems reasonable (Zhang *et al*, 2008; Huber and Mellace, 2015).

Table 2-3 provides the lower and upper bounds for the three groups analyzed and in what follows, I provide a brief example of how they were calculated under the assumption of monotonicity. For monotonicity, the starting point is to calculate the trimming share by using the employment rate for the treated and control group $((P_{1|1} - P_{1|0}) / P_{1|1})$. For the main sample at t_0 this results in a trimming value of 15.4 percent for the employed treated sample, which means that for the upper (lower) bound the lower (upper) part of the (sorted) income distribution is dropped. The income distribution of the employed controls is not trimmed and the average daily gross income is € 55.32 at t_0 . For the treated, the average daily gross income is € 62.50 at t_0 for the upper bound (the lower part of the income distribution was dropped) and it is € 46.15

¹⁹ In the Principal Strata Framework, income is truncated for those who are not employed and women can belong to either one of the following four groups. First, those women who are employed regardless of being treated or not are part of the EE group (always observed, i.e. employed under treatment and control status). Second, women who would be employed when divorced but not employed when married belong to the EN group (employed under treatment and not employed under control status). Third, women who would be non-employed when divorced but would be employed when married belong to the NE group. Lastly, women who would be non-employed whether divorced or not belong to the NN group. The observed employed women (income $Y_i > 0$) from the treatment group consist of the groups EE and EN and the observed employed women from the control group consist of EE and NE. Thus, even controlling for employment is not sufficient since for causal inference treated and control women need to consist of one common set, i.e. only of the EE group. Causal inference is only valid if the EN group from the treated and NE group from the controls are eliminated, such that the income difference is measured at the EE group only, i.e. $\bar{Y}_{EE(\text{treated})} - \bar{Y}_{EE(\text{control})}$ (with $Y_i > 0$). (Zhang and Rubin, 2003)

²⁰ Note, confidence intervals may be constructed to take account of sampling variation according the approach by Imbens and Manski (2004) (for an applied example see Lee (2009)). I skipped the calculation of confidence intervals since under stochastic dominance all bounds contain “zero” anyway and in those cases where the lower bound was above zero (monotonicity), the plausibility of the assumption is not straightforward.

for the lower bound (the upper part of the income distribution was dropped). The bounds under monotonicity are now simply the difference in mean values between the treated and controls.²¹

Table 2-3: Sample bounds for the income effect of divorce on daily gross income (regular employment) for the “always observed” under stochastic dominance and/or monotonicity

	P _{1 0}	P _{1 1}	stochastic dominance		monotonicity		stochastic dominance and monotonicity	
			lower	upper	lower	upper	lower	upper
<i>main sample</i>								
at day t ₀	0.479	0.566	-59.25	65.56	-9.17	7.18	0.61	7.18
at day t ₃₆₅	0.474	0.544	-79.22	86.15	-6.68	7.34	1.82	7.34
at day t ₇₃₀	0.477	0.571	-57.85	69.88	-7.60	10.06	2.80	10.06
at day t ₁₀₉₅	0.502	0.607	-43.20	53.03	-10.36	8.98	0.89	8.98
<i>group A - women not regularly employed in t₋₂₁₈₉ to t₋₇₃₀</i>								
at day t ₀	0.134	0.268	-110.45	38.70	-18.41	7.47	-5.29	7.47
at day t ₃₆₅	0.158	0.331	-129.05	37.16	-18.51	7.29	-5.77	7.29
at day t ₇₃₀	0.179	0.415	-96.87	37.78	-17.66	13.88	-1.87	13.88
at day t ₁₀₉₅	0.220	0.472	-131.59	48.40	-16.70	15.02	-1.12	15.02
<i>group B - women with strong labour market attachment (regular employment) in t₋₂₁₈₉ to t₋₇₃₀</i>								
at day t ₀	0.847	0.857	-3.89	11.40	1.82	4.74	3.55	4.74
at day t ₃₆₅	0.827	0.813	-4.04	15.00	5.19	7.54	5.94	7.54
at day t ₇₃₀	0.794	0.777	-3.15	20.77	7.69	10.60	8.72	10.60
at day t ₁₀₉₅	0.795	0.768	-5.85	19.76	5.37	10.36	7.34	10.36

Note: Values are in € and P_{1|0} and P_{1|1} are the employment rates (regular employment) for the controls (column 1) and treated (column 2) with $P_{1|0} = \frac{\sum_{i=1}^N S_i * (1 - T_i) * w_i}{\sum_{i=1}^N (1 - T_i) * w_i}$ and $P_{1|1} = \frac{\sum_{i=1}^N S_i * T_i}{\sum_{i=1}^N T_i}$ with S_i ∈ (0,1) if non-employed or employed and T_i ∈ (0,1) if control or treated. T₀ is the day the divorce file was opened. Cumulative days of regular employment for group A in t₋₂₁₈₉ to t₋₇₃₀ equals zero and for group B is between 967 to 1460 days. Lower and upper bounds are derived with weights from the logit model and kernel matching. Note that differences in employment rates (between P_{1|0} and P_{1|1}) are different to Table 2-2 (column 4) because in Table 2-2 we estimated the ATT-DiD. The difference here (between P_{1|0} and P_{1|1}) represents the ATT.

In Table 2-3 (column 3 and column 4), I see that under the stochastic dominance assumption the lower and upper bounds contain zero. Hence, I cannot rule out that divorce might only have a pure labour supply effect by encouraging women to enter regular employment while leaving daily earnings unaffected.

For my main sample and group A all bounds are also very large and uninformative. In addition, while for the main sample negative or positive income effects are equally likely, for group A the negative effects are dominating the positive effects (column 3 and 4). Thus, those results highlight that women from group A (with many being mothers, see Table A. 2-2, Appendix) are very likely disadvantaged in terms of income effects, when it comes to divorce. One might

²¹ Note, the calculation under the assumption of stochastic dominance as well as monotonicity and stochastic dominance combined are different (see Zhang and Rubin, 2003; Zhang *et al.*, 2008; Huber and Mellace, 2015).

argue, that this is rooted in the double burden of employment and child rearing because the share of mothers is highest in this sample.

For group B, however, the bounds are narrower with the lower bound quite close to zero. The width of the bounds is reasonably small and is (in comparison to the main sample and group A) suggestive of positive income effects because the negative region of the bound is small compared to the positive region. The evidence provided shows that the actual causal effect on daily income caused by divorce under stochastic dominance is somewhere between € -3.89 and € 11.40 at t_0 in my sample. Note that the bounds are slightly narrower (but with the lower bound still below zero) if I apply the weights from the GBM model (results are not shown in the table). Figures A. 2-7 and A. 2-8 (Appendix) provide an overview of lower and upper bounds for group B and for each day in the observation period.

If I also assume monotonicity then I am subsequently able to combine both assumptions, which delivers sharper bounds well above zero for the main sample and group B (Table 2-3, column 7 and 8). This indicates a causal impact of divorce on individual labour earnings in the samples. However, although such results are promising, the assumption of positive (negative) monotonicity requires that the treatment always leads to higher (lower) labour market participation and rules out increased (decreased) reservation wages (Zhang and Rubin, 2003). This assumption might be too strong in the context of divorce and the discussion in the theoretical part has shown that individual labour market exits due to divorce are plausible. Therefore, the plausibility of monotonicity might be too much of a stretch because it rules out the existence of women who would be non-employed when divorced but employed when married.

2.9 Sensitivity Analysis

Until now, I derived the employment effects under the premise that unobserved confounders do not exist or are not relevant. In this section, I scrutinize this assumption and consider selection on unobserved covariates (hidden bias). The reason is that if treated and control units differ in unobserved confounders, i.e. characteristics that simultaneously influence treatment assignment and employment, then the estimated divorce effect is biased.

In Table 2-4, I display the e^Y values and the respective significance levels for the main sample and group A. I skipped group B because a sensitivity analysis for non-significant employment effects (Table 2-2, last panel) is not meaningful (Becker and Caliendo, 2007).

What is e^γ ? The idea of the sensitivity analysis is to check whether the CIA holds. For that reason, I explicitly imply unobserved covariates (hidden bias) and study the influence on the estimated employment effect. Rosenbaum (1995) has shown that the log-odds can be written as a function of observable characteristics x_i and unobserved characteristics u_i with $F(\beta x_i + \gamma u_i)$. If I denote the treatment (D) probability $P_i = P(D_i = 1|x_i, u_i)$, then the odds ratio for two women i and j are given by:

$$\frac{\frac{P_i}{1 - P_i}}{\frac{P_j}{1 - P_j}} = \frac{e^{(\beta x_i + \gamma u_i)}}{e^{(\beta x_j + \gamma u_j)}} = e^{(\beta(x_i - x_j) + \gamma(u_i - u_j))}$$

In the case of a randomized controlled trial, randomization ensures that observed characteristics are $x_i = x_j$ and unobserved characteristics are $u_i = u_j$. Hence, each cancel out so that $e^0 = 1$ remains and both women i and j have the same chance of receiving the treatment (which also implies that no unobserved selection bias exists and the estimated ATT is the true unbiased treatment effect). However, in a study based on administrative data (without being able to randomize women into the control group or treatment group) there is very likely a hidden bias coming from unobserved covariates like marriage quality or the motivation to or not to divorce. In this case, the two women have the same observed characteristics x_i and x_j with $\beta = 0$ (as I can show in Table 2-1, A. 2-2, A. 2-3 and A. 2-4) but they very likely differ in unobserved characteristics with $\gamma \neq 0$ and thus, might also differ in the treatment probability. For $\gamma \neq 0$, I can now bound the possible range of the odds ratio by:

$$\frac{1}{e^\gamma} \leq \frac{\frac{P_i}{1 - P_i}}{\frac{P_j}{1 - P_j}} \leq e^\gamma$$

With $e^\gamma = 1$ the range is simply from 1 to 1 and implies no selection bias but if, for example, $e^\gamma = 2$ then the range broadens from $\frac{1}{2}$ to 2 and the odds of the two women could differ up to a factor of 2 or 100%. Intuitively, as the odds ratio differ (and thus, the selection into treatment) the estimated treatment effect and the ATT might be as small as the minimum value (derived for the lower bound) or as high as the maximum value (derived for the upper bound). The task of the sensitivity analysis is to find the point (by slowly increasing γ) where the confidence intervals for the ATT include zero. If e^γ close to one already changes the inference about the divorce effect, then the estimates are highly sensitive to hidden bias. However, if the inference is unchanged even for high values of e^γ , then the estimated effects are said to be insensitive to

hidden bias. This approach does not show that unobserved confounders are present nor that they not exist, but it provides useful information for the discussion to what extent unobserved confounders could alter the treatment effect if they were present (Rosenbaum, 1991).

Table 2-4: Sensitivity analysis for unobserved heterogeneity (based on the logit model)

	marginal employment		regular employment	
	e^{γ}	significance level	e^{γ}	significance level
<i>main sample</i>				
at day t_0	1.70	0.048	2.00	0.046
at day t_{365}	1.65	0.045	1.75	0.048
at day t_{730}	1.52	0.045	1.75	0.045
at day t_{1095}	1.38	0.041	1.80	0.048
<i>group A - women not regularly employed in t_{2189} to t_{730}</i>				
at day t_0	1.38	0.048	1.85	0.048
at day t_{365}	1.60	0.045	2.35	0.049
at day t_{730}	1.68	0.045	2.63	0.046
at day t_{1095}	1.60	0.048	2.50	0.048

Note: T_0 is the day the divorce file was opened. Cumulative days of regular employment for group A in t_{2189} to t_{730} equals zero. E^{γ} and $1/e^{\gamma}$ provide sharp bounds for the selection into treatment. The hypothetical selection bias (due to unobserved or unmeasured confounders) within the bounds, however, does not drive the confidence intervals of the treatment effects from Table 2-2 (column 3 and 4) into zero (based on 5% significance level).

Table 2-4 highlights that results for regular employment in both samples are relatively insensitive to deviations from the CIA as e^{γ} is ≥ 1.75 which I consider to be large given my observed baseline covariates and the successful randomization (or balance on observed covariates). I can therefore conclude that even large amounts of unobserved heterogeneity would not deteriorate the estimated employment effects in Table 2-2. Regarding marginal employment, however, the smallest value for e^{γ} is 1.38. Estimated employment effects in Table 2-2 are therefore much more vulnerable to unobserved covariates that simultaneously influence divorce assignment and labour market participation. Thus, inference about the impact of divorce on marginal employment (at least for t_0 in group A and t_{1095} for the main sample) should be drawn with less confidence.

2.10 Conclusion

In this paper, I addressed the causal impact of divorce on labour supply and individual income. To that end, I relied on kernel matching and DiD as well as on odd weighting and DiD. I applied two different techniques to estimate the propensity score and can show that the way in which I derived these scores did not affect my estimates. I thus consider my results to be robust to misspecification.

Prior descriptive research had generally shown that divorce leads to an increase in women's employment and individual labour earnings after divorce (Hauser *et al.*, 2016; Bröckel and Andreß, 2015). My more causal investigation that differentiates by different types of employment shows a different and more nuanced pattern. First, I do not find that employment increases after divorce if overall employment is the outcome of interest. However, if overall employment is split into regular and marginal employment, then different employment patterns appear. I find a strong impact of divorce on the type of employment. On average, marginal employment is reduced by approximately 9pp, while at the same time regular employment increases by 8pp. The effects are even stronger for women who were not regularly employed in the most recent years preceding separation. For this group, marginal employment is reduced by up to 25pp while at the same time regular employment soars by 13pp up to 25pp in the aftermath of divorce. For women with high labour market attachment a divorce did not affect the employment rate.

Regarding the income estimation, my approach shows that beside a pure labour supply effect a divorce does not seem to have an impact on daily earnings. An exception might be women with a strong labour market attachment because lower bounds for the income effect under stochastic dominance are only slightly negative.²²

Although I tried my best to adopt a causal approach, remaining caveats must be mentioned. First, I did not know the date when women began to anticipate their divorce and when the “treatment” exactly began. I assumed that women typically anticipated a subsequent divorce, changing their working life accordingly before it occurred and thus set the baseline day at t_{-730} .

Moreover, while the employment effect strongly depends on the CIA (for an unbiased estimation of the causal effect), the income effect relies on additional assumptions. I addressed the CIA explicitly in a sensitivity analysis and found that in particular employment effects for regular employment are insensitive to unobserved confounders. However, employment effects for marginal employment are much more dependent on the CIA. Income effects rely in particular on the stochastic dominance assumption. If monotonicity is also assumed, then I am able to derive lower bounds for the effect of divorce on daily income that are above zero and thus imply a positive treatment effect. While stochastic dominance seems to be plausible, I did not find convincing arguments that monotonicity applies too.

²² Only if one is willing to also assume monotonicity, then the lower bounds for daily gross incomes are positive and in the main sample are between € 0.61 to € 2.8 and in group B (high labor market attachment) between € 3.55 to € 8.72. Notable, in group A (low labor market attachment) they are still below zero (€ -5.77 to € -1.12).

In addition, the causal estimates are based on women with a file opening in 2002. Since labour markets and institutional settings are not static, the estimated effects do not necessarily apply to earlier or later periods. In particular, due to a maintenance reform in 2008 and various reforms to increase the provision of day care for children since 2005, it is very likely that employment and income effects are more pronounced in more recent years.

Furthermore, as the pension data only include divorces with pension point adjustments, my sample might be selective and does not represent the total population of all divorcees in Germany in 2002. I, therefore, limit my results to the well-defined population of women with pension rights adjustments in the divorce process (which are roughly two thirds of the total divorce population).

List of abbreviations

ATT – average treatment effect on the treated

CIA – conditional independence assumption

DiD – difference-in-difference

VA – Versorgungsausgleichsstatistik (dataset)

GBM – general boosted models

KS – Kolmogorov-Smirnov-Statistic

ND – normalized difference

pp – percentage points

SUTVA – stable unit treatment value assumption

VSKT – Versicherungskontenstichprobe (dataset)

JEL classification

C14, J3, J12, J22

Declarations

Availability of data and materials

The data that support the findings of this study are available from the statutory German pension fund but restrictions apply to the availability of these data. The datasets generated and/or analyzed during the current study are available only in-house, i.e. in terms of a research stay at the pension system. However, scientific use files for the data sets VSKT and VA (coming soon) are available from the research center of the statutory German pension system at:

<http://forschung.deutsche-rentenversicherung.de/FdzPortalWeb/>

Competing interests

The author declares that he has no competing interests.

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Authors' contributions

Single authorship.

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Appendix

Table A. 2-1: Various logit estimations on the treatment indicator and employment (regular) status on all baseline covariates

Variable	logit	raw	logit matched	logit matched	logit matched	logit matched	Logit			
treatment indicator (divorce)	na	na	0.5733	***	0.4268	**	0.5186	***	0.5546	***
age at t ₀	-0.0016		0.0068		-0.0417		0.0194		-0.0367	
age difference to spouse at t ₀	0.0042		0.0010		0.0347		0.0283		0.0007	
marriage duration at t ₀	0.0315		-0.0114		0.038		-0.0001		-0.0263	
child before wedding (one if yes)	0.3437		-0.0650		0.0059		-0.1006		-0.065	
number of child 0-2 at t ₇₃₀	-0.6292	**	-0.0745		-0.3893		-0.3698		-0.2919	
number of child 3-5 at t ₇₃₀	-0.0813		-0.0404		-0.3759		-0.7683	***	-0.4616	**
number of child 6-15 at t ₇₃₀	-0.0451		0.0225		0.0654		-0.2236		-0.1841	
number of child 16 and older at t ₇₃₀	-0.2278		0.0371		0.0195		0.1552		0.0013	
childbirth in t ₇₂₉ to t ₃₆₅ (one if yes)	-2.6353	***	-0.0107		-3.2759	***	-2.7016	***	-1.2672	**
days schooling (higher) in t ₂₁₈₉ to t ₁₈₂₅	-0.0005		0.0007		-0.0005		-0.0014		0.0002	
days schooling (higher) in t ₁₈₂₄ to t ₁₄₆₀	0.0023		-0.0005		-0.0041		-0.0024		0.0006	
days schooling (higher) in t ₁₄₅₉ to t ₁₀₉₅	-0.0006		0.0003		0.0019		-0.0006		-0.0008	
days schooling (higher) in t ₁₀₉₄ to t ₇₃₀	0.0019		0.0004		0.001		0.0018		-0.0005	
days vocational training in t ₂₁₈₉ to t ₁₈₂₅	0.0012		-0.0008		-0.0025		-0.0031		-0.0036	
days vocational training in t ₁₈₂₄ to t ₁₄₆₀	0.0013		0.0021		0.0033		-0.0017		0.004	
days vocational training in t ₁₄₅₉ to t ₁₀₉₅	-0.0061		-0.0024		-0.003		0.0022		-0.0056	
days vocational training in t ₁₀₉₄ to t ₇₃₀	0.0067	*	-0.0004		0.0096	**	0.0069		0.0111	***
days care in t ₂₁₈₉ to t ₁₈₂₅	-0.0012		0.0001		0.0063		0.0042		0.0002	
days care in t ₁₈₂₄ to t ₁₄₆₀	0.0025		-0.0003		-0.0091		-0.0031		0	
days care in t ₁₄₅₉ to t ₁₀₉₅	-0.0028		0.0011		0.0037		0.0003		0.0024	
days care in t ₁₀₉₄ to t ₇₃₀	0.0022		-0.0006		-0.0021		0.0015		-0.0018	
days parental leave in t ₂₁₈₉ to t ₁₈₂₅	-0.0012		-0.0002		0.0037	**	0.0035	*	0.0022	
days parental leave in t ₁₈₂₄ to t ₁₄₆₀	0.0013		-0.0002		-0.0034		-0.0013		-0.0001	
days parental leave in t ₁₄₅₉ to t ₁₀₉₅	-0.0001		0.0003		-0.0015		-0.0021		-0.0023	
days parental leave in t ₁₀₉₄ to t ₇₃₀	-0.0013		0.0000		0.0023		0.0025	*	0.0025	*
days disabled in t ₂₁₈₉ to t ₁₈₂₅	0.0019		0.0020		-0.0048		-0.0126		-0.012	
days disabled in t ₁₈₂₄ to t ₁₄₆₀	-0.0006		0.0000		-0.021	**	-0.0104		-0.0054	
days disabled in t ₁₄₅₉ to t ₁₀₉₅	0.0041		-0.0049		0.0063		0.0029		0.0067	
days disabled in t ₁₀₉₄ to t ₇₃₀	0.0082	*	0.0056		-0.0045		-0.0075		-0.0026	
days unemployed in t ₂₁₈₉ to t ₁₈₂₅	0.0011		-0.0002		-0.0009		-0.002		-0.0028	*
days unemployed in t ₁₈₂₄ to t ₁₄₆₀	-0.0010		0.0002		0.0013		0.0023		-0.0011	
days unemployed in t ₁₄₅₉ to t ₁₀₉₅	0.0014		0.0002		-0.0033	*	-0.0027		0.0014	
days unemployed in t ₁₀₉₄ to t ₇₃₀	-0.0014		-0.0001		0.0015		0.0017		0.0006	
days marginal employed in t ₁₄₅₉ to t ₁₀₉₅	0.0045		0.0000		0.0018		0.0007		0.0021	
days marginal employed in t ₁₀₉₄ to t ₇₃₀	0.0014		0.0001		0.0054	**	0.0037	*	0.0034	*
days regular employed in t ₂₁₈₉ to t ₁₈₂₅	-0.0001		-0.0001		0.0006		0.0004		-0.0002	
days regular employed in t ₁₈₂₄ to t ₁₄₆₀	0.0010		-0.0002		0.0013		0.0029		0.0014	
days regular employed in t ₁₄₅₉ to t ₁₀₉₅	-0.0003		0.0000		0.0019		-0.002		0.0002	
days regular employed in t ₁₀₉₄ to t ₇₃₀	0.0013		0.0003		0.0055	**	0.0038	*	0.0042	*
daily marginal income in t ₁₄₅₉ to t ₁₀₉₅	0.0238		0.0047		0.0208		0.0102		0.0375	
daily marginal income in t ₁₀₉₄ to t ₇₃₀	0.0390		-0.0010		0.0208		0.0032		0.0357	
daily regular income in t ₂₁₈₉ to t ₁₈₂₅	-0.0042		0.0018		0.0039		0.0156	*	0.0094	
daily regular income in t ₁₈₂₄ to t ₁₄₆₀	0.0037		-0.0002		0.0055		0.008		0.0045	
daily regular income in t ₁₄₅₉ to t ₁₀₉₅	0.0032		0.0003		0.0141		0.0067		0.0039	
daily regular income in t ₁₀₉₄ to t ₇₃₀	0.0073		0.0026		0.0172	**	0.0158	*	0.0079	
yearly marginal income in t ₁₄₅₉ to t ₁₀₉₅	-0.0006		-0.0001		-0.0001		0		-0.0004	
yearly marginal income in t ₁₀₉₄ to t ₇₃₀	-0.0002		0.0000		-0.0007	**	-0.0004		-0.0005	
yearly regular income in t ₂₁₈₉ to t ₁₈₂₅	0.0000		0.0000		0		0		0	
yearly regular income in t ₁₈₂₄ to t ₁₄₆₀	0.0000		0.0000		-0.0001		-0.0001		0	
yearly regular income in t ₁₄₅₉ to t ₁₀₉₅	0.0000		0.0000		-0.0001	*	0		0	
yearly regular income in t ₁₀₉₄ to t ₇₃₀	0.0000		0.0000		0		0		0	
lifetime days schooling (higher)	-0.0001		0.0000		0.0002		0.0003	*	0.0001	
lifetime days vocational training	0.0000		0.0000		0.0003		0.0003		0.0005	**
lifetime days tenure	0.0000		0.0000		0		0.0001		0.0002	*
lifetime days tenure yearly	0.0004		-0.0003		0.003		0.0022		-0.0006	

cons	-1.3570	-0.1059	-1.5716	-1.7672 *	-2.4289 **	-0.3857
N	1850	1846	1846	1846	1846	1846
Pseudo R ²	0.0792	0.0033	0.3482	0.2904	0.2351	0.1928

legend: * p<0.05; ** p<0.01; *** p<0.001

Note: Training is occupational training. School is attending higher school. Following Sianesi (2004) the Pseudo R² in column 2 signals that the distribution of covariates is very well balanced within the matched sample because the value is very low. Thus, this is another sign that matching was successful (for comparison see value in column 1 for the raw sample). *Lifetime school*, *Lifetime training* and *Lifetime tenure* are cumulated days since age 15 to t₇₃₀. *Lifetime tenure yearly* is calculated by: $Lifetime\ tenure/[age -16-(Lifetime\ school/365)-(Lifetime\ training/365)]$.

Table A. 2-2: Selected baseline covariates used in logit estimation for the propensity score before and after matching (group A)

	raw sample			matched sample		
	treated	control	ND	treated	control	ND
<i>at t₀</i>						
age	38.33	36.73	0.25	38.33	38.06	0.04
age difference to spouse	3.33	3.19	0.03	3.33	3.23	0.02
marriage duration	14.86	13.00	0.27	14.86	14.62	0.03
<i>in t₇₂₉ to t₃₆₅</i>						
childbirth (one if yes)	0.01	0.05	-0.51	0.01	0.01	-0.03
<i>at t₇₃₀</i>						
number of child 0-2	0.14	0.28	-0.37	0.14	0.16	-0.04
number of child 3-5	0.37	0.50	-0.23	0.37	0.39	-0.04
number of child 6-15	1.06	1.08	-0.02	1.06	1.08	-0.02
number of child 16 and older	0.37	0.22	0.21	0.37	0.35	0.03
<i>in t₁₀₉₄ to t₇₃₀</i>						
days schooling (higher)	6.50	3.78	0.06	6.50	4.93	0.04
days vocational training	0.54	1.14	-0.10	0.54	0.79	-0.04
days care	11.70	9.89	0.03	11.70	13.75	-0.03
days parental leave	223.29	261.79	-0.22	223.29	229.19	-0.03
days work disability	0.00	0.00	NA	0.00	0.00	NA
days unemployed	6.78	12.36	-0.13	6.78	7.64	-0.02
days employed (marginal)	150.00	113.09	0.23	150.00	136.66	0.08
days employed (regular)	0.00	0.00	NA	0.00	0.00	NA
daily income (marginal)	3.76	3.14	0.16	3.76	3.49	0.07
daily income (regular)	0.00	0.00	NA	0.00	0.00	NA
yearly income (marginal)	1114.17	870.26	0.18	1114.17	1018.44	0.07
yearly income (regular)	0.00	0.00	NA	0.00	0.00	NA
<i>from first record to t₇₃₀</i>						
lifetime days schooling (higher)	309.07	419.36	-0.19	309.07	327.55	-0.03
lifetime days vocational training	425.54	401.22	0.05	425.54	412.22	0.03
lifetime days tenure	1517.30	1406.73	0.08	1517.30	1457.04	0.04
lifetime days tenure (yearly)	79.69	78.78	0.01	79.69	77.26	0.04
case numbers	144	654		142	642	

Note: Mean values for selected covariates (baseline). ND is the normalized difference: $\frac{\bar{x}_t - \bar{x}_c}{\sqrt{\frac{V_{xt} + V_{xc}}{2}}}$ with $V_{xt} = \frac{1}{(N_t - 1)}$ *

$\sum_{i=1}^{N_t} (x_{it} - \bar{x}_t)^2$ (V_{xc} respectively) (Rosenbaum and Rubin, 1985). Matched sample is based on kernel matching (Epanechnikov) with $h=0.082$. *Lifetime days tenure (yearly)* is calculated by: $Lifetime\ days\ tenure/[age-16-(lifetime\ days\ school/365)-(lifetime\ days\ training/365)]$. T₀ is the day the divorce file was opened. Case numbers for the raw and matched sample deviate because some cases are lost by kernel matching under the chosen bandwidth.

Table A. 2-3: Selected baseline covariates used in logit estimation for the propensity score before and after matching (group B)

	raw sample			matched sample		
	treated	control	ND	treated	control	ND
<i>at t₀</i>						
age	40.41	39.89	0.07	40.41	40.36	0.01
age difference to spouse	2.73	2.67	0.02	2.73	2.84	-0.02
marriage duration	16.50	15.52	0.13	16.50	16.53	0.00
<i>in t₇₂₉ to t₃₆₅</i>						
childbirth (one if yes)	0.00	0.00	NA	0.00	0.00	NA
<i>at t₇₃₀</i>						
number of child 0-2	0.04	0.11	-0.39	0.04	0.03	0.04
number of child 3-5	0.04	0.09	-0.28	0.04	0.03	0.04
number of child 6-15	0.66	0.67	-0.01	0.66	0.62	0.04
number of child 16 and older	0.51	0.52	-0.01	0.51	0.53	-0.03
<i>in t₁₀₉₄ to t₇₃₀</i>						
days schooling (higher)	0.00	0.00	NA	0.00	0.00	NA
days vocational training	0.00	0.00	NA	0.00	0.00	NA
days care	1.86	1.20	0.03	1.86	2.78	-0.05
days parental leave	88.39	133.00	-0.29	88.39	82.04	0.04
days work disability	3.28	1.91	0.09	3.28	3.21	0.01
days unemployed	6.74	8.69	-0.05	6.74	5.18	0.04
days employed (marginal)	3.12	4.17	-0.04	3.12	1.62	0.05
days employed (regular)	344.89	333.45	0.16	344.89	346.30	-0.02
daily income (marginal)	0.14	0.21	-0.06	0.14	0.08	0.06
daily income (regular)	56.00	54.83	0.04	56.00	56.58	-0.02
yearly income (marginal)	30.13	24.73	0.02	30.13	15.45	0.05
yearly income (regular)	19681.67	18539.90	0.11	19681.67	19881.25	-0.02
<i>from first record to t₇₃₀</i>						
lifetime days schooling (higher)	393.00	348.57	0.06	393.00	398.00	-0.01
lifetime days vocational training	534.91	585.07	-0.10	534.91	529.90	0.01
lifetime days tenure	4931.96	4989.86	-0.02	4931.96	4960.00	-0.01
lifetime days tenure (yearly)	250.76	261.38	-0.13	250.76	251.46	-0.01
case numbers	134	328		112	293	

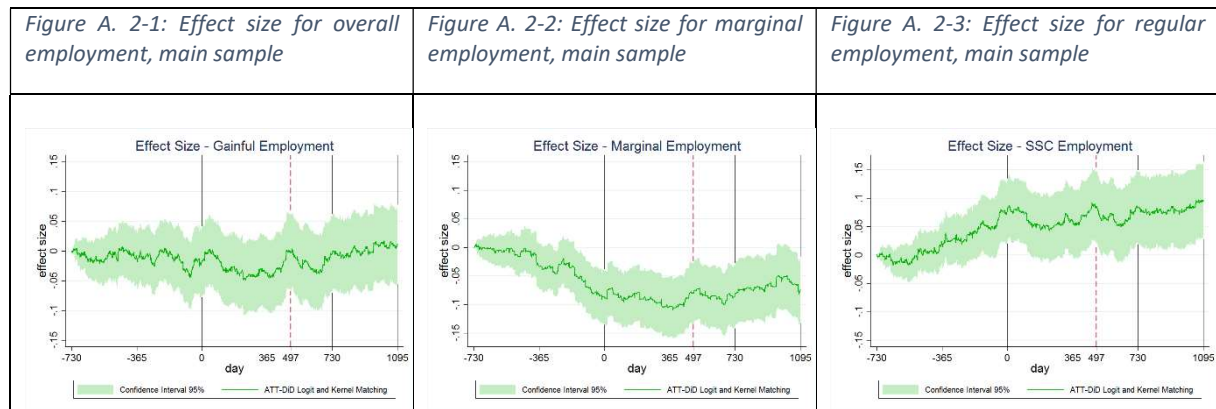
Note: Mean values for selected covariates (baseline). ND is the normalized difference: $\frac{\bar{x}_t - \bar{x}_c}{\sqrt{\frac{V_{xt} + V_{xc}}{2}}}$ with $V_{xt} = \frac{1}{(N_t - 1)} * \sum_{i=1}^{N_t} (x_{it} - \bar{x}_t)^2$ (V_{xc} respectively) (Rosenbaum and Rubin, 1985). Matched sample is based on kernel matching (Epanechnikov) with $h=0.038$. *Lifetime days tenure (yearly)* is calculated by: $Lifetime\ days\ tenure / [age - 16 - (lifetime\ days\ school / 365) - (lifetime\ days\ training / 365)]$. T_0 is the day the divorce file was opened. Case numbers for the raw and matched sample deviate because some cases are lost by kernel matching under the chosen bandwidth.

Table A. 2-4: Balance quality for raw, matched and weighted sample

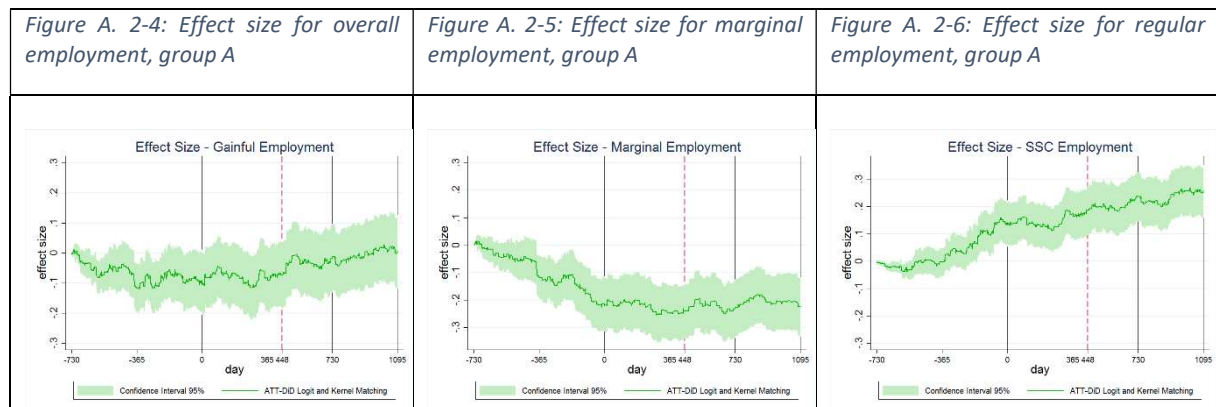
	number		normalized difference		Kolmogorov-Smirnov	
	treated	control	max ND	mean ND	max KS	mean KS
<i>main sample</i>						
raw sample	410	1436	0.988	0.125	0.186	0.061
logit model and kernel matching	410	956	0.078	0.012	0.066	0.020
GBM model and odd weighting	410	1104	0.229	0.037	0.051	0.026
<i>group A - women not regularly employed in $t_{.2189}$ to $t_{.730}$</i>						
raw sample	142	642	0.510	0.135	0.163	0.044
logit model and kernel matching	142	430	0.083	0.036	0.094	0.023
GBM model and odd weighting	142	428	0.203	0.065	0.088	0.024
<i>group B - women with strong labour market attachment (regular employment) in $t_{.2189}$ to $t_{.730}$</i>						
raw sample	112	293	0.691	0.111	0.151	0.041
logit model and kernel matching	112	177	0.079	0.031	0.113	0.033
GBM model and odd weighting	112	160	0.154	0.045	0.099	0.029

Note: Numbers in column 1 and 2 refer to the effective sample size. Numbers for the control group are based on either the kernel weights or the odd weights. Briefly, the effective sample size gives an estimate of the number of controls that are comparable to the treatment group. For an introduction, see for example Ridgeway *et al.* (2015: 9).

Figures for employment effects and income effects



Note: T_0 is the day the divorce file was opened. Red dashed vertical line represents the average day of divorce. SSC means employment with social security contribution, i.e. regular employment. The shaded area represents the 95% confidence interval.



Note: T_0 is the day the divorce file was opened. Red dashed vertical line represents the average day of divorce. SSC means employment with social security contribution, i.e. regular employment. The shaded area represents the 95% confidence interval.

Figure A. 2-7: Lower and upper bound for daily income based on weights from the logit model and derived under stochastic dominance, group B

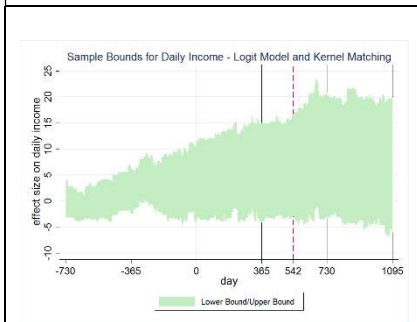
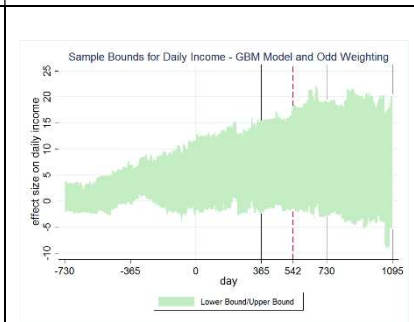


Figure A. 2-8: Lower and upper bound for daily income based on weights from the GBM model and derived under stochastic dominance, group B



Note: T_0 is the day the divorce file was opened. Red dashed vertical line represents the average days of divorce. Lower and upper bounds of daily incomes for the subgroup of women with strong labour market attachment in regular employment while married were derived by the principal strata framework (Zhang and Rubin, 2003) under the assumption of stochastic dominance.

Chapter 3

Brüggmann, D., Kreyenfeld, M. (2021): Earnings Trajectories after Divorce: The Legacies of the Earner Model during Marriage.

3 Earnings Trajectories after Divorce: The Legacies of the Earner Model during Marriage

3.1 Introduction

A divorce or separation is a turning point in a person's life course. As well as marking the endpoint of a romantic relationship, a divorce generally means that the couple will no longer live in a joint household (Thomas, Mulder, & Cooke, 2017). The partners' resources are no longer pooled, and their assets have to be split. In recent years, governments are increasingly adopting the concept of a 'clean break', in which the partners' financial obligations and claims terminate upon divorce (Martiny, 2012; Miles & Scherpe, 2020). Germany is also moving in this direction. In 2008, the German government enacted a 'maintenance reform', which emphasised the economic independence of both parties after divorce (Deutscher Bundestag, 2006). While ex-spousal support was fairly generous prior to this reform, it has since been restricted to exceptional cases (such as support for custodial divorcees with children under age three). The reform was motivated by the belief that in Germany, the compatibility of childrearing and employment has improved in recent years, and the division of labour has become more equal within couple households.²³

Despite the terminal nature of a divorce, the ex-partners share a joint past that may continue to be relevant over time. Past decisions – such as to reduce employment in order to take care of children – cannot be undone, but they continue to influence the employment and career options of the ex-spouses after divorce. This paper uses data from the German pension registers to study how the gendered division of labour during marriage affect the employment trajectories of ex-couples after divorce. We analyse the earnings and marital histories of 31,747 ex-couples (31,747 men and 31,747 women), all of whom filed for divorce in the year 2013. We have selected that particular year because it is recent enough to allow us to study the behaviour of the members of a divorce cohort who were separated after the enactment of the abovementioned reform, which assumes that both parties can be economically independent after divorce. We did not select a later divorce cohort because the observation window for examining their post-divorce behaviour would have been more limited.

Our study adds to the large body of longitudinal studies that investigated the economic ramifications of divorce and separation (Bonnet, Garbinti, & Solaz, 2021; Burkhauser, Duncan, & Hauser, 1991; Damme, Kalmijn, & Uunk, 2009; Duncan & Hoffman, 1985; Kalmijn, 2005;

²³ The draft law speaks of 'geänderte Rollenverteilung innerhalb der Ehe' (changing division of labour within marriages) (<https://dserver.bundestag.de/btd/16/018/1601830.pdf>).

McManus & DiPrete, 2001; Raz-Yurovich, 2013; Thielemans & Mortelmans, 2019; Weiss, 1984). These studies have shown that after a divorce or separation, women tend to expand their employment activities, and often earn more than they did while married, particularly in countries such as Germany (Bröckel & Andreß, 2015; Damme *et al.*, 2009). However, the research findings on the effects of divorce or separation on men's employment and earnings have been less conclusive than those for women (Covizzi, 2008; Kalmijn, 2005; McManus & DiPrete, 2001). Moreover, only a few studies have examined how the gendered division of labour during marriage affects the subsequent life courses of ex-couples (see, however, Bonnet *et al.*, 2021).

This paper contributes to the literature in several ways. *First*, we use large-scale register data to examine the ramifications of the specialisation within marriage for the ex-spouses' earning trajectories after divorce. *Second*, we contribute to the sparse literature on men's behaviour after divorce by comparing the earnings of women and men. *Third*, in addition to comparing the post-divorce trajectories of individual women and men, we follow ex-couples over time. Thus, the earnings that we observe for divorced women in the data can be directly compared to the earnings of their ex-husbands, which enables us to provide a precise account of the gender pay gap after divorce.

3.2 Prior research

3.2.1 Divorce, separation, and household income

In recent decades, the body of scholarly literature on the economic consequences of divorce and separation has grown substantially. It was, in particular, the increasing availability of longitudinal surveys since the 1980s that boosted research in this area (Burkhauser *et al.*, 1991; Duncan & Hoffman, 1985; Weiss, 1984). Most of these earlier studies were conducted by scholars with a strong interest in poverty research. As a result, these authors were less concerned with how divorce affects individual employment careers and earnings, and were more interested in examining the extent to which household income and government transfers ameliorate some of the negative consequences of divorce and separation. A consistent finding from this body of research is that women typically experience a sharp decline in equivalent household income after union dissolution (Burkhauser *et al.*, 1991). While the observation that divorce has adverse effects on women's economic standing is difficult to dispute, the evidence on the effects of divorce on men's economic well-being is more ambivalent. Scholars have found that in some

countries, including in Germany, men's equalised household income tends to increase substantially after divorce (Andreß, Borgloh, Brockel, Giesselmann, & Hummelsheim, 2006).

Concerns have been raised that some of these findings may be sensitive to the equivalent scales that standardised the household income (Boll & Schüller, 2021; Bonnet *et al.*, 2021). Household size shifts after a divorce because the ex-spouses split into different households. As the couple's children usually reside with their mother after a union dissolution, the gender differences are sensitive to the use of an equivalent scale, and, thus, to the weights that are used to account for the presence of children in the household (Boll & Schüller, 2021, p. 19; Bonnet *et al.*, 2021). The findings of studies that have examined subjective economic well-being do not support the assumption that divorce boosts men's economic standing. They generally show that both women and men experience a decline in subjective economic well-being following a divorce or union dissolution, although the effect tends to be slightly more pronounced for women than for men (Leopold, 2018). In a very recent paper, Bonnet *et al.* (2021) used French register data to examine the household income of women and men after divorce, and found that about one-half of all men and three-quarters of all women see a decline in their equalised income (after tax and transfers). The authors also observed stark differences in the results of the past earner model, which was operationalised over the share the respective partner contributed to the household income during the marriage. While the 'secondary earner' (regardless of gender) generally experienced a decline in his/her equalised household income, the 'main provider' (who contributed more than 80% of the household income) usually saw an increase in his/her equalised household income after union dissolution.

3.2.2 Divorce, separation, and individual earnings

A separate, but related strand of literature has examined how divorce and separation affects individual employment and earning careers. A consistent finding across most studies is that divorce leads to changes in women's employment and earnings, particularly in countries where married women tend to be less attached to the labour market. For example, based on survey data from the US, Tamborini, Couch, and Reznik (2015) reported that women's earnings increased substantially after divorce. Similarly, using data from the Divorce in Flanders Survey, Thielemans and Mortelmans (2019) found that in the years immediately after divorce, women's employment rates rose sharply. In a cross-national study, Van Damme, Kalmijn, and Uunk

(2009) observed more modest employment effects for some countries, but large employment effects for West Germany.

By contrast, the results for men suggest that union disruption negatively affects their earnings and employment rates. However, the magnitude of the observed effect differs greatly across studies and outcome variables. In an analysis of register data from Israel, Raz-Yurovich (2013) reported that while men's employment stability (defined as continuous employment throughout a given year) deteriorated after union dissolution, divorce did not affect men's earnings or their salary growth rates. In a study based on Dutch survey data, Kalmijn (2005) found that men's risk of unemployment increased after divorce. These results were corroborated by Covizzi (2008) using data from the Swiss Household Panel. The study's findings indicated that union dissolution had a more pervasive effect on men's than on women's likelihood of being unemployed, and that poor health was an important intervening variable (see also Biotteau, Carole, & Cambois, 2019; Couch, Tamborini, & Reznik, 2015).

3.3 Institutional background and research question

3.3.1 Family policies and maintenance regulations

Whether and, if so, how divorce affects the ex-spouses' employment and earning careers depends on the country context, the prevailing gendered employment patterns, and the policies that regulate post-separation behaviour. The German government has introduced important policy reforms in recent years, including the expansion of day care for children under age three as well as the implementation of an income-related parental leave benefit system in 2007. Various studies have shown that these reforms had a sizeable impact on mothers' full-time employment rates (Geyer, Haan, & Wrohlich, 2015) and fathers' uptake of parental leave (Geisler & Kreyenfeld, 2019).

Regardless of these developments, maternal and paternal employment patterns still differ radically in Germany. While most fathers work full-time, most mothers substantially reduce their working hours after their first child is born. Similarly, the division of household labour has remained strongly gendered, although there are signs that fathers are starting to perform more child care (Köppen & Trappe, 2019). There are many reasons why these gender differences in care and employment persist. It is, however, clear that although the recent reforms signal the German government's clear commitment to the dual-earner model, other policies have remained in place that support the married breadwinner family model, including the

'income splitting' system and the free co-insurance of the non-working or marginally employed spouse in the public health care system (BMFSFJ, 2021). These benefits only accrue to married couples. Thus, divorce leads not only to a change in the tax treatment of the ex-spouses, but also to the non-working or marginally employed spouse in a single-earner constellation losing his/her free health insurance. Therefore, divorce creates a strong incentive for previously non-working or marginally employed ex-spouses to find employment.

Some of the adverse effects of divorce are buffered by the possibility of collecting ex-spousal maintenance payments. In line with the logic of a conservative male breadwinner regime, the German regulations shielded the 'weaker part' in a marriage from the loss of the male breadwinner. Until recently, this system was (from the perspective of the claimant) probably one of the most generous in Europe. Ex-spousal support payments were based on the logic that 'marital solidarity' extends beyond the breakdown of a union, and that ex-spousal maintenance payments should reflect the prior standard of living of the couple. The 'care-giving spouse' was not obliged to be employed before the couple's youngest child turned eight years old, and was expected to work part-time when the child was between ages eight and 15. Thus, the resident parent was not expected to be in full-time employment until the child was 15 years old, and it was only at that point that the refusal to take up full-time work could be used as a reason to curb ex-spousal support payments.

In 2008, a major reform came into force. Instead of 'spousal solidarity', the reform emphasised the economic independence of the ex-partners after divorce. Whereas previously, the resident parent was not obliged to be employed until the couple's youngest child was eight years old (or to be full-time employed until the child was 15 years old), the threshold at which the resident parent had to be employed was lowered to the youngest child's third birthday. Under the reform, the amount and particularly the duration of ex-spousal support were sharply reduced, with support generally being provided only in the period immediately after the divorce (Peschel-Gutzeit, 2008; Röthel, 2009). Several observers argued that the reform was too extreme, as it did not take into account the actual behaviour (Lenze & Funcke, 2016). The assumption underpinning the reform that the resident parent would be economically independent did not seem to fully match the realities in Germany, where most married couples still practiced a rather traditional division of labour. In 2013, the regulations were adjusted, with a clause being added to the law that allowed for ex-spousal support to be granted in hardship cases, such as in cases in which the couple had been in a very long marriage with a very unequal division of labour. There is, unfortunately, little evidence on the effects of these changes in ex-spousal support

payments. Estimates based on the German Family Panel suggest that the share of female divorcees with children who received ex-spousal support declined from around 10% in 2009/10 to 3% by 2018/19.²⁴ Moreover, the official court statistics indicate that there was a rapid decline in cases related ex-spousal maintenance. For example, the share of the family court proceedings that dealt with ex-spousal support decreased from 12% in 2006 to 4% in 2019 (Statistisches Bundesamt, 2020).²⁵

Apart from providing ex-spousal support, the non-resident parent is obliged to pay child maintenance, with the amount varying depending on the income of the non-resident parent. Currently, the question of whether maintenance regulations should better account for shared physical custody arrangements is being debated. However, only a very small share of divorced parents in Germany practice shared physical custody. In the overwhelming majority of cases, the couple's children live with their mother after divorce, while the other parent has a 'right of contact' (*Umgangsrecht*). As in other countries, a large fraction (roughly 40%) of non-resident parents in Germany do not pay any or the full child maintenance amount (Hubert, Neuberger, & Sommer, 2020), usually because the non-resident parent is unable to pay, or the resident parent wants to avoid conflict or contact with the non-resident parent (Andreß, Borgloh, Güllner, & Wilking, 2003).

3.3.2 Research question & expectations

The maintenance reform of 2008 replaced the notion of 'ex-spousal economic solidarity' with the concept of 'individual economic independence'. In the following, we examine whether divorcees are indeed able to achieve this 'economic independence' by analysing their earning trajectories around the time of their divorce. In particular, we investigate how the divorcees' earning trajectories are related to the earner model they followed during their marriage. While getting divorced may not affect the earning trajectories of dual-earner couples, it should have a large impact on couples who had previously been organised as a male/female breadwinner family. As secondary earners face pressure to earn their own living after a divorce, the earnings of secondary earners should increase around the time of the divorce. In most cases, secondary earners see their earnings increase because they have expanded their working hours, or because

²⁴ Own estimates based on the German Family Panel (Huinink *et al.*, 2011).

²⁵ These proceedings also include pending cases as well as re-negotiations of ex-spousal support of past divorces.

they have moved to a job that offers better career options and higher wage growth. In Germany, significant shares of married women are not employed or are only marginally employed. These women lose their access to free health insurance coverage through their ex-spouse when they divorce, which creates a distinct incentive for them to search for regular employment.

Marital specialisation usually entails a devaluation of human capital for the party who specialises in domestic work. While this specialisation may already factor in that the secondary earner expects to have lower earnings than the prime earner, she (and, in some cases, he) may have missed out on crucial career opportunities during marriage, which can increase the differences in the earning potential of the two partners in a single-earner constellation. As a result, it may be difficult for the secondary earner to earn enough after a divorce to achieve economic independence. Furthermore, joint physical custody arrangements are still rare in Germany. In most cases, the couple's children continue to reside with the parent who had previously been the main care-giver (usually the mother). Thus, child care responsibilities maybe an additional factor that inhibits the labour market participation and the employment success of a divorcee who had previously been the secondary earner in a single-earner constellation.

While divorce creates a strong incentive for the secondary earner to establish economic independence, the pressure on the prime earner to guarantee the economic security of the household may ease after divorce. As a result, s/he may be more likely than before to turn down less attractive opportunities to advance in his/her career. Most importantly, however, the prime earner loses the privileges that come from the tax and transfer system. During marriage, the main earner is not fully taxed, as s/he receives a tax credit if his/her spouse is earning less or is not working.²⁶ These benefits are lost upon divorce, such that each additional increase in earnings is more heavily taxed than before, which may, in turn, create negative incentives for the main earner to expand his/her employment or to advance in his/her career. There are also other monetary incentives that may affect the behaviour of divorcees. If the couple had dependent children, the non-resident parent is obliged to pay child maintenance based on his/her

²⁶ The 'income splitting system' means that earnings are summed up and the sum is divided by two before taxation. Due to the progressive tax schedule, this pooling is advantageous for couples where the spouses' earnings differ, as it leads to a lower overall tax burden than individual taxation. Assuming couples actually pool their resources, both partners in a married couple may profit from the system of income splitting. In practice, however, the earnings are taxed based on an individual tax bracket ('Steuerklassen'). The main earner often chooses the tax bracket #5, which means that his (and, in theory, also her) earnings are taxed less heavily, while the earnings of the secondary earner are taxed more heavily (for details, see BMSFJ 2021, section 8). As a result, the net individual earnings of the primary earner are substantially higher in this system than in a system of individual taxation. The net earnings of the secondary earner, by contrast, would be lower than in a system of individual taxation.

monthly net income.²⁷ It is possible that some non-resident divorcees deliberately reduce their employment engagement in order to avoid making child maintenance payments. Moreover, the divorcees' health, which may deteriorate around the time of their divorce, could be an important intervening variable that causes their earning trajectories to be less steep than those of a control group of people who did not undergo a divorce.

3.4 Data and Methods

3.4.1 Data and analytical sample

The empirical analysis presented in this paper is based on register data from the German Pension Fund. The German pension registers cover more than 90% of the resident population in Germany, but members of certain professions (such as farmers and lawyers) and civil servants are not included. The pension registers have been used in the past for research on the labour market (Guertzgen & Hank, 2018), fertility behaviour (Hofmann, Kreyenfeld, & Uhlenborff, 2017), and the economic consequences of divorce (Brüggmann, 2020). We have combined two data files from the registers for our investigation (via linkage over a unique identifier): i.e., the VA statistics (*Versorgungsausgleichsstatistik*), which contain biographical information on the dates of marriage, separation, and divorce for the divorced population (Keck, Radenacker, Brüggmann, Kreyenfeld, & Mika, 2019) were combined with the AKVS statistics (*Aktiv-Versicherten-Statistik*) for the years 2010 to 2015, which include employment and earnings records.

We have restricted our analytical sample to couples who filed for divorce in the year 2013. Thus, this year is well after the date when the abovementioned reform of the maintenance law was implemented. It is, however, early enough that we have a sufficient time to observe the ex-spouses after their divorce. In addition, we have restricted the analysis to couples in which the man was between ages 30 and 55 at time of separation (or, more specifically, when the divorce was filed). Thus, outliers who divorced very early in life are not considered. Moreover, divorces at older ages are not included, as these older individuals may have retired in our observation

²⁷ Child alimony depends on the earnings of the non-resident parent, whereby the payments decline with increasing earnings on a sliding scale. As a result, non-resident parents with low incomes are relatively heavily burdened by child alimony payments. However, non-resident parents with very low incomes are exempt from the payments. In 2020, the threshold was €1,160 per month for full-time employed non-residential parents and €960 per month if the non-residential parent was not working full-time or was unemployed. https://www.olg-duesseldorf.nrw.de/infos/Duesseldorfer_Tabelle/Tabelle-2020/index.php

window. We also omitted shorter marriages (less than three years), as they are not fully covered in the data (for details, see Keck *et al.*, 2019). In addition, we restricted the analysis to western Germany because the sample sizes are much smaller for eastern Germany (especially for subsamples that we construct), and because non-marital childbearing is very prevalent in eastern Germany, which means that marriage is more selective (Konietzka & Kreyenfeld, 2002). We also excluded foreign nationals from the investigation because information on their employment, marital, and fertility careers may be incomplete in the registers; i.e., it may not be available for the period prior to migration. The final sample includes 31,747 couples, and thus 63,494 persons (31,747 men and 31,747 women).

3.4.2 Method and analytical strategy

When couples divorce, the earnings of the ex-spouses may increase or decrease. However, there are other factors as well that can lead to changes in the earnings of divorcees over time. Divorce typically happens around age 40, and thus at a point in the life course when people can still experience an increase in their earnings. Thus, in order to assess the ‘true’ impact of divorce on earnings, it is important to compare divorcees with a suitable control group of people who did not divorce in the given time window. Furthermore, if divorcees have particular characteristics, such as poor health status, the true effect of divorce may be overestimated.

We used a matching approach to address the abovementioned issues. Thus, we constructed a control group of married couples drawn from the same datasets who separated later, but not before 2018. Hence, we assume that those couples did not anticipate their separation five years in advance. Moreover, the control group consists only of couples who were in their first marriage in 2010. Thus, couples who had been married more than once were excluded from the control group (for further details on the construction of the control group, see the appendix). Our aim was to compare the earning trajectories of the divorcees with the earning trajectories of the control group. We achieved balanced treated and control samples by applying TWANG from the R library (Cefalu *et al.* 2021, McCaffrey *et al.* 2004).

Most earlier matching algorithms used logistic regression to calculate the propensity score. The question of how these models should be specified has yet to be fully resolved, even though numerous covariate selection algorithms have been written to address this issue (Dehejia and Wahba 1999; Hirano and Imbens 2001; McCaffrey *et al.* 2004; West *et al.* 2000: 69-70). With the development of machine learning (ML), new, flexible, nonparametric ways of addressing

the covariate selection problem have become available. Research has shown that simple logistic models without interaction terms are often not sufficient to balance covariates. ML algorithms outperform the standard approach to estimating the propensity score by means of logistic regression techniques (Pirracchio *et al.* 2014; Westreich *et al.* 2010; Lee *et al.* 2010). We followed that strand of research by applying the data-adaptive approach of TWANG. The appendix includes further details on the matching procedure, as well as results from alternative estimation strategies that have generated results similar to those presented below (fixed-effects models as well as fixed-effects models combined with weighting).

3.4.3 Variables

The main outcome variable of interest is *annual gross earnings*. Earnings records are stored in the registers as earning points. One earning point denotes the average earnings of an employed worker in Germany. Individuals who were not employed enter the calculation of the mean with zero earnings.²⁸ For descriptive purposes, we also present further outcome variables that map each individual's employment situation. *Employment* was operationalised over the share of days in a given calendar year during which a person has been in 'regular employment'²⁹ (not including marginal employment). We also account for *marginal employment*, as measured by the share of days in a calendar year that a person spent in marginal employment. Finally, we also provide insight into *unemployment*, as measured by the share of days in a calendar year that a person has been registered as unemployed.

The key variable of interest is *divorce*. German divorce law includes a separation period, which means that spouses have to be separated for at least one year before their marriage can be legally dissolved. Thus, a legal divorce may not be finalised until several months or even years after the breakdown of the union. The register data also contain information on the date when the divorce was filed (i.e., the date when the defendant received the divorce petition). We use the latter date and label this as the separation date. The analytical sample includes couples who filed for their first divorce in 2013. Higher order divorces are not considered. The divorcees'

²⁸ In addition, earnings are capped in the registers due to the income threshold (ceiling) for social security contributions. The threshold varied between €66,000 in 2010 and €72,600 in 2015. This means that income changes due to divorce are not fully observed for high earners.

²⁹ Regular employment is defined as employment with social security contributions.

earnings and employment outcomes are estimated for a five-year period around the time of their separation, resulting in a panel structure from t_{-2} to t_{+2} (2011 to 2015).³⁰

Another key variable of interest is the *earner model* that the divorcees practiced during their marriage. We identified four different earner models based on the spouses' income from employment with social security contributions for two consecutive years (2010 and 2011). In the *male breadwinner model*, the woman earned less than 0.5 pension points in the years 2010 to 2011, while the man earned 0.5 pension points or more. In the *dual-earner model*, both spouses had an income equal or greater than 0.5 pension points. In the *female breadwinner model*, the man earned less than 0.5 pension points, while the woman earned 0.5 pension points or more. In the *both low income model*, both spouses earned less than 0.5 pension points.

We use a battery of *control variables* for the matching procedure (see the appendix for details). The main socio-demographic variables are the ages of the spouses at separation and the duration of the marriage (between marriage and separation). Unfortunately, this dataset does not include full birth biographies, which means that we cannot control for the number and the ages of the children, or conduct investigations by the number and the ages of the children. We do, however, have some information on births that occurred in the observation period. This information is used for the matching procedure.

Table 3-1 provides descriptive statistics of our sample. It shows that the average duration of marriage (up to the date the divorce was filed) was 15.5 years, and that the average age at separation was 43.4 for men and 40.8 for women. A majority of the divorced couples followed the male breadwinner model (56%), while a smaller share followed the dual-earner model (24%). Only 6% of the couples followed the female breadwinner model. In 14% of the cases, neither of the spouses received more than 0.5 earning points, meaning that their earnings were 50% below average.

The table also provides some initial insights into the gender differences in earnings. While divorced men acquired, on average, 1.06 earning points in the 2011 to 2015 (t_{-2} to t_{+2}) period, divorced women accumulated, on average, 0.44 earning points over this period. An earning point of one equals the average earnings in a given year. Thus, around the time of their divorce, women were earning only 44% of the average earnings. The table also provides insights into other labour market outcomes. Among the divorcees in the sample, men had an employment

³⁰ Data for 2010 are also used for the construction of the control group (see the appendix for details).

rate of 81%, while women had an employment rate of 63%. Conversely, much larger shares of women (21%) than of men (8%) were in marginal employment. Finally, we observe that 7% of the men and 14% of the women were registered as unemployed.

Table 3-1: Descriptive statistics, mean and %

	Men	Women
Age at separation (mean)	43.4	40.8
Earning points (mean)	1.06	0.44
% regular employment	80.5	62.9
% marginal employment	8.0	20.9
% unemployed	6.7	13.5
Marriage duration until separation in years (mean)	15.5	
Sample size		
Male breadwinner couples	17,897	
Dual-earner couples	7,531	
Female breadwinner couples	1,759	
Both low income couples	4,560	
Total number of couples	31,747	
Number of couple years	190,482	

3.5 Results

3.5.1 Men's and women's earnings and employment around the time of divorce

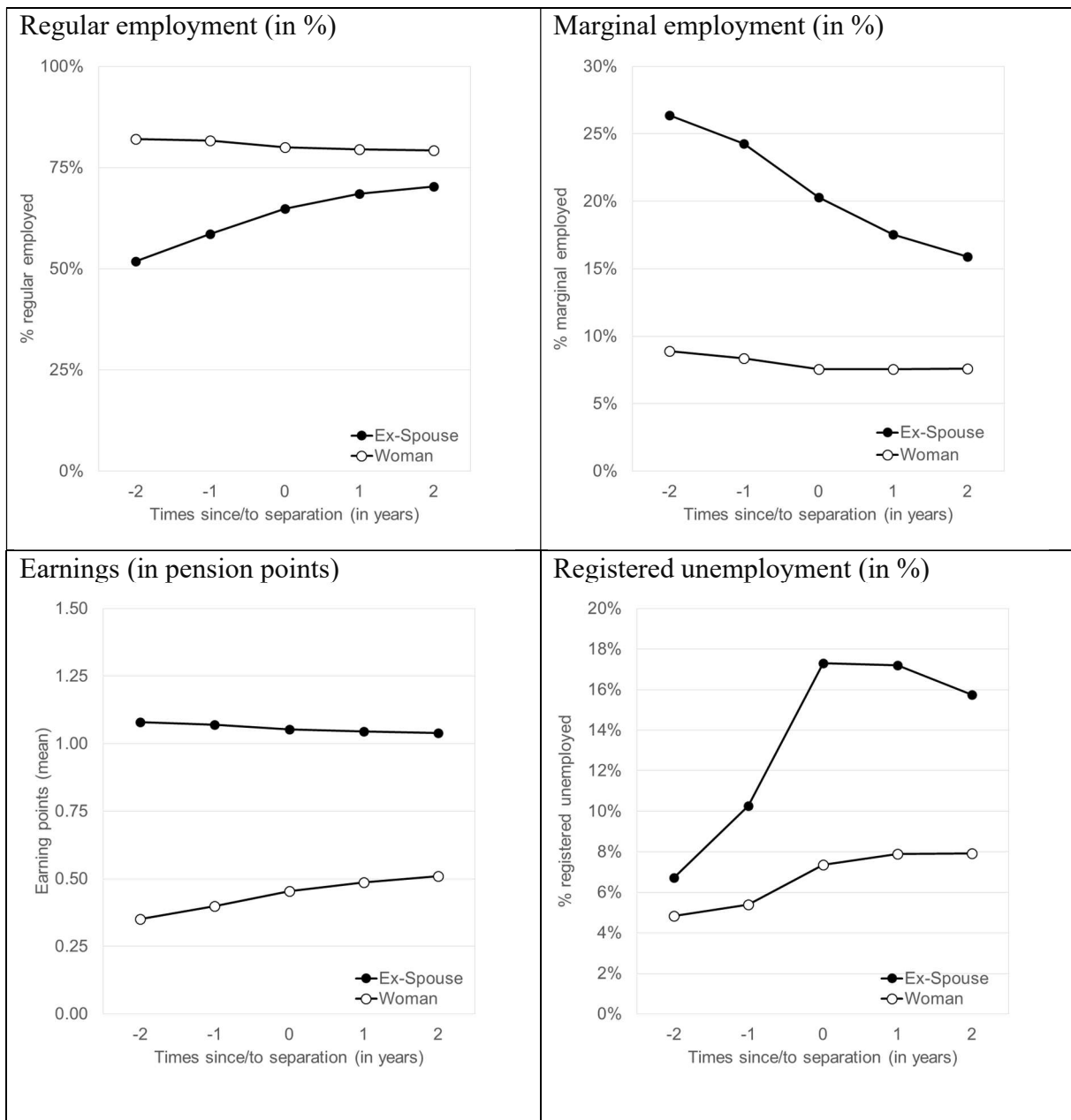
Figure 3-1 maps the employment and earning trajectories of divorcees around the time of divorce. The upper-left panel provides the regular employment rates (excluding marginal employment). In the figure, the female divorcees are labelled 'women' and then men 'ex-spouses' to illustrate that the sample includes previously married partners. The figure shows that the men's regular employment rates remained rather constant across time, while divorce seemed to prompt women to enter the labour market. Female employment rates increased from 50% two years prior to the separation to roughly 70% two years after. Even though a small gender gap in employment remained, by two years after the separation, the ex-spouses had almost reached parity in terms of regular labour market participation.

Much of the increase in women's regular employment was the result of a rapid decline in marginal employment around the time of separation (Figure 3-1, upper-right panel). Although large numbers of women entered the labour market around the time of their separation, they did

not get close to earning as much as their ex-husbands (Figure 3-1, lower-left panel). Two years after their separation, the divorced women were earning roughly 50% of average earnings, while their ex-spouses were earning a bit more than the average earnings.

Figure 3-1 (lower-right corner) also provides insights into the development of unemployment around divorce. As this figure shows, female unemployment rose sharply in the year of separation. This likely occurred because unemployment benefits are means-tested for individuals who have not been working before or who are unemployed for a longer duration. Married women often did not have access to this benefit because the earnings of their partner were assessed in the calculation of the benefits. Upon divorce, these women were not only more likely to be in need of this benefit, they were also more likely to be eligible, because the husband's income was no longer assessed in the calculation of the benefits. Being registered for unemployment also meant that these women gained access to health insurance coverage, which they may have lost when they divorced because they were previously covered by the health insurance of their ex-spouse if they were not working.

Figure 3-1: Regular employment, marginal employment, earnings, and unemployment before/after separation



Source: Combined dataset consisting of VA 2019 (Versorgungsausgleichsstatistik) and AKVS (Aktiv-Versicherten-Statistik) for the years 2011 to 2015.

3.5.2 Earning trajectories by the prior earner model

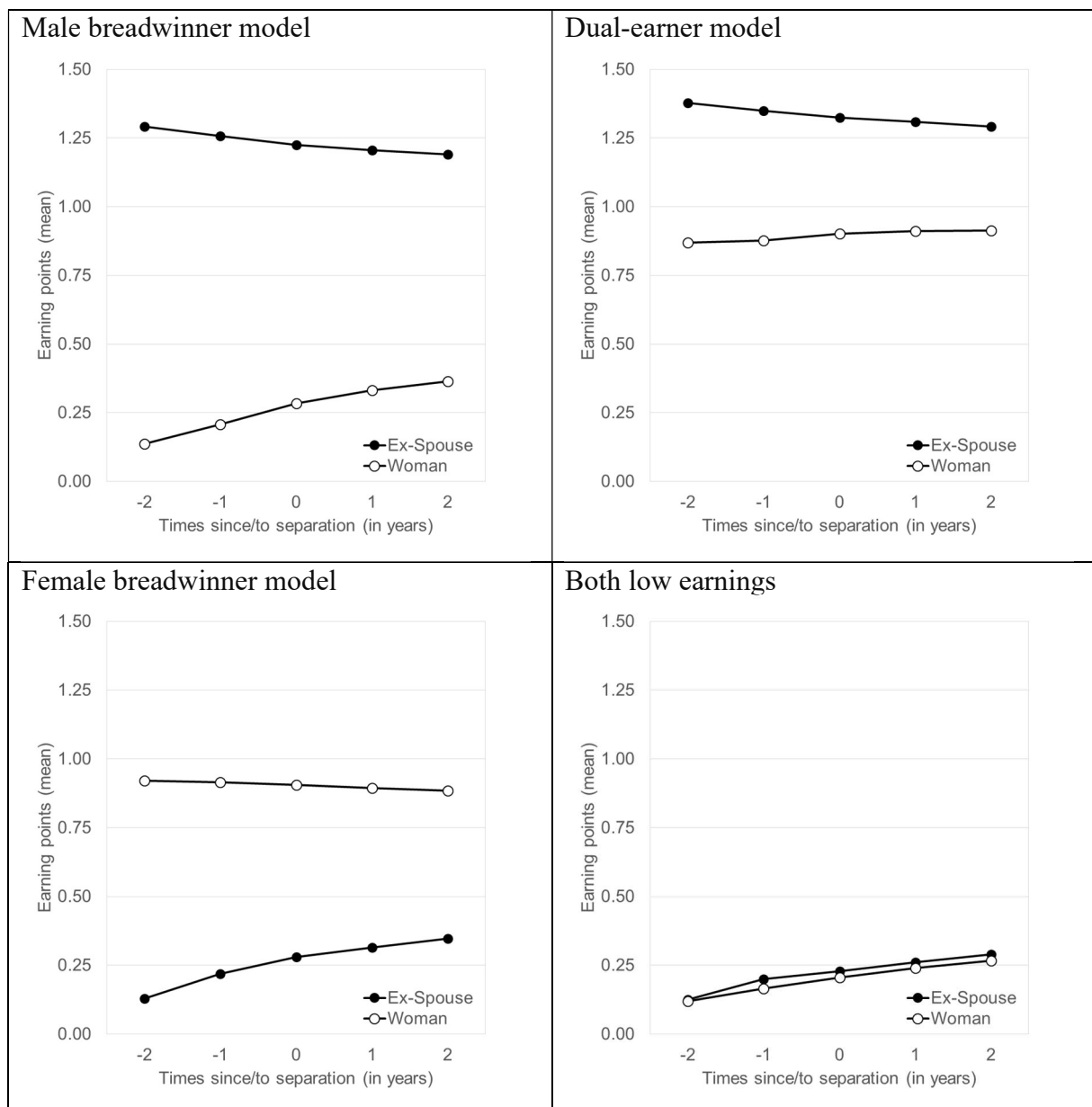
Figure 3-2 displays the earning trajectories by the prior earner model. The upper-left corner displays the earning trajectories of the couples who followed the male breadwinner model (the wife earned less than 50% of average earnings, while the husband earned more than 50%). We can see glaring gender differences in earnings. Although the women's earnings increased and the men's earnings declined across the observation window, large differences persisted. Two

years after their separation, the women were earning 65% less than average wages, while the men were earning 20% more than average wages. Thus, based on these simple descriptive statistics, the assumption that women who were previously in a male breadwinner constellation were able to achieve economic independence after divorce can be refuted. Instead, it seems that the weaker spouse in a couple with a high degree of specialisation during marriage was economically punished after divorce, and that the loss in relevant labour market skills had a prolonged effect on the wage gap between the ex-partners.

The results of the analysis also suggest that divorce led to a decline in the labour market engagement of men who had previously been breadwinners. The average earnings of the men in this group decreased from 29% to 20% above average. Interestingly, the patterns of the female and the male breadwinner constellations (lower-left panel of Figure 3-2) were very similar. In the female breadwinner couples, the earnings of the breadwinner also declined in the observation window. In the dual-earner couples (upper-right corner of Figure 3-2), the earning trajectories of the women were not greatly affected by the divorce, although the labour market earnings of the men declined over time. In couples in which neither partner was well-established in the labour market during their marriage (lower-right panel of Figure 3-2), there was some degree of gender equality in their post-divorce trajectories: i.e., the earnings of both spouses increased, but remained at very low levels (roughly 75% below average earnings two years after the divorce).

A conspicuous result of these investigations is that the breadwinners as well as the men in dual-earner arrangements experienced a decline in their earnings across the observation window. This effect may be described a ‘floor effect’, whereby individuals who already had high earnings before their divorce were at higher risk of seeing their earnings drop after their divorce than individuals who previously had low earnings. Thus, it is important to compare the trends among divorcees to those of a similar control group (see below).

Figure 3-2: Average earnings of the treated by time since separation, gender, and earner model



Source: Combined dataset consisting of VA 2019 (Versorgungsausgleichsstatistik) and AKVS (Aktiv-Versicherten-Statistik) for the years 2011 to 2015.

3.5.3 Earning trajectories of the treated and the control group

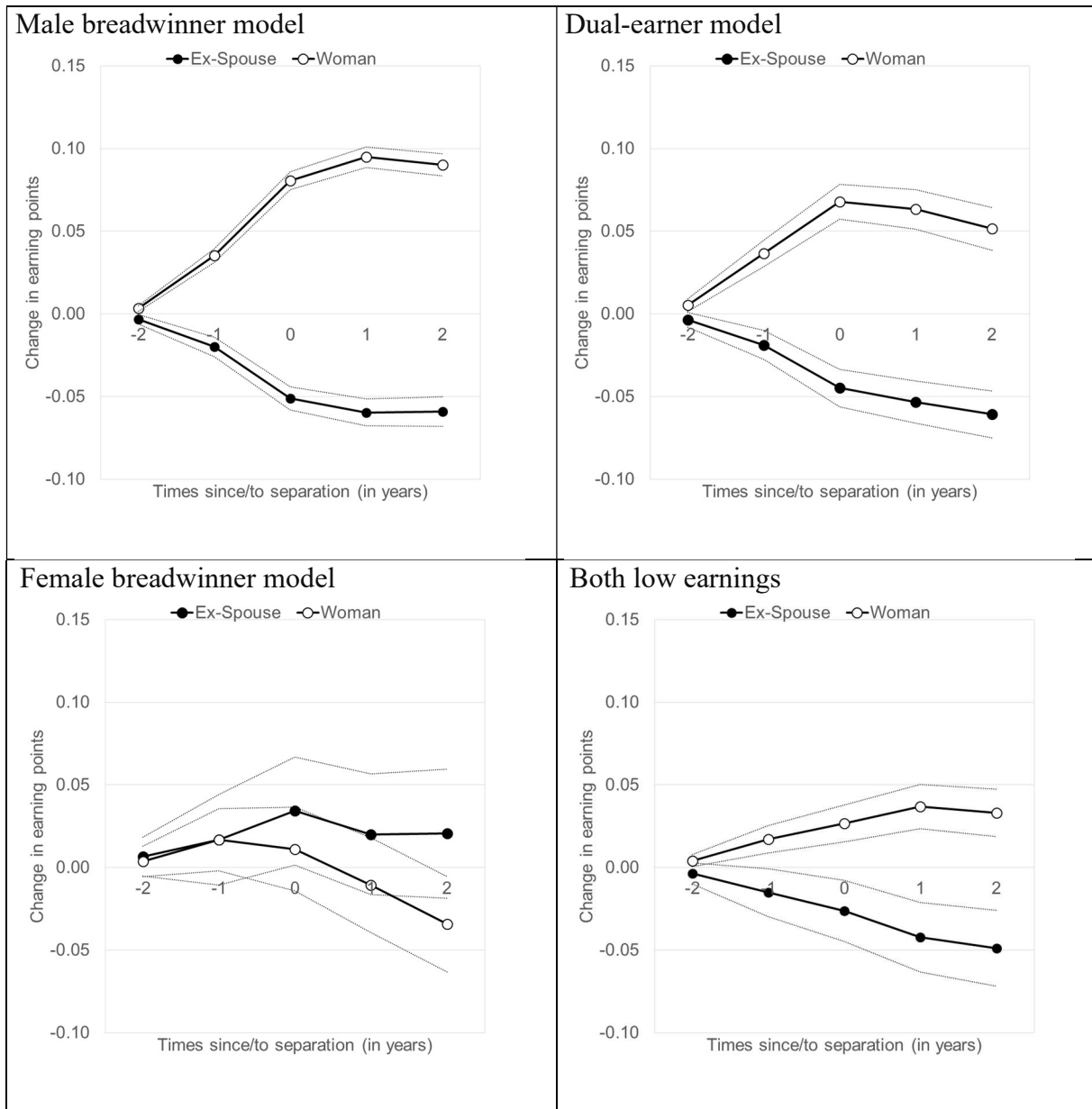
The descriptive statistics (Figure 3-2) can be misleading in the absence of a counterfactual. Figure 3-3 compares the earning trajectories of the treated group with those of a control group with similar characteristics. To the extent that the conditional independence assumption (CIA) is satisfied, the outcome of the control group is the counterfactual outcome of the treated group if they had not experienced a divorce (see the appendix for further assumptions). The figure shows the increase in earnings in relation to t_2 . Apart from the women in female-earner

constellations (who make up only a small fraction of the sample), the earnings of the women in the treated group increased in all constellations in comparison to $\frac{1}{2}*(t_3+t_2)$, and in comparison to the earnings of the women in a control group with similar baseline characteristics. The increase was greatest in the male breadwinner arrangement. At t_{+2} the treated women earned, on average, 0.09 earning points more than the women in the control group.³¹ If we translate those 0.09 earning points into euros, then the difference to the control group's average in 2015 amounted to €3,183.³² While the treated women saw their earnings increase, the treated men in the dual-earner, the male breadwinner and both-low-income constellations saw their earnings decrease relative to those of the men who did not separate in 2013. In the male breadwinner and the dual-earner constellation, earnings declined by 0.06, while in the both-low-income group, earnings decreased by 0.05 earnings points.

³¹ Here we estimate a difference-in-difference. Since we matched the treated group with a control group, we can reasonably argue that both groups have similar incomes in t_2 and t_3 (see Table A. 3-2 in the appendix). Hence, the difference-in-difference simply converges to the difference in incomes in 2015.

³² In 2015, one earning point equals €35,363.

Figure 3-3: Difference-in-difference approach. Change in earnings (reference year t_2). Effect for the treatment group (divorced) compared to the control group (married)



Source: Combined dataset consisting of VA 2019 (Versorgungsausgleichsstatistik) and AKVS (Aktiv-Versicherten-Statistik) for the years 2011 to 2015.

3.6 Conclusion

This paper examined the earnings of divorcees in western Germany. We sought to answer the question of whether and, if so, how the earner model men and women followed during their marriage affected their earnings trajectories after a union dissolution. We argued that German law increasingly demands that the previously married partners achieve economic independence, particularly since the implementation of the maintenance reform of 2008. This means that following a divorce, the secondary earner in a single-earner constellation experiences

immediate pressure to enter the labour market or to increase his/her working hours. In the German context, this pressure can be especially acute because single-earner couples have privileges that they lose when they divorce, such as the free co-insurance of the non-earning or marginally employed spouse in the health care system. We therefore argued that women in male breadwinner constellations face strong pressure to be economically independent after a divorce. However, whether these women are able to achieve economic independence was an empirical question that had yet to be answered.

To provide evidence to help answer this research question, we used data from the German pension registers. We selected a homogenous cohort of couples, all of whom separated in the year 2013. Thus, we examined couples who experienced a divorce well after the abovementioned maintenance reform, which requires both partners to achieve economic independence. Moreover, by selecting divorcees from only one divorce cohort, we made sure that the results were not driven by changes in behaviour across cohorts or time periods. An additional advantage of using these data was that they were for couples, which enabled us to directly compare the earnings of the ex-partners after divorce. Our descriptive investigation showed that the employment rates of male and female divorcees converged in the process of separation. This convergence was the result of a significant share of women transitioning from marginal to regular employment around the time of their divorce. However, women's rates of registered unemployment also rose sharply around the time of their divorce. While average female earnings increased in the process of divorce, substantial gender differences persisted. These differences were largest among divorcees who were previously in a single-earner constellation. In this group, the women earned roughly 65% less than average, while the men earned 20% more than average after their divorce. The overwhelming majority of single breadwinner constellations were male breadwinner arrangements. However, even in the small share of female breadwinner constellations, we found comparable patterns, although the gender gap was less pronounced.

An intriguing finding from our investigation was that men's earnings declined in the process of divorce. We adopted a rigorous causal approach to identifying the size of this 'divorce penalty'. Such a causal approach seems warranted, because a simple comparison of the earnings of divorcees before and after divorce may understate the true effect of divorce, as earnings generally increase across the life course. We employed matching techniques to compare the behaviour of the treated (divorced) group with that of a group of married individuals with similar characteristics (control). We also employed fixed-effects modelling as an alternative

way to determine the causal effect of divorce (see the appendix). These different causal approaches produced results that were comparable in terms of both the direction and the size of the effects. Thus, we are fairly confident that men in male breadwinner constellations saw a decline in their earnings of roughly five percent, compared to the counterfactual situation in which they had remained married.³³

While we identified a large and negative divorce penalty for men, explaining that pattern is more difficult. We had assumed that this divorce penalty was attributable to the change in tax treatment that divorced couples experience, which incentivises the secondary earner to work more, while the main breadwinner is incentivised to work less. The main breadwinner's individual earnings are more heavily taxed following a divorce, which reduces his/her incentives to engage in career advancement. We therefore assumed that we would see a decline in the earnings of prime earners in single-earner constellations, while the earnings trajectories of dual-earner couples would be less affected by divorce. However, we observed a similar male divorce penalty in dual-earner couples. Thus, we do not have any firm evidence for our claim. It is possible that our measure of earner models was too rough, as in our dual-earner constellations as well, the woman generally earns less than her partner. It is also possible that other mechanisms were at play that we were unable to fully account for in our investigation, such as the deterioration of the health status of the divorcees after their marital breakdown. We used propensity score weighting techniques, and, in a robustness check, we also used fixed-effects models combined with weighting approaches. Nevertheless, our results may still have been affected by time-varying heterogeneity, such as the worsening of the health of the divorcees over the divorce process.³⁴ It may also be the case that the men, who were more likely than the women to be the non-resident parent, reduced their engagement in the labour market in order to lower their child maintenance payments. Conversely, the divorced men may have become more engaged in child care responsibilities after their divorce than they were before, which could have inhibited their career advancement. Although shared physical custody is rare in Germany, we cannot rule out the possibility that this mechanism was also at play. Beyond health, maintenance payments, and physical custody, we were not able to control for patterns of re-partnering, and thus for whether the divorcees entered a new earner model. Another

³³ In male breadwinner constellation, men earn roughly 1.29 pension points two years before their divorce. Their earnings decline by approximately 0.06 until two years after their divorce, which corresponds to a drop of 5%.

³⁴ We account for whether a person has been part of a rehabilitation measure (see list of covariates in appendix). However, this variable only accounts for severe and mostly chronic diseases.

important limitation of our study was that we had no information on the number of children, and were therefore unable to investigate whether children moderated the relationship between divorce and earnings. In addition, having access to more refined information on the divorcees' occupational status and working hours would have been desirable, as such data would have helped us better understand why the divorced women in our sample had earnings that were so far below average.

Despite these many limitations, our investigations provide clear and alarming evidence of the gender differences in earnings after divorce. By examining previously married couples, we provided evidence of the diverging fates of women and men in male breadwinner constellations. Although the women in male breadwinner constellations expanded their employment after their divorce, they did not come close to earning as much as the men. As resources were no longer pooled after divorce and ex-spousal maintenance was only rarely granted for the divorcees in our sample, the secondary earners in prior breadwinner constellations often experienced a massive decline in living standards and economic well-being after divorce. The maintenance reform that was enacted in Germany in 2008 was motivated by the belief that women would be able to achieve economic independence after divorce. Given our finding that recent female divorcees were earning around 50-60% less than average gross earnings (see Table 3-1), it is clear that the majority of female divorcees in Germany are nowhere close to attaining economic independence.

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APPENDIX

Appendix I: Matching and weighting framework

Here, we address the main assumptions and requirements in the matching and weighting framework to produce unbiased estimates of the causal effect.

Conditional independence assumption (CIA):

The CIA addresses the existence of unobserved covariates that simultaneously affect the treatment assignment and the outcome. In an observational study, we can control for observed covariates, but there may be unobserved factors that select people into treatment. The CIA holds only if all unobserved covariates have no impact on earnings or the selection into treatment. It is only then that the estimated treatment effect equals the true unbiased effect. We could not rule out the possibility that there were unobserved covariates that biased our estimation. We addressed this problem with a sensitivity analysis in which we included additional ‘artificial’ covariates, and evaluated their impact on the treatment decision and the outcome (Appendix III: Sensitivity analysis)

Stable unit treatment value assumption (SUTVA):

The SUTVA addresses the possibility that the treated individuals will compete on the labour market with persons from the control group. The divorcees might have strong incentives to enter the labour market or to increase their working hours (if they are already employed) due to the financial burden of divorce. As a consequence, employers might substitute individuals from the control group with divorcees who are willing to accept lower wages or worse employment conditions. As a result, our estimated treatment effect might be distorted because the outcome of the controls serves as the counterfactual outcome of the divorcees. While this scenario is in principle possible, we believe that this effect would not be large, given that only around 200,000 divorces occur in Germany each year. The number of divorcees seems too low for their behaviour to affect the labour market options of married people. Thus, we assume the SUTVA holds.

Common support requirement:

The common support is a requirement in the matching framework. However, it is less clear whether it is relevant in the weighting framework. Ridgeway *et al.* (2021: 9), for example, argued that excellent covariate balance can be achieved, even when the propensity scores estimated for the treated and the control group overlap only a little. In our sample, the

distribution of the propensity scores do not overlap perfectly, but they overlap to a large extent. Thus, in light of the argument by Ridgeway *et al.* (2021) and the fairly good overlap in our sample, we decided against trimming, and estimated the treatment effect without the common support requirement.

Balance results:

The balance results are important for assessing whether weighting removed systematic differences between the treated group and the control group. Our balance statistics are shown in Tables A. 3-1 and A. 3-2. Before weighting (Table A. 3-1), the groups have statistically significant differences in many variables. Often, the mean between the treated group and the control group in a single variable deviates by more than 25% of a standard deviation (column SD max for unweighted rows). This is clearly inconsistent with what we would expect if the groups had been formed by random assignment. However, after weighting, the imbalance in a single variable is greatly reduced and only some minor outliers exceed 10% of a standard deviation (see Table A. 3-1 (column SD max and SD mean) and A2 (columns SD)). Thus, we believe that the balance results are sufficiently good for our estimation strategy, and that we are sufficiently close to what we would expect from random assignment. Note that we also provide the Kolmogorov-Smirnoff statistic in Table A. 3-1 to highlight that it is not only the mean values that are comparable between the treated group and the control group after weighting, but also the distributions.

Table A. 3-1: Balance statistics for subsamples before and after weighting

		N treat	N control	ESS treat	ESS control	SD max	SD mean	KS max	KS mean
both low earnings	unweighted	4560	2546	4560	2546	0.242	0.027	0.094	0.005
	weighted				1977	0.239	0.013	0.076	0.004
male breadwinner	unweighted	17897	13541	17897	13541	0.301	0.017	0.093	0.004
	weighted				10749	0.101	0.007	0.031	0.002
dual-earner	unweighted	7531	4619	7531	4619	0.255	0.021	0.088	0.003
	weighted				3973	0.091	0.010	0.035	0.002
female breadwinner	unweighted	1759	845	1759	845	0.268	0.041	0.124	0.006
	weighted				580	0.156	0.022	0.079	0.005

Note: ESS: effective sample size; SD: standardized difference; KS: Kolmogorov-Smirnoff statistic.

Table A. 3-2: Balance statistics of selected baseline covariates after weighting from the TWANG framework (interaction terms and 357 region dummies are not displayed)

	both low earnings		male breadwinner		dual-earner		female breadwinner	
	treated	SD	treated	SD	treated	SD	treated	SD
age male	42.43	0.02	42.87	-0.01	44.83	0.04	44.76	0.04
age female	39.38	0.00	40.29	-0.02	42.58	0.01	42.39	0.07
marriage duration	14.68	0.03	15.17	0.01	16.66	0.06	16.31	0.10
average pension deduction by divorce (ep)	-0.08	-0.03	-0.34	0.04	-0.23	-0.07	-0.04	-0.16
average pension surcharge by divorce (ep)	0.13	0.17	0.01	0.08	0.03	0.06	0.24	-0.07
dummy for giving birth in 2009	0.09	-0.02	0.06	0.01	0.01	-0.06	0.02	0.01
dummy for giving birth in 2010	0.06	-0.01	0.04	0.00	0.00	-0.03	0.00	-0.09
dummy for giving birth in 2011	0.03	0.02	0.02	-0.01	0.01	-0.02	0.01	-0.11
labour market outcomes at 31.12.2009								
female regular employed	0.19	-0.01	0.22	-0.02	0.94	-0.05	0.93	0.01
female self employed	0.03	-0.03	0.02	0.00	0.00	0.02	0.01	0.03
female unemployed	0.46	0.01	0.07	0.04	0.01	0.03	0.04	0.05
female mini-job	0.04	-0.04	0.03	0.01	0.00	0.01	0.00	0.04
female work disability	0.01	-0.02	0.01	0.04	0.01	0.02	0.01	-0.05
female marginal employment	0.25	-0.01	0.36	-0.01	0.11	0.06	0.11	0.02
male regular employed	0.19	0.03	0.95	-0.04	0.96	-0.03	0.19	-0.04
male self employed	0.14	-0.03	0.00	0.01	0.00	0.01	0.12	0.03
male unemployed	0.23	0.02	0.02	0.02	0.01	0.00	0.14	0.07
male mini-job	0.01	-0.05	0.00	0.00	0.00	0.01	0.02	0.06
male work disability	0.04	0.04	0.01	0.02	0.01	-0.02	0.05	0.02
male marginal employment	0.11	0.03	0.08	0.01	0.07	0.02	0.14	-0.05
labour market outcomes in 2010								
female income regular employment (ep)	0.09	0.00	0.10	-0.01	0.84	0.02	0.90	0.00
female income other sources (ep)	0.03	-0.04	0.05	-0.02	0.01	0.06	0.01	0.01
female days of work disability	3.27	0.02	3.28	0.06	2.63	0.03	2.93	-0.05
female days unemployed	41.37	0.00	10.89	0.03	1.51	0.03	2.38	0.01
female days regular employed	90.13	-0.01	95.19	-0.01	354.57	-0.05	354.29	0.01
female days of other employments	95.52	-0.04	136.57	-0.02	34.83	0.06	37.39	0.03
female days marginal employed	91.61	-0.04	132.40	-0.02	34.62	0.06	37.20	0.03
female days compulsory contribution for children	5.81	0.00	7.76	-0.02	2.12	-0.03	1.33	-0.02
male income regular employment (ep)	0.11	0.06	1.29	-0.03	1.38	-0.01	0.10	0.02
male income other sources (ep)	0.04	-0.04	0.01	0.01	0.01	0.02	0.04	-0.01
male days of work disability	11.92	0.05	3.24	0.04	3.82	0.02	12.89	0.06
male days unemployed	36.33	0.03	4.05	0.03	2.98	0.01	29.90	-0.01
male days regular employed	64.44	0.04	353.66	-0.04	354.91	-0.03	64.03	0.04
male days of other employments	59.87	-0.03	30.83	0.01	24.76	0.02	73.72	-0.07
male days marginal employed	40.92	0.00	30.55	0.01	24.50	0.02	53.22	-0.09
male days compulsory contribution for children	0.29	0.01	0.03	0.00	0.07	-0.01	0.60	-0.12
labour market outcomes in 2011								
female income regular employment (ep)	0.12	0.05	0.14	0.03	0.87	0.05	0.92	0.02
female income other sources (ep)	0.03	-0.05	0.04	-0.03	0.01	0.05	0.01	0.01
female days of work disability	4.04	0.08	3.33	0.05	4.23	0.09	4.81	0.05
female days unemployed	100.24	0.05	17.20	0.10	1.47	0.02	2.32	0.06
female days regular employed	110.64	0.01	122.66	0.02	356.85	-0.04	355.91	-0.01
female days of other employments	96.77	-0.05	133.55	-0.03	34.41	0.06	39.09	0.02
female days marginal employed	92.67	-0.04	128.97	-0.03	34.27	0.06	39.02	0.02
female days compulsory contribution for children	28.16	0.24	24.45	0.10	4.29	0.09	7.95	0.14
male income regular employment (ep)	0.13	0.02	1.29	-0.04	1.38	-0.03	0.13	0.08
male income other sources (ep)	0.04	-0.04	0.01	0.01	0.01	0.02	0.04	-0.04
male days of work disability	9.78	0.02	5.02	0.05	5.15	0.05	12.29	0.07
male days unemployed	93.76	0.03	3.50	0.04	2.31	0.02	29.74	0.11
male days regular employed	78.65	0.02	353.94	-0.06	355.06	-0.05	82.12	0.09
male days of other employments	58.71	-0.05	32.08	0.00	24.82	0.01	74.49	-0.09
male days marginal employed	40.39	-0.03	31.81	0.00	24.49	0.01	53.71	-0.07
male days compulsory contribution for children	0.40	0.02	0.03	0.01	0.10	0.00	1.28	-0.03

education

female no information	0.34	0.01	0.24	-0.04	0.19	-0.01	0.23	0.02
female without school diploma	0.02	0.02	0.01	0.01	0.00	-0.01	0.01	-0.11
female elementary school diploma	0.24	0.04	0.24	0.04	0.14	-0.02	0.14	-0.01
female secondary school diploma	0.28	-0.03	0.37	0.00	0.41	0.01	0.40	0.09
female high school diploma	0.13	-0.03	0.15	0.00	0.26	0.01	0.24	-0.10
male no information	0.41	0.01	0.21	0.00	0.20	-0.01	0.39	-0.13
male without school diploma	0.02	-0.01	0.01	0.00	0.01	0.03	0.01	-0.01
male elementary school diploma	0.27	0.00	0.32	0.00	0.27	0.03	0.23	0.05
male secondary school diploma	0.18	0.03	0.28	0.03	0.28	0.01	0.20	0.07
male high school diploma	0.11	-0.05	0.18	-0.03	0.24	-0.03	0.17	0.05

higher education

female no information	0.31	-0.01	0.20	-0.01	0.07	0.01	0.08	-0.06
female without vocational training	0.13	0.06	0.08	0.02	0.05	0.01	0.06	0.01
female vocational training	0.51	-0.04	0.65	0.00	0.76	0.01	0.75	0.10
female bachelor diploma	0.01	-0.01	0.01	0.00	0.01	-0.02	0.01	0.02
female master diploma	0.04	0.00	0.04	0.00	0.08	-0.02	0.08	-0.08
female master/technician college qualification	0.02	0.02	0.02	-0.01	0.04	0.00	0.03	-0.04
female doctorate	0.00	0.01	0.00	0.00	0.00	-0.04	0.00	0.00
male no information	0.39	0.00	0.10	0.01	0.09	-0.02	0.37	-0.12
male without vocational training	0.09	0.02	0.05	0.00	0.04	0.02	0.06	-0.01
male vocational training	0.42	0.01	0.65	0.02	0.63	0.02	0.44	0.08
male bachelor diploma	0.01	-0.03	0.01	-0.02	0.01	-0.03	0.01	0.01
male master diploma	0.04	-0.04	0.09	-0.01	0.12	-0.01	0.06	0.01
male master/technician college qualification	0.05	-0.03	0.09	-0.02	0.11	0.00	0.07	0.07
male doctorate	0.01	0.03	0.01	0.01	0.01	-0.05	0.01	0.04

Note: Income is measured in earning points (ep). Mini-jobs are jobs with monthly income thresholds between €450 and €850. SD is the standardised difference between the averages of the treated and control group. The ‘treated’ columns display the average of the respective variable.

Appendix II: Robustness check

Dummy impact fixed-effects approach

While our results in Figure 3-3 are derived from simple matching based on the TWANG framework, here we add two different estimation frameworks. As an additional robustness test, we re-estimated the treatment effect by a dummy impact fixed-effects estimation (see Table A. 3-3). Here, we only present the results for the male sample for whom we observed a large drop in earnings after divorce. While in the matching framework, the counterfactual outcome of the treated is derived from a control group of individuals who have not undergone the treatment, in the fixed-effects framework, the control group consists of the treated individuals themselves. In addition, while matching relies on a rich set of covariates to erase systematic differences between the treated and the control group, it is still prone to the influence of unobservable covariates. In the fixed-effects framework, at least time-constant unobservable covariates are accounted for. Compared to the results derived from matching (Figure 3-3), the fixed-effects results are similar in magnitude.

Table A. 3-3: Regression results. Outcome variable: men's earning points. Method: dummy impact fixed-effects regression

	both low earnings	male breadwinner	dual-earner	female breadwinner
Reference: income average of year 2010 and 2011				
Year t_{-1} (2012)	-0.013	-0.011 ***	-0.011 **	0.027 *
Year t_0 (2013)	-0.026 **	-0.036 ***	-0.038 ***	0.043 **
Year t_{+1} (2014)	-0.045 ***	-0.048 ***	-0.051 ***	0.032
Year t_{+2} (2015)	-0.050 ***	-0.054 ***	-0.060 ***	0.032

Note: Age and period dummies are included in all regressions, as well as measures for work disability (men and women) and income, unemployment, marginal employment, compulsory contributions for children, and births for women (see Table A. 3-2 for a subset of those variables in the years 2009, 2010, and 2011).

Matching combined with fixed-effects approach

Fixed-effects estimations are also prone to biased (overestimated) results if earnings dynamics prior to treatment differed between divorcees and married persons (Brüderl & Ludwig, 2015). In other words, due to diverging earnings trends, the assumption of parallel outcome trends is violated, and the estimation suffers from self-selection into treatment; i.e., those with lower or

negative earnings dynamics have higher divorce risks. For example, it may be assumed that the men who will separate are on a lower earning trajectory than the married men. Poor health may be time-varying trait that severely distorts the analysis, as this variable is correlated with both separation and unemployment and earnings. In order to overcome this problem, combining fixed-effects estimations with propensity score weighting has been suggested (Arkhangelsky and Imbens 2019; Jones & Lewis 2015). In this set up, weighting techniques are used to re-weight the control group such that their observable characteristics mimic the features of the treated group prior to treatment. In this logic, ‘pre-processing’ provides a remedy for the (very likely) violation of the parallel trend assumption.

Table A. 3-4 displays the results from a combination of fixed effects with a weighted control group (the weights (here, odd weights) were again derived from the TWANG framework). The estimates are a bit more conservative here than in the prior investigation. For example, the model suggests a drop in earnings points of 0.047 for men in male breadwinner constellations two years after separation. While the previous analysis had suggested a drop of 0.054, the simple matching approach in Figure 3-3 suggested a drop by 0.059. The overall pattern, however, is similar across the estimation techniques.

Table A. 3-4: Regression results. Outcome variable: men’s earning points. Method: weighted dummy impact fixed-effects regression with odd weights derived from the propensity scores

	both low earnings	male breadwinner	dual-earner	female breadwinner
Reference: income average of year 2010 and 2011				
Year t_{-1} (2012)	-0.009	-0.009 **	-0.010 *	0.023
Year t_0 (2013)	-0.015	-0.032 ***	-0.036 ***	0.041 *
Year t_{+1} (2014)	-0.030 **	-0.042 ***	-0.049 ***	0.029
Year t_{+2} (2015)	-0.038 **	-0.047 ***	-0.058 ***	0.025

Note: Age and period dummies are included in all regressions as well as measures for work disability (men and women) and income, unemployment, marginal employment, compulsory contributions for children, and births for women (see Table A. 3-2 for a subset of those variables in the years 2009, 2010, and 2011).

Appendix III: Sensitivity analysis

As we noted earlier, the matching and weighting methods are not robust to the impact of unobserved variables that simultaneously influence the treatment decision and the outcome. Sensitivity analyses can be used to assess how strongly an unobserved covariate must be correlated with the treatment assignment and the outcome in order to undermine the estimated treatment effect. To assess the impact of those unobserved covariates, we use OVtool from the R library (Pane *et al.* 2021).

In Figure A. 3-1, we simulate the impact of unobserved factors at the treatment effect of divorce. The outcome is males pension points earned in 2015 in comparison to $\frac{1}{2}*(t_3+t_2)$, and in comparison to the control group (i.e. the difference-in-difference estimate). The average income change is 0.059 points (p-value > 0.01) lower in the treated group than in the control sample (see Figure 3-3 upper left panel).

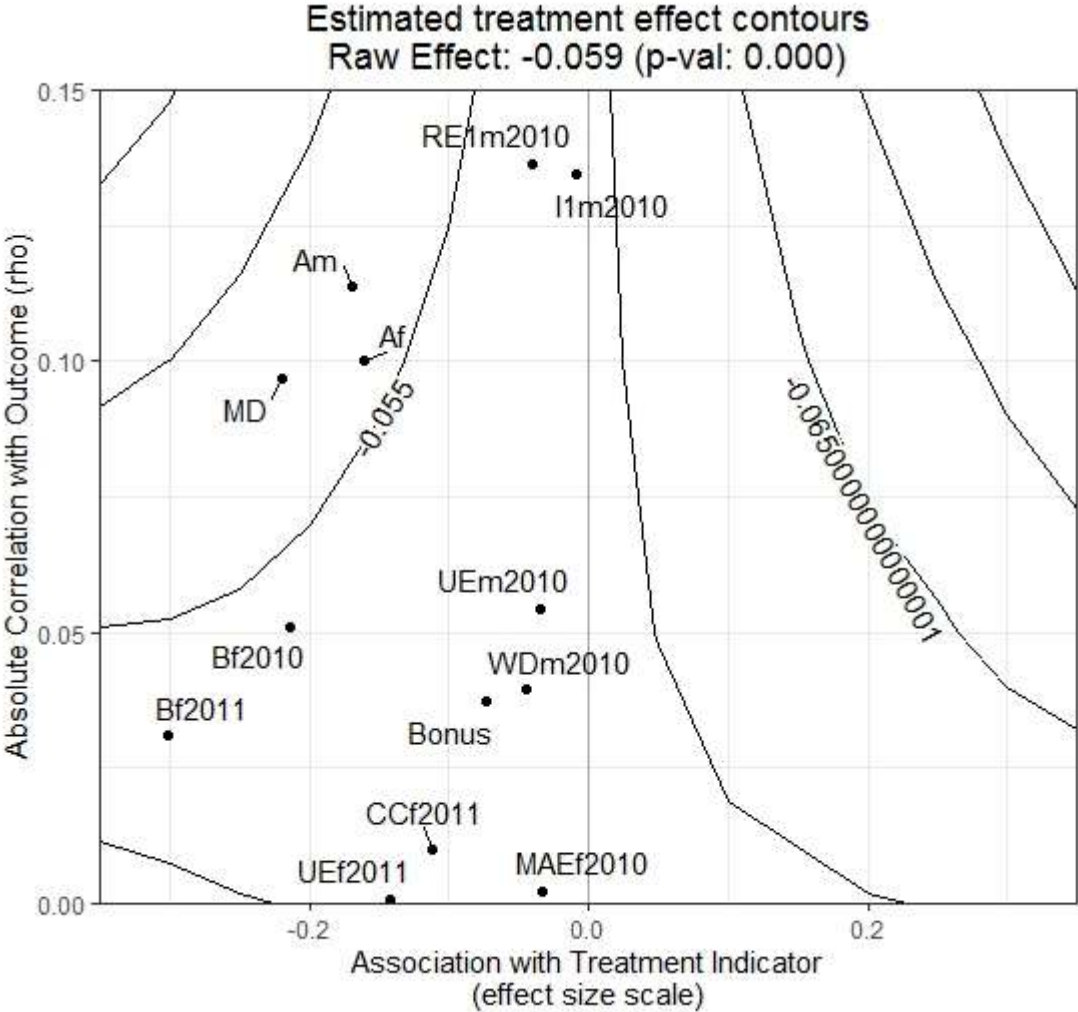
The y-axis in Figure A. 3-1 represents the unobserved confounder's absolute correlation with the outcome and the x-axis is the association between the unobserved confounder and the treatment indicator. The effect size scale is defined as the standardized mean difference between the treated and the control group. Rules of thumb for (absolute) effect sizes suggest that effect sizes greater than 0.2 would be considered small, 0.5 would be moderate and 0.8 would be large (Cohen, J., 1988). The solid black lines represent the impact of various scenarios of unobserved confounders on the treatment effect.

For example, marriage duration (MD) and the outcome have an absolute correlation of 0.09 and the mean values of MD in the treated group differs by (absolute) 22% of a standard deviation to the mean value of the control group (before weighting). If an unobserved confounder had a similar relationship, then we would observe that the treatment effect shifts down to -0.053. Note, that the result under the impact of that unobserved confounder would also be closer to the results derived from the fixed effect estimation (-0.054) and the weighted fixed effect estimation (-0.047).

However, how plausible is it that such an unobservable covariate exists? In Figure A. 3-1, we present a selection of 13 (chosen because of their relative strength in the weighting procedure) variables from the weighting algorithm. If unobserved factors would behave like those observed covariates, then unobserved covariates would not change the treatment effect in a meaningful way. At most, results move closer to the alternative estimation strategies from the fixed effect framework.

We therefore conclude that the sign of the estimated effect is expected to remain consistent when simulated unobserved confounders have the same strength of association with the treatment indicator and outcome that are seen in the observed confounders. In the most extreme observed case, the estimated treatment effect shifts from -0.059 to -0.053. Furthermore, statistical significance at the 0.01 level is expected to be robust to unobserved confounders. In the most extreme observed case, the p-value remains well below 0.01.

Figure A. 3-1: Impact of unobserved covariates on the treatment effect of divorce on pension points derived from regular employment for males in 2015



Note: Sensitivity analysis on selected covariates: marriage duration (MD), age male (Am), age female (Af), giving birth in 2010 (Bf2010), giving birth in 2011 (Bf2011), days of unemployment female in 2011 (UEf2011), days of unemployment male in 2010 (UEm2010), days of marginal employment female in 2010 (MAEf2010), received pension points due to pension splitting in divorce process (Bonus), days of regular employment male in 2010 (RE1m2010), income male derived from regular employment in 2010 (I1m2010), days of work disability male in 2010 (WDM2010), days with compulsory contribution for children in 2011 (CCf2011).

Chapter 4

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4 Work Disability and Divorce

4.1 Introduction

Research has suggested that married people enjoy better mental and physical health than their divorced peers (Williams *et al.* 2008). The explanations for this pattern refer to two different (but not necessarily mutually exclusive) modes of action between the state of health and the event of divorce. The social causation argument posits that changes associated with divorce or separation have a negative impact on health (Wade and Pevalin 2004). According to this logic, experiences typically associated with divorce – such as having to adapt to the loss of the spouse, a deterioration in living standards, the disruption of social networks, the loss of social support, and having to bear the double burden of single parenting and employment – are detrimental to health and/or are promote unhealthy behaviours. The literature has also posed the question of whether these effects are of a short-term nature (i.e., individuals become accustomed to their new conditions) or are longer lasting (Couch *et al.* 2015; Tamborini *et al.* 2016). The social selection argument states that with declining health, the quality of a couple’s marriage decreases and their risk of divorce increases (Goldman 1993; Wade and Pevalin 2004). According to this logic, the effects of a divorce should be minor, and the relatively poor health observed among people who are divorced is a consequence of selection.

In this chapter, I use register data from the statutory German pension fund to examine the health consequences of divorce in West Germany. The outcome of interest is the uptake of work disability, which is defined as sick leave starting after six weeks of illness. Work disability is an important measure because at an individual level, taking work disability limits the scope of an individual’s labour market participation, and reduces his/her income. Taking extended periods of sick leave might also significantly reduce a worker’s retirement income, and lead to social isolation, depression, and low self-esteem. At the macro level, work disability claims create public costs in the form of sick pay, medical expenses, rehabilitation costs, lost working days, and reduced productivity. The aim of this study is twofold. First, I provide easily accessible statistics that illustrate how the likelihood of taking work disability leave changes around divorce. Second, I examine the question of whether individuals’ health status after divorce is partially related to selection into divorce. As a method, I employ the nearest neighbor matching approach, which allows me to generate a comparable control group for the divorced population. I have restricted the analysis to West German men and women who separated between 2000 and 2010. I analyse men and women separately. For women, I differentiate

between mothers and childless women, as I assume that divorce weighs more heavily on the health status of mothers than of childless women.

4.2 Theoretical considerations

4.2.1 Benefits of marriage

Apart from the reasonable assumption that healthier and wealthier individuals are privileged in the partner market – i.e., that those individuals might be more likely to select into marriage – marriage is found to improve health (Lillard and Panis 1996) and material well-being (Wilmoth and Koso 2002). Material well-being increases because married couples benefit from economies of scale by sharing housing, food, and utilities. Sharing resources minimises the partners' cost of living and provides them with insurance against unexpected events, like unemployment or illness (Wilmoth and Koso 2002). Moreover, in some countries, including in Germany, marriage can provide institutional support that is not available to non-married individuals, like free health insurance for spouses or tax benefits. Thus, on average, married couples have lower poverty rates and more assets than their unmarried counterparts. These effects of marriage are usually assumed to reduce stress and to increase security, which may, in turn, have positive effects on health. Additionally, marriage provides a healthy social environment that inhibits individuals from engaging in self-destructive acts; i.e., a married person is more likely than a single person to have someone in his/her life who regulates his/her behaviour, either by imposing sanctions or by causing the person to internalise norms that encourage conventional behaviour (Umberson 1987). For these reasons, marriage has been found to be one of the most important categories of social ties that help to buffer people from the effects of negative life events (Umberson 1987).

Although marriage offers a range of socio-emotional and economic resources that can enhance the partners' health and well-being, the benefits of marriage for a given individual depend on the person's gender, socio-demographic characteristics, and relationship characteristics. Compared to men, women seem to gain more from marriage in material terms, and but are less likely to rely exclusively on their partner for emotional support (Williams *et al.* 2008). In contrast, compared to women, men are less likely to benefit from marriage economically, but are more likely to rely on their partner for emotional support and social integration (Gerstel *et al.* 1985). The benefits of marriage also depend on the duration and the timing of the marriage. There is evidence that marriage duration is correlated with longevity. However, it has also been shown that the benefits of marriage are diminished if the partners form the union while very

young, because early marriage tends to be associated with reduced financial resources and greater marital distress (Dupre *et al.* 2009). In particular, marital distress has been found to counteract the protective effects of marriage, as conflict-ridden marriages are associated with emotional loneliness, drinking, and depression (Waite 1995; Dykstra and Fokkema 2007; Umberson *et al.* 2006).

To sum up, marriage seems to provide the partners with financial resources and social support that promotes health by either reducing their economic uncertainty or prompting them to adopt a healthier lifestyle. Conversely, men and women who are experiencing marital disruption tend to have substantially higher stress levels, worse subjective well-being, a higher risk of drinking, and lower body weight (Waite 1995; Umberson 1992). Research has also shown that divorcees have an elevated risk of psychiatric illness, suicide, motor vehicle accidents, homicide, physical illness, and misuse of various substances; and tend to report higher levels of depression, anxiety, and unhappiness (Booth and Amato 1991). In general, it seems that compared to married people, divorcees are more likely to engage in negative health behaviours, and are less likely to have an orderly lifestyle. These unpleasant outcomes are addressed in the “divorce-health” literature, and are condensed in the social causation framework.

4.2.2 Health consequences of divorce

The “divorce-health” literature has shown that separation and divorce are stressful events with adverse effects on health. There are many reasons why divorce has a negative impact on health, but among those that are mentioned most frequently are that divorcees often experience a deterioration in living standards, a change in residence, the disruption of their social networks, the loss of social support, and the pressure to take on the double burden of single parenting and employment. The stress associated with these changes and with the loss of a partner seem to promote unhealthy behaviours, which, in turn, increases the risk of poor health and mortality (Zhang and Hayward 2006). Divorcees are especially likely to report symptoms of poor mental health, in part because a divorce can lead to the loss of supportive social networks, and force them to reorganise their network outside of their marriage. Moreover, the networks people build after a divorce are often not of the same quality as the networks they had while married. It has, for example, been shown that divorcees’ new networks are often burdensome, and may undermine their health, rather than supporting it (Gerstel *et al.* 1985). There is, however, evidence that women are better than men at developing new networks and maintaining their

ties (Gerstel *et al.* 1985). The differences in the network structures of men and women might also be responsible for their different health outcomes after divorce. To the extent that network size and quality correlates with loneliness, the greater decline in social support found among men than among women could mean that for men, divorce has especially negative effects on their levels of social control and lifestyle choices. Shor *et al.* (2012) suggested that the risk of death is higher for divorced men than for divorced women because men are more likely to experience a substantial decline in social support. Similarly, Umberson (1987) found that men suffer more than women from the loss of social control, which may cause them to develop drinking problems. By contrast, compared to their male counterparts, divorced women are more prone to experiencing financial strain, and having a lower household income coupled with increased parental responsibilities increases the likelihood of having poor mental health. Although men also frequently have a lower household income after a divorce, men's income losses tend to be smaller than those of women (Andreß and Bröckel 2007; see also Mortelmans in this volume). Research on the impact of the time that has elapsed since the union dissolution on the well-being of divorcees has shown that the negative consequences of divorce are most pronounced around the time of the event itself, and then usually attenuate and lose their effect. It has, for example, been found that getting divorced more than doubles mortality for men (133%) and women (132%) in the first two years after the divorce, but that this effect peters out in later years (Brockman and Klein 2004). Having been recently divorced has also been shown to be associated with lower life satisfaction for men and women. It appears, however, that this effect is stronger for men than for women, as women tend to have smaller reductions in life satisfaction, and generally return to their baseline values more quickly (Leopold and Kalmijn 2016). However, while some of the negative consequences of divorce seem to be short-lived or to diminish over time, there is also evidence that divorce can have long-term consequences. Divorce has been linked to an increased cumulative probability of taking work disability leave and of receiving disability benefits for many years after the divorce (Couch *et al.* 2015; Tamborini *et al.* 2016). These results strengthen the view that life-changing events can lead to cumulative health strains that emerge slowly.

4.2.3 Selection into divorce

While the “divorce-health” literature has highlighted the stressful nature of divorce, the “health-divorce” literature has pointed out that poor health, psychological problems, and financial hardship increase the risk of divorce (Fu and Goldman 2000; Wade and Pevalin 2004). Hence,

the often-observed poor health condition of divorcees is not necessarily attributable to the event itself, but may instead be a result of selection. If the decline in a spouse's health leads to constraints in his/her everyday functioning, the healthy spouse might have to take over more of the cleaning, cooking, maintenance, and childcare (Booth and Johnson 1994). The change in the division of household tasks may be a source of marital unhappiness. The persisting poor health of one of the partners might also lead to a reduction in the couple's shared activities, changes the set of assumptions the marriage was based on, and a reduction in family income that increases financial stress (Teachman 2010). These shifts might, in turn, lead to a renegotiation of marital tasks, a reduction in the benefits of marriage for the healthy spouse, and an increased risk of divorce (*ibid.*).

The assumption that one of the spouses being in poor health worsens the quality of the marriage may be overly pessimistic. The poor health of one of the spouses might also be perceived as a common experience with the power to strengthen the couple's existing bonds. Syse and Kravdal (2007), for example, have found that a spouse having an illness like cancer does not necessarily increase the risk of divorce, and may even reduce it. However, this result might be driven by the normative pressure not to leave a seriously ill partner, or by the rationale that leaving a seriously ill spouse might not make sense if death is anticipated (Syse and Kravdal 2007).

To the extent that social selection precedes separation, any measured health consequence after divorce cannot be linked directly to divorce, because divorcees are then a selected group in especially poor or especially good health. The "health-divorce" literature has provided support for the selection argument, with one study showing that some of the excess mortality and health problems observed among divorcees result from a health-related selection process out of marriage (Fu and Goldman 2000). Another study found that work-related health limitations are associated with marital instability rather than the reverse, but this result referred only to the health of the husband, and not to the health of the wife (Teachman 2010). These results are in line with the findings of Yorgason *et al.* (2008): i.e., that when a wife's health declines, the husband is more likely to report a decline in marital happiness; but that when the husband's health declines, the wife is more likely to report not only a decline in happiness, but increases in disagreement levels, marital problems, and divorce proneness (Yorgason *et al.* 2008).

To sum up, the "divorce-health" literature provides evidence that divorce has an impact on health, and the "health-divorce" literature provides evidence of a selection into divorce due to poor health. Both frameworks are important, and need to be addressed in the empirical investigation.

4.3 Data and analytical sample

In the present study, I used linked data from the statutory German pension system. I linked the records of the Sample of Active Pension Accounts (VSKT) with the records of the Pension Rights Adjustments Statistic (VA). The VSKT is a random sample of individuals with a pension account. It provides detailed pension-relevant information, such as information on the individuals' employment and earnings history, spells of parental leave, and childbirths (Stegmann and Himmelreicher 2008). The VA contains the dates of separation and divorce (Keck and Mika 2016). The pension fund collects these data, because Germany has a system of "income splitting", whereby pension entitlements are split after divorce (for more details, see Keck *et al.* 2017). The great advantage of using these data is that they provide me with a reasonably large sample size. Unlike prospective survey data, register data do not suffer from attrition, which is especially likely to occur after a separation or a divorce. However, there are other caveats that I need to mention. One limitation of the data is that the register data do not include the full resident population, but cover only those who have a pension account. About 90% of the resident population are included in the data, but people in certain professions, such as civil servants and farmers, are not included (Kruse 2007). Furthermore, not all divorces are included in the data because the register data only contain information on divorces that result in pension splitting. Pension splitting is, in theory, mandatory, but certain couples – and particularly those with short marriages – can avoid pension splitting (Keck *et al.* forthcoming). Thus, the observed divorcees might not be a representative subpopulation of all divorcees in Germany.

The analytical sample consists of individuals who separated between 2000 and 2010. Separation (t_0) is defined as the year that the divorce file was opened; i.e., the year when the defendant received the divorce petition.³⁵ In the following, I use the term "divorce" to refer to the date the file was opened to make the text easier to read. Note, however, that a divorce may not be legally finalised until months or even years after the file was opened. I restricted the analysis to West Germans, and defined a West German as a person who has never worked in the Eastern states of Germany. East Germany was excluded, in part because the case numbers were low, especially for childless women; and in part because there are considerable differences between

³⁵ Instead of calendar year, I defined years by the exact time since divorce. If the divorce file was opened in, for example, April 2003, then t_0 spans the period 16 April 2002 to 15 April 2003; and t_{-1} is from 16 April 2001 to 15 April 2002; and so on.

the two parts of Germany in terms of female labour market participation and marriage and divorce patterns. I have furthermore limited the investigation to the time window of seven years before the separation up to four years after the separation. Thus, I followed individuals from t_{-7} to t_{+4} . I chose t_{-7} to address separation and the anticipation of separation, and to properly isolate prior health selection (see, for example, Johnson and Skinner (1986) for changes in labour market participation). The choice of t_{+4} was driven by constraints in the data availability for the most recent years. I organised the data as a person-year dataset. Thus, each individual contributes several years of data to the investigation. I furthermore restricted the sample to individuals who were divorced for the first time and who were married at the beginning of the observation period in t_{-7} . Thus, shorter marriages are not included in this investigation. Time is defined as the exact time since separation. The final sample includes 4467 men and 6192 women (see Table 4-1). The subsamples of mothers and childless women consist of 4826 and 729 women, respectively. The numbers do not sum up to 6192 because the mothers were women who already had children in t_{-7} , and the childless women were childless until t_{+4} . The women who gave birth between t_{-7} and t_{+4} account for the remaining difference.

4.4 Analytical approach

The aim of this study is to describe the work disability uptake pattern around separation by comparing the health status of divorcees to the health status of an appropriate control group. In order to design a control group, I relied on matching techniques. The use of matching techniques was necessary because the characteristics of the people who did not undergo a divorce differed sharply from the characteristics of the divorcees. This becomes clear when looking at Table 4-1, which compares the socio-demographic characteristics of the “raw sample” and the “matched sampled” (see the row “matched” and “raw”). The most important aim of matching is to exclude all of the individuals from the control group who are not similar to the individuals from the divorced group.

For our purposes, I relied on four nearest neighbour matching, with the common support restriction and a caliper of 0.02 (i.e., I chose only individuals from the comparison group whose propensity scores did not differ by more than ± 0.02). All of the individuals from the control group who were not a valid “neighbour” were deleted, and have not been included in my analysis. The lines marked “matched” in Table 4-1 show the mean values for the selected covariates after matching, and demonstrate that dropping the non-comparable resulted in a much more balanced control sample. Additionally, in Table A. 4-1 in the Appendix, I provide

further details of the matching procedure. These findings suggest that after matching, the two groups (divorced and control sample) were highly comparable. Obviously, I could only match on observable characteristics; which means that unobserved factors could still bias the investigation. Finally, as the people in the control group obviously did not have a date of divorce, I had to randomly assign them a date of divorce.

In the first step of the investigation, I display sample statistics at the start of the observation period (t_{-7}) and at the end of the observation period (t_{+4}). I also provide the mean values of the key dependent variables (the cumulated days of work disability and the yearly work disability rate) for these two time points. The second step of the investigation contains a pooled OLS-regression analysis. Here, I use the person-year data that was pooled over the entire observation period. I interact a dummy for the control group with the time variable (t_{-7} to t_{+4}) to illustrate how disability changes around divorce. All of these analyses are done separately for men and women. For women, I also conduct a separate analysis for mother and childless women.

4.5 Variables

Health is defined based on an individual's history of work disability. It is important to note that this term refers only to long-term disability, because the pension data only includes information on work disability if the individual or the employer was paying social security contributions to the pension system. During the first 42 days of illness, employees in Germany are entitled to sick pay benefits that cover their full income. After 42 days of illness, employees are entitled to receive a reduced sickness benefit that usually amounts to 70% of their former income, and that is recorded in the pension data. There are two other important shortcomings in the data that pertain to the outcome variable. The outcome variable may be biased upwards because sick pay for children is also included in the pension data, and is recorded from the first day of sickness. The uptake of sick pay for children is, however, very low in Germany. Analyses of health data have shown that the sick pay days for children account for less than 2% of all recorded sick pay days, and those days that are recorded are mainly granted for mothers (>85%) and very rarely for fathers (<15%) (Sondergutachten 2015). While children's sick days bias the absolute values upwards, the outcome variable does not include the health impairments of unemployed and non-working individuals, which biases the absolute days of sick leave downwards. This aspect has to be taken into account when I discuss the absolute values of sick leave. However, my interest is less in the absolute number of disability days taken. Instead, the analysis compares

the work disability days taken by divorcees and a control group. Thus, the difference is of greater interest than the absolute values (see below).

I use two outcome variables for this investigation:

- The main variable of interest is the **cumulated days of work disability**. This variable was constructed by cumulating the number of work disability days taken since age 15.
- The yearly **work disability rate**. It was calculated by the number of work disability days taken in the respective year divided by 365.

I used several socio-demographic variables in matching the control group. These variables are also employed later in the OLS regression. I controlled for German **citizenship**, distinguishing between German citizens and persons with foreign citizenship. I included **age** (and squared) in years to account for different health risks across the life course. I controlled for the **unemployment rate** in West Germany, because the uptake of work disability correlates with times of recessions and prosperity (Benítez-Silva *et al.* 2010). I also used **cumulated days in employment with social security contributions** (and squared), because employment is a protective factor against the economic risk of marriage dissolution, as well as a source of self-esteem and social support.³⁶ For a woman, being employed may increase her economic independence, thereby lowering her exit costs. Thus, a woman's employment could make it easier for her to dissolve an unsatisfactory, conflict-ridden marriage. Moreover, a woman's employment might increase her psychological independence and strengthen her belief that she is competent and capable of establishing an independent household (Kalmijn and Portman 2006). I also accounted for **cumulated earnings** (and squared). Earnings are measured in individual pension points. An individual earns one pension point if the yearly gross income equals the average gross income in West Germany of the respective year. I also added **cumulated days of vocational training** to the models as a proxy for education. The **month and the year the divorce file was opened** was included to control for seasonality. I controlled for the **number of children**, because the presence of children increases a family's economic needs and stress. I controlled for **cumulated days in parental leave** to account for how soon after childbirth the women returned to the labour market. This variable might reflect financial necessity or a desire to participate in the labour force. The latter two variables were only

³⁶ Cumulated covariates accumulate the outcome from age 15 up to the respective year.

available for the women, and are thus used only for the analysis of the women. For the men, time spent in military service was also included.

4.6 Descriptive findings

Table 4-1 gives an overview of the selected baseline covariates at the beginning of the observation period at t_7 . I display their mean values before (raw) and after matching (matched). I can see that the average age of the men in the matched sample was approximately 35.5 years at t_7 . The men had accumulated up to that date roughly 4100 days in employment with social security contributions. The days spent in military service are less relevant, and mainly refer to days spent in basic military service. The women were, on average, younger than the men, and had accumulated only half of the men's lifetime employment. The income (measured in earning points) of the average woman was roughly one-third of the income accumulated by the average man. This finding suggests that the women earned less and were less likely to be in full-time employment than the men. On average, the mothers were one year older and the childless women were one year younger than all of the women in the sample. The mothers and the childless women both accumulated roughly 2000 days in employment; thus, the labour market participation and income levels of childless women were higher. At t_7 , the mothers had, on average, at least one child over age six.

Table 4-1: Covariate mean values before and after four nearest neighbour matching at t_{-7}

Variable		Men		Women		Women with children		Women children	without
		divorced	control	divorced	control	divorced	control	divorced	control
Foreign citizenship	raw	0.30	0.58	0.32	0.52	0.29	0.39	0.40	0.62
	matched	0.30	0.30	0.32	0.32	0.29	0.30	0.40	0.40
Age	raw	35.3	30.4	32.6	30.3	33.7	37.9	31.2	27.8
	matched	35.3	35.5	32.6	33.0	33.7	33.8	31.2	31.4
Number children <3	raw			0.310	0.089	0.398	0.279		
	matched			0.310	0.298	0.398	0.392		
Number children 3-6	raw			0.280	0.083	0.359	0.261		
	matched			0.280	0.278	0.359	0.357		
Number children >6	raw			0.883	0.454	1.133	1.425		
	matched			0.883	0.921	1.133	1.144		
Parental leave	raw			199.9	43.2	256.4	135.1		
	matched			199.9	195.2	256.4	258.2		
Unemployment rate	raw	9.06	9.04	9.04	9.04	9.03	9.05	9.08	9.04
	matched	9.06	9.07	9.04	9.04	9.03	9.02	9.08	9.06
Employment	raw	4094	1627	1940	1416	2032	2731	2063	839
	matched	4094	4104	1940	2042	2032	2057	2063	2093
Earnings employment	raw	12.21	4.67	3.66	2.80	3.76	5.07	4.32	1.84
	matched	12.21	12.15	3.66	3.88	3.76	3.83	4.32	4.39
Military service	raw	132	63						
	matched	132	135						
Vocational training	raw	480	184	363	180	365	260	315	102
	matched	480	469	363	359	365	364	315	307
N	raw	4467	165621	6192	154269	4826	49187	729	80246
	matched	4467	15104	6192	19333	4826	14030	729	2757

Note: Further matching variables were: year and month the file was opened, age squared, employment, and earnings squared.

Table 4-2 provides summary statistics for the outcome variables for t_{-7} and t_{+4} . The upper panel of the table shows the cumulated days of work disability. Looking at the table, I first note that the number of cumulated work disability days was much lower for the women than for the men. It is, however, important to consider that the lifetime employment participation of the men was twice that of the women. On average, a divorced man had accumulated 32 work disability days at t_{-7} . Four years after the divorce, the value has increased to 79 days. In relative terms, this represented an increase of 146%. For the control group, I observe an increase of only 93%. Thus, the increase in the number of work disability days was 27% higher for the divorced men than for the control group. While similar increases are found for childless women, all of the women and the mothers had substantially smaller increases.

The lower panel displays the yearly work disability rate at t_{-7} and t_{+4} . Note that, in contrast to the cumulated outcome, the yearly focus might be more volatile and prone to outliers. Changing the base year, for example, from t_{-7} to t_{-6} might substantially alter the result. However, comparing t_{+4} with t_{-7} shows that the divorced men had a rate that was 5% higher than that of

the control group. The sample of all women had a rate that was 45% higher than that of the control group, and the mothers had the smallest increase.

Table 4-2: Average cumulated days of work disability (in days) and yearly work disability rate (in 100)

	Men		Women		Women with children		Women children	without
	divorced	control	divorced	control	divorced	control	divorced	control
Cumulated work disability at t ₇	32.2	30.3	17.0	13.8	18.0	14.1	18.0	13.3
Cumulated work disability at t ₊₄	79.1	58.6	36.7	26.7	37.8	28.0	40.8	23.3
Ratio (t ₊₄) / (t ₇)	2.46	1.93	2.16	1.93	2.11	1.98	2.26	1.76
Ratio divorced / control	1.27		1.12		1.06		1.29	
Yearly work disability rate at t ₇	0.0096	0.0069	0.0043	0.0039	0.0043	0.0036	0.0052	0.0023
Yearly work disability rate at t ₊₄	0.0114	0.0078	0.0066	0.0041	0.0065	0.0052	0.0094	0.0029
Ratio (t ₊₄) / (t ₇)	1.19	1.13	1.53	1.06	1.51	1.44	1.81	1.28
Ratio divorced / control	1.05		1.45		1.04		1.42	

Note: Presented are the sample mean values for the cumulated work disability days and the yearly work disability rate. For each individual, the cumulated work disability days are measured since age 15 up to the respective year. T₀ represents the opening of the divorce file. Each individual's yearly work disability rate is measured by the number of work disability days in the respective year divided by 365.

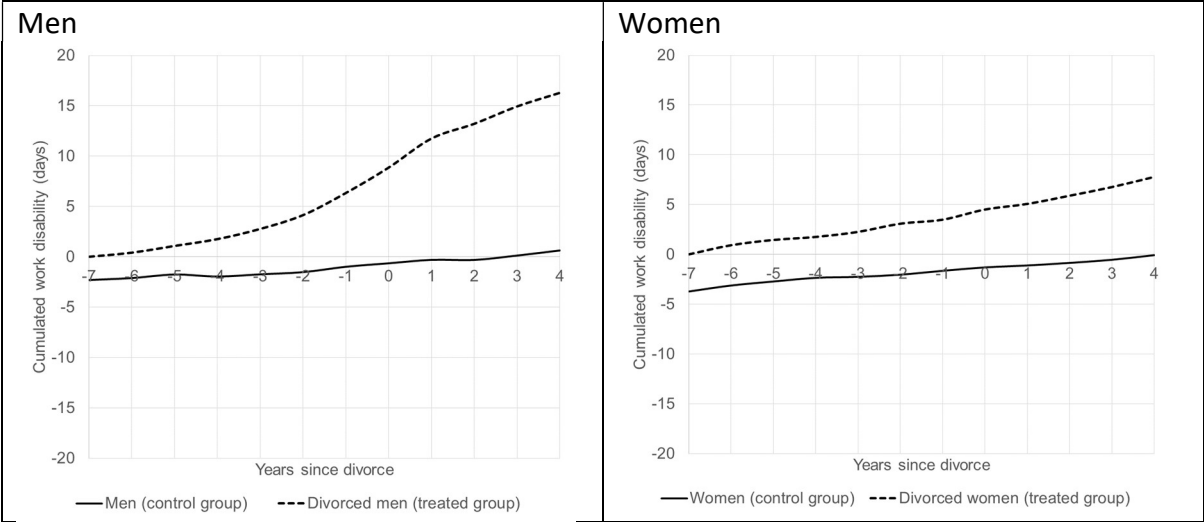
4.7 Regression results

4.7.1 Cumulated work disability

The results from the pooled OLS regression on the matched sample are displayed in Table A. 4-2 in the Appendix. I do not discuss the effect of the control variables, but instead focus on the effect of the time since separation, which is displayed in a graph. The aim of using the pooled OLS regression is simply to standardise for the covariates applied and to retrieve the net effect; i.e., the net, for example, of ageing, childbirth (women only), and labour market participation. I start with the pattern for the cumulated receipt of work disability benefits. Figure 4-1 displays the pattern for the male and female sample. The slope of the figure for the control group reflects the general trend. As I can see, already at t₇ the health of the men and women from the divorced population was worse than that of the control group. At t₇, the difference in all of the accumulated work disability days since age of 15 was 2.3 days for men and 3.7 days for women. These findings strongly support the selection argument, and highlight the importance of controlling for health selection *before* separation. However, I also note that the difference at t₇ was statistically significant (p<0.01) for women, but not for men (see Table A. 4-3, Appendix). Wald tests for the equality of two coefficients show that the control/divorce and time interaction coefficients displayed in Figure 4-1 were statistically equal for men until t₃, but differed thereafter (t₂ p<0.05; t₁ to t₊₄ p<0.01). For women, the coefficients were statistically different for all time points (p<0.01) (Table A. 4-3, Appendix). Given the change between t₇ and t₊₄ in

the control group and the divorced sample, I calculate a difference-in-difference (DiD) effect of 13.4 days for men ($p < 0.01$) and 4.1 days for women ($p < 0.05$).

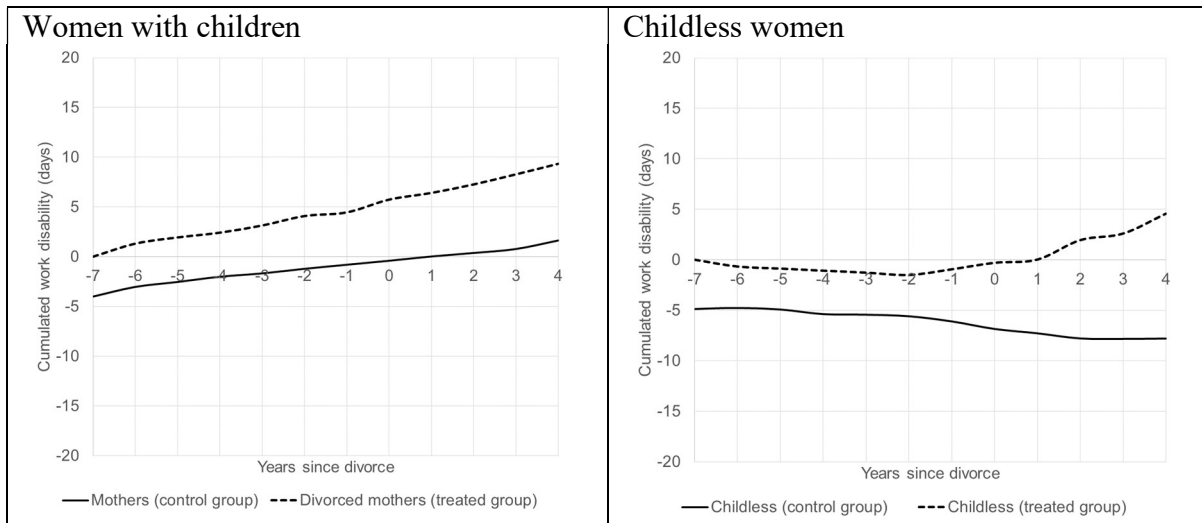
Figure 4-1: Beta coefficient from the OLS model. Outcome variable: Cumulated days of disability since age 15 (Reference category: Divorced at t_0)



Note: Pooled OLS models of cumulated work disability days around the time the divorce file was opened. Displayed are the coefficients of group and time interaction from Table A. 4-2 (Appendix). The control group is chosen by four nearest neighbour matching, with common support and caliper 0.02 at baseline covariates in t_{-7} . Coefficients are shown separately for men and women.

Figure 4-2 displays the results for the mothers and the childless women. The figures again show that the divorcees tended to be in poor health before their divorce. Against my expectation, I find that divorce had a greater impact on the health of the childless women than on the health of the mothers, as the curve was much steeper for the childless women than for the divorced mothers. I again calculated a DiD for the period t_{-7} and t_{+4} . I obtained a value of 3.7 days for the mothers and a value of 7.5 days for the divorced women without children. Thus, the increase seems to have been more pronounced for the childless women. However, as the p-values were 0.09 and 0.17, respectively; I have to conclude that neither of the changes was of statistical significance.

Figure 4-2: Beta coefficient from the OLS model. Outcome variable: Cumulated days of disability since age 15 (Reference category: Divorced at t_0)



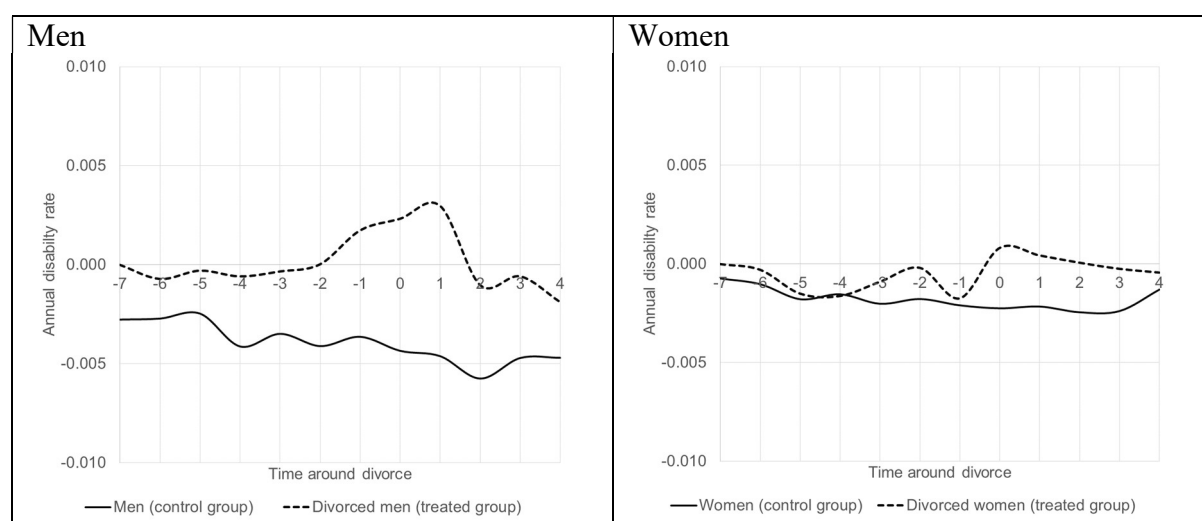
Note: Pooled OLS models of cumulated work disability days around the time the divorce file was opened. Displayed are the coefficients of group and time interaction from Table A. 4-2 (Appendix). The control group is chosen by four nearest neighbour matching, with common support and caliper 0.02 at baseline covariates in t_{-7} . Coefficients are shown separately for mothers and childless women.

4.7.2 Work disability rate

The analysis of cumulated work disability days revealed that the divorced women were already a select group before their separation. I now display the standardised yearly rate in order to highlight the fluctuation around t_0 . In Figure 4-3, I display the results for the men and the women. For the mothers and the childless women, the sample sizes are, unfortunately, too small to allow me to conduct an equivalent analysis. The figure shows that the disability rate of the control group was always lower than that of the divorcees. Indeed, it appears that the health status of the control group improved slightly over time. The increase in the disability rate over time can very likely be attributed to a shift in job profiles to the service sector, improvements in workplace security, and better medication and rehabilitation over time. The initial difference between the divorced and the control sample was small, amounting to 0.0027 for the men and to 0.00048 for the women. However, beginning with t_{-4} for the men and t_{-3} for the women, the rates started to dynamically diverge from those of the control group ($p < 0.01$). I interpret this pattern as signalling the beginning of the separation process or the anticipation of the separation. For the men, this process peaked at t_{+1} which coincides with the median date when the divorce was legally finalised. The pattern for the women was more irregular. The disability rate had already peaked at t_0 and had declined considerably at t_{-1} (p-value of 0.35, Table A. 4-3, Appendix). The results of the analysis suggest that the health of the men (as shown in the

pension data) was more affected by divorce than that of the women. I should, however, point out that my approach does not allow for a direct comparison of effect sizes, because I analysed the men and the women separately. The effect sizes for the men were greater because most of the men worked full-time. As the women were often working part-time or only marginally, they may have adopted different strategies for coping with health impairments. In addition, many of the women entered employment after their divorce, and may have shied away from taking large numbers of days off for health reasons.

Figure 4-3: Beta coefficient from the OLS model. Outcome variable: Yearly work disability rate (Reference category: Divorced at t_0)



Note: Pooled OLS models of the yearly work disability rate around the time the divorce file was opened. Displayed are the coefficients of group and time interaction from Table A. 4-2 (Appendix). The control group is chosen by four nearest neighbour matching, with common support and caliper 0.02 at baseline covariates in t_{-7} . Coefficients are shown separately for men and women.

4.8 Discussion

Using administrative pension data, this study examined work disability patterns among divorcees in West Germany. I provided an estimate of the effect of divorce on health impairments. I did so by calculating the difference between divorcees and a control group in the uptake of work disability. This value summed up to 13.4 days for the men, 4.1 days for the women, 3.7 days for the mothers, and 7.5 days for the childless women compared to the control group, and holding control variables constant. This increase was, however, statistically significant only for the men and the sample of all of the women. Although my findings suggest that men's health was more affected by divorce than that of women, I want to emphasise that direct comparisons of effect sizes were not possible in my framework. In particular, it is important to keep in mind that most of the men were working full-time, while most of the

women were in part-time or marginal work. It is therefore possible that the women were less likely than the men to register as sick with an employer, even if they were grappling with similar health impairments. In addition, many women started working around the time of their divorce. As they had to establish themselves in the labour market, they may have shied away from taking long periods of sick leave. While I could not compare the size of the effect across the subsamples, I was able to compare the temporal ordering of divorce and health impairments. The findings indicate that, on average, the women adapted to their new life earlier (peak at t_0), while the men's health did not start to improve until after t_{+1} .

The study also examined health selection. The results of my analysis show that the women, and particularly the mothers, were, at t_{-7} , already showing signs of poor health, as they had four more cumulated work disability days (counted since the age of 15) than a control group with similar baseline characteristics. These values are significant, and seem to support the argument that social selection contributes to the likelihood of a divorce. Thus, my results stress the point made by Fu and Goldman (2000), who observed that if selection is important, then researchers might have been overstating the negative effects of dissolution on health and exaggerating the benefits of marriage. In other words: “... *sample selection temper conclusions about divorce being causal in driving health. The primary argument is that worse health outcomes among the divorced reflect elevated divorce risks among individuals with worse health*” (Couch *et al.* 2015: 1491).

However, although I found some evidence of selection, I also observed that divorce had a large impact on health status. The findings from this investigation allow me to draw some policy-relevant conclusions. First, I note that the uptake of work disability is an important outcome, because work disability limits the scope of labour market participation and of access to secure income. Spending longer periods in work disability might even reduce an individual's employability, retirement income, and material well-being. As well as having personal costs, long-term disability creates public costs, including the loss of working days and the costs associated with providing sick pay, health services, and rehabilitation services. My results show that separation and the anticipation of separation had immediate effects on health for all of the subsamples. Thus, I conclude that to ensure that spouses and their children emerge from the divorce process less compromised and healthier, psychological help or mediation services should be made available (Hannighofer *et al.* 2017).

Finally, there are several caveats to this study. First, the register data do not constitute a full sample of the population. For example, civil servants and farmers are not included. It is possible

that these groups behave very differently. Most importantly, my approach was based on a nearest neighbour matching method that relied on the observable covariates in the data. Variables such as psychological disposition, lifestyle factors, and work characteristics were not included in the data, but may be important for understanding health impairments after divorce. Furthermore, to allow for a causal interpretation of my results, more rigorous testing and further sensitivity analysis would be needed.

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Appendix

Table A. 4-1: Four nearest neighbour matching summary parameter

Mean bias	1.2	2.5	0.8	1.9
Median bias	0.8	2.7	0.9	2.0
Max. difference in propensity score	.002372	.0114435	.0060626	.0004681

Note: The mean and median bias are summary indicators of the standardised percentage bias. The bias refers to the % difference of the sample means in the divorced and control sub-samples (for details see Rosenbaum and Rubin, 1985).

Table A. 4-2: Weighted pooled OLS estimation with cumulated work disability and disability rate as the dependent variable over 12 years (t_{-7} to t_{+4})

	Men		Women				Women with children		Childless women		
	cumulated	rate	cumulated	rate	cumulated	rate	cumulated	rate	cumulated	rate	
Foreign	1.46		2.39	***	0.001	**	4.76	***	-9.67	***	
Age	-4.24	***	0.000		-2.96	***	-0.001	***	-4.69	***	0.51
Age ²	0.08	***	0.000	*	0.04	***	0.000	***	0.05	***	0.00
Year file opening	0.15		0.000	**	0.06		0.000	***	0.47	***	-0.87
Month file opening	0.07		0.000		-0.10		0.000		-0.01		-0.05
# children < age 3					2.30	***	0.000		1.39	**	
# children age 3-6					2.64	***	0.000	*	2.41	***	
# children > age 6					2.75	***	0.001	***	4.38	***	
Parental leave					-0.01	***	0.000	***	-0.01	***	
Unemployment rate	0.49		-0.001	***	0.38		0.000	***	0.20		0.22
Employment	0.02	***	0.000	***	0.01	***	0.000	***	0.01	***	0.02
Employment ²	0.00	***	0.000	***	0.00	***	0.000	***	0.00	***	0.00
Earnings employment	-3.59	***	-0.001	***	-1.33	***	0.000	***	0.27		-4.15
Earnings employment ²	-0.01	***	0.000	***	0.00		0.000		-0.03	***	0.06
Military service	-0.04	***	0.000	***							
Vocational training	0.01	***	0.000	*	-0.01	***	0.000	***	-0.01	***	-0.01
Constant	-255		0.366	**	-70		0.373	***	-854	***	1730
R ²	0.06		0.01		0.04		0.01		0.04		0.01

Note: *** p<0.001, ** p<0.01, * p<0.05. Further control variables are time since separation (coefficients are displayed in Figure 4-1 to 4-3).

Table A. 4-3: Adjusted Wald Test for the equality of two coefficients

year	Men cumulated	Men rate	Women cumulated	Women rate	Women with children cumulated	Women without children cumulated
-7	0.206	0.009	0.000	0.388	0.000	0.107
-6	0.192	0.050	0.000	0.119	0.000	0.188
-5	0.172	0.061	0.000	0.333	0.000	0.208
-4	0.091	0.001	0.000	0.911	0.000	0.197
-3	0.051	0.005	0.000	0.009	0.000	0.232
-2	0.020	0.001	0.000	0.001	0.000	0.257
-1	0.004	0.000	0.000	0.346	0.000	0.173
0	0.000	0.000	0.000	0.000	0.000	0.095
1	0.000	0.000	0.000	0.000	0.000	0.069
2	0.000	0.000	0.000	0.000	0.000	0.024
3	0.000	0.001	0.000	0.006	0.000	0.021
4	0.000	0.024	0.000	0.014	0.000	0.008

Note: Displayed are p-values for the H0 hypothesis that two coefficients (control and divorced) are equal.

Chapter 5

Keck, W., Radenacker, A., Brüggmann, D., Kreyenfeld, M., Mika, T. (2019): Statutory Pension Insurance Accounts and divorce: A new scientific use file, Jahrbücher für Nationalökonomie und Statistik.

<https://doi.org/10.1515/jbnst-2019-0064>

5 FDZ-RV Scientific Use File: Statutory Pension Insurance Accounts and Divorce

5.1 Introduction

The Scientific Use File of the “Sample of Insurance Accounts” (*Versicherungskontenstichprobe, VSKT for short*) is one of the main products of the Research Data Center of the German Pension Insurance. This product offers, for example, the complete monthly employment and earning histories, measured in terms of earning points, of the persons covered in the German Statutory Pension Insurance. It also includes demographic variables, such as the monthly dates of childbirth, albeit for one parent only (nearly always the mother). The data has been used to study many facets of behavioural change in Germany by exploiting the rich longitudinal information available in this data (www.fdz-rv.de/Literatursuche). As of reporting year 2015, it will include further demographic variables, namely monthly dates of marriage and divorce for the divorcees, opening up new avenues for research, in particular to study the economic ramifications of divorce in Germany based on register data.

Information on dates of marriage and divorce were extracted of the “Statistics for the Equalization of Pension Entitlements after Divorce” (*Versorgungsausgleichsstatistik*). The background of this data is the fact that since 1977, the statutory pension rights between ex-spouses have to be adjusted at divorce. This process is conducted by the German Pension Insurance, which stores information related to the process of pension adjustment. This article explains in more detail the *Versorgungsausgleichsstatistik*, which we will abbreviate with VA-Statistics in the following for improved readability of the text. We compare the sample of insurance accounts extended by variables on divorce with other sources on the divorced population and examine to what extent divorces are captured in the pension data. Finally, we explain how this data will be made available to the scientific community and show how it can be used for empirical investigations.

5.2 The Statistics on the Equalization of Pension Entitlements after Divorce (VA-Statistics)

5.2.1 Legal Background of the VA-Statistics

The Statistics on the Equalization of Pension Entitlements after Divorce (VA-Statistics) is a complete register of persons who have gotten divorced since 1977 and whose pension entitlements were equalized after divorce. There are a couple of caveats that must be mentioned, which pertain to the completeness of the data. First, divorced persons have to be alive at the time of the data retrieval, which is in 2015 for this release of the data. Thus, there may be a

selectivity on survival for the older divorce cohorts in the sample. Second, and most importantly, only divorces are included that resulted in an equalization of pension entitlements. As this hinge on the legal regulations, we will explain these in greater detail in the following.

The equalization of pension entitlements between ex-spouses was first introduced as a mandatory element of the divorce process in the Federal Republic of Germany in 1977. In 1992³⁷, it was implemented in East Germany as well. After a divorce, the earning points of the couples that had been gathered during marriage are added and equally divided. A motivation for the introduction of this regulation is the close linkage between prior earnings and pension entitlements on the one hand and the unequal division of paid and unpaid labour between spouses that exists in Germany on the other. The equalization of pension entitlements basically protects the rights of the “economically weaker” spouse and aims at compensating the unequal distribution of tasks during the marriage. While it is obligatory to have the pension splitting included in the divorce proceeding, it is also possible to opt out from this procedure. Legal scholars however seem to agree that the possibilities to arrive at an agreement other than the standard procedure are not much used by spouses (see e.g. Langenfeld and Milzer 2015:213, fn. 8) which leads to the assumption that the equalization of pension entitlements according to the law is conducted in the majority of divorce proceedings. We will later see that this assumption may be incorrect and that a substantial share of couples has opted out of the procedure.

Beyond that, there has been a legal change that also affected the way in which divorces are covered in the pension data. Until 2009, statutory pensions, occupational and private pensions were considered for the division of pension rights after divorce. The procedure was generally judged as very complicated, because the occupational and private pensions had to be reevaluated in terms of their monetary future value and were then recalculated as entitlements to the statutory pension insurance scheme. The whole adjustment of old age security between the partners’ entitlements took then place in the pension insurance. Both parties were entitled to reopen the settlement for readjustment if they considered the given result unjust due to changes in the pension entitlements. In 2009, the so-called “*Strukturreform des Versorgungsausgleichs*” (structural reform of the equalization of pension entitlements) came into force. This reform aimed at simplifying the procedure and reduced the ongoing adjustments

³⁷ Gesetz zur Überleitung des Versorgungsausgleichs auf das Beitrittsgebiet (VAÜG), <https://www.jurion.de/gesetze/rueg/31/>.

of the entitlements after the divorce. One cornerstone of this reform applied to the private and occupational pensions. Since the reform, the respective entitlements after a divorce are no longer administered by the German Pension Insurance (Reimann and Wiechmann 2009:79).³⁸ These legal changes simplified the calculations and limited the division of pension rights processed by the German Pension Insurance. However, the change of regulations affected the comparability of the data across time (FDZ-RV 2018; Wagner 2012). Firstly, the pension adjustments no longer include private and occupational pension entitlements. Secondly, the share of divorces covered in the data was affected. Couples who did not have any public pension insurance entitlements, but only occupational or private pensions, were no longer recorded in the data (FDZ-RV 2018:14). Another cornerstone of the reform concerned marriages of short duration (less than three years), which were now, by default, exempt from an adjustment of the pension rights unless one spouse demanded the adjustment. This regulation means that the data prior and after 2009 are not fully comparable.

5.2.2 Coverage of Divorces in the VA-Statistics

In order to get an understanding of the coverage of divorces in the Pension Insurance Data, we compared the number of divorces in the pension registers with the number of divorces in the divorce statistics. In the period 1992 to 2015 there are 4.407.695 divorces recorded by the German Statistical Office (Statistisches Bundesamt 2017), but there are only 2.815.334 divorces in the pension registers³⁹. Thus, for this period only about 64 percent of all divorces are represented in the pension data. The under-coverage can be attributed to the following reasons:

- The VA-Statistics 2015 only includes divorces when at least one partner is still alive in 2015. Thus, for earlier years, under-coverage can be due to **selection on survival**. For later years, only a smaller fraction of divorces are lost due to death. Additional analyses have shown that in 2015, for example, about 2 percentage points of the gap between the VA-Statistics and the data from the German Statistical Office in 2008 can be explained by deaths.
- There is a small fraction of divorces that were enacted in 2015, but that are not yet included in the pension registers. The reason is a **delay of inclusion in the pension**

³⁸ Versorgungsausgleichsgesetz (VersAusglG), <https://dejure.org/gesetze/VersAusglG>.

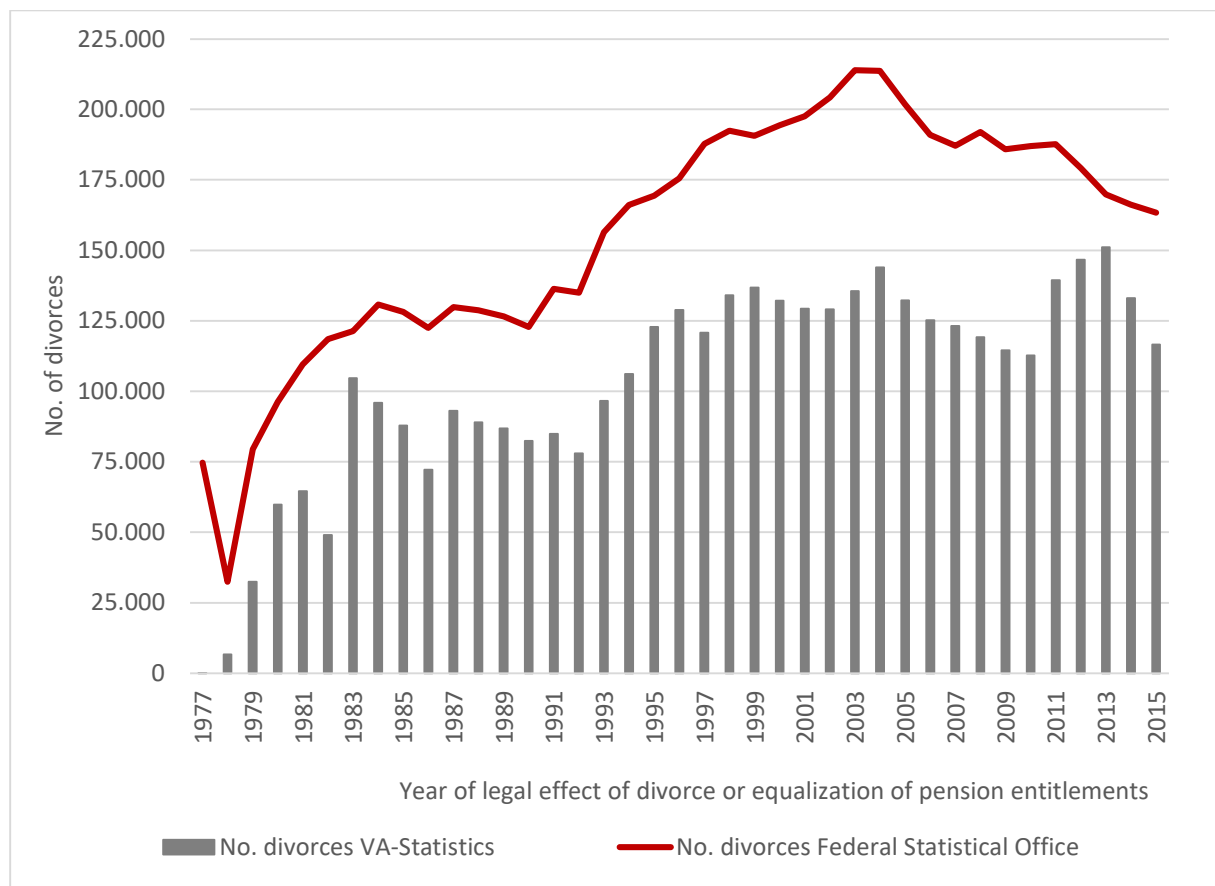
³⁹ For this calculation, we only considered divorces of females.

registers. It applies to 6.1 percent of the total cases in 2015. There is no particular selectivity associated with these cases and it only affects the most recent year.

- Another way of dividing pension rights between spouses is by **agreements under the law of obligations** (*schuldrechtlicher Versorgungsausgleich*). This kind of agreement is not part of the VA-Statistics because no pension claims are transferred. However, it is unknown to what extent this kind of agreement is applied in practice.
- One important reason for the discrepancy is probably that **spouses avoided the regulations** and did not initiate the process of pension rights adjustment during the divorce procedures. In fact, it is often assumed in the legal literature that deviations from the standard regulation are rare and uncommon (Langenfeld and Milzer 2015: Rn. 679). However, the statistics of the family courts reports the share of divorces without pension splitting and gives numbers of around 25 percent for the years 2011 until 2018 (Statistisches Bundesamt, div. Jahrgänge). To our knowledge, there is no external source that gives information on the reasons why couples avoid the regulation. It is, however, likely that the couples who did avoid the procedure are selective. The following reasons may come into play:
 - One possible reason is that the **difference between the pension entitlements between the partners is negligible**. In this case, the legal fees paid to divorce lawyers and the court would be high in comparison to the pension credits that are transferred. As a consequence, the VA-Statistics is more likely to cover the standard “male breadwinner couple” than the “dual earner couples”, where both partners earn equal wages.
 - It can also be assumed that **marriages of shorter duration** are less likely to be covered, because the amount of pension points that must be split are often negligible. Since the reform in 2009, marriages with a duration of less than three years are furthermore exempt by default from the pension rights adjustment after divorce.
 - Partners may also have avoided the regulations because they have entered into a more general agreement on how assets should be divided after divorce. This applies, in particular, to **couples with large amounts of wealth** and private property. Wealthy couples, regardless of the division of labour, may be less likely to be included in the data.

The Federal Statistical Office collects the number of divorces by the year when the divorce was legally effective. In general, the year of the legal divorce matches the year of pension right adjustment. However, there are exceptions to this rule. In order to illustrate this, we have plotted in Figure 5-1 the number of divorces gathered from the data of the Federal Statistical Office and the VA-statistics. Divorce from the first data source are displayed by the legal date of divorce. Divorces from the latter data source are displayed by the date when the pension equalization became final. The figure clearly shows that pension right adjustments are delayed when a legal reform is implemented. For example, the figure shows a drop and subsequent spike of in the VA-data for the year 1983. This year saw the introduction of a new legislation on pension adjustments on divorce in case of hardship. There is also a drop and a subsequent increase around 1992, which coincides with the introduction of the pension splitting in East Germany. The figure shows another peak in 2013 as a consequence of the reform of the pension rights adjustment in 2009. The explanation for the steep increase and the subsequent decrease lies in the fact that before the reform pension entitlements from East and West Germany could not be set off against each other. In such cases, the equalization of pension entitlements was postponed and the marriage was legally divorced without a pension equalization. After the reform, the suspended procedures were resumed and were supposed to be realized within five years, i.e. before September 1, 2014, which explains the abrupt decline starting in 2014.

Figure 5-1: Number of divorces in the Statistics of the German Federal Statistical Office (by year of legal divorce) and in the VA-Statistics (by year of pension adjustment)



Source: Federal Statistical Office 2018, FDZ-RV: VA-Statistics 2016, own estimations.

5.3 Distribution of Data to the Scientific Community

5.3.1 VSKT & VA-Statistics 2015

The VA-Statistics is a separate register. However, it will not be provided to the scientific community as a separate product. Instead, information from the VA-Statistics was merged to the VSKT by an individual identifier. The VSKT is one of the main and established products of the Research Data Center of the German Pension Insurance (Stegmann 2018). It is a sample of insurance histories of all insured persons in the German Pension Insurance aged 15 to 67 years. The universe of insurance accounts covers about 90 percent of the resident population of Germany. It includes all persons who have had at least a one-month creditable period in the German Pension Insurance during their lifetime. In addition, all persons currently living abroad but who do have at least a one-month creditable period in Germany are also covered. From this overall population a one-percent sample has been drawn (see Stegmann 2018 for details). The Scientific Use File of this sample is furthermore restricted to all insured persons of the birth cohorts 1948 to 1985 with German citizenship living in Germany.

The combined data set is labeled VSKT-VA. The first release of it is available for the year 2015. For this year, it includes 267,812 individuals of which more than 34,000 are divorced. Table 5-1 lists the new variables available in this source. These are the year and month of the marriage as well as the dates of the effective divorce. First and second order divorces and their respective marriage and divorce dates are included. Higher order divorces are not included due to data protection reasons. However, a flag for people with more than two divorces is provided. The data does not record the date when the couple separated. However, it includes information on the date when the defendant receives the divorce petition (thus one of the partners officially initiated the divorce proceeding with a lawyer). As the German family law requires couples to be separated for at least one year before they can get legally divorced, the date of separation is at least ten months before the date when the defendant receives the divorce petition. Moreover, variables are added that contain information on the pension entitlements that have been transferred between spouses within the statutory pension insurance. Finally, there is information about the legislation under which the divorce was obtained. The reform of 2009 took effect in almost half of the court decisions on the equalization of pension entitlements in 2010 and was only fully implemented after 2011. This variable allows data users to clearly identify cases for which the new legislation was in force. All dates in the data were also transferred into reference months, starting from the January of the year a person turned 14 years of age (which is the process time by which the employment and earning histories are stored in the data).

Table 5-1: New variables on marriage, divorce, and the equalization of pension entitlements

Variable	Label
EB n _Jahr	Year in which marriage n * began
EB n _Monat	Month in which marriage n began
EB n _Bio	Month in which marriage n began in the individual's biographical timeline
ES n _Jahr	Year in which marriage n ended**
ES n _Monat	Month in which marriage n ended**
ES n _Bio	Month in which marriage n ended*** in the individual's biographical timeline
ES n _RS	Legislation under which the divorce n was obtained
VAZU_ES n	Bonus out of pension splitting for marriage n
VAAB_ES n	Deduction out of pension splitting for marriage n
DRK_Jahr_ n	Year of effect of divorce n or of equalization of pension entitlements
DRK_Monat_ n	Month of effect of divorce n or of equalization of pension entitlements
Anz_Scheidung	Total number of divorces in the VA-Statistics

* Information is provided for the first two divorces ($n=1$ or 2).

** End of marriage is the month preceding the month when the defendant receives the divorce petition.

5.3.2 Selectivity of Divorces in the VSKT-VA

To gain further insights into the selectivity of divorcees in the VSKT-VA, we compared basic indicators with data from the Socio-Economic Panel (SOEP). We restricted both samples in a way that they were comparable. We focused on women of the birth cohorts 1948 to 1985 with German citizenship. Furthermore, we considered only marriages which lasted more than three years starting from the month of the marriage until the month before the defendant receives the divorce petition. We restricted women's age at divorce to range from 25 to 55 and limited to divorces that occurred in the years 2013 to 2015. In the SOEP, we excluded respondents who were civil servants, farmers, salaried professional workers like lawyers and tax accountants, and the self-employed at the time of interview. The reason for this selection is that the pension data do not include these professions.

In Table 5-2 we provide mean values of selected socio-demographic variables for both samples and the respective t-test of equal means. The comparison with the SOEP shows that overall patterns are very similar. However, the average number of children is slightly lower in the VSKT-VA than in the SOEP. This may be due to the fact that children were assigned in some instances to the fathers in the VSKT-VA. However, we cannot rule out that the number of

children is too high in the SOEP. Unit non-response is lower for women with children in social science surveys as interviewers can more easily reach them. Albeit the SOEP analysis is weighted, the weights may not fully account for this. We also see differences in marriage duration. This is particularly pronounced in the East German sample. These differences pertain to the different definitions of divorce dates in the two data sets, which is the date of legal divorce in the SOEP and the legal date of pension rights adjustment in the VSKT-VA. The VSKT-VA includes the date when the divorce petition was filed, which is usually a couple of months before the legal enactment of the divorce. This is a better measure for the duration of marriages and also matches better the SOEP data.

Table 5-2: Mean values of selected variables in the SOEP and the VSKT-VA 2015, separately for West and East Germany, divorced women*

Variable	Germany		
	SOEP	VSKT-VA	t-test $p> t $
Age at divorce	42.35	42.26	0.853
Marriage duration	14.45	16.10	0.001
Years until filing of divorce	--	14.05	--
Number of minor children at divorce	1.44	1.11	0.000
Age of youngest minor child at divorce	9.85	9.33	0.139
Employment rate	0.72	0.69	0.196
Pension points	0.060	0.065	0.102
Nominal gross income in €	1,973	2,096	0.179
N	283	1,781	
	West Germany		
Age at divorce	42.57	41.62	0.045
Marriage duration	14.58	15.27	0.190
Years until filing of divorce	--	13.86	--
Number of minor children at divorce	1.44	1.20	0.002
Age of youngest minor child at divorce	10.04	9.21	0.028
Employment rate	0.73	0.67	0.078
Pension points	0.059	0.060	0.595
Nominal gross income in €	1,968	2,027	0.547
N	241	1,516	
	East Germany		
Age at divorce	41.07	45.95	0.000
Marriage duration	13.69	20.81	0.000
Years until filing of divorce	--	15.11	--
Number of minor children at divorce	1.45	0.58	0.000
Age of youngest minor child at divorce	8.73	10.47	0.083
Employment rate	0.71	0.78	0.310
Pension points	0.069	0.083	0.127
Nominal gross income in €	2,008	2,376	0.149
N	42	265	

* Divorced women of the cohorts 1948 to 1985 who are at age 25 to 55 at divorce with minimum marriage duration of three years, divorces of years 2013, 2014 and 2015. For the SOEP, West and East Germany refers to the federal state the person lived in at the time of the survey. For the VSKT-VA, West and East is constructed by a variable that states whether pension points were exclusively accumulated in either West Germany (OPXAZ=0) or East Germany (OPXAZ=1). Income in SOEP is top-coded according to the earnings ceilings of the statutory pension insurance with different amounts for East and West Germany for the respective year. Nominal gross income is not deflated. Pension points accumulated in East Germany are weighted with the following values to account for different wage levels in East and West Germany: 1.1785 (2012), 1.1762 (2013), 1.1665 (2014). Employment, pension points and income refer to the year prior divorce. Pension points and gross incomes are calculated for those who were continuously employed (12 months) in employment subject to social security contributions.

5.4 Recommendations for Data Use

The data opens up the potential to track men's and women's employment and earnings over the divorce process. While prior studies were often confined by small sample sizes, this data makes it possible to study population subgroups and explore changes in the ramifications of divorce

across time. A limitation of the data is that the definition of the date of divorce deviates from the one used in the official statistics. Furthermore, the data do not cover all divorces enacted in Germany. We refrain from providing a weighting factor for this data. However, a couple of recommendations should be given:

- **Shorter marriages** (of less than three years) should be excluded if a comparison across time is conducted that includes the period as of 2009.
- Divorces in **East Germany** are only covered as of 1992. Thus, East Germany must be excluded from the analysis if investigations are conducted for the time prior to 1992.
- For divorcees with employment biographies in both East and West Germany and consequently with earnings points from East as well as West Germany, the effective date of divorce is not available because the pension splitting had to be suspended at the time of divorce until after the reform of 2009. Instead of the legal effect of the divorce (documented in the pension data at the date when the equalization of pension entitlements was carried out) we strongly recommend to use the date when the **divorce petition was filed**.
- It seems likely that standard “male breadwinner couples” are more likely to be included in the data than “dual earner constellations”. We do not see any possibility to weight the data to cure that problem. However, separate analyses can be conducted by the prior labour market attachment of women, which somehow ameliorates the problem.

5.5 Conditions of Usage

The SUF-VSKT_VA 2015 with extended information on VA-Statistics is available through the Research Data Center of the German Pension Insurance (*Forschungsdatenzentrum der Rentenversicherung, FDZ-RV*). Researchers at scientific institutions and universities can apply for using a scientific use file or for working with the data onsite at the *FDZ-RV*. The application form is available online (www.fdz-rv.de). The data file and the corresponding material (codebook) will be available in German and English.

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6 Declaration: List of individual papers and pre-publications

This cumulative dissertation includes a general introduction (Chapter 1), three research papers (Chapters 2, 3 and 4) and one data observer article (Chapter 5).

Chapter 2 is single-authored.

Chapter 3 is co-authored with Michaela Kreyenfeld.

Chapter 4 is single-authored.

Chapter 5 is co-authored with Wolfgang Keck, Anke Radenacker, Michaela Kreyenfeld and Tatjana Mika.

Chapter 2: Women's employment, income and divorce in West Germany: A causal approach.

It is published in the Journal for Labour Market Research:

Brüggmann, D. (2020): Women's employment, income and divorce in West Germany: A causal approach, Journal for Labour Market Research 54(5).

<https://doi.org/10.1186/s12651-020-00270-0>

Chapter 3: Earnings Trajectories after Divorce: The Legacies of the Earner Model during Marriage.

It is published in Population Research and Policy Review.

Brüggmann, D., Kreyenfeld, M. (2023): Earnings Trajectories after Divorce: The Legacies of the Earner Model during Marriage, Population Research and Policy Review, 42(23).

<https://doi.org/10.1007/s11113-023-09756-4>

Chapter 4: Work disability and divorce.

It is published as a book chapter in: Parental life courses after separation and divorce in Europe.

Brüggmann D. (2020): Work disability and divorce, In: Kreyenfeld M., Trappe H. (eds) Parental life courses after separation and divorce in Europe, Life Course Research and Social Policies 12, Springer, Cham.

https://doi.org/10.1007/978-3-030-44575-1_14

Chapter 5: Statutory Pension Insurance Accounts and divorce: A new scientific use file.

It is published in the Journal of Economics and Statistics.

Keck, W., Radenacker, A., Brüggmann, D., Kreyenfeld, M., Mika, T. (2019): Statutory Pension Insurance Accounts and divorce: A new scientific use file, Jahrbücher für Nationalökonomie und Statistik.

<https://doi.org/10.1515/jbnst-2019-0064>