Marriage and Divorce: The Role of Unemployment Insurance*

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Abstract

This paper examines how changes in household-level risk sharing affect the marriage market. We use a German unemployment insurance (UI) reform that tightened means-testing based on the partner's income as our laboratory. The reduced generosity of UI increased the demand for household-level risk sharing, which lowered the attractiveness of individuals exposed to unemployment risk. Because unemployment risk correlates with non-German nationality, our main finding is that the UI reform led to a decrease in intermarriage. The 2004 expansion of the European Union had a comparable effect on intermarriage for the affected nationalities. Both reforms increased marital stability, which is consistent with a better selection of couples.

Keywords: Marriage, Divorce, Household Risk Sharing, Unemployment Insurance, Labor Market Reform, Intermarriage, EU Expansion
JEL Classifications: J10, J12, J15, J64, J65

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1 Introduction

Living in a union with another individual is beneficial for many reasons. Besides the emotional value of companionship and love, economic motives matter for partner choice, too. First, economies of scale and household specialization increase joint consumption and utility (Muellbauer, 1977; Becker, 1981; Grossbard-Shechtman, 1984). Second, the family facilitates risk sharing: a working spouse provides insurance against income shocks, e.g. due to unemployment or sickness (Lundberg, 1985; Cullen and Gruber, 2000; Shore, 2010; Chiappori and Reny, 2016). Under the assumption that utility is transferable within the household (Becker, 1973), these economic rents generate a *marital surplus* that is shared between the spouses and governs marriage and divorce decisions.

While a thriving literature analyzes household consumption choices, sharing rules, and welfare empirically,[1] relatively little is known about the quantitative importance of household-level risk sharing. Most existing studies either focus on time-series correlations between marriage, divorce, and unemployment rates at the macro level[2] or on associations between unemployment and marital instability at the micro level.[3] We provide a complementary study that exploits both variation in the exposure to unemployment risk and a social security reform to identify the effect of within-household insurance on marital surplus.

The idea underlying our identification strategy is that insurance against income shocks is not exclusively provided at the household level. Social insurance is a substitute. The value of this substitute varies over time as policies change, altering the demand for within-household insurance and marital surplus. Consequently, social insurance reforms can affect marital surplus and influence marriage and divorce decisions.

Our laboratory to test this hypothesis is the German unemployment insurance (UI) system. UI is a substitute for spousal insurance because unemployment benefits reduce

[1] Blundell et al. (1994), Pesaran and Wickens (1999), and Chiappori and Mazzocco (2017) are excellent surveys of this literature. Lise and Seitz (2011), Browning et al. (2013), and Cherchye et al. (2015) are examples of recent contributions.

[2] A common finding in this literature is that marriage and divorce rates are pro-cyclical, that is, they decrease in recessions. Correlations with the unemployment rate are typically negative (Amato and Beattie, 2011; Hellerstein and Morrill, 2011; Schaller, 2013; González-Val and Marcén, 2017a,b). Wang (2019) studies joint job search decisions of couples in a life-cycle model with risk sharing. Using US micro data, she finds that gender differences in the cyclicality of unemployment can be explained by household-level risk sharing.

[3] Based on Danish and Norwegian micro data, Jensen and Smith (1990) and Hansen (2005) suggest that unemployment is an important contributing factor to marital instability.

the dependence on the partner upon job loss. In January 2003, the Hartz I reform—the first of four labor market reform packages implemented in Germany between 2003 and 2005—sharply tightened the means testing of long-term unemployment assistance against the partner's income. This increased the demand for within-household insurance and, thus, made individuals who are exposed to unemployment risk less attractive in the marriage market. We study the variation in unemployment risk at the individual level and estimate how labor market transition probabilities correlate with different observable characteristics. Using social security data from the Federal Employment Agency, we find that nationality is a quantitatively important determinant of unemployment risk, even conditional on age, education, gender, time, and region.

We compute marital surplus, our primary outcome variable, based on the Choo and Siow (2006) model of marriage market matching. In this model, agents have unobserved and heterogeneous tastes for different partner types. A key advantage of the model-based estimator is that both time-varying numbers of men and women and permanent differences between types, e.g., culture and language, are explicitly taken into account. We compute the surplus based on the flow of new marriages recorded in the German marriage register, which contains information on all legal marriages between 1997 and 2013. The stocks (numbers) of single individuals are extracted from the German Microcensus. Moreover, we use the German divorce register to study reform effects on marital stability. To the best of our knowledge, this is the first paper in the family economics literature that uses the German marriage and divorce registers.

We estimate effects of the Hartz I labor market reform on both marital surplus and marital stability in a differences-in-differences framework. Based on our finding that non-German nationality increases unemployment risk, we define as the treatment group intermarriages between German citizens and spouses of foreign nationality.[4] The idea is that intermarriages are on average more exposed to unemployment risk. We verify the composition of the treatment/control groups based on language ability (proxied by linguistic distance to German) and labor market access (proxied by European Union (EU) membership of the non-German spouse's home country).

Our main finding is that the labor market reform had a sizable negative effect on the

[4]Note that our definition is based on citizenship and not ethnicity. In related research, Caucutt et al. (2018) use a comparable empirical design to investigate to what extent racial differences in marriage market outcomes in the U.S. are explained by high unemployment and incarceration rates of black men.

marital surplus of intermarriages in Germany. According to our preferred specification, the marital surplus of all treated marriages decreased by 0.410 log-points. Compared to the pre-reform marital surplus, this is equivalent to a reduction of 5.2% for marriages between Germans and citizens from the eastern European states that joined the EU in 2004[5] and 6.6% for marriages between Germans and citizens from countries outside the EU. Regarding marital stability, we find that intermarriages formed after the reform were significantly less likely to divorce. We argue that this effect due to positive selection. Moreover, we directly take into account the effect that the EU expansion in 2004. It also had a sizable negative effect on the surplus of intermarriages with citizens of the new member states because the right to live and work in Germany was no longer part of the surplus. This finding is in line with Adda et al. (2022) for Italy.

The effect of the Hartz I labor market reform on the marriage market, and on intermarriages in particular, is a finding of high policy relevance. For one thing, the marriage market ramifications of a reform that was designed to reduce unemployment were most likely not intended by the policy-maker. Apart from that, intermarriages are an important vehicle for the integration of migrants (Azzolini and Guetto, 2017; Adda et al., 2022). Social insurance reforms that make intermarriages less attractive may therefore conflict with a successful migration policy.

Other papers in the related literature share with ours the focus on interactions between social policy and the marriage market. Ortigueira and Siassi (2013) assess the quantitative effects of within-household risk sharing on savings and labor supply in a model with idiosyncratic income risk (Aiyagari, 1994) and two decision makers within the household. Among other findings, their model matches well the elasticity of spousal labor supply with respect to UI estimated by Cullen and Gruber (2000). Low et al. (2018) find that a U.S. welfare reform that introduced lifecycle time limits on the receipt of welfare led, inter alia, to higher marital stability. Persson (2020) argues that the elimination of survivor insurance in Sweden had effects on marriage formation decades before expected payout and, additionally, raised the divorce rate and the degree of assortative matching in the marriage market. Anderberg et al. (2020) study how raising the school-leaving age in the UK in 1972 affected partner choices both in terms of (unobserved) ability and

[5]In 2004, Cyprus, Malta, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Slovakia, and Slovenia joined the EU. We refer to this group of countries as EU10. Before the expansion, the EU had 15 member states referred to as EU15: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, United Kingdom.

qualification. Chen et al. (2021) study the elimination of the Social Security Student Benefit Program in the US and show that it had implications for education-based marital sorting. Finally, our study is related to a number of papers with mixed results that study intermarriage in relation to labor market outcomes (Kantarevic, 2005; Meng and Gregory, 2005; Furtado and Theodoropoulos, 2009; Meng and Meurs, 2009; Basu, 2015; Dribe and Nystedt, 2015).

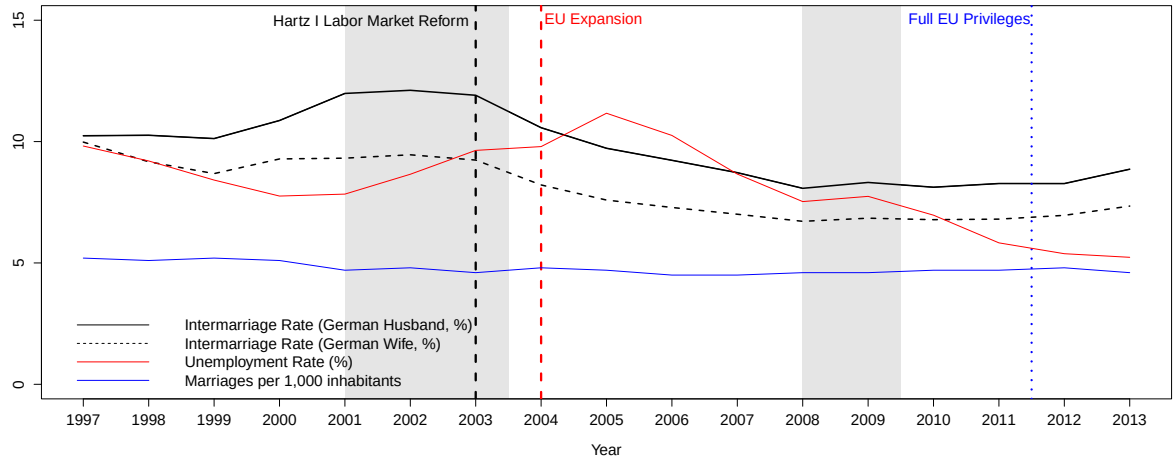
The remainder of our paper is structured as follows: Section 2 describes how marriage and unemployment rates are associated in aggregate data and studies unemployment risk at the individual level. Section 3 introduces our estimator for marital surplus. Section 4 introduces the marriage market data. Section 5 presents our empirical design, estimation results for marital surplus, and robustness checks. Section 6 contains the estimation results for marital stability and Section 7 concludes.

2 Marriage and Unemployment Risk

Figure 1 shows time series of the unemployment and (inter)marriage rates in Germany between 1997 and 2013. Starting from a relatively high level, the unemployment rate increased during and after the recession of the early 2000s and reached a peak of 11.2% in 2005. Thereafter, the unemployment rate decreased and reached 5.2% in 2013. Hartung et al. (2022) calculate that absent the Hartz reforms the unemployment rate in Germany would have been 50% higher at this period.

The number of marriages per 1,000 inhabitants remained flat during the period we consider. Using the marriage register, we zoom in on the flow of new marriages and calculate the intermarriage rate, i.e., the share of all new marriages between Germans and partners with non-German citizenship. Figure 1 depicts the intermarriage rate of German males and females, respectively. The rates evolve in parallel, and intermarriage is more common for German men. In the late 90s, intermarriage became more common while unemployment fell. Then, the rates flattened out during the recession and started to decrease markedly after 2003. Notably, this was the year in which the Hartz I reform was implemented (black dashed line). The downward trend was not affected by the EU expansion (red dashed line) and did not revert before the year 2011 when the unemployment rate approached a historical low.

Figure 1: Marriage and Unemployment Rates in Germany



Notes: The black dashed vertical line indicates the year in which the Hartz I Reform became effective (2003), the red dashed vertical line marks the year in which the EU expansion took place (2004), the blue dashed vertical line marks the year in which citizens of the 2004 EU expansion countries gained full legal EU privileges in Germany. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register, 1997-2013, own calculations. The unemployment rate is extracted from OECD data.

The Hartz I reform is primarily known for policies designed to increase labor demand by deregulating temporary employment and subcontracted labor. A lesser-known reform element is a sharp tightening of household-level means testing of, at that time, long-term unemployment benefits. Before the reform, a maximum of 33,800 Euros of annual income of the partner was exempt from means testing, i.e., not counted against the unemployed person's benefit entitlements. This threshold decreased by more than 60% to 13,000 Euros.[6] Because means testing is only relevant for individuals who share their household with a partner, this UI reform element specifically affected couples but not singles. This feature sets the means testing reform apart from other policy changes that affected both couples and singles. Thus, we focus on the means testing reform.[7] In Section 3, we discuss in more detail how the different reform elements affected the marriage market through the lens of the Choo and Siow (2006) model.

The temporal coincidence of decreasing intermarriage rates and the means testing reform suggests that the tightening had a specific effect on couples in which one individual is not a German national. A plausible explanation is that such couples are on average

[6]The exemption was proportional to the partner's age. Before the reform, the amount was 520 Euros per year of the partner's age (maximum of 33,800 Euros at age 65). This amount decreased to 200 Euros per year of the partner's age.

[7]We cannot rule out that other reform elements also affected couples and singles differentially because either married or single individuals could be over-represented in the treatment groups of specific reform elements. If these effects are quantitatively important, our estimator will capture the total reform effect.

more exposed to unemployment risk. We test whether foreign nationals face a higher risk of job loss in Germany using process-generated micro data from the UI system. We rely on the Sample of Integrated Labour Market Biographies (SIAB).[8] The SIAB is a 2% random sample drawn from social security registers. We use data for the years 1997–2002.[9]

One observation corresponds to an (un)employment spell with at least one of the following characteristics: (i) employment subject to social security, (ii) marginal or part-time employment, (iii) UI benefit receipt, (iv) officially registered job-seekers. We observe the precise start and end date of each spell. To identify the rate of job loss, we count transitions from employment into unemployment and from employment into inactivity. We also estimate job-finding rates based on transitions from unemployment into employment.

The covariates we consider are age, gender, nationality (German, non-German), region (municipality), and education.[10] Note that German social security records do not generally track marital status or information about the partner. We estimate Cox (1972) proportional hazard models, including nationality, gender, and education group dummies. All specifications include region and time effects but show specifications with and without age effects separately.[11] Table 1 presents the results. Columns (1) and (2) show estimated job-loss hazard rates; columns (3) and (4) show estimated job-finding rates.

Column (1) suggests that German nationals have a job loss hazard rate that is 24% lower than the rate for workers without German citizenship. However, this big difference can partly be attributed to age differences between German and foreign workers. Including age effects in column (2) reduces the hazard rate difference to 5.5% because foreign workers are on average younger. This effect is highly significant, so foreign workers are indeed more likely to lose their job. Crucially, this difference is not driven by the gender or educational composition in the respective groups because education and gender are controlled for in the regression. For transitions into employment, the hazard rate

[8] We use the factually anonymous Sample of Integrated Labor Market Biographies (File: `SIAB_7514`). Data access is provided by the Research Data Center (RDC) of the German Federal Employment Agency (BA) at the IAB, project no. 101693. See Ganzer et al. (2016) for more details on the data set.

[9] 1997 is the first year for which the marriage and divorce registers are available. To avoid capturing reform effects on unemployment risk, we exclude all years after 2002.

[10] The education variable in German social security data suffers from missing values and inconsistencies, essentially because misreporting has no negative consequences. We impute missing and inconsistent observations using the methodology proposed by Fitzenberger et al. (2006). We use five levels of education: Lower secondary education without/with vocational training, higher secondary education without/with vocational training and tertiary education (University, University of Applied Sciences). The distribution of German and foreign men and women across educational categories is shown in Appendix Table A.1.

[11] We use six age groups: 18–25, 26–32, 33–39, 40–46, 47–54, and 55–68.

Table 1: Estimated Labor Market Hazard Rates

	Transitions into Unemployment		Transitions into Employment	
	(1)	(2)	(3)	(4)
German citizen	-0.281***	-0.057***	-0.117***	0.006
	(0.012)	(0.012)	(0.010)	(0.010)
	[0.755]	[0.945]	[0.890]	[1.006]
Female	-0.092***	-0.074***	-0.107***	-0.113***
	(0.010)	(0.008)	(0.012)	(0.011)
	[0.912]	[0.929]	[0.899]	[0.893]
Lower secondary education	0.004	0.003	0.163***	0.247***
with vocational training	(0.015)	(0.008)	(0.011)	(0.008)
	[1.004]	[1.003]	[1.177]	[1.280]
Higher secondary education	0.536***	0.166***	0.655***	0.355***
without vocational training	(0.033)	(0.028)	(0.021)	(0.019)
	[1.709]	[1.181]	[1.925]	[1.426]
Higher secondary education	0.308***	0.129***	0.414***	0.359***
with vocational training	(0.021)	(0.014)	(0.015)	(0.015)
	[1.361]	[1.138]	[1.513]	[1.432]
Tertiary education	-0.202***	-0.206***	0.239***	0.343***
	(0.021)	(0.016)	(0.016)	(0.018)
	[0.817]	[0.814]	[1.27]	[1.409]
Observations	283,608	283,608	258,413	258,413
Year & Region FE	✓	✓	✓	✓
Age Group FE		✓		✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors (clustered by region) in parentheses. Hazard rates reported in square brackets. The estimation sample in columns (1) and (2) includes employed individuals (both full and part time). The estimation sample in columns (3) and (4) includes unemployed individuals. The omitted education level is "Lower secondary education without vocational training". *Data Source*: RDC of the Institute for Employment Research (IAB) at the Federal Employment Agency, SIAB SUF 7514, 1997–2002, own calculations.

of Germans is 11% lower than the rate for foreigners without age group dummies, see Column (3). Conditional on age, however, this difference is insignificant. In column (4), the hazard rate difference implies that Germans only have a 0.6% higher rate of transitioning into employment. We conclude that, conditional on age, the higher exposure of foreigners to unemployment risk is driven by an elevated job-loss hazard and not by a longer average unemployment duration.

It is worth noting that education has an non-linear effect on the hazard rates. Specifically, compared to an individual with lower secondary education and no vocational training (the reference category), basic secondary education and vocational training does not reduce the job loss risk. Individuals with a higher secondary degree even face higher job loss risks. Only university education is associated with an average job loss risk below the level of individuals with basic secondary education.

Women are roughly 7% less likely to become unemployed and about 11% less likely to move into employment compared to men in the specifications that include age effects. That is, women have on average longer employment durations, but it also takes them longer to find new jobs out of unemployment. From the estimated labor market hazard

rates alone, it is therefore not clear whether intermarriages in which the female is non-native are more exposed to unemployment risk than couples in which the male is non-native. We will get back to this question below.

Based on the evidence presented in this section, we focus our analysis on intermarriages. Exposure to unemployment risk clearly differs between native and non-native workers in the German labor market, even conditional on age, education, gender, region, and time effects. Despite the non-linear effect of education on the job-loss hazard rate, an alternative strategy would be to use education as a proxy for unemployment risk. Education often plays a prominent role in studies of marriage market matching (e.g., Greenwood et al., 2016; Chiappori et al., 2017). However, while the marriage register has the big advantage of completely covering the flow of new marriages, it does not contain information on the spouses' education, and it is legally prohibited to merge it with information from different sources at the individual level. This prevents us from using education to define partner types. Alternative German micro data sources that include education are not well-suited to study reform effects on marriage markets. The German Microcensus, which we use to calculate the single populations, does not include the year of marriage after 2004 and does not follow individuals over time. The German Socio-Economic Panel Study (GSOEP) follows households over time but attrition is likely as households form or dissolve. Thus, the marriage register is the best available data source to study the association of marriage and unemployment risk in the German context.

3 Marital Surplus

To investigate how changes of unemployment insurance generosity affect the gains from marriage across heterogeneous couples, we need to measure marital surplus. We rely on the non-parametric estimator that arises fr the frictionless marriage market matching model with transferable utility of Choo and Siow (2006), a workhorse model in family economics.[12] In this model, marital surplus reflects the gains from marriage for both partners, which depend on their observable types. These types are age and nationality in our application.[13]

[12]See, e.g., Choo (2015), Gayle and Shephard (2019), Mourifié (2019), and Galichon and Salanié (2021) for recent applications and extensions of this workhorse model in family economics.

[13]The model also incorporates unobserved heterogeneity, which affects marital surplus quasi-additively and is i.i.d. following a standard type I extreme value distribution. See Appendix A.2 for details.

A single cross section of data on the married and single populations suffices to compute marital surplus according to the static Choo and Siow (2006) model. In our empirical application, we study reform effects on marital surplus and are interested in changes of surplus over time. Therefore, we calculate marital surplus based on the flow of new marriages in every age-nationality cell relative to the respective single stocks (details in Section 4). Essentially, we measure the flow out of singlehood. This approach is well-suited to study reform effects on the marriage market, which are hard to detect based on the slow-moving married-population stock.[14]

Formally, the types are combinations of age (a) and nationality (n). We denote the type of males and females $m_{a,n}$ and $f_{a,n}$, respectively. The model yields the following non-parametric estimator for type-dependent marital surplus, which we derive in Appendix A.2:

$$\Phi\left(f_{a,n}, m_{a,n}\right)_t = \ln\left(\frac{\mu(f_{a,n}, m_{a,n})_t}{\sqrt{\mu(0, f_{a,n})_t \ \mu(m_{a,n}, 0)_t}}\right). \tag{1}$$

Marital surplus $\Phi\left(f_{a,n}, m_{a,n}\right)_t$ is determined by (the log of) the ratio of new marriages between type (a, n) males and females in year t and the geometric average of the respective single populations in the same year. Thus, $\mu(f_{a,n}, m_{a,n})_t \geq 0$, $\mu(m_{a,n}, 0)_t \geq 0$, and $\mu(0, f_{a,n})_t \geq 0$, denote the masses (numbers) of new marriages, single men, and single women. $\mu(f_{a,n}, m_{a,n})_t$ is also known as the marriage matching function in the literature.[15]

Intuitively, the denominator captures the number of potential matches. Thus, if the number of (new) marriages in the numerator is high relative to the denominator, estimated marital surplus will be high. Note that changes in the number of potential matches, i.e., changes in the underlying single populations are also taken into account through the denominator. For example, constant marital surplus implies that the number of new marriages adjusts proportionately to the underlying single populations, which may change over time.

Consider how the labor market reform we are interested in affects marital surplus in this model. The means-testing reform, Hartz I, affected couples but not singles. The reason is that singles do not have a partner whose income could be counted against benefit entitlements, so means testing does not affect the value of singlehood. For couples, stricter

[14] Our approach is similar to Adda et al. (2022), who estimate marital surplus according to the Choo and Siow (2006) model using Italian data.

[15] An alternative way of taking the Choo and Siow (2006) model to the data is to solve equation (1) for $\mu(f_{a,n}, m_{a,n})_t$ as a function of marital surplus and the single stocks (see, e.g., Chen et al., 2021).

means testing reduces the gains from marriage, and the extent of this reduction depends on couples' heterogeneous exposure to unemployment risk, which we proxy by nationality (Section 2). Therefore, the model predicts a substitution towards partners with lower exposure to unemployment risk, but, because singles are unaffected, not necessarily a changed number of marriages.[16] This is consistent with the development depicted in Figure 1, which shows that the overall marriage rate has been constant between 1997 and 2013. However, intermarriage rates plummet during times of high unemployment, suggesting that foreign partners are substituted with German partners when concerns about unemployment rise. The model rationalizes this development through a falling marital surplus of intermarriages relative to marriages among Germans.

Other elements of the Hartz reforms that affected, e.g., labor demand (Hartz I/II) and matching efficiency (Hartz III) treated, in principle, both couples and singles equally. The reason is that these policy changes applied to all individuals irrespective of their marital status. What we cannot rule out, however, is that, say, married individuals are over-represented in the treatment groups of certain reform elements, which could imply that these policy changes are more relevant for couples than for singles. We abstract from this possibility in the present paper because we do not observe the employment status in our primary data source, the marriage register, so we cannot condition marital surplus on the employment status.

The Hartz IV reform, which further reduced the generosity of the UI system and specifically the level of long-term benefits, potentially changed marital surplus. On the one hand, the insurance value of the partner's income rises if expected transfer income falls, so surplus could rise. On the other hand, only the gains of marriage for the spouse with the higher unemployment risk increase. The gains for the less-at-risk spouse, who now has to insure a more volatile income stream, fall, and this is reinforced by means testing.[17] Figure 1 suggests that the overall marriage rate has not changed around Hartz IV. Moreover, as we show below, the estimated marital surplus in the groups we consider either falls or stays flat. Thus, the theoretically possible positive effect of a UI generosity reduction on marital surplus appears negligible in our setting.

[16] Through the lens of the model, only the relative attractiveness of different partner types changes. See equation (A.3): after the reform, a different V_{ijg} delivers the highest attainable utility, but V_{i0g} and V_{0jg} remain unchanged.

[17] Note that Hartz IV tightened means testing further because long-term unemployment benefits were abolished and the lower social benefits do not have a means testing exemption.

Note also that in the empirical model developed below, any reform effect on selection into marriage that effects all nationalities will be captured by time fixed effects. Similarly, time-constant differences between natives and non-natives are captured by nationality fixed effects. Thus, our model captures all reform effects on intermarriages. Accordingly, a conservative interpretation of our results would be that we capture the total reform effect, and not just the effect of the Hartz I means testing reform, for which the matching model's predictions are clear-cut.

The EU expansion in 2004 is another event that potentially affected the marriage market. Before the expansion, marriage was one way to obtain the right to live and work in Germany. After the EU expansion, EU10 citizens obtained these right automatically (with initial restrictions). Thus, intermarriage became less attractive for EU10 citizens due to lower gains from marrying Germans. Additionally, their value of singlehood increased because the new rights were granted independently of marital status. Thus, through the lens of the model, the EU expansion also implies lower surplus and falling intermarriage rates, but this effect is reinforced by an increasing value of singlehood. Thus, we expect a bigger effect on marital surplus compared to the labor market reform, and potentially also an increase in the affected single population.

To sum up, the theory suggests that both the Hartz I labor market reform and the EU expansion had a negative effect on the surplus of intermarriages, and additionally, the EU expansion increased the value of singlehood for affected individuals. We test these implications empirically in Section 5, along with a number of robustness checks related to the construction of marital surplus, our model-based outcome variable.

4 Data

4.1 Marriage and Divorce Registers

The marriage and divorce registers, referred to as MR and DR in the following, cover all marriages and divorces in Germany. These registers originate from the German civil registry offices and divorce courts, respectively.[18] Both data sources contain information on legally registered marriages of different-sex couples. We have access to the registers for the periods 1991–2013 (MR) and 1995–2013 (DR). A few federal states did not report

[18]Data access is provided through the statistical offices of the German federal states.

data prior to 1997, so we discard earlier years. We clean the data by removing duplicates, observations where important variables are missing, and marriages formed outside Germany.[19] Moreover, we exclude marriages in which one of the individuals' birth date implies an age below 18. Both data sets are organized at the couple level and contain information on the birth dates of both spouses, the date of marriage, and, in the DR, the date of divorce. Additionally, the data contain various covariates including citizenship of both spouses, religion, place of residence, number of children (before marriage and at the time of divorce), who filed for divorce, and the ruling of the court. We do not observe education, income, or other indicators of socioeconomic status.

To estimate marital surplus based on the Choo and Siow (2006) model, we combine the flow of new marriages from the MR data with single stocks by nationality and age group from the German Microcensus (described below). We can merge these single stocks with the MR data only for cells in which the number of observations is sufficiently large. Thus, we use seven (groups of) nationalities: Germany, EU15 (excluding Germany), Poland, Turkey, EU10 (excluding Poland), former Yugoslavia, and "Rest of the World" (residual category). We use six age groups: 18–25, 26–32, 33–39, 40–46, 47–54, and 55–68.

German data protection legislation forbids merging the MR and DR registers at the level of the individual couple. To study marital stability in Section 6, we link both registers by counting observations in cells formed by the quarter of marriage and both spouses' nationalities. We merge both data sets at this level and "unpack" the linked data-set into individual marriage spells. This allows us to estimate the divorce hazard for different types of marriages formed before and after the law changes.

4.2 The German Microcensus

The German Microcensus (MC) is an annual representative survey that samples 1% of all persons legally residing in Germany.[20] We select all individuals between 18 and 68 years of age who live in private households. For the period after German reunification (1993–2013), this sample represents a roughly constant population of about 53 million

[19]Marriages formed outside Germany were not recorded before 2008 and represent only 0.15% of all marriages thereafter.

[20]Data access is provided by the research data centers of the statistical offices of the German federal states. The survey program of the MC consists of a set of core questions that remains the same in each wave, covering general demographic and socioeconomic characteristics like marital status, education, employment status, individual and household income, among many other things.

individuals, of which 47% are male.[21] 72% of men and 64% of women are married. To calculate the single stocks by age and nationality, we include all non-married individuals (never-married, divorced, widowed). The MC allows us to distinguish between non-married individuals with and without cohabiting partners, and we exclude cohabiting individuals from the analysis.

The implementation of the MC survey changed from a fixed reference week to continuous interviews over the course of the year in 2005. For the first couple of years, this led to irregularities in the sampling procedure. To make sure that our findings are not affected by this change, we interpolate single stocks for the years 2005–2009. Our findings do neither depend on whether or not we interpolate, nor on the specific technique used.[22]

We interpret Germany as one big marriage market and, thus, compute the single stocks at the national level. While there is substantial variation in the foreign population share across German regions, this strategy has two advantages. First, the sampling error in the MC is not amplified by extrapolating very small numbers of foreign individuals in some regions to the population level using weights. Second, we ensure that we have large enough numbers of observations to merge the MC and MR data without violating German data protection regulation.

4.3 Descriptive Evidence

Table 2 presents the distribution of nationalities in all new marriages between 1997 and 2013 for men and women, respectively. We observe a total of 6,626,086 marriages. Roughly 6 million of these marriages have at least one spouse with German nationality. The largest groups of non-Germans who get married in Germany are citizens of the other EU15 member states, Turkish men, and Polish women. Interestingly, the numbers of Turkish women and Polish men, respectively, are much smaller. For most nationalities, the foreign spouse is more often the wife. Exceptions are the EU15 countries and Turkey, for which the number of foreign husbands is higher. Marriages in which at least one spouse is from a different country ("Rest of the World") also make up a significant share of all observed marriages in Germany.

[21] Extrapolated from information on 8,426,756 surveyed individuals using sample weights. The average number of observations per wave is 443,513.

[22] See Appendix A.4 and Statistisches Bundesamt (2012) for details. For our baseline results, we rely on a piecewise cubic Hermite interpolation. Results for different interpolations are available upon request.

Table 2: Number of Marriages by Nationality and Gender

Nationality	Men	Women
German	6,090,937	5,978,703
EU15 (w/o Germany)	121,023	83,040
Poland	13,380	81,368
Turkey	100,981	55,487
EU10 (w/o Poland)	1,446	15,644
Former Yugoslavia	33,614	40,045
Rest of the World	264,705	371,799
Total	6,626,086	

Data Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register, 1997–2013. EU15 (w/o Germany) countries are Austria, Belgium, Denmark, Finland, France, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden and United Kingdom. EU10 (w/o Poland) countries are Cypress, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Slovakia and Slovenia.

Table 3 provides a closer look by showing numbers of observations, mean ages, and the mean age difference for all combinations of the four big (groups of) nationalities: German, EU15, Polish, and Turkish. Marriages in which both spouses are foreign citizens are relatively rare. They constitute less than 1% of the total number of marriages for the subsample in Table 3. 0.36% are marriages among Turks and 0.37% are marriages among EU15 citizens (not necessarily the same nationality).[23]

In 8.2% of all marriages, one spouse is German and the other spouse is a foreign citizen. This is the time average of the intermarriage rate in our sample. There are more marriages between German women and foreign men than there are between German men and foreign women. To accommodate the gender asymmetry in our empirical analysis, we later present results for marriages in which the German spouse is either the man or the woman separately, along with a pooled baseline sample.

Age differences between men and women are almost always positive, that is, the husband is usually older. Compared to German-German couples, the age difference is bigger when German men marry non-German women. In case the wife is Turkish, both spouses are significantly younger. Conversely, German women who marry non-German men are on average younger compared to German-German couples, and again much younger if the husband is Turkish. The only case with a (slightly) negative average age difference are couples of EU15 women and Polish men, but this is a very small group. The

[23]Due to the small number of marriages without any German spouse, and because marriages among foreign nationals may not show up in the German marriage registers (married abroad), we restrict our main analysis to marriages where at least one spouse is German.

Table 3: Marriage Characteristics by Nationality and Age

		Wife German	Wife EU15 (not German)	Wife Polish	Wife Turkish
Husband German	Share	93.79%	0.89%	1.18%	0.49%
	Mean Age Husband	35.82	36.18	37.40	30.34
	Mean Age Wife	32.91	33.21	31.08	26.37
	Difference	2.91	2.96	6.32	3.97
Husband EU15 (not German)	Share	1.38%	0.37%	0.04%	0.02%
	Mean Age Husband	35.89	30.92	35.62	30.16
	Mean Age Wife	32.87	27.83	28.90	26.54
	Difference	3.02	3.09	6.72	3.62
Husband Polish	Share	0.16%	0.00%	0.06%	0.00%
	Mean Age Husband	30.32	29.27	33.01	30.00
	Mean Age Wife	29.68	29.76	29.72	27.46
	Difference	0.64	-0.49	3.29	2.54
Husband Turkish	Share	1.19%	0.03%	0.02%	0.37%
	Mean Age Husband	27.94	27.23	32.46	27.17
	Mean Age Wife	27.79	26.24	27.55	24.51
	Difference	0.14	0.99	4.91	2.67

Data Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register, 1997–2013. Values rounded to two decimal places. Total number of observations in the table is 5,957,349.

largest average age differences exist between Polish women and German or EU15 men. In these marriages, the woman is on average more than 6 years younger than the man. This is more than twice the average age gap in German-German couples. To take into account the differences in the age structure across different couple types, our regression models include fixed effects for both the wife's and the husband's age group.

5 Reform Effects on Marital Surplus

5.1 (Pre-)Trends of Marital Surplus

Figure 2 depicts how the estimated marital surplus according to equation (1), denoted $\hat{\Phi}$, changes over time. We plot the surplus for marriages where at least one spouse, either the wife or the husband, is German, and aggregate nationalities into four groups: German-German marriages (black), German-EU15 marriages (blue), German-EU10 marriages (orange), and German-Other marriages (gray) in which the non-German spouse has any of the remaining nationalities (Turkey, former Yugoslavia, Rest of the World). Figure A.1 in the appendix shows two versions of the same plot in which we condition on the gender of the German spouse.

The visible ranking of marital surplus for different couples reflects, according to the

model, differences in the gains from marriage. On the one hand, factors like cultural distance or (gender-specific) preferences tend to lower marital surplus relative to German-German couples.[24] On the other hand, if access to the labor market is gained by marrying a German citizen, marital surplus tends to be higher, see the surplus difference between German-EU15 and German-EU10 marriages before the EU expansion. Interestingly, this is driven by German husbands with EU-10/other wives, see Figure A.1. Over time, as EU10 citizens earned the right to live and (later) work in Germany, the surplus converged and eventually the ranking even changed.[25] Although the surplus falls for marriages with both EU10 and "other" spouses after 2003—according to our main hypothesis as a result of the labor market reform—the "other" line remains above the EU15 line. This is consistent with the idea that spouses from non-EU countries still earn labor market access by marrying a German citizen and thus enjoy higher gains from marriage.

From 1997 until the implementation of the Hartz I labor market reform (black dashed line), the marital surplus evolves in parallel for all nationality combinations and is essentially flat. After 2003, the trends notably diverge. While the surplus for German-German and German-EU15 marriages remains flat, we observe a decline for marriages in which one spouse has EU10 or "other" citizenship. Recall that according to our model, changes in marital surplus reflect deviations from a constant relationship between the single populations and the flow of new marriages. The falling surplus we observe for German-EU10 and German-Other marriages therefore reflects "too few" new marriages relative to the single stocks in the respective groups, which is consistent with the falling intermarriage rates shown in Figure 1.[26]

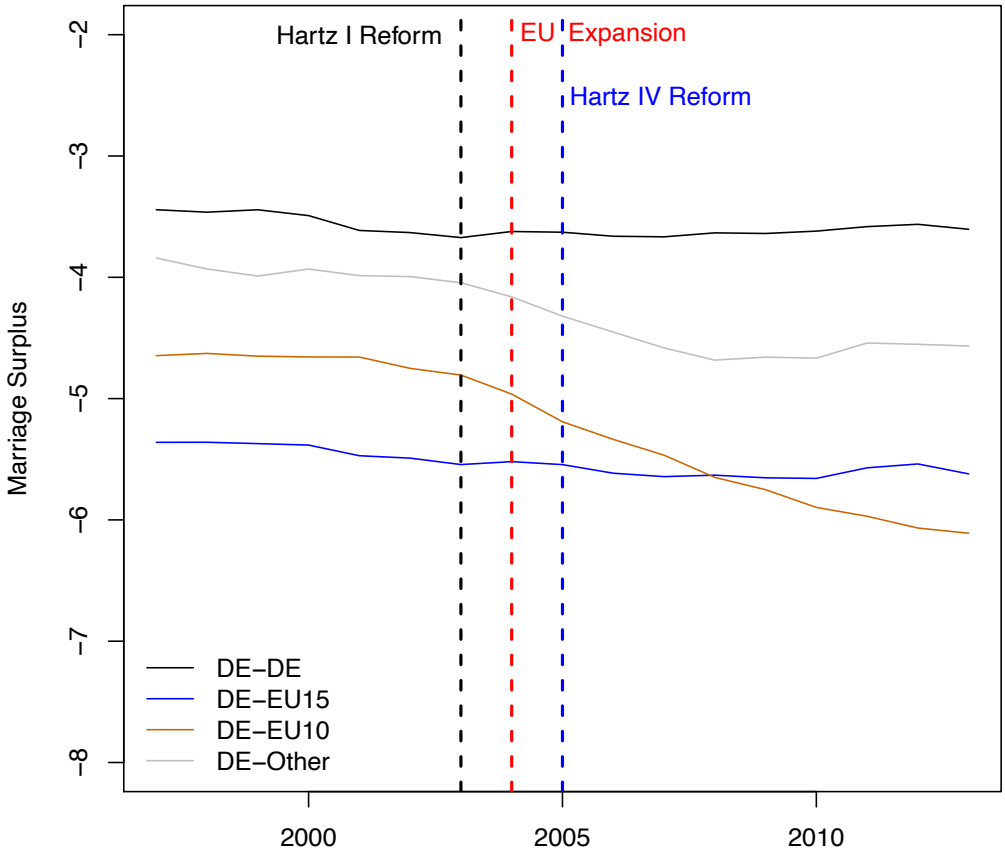
The lack of new marriages in these groups after the reform is, according to our main hypothesis, due to the relatively high unemployment risk that these households face. Following the tightening of the means-testing regulations, marriages in which one spouse had a foreign nationality and, thus, on average a higher unemployment risk (see Section 2), required more insurance from the partner and, thus, became less attractive.

[24] In Section 5.3, we control for such time-invariant differences by using nationality fixed effects.

[25] Again, this is driven by marriages between German men and EU-10 women. The marital surplus for German women and EU-10 men is the lowest overall, see Figure A.1 (a).

[26] It is conceivable that fewer new marriages in the respective groups reflect a substitution of marriage with cohabitation. To check this, we calculate the cohabitation rate, which we define as the population share of individuals who are unmarried and cohabit with their partner (with or without children), using MC data. The slow but steady trend towards more cohabitation is in fact interrupted in 2004 and 2005 and the rate even falls by 0.1 percentage points in 2006. This is inconsistent with substitution.

Figure 2: Development of Marital Surplus ($\hat{\Phi}$) over Time



Notes: Marriage surplus for marriages where at least one spouse is German by nationality of the non-German spouse. Single stocks based on piecewise cubic Hermite interpolation. The black dashed vertical line indicated the year in which the Hartz I and IV reforms became effective, the red dashed vertical line marks the year 2004 in which the EU expansion took place. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register and Microcensus, 1997–2013, own calculations.

In the aftermath of the Hartz I reform, the negative trend in marital surplus for German-EU10 and German-Other marriages appears to be unaffected by the EU expansion (red dashed line) and the Hartz IV reform (blue dashed line). The surplus of German-German and German-EU15 marriages remains flat around the same two law changes. After 2008, the German-Other surplus stabilizes while the German-EU10 continues to fall. This divergence can be explained by the fact that EU10 citizens gradually gained labor market access in Germany while citizens from "other" (i.e. third) countries still needed a German spouse to be allowed to work.

5.2 Empirical Setup

We are now in a position to estimate the effect of the Hartz I labor market reform on marital surplus. We use a differences-in-differences specification to identify the effect of

Table 4: Types of Marriages in Treatment and Control Groups

Nationalities of Spouses (c_h, c_w)	Hartz Treatment (Jan 01, 2003)	EU Treatment (May 01, 2004)	Treatment Dummy $Treat_{HartzI}$	$Treat_{EU}$
German-German	No	No	$= 0$	$= 0$
German-EU15	No	No	$= 0$	$= 0$
German-EU10	Yes	Yes	$= 1$	$= 1$
German-Other	Yes	No	$= 1$	$= 0$

the reform on the treated population. We restrict attention to marriages in which at least one spouse is German and define treatment and control groups as illustrated in Table 4. In line with the trends presented in Figure 2, German-German and German-EU15 marriages are the control group. We verify the composition of the control group in Section 5.4. All other intermarriages form the treatment group for estimating the labor market reform effect. We are able to separately identify the effects of the labor market reform and the EU expansion because couples with an EU10-spouse were treated by both reforms while couples in which the spouse has another foreign nationality (i.e. not EU10 or EU15) were treated by the labor market reform only.

To capture the labor market reform effect, we define a dummy variable $Treat_{HartzI}$ that takes on the value 1 for marriages where the non-native partner has one of the following citizenships: EU10, Turkish, former Yugoslavia, Rest of the World. The indicator function $\mathbb{1}\{t \geq 2003\}$ returns the value 1 for marriages formed after January 1 2003, the enactment date of the reform. It follows that our empirical specification to estimate the effect of the labor market reform has the following form:

$$\begin{aligned}
\hat{\Phi}_t(n_m, n_f, a_m, a_f) &= \beta_1 \cdot Treat_{HartzI}(n_m, n_f) + \beta_2 \cdot \mathbb{1}\{t \geq 2003\} \\
&+ \beta_3 \cdot Treat_{HartzI}(n_m, n_f) \cdot \mathbb{1}\{t \geq 2003\} \\
&+ \beta_4 \cdot Treat_{EU}(n_m, n_f) + \beta_5 \cdot \mathbb{1}\{t \geq 2004\} \\
&+ \beta_6 \cdot Treat_{EU}(n_m, n_f) \cdot \mathbb{1}\{t \geq 2004\} \\
&+ \eta_t + \delta_c + u_t(n_m, n_f, a_m, a_f), \qquad (2)
\end{aligned}$$

where one coefficient of interest is β_3. It represents the treatment effect on the treated of the Hartz I labor market reform. n_m and n_f are the nationality of the male (husband) and female (wife). a_m and a_f are the age of the male (husband) and female (wife). The year

fixed effect, η_t, controls for time trends. The fixed effect for the foreign spouse's nationality, δ_c, controls for any confounding factors specific to intermarriages with particular nationalities. This takes care of any unobserved time-invariant determinants of marital surplus. The outcome, $\hat{\Phi}_t(n_m, n_f, a_m, a_f)$, is the marital surplus for a particular combination of age and country of origin for both partners in year t. In all regressions, we also include the effect of the EU expansion in 2004. The treatment dummy $Treat_{EU}(n_m, n_f)$ takes on the value 1 for marriages in which the non-native partner has EU10 citizenship. The interaction $Treat_{EU}(n_m, n_f) \cdot \mathbb{1}\{t \geq 2004\}$ captures the treatment effect on the treated of the EU expansion and β_6 is the respective coefficient of interest.[27] Lastly, $u_t(n_m, n_f, a_m, a_f)$ is the residual. We estimate equation (2) by weighted least squares (WLS) and use the observation numbers per age-nationality cells as weights.

5.3 Main Results

We present estimation results for multiple specifications in Table 5. Columns (1) and (2) include all marriages where at least one spouse is German. Columns (3) and (4) condition on the husband being German and columns (5) and (6) condition on the wife being German, respectively. Columns (1), (3) and (5) include fixed effects for the year and the nationality of the non-German spouse, so these specifications correspond exactly to equation (2). The specifications that lead to the results shown in columns (2), (4), and (6) additionally include fixed effects for the age (group) of both spouses.

Overall, the labor market reform had a significant and sizable negative effect on the surplus of intermarriages in which the foreign spouse has a non-EU15 citizenship. The estimated coefficient $\hat{\beta}_3$ is negative and highly significant in all specifications. Robust standard errors are reported in parenthesis.[28] Specification (1) finds a 0.323 log points decrease in the surplus of treated marriages. That is, relative to the 1997–2002 average for marriages between a German and a EU10 (other) spouse marital surplus decreased by 4.2% (5.2%). Including age fixed effects for husband and wife in specification (2) leads to a slightly larger decrease of 0.410 log points or 5.2% (EU10) and 6.6% (other).

When we condition the estimation on either the wife or the husband being German,

[27] We focus on the 2004 EU expansion. Romania joined the EU later and is thus not in the treatment group but in the "Rest of World"-category.

[28] Clustered standard errors (by year, unreported) do not affect the significance of our estimated coefficients. To interpret our findings conservatively, we report the larger robust standard errors throughout the paper.

Table 5: Reform Effects on Marital Surplus

Dependent Variable	Marital Surplus ($\hat{\Phi}$)					
	All Marriages		German Husband		German Wife	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	-0.323^{**}	-0.410^{***}	-0.321^{**}	-0.398^{***}	-0.337^{*}	-0.459^{***}
	(0.146)	(0.081)	(0.150)	(0.076)	(0.178)	(0.127)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	-0.556^{***}	-0.492^{***}	-0.579^{***}	-0.526^{***}	-0.607^{**}	-0.632^{***}
	(0.135)	(0.102)	(0.138)	(0.099)	(0.242)	(0.224)
Constant	-3.477^{***}	-3.526^{***}	-3.482^{***}	-3.601^{***}	-3.470^{***}	-3.524^{***}
	(0.260)	(0.156)	(0.278)	(0.178)	(0.276)	(0.166)
	Controls					
Year, Nation FE	✓	✓	✓	✓	✓	✓
Age FEs		✓		✓		✓
Observations	6,731	6,731	3,704	3,704	3,635	3,635

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Observations are weighted according to the number of observed marriages per cell. Single stocks based on piecewise cubic Hermite interpolation. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register and Microcensus, 1997–2013, own calculations.

we see that the negative effects are bigger for marriages with German wives as compared to German husbands. We find a maximum decline of 0.459 log points in specification (6), which corresponds to a surplus reduction of 7.6% (EU10) or 7.3% (other). In specification (4), the negative impact is 0.398 log-points or 6.4% (EU10) and 8.0% (other). One possible explanation for the asymmetric impact across genders could be that marriages in which the husband is more exposed to labor market risk are generally more vulnerable. Labor force participation and income is on average lower for women in Germany, which is at least partly due to strong and persistent gender norms (Bauernschuster and Rainer, 2012; Lippmann et al., 2019).

Overall, we find that the Hartz I reform significantly reduced the surplus, and, thus, the relative attractiveness of intermarriage in Germany. Note that, under the assumptions of the Choo and Siow (2006) model, our estimates represent causal effects. Our hypothesis that the Hartz I labor market reform had significant repercussions in the marriage market is confirmed, and this is of interest for at least two reasons. First, it is conceivable that policy-makers did not intend to affect the marriage market with a reform that was primarily designed to reduce unemployment. Second, intermarriages are often viewed as a vehicle for the integration of ethnic minorities and immigrants (Azzolini and Guetto, 2017; Adda et al., 2022). Living with natives can improve labor market access, e.g., by providing additional incentives to learn the language or through access to labor market

networks. By negatively affecting intermarriage rates, the labor market reform potentially hampered the integration of the foreign-born population in Germany.

Next, we turn to the effect of the EU expansion on marital surplus. To see how we capture it, recall Table 4 and Equation (2): we compare intermarriages in which the non-native spouse is from a country that joined the EU in 2004 (EU10) with intermarriages in which the non-native spouse is from a country unaffected by the EU expansion (Turkey, former Yugoslavia, Rest of the World). Thus, the treatment dummy $Treat_{EU}(n_m, n_f)$ takes on the value 1 for marriages in which the non-native partner has EU10 citizenship. The interaction $Treat_{EU}(n_m, n_f) \cdot \mathbb{1}\{t \geq 2004\}$ captures the treatment effect on the treated of the EU expansion and β_6 is the respective coefficient of interest.[29]

Our estimates of β_6 are included in Table 5, again separately for all marriages, marriages with German husbands, and marriages with German wives. In line with our theoretical prediction and similar to the Italian case discussed in Adda et al. (2022), we find negative and significant effects of the EU expansion on the marital surplus of German-EU10 marriages. Similar to the labor market reform, the effect is larger for intermarriages with German wives. This suggests that the right to live and work in Germany was valued more highly by males from EU10 countries than by females from the same countries prior to the expansion. The point estimates are larger than those for the labor market reform. This confirms our conjecture that the EU expansion had a larger negative effect on the marital surplus because the value of singlehood increased. Recall that the means testing reform did not affect the value of singlehood.

Our finding that the EU expansion affected the German marriage market corroborates the results of Adda et al. (2022) for Italy. Germany has a different institutional background and migration history. First, Germany initially restricted labor market access for citizens of the new member states. Second, Germany has been receiving migrants longer than Italy[30], and intermarriage is relatively common.[31] Still, the EU expansion has affected the marriage market in similar ways in both countries.

[29]Note that we define the indicator function $\mathbb{1}\{t \geq 2004\}$ such that it returns 1 for all marriages formed after January 1 2004 although the new member states joined the EU only on May 1 2004. This is necessary because MC single stocks are only available on an annual basis.

[30]According to Adda et al. (2022), the share of foreign residents in Italy was below 2% during the 1990s and only started increasing in the 2000s. It reached around 9% in 2013. In contrast, migrants have been flowing into Germany since the 1950s/60s. The share of residents without German citizenship was stable at around 8–9% of the population during the period we study (Federal Statistical Office).

[31]In 1997, about 10% of all marriages were intermarriages. In contrast, Adda et al. (2022) report intermarriage rates of below 3% for Italian men and around 1% for Italian women in 1996.

5.4 Robustness Checks

We argue that the reduction of marital surplus reflects fewer marriages between Germans and foreigners (recall Figure 1). We ascribe this trend to, on the one hand, higher exposure to unemployment risk when faced with stricter means testing and, on the other hand, more rights for EU10 citizens after the expansion. However, according to the Choo and Siow (2006) model, the flow of marriages can only be interpreted relative to the number of available singles.[32] Therefore, we scrutinize further the role that the single stocks play for our findings. First, we check how restrictive it is to compute the marital surplus based on the contemporaneous single stocks, which is what the static Choo and Siow (2006) model suggests. Second, we analyze to what extent the single stocks have changed over time. Third, we revisit the composition of our treatment and control groups.

In the static Choo and Siow (2006) model, only the contemporaneous single stocks matter for the marital surplus. In reality, however, partnership formation takes time. Individuals often live together for years before getting formally married. Moreover, we use the flow of new marriages to construct marital surplus, which could in principle depend on the available singles in previous periods. Thus, an observed marriage in a given period could depend on decisions made earlier, and at this earlier point in time the availability of potential partners may have been different. To evaluate whether our results are sensitive to the way the marital surplus is specified, we recalculate the surplus based on single stocks from up to three years earlier, and then re-estimate our main specifications. The results are presented in Table 6.

Panels A, B, and C show results where we replace the year t single stocks $\mu(m_{a,n}, 0)_t$ and $\mu(0, f_{a,n})_t$ with the respective values for $t-1$, $t-2$, and $t-3$. Columns (1), (4), and (7) are directly comparable to our baseline specification with age dummies in Table 5. Reassuringly, effect sizes and significance levels remain fairly unaffected by the change. The gender differences discussed in the context of Table 5 are no longer visible, but overall none of our substantive conclusions change when using lagged single stocks. In columns (2), (5) and (8), we additionally add the contemporaneous single stock for both genders as a control. In this case, the effect is again larger for intermarriages with German wives. Adding the lagged single stock (same lag as used for the construction of

[32] Ceteris paribus, an increase in the number of available singles implies a proportionate increase in the number of marriages. A lack of new marriages for a given single stock implies a deviation from the constant relationship and, therefore, falling marital surplus.

Table 6: Robustness: Single Stocks

Dependent Variable	Marital Surplus ($\hat{\Phi}$)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A:	Lagged (1 year) Single Stocks								
	All Marriages			German Husband			German Wife		
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	−0.415***	−0.467***	−0.455***	−0.426***	−0.333***	−0.307***	−0.425***	−0.492***	−0.487***
	(0.085)	(0.097)	(0.097)	(0.080)	(0.115)	(0.117)	(0.136)	(0.143)	(0.141)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	−0.483***	−0.505***	−0.491***	−0.498***	−0.437***	−0.435***	−0.662***	−0.785***	−0.766***
	(0.107)	(0.109)	(0.108)	(0.104)	(0.114)	(0.110)	(0.236)	(0.236)	(0.235)
Panel B:	Lagged (2 years) Single Stocks								
	All Marriages			German Husband			German Wife		
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	−0.416***	−0.463***	−0.420***	−0.430***	−0.350***	−0.291**	−0.413***	−0.471***	−0.450***
	(0.092)	(0.104)	(0.103)	(0.087)	(0.120)	(0.117)	(0.151)	(0.158)	(0.155)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	−0.494***	−0.518***	−0.476***	−0.509***	−0.456***	−0.418***	−0.698***	−0.800***	−0.761***
	(0.114)	(0.115)	(0.114)	(0.111)	(0.121)	(0.117)	(0.253)	(0.253)	(0.252)
Panel C:	Lagged (3 years) Single Stocks								
	All Marriages			German Husband			German Wife		
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	−0.443***	−0.495***	−0.410***	−0.455***	−0.394***	−0.293**	−0.436**	−0.493***	−0.449**
	(0.103)	(0.114)	(0.113)	(0.097)	(0.127)	(0.120)	(0.173)	(0.181)	(0.177)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	−0.453***	−0.491***	−0.411***	−0.474***	−0.428***	−0.383***	−0.661**	−0.765***	−0.681**
	(0.124)	(0.125)	(0.121)	(0.122)	(0.131)	(0.122)	(0.274)	(0.276)	(0.271)
				Single Stock Controls					
Current (both)		✓			✓			✓	
Lagged (both)			✓			✓			✓
Year, Nation, Age FE (All Panels)	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations (Panel A)	6,319	6,319	6,319	3,477	3,477	3,477	3,415	3,415	3,415
Observations (Panel B)	5,908	5,908	5,908	3,253	3,253	3,253	3,193	3,193	3,193
Observations (Panel C)	5,500	5,500	5,500	3,028	3,028	3,028	2,974	2,974	2,974

Notes: Robust standard errors in parentheses. Observations are weighted according to the number of observed marriages per cell. Single stocks based on piecewise cubic Hermite interpolation. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register and Microcensus, 1997–2013, own calculations.

the marital surplus) as a control instead does not change the picture, see columns (3), (6), and (9). Overall, these alternative specifications do not challenge our results. We find these consistent and significant patterns reassuring with respect to the conclusions we have drawn so far.

Next, we consider the time dynamics of the single stocks. Our main finding, the reduced marital surplus for intermarriages, would also be consistent with an increasing numbers of singles in the same groups and a constant flow of (inter)marriages. The EU expansion is one reason to expect increased migration flows into Germany, that is, the number of non-German singles could have increased. We check whether the single stocks responded to either the EU expansion or the labor market reform by repeating our regression analysis with the single stocks instead of the marital surplus as the outcome variable. The results are presented in Table A.2, along with further details on these specifications.

Reassuringly, the single stocks have not changed systematically in response to either of the reforms. Specifically, the EU expansion did not lead to more singles in the EU10

group relative to the untreated nationalities. The point estimate for the labor market reform is larger and, as one might expect, negative. But statistically it is indistinguishable from zero. Moreover, the included time dummies do not suggest a general trend in the single stocks. Overall, these results are consistent with the flat overall marriage rate (recall Figure 1) and the aforementioned fact that Germany had a sizable but stable migrant population during the period we consider. We conclude that our main results are not driven by the time dynamics of the single stocks.

As a final robustness check, we revisit the composition of our treatment and control groups. In the main analysis, we use marriages formed between two Germans and Germans with members of an EU15 country as the control group. This choice is supported by the trends in Figure 2. Moreover, from a legal perspective, employers are not allowed to discriminate between native Germans and members of the EU15 countries, which might explain why the attractiveness of EU15 partners has not been negatively affected by the labor market reform. Still, the EU15 group includes a diverse group of foreigners and, thus, could mask important heterogeneity.

To open this black box, we exploit differences between the German language and the languages spoken in the remaining EU15 countries. The idea is that speaking a Germanic language facilitates labor market access for foreign-born individuals (Dustmann, 2003; Aldashev et al., 2009; Wong, 2023). Thus, it could lower the exposure to unemployment risk and make individuals from countries with Germanic languages more attractive from the risk-sharing perspective. To operationalize this idea in the data, we separate the EU15 countries into "linguistically close" (Belgium, Denmark, Luxembourg, Netherlands, Austria, Sweden) and "linguistically distant" (Finland, France, Greece, Ireland, Italy, Portugal, Spain, United Kingdom) countries relative to Germany.

Figure A.2 in the Appendix shows the development of marital surplus between Germans and EU15 nationals when the EU15 group is separated by linguistic distance.[33] As before, German-EU10 and German-Other marriages experience a fall in surplus after 2003 but the surplus of German-German, German-EU15 (close) and German-EU15 (distant)

[33]Interestingly, the marriage surplus of DE-EU10 marriages converges to the surplus of DE-EU15 (close) marriages over time (as the initial labor market restrictions for EU10 citizens become less binding) Thus, in terms of marital surplus with a German citizen, EU10 nationals are more comparable to EU15 (close) than to EU-15 (distant) citizens. This can be explained with the close historic ties between Germany and the Eastern European EU10 countries, for example due to the influence of the Prussian and Austro-Hungarian Empires in the 18th and 19th centuries.

marriages remains stable over time. One could have suspected that marriages in which the non-German spouse is from a EU15 (distant) country are also (partly) treated due to, on average, lower language skills and labor market attachment. This does not appear to be the case, and this validates our decision to include both German-German and all German-EU15 marriages in the control group.

To further investigate the language channel, we repeat our main analysis with four different sets of treatment and control groups. First, we re-estimate our baseline model using EU15 (close) and EU15 (distant) as two separate control groups. Given that we use weighted (by the number of marriages per cell) OLS, the results should be unaffected by this. Indeed, the results reported in Panel A of Table 7 are virtually identical to our baseline results. Next, we estimate the model using German-German and German-EU15 (close) marriages as the only control group.[34] The coefficients for the labor market reform effect are reported in Panel B of Table 7. They decrease in size but remain significant and quantitatively important throughout all but one specification. We also test the counter-intuitive case in which only German-German and German-EU15 (distant) marriages are the control group (Panel C). Again, we get very similar and significant estimates. Lastly, we restrict the sample to include German-German, German-EU15 (close), and German-EU15 (distant) marriages only. We estimate the effect of interest using German-German marriages as the only control group. Essentially, this is a falsification test. If we did find significant effects, there would be significant treatment differences within the control group of the baseline specification. Reassuringly, the estimated coefficients become small and insignificant, see Panel D of Table 7.

6 Reform Effects on Marital Stability

In the final step of the analysis, we use the German divorce register (DR) to compare the stability of marriages formed before and after the two law changes. As explained in Section 4, we combine the marriage and divorce registers at the quarter of marriage-nationality-nationality level to study the survival of different types of marriages.

Our results so far show that the declining marital surplus after the labor market reform is a reflection of fewer new intermarriages. We conjecture that the remaining

[34] A detailed overview over the treatment and control groups we use for this exercise is provided in Table A.3 in the Appendix.

Table 7: Labor Market Reform Effects with Language Distance Separation

Dependent Variable	Marital Surplus ($\hat{\Phi}$)					
Panel A:	Baseline Results with Linguistic Distance Separation					
	All Marriages		German Husband		German Wife	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	-0.323^{**}	-0.410^{***}	-0.321^{**}	-0.398^{***}	-0.337^{*}	-0.459^{***}
	(0.146)	(0.081)	(0.150)	(0.076)	(0.178)	(0.127)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	-0.556^{***}	-0.492^{***}	-0.579^{***}	-0.526^{***}	-0.607^{**}	-0.632^{***}
	(0.135)	(0.102)	(0.138)	(0.099)	(0.242)	(0.224)
Panel B:	Control: German-German & German-linguistic close					
	All Marriages		German Husband		German Wife	
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	-0.281^{*}	-0.361^{***}	-0.297^{**}	-0.365^{***}	-0.274	-0.386^{***}
	(0.146)	(0.078)	(0.149)	(0.074)	(0.169)	(0.113)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	-0.592^{***}	-0.535^{***}	-0.600^{***}	-0.554^{***}	-0.659^{***}	-0.694^{***}
	(0.132)	(0.100)	(0.136)	(0.097)	(0.236)	(0.218)
Panel C:	Control: German-German & German-linguistic distant					
	All Marriages		German Husband		German Wife	
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	-0.313^{**}	-0.391^{***}	-0.316^{**}	-0.385^{***}	-0.320^{*}	-0.428^{***}
	(0.146)	(0.079)	(0.149)	(0.075)	(0.173)	(0.121)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	-0.565^{***}	-0.510^{***}	-0.584^{***}	-0.537^{***}	-0.621^{***}	-0.659^{***}
	(0.134)	(0.101)	(0.137)	(0.098)	(0.239)	(0.221)
Panel D:	Comparing only German and EU15 Marriages (Falsification Test)					
	All Marriages		German Husband		German Wife	
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	-0.026	-0.021	-0.084	-0.041	0.005	-0.015
	(0.155)	(0.072)	(0.170)	(0.079)	(0.169)	(0.078)
	Controls (All Panels)					
Year, Nation FE	✓	✓	✓	✓	✓	✓
Age FEs		✓		✓		✓
Observations (Panels A, B & C)	7,692	7,692	4,171	4,171	4,129	4,129
Observations (Panel D)	2,640	2,640	1,598	1,598	1,650	1,650

Notes: Robust standard errors in parentheses. Observations are weighted according to the number of observed marriages per cell. Single stocks based on piecewise cubic hermite interpolation. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register and Microcensus, 1997–2013, own calculations.

intermarriages—the ones that are formed after 2003 despite the reforms—are positively selected compared to intermarriages formed before the reform. The reason is that these couples were aware of the reduced generosity of the unemployment insurance system when they got married, while pre-reform couples based their decision to get married on a more generous UI system. Thus, we expect that post-reform marriages are more stable, i.e., have a lower divorce probability. One reason could be a higher ability to absorb economic shocks—precisely because post-reform couples expected to insure each other against income shocks at the time of marriage.

Table 8: Divorce Hazard - Diff-in-Diff Estimates

Dependent Variable	Duration until Divorce								
	All Marriages			German Husband			German Wife		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	0.089***	−0.306***	−0.456***	−0.085***	−0.388***	−0.453***	0.178***	−0.280***	−0.475***
	(0.010)	(0.009)	(0.010)	(0.016)	(0.016)	(0.016)	(0.012)	(0.012)	(0.012)
	[1.093]	[0.736]	[0.634]	[0.919]	[0.678]	[0.636]	[1.195]	[0.756]	[0.622]
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$	−0.307***	−0.595***	−0.486***	−0.208***	−0.646***	−0.562***	−0.022	−0.163***	−0.211***
	(0.024)	(0.024)	(0.024)	(0.029)	(0.029)	(0.028)	(0.051)	(0.051)	(0.051)
	[0.736]	[0.552]	[0.615]	[0.812]	[0.524]	[0.570]	[0.978]	[0.850]	[0.810]
Divorce Year FE		✓			✓			✓	
Divorce Year Strat.			✓			✓			✓
Observations	6,592,292	6,592,292	6,592,292	6,417,362	6,417,362	6,417,362	6,431,657	6,431,657	6,431,657

Notes: Robust standard errors in parentheses. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage and Divorce Registers, 1997–2013, own calculations.

For the EU expansion, the expected effect on marital stability goes in the same direction but the mechanism is different. For German-EU10 marriages formed after the expansion, gaining the right to live and work in Germany is no longer part of the surplus. Couples that form despite this negative surplus change are likely positively selected. This effect is reinforced by the increased value of singlehood for EU10 citizens in Germany.

To test these conjectures, we re-apply our differences-in-differences estimation strategy in a Cox proportional-hazard framework (Cox, 1972). In our application, the baseline hazard is that of a marriage of two individuals who are both neither affected by the labor market reform, nor the EU expansion. As before, this applies to marriages between natives and citizens of EU15 member states. The coefficients of interest are again the ones associated with the treatment dummy interactions $Treat_{HartzI}(n_m, n_f) \cdot \mathbb{1}\{t \geq 2003\}$ for the labor market reform and $Treat_{EU}(n_m, n_f) \cdot \mathbb{1}\{t \geq 2004\}$ for the EU expansion. That is, we compare the stability of marriages in which one partner is of a treated nationality before and after the respective law change.

We either stratify by divorce year or include fixed effects to control for influences specific to the year of divorce. When stratifying by divorce year, one allows for different baseline hazards for every single divorce year. This is tantamount to assuming that all divorcing couples in a given year are exposed to the same environment, e.g. the same aggregate labor market situation and legal framework.[35]

The results are presented in Table 8, separately for all marriages, marriages with German husbands, and marriages with German wives. Column (1) shows the results in

[35]In contrast, stratification by marriage year would assume that all couples married in a given year face the same baseline hazard, which seems hard to defend.

the full sample without taking divorce year effects into account. The estimated coefficient of $Treat_{HartzI}(c_h, c_w) \cdot \mathbb{1}\{t \geq 2003\}$ indicates that the divorce hazard increased by 9.3% for marriages treated by the labor market reform. This would suggest that the labor market reform lowered marital stability, which is not in line with the expected selection effect. However, the sign of the effect flips in columns (2) and (3) where divorce year effects are taken into account. In both specifications, we find significant and sizable negative effects of the labor market reform on the divorce hazard that range from 26.4% to 36.6% relative to the baseline. In other words, marriages with one spouse from a non-EU15 country became more stable after the reform, in line with positive selection. We confirm the same trends for the sub-samples of marriages where the husband is German and where the wife is German. There is always a large reduction of the divorce hazards once we control for year fixed effects or stratify by divorce year. We see no clear difference in the effect sizes for couples with German husbands and wives in this case.

The EU expansion had a further stabilizing effect on the (remaining) marriages between Germans and citizens of the new member states. The effect is slightly larger than the effect of the Hartz I labor market reform, which is again in line with the additional effect through the value of singlehood. The effect of the EU expansion is larger for marriages with German husbands as compared to German wives. This might be due to the fact that marriages between German women and EU10 men are relatively rare. Interestingly, the effect of the labor market reform is substantially larger than the effect of the EU expansion for intermarriages with German wives. This can be rationalized with a male-breadwinner norm. In this case, the labor market reform should have a stronger effect on the partner selection of women. Thus, intermarriages with German wives that formed despite the labor market reform are likely to be particularly well-selected.

7 Conclusion

In this paper, we empirically investigate the importance of within-household insurance for marriage formation and stability. Exploiting a sharp generosity reduction in the German unemployment insurance system—stricter means testing, which started with the Hartz I reform in 2003—we find that marriages in which one partner had an elevated unemployment risk, proxied by nationality, became significantly less attractive. Provided

that both our identifying assumption linking unemployment risk to nationality and the assumptions underlying the Choo and Siow (2006) model hold, the estimated reform effect on marital surplus can be interpreted as causal.

Furthermore, we provide external validity to the study by Adda et al. (2022), who investigate the effect of the EU expansion in Italy. Even in a different institutional setting and conditional on the earlier labor market reform, we find a significant negative effect of the EU expansion on marital surplus for the affected nationalities. However, the EU expansion only affected a fraction of Germany's relatively large and diverse migrant population. Overall, the labor market reform had a larger impact. Moreover, we find that intermarriages formed after the two reforms are significantly more stable than those formed before. Our interpretation is that the law changes resulted in fewer, but better selected intermarriages.

The significant and quantitatively important negative effect on the marital surplus of intermarriages in Germany is a finding of high policy relevance. The marriage market ramifications of the labor market reform were probably not intended by the policy-maker. Moreover, intermarriage is often seen as an indicator for the successful integration of migrants. Social security reforms that make intermarriage less attractive may therefore interfere with the integration of migrants and have negative long-run effects.

Declarations

The content of this paper is solely the responsibility of the authors and does not necessarily represent the views of the institutions providing funding or data access.

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Data Availability: The empirical analysis in this paper is based on German register data, which are not publicly available. Interested researchers can get access to the data used in this paper through the Research Data Centres of the German Federal States and the German Federal Employment Agency. The paper includes a detailed description of the data and our cleaning procedures. Programs are available upon request.

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A Appendix

A.1 Additional Descriptive Results

Table A.1: Education Distribution

Educational Level	Lower secondary		Higher secondary		Tertiary
Vocational training	No	Yes	No	Yes	–
Panel A (Sample of Employed Individuals)					
German Men	10.8%	73.3%	1.2%	5.9%	8.8%
Foreign Men	38.1%	52.0%	2.0%	3.7%	4.3%
German Women	14.5%	66.9%	1.4%	8.4%	8.8%
Foreign Women	46.1%	40.3%	2.6%	5.4%	5.6%
Panel B (Sample of Unemployed Individuals)					
German Men	11.3%	72.9%	1.4%	6.6%	7.9%
Foreign Men	37.8%	52.7%	2.1%	4.3%	3.2%
German Women	14.1%	66.0%	1.4%	9.5%	8.9%
Foreign Women	44.6%	40.9%	2.6%	6.8%	5.1%

Notes: Lower secondary corresponds to at least compulsory schooling (9 or 10 years). Higher secondary corresponds to 12 or 13 years of schooling. Tertiary includes undergraduate and graduate programs at technical colleges and universities. Panel (A) is based on the sample of employed individuals uses to study transitions into unemployment (Columns 1 and 2 in Table 1. Panel (B) is based on the sample of unemployed individuals uses to study transitions into employment (Columns 3 and 4 in Table 1. *Data Source*: RDC of the Institute for Employment Research (IAB) at the Federal Employment Agency, SIAB SUF 7514. own calculations.

A.2 Derivation of Marital Surplus

The marital surplus reflects the gains from marriage for both partners, and those gains depend on their observable types. In our application the types i and j of men and women, respectively, are is a combination of age and nationality. I and J denote the total numbers of male/female types, i.e., age-nationality combinations. For each type, the total number of individuals (married and single) in the marriage market is denoted M_i for males and F_j for females, respectively. Two accounting identities hold:

$$\mu_{i0} + \sum_{j=1}^{J} \mu_{ij} = M_i \ \forall i, \quad \mu_{0j} + \sum_{i=1}^{I} \mu_{ij} = F_j \ \forall j, \tag{A.1}$$

where $\mu_{ij} \geq 0$, $\mu_{i0} \geq 0$, and $\mu_{0j} \geq 0$, are the numbers of ij marriages, single men of type i, and single women of type j, respectively.

Following Choo and Siow (2006), the utility of a type i man indexed g who is married

to a type j woman is denoted V_{ijg}:

$$V_{ijg} = \tilde{\alpha}_{ij} - \tau_{ij} + \epsilon_{ijg}. \tag{A.2}$$

Utility consists of a systematic component, $\tilde{\alpha}_{ij}$, the utility transfer from a type i man to a type j woman, τ_{ij}, and a random component, ϵ_{ijg}. The gain from marriage for the man is given by $\tilde{\alpha}_{ij} - \tau_{ij}$. This gain is independent of both spouses' identity but it depends on observable characteristics. The random component ϵ_{ijg} is an identity-specific idiosyncratic shock that allows for unobserved heterogeneity through deviations from the systematic utility gain for any combination of types. It is assumed that ϵ_{ijg} is i.i.d. with a type I extreme-value distribution. The systematic component for a female of type j who is married to a type i man is similarly defined and denoted $\tilde{\gamma}_{ij}$.

Observing all potential levels of utility, a male (female) individual g chooses whom to marry in the frictionless marriage market by simply picking the highest attainable utility:

$$V_{ig} = \max_j \{V_{i0g}, ..., V_{ijg}, ..., V_{iJg}\}, \tag{A.3}$$

where V_{i0g} denotes the utility from remaining single. As shown by McFadden (1974), this random utility model, together with the assumed type I extreme-value distribution of the idiosyncratic component and large numbers of men and women, yields a simple quasi-demand function for the number of ij marriages demanded by type i men:

$$\begin{aligned} \ln \mu_{ij}^d &= \ln \mu_{i0}^d + \tilde{\alpha}_{ij} - \tilde{\alpha}_{i0} - \tau_{ij} \\ &= \ln \mu_{i0}^d + \alpha_{ij} - \tau_{ij}, \end{aligned} \tag{A.4}$$

which depends on the number of type i singles and $\alpha_{ij} = \tilde{\alpha}_{ij} - \tilde{\alpha}_{i0}$, which is the systematic gross return to a type i man from being in a type ij marriage relative to being unmarried. Similarly, $\gamma_{ij} = \tilde{\gamma}_{ij} - \tilde{\gamma}_{i0}$ is the systematic gross return to a type j woman from being in a type ij marriage relative to being unmarried. The number of ij marriages demanded by type j women is given by the quasi-supply function:

$$\ln \mu_{ij}^s = \ln \mu_{0j}^s + \gamma_{ij} + \tau_{ij}. \tag{A.5}$$

The marital surplus for a type i man and type j woman, our object of interest, can be calculated by adding up equations (A.4) and (A.5). The utility transfer cancels out and the two systematic components, α_{ij} and γ_{ij}, remain.

$$\frac{\alpha_{ij} + \gamma_{ij}}{2} = \ln \mu_{ij} - \frac{\ln \mu^d_{i0} + \ln \mu^s_{0j}}{2}, \tag{A.6}$$

where the LHS is the average systematic gain, which we refer to as marital surplus, Φ_{ij}:

$$\Phi_{ij} = \ln \left(\frac{\mu_{ij}}{\sqrt{\mu_{i0}\ \mu_{0j}}} \right). \tag{A.7}$$

The number of marriages between type i men and type j women, μ_{ij}, in the numerator is scaled by the geometric average of the numbers of single men and single women of the respective types, μ_{i0} and μ_{0j}. Thus, according to the Choo and Siow (2006) model, the surplus of marriage for any ij pair is high if we observe many ij marriages relative to the respective single populations. The measure exploits that the observed number of singles of a given type is informative about the gains for this type in the marriage market. However, due to market clearing, the surplus does not depend on the availability of other types of men and women.

A.3 Single Stock Regressions

In this exercise, we show that the single stocks did not change following both the labor market reform and the EU expansion. The regression specification is shown in Equation A.8, results are presented in Table A.2. The definition of the treatment and control group corresponds to the definition used for the main analysis. All regressions control for year, nation, sex and age-group fixed effects.

$$\begin{aligned} SingleStock_t(e,c,s) &= \beta_1 \cdot Treat_{HartzI}(e,c) + \beta_2 \cdot \mathbb{1}\{t \geq 2003\} \\ &+ \beta_3 \cdot Treat_{HartzI}(e,c) \cdot \mathbb{1}\{t \geq 2003\} \\ &+ \beta_4 \cdot Treat_{EU}(c) + \beta_5 \cdot \mathbb{1}\{t \geq 2004\} \\ &+ \beta_6 \cdot Treat_{EU}(c) \cdot \mathbb{1}\{t \geq 2004\} \\ &+ \alpha_t + \delta_c + \delta_s + u_t(e,c,s), \end{aligned} \tag{A.8}$$

Table A.2: Single Stock Regressions

Dependent Variable	Available Singles	
	(1)	(2)
$Treat_{HartzI}$	-947264.5***	-947956.2***
	(22780.8)	(22785.8)
$\mathbb{1}\{t \geq 2003\}$	26846.4	27537.8
	(21216.8)	(21947.1)
$Treat_{HartzI} \cdot \mathbb{1}\{t \geq 2003\}$	-19149.0	-18080.0
	(14143.7)	(14300.1)
$Treat_{EU}$		-28683.4***
		(3717.8)
$\mathbb{1}\{t \geq 2004\}$		-624.6
		(13834.3)
$Treat_{EU} \cdot \mathbb{1}\{t \geq 2004\}$		-2939.7
		(4542.2)
	Controls	
Year, Nation, Sex, Age FEs	✓	✓
incl. EU Expansion		✓
Observations	1632	1632

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Single stocks based on piecewise cubic Hermite interpolation. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register and Microcensus, 1997–2013, own calculations.

We estimate two specifications. In column (1), we abstract from the EU expansion and only include the terms related to the labor market reform. The negative and significant coefficient of the $Treat_{HartzI}$ dummy reflects that the number of singles for the treated nationalities is smaller compared to German and EU15 singles. Both the time dummy and the treatment interaction are statistically indistinguishable from 0, so the single stocks did not increase in the aftermath of the labor market reform, neither overall nor in the treatment group relative to the control group. It also worth mentioning that all individual year effects (not shown, expressed relative to 1997) are insignificant. In column (2), we include the EU expansion. Notably, the estimated coefficients of the labor market reform terms hardly change. The time dummy and the treatment interaction are also insignificant for the EU expansion, so the single stocks for citizens of the accession countries did not increase significantly in the aftermath fo the expansion.

A.4 Microcensus Sampling Change

In 2005, the statistical offices in Germany changed their sampling procedure for the Microcensus by conducting interviews over the whole course of the year instead of using

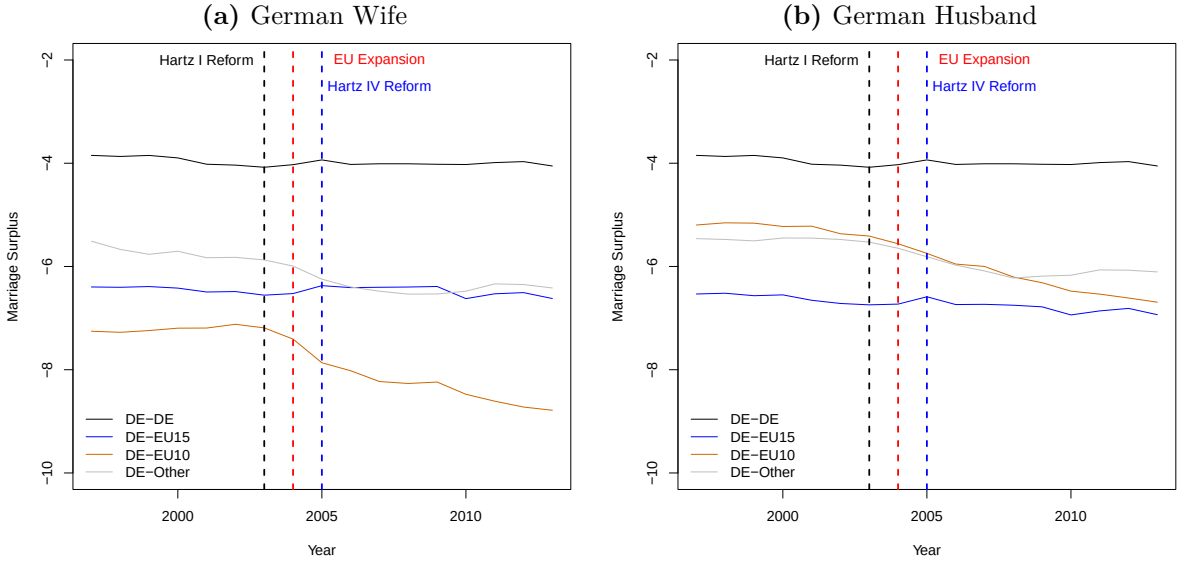
a fixed reference week. This lead to distortions in the sampling procedure during the first couple of years after the change (2005–2009). Specifically, the sample weights for households that were "hard to reach" for the interviewers at their private address became temporarily unreliable.[36] For details of the change and the irregularities it caused, see Statistisches Bundesamt (2012).

To make sure that these artificial breaks in the data do not affect our results, we impute values in the affected data range (2005–2009) at the cell (Nationality × Gender × Age) level using 5 different imputation methods: Cubic Interpolation, Linear Interpolation, Natural Cubic Spline Interpolation, Piecewise Cubic Hermite Interpolation, and Inverse Distance Weighted Interpolation. In the main analysis, we report results based on the Piecewise Cubic Hermite Interpolation. Results using other imputations techniques or raw data are consistent with the baseline findings and available upon request.

[36] An example of affected households are single individuals who live alone but are at work all day.

A.5 Robustness: Marital Surplus by Sex

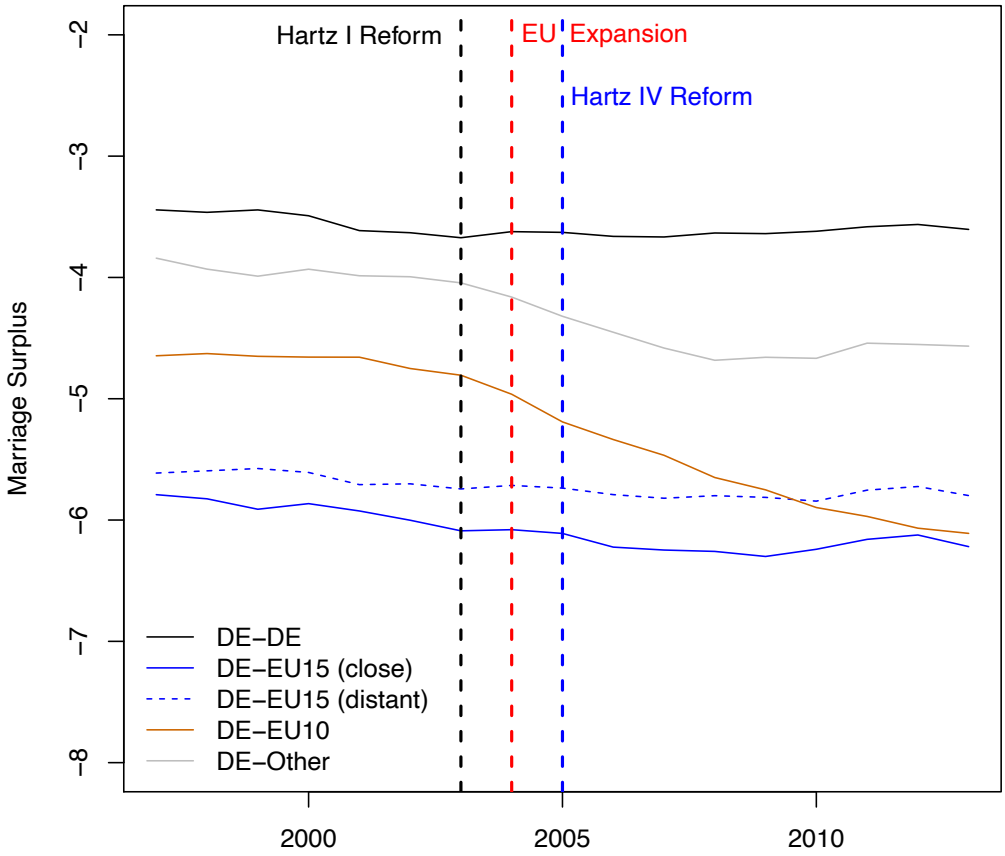
Figure A.1: Marital Surplus ($\hat{\Phi}$) by Sex



Notes: Marital surplus for marriages where at least one spouse is German by nationality of the non-German spouse and by sex of the German spouse. The black dashed vertical line indicated the year in which the Hartz I and IV reforms became effective, the red dashed vertical line marks the year 2004 in which the EU expansion took place. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register and Microcensus, 1997–2013, own calculations.

A.6 Robustness: Marital Surplus with Linguistic Distance

Figure A.2: Marital Surplus ($\hat{\Phi}$) incl. Linguistic Distance over Time



Notes: Marital surplus for marriages where at least one spouse is German by nationality of the non-German spouse when separating EU15 nationalities by linguistic distance to German. The black dashed vertical line indicated the year in which the Hartz I and IV reforms became effective, the red dashed vertical line marks the year 2004 in which the EU expansion took place. *Data Source*: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Marriage Register and Microcensus, 1997–2013, own calculations.

Table A.3: Treatment and Control Groups with Linguistic Distance

Nationalities of Spouses (c_h, c_w)	Hartz Treatment (Jan 01, 2003)	EU Treatment (May 01, 2004)	Treatment Dummy $Treat_{HartzI}$	$Treat_{EU}$
Panel A				
German-German	No	No	$= 0$	$= 0$
German-EU15 (close)	No	No	$= 0$	$= 0$
German-EU15 (distant)	No	No	$= 0$	$= 0$
German-EU10	Yes	Yes	$= 1$	$= 1$
German-Other	Yes	No	$= 1$	$= 0$
Panel B				
German-German	No	No	$= 0$	$= 0$
German-EU15 (close)	No	No	$= 0$	$= 0$
German-EU15 (distant)	Yes	No	$= 1$	$= 0$
German-EU10	Yes	Yes	$= 1$	$= 1$
German-Other	Yes	No	$= 1$	$= 0$
Panel C				
German-German	No	No	$= 0$	$= 0$
German-EU15 (close)	Yes	No	$= 1$	$= 0$
German-EU15 (distant)	No	No	$= 0$	$= 0$
German-EU10	Yes	Yes	$= 1$	$= 1$
German-Other	Yes	No	$= 1$	$= 0$
Panel D				
German-German	No		$= 0$	
German-EU15 (close)	Yes		$= 1$	
German-EU15 (distant)	Yes		$= 1$	