German Internal Migration: A Marriage Market Perspective*

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Abstract

This paper investigates whether young, female-skewed East–West migration after German reunification altered marital stability in receiving West German districts. I construct district-year measures of cumulative exposure to young East German migrants, expressed in imputed young-female units and scaled by fixed pre-treatment population, and relate this exposure to district-level divorce rates. To address endogenous migrant sorting, I use a historical-share-by-push instrument that interacts pre-reunification East-linked settlement shares with a cumulative female-weighted East German labor-market push component. The preferred IV estimates show that districts with greater predicted exposure experienced lower post-reunification divorce rates after conditioning on state-year shocks and predetermined district characteristics. The evidence supports a stabilizing relationship between predicted young East–West migration exposure and divorce rates in receiving districts, while the mechanism evidence does not isolate a purely female-specific or unilateral filing channel.

Keywords: Migration, divorce, sex-ratio, Germany.

JEL Codes: J12, J61, R23, D10.

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1 Introduction

German reunification triggered a large and persistent wave of internal migration from East to West Germany that reshaped local demographic and economic conditions across West German districts (*Kreise* and *kreisfreie Städte*) (Fuchs-Schündeln & Schündeln, 2009; Glorius, 2010; Stawarz et al., 2020). Since reunification, East Germany experienced a large net population loss through internal migration to West Germany, exceeding 1.2 million people over the post-reunification period; gross East–West mobility was substantially larger (Rosenbaum-Feldbrügge et al., 2022; Stawarz et al., 2020). These flows were also strongly selective. Young and relatively educated women moved disproportionately, while return migration was more common at older ages (Fuchs-Schündeln & Schündeln, 2009; Kröhnert & Vollmer, 2012; Leibert, 2016). As a result, post-reunification migration changed local population composition in both origin and destination regions and contributed to pronounced sex-ratio imbalances in parts of East Germany (Gulczyński, 2023; Stawarz et al., 2024). This demographic reallocation matters because marriage and divorce affect household resources and the incentives for marriage-specific investments, including children and within-household specialization (Becker et al., 1977; Bröckel & Andreß, 2015). The post-reunification migration wave therefore provides a useful setting for studying how internal migration can affect marital stability through changes in local partner-market conditions.

A large literature shows that sex ratios and partner availability influence relationship formation and dissolution. When potential partners of the opposite sex become more abundant, search frictions may fall, outside options may improve, and the incentives governing marriage and divorce may change (Angrist, 2002; South & Lloyd, 1992). Existing evidence suggests that the direction of these effects is context dependent. Gender imbalances have been linked to marriage, fertility, bargaining, labor supply, and long-run gender norms in a range of settings (Grosjean & Khattar, 2019; Kesternich et al., 2020; Ogasawara & Igarashi, 2025). In Denmark, opposite-sex exposure in the workplace predicts divorce risk, suggesting that partner-market opportunities may operate through concrete interaction environments rather than only broad geographic sex ratios (Uggla & Andersson, 2018). German micro-level evidence has examined district sex ratios as contextual predictors of relationship dissolution, but the estimated district-level sex-ratio effects are not robustly significant. This makes the German case an open setting rather than a settled empirical fact (Obersneider et al., 2019). Evidence from college dating markets shows that local sex ratios affect relationship behavior: where women are relatively more numerous, they are less likely to form committed relationships and more likely to report less traditional dating patterns (Uecker & Regnerus, 2010). Against this background, female-skewed East–West migration among young adults after 1990 offers a particularly informative within-country setting in which to study whether a large and demographically distinctive migration episode to receiving areas altered local marriage markets and, in turn, divorce rates.

Despite a large literature on post-reunification internal migration, relatively little work examines whether migration changed local partner-market conditions in receiving West German districts. This paper asks whether greater exposure to female-skewed East–West migration among young adults affects divorce decisions in destination districts. The theoretical sign of this effect is not obvious a priori. A larger inflow need not necessarily destabilize existing unions. In the setting studied here, a disproportionately female inflow can make women relatively less scarce in local marriage markets and thereby weaken their remarriage prospects and bargaining position outside marriage. If divorce responds to the expected gains from separation, this mechanism predicts lower divorce in more exposed receiving districts.

The central empirical problem is that migrant destination choice is not random. East German migrants may have sorted into economically dynamic, socially distinctive, or geographically advantaged West German districts, and those same districts may have experienced different family trajectories even in the absence of migration. The empirical strategy addresses this concern by exploiting cross-district variation in cumulative migration exposure while conditioning on state-by-year fixed effects and predetermined district characteristics. To deal with potential endogeneity, I instrument imputed young-female migration exposure with a one-year-lagged shift-share measure that interacts a district's historical East-linked settlement share, constructed on harmonized 1961 district geography, with a cumulative post-1991 East German female labor-market push factor. The historical-share component follows the broader logic of Burchardi and Hassan (2013), who use historically rooted East-linked settlement patterns to proxy for social ties to East Germany. The measure used here differs in construction and is used to predict migration exposure rather than income growth. This design combines predetermined destination-side heterogeneity with an origin-side shock in order to isolate variation in local migration exposure that is less directly driven by contemporaneous destination conditions. The destination-share component is motivated by evidence that historically rooted East–West social ties shaped post-reunification regional adjustment and migrant destination choice (Burchardi & Hassan, 2013; Dorner et al., 2016). At the same time, the design must account for spatial confounds near the former border, including changes in market access and place-based policy exposure (Ehrlich & Seidel, 2018; Redding & Sturm, 2008).

The main finding is that districts with greater predicted exposure to young East–West migration, expressed in imputed young-female units, experienced lower divorce rates after reunification. In the preferred IV specification, a one-standard-deviation increase in cumulative predicted exposure—about four imputed young-female migrants per 1,000 baseline residents—is associated with roughly 0.26 fewer annual divorces per 1,000 residents, approximately 10 percent of the sample mean. The first stage is strong, and the negative estimate is robust to alternative outcome definitions, distance-bin-by-year fixed effects, two-way clustering, population weighting, and a long-difference specification.

This paper contributes to three strands of the literature. First, it makes a data contribution: I assemble, to my knowledge for the first time, a harmonized district-level panel of divorce counts for West Germany covering 1985–2007, digitized from printed publications of the state statistical offices and mapped to a stable 1991 district geography. Second, it contributes to the literature on German internal migration after reunification by showing that the consequences of East–West mobility extended beyond labor-market adjustment and demographic redistribution to family outcomes in receiving regions (Fuchs-Schündeln & Schündeln, 2009; Glorius, 2010; Stawarz et al., 2020). Third, it contributes to the economics of marriage markets and to the literature on migration shocks and predetermined networks by providing district-level evidence that a large, young, female-skewed internal migration episode was followed by lower marital dissolution in more exposed destination markets, a pattern directionally consistent with partner-availability mechanisms (Angrist, 2002; Becker et al., 1977; Chiappori et al., 2002; South & Lloyd, 1992), while showing that the implied magnitudes exceed what net sex-ratio changes alone can explain. The empirical design combines the new panel with a historically grounded exposure instrument tailored to the reunification setting (Goldsmith-Pinkham et al., 2020).

The remainder of the paper is organized as follows. Section 2 reviews the historical background and develops the paper's testable hypotheses. Section 3 describes the data, sample construction, and descriptive evidence. Section 4 presents the empirical framework and identification strategy. Section 5 reports the main results, heterogeneity analyses, and mechanism evidence. Section 6 concludes.

2 Background

German reunification triggered one of the largest internal migration episodes in an advanced economy. These flows were heavily age-selective: migration rates peaked among young adults, especially those aged 18–29. The 18–24 age group accounted for roughly one third of net East–West losses, and the cumulative net migration balance for this age group reached about −44% over the 1990s and early 2000s (Glorius, 2010; Rosenbaum-Feldbrügge et al., 2022).[1] Using official migration statistics for 1991–2012, recent work shows that in the key migration ages 18–25 more women than men move from East to West and that, once Berlin is excluded, official data imply only about 0.87 male migrants per female migrant (1.154 million men versus 1.326 million women) (Stauder, 2018). Geissler, Leopold and Pink report that among East German "home leavers" between

[1]This statistic refers to an age-group-specific cumulative migration balance, not to one fixed birth cohort. Over this period, successive birth cohorts enter and leave the 18–24 age group.

2000 and 2010, 24.1% of young women but only 12.7% of young men relocated to the West (Geissler et al., 2013). The demographic imprint is substantial: in the 18–29 age group the eastern sex ratio fell to around 89 women per 100 men overall and to 80 women per 100 men in some rural districts, and recent work documents rural eastern counties with fewer than 70 women per 100 men aged 18–30 (Kröhnert & Vollmer, 2012; Stawarz et al., 2024).

The female bias is particularly strong for education-related moves: by age 28, around 87,000 East-born men but 239,000 East-born women had migrated West for education. Hence, the number of female educational migrants is nearly three times the number of male ones, with the gap being largest among highly educated women (Stauder, 2018). Micro-data further show that these migrants were disproportionately single and positively selected: in social insurance records only about 40% of employed East–West migrants in the mid-1990s were married compared to roughly 57% of East German non-migrants. Panel evidence indicates that individuals in partnerships are significantly less likely to move, while older single migrants are more likely to return to the East than to remain in the West (Brücker & Trübswetter, 2007; Fuchs-Schündeln & Schündeln, 2009). Overall, post-reunification East–West migration can be characterized as a persistent outflow of young, comparatively skilled women, especially those moving for education, which exposed West German partner and labor markets to a disproportionately female inflow.

Figure 1 plots the national net internal migration balance by sex, defined as total inflows minus total outflows.[2] Two features stand out. First, before the opening of the Berlin Wall, recorded East–West mobility was very limited, reflecting the institutional constraints of German division rather than a normal low-migration equilibrium. Second, the opening of the Wall on 9 November 1989 and the subsequent institutional transition culminating in formal unification on 3 October 1990 coincide with an extraordinary and short-lived surge in net migration.

In the late 1980s, net internal migration fluctuates around only a few tens of thousands per year (roughly 10,000 to 30,000), with fairly similar contributions from men and women. A sharp discontinuity occurs in 1989: the net balance jumps to the order of 300,000 and remains similarly high in 1990. Interpreting annual totals, this spike should be read as the rapid mobility response following the border opening in November 1989 and the subsequent transition phase; importantly, the 1989 value reflects only the final weeks of that year with an open border, yet already reaches the same order of magnitude as 1990. By 1991, the net balance falls markedly (to roughly 150,000), indicating fast normalization relative to the immediate shock.

[2] Series are first aggregated at the state×year level and then summed to national totals. This figure restricts the sample to *German nationals*. For historical background and dates (Berlin Wall opening: 9 Nov 1989; formal unification: 3 Oct 1990; Two-Plus-Four Treaty entry into force: 15 Mar 1991), see, e.g., the chronology in the Federal Agency for Civic Education (bpb) and the Bundestag research note on the Two-Plus-Four Treaty.

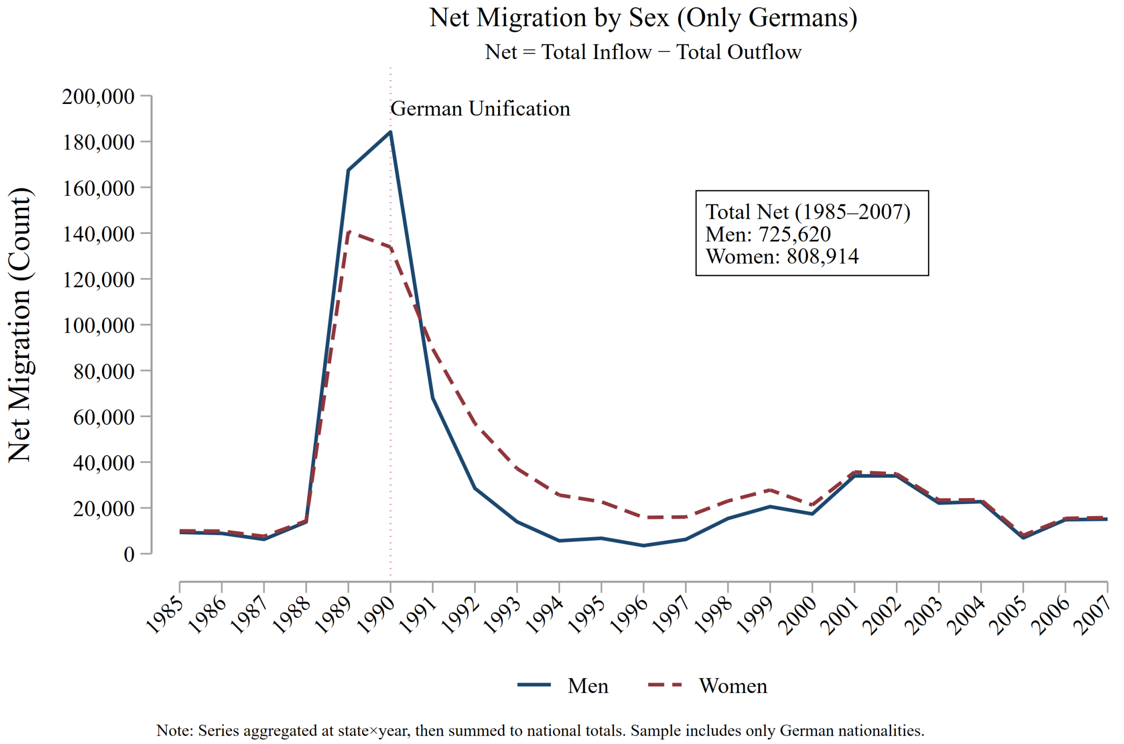

Figure 1: Net East–West internal migration by sex (German nationals only), 1985–2007

Notes: Net migration is defined as inflows from East Germany to West Germany minus outflows from West Germany to East Germany. Series are aggregated at the state×year level and summed to national East–West totals. The vertical line marks German unification.

By sex, the 1989–1990 surge is pronounced for both groups. Men contribute somewhat more at the peak (on the order of roughly 175,000 net) than women (roughly 130,000 to 140,000), implying that the initial wave is broad-based rather than purely female-driven. After 1991, net migration declines sharply: it drops below 100,000 by 1992 and settles into a much lower positive range (often 20,000 to 50,000) through much of the mid-1990s and mid-2000s. The series exhibits a secondary rise in the early 2000s, reaching roughly 70,000 at its local maximum, before returning toward the lower range.

Compositionally, the post-1991 period is more female-skewed: women account for a larger share of the net balance in many years, with male net migration reaching low levels in the mid-1990s while female net migration remains in the tens of thousands. From 1985 to 2007, the cumulative net balance is roughly 1.5 million, with women contributing slightly more than half.

Figure 2 complements the aggregate net balance by showing the *age composition of East-West inflows* (levels) from 1991 onward.[3] Two patterns stand out. First, the early post-unification inflow is concentrated among younger and prime-age cohorts: in

[3]In contrast to Figure 1, the age-disaggregated inflow series does *not* impose a nationality restriction and therefore pools all movers. Moreover, age-disaggregated inflows start in 1991 in these data, so the immediate 1989–1990 surge visible in Figure 1 cannot be decomposed by age here.

1991, inflows are largest for ages 18–25 (about 65,000) and 30–50 (about 50,000), with substantial inflows of minors under 18 (about 50,000) and a smaller but meaningful contribution from ages 25–30 (about 30,000). Older-age inflows are an order of magnitude smaller (only a few thousand), indicating that the migration process is overwhelmingly driven by cohorts most relevant for labor-market entry, family formation, and childrearing decisions.

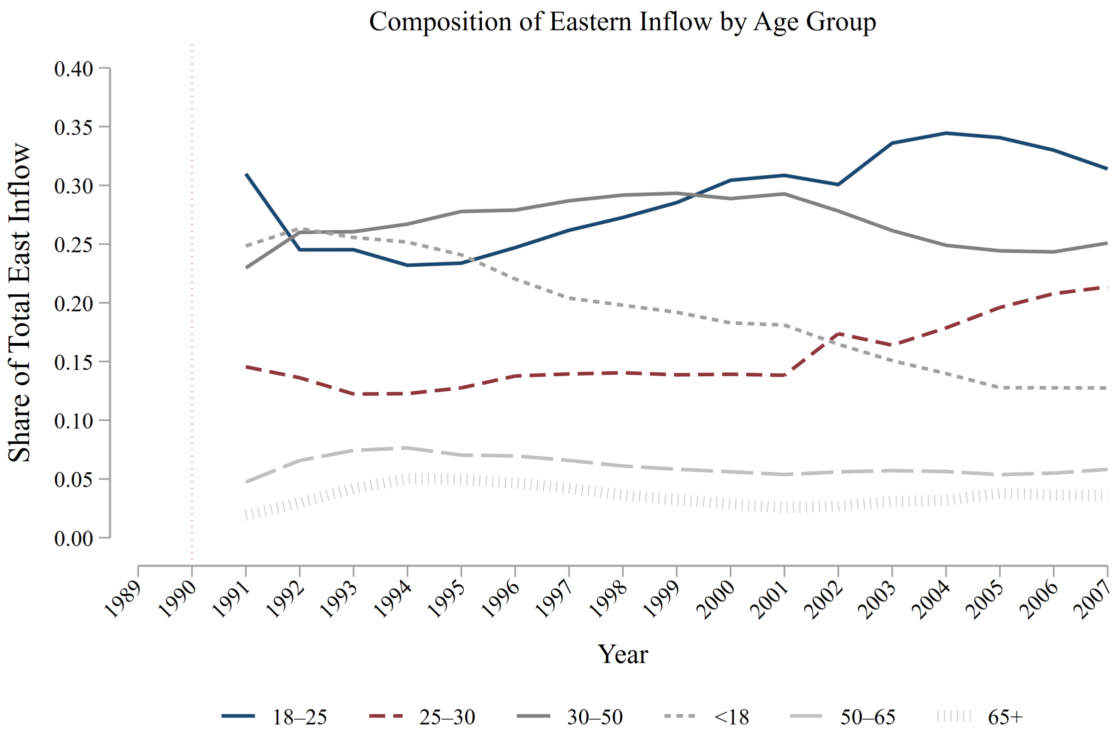

Figure 2: East–West inflows by age groups, 1991–2007

Notes: The vertical line marks German unification.

Second, the time profile mirrors an "early drop–late rebound–later decline" dynamic. From 1991 into the mid-1990s, inflows fall sharply across all major age groups (e.g., ages 18–25 drop to roughly 30,000, and under-18 inflows to roughly 25,000). From the late 1990s into the early 2000s, inflows rise again, with the rebound concentrated in the same young and prime-age groups (around 2001, ages 18–25 and 30–50 return to roughly 50,000 each, while under-18 inflows recover to roughly 30,000). After 2002, inflows decline again, especially for minors and prime-age adults, while ages 18–25 remain the single largest group through the end of the sample. Overall, the age profile supports interpreting post-1990 East-West mobility as an economically and demographically salient migration process dominated by young adults and prime-age movers, with minors moving alongside them and very limited participation of older cohorts.

Existing work on German reunification shows that historically rooted East–West ties shaped post-1989 economic behavior and spatial allocation. Burchardi and Hassan (2013) show that West German regions with stronger social ties to East Germany experienced faster post-reunification income growth, using historically determined East-linked settlement patterns and wartime destruction as a source of exogenous variation. Dorner et al. (2016) extend this logic to the migration of East German inventors and show that regions with stronger social ties to the East attracted more inventors after 1990. This literature makes it plausible that historically mediated East-links affected where migrants went after reunification. At the same time, spatial frictions and border shocks also mattered for regional adjustment near the former inner-German border (Redding & Sturm, 2008), and place-based interventions may have had persistent local effects (Ehrlich & Seidel, 2018). These considerations motivate an identification strategy that accounts for endogenous sorting, historically mediated destination choice, and concurrent spatial shocks.

The effect of internal migration on divorce depends on the demographic composition of the inflow and on how that inflow changes local partner-market conditions. In the present setting, the relevant migration episode is young and strongly female-skewed East–West migration after reunification. In receiving West German districts, such an inflow may reduce women's relative scarcity in local partner markets and alter the expected gains from marital dissolution (Angrist, 2002; South & Lloyd, 1992). The first hypothesis is therefore that higher cumulative exposure to young East–West migration, expressed in imputed young-female units, reduces district-level divorce rates.

The mechanism, however, is not directly observed. If the response operates mainly through one-sided outside options or intra-household bargaining, divorce responses may differ between man-filed and woman-filed cases. If the response instead reflects broader marital stability, mutual reassessment of separation gains, or changes in match continuation values, joint filings may also respond. I therefore treat filing-side decompositions as exploratory mechanism evidence rather than as separate causal tests. Similarly, marriage counts are used to assess whether the estimated divorce response reflects changes in marriage formation rather than dissolution.

Finally, the effect may vary across local marriage markets. A given inflow should matter more where partner markets are thinner, where migration frictions are lower, or where historical East–West ties made post-reunification mobility more salient. I therefore examine heterogeneity by district type and by distance to the former inner-German border. These heterogeneity tests are interpreted descriptively: they help characterize where the baseline effect is strongest, but they do not by themselves prove the behavioral mechanism.

3 Data and Sample

This section describes the construction of the district-year panel used in the analysis. The final dataset combines four building blocks: annual divorce outcomes for West German districts, district-level exposure to East–West migration after reunification, the two components of the shift-share instrument, and a set of predetermined district controls. The unit of observation is a West German district-year. Throughout the paper, "district" refers to the harmonized 1991 Kreis geography, including counties and independent cities (*kreisfreie Städte*). The starting point is the set of West German districts defined on a stable 1991 district geography. I exclude East German districts and the city-states Berlin, Hamburg, and Bremen. Berlin is excluded because its division-era status and reunification shock make it incomparable to the West German receiving districts analyzed here. Hamburg and Bremen are also excluded because their city-state structure does not correspond to the district-level setting used in the analysis. In all three cases, the administrative unit is simultaneously a city-state and a local unit, so these observations are not comparable to West German districts embedded within larger federal states and contribute little to within-state-year identification once state-by-year fixed effects are included.

The empirical analysis uses a balanced panel of West German districts spanning 1985–2007. The outcome data begin in the mid-1980s, which provides a pre-reunification baseline, while the measured migration exposure begins in 1991, the first year for which the age-disaggregated district-level inflow data used in the analysis are available. The full panel therefore contains a pre-treatment period, 1985–1990, and a treatment period, 1991–2007; I end the sample in 2007 to keep the analysis entirely pre-reform, since major changes to German divorce-related family law began in 2008 and continued with the restructuring of pension equalization in divorce proceedings effective 1 September 2009 ("Unterhaltsrechtsreform 2008," 2007).

One timing feature of the data deserves emphasis. District-level East–West inflows disaggregated by age are first available in 1991, so the exposure measure cumulates from 1991 onward and excludes the large 1989–1990 surge documented in Figure 1. Those early arrivals plausibly flowed through the same East-linked networks that the instrument exploits, with two consequences. First, the measured post-1991 exposure understates true cumulative exposure by the 1989–1990 component, so the estimates should be read as effects per unit of *post-1991 measured* exposure. Second, 1989 and 1990 cannot be treated as clean pre-treatment years: arrivals during the border-opening phase already differ systematically across districts. The pre-trend analyses below therefore rely on 1985–1988 as the uncontaminated pre-period, and event-study coefficients for 1989–1990 are interpreted as transition-period estimates rather than as placebo checks.

I additionally exclude a small set of Lower Saxony districts affected by administrative boundary changes, particularly in the Göttingen and Hannover areas. These units cannot be followed consistently over time on a stable district geography, which makes it difficult to construct a reliable district-year panel for the full sample period. The same districts also lack a consistently usable historical-share measure for the instrument on the harmonized

1991 geography.[4] After these restrictions, the analysis file contains 320 West German districts.

The resulting dataset is a balanced panel with 320 districts observed annually from 1985 through 2007, yielding 7,360 district-year observations. Restricting the sample to the post-reunification period 1991–2007 yields 5,440 observations. Because the main treatment and instrument enter with a one-year lag, the effective IV estimation sample begins in 1992 and contains 5,120 district-year observations.

The core outcome variables are drawn from annual administrative divorce statistics published by the statistical offices of the West German federal states. Because no single ready-to-use national district-level file exists for the full sample period, I assembled the outcome panel from printed statistical reports issued by the individual state statistical offices.[5] I digitized these district-year tables from the original printed sources and harmonized them to the common 1991 district geography used throughout the paper. To my knowledge, this paper is the first to assemble and use a West German district-level divorce panel of this scope for the 1980s and 1990s.

The main analysis uses annual divorce counts scaled by fixed pre-treatment population. Let D_{ct} denote the number of divorces in district c and year t, and let $N_{c,1987}$ denote district c's population in 1987. The main outcome is defined as

$$Y_{ct} = 1000 \times \frac{D_{ct}}{N_{c,1987}}. \tag{1}$$

Thus, Y_{ct} measures annual divorce rates per 1,000 residents in the fixed 1987 population. I use a fixed pre-treatment denominator rather than contemporaneous population because migration exposure can itself affect district population. A conventional divorce rate using contemporaneous population would therefore combine changes in divorce counts with mechanical changes in the denominator induced by migration. The fixed-denominator construction keeps the outcome on a common pre-treatment population scale and avoids building the treatment mechanically into the dependent variable.

[4] Specifically, these districts either lack a recorded *Deutsche aus SBZ/Berlin (ohne Vertriebene)* population in the 1961 census as harmonized to 1991 boundaries, or cannot be matched to a consistent district identifier across the 1961 and 1991 administrative geographies. Because the shift-share instrument requires a well-defined historical share for every district in the estimation sample, retaining these units would require imputing the share component or dropping them only from IV specifications. To keep the OLS and IV samples comparable, I exclude them throughout. These exclusions are necessary for two related reasons. First, administrative reforms in the Göttingen and Hannover areas prevent a consistent district-level panel from being constructed over the full sample period. Second, for the affected units, the historical-share component of the instrument cannot be recovered reliably on the harmonized 1991 geography. Retaining these districts would therefore either require ad hoc imputations or force different estimation samples across OLS and IV specifications. To keep the sample consistent throughout the paper, I exclude them from all analyses.

[5] The titles of these publications vary across states and years. In practice, the relevant tables come from annual statistical reports or statistical yearbooks on marriages, divorces, and natural population movement that report district-level divorce counts by year.

Two limitations of this outcome definition are important. First, Y_{ct} measures divorce counts against a fixed population base, not a dissolution hazard. Because the stock of marriages at risk is not observed at the district-year level, the estimates cannot formally distinguish a lower dissolution probability among existing marriages from changes in the size of the married population at risk. This concern is partly mitigated by the timing of divorce: the average duration of marriage at divorce in Germany was around twelve years in the mid-1990s and about thirteen to fourteen years in the 2000s (Statistisches Bundesamt, 2025-06-26, 2025, 2026-02-05, 2026). Many divorces observed in the 1990s were therefore likely drawn from marriages formed before reunification. The at-risk-stock concern is consequently most relevant for the later sample years. Second, because divorces are assigned to the district of residence, out-migration of incumbent couples could mechanically reduce measured divorce rates in origin districts if those couples divorce elsewhere. Young single in-migrants cannot mechanically reduce divorce counts, so such a compositional explanation would have to operate through the displacement or relocation of incumbent married couples. The available district-level data cannot directly rule out this channel, so I treat it as a maintained caveat.

In the preferred specification, I include the district's 1987 divorce rates as a predetermined control. This allows the analysis to compare post-reunification divorce rates conditional on the pre-reunification divorce level, while avoiding the mechanical anchoring of the dependent variable to a single baseline year. In robustness checks, I also use baseline-normalized outcomes that subtract the 1987 divorce rates or the mean pre-period divorce rates before scaling by fixed 1987 population.

The endogenous regressor is district-level exposure to East–West internal migration after German reunification. The underlying migration data come from official district-year internal migration statistics. These data record annual inflows from East Germany into West German districts and, for the full post-reunification period, allow the construction of age-specific inflow measures. The analysis focuses on young female migration exposure because the paper studies whether female-skewed migration altered local marriage-market conditions in receiving districts.

The building block for the preferred treatment is the observed district-year inflow of young East–West migrants. I define young inflows as the sum of the 18–25 and 25–30 age groups. [6] District-level young-female inflows are then constructed by applying the corresponding state-year female share among East–West German inflows to this observed young inflow total. The resulting series should therefore be interpreted as an imputed measure of young female migration exposure at the district-year level.

[6] The age-disaggregated inflow series pools all nationalities, whereas the state-year female share used below is computed among German East–West inflows. The mismatch is quantitatively minor in this setting: the resident foreign population of the GDR was on the order of one percent of the population in 1989, so East–West flows over the sample period are overwhelmingly flows of German nationals. The imputed series should nevertheless be read as German-female-share-weighted total young inflows.

The main treatment variable is a cumulative stock measure of young East–West migration exposure, expressed in imputed young-female units and scaled by fixed pre-treatment population. The imputation is necessary because the district-year migration data identify East–West inflows by age group from 1991 onward, but they do not consistently report the sex composition of these age-specific district inflows for the full sample period. I therefore combine the observed district-year young inflow total with the female share of East–West German inflows observed at the state-year level.

Let F_{ct}^{Y} denote observed young East–West inflows into district c in year t, where young inflows are defined as the sum of the 18–25 and 25–30 age groups. Let s_{st}^{f} denote the female share among East–West German inflows into state s in year t. I construct imputed young-female inflows as

$$\widehat{F}_{ct}^{Y,f} = F_{ct}^{Y} \times s_{st}^{f}. \tag{2}$$

Thus, the imputation preserves the observed district-year variation in young East–West inflows and assigns the sex composition using the corresponding state-year female share.

Let $N_{c,1988}$ denote district c's population in 1988. The cumulative exposure measure is then defined as

$$M_{ct} = \sum_{\tau=1991}^{t} \frac{1000 \times \widehat{F}_{c\tau}^{Y,f}}{N_{c,1988}}. \tag{3}$$

Thus, M_{ct} measures cumulative young East–West migration exposure per 1,000 baseline residents, expressed in imputed young-female units. I lag the exposure measure by one year in the regressions to ensure that migration exposure is predetermined with respect to the divorce outcome in year t and to reduce simultaneity concerns from contemporaneous district-year shocks. The divorce outcome is scaled by 1987 population, while migration exposure is scaled by 1988 population; both denominators are fixed before reunification and are used only to express outcomes and exposure on a comparable pre-treatment population scale.

Because the preferred treatment variable relies on imputing the female share of young inflows before 2000, it is important to assess whether this imputation rule is empirically reasonable. I do so in the period 2000–2007, when district-level East–West inflows by sex are observed. In that validation window, I compare observed district-level German inflows by sex to predicted inflows constructed from the state-year female share rule. The validation sample contains 2,560 district-year observations. The predicted series tracks the observed inflow series reasonably well. The correlation between observed and predicted inflows is 0.888, and the corresponding log correlation is 0.870. The weighted absolute percentage error is 0.208, meaning that the prediction error is about 21 percent of observed inflows on average when weighted by flow size. A calibration regression of observed on predicted inflows yields a slope of 1.06, close to the one-to-one benchmark. These diagnostics suggest that the state-year sex-composition rule captures much of the observable district-year variation in sex-specific inflows during the period in which the true sex split is observed.

These results suggest that the state-year sex-composition rule tracks the observable total inflow margin reasonably well and is therefore informative for constructing the main treatment variable. The validation should nevertheless be interpreted narrowly. It evaluates the imputation of the sex split on the observed total-flow margin, not the full age-by-sex cell that defines young female inflows before 2000. The preferred migration measure should therefore be interpreted as an imputed proxy rather than a directly observed district-year young-female series.

3.1 Shift-share variables

For the IV analysis, I construct a district-year shift-share predictor from two components: a time-invariant historical East-linked settlement share and a time-varying East German female labor-market push component. This subsection describes the construction of both components.

The destination component is a historical settlement share constructed from the 1961 population census. Specifically, I use the district-level share of residents classified as *Deutsche aus SBZ/Berlin (ohne Vertriebene)*. Conceptually, this group captures historical East-linked population ties in West German destination districts while excluding the post-war expellee population. Because district boundaries changed substantially between 1961 and the post-reunification period, I harmonize the 1961 information to the stable 1991 district geography used throughout the paper using historical administrative boundary data and a crosswalk from 1961 districts to 1991 West German districts. The resulting variable, denoted s_c^{1961}, is fixed at the district level.

The time-series component is a female-specific aggregate East German labor-market push factor. I construct it from sectoral employment data in the German Regional Accounts and pre-transition female employment weights from the 1988 GDR Statistical Yearbook. Let $j \in \mathcal{J}$ index aggregate economic sectors, let $\omega_{j,f}^{1988}$ denote the share of the East German female workforce employed in sector j in 1988, and let $E_{j,t}$ denote East German employment in sector j in year t. The annual push component is defined as

$$P_t^f = \sum_{j \in \mathcal{J}} \omega_{j,f}^{1988} \left(\frac{E_{j,1991} - E_{j,t}}{E_{j,1991}} \right). \tag{4}$$

This term measures the female-weighted employment shortfall in year t relative to the 1991 baseline. Because the endogenous regressor is cumulative migration exposure, I also cumulate the push component over time:

$$\text{CumPush}_t^f = \sum_{\tau=1991}^{t} P_\tau^f. \tag{5}$$

The district-year shift-share variable is then defined as

$$Z_{ct} = s_c^{1961} \times \text{CumPush}_t^f. \tag{6}$$

In the regression analysis, I use the one-year lag of this measure, $Z_{c,t-1}$.

3.2 Summary Statistics

Table 1 reports summary statistics for the main and control variables used in the preferred estimation sample. The estimation sample is restricted to the post-reunification period in which the lagged endogenous regressor and the lagged instrument are defined, yielding 5,120 district-year observations. The table shows substantial cross-district and over-time variation in the outcome, migration exposure, and instrument components. The mean value of the main outcome indicates that districts recorded about 2.52 annual divorces per 1,000 residents in the fixed 1987 population during the preferred estimation period. At the same time, lagged cumulative young-female migration exposure displays substantial dispersion across district-years.

The historical East-linked settlement share also varies meaningfully across West German districts, and the cumulative East German female labor-market push component evolves over time by construction. The lagged shift-share instrument combines these two sources of variation and exhibits non-trivial dispersion in the estimation sample. Population size and distance to the former inner-German border are also highly heterogeneous across districts, underscoring the importance of conditioning on pre-determined district characteristics and state-by-year fixed effects in the empirical analysis. The controls capture pre-treatment differences in family structure, density, commuting patterns, sex ratios, labor-market conditions, religion, age structure, and female employment across West German districts. All are measured before the main migration shock and are included to absorb systematic pre-existing differences that could otherwise confound the relationship between migration exposure and divorce outcomes.

3.3 Descriptive Statistics

The descriptive evidence highlights two features of the empirical setting. First, divorce outcomes display substantial cross-district heterogeneity even before reunification, and that heterogeneity remains pronounced by the end of the sample period. Second, exposure to East–West internal migration is far from uniform across West German districts. Some

Table 1: Summary Statistics

Variable	Mean	SD	Min	Median	Max
Panel A. Main variables					
Outcome: Annual divorces per fixed 1987 population	2.519	0.532	0.593	2.509	5.480
Annual divorce count	442.449	360.336	41	325	3364
Lagged cumulative young-female migration exposure	4.897	3.997	0.169	3.798	30.446
Historical East-linked settlement share (1961)	0.043	0.020	0.007	0.040	0.138
Cumulative East German female labor-market push	0.164	0.066	0.025	0.170	0.275
Lagged shift-share instrument	0.006	0.004	0.000	0.006	0.035
Baseline population (1988) (1000s)	175	133	33	132	1211
Distance to former inner-German border (km)	156.462	80.401	2.489	167.082	348.787
Panel B. Baseline controls					
Share single, 1987	0.386	0.026	0.326	0.385	0.476
Log population density, 1987	5.628	1.081	3.665	5.285	8.248
In-commuting rate, 1987	0.170	0.079	0.054	0.149	0.701
Sex ratio ages 20–35, 1987	1.056	0.043	0.936	1.056	1.239
Unemployment rate, 1987	0.066	0.025	0.028	0.065	0.143
Catholic share, 1970	0.501	0.286	0.032	0.508	0.953
Young-adult share, 1987	0.237	0.017	0.200	0.235	0.325
Female employment share, 1970	0.360	0.040	0.270	0.358	0.449
Divorce rate, 1987	0.00196	0.00059	0.00074	0.00190	0.00428

Notes: The table reports summary statistics for the preferred estimation sample, defined as district-years from 1992 to 2007 for which the lagged endogenous regressor and lagged instrument are observed. The main outcome is the annual district divorce count scaled by fixed 1987 population, $1000 \times D_{ct}/N_{c,1987}$. Time-invariant baseline controls are repeated across district-year observations in the estimation sample. Baseline-normalized divorce changes are used in robustness checks. Lagged cumulative young-female migration exposure is the preferred endogenous regressor. The lagged shift-share instrument interacts each district's predetermined 1961 East-linked settlement share with the cumulative post-1991 aggregate female labor-market push from East Germany. All baseline controls are measured before the main migration shock. The estimation sample contains 5,120 district-year observations.

districts received only limited inflows, while others accumulated large migration stocks relative to their pre-treatment population. These raw spatial differences motivate the district-level design used in the empirical analysis.

Figure 3 maps district-level divorce rates in 1987 and 2007. Two patterns stand out. First, divorce rates were already heterogeneous across West German districts before the main migration shock. Second, the cross-district distribution shifts upward by 2007, with higher divorce rates becoming more widespread across the map.

Figure 4 maps East–West migration exposure across West German districts. Exposure is measured as cumulative inflows from East Germany per 1,000 residents in baseline 1988 population. Even in 1991, exposure is already uneven across districts. By 2007, cumulative exposure is substantially more dispersed, with especially high values concentrated in districts closer to the former inner-German border and in selected southern districts, while many western districts remain less exposed. This geographic heterogeneity motivates the district-level empirical analysis.

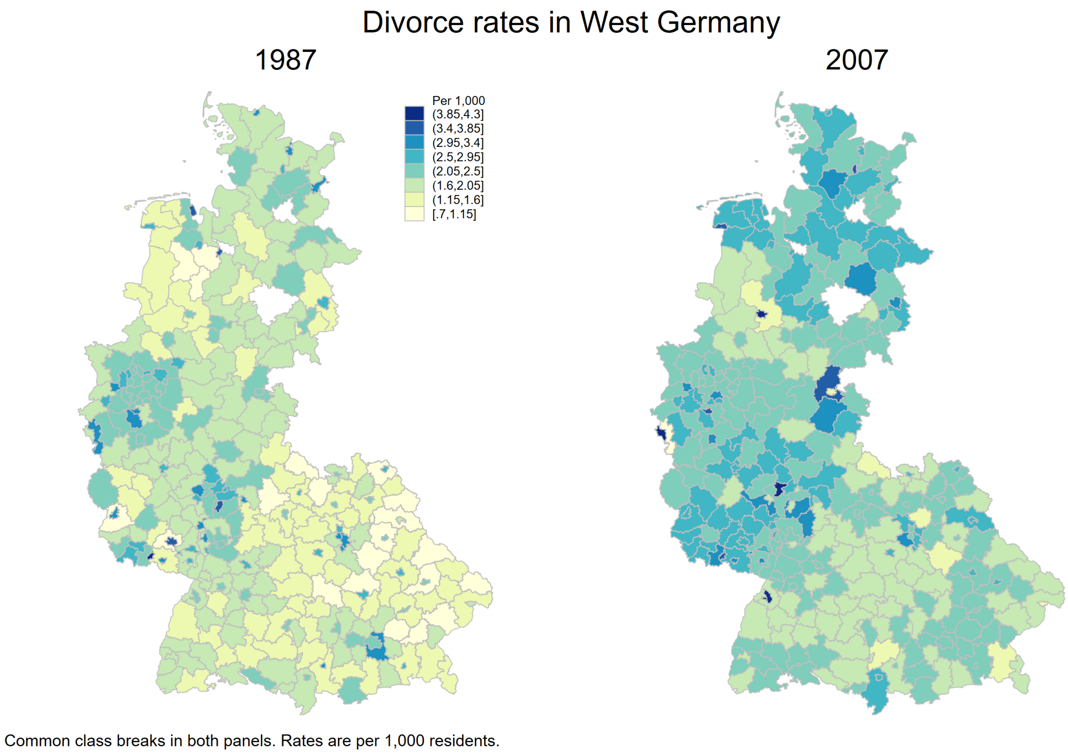

Figure 3: Spatial distribution of divorce rates in West Germany, 1987 and 2007

Notes: The figure maps district-level divorce rates in West Germany in 1987 and 2007, measured as annual divorces per 1,000 residents. Both panels are drawn on the harmonized district geography used in the analysis. The class breaks are identical in both panels, so differences in shading are directly comparable across years. Darker shading indicates higher divorce rates. The city-states Bremen, Hamburg, and West Berlin are excluded from the analysis sample. Six districts in the Braunschweig and Hannover regions of Lower Saxony are additionally excluded owing to inconsistent coverage on the harmonized 1991 district geography across the sample period.

4 Identification Strategy

This section presents the empirical strategy used to estimate the effect of female-skewed East–West migration on marital stability in receiving West German districts. The central identification problem is that migrant settlement after reunification was unlikely to be random. If districts that received larger inflows were already on different demographic, economic, or family trajectories, ordinary least squares estimates would confound the effect of migration exposure with pre-existing local differences.

Several threats to identification are immediate. First, migrants may have sorted into districts with stronger labor demand, better amenities, or different social environments, generating correlation between migration exposure and unobserved determinants of divorce (Card, 2001). Second, historical East-linked settlement patterns may have shaped the spatial distribution of post-reunification inflows, but may also proxy for persistent district characteristics that independently affect family outcomes (Bauer & Zimmermann, 1997; Dietz, 1999). Third, districts closer to the former inner-German border were ex-

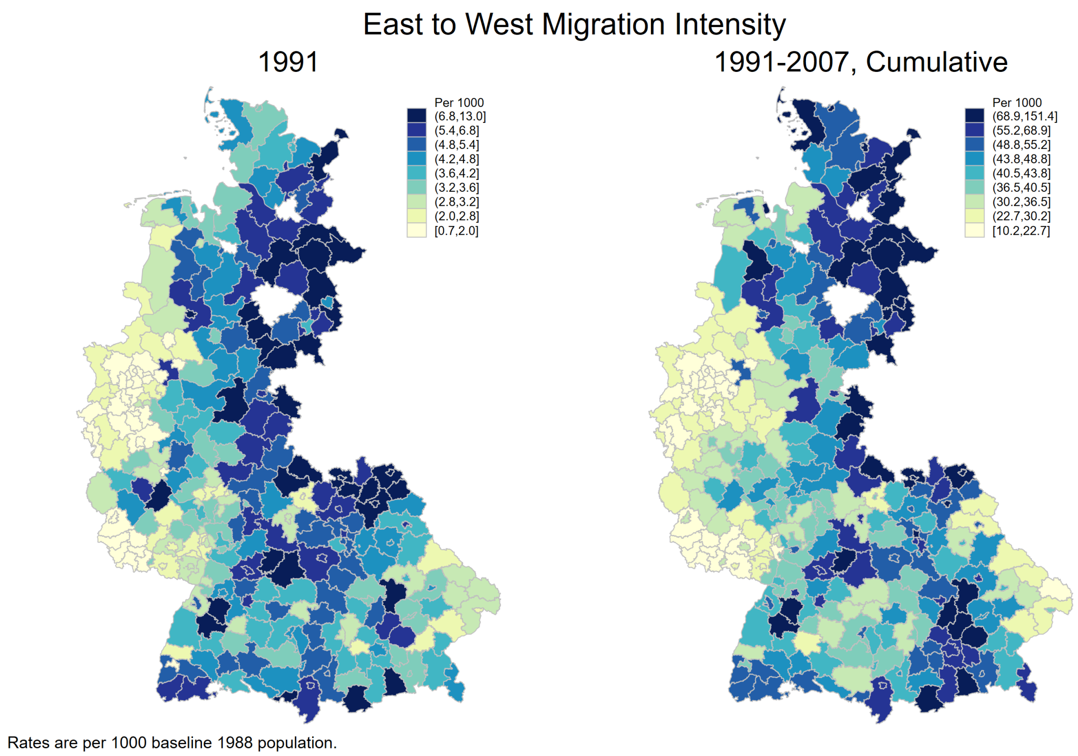

Figure 4: East–West migration intensity across West German districts

Notes: The left panel shows migration intensity in 1991, and the right panel shows cumulative migration intensity over 1991–2007. Migration intensity is defined as East–West inflows per 1,000 residents in baseline 1988 population. Darker shading indicates higher exposure. The two panels use panel-specific quantile classifications, so the colors should be interpreted within each panel rather than as directly comparable absolute levels across panels. The city-states Bremen, Hamburg, and West Berlin are excluded from the analysis sample. Six districts in the Braunschweig and Hannover regions of Lower Saxony are additionally excluded owing to inconsistent coverage on the harmonized 1991 district geography across the sample period.

posed to other spatial shocks around reunification, including changes in market access and industrial restructuring (Redding & Sturm, 2008). One such shock is sharply time-varying in a way that mimics the instrument: place-based support for the former *Zonenrandgebiet* was scaled back after 1991 and expired in 1994, so border-proximate districts—which also have systematically higher 1961 East-linked settlement shares—experienced a destination-side policy withdrawal whose timing coincides with the accumulation of the push component (Ehrlich & Seidel, 2018). The distance-bin-by-year fixed effects reported in Section 5.4 are included specifically to absorb shocks of this type.

To address these concerns, the empirical design combines three elements. First, the dependent variable is defined using a fixed pre-treatment denominator, so that migration-induced population growth does not mechanically enter the outcome. Second, the endogenous regressor is a cumulative stock of imputed young-female East–West migration exposure, which matches the idea that local marriage-market conditions adjust through the accumulated presence of migrants rather than through one-year inflow spikes. Third, I instrument this cumulative exposure with a one-year-lagged shift-share measure that interacts a predetermined historical East-linked settlement share, measured on harmonized 1961 district geography, with a cumulative post-1991 East German female labor-market push factor. The identifying assumption is that, conditional on state-by-year fixed effects and pre-treatment district characteristics, districts with different historical East-linked settlement shares would not have experienced systematically different divorce dynamics in response to post-reunification East German labor-market distress except through differential migration exposure.

4.1 Endogenous Migration Exposure

The endogenous regressor is *imputed* cumulative exposure to young female migrants from East Germany. The district-level migration data identify East–West inflows by age group from 1991 onward, so the young inflow margin is observed directly at the district-year level. Specifically, let

$$F_{ct}^{Y} = F_{ct}^{18\text{–}30} \tag{7}$$

where $F_{ct}^{18\text{–}30}$ denotes East–West inflows into district c in year t for the corresponding age group.

What is not observed at the district-year level before 2000 is the sex composition of these young inflows. I therefore recover a young-female inflow series by applying the female share among East–West German inflows at the state-year level to the observed district-level young inflow total:

$$\widehat{F}_{ct}^{Y,f} = F_{ct}^{Y} \times s_{st}^{f}, \tag{8}$$

where s_{st}^{f} is the share of women among East–West German inflows in state s and year t.

The treatment variable is then the cumulative stock of these imputed young-female inflows, scaled by fixed 1988 population:

$$M_{ct} = 1000 \times \frac{\sum_{\tau=1991}^{t} \widehat{F}_{c\tau}^{Y,f}}{N_{c,1988}}, \tag{9}$$

where $N_{c,1988}$ is district c's population in 1988. In the estimating equation, I use the one-year lag $M_{c,t-1}$.

This cumulative specification follows directly from the mechanism of interest. The relevant treatment is not a one-period flow shock, but the gradual buildup of young female migrant presence in the local marriage market. If migration affects divorce through partner availability, remarriage prospects, or outside options, these are stock-based mechanisms. A district receiving modest annual inflows over many years may experience a substantial shift in local marriage-market conditions even if no single annual inflow is especially large.

The main estimating specification is

$$Y_{ct} = \beta M_{c,t-1} + X_c'\Gamma + \lambda_{st} + u_{ct}, \tag{10}$$

where Y_{ct} is the annual district divorce count scaled by fixed 1987 population, $M_{c,t-1}$ is lagged cumulative migration exposure, X_c is a vector of predetermined district characteristics, λ_{st} denotes state-by-year fixed effects, and u_{ct} is an error term.

The coefficient of interest is β. It captures the change in annual district divorce rates, measured per 1,000 residents in the fixed 1987 population, associated with one additional unit of cumulative predicted migration exposure. The state-by-year fixed effects absorb all shocks common to districts within a state in a given year, so identification comes from differential exposure across districts within the same state-year after conditioning on predetermined district characteristics.

Estimating (10) by ordinary least squares is unlikely to recover a causal effect because migration exposure is endogenous:

$$\mathbb{E}[M_{c,t-1} u_{ct}] \neq 0. \tag{11}$$

Migrants may sort into districts with stronger labor demand, different social norms, or distinct pre-existing family trajectories. These concerns motivate the instrumental-variable strategy developed below.

4.2 Shift-Share Instrument

To address the endogeneity of migration exposure, I use a shift-share instrument that combines a predetermined destination-side share with a common post-reunification origin-side push component. Recent work emphasizes that shift-share designs can be justified either by quasi-random variation in the shifts or by plausibly exogenous cross-sectional variation in exposure shares (Borusyak et al., 2025). The present design is closer to the second logic. Identification does not come from many independent shocks allocated across districts. Instead, it comes from differential exposure across West German districts to a common East German migration push, where exposure is determined by historically predetermined East-linked settlement patterns.

Formally, the instrument is

$$Z_{c,t-1} = s_c^{1961} \times \text{CumPush}_{t-1}, \tag{12}$$

where s_c^{1961} is district c's historical East-linked settlement share and CumPush_{t-1} is the one-year-lagged cumulative East German push component. The logic is that post-reunification labor-market distress in East Germany generated aggregate migration pressure, while pre-existing East-linked ties shaped how strongly that pressure translated into inflows across West German destination districts.

The destination-side share, s_c^{1961}, is measured as the 1961 share of residents in district c classified as *Deutsche aus SBZ/Berlin (ohne Vertriebene)*, harmonized to the 1991 district geography (Blasius & Antoine, 1990). This measure is intended to capture historically rooted East-linked settlement patterns in West Germany before reunification. Because it is constructed decades before the migration shock studied here, it is predetermined with respect to post-1991 divorce outcomes and post-reunification migration flows.

The role of s_c^{1961} in the instrument is not to proxy for generic exposure to arbitrary shocks. Its role is narrower: it captures cross-district heterogeneity in pre-existing East-related settlement ties that may have facilitated the allocation of East–West migration after reunification. The credibility of the design therefore depends on whether this historical share isolates variation relevant to migration exposure rather than unrelated long-run determinants of family outcomes.

The second component of the instrument is a time-varying measure of labor-market distress in East Germany, constructed to capture the aggregate migration pressure specifically experienced by women after reunification. To avoid arbitrary sector selection, I construct this index across the entire economy using sectoral employment data from the German Regional Accounts (VGRdL). To isolate the female-specific demand shock from the gender-blind aggregate employment drops, I weight the sectoral declines by the exact pre-transition female employment shares derived from the 1988 GDR Statistical Yearbook.

Let $j \in \mathcal{J}$ index all aggregate economic sectors. Let $\omega_{j,f}^{1988}$ denote the share of the total East German female workforce employed in sector j in 1988, such that $\sum_{j \in \mathcal{J}} \omega_{j,f}^{1988} = 1$. Let $E_{j,t}$ denote total East German employment in sector j in year t. The annual female push component is defined as:

$$P_t^f = \sum_{j \in \mathcal{J}} \omega_{j,f}^{1988} \left(\frac{E_{j,1991} - E_{j,t}}{E_{j,1991}} \right). \tag{13}$$

This term computes the expected value of displacement for the average East German woman in year t. The bracketed term measures the fractional employment shortfall in sector j relative to the 1991 baseline. By multiplying these aggregate job losses by the historical female exposure weights, the instrument mechanically scales the broader macroeconomic shock to the exact structural vulnerability of the female labor force. A higher value of P_t^f indicates a more severe aggregate displacement of female workers.

Because the endogenous regressor is a cumulative migration exposure, I also cumulate the annual push component over time:

$$\text{CumPush}_t^f = \sum_{\tau=1991}^{t} P_\tau^f. \tag{14}$$

This mathematical accumulation is both substantively natural and econometrically required in the present setting. The treatment variable is a stock of migration exposure. If unresolved structural displacement creates continuous migration pressure, such that a job destroyed in 1992 that remains missing in 1993 exerts out-migration incentives in both years, then a purely contemporaneous flow measure would fail to properly map onto the long-run buildup of migrants in destination districts. The integrated instrument therefore mirrors the stock-flow structure of the treatment: an accumulated economic push is used to predict an accumulated migrant stock.

Using the 1988 employment structure to form the weights fixes sectoral exposure before the migration shock and before the institutional collapse of the GDR economy. This makes the weights predetermined with respect to post-reunification migration and divorce outcomes, although it does not by itself establish the exclusion restriction. Furthermore, I explicitly exclude East Berlin from both the construction of the baseline weights and the East-side employment series. This prevents spatial mismatch and avoids conflating the broader East German industrial and service shocks with the distinct, bureaucratically inflated composition of the former capital.

The first-stage relationship is

$$M_{c,t-1} = \pi Z_{c,t-1} + X_c'\Phi + \lambda_{st} + \eta_{ct}, \tag{15}$$

where $M_{c,t-1}$ is lagged cumulative young-female migration exposure, $Z_{c,t-1}$ is the one-year-lagged historical-share-by-push instrument, X_c is the vector of predetermined district characteristics, and λ_{st} denotes state-by-year fixed effects for the outcome year t.

Instrument relevance requires

$$\pi \neq 0. \tag{16}$$

Whether this condition is satisfied is an empirical question, and I assess it directly in the first-stage results below. The maintained economic intuition is straightforward: districts with stronger historical East-linked settlement patterns should have been more exposed to the common post-reunification migration push generated by persistent female-relevant labor-market distress in East Germany.

For the IV strategy to identify a causal effect, the exclusion restriction requires

$$\mathbb{E}[\, Z_{c,t-1}\, u_{ct} \,|\, X_c,\, \lambda_{st}] = 0. \tag{17}$$

That is, after conditioning on the predetermined district characteristics and state-by-year fixed effects, the lagged interaction between historical East-linked settlement shares and the East German push must affect West German divorce outcomes only through realized migration exposure.

This assumption is plausible only under a restricted interpretation. The historical-share component is predetermined, and the push component is driven by origin-side labor-market collapse rather than destination-side family conditions. In addition, state-by-year fixed effects absorb common shocks at the state-year level, while the baseline controls absorb observable cross-district differences in demographic structure, labor-market conditions, and social composition.

Still, the exclusion restriction is not automatic. Historical East-linked settlement may proxy for persistent district characteristics that are themselves related to long-run family dynamics. Likewise, districts nearer the former inner-German border may have experienced other changes in market access, restructuring, or policy exposure around reunification. For that reason, the IV design should not be read as mechanically exogenous. Its credibility depends on the narrower claim that, conditional on the included controls and fixed effects, the remaining variation in the interaction between historical settlement patterns and East German push operates primarily through migration exposure. I therefore treat balance checks, pre-trend diagnostics, and robustness analyses as central evidence on identification rather than as peripheral add-ons.

The vector X_c includes a parsimonious set of district characteristics measured before reunification. These controls are intended to capture pre-existing differences across districts that may be correlated both with migrant settlement and with subsequent divorce dynamics.

The baseline control set includes the divorce rate in 1987, the share of single individuals in 1987, the logarithm of population density in 1987, the in-commuting rate in 1987, the sex ratio among young adults in 1987, the unemployment rate in 1987, the Catholic population share in 1970, the young-adult population share in 1987, and the female employment share in 1970.

These controls serve two purposes. First, they absorb observable pre-treatment heterogeneity in local demographic structure, labor-market conditions, and family composition. Second, they make the identifying assumptions more plausible by reducing the scope for the historical settlement share to proxy for unrelated long-run district characteristics.

I deliberately keep the control strategy parsimonious. The objective is not to maximize in-sample fit, but to account for the most salient pre-existing differences across districts without introducing post-treatment variables or bad controls. Because all controls are measured before reunification, they cannot themselves be outcomes of the migration shock studied here.

Under the maintained identifying assumptions, the coefficient β in equation (10) captures the effect of greater predicted exposure to young East–West migration, expressed in imputed young-female units, on annual district divorce rates. Since the treatment is measured as cumulative migration exposure per 1,000 baseline residents and the outcome is annual divorces per 1,000 residents in the fixed 1987 population, β can be interpreted as the change in annual district divorce rates associated with one additional unit of cumulative predicted migration exposure. In the preferred controlled specification, the vector X_c includes the district's 1987 divorce rates, so the estimate conditions on the pre-reunification level of divorce rates. More precisely, the 2SLS coefficient should be interpreted as a local IV estimand for the component of district-year migration exposure induced by the historical-share-by-push instrument. It therefore identifies the effect of predicted migration exposure generated by historically mediated differences in districts' exposure to the common post-reunification East German push, rather than the average effect of all migration flows in all districts. Because the female component of the treatment is imputed, the estimate should not be interpreted as a separately identified effect of female migration alone.

5 Results

I begin by assessing whether the instrument defined in (12) has meaningful predictive power for migration exposure and by reporting the reduced-form association between the instrument and divorce outcomes. Because the endogenous regressor in the second stage is a cumulative stock of imputed young-female East–West migration exposure, Table 2 reports two complementary first-stage exercises. Columns (1)–(3) provide a construction check using annual total East–West inflow intensity. Columns (4)–(6) report the main first stage for the lagged cumulative young-female migration exposure used in the 2SLS specification.

The construction-check regressions show that the instrument predicts annual total East–West inflow intensity. The coefficient is positive and statistically significant across all three specifications. This is useful because it verifies that the historical-share-by-push instrument is not merely predicting the imputed young-female exposure mechanically; it also predicts observed total inflow intensity. The first stage remains strong after adding predetermined district controls and after moving to the more demanding district-fixed-effects specification.

The main first-stage results are stronger. With state-by-year fixed effects only, the coefficient on the instrument is 30.222 and the instrument F-statistic is 41.61. Adding predetermined district controls increases the coefficient to 34.820 and yields an F-statistic of 47.14. In the district-fixed-effects specification, the coefficient remains similar in magnitude, 30.871, and the instrument F-statistic rises to 63.73. Instrument relevance therefore does not depend on omitting observable pre-treatment district characteristics or on relying only on cross-district comparisons within state-years.

Table 2: First-stage evidence for the instrument

	Construction check			Main first stage		
	Annual total inflow intensity			Lagged young-female exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument	11.324***	14.126***	7.825***	30.222***	34.820***	30.871***
	(1.435)	(1.783)	(1.418)	(4.685)	(5.072)	(3.867)
Baseline district controls		✓			✓	
District fixed effects			✓			✓
State × year fixed effects	✓	✓	✓	✓	✓	✓
Observations	5,120	5,120	5,120	5,120	5,120	5,120
R^2	0.4450	0.4953	0.8295	0.7015	0.7324	0.9219
Instrument F statistic	62.23	62.76	30.47	41.61	47.14	63.73

Notes: Columns (1)–(3) report construction-check first-stage regressions in which the dependent variable is annual total East–West inflow intensity per 1,000 baseline 1988 residents. Columns (4)–(6) report the main first-stage regressions in which the dependent variable is lagged cumulative imputed young-female East–West migration exposure per 1,000 baseline 1988 residents, the endogenous regressor used in the 2SLS specification. The instrument is the one-year-lagged historical-share-by-push predictor formed by interacting each district's historical East-linked settlement share with the lagged cumulative female-weighted East German labor-market push component, $Z_{c,t-1}$. Columns (1) and (4) include state-by-year fixed effects only. Columns (2) and (5) additionally include the predetermined district control set: the 1987 divorce rate, share single in 1987, log population density in 1987, in-commuting rate in 1987, the 1987 sex ratio among young adults, the 1987 unemployment rate, the Catholic share in 1970, the young-adult share in 1987, and female employment share in 1970. Columns (3) and (6) include district fixed effects and state-by-year fixed effects. Time-invariant baseline controls are absorbed by district fixed effects and are therefore not included separately in these columns. All specifications use district-clustered standard errors. The reported F-statistics are cluster-robust tests of the instrument. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

These first-stage results address relevance, not validity. They show that historical East-linked settlement shares interacted with the East German female-weighted labor-market push component strongly predict the cumulative young-female migration exposure used in the second stage. They do not by themselves establish the exclusion restriction. The key identifying question remains whether the historical settlement share, conditional on state-year shocks, predetermined controls, and the pretrend checks below, is unrelated to other determinants of district-level divorce dynamics.

Table 3 complements the first-stage evidence by reporting reduced-form regressions of the main divorce outcome directly on the instrument. The dependent variable is the annual district divorce count scaled by 1987 population, $1000 \times D_{ct}/N_{c,1987}$. The reduced-form evidence is informative for two reasons. First, it reports the numerator of the IV estimand: whether the component of migration exposure predicted by the historical-share-by-push instrument is associated with post-reunification divorce outcomes. Second, the sign pattern mirrors the main IV results. With state-by-year fixed effects only, the reduced-form coefficient is positive. After adding predetermined district controls, the coefficient becomes negative and statistically significant. The district-fixed-effects specification also yields a negative and statistically significant reduced form.

Table 3: Reduced-form relationship between the instrument and divorce outcomes

	Dependent variable: Annual divorce rates		
	(1)	(2)	(3)
Instrument	1.703***	-2.287***	-3.319***
	(0.454)	(0.409)	(0.494)
Baseline district controls		✓	
District fixed effects			✓
State × year fixed effects	✓	✓	✓
Observations	5,120	5,120	5,120
R^2	0.3529	0.5540	0.7247

Notes: The dependent variable is the annual district divorce count scaled by 1987 population, $1000\times D_{ct}/N_{c,1987}$. The regressor of interest is the instrument, the one-year-lagged historical-share-by-push predictor $Z_{c,t-1}$. Column (1) includes state-by-year fixed effects only. Column (2) additionally includes the predetermined district control set: the 1987 divorce rate, share single in 1987, log population density in 1987, in-commuting rate in 1987, the 1987 sex ratio among young adults, the 1987 unemployment rate, the Catholic share in 1970, the young-adult share in 1987, and female employment share in 1970. Column (3) includes district fixed effects and state-by-year fixed effects. Time-invariant baseline controls are absorbed by district fixed effects and are therefore not included separately in column (3). Standard errors are clustered at the district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

This sign reversal is substantively important. It indicates that districts with stronger historical East-linked settlement shares differ from other districts in baseline characteristics that matter for divorce levels. For this reason, the preferred specification conditions on predetermined district characteristics, including the 1987 divorce rate. In the controlled and district-fixed-effects specifications, the reduced form supports the main interpretation: districts with higher predicted exposure to young East–West migration experienced lower post-reunification divorce rates.

Taken together, Tables 2 and 3 establish the two empirical components behind the IV estimates. The instrument strongly predicts cumulative young-female migration exposure, and, conditional on baseline district characteristics or district fixed effects, it is associated with lower divorce rates in the reduced form. The remaining sections examine the corresponding 2SLS estimates, heterogeneity, mechanisms, and robustness checks.

5.1 Main findings

Table 4 reports the main relationship between cumulative exposure to imputed young female migrants from East Germany and district-level divorce outcomes in West Germany after reunification. The dependent variable is the annual district divorce count scaled by 1987 population. The table is organized around the identifying variation. Columns (1)–

(3) report OLS estimates, while columns (4)–(6) report 2SLS estimates. Within each estimator, the first column absorbs state-by-year shocks, the second adds the baseline district controls, and the third adds district fixed effects in addition to state-by-year fixed effects. I do not report specifications with neither fixed effects nor controls, since they are not informative about the relevant identifying variation.

Table 4: Young-female East–West migration exposure and district divorce outcomes

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged young-female exposure	0.006	-0.004	-0.022***	0.056***	-0.066***	-0.108***
	(0.007)	(0.006)	(0.008)	(0.017)	(0.014)	(0.017)
Baseline district controls		✓			✓	
District fixed effects			✓			✓
State × year fixed effects	✓	✓	✓	✓	✓	✓
Observations	5,120	5,120	5,120	5,120	5,120	5,120
Model F-statistic	0.960	31.590	7.850	10.850	28.140	40.720
R^2	0.342	0.542	0.715	-0.076	0.202	-0.129
Kleibergen–Paap F				41.61	47.14	63.73

Notes: The dependent variable is the annual district divorce count scaled by 1987 population: $1000 \times D_{ct}/N_{c,1987}$. Columns (1)–(3) report OLS estimates. Columns (4)–(6) report 2SLS estimates in which lagged cumulative imputed young-female East–West migration exposure is instrumented with the one-year-lagged historical-share-by-push instrument, formed by interacting each district's historical East-linked settlement share with the cumulative female-weighted East German labor-market push component. Columns (1) and (4) include state-by-year fixed effects only. Columns (2) and (5) additionally include the predetermined district control set: the 1987 divorce rate, share single in 1987, log population density in 1987, in-commuting rate in 1987, the 1987 sex ratio among young adults, the 1987 unemployment rate, the Catholic share in 1970, the young-adult share in 1987, and female employment share in 1970. Columns (3) and (6) include district fixed effects and state-by-year fixed effects. Time-invariant baseline controls are absorbed by district fixed effects and are therefore not included separately in these columns. Standard errors are clustered at the district level. The Kleibergen–Paap rk Wald F-statistic is reported for IV specifications. The centered second-stage R^2 reported for IV columns can be negative and is not directly comparable to OLS R^2. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The OLS results show little evidence of a negative relationship until district fixed effects are added. With state-by-year fixed effects only, the coefficient is positive but statistically insignificant. Adding the baseline control set changes the point estimate to -0.004, but it remains small and statistically insignificant. With district fixed effects and state-by-year fixed effects, the OLS coefficient becomes negative, -0.022, and statistically significant. This pattern is consistent with the idea that simple conditional correlations are sensitive to how pre-existing district differences are handled.

The IV estimates are larger in magnitude. With state-by-year fixed effects only, the coefficient is positive, 0.056, and statistically significant. Once the predetermined district controls are added, however, the estimate becomes negative and statistically significant: the preferred controlled IV coefficient is -0.066. This means that an additional unit of lagged cumulative young-female migration exposure, measured per 1,000 baseline residents, is associated with about 0.066 fewer annual divorces per 1,000 residents in the fixed 1987 population. The corresponding first-stage diagnostic remains strong, with a Kleibergen–Paap F-statistic of 47.14.

The district-fixed-effects IV specification yields an even larger negative coefficient, -0.108, with a Kleibergen–Paap F-statistic of 63.73. This specification absorbs all time-invariant district heterogeneity and identifies the effect from within-district changes in predicted cumulative exposure relative to other districts in the same state-year. I interpret it as an important robustness check rather than as the sole benchmark specification, because it is more demanding and shifts the identifying variation toward within-district changes in cumulative exposure.

The sign reversal between the state-year-only IV estimate and the controlled IV estimate is important. It indicates that districts with stronger historical East-linked settlement shares differed systematically in baseline characteristics that are relevant for divorce outcomes. Conditioning on the 1987 divorce rate and other predetermined district characteristics is therefore central to the preferred specification. Once those differences are accounted for, predicted exposure to young East–West migration is associated with lower district-level divorce rates.

The magnitude of the preferred estimate is economically meaningful. The standard deviation of lagged cumulative young-female migration exposure in the estimation sample is about four migrants per 1,000 baseline residents. Multiplying this by the preferred coefficient implies that a one-standard-deviation increase in predicted exposure is associated with roughly 0.26 fewer annual divorces per 1,000 residents. Relative to the sample mean of about 2.52 annual divorces per 1,000 fixed baseline residents, this corresponds to roughly 10 percent of mean divorce rates. This magnitude is useful for interpretation, but it should not be read as evidence for a pure net-sex-ratio channel. Rather, it constrains that interpretation: the estimated response is large relative to the likely mechanical change in local sex ratios, suggesting that broader young-migration exposure or other receiving-market adjustments may be involved.

Substantively, the sign of the preferred IV estimate matters. The results do not support a simple destabilization story in which female-skewed inflows increase divorce by expanding outside options and destabilizing existing unions. Instead, the preferred estimates are consistent with a receiving-market interpretation in which young East–West migration changed local partner-market conditions in ways associated with lower marital dissolution. At the same time, the treatment is measured in imputed young-female units, and the design does not separately identify a purely female-specific effect. The estimate should therefore be interpreted as the effect of predicted young East–West migration exposure, expressed in imputed young-female units, on district-level divorce rates.

5.2 Heterogeneity

I next examine whether the estimated effect varies across local environments. I focus on two dimensions tied to the proposed receiving-market mechanism: district type and proximity to the former inner-German border. The heterogeneity tables follow the same specification logic as the main-results table. The dependent variable is the annual district divorce count scaled by 1987 population, $1000 \times D_{ct}/N_{c,1987}$. For each subgroup, I report OLS and 2SLS estimates under three specifications: state-by-year fixed effects only, state-by-year fixed effects plus predetermined controls, and district fixed effects plus state-by-year fixed effects.

These split-sample estimates should be interpreted as secondary evidence. They are useful for characterizing where the baseline effect is strongest, but they are not as clean as the pooled specification because sample sizes fall and first-stage strength varies across subgroups. In particular, whenever the Kleibergen–Paap statistic falls below conventional comfort thresholds, the corresponding IV coefficient should be read as suggestive rather than load-bearing.

Table 5 reports separate estimates for urban-core districts and rural-type districts. The urban-core estimates provide no clear evidence of a negative effect. The OLS coefficients are small and statistically insignificant across all specifications. The IV estimates are also imprecise, and the first stage is weaker than in the pooled sample. The Kleibergen–Paap statistics range from 6.65 to 10.95, below the conventional 16.38 benchmark for rejecting weak identification at the 10 percent maximal-size threshold.

The rural-type estimates are more informative. In the state-year fixed-effects specification without controls, the IV coefficient is positive and statistically significant. After adding the predetermined control vector, the coefficient becomes negative, -0.027, and statistically significant. In the district-fixed-effects specification, the coefficient is also negative and statistically significant, -0.063. Thus, the negative rural-type effect is visible both in the preferred controlled state-year specification and in the more demanding within-district specification, with strong first stages in both cases.

This pattern is broadly consistent with the idea that local marriage-market effects may be more relevant outside large urban cores, but it should not be overstated. The district-type split does not by itself prove the mechanism under the level-scaled outcome. The safer conclusion is that urban-core estimates are too imprecise to be informative, while rural-type districts show a negative response in the controlled state-year specification and in the district-fixed-effects specification.

Table 6 reports the same specification sequence by distance to the former inner-German border. I divide districts into three groups: within 100 km, between 100 and 200 km, and above 200 km. The near-border sample provides the clearest heterogeneity evidence. The OLS estimates are small in the state-year specifications, but the district-

Table 5: Heterogeneity by district type

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Urban-core districts						
Lagged young-female exposure	-0.014	-0.011	-0.006	-0.015	-0.024	0.035
	(0.012)	(0.011)	(0.012)	(0.039)	(0.034)	(0.046)
Kleibergen–Paap F				6.65	10.95	8.15
Observations	1,376	1,376	1,376	1,376	1,376	1,376
Panel B. Rural-type districts						
Lagged young-female exposure	0.013	0.005	-0.002	0.075***	-0.027**	-0.063***
	(0.008)	(0.007)	(0.009)	(0.024)	(0.013)	(0.020)
Kleibergen–Paap F				35.62	50.56	41.22
Observations	3,744	3,744	3,744	3,744	3,744	3,744
Baseline district controls		✓			✓	
District fixed effects			✓			✓
State × year fixed effects	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is the annual district divorce count scaled by 1987 population: $1000 \times D_{ct}/N_{c,1987}$. Columns (1)–(3) report OLS estimates. Columns (4)–(6) report 2SLS estimates in which lagged cumulative imputed young-female East–West migration exposure is instrumented with the lagged historical-share-by-female-push instrument. Urban-core districts are defined as *kreisfreie Städte*; rural-type districts are *Landkreise*. Columns (1) and (4) include state-by-year fixed effects only. Columns (2) and (5) additionally include the predetermined district control vector, including the 1987 divorce rate. Columns (3) and (6) include district fixed effects and state-by-year fixed effects; time-invariant controls are absorbed by district fixed effects and are therefore not included separately. Standard errors are clustered at the district level. Kleibergen–Paap F statistics are reported for IV columns. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

fixed-effects OLS estimate is negative and statistically significant. The IV estimates become negative once controls or district fixed effects are included. The controlled state-year IV estimate is -0.048 and statistically significant, with a Kleibergen–Paap statistic of 19.20. The district-fixed-effects estimate is larger in magnitude, -0.081, and statistically significant, although its Kleibergen–Paap statistic of 15.19 is slightly below the 10 percent maximal-size benchmark. This pattern is substantively plausible: migration frictions were likely lower closer to the East, and historical East–West settlement ties may have been more relevant in these districts.

The middle-distance group also shows negative IV estimates once controls or district fixed effects are included, but the evidence is less stable. The controlled state-year IV estimate is large and negative, -0.298, but the first stage is weak, with a Kleibergen–Paap statistic of 6.44. The district-fixed-effects IV estimate remains large and negative, -0.212, and is supported by a stronger first stage. Because the magnitudes are large and the controlled state-year first stage is weak, I interpret this group as suggestive rather than central.

The far-distance group does not provide credible evidence of a stable negative effect. The OLS estimates are positive and statistically insignificant across specifications. The IV estimates vary in sign: the state-year-only IV estimate is positive, the controlled state-year estimate is negative but statistically insignificant, and the district-fixed-effects estimate is negative but only marginally significant. The Kleibergen–Paap statistics are below conventional comfort thresholds in all three IV specifications. These estimates should therefore be interpreted cautiously.

Overall, the distance results do not establish a clean monotonic gradient. The safer conclusion is that the negative effect is most credibly identified in districts within 100 km of the former inner-German border. The middle-distance estimates are negative in the controlled and district-fixed-effects IV specifications but less stable, while the far-distance estimates are weakly identified and not robust.

The heterogeneity evidence is therefore more limited than the baseline results. The district-type split suggests that the negative response is more visible in rural-type districts than in urban-core districts, but this should be interpreted descriptively. The border-distance split is more informative: the negative effect is most credible near the former inner-German border, where the controlled IV estimate is negative and the first stage remains relevant. These results are best treated as supportive descriptive evidence, not as independent proof of the receiving-market mechanism.

5.3 Mechanism evidence

I next examine whether the aggregate divorce response appears in margins consistent with a receiving-market marriage-market interpretation. These exercises are not separate causal designs. They decompose the aggregate divorce response into observable behavioral margins: who files for divorce, whether joint filings respond, and whether marriage formation changes alongside the decline in divorce. They do not directly identify deeper mechanisms such as search frictions, remarriage probabilities, partner-market tightness, or intra-household bargaining. The evidence is therefore suggestive rather than definitive. I use the preferred controlled state-by-year specification for these decompositions in order to keep the identifying variation comparable to the main benchmark estimate. District-fixed-effects versions are more demanding and are better interpreted as robustness checks, especially because the filing-side outcomes are available for a smaller and selected set of district-years.

Table 7 reports filing-side decompositions and marriage responses. To remain consistent with the main outcome, all outcomes are scaled by fixed 1987 population. The first column reports the aggregate divorce outcome from the preferred main specification, while columns (2)–(4) decompose divorce filings into man-filed, woman-filed, and

Table 6: Heterogeneity by distance to the former inner-German border

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Within 100 km						
Lagged young-female exposure	0.007	-0.010	-0.030***	0.021	-0.048***	-0.081***
	(0.009)	(0.008)	(0.011)	(0.017)	(0.016)	(0.019)
Kleibergen–Paap F				9.90	19.20	15.19
Observations	1,360	1,360	1,360	1,360	1,360	1,360
Panel B. 100–200 km						
Lagged young-female exposure	0.010	0.005	-0.040**	0.094	-0.298**	-0.212***
	(0.013)	(0.013)	(0.016)	(0.058)	(0.127)	(0.054)
Kleibergen–Paap F				11.66	6.44	18.11
Observations	2,000	2,000	2,000	2,000	2,000	2,000
Panel C. Above 200 km						
Lagged young-female exposure	0.022	0.003	0.015	0.203**	-0.047	-0.146*
	(0.015)	(0.014)	(0.016)	(0.090)	(0.056)	(0.075)
Kleibergen–Paap F				8.28	6.34	10.25
Observations	1,744	1,744	1,744	1,744	1,744	1,744
Baseline district controls		✓			✓	
District fixed effects			✓			✓
State × year fixed effects	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is the annual district divorce count scaled by 1987 population: $1000 \times D_{ct}/N_{c,1987}$. Columns (1)–(3) report OLS estimates. Columns (4)–(6) report 2SLS estimates in which lagged cumulative imputed young-female East–West migration exposure is instrumented with the lagged historical-share-by-female-push instrument. Distance bins are defined using each district's distance to the former inner-German border. Columns (1) and (4) include state-by-year fixed effects only. Columns (2) and (5) additionally include the predetermined district control vector, including the 1987 divorce rate. Columns (3) and (6) include district fixed effects and state-by-year fixed effects; time-invariant controls are absorbed by district fixed effects and are therefore not included separately. Standard errors are clustered at the district level. Kleibergen–Paap F statistics are reported for IV columns. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

joint-filed divorces. Column (5) reports marriage formation. All specifications include the predetermined district control vector and state-by-year fixed effects.

The mechanism evidence is weaker than the aggregate result. Column (1) reproduces the preferred aggregate IV pattern: predicted young-female migration exposure is associated with lower divorce rates. The coefficient is -0.066 and statistically significant. However, the filing decomposition does not show that this aggregate decline is concentrated in unilateral filings. The IV coefficient for man-filed divorces is essentially zero. The coefficient for woman-filed divorces is negative, -0.017, but statistically insignificant. Thus, the filing data do not provide evidence of a statistically clear unilateral filing response.

Table 7: Behavioral margins of adjustment

	Total divorces (1)	Man-filed (2)	Woman-filed (3)	Joint-filed (4)	Marriages (5)
Panel A. OLS					
Lagged young-female exposure	-0.004	-0.000	0.000	-0.007	0.033
	(0.006)	(0.004)	(0.007)	(0.006)	(0.025)
Panel B. 2SLS					
Lagged young-female exposure	-0.066***	-0.000	-0.017	-0.039***	0.010
	(0.014)	(0.008)	(0.013)	(0.014)	(0.041)
Kleibergen–Paap F	47.14	25.38	25.38	25.38	39.67
Observations	5,120	2,487	2,487	2,487	1,536
Baseline district controls	✓	✓	✓	✓	✓
State × year fixed effects	✓	✓	✓	✓	✓

Notes: Each column reports a separate regression. Outcomes are scaled by fixed 1987 population. Column (1) uses the annual district divorce count scaled by 1987 population, $1000 \times D_{ct}/N_{c,1987}$. Columns (2)–(4) use man-filed, woman-filed, and joint-filed divorce counts per 1,000 residents in 1987. Column (5) uses marriages per 1,000 residents in 1987. The treatment variable is lagged cumulative imputed young-female East–West migration exposure, measured per 1,000 baseline residents. Panel A reports OLS estimates. Panel B reports 2SLS estimates in which lagged migration exposure is instrumented with the lagged historical-share-by-female-push instrument. All specifications include the predetermined district control vector, including the 1987 divorce rate, and state-by-year fixed effects. Standard errors are clustered at the district level. The filing decomposition is estimated on a smaller sample because filing subcomponents are not observed for all district-years. Kleibergen–Paap F statistics are reported for IV specifications. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Instead, the only filing component with a clear negative IV coefficient is joint-filed divorce. The coefficient is -0.039 and statistically significant. This changes the mechanism interpretation. The filing decomposition is not consistent with a simple one-sided bargaining story in which the effect operates mainly through reduced woman-filed divorces. Rather, the observed decline in the filing subsample appears to be driven more by jointly filed divorces, with weaker and imprecise responses in the man-filed and woman-filed margins.

The marriage column further narrows the interpretation. If female-skewed migration reduced divorce mainly by increasing marriage formation, one would expect a positive and precisely estimated IV response in marriage counts. I do not find such evidence. The OLS marriage coefficient is positive but statistically insignificant, and the IV coefficient is smaller and also statistically insignificant. This weakens a pure marriage-entry explanation and points instead toward reduced marital dissolution as the relevant aggregate margin.

These results should be interpreted cautiously. First, the filing and marriage outcomes are available for much smaller and selected samples than the aggregate divorce outcome. Second, the filing decomposition does not map cleanly into deeper mechanisms such as bargaining power, remarriage expectations, or search frictions. Third, the results do not support the interpretation that the response is concentrated in unilateral filing margins. The safer conclusion is that the aggregate decline in divorce is robust, but the available filing data do not identify a precise behavioral channel. The mechanism evidence should therefore be read as descriptive and supportive at most, not as load-bearing identification evidence.

5.4 Robustness checks

I next assess whether the main negative IV estimate is sensitive to the most relevant specification choices. Table 8 reports controlled 2SLS estimates that build on the preferred state-by-year fixed-effects specification. The first column repeats the benchmark estimate from Table 4. The remaining columns vary the outcome normalization, add additional spatial controls, change the inference procedure, weight districts by baseline population, or collapse the panel into a long-difference specification. The purpose of this table is not to exhaust all possible variants, but to check whether the main result survives the most important threats to interpretation.

The negative IV estimate is robust to changing how the divorce outcome is normalized. Column (2) replaces the benchmark level-scaled outcome with a pre-period-normalized outcome that subtracts each district's mean divorce count over 1985–1988 before scaling by 1987 population. The coefficient becomes larger in absolute value, −0.083, and remains precisely estimated. This suggests that the benchmark result is not driven by using annual divorce rates in levels scaled by fixed baseline population rather than by measuring divorce rates relative to a pre-reunification baseline.

The result is also robust to absorbing additional spatially structured post-reunification shocks. Column (3) adds distance-bin-by-year fixed effects, which flexibly absorb time-varying shocks common to districts at similar distances from the former inner-German border. The coefficient remains negative and statistically significant, −0.085, with a Kleibergen–Paap statistic of 29.23. This reduces the concern that the main estimate is simply capturing border-distance-specific dynamics after reunification.

The inference and weighting checks point in the same direction. Column (4) clusters standard errors two ways by district and year. The coefficient remains −0.066 and statistically significant. Column (5) weights districts by 1987 population and produces a somewhat larger estimate, −0.078, which is also statistically significant. These checks indicate that the result is not driven by the choice of district-only clustered standard errors or by small districts receiving equal weight.

Finally, column (6) reports a long-difference specification. This specification collapses the data to the district level and compares mean divorce rates in 2005–2007 with mean divorce rates in 1985–1987. The estimate remains negative and statistically significant,

Table 8: Robustness of the main 2SLS estimate

Panel A. Alternative outcome normalization and spatial controls			
	Benchmark outcome (1)	Pre-period baseline (2)	Distance-bin × year FE (3)
Lagged young-female exposure	-0.066***	-0.083***	-0.085***
	(0.014)	(0.016)	(0.020)
Kleibergen–Paap F	47.14	47.14	29.23
Observations	5,120	5,120	5,120
State × year fixed effects	✓	✓	✓
Baseline district controls	✓	✓	✓
Distance-bin × year fixed effects			✓
Panel B. Inference, weighting, and long-difference specification			
	Two-way clustering (4)	Population weighted (5)	Long diff. 2005–07 (6)
Lagged young-female exposure	-0.066***	-0.078***	-0.037***
	(0.017)	(0.017)	(0.014)
Kleibergen–Paap F	50.31	49.49	39.68
Observations	5,120	5,120	320
State × year fixed effects	✓	✓	
Baseline district controls	✓	✓	✓
Two-way clustered standard errors	✓		
Population weights		✓	
State fixed effects			✓

Notes: The endogenous regressor is lagged cumulative imputed young-female East–West migration exposure per 1,000 baseline residents. The instrument is the lagged historical-share-by-push instrument. Column (1) repeats the preferred controlled IV specification, where the dependent variable is the annual district divorce count scaled by 1987 population, $1000 \times D_{ct}/N_{c,1987}$. Column (2) uses an alternative baseline-normalized outcome that subtracts the district's mean divorce count in 1985–1988 before scaling by 1987 population. Column (3) adds distance-bin-by-year fixed effects. Column (4) clusters standard errors two ways by district and year. Column (5) weights by 1987 population. Column (6) reports a district-level long-difference IV specification comparing mean divorce counts in 2005–2007 to mean divorce counts in 1985–1987; it includes state fixed effects and the baseline control vector. The baseline district control vector includes the 1987 divorce rate. Standard errors are clustered at the district level except in column (4), where they are clustered by district and year, and in column (6), where robust standard errors are reported. Kleibergen–Paap F statistics are reported for all IV specifications. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

with a coefficient of -0.037. The long-difference specification sacrifices annual panel variation, but it provides a transparent comparison between pre-reunification divorce rates and late post-reunification divorce rates using the same broad source of instrumented exposure.

Overall, the robustness checks support the stability of the negative sign. The result survives an alternative pre-period-normalized outcome, distance-bin-by-year fixed effects, two-way clustering, population weighting, and a long-difference specification. The coefficient varies across specifications, so I do not interpret the estimate as a precise structural parameter. The robust conclusion is narrower: predicted exposure to young East–West migration, expressed in imputed young-female units, is consistently associated with lower district-level divorce rates in the post-reunification period.

5.5 Pretrend and trend-adjustment checks

The central identifying concern is not instrument relevance. The first-stage estimates are strong. The main threat is the exclusion restriction: historical East-linked settlement shares may be correlated with other persistent district characteristics that affected divorce trajectories after reunification. I therefore report three validity-oriented checks. First, I test whether the historical settlement share predicts differential pre-reunification divorce-rate trends. Second, I control directly for pre-reunification divorce trends and baseline divorce dynamics in the post-reunification IV specification. Third, I estimate a demanding specification with district-specific linear trends.

Panel A shows that the clean pretrend test does not reject differential pre-reunification trends. The interactions between the historical East-linked settlement share and the 1985–1987 year indicators are jointly insignificant, with $p = 0.233$. This is important because the design relies on historical East-linked settlement shares being unrelated to counterfactual divorce trajectories after conditioning on state-year shocks and baseline district characteristics. The 1986 interaction is marginally significant, so I do not interpret the pre-period as perfectly flat. The correct statement is narrower: the joint test does not reject differential pre-reunification divorce-rate trends.

Panel B shows that the benchmark estimate remains negative after allowing for additional baseline divorce dynamics. Adding the baseline divorce-level and pretrend controls reduces the coefficient from -0.0657 to -0.0352, but the estimate remains statistically significant and the first stage remains strong, with a Kleibergen–Paap statistic of 37.06. This suggests that the benchmark result is not solely driven by observable differences in pre-reunification divorce levels or simple pre-period divorce trends.

The district-specific linear-trend specification is much more demanding. Once every district is allowed to follow its own linear trend, the coefficient becomes close to zero and statistically insignificant. This result should be interpreted carefully. Because the treatment is a cumulative exposure stock that evolves gradually after reunification, district-specific trends absorb much of the variation that identifies the baseline effect. Nevertheless, the result is an important limitation: the negative estimate is not robust to arbitrary district-specific linear trend adjustment.

These checks improve the credibility of the design, but they do not eliminate all

Table 9: Pretrend and trend-adjustment checks

Panel A. Clean pretrend test, 1985–1988			
	Dependent variable: divorce rate per 1,000 residents		
	1985	1986	1987
	(1)	(2)	(3)
Historical share × year	2.934	2.992*	0.326
	(1.838)	(1.728)	(1.409)
Joint test of pretrend interactions	$F(3, 319) = 1.43$, $p = 0.233$		
Observations	1,280		
District fixed effects	✓		
State × year fixed effects	✓		
Baseline controls × year	✓		

Panel B. IV validity-oriented robustness checks			
	Benchmark IV	Add baseline divorce trends	District-specific linear trends
	(4)	(5)	(6)
Lagged young-female exposure	-0.0657***	-0.0352***	0.0070
	(0.0137)	(0.0134)	(0.0586)
Kleibergen–Paap F	47.14	37.06	43.11
Observations	5,120	5,120	5,120
Baseline district controls	✓	✓	
State × year fixed effects	✓	✓	✓
District fixed effects			✓
District-specific linear trends			✓
Baseline divorce level/trend controls		✓	

Notes: Panel A reports a pre-reunification falsification test. The dependent variable is the district divorce rate per 1,000 residents. The sample is restricted to 1985–1988. The reported coefficients are interactions between the historical East-linked settlement share and year indicators; 1988 is omitted as the reference year. The specification absorbs district fixed effects and state-by-year fixed effects and interacts the baseline control vector with year indicators. Panel B reports IV estimates for the main post-reunification outcome, $1000 \times D_{ct}/N_{c,1987}$. Column (4) repeats the benchmark controlled IV specification. Column (5) adds the 1987 divorce rate and interactions between baseline divorce/pretrend measures and a post-1991 linear trend. Column (6) absorbs district fixed effects, state-by-year fixed effects, and district-specific linear trends. Standard errors are clustered at the district level. Kleibergen–Paap F statistics are reported for all IV specifications. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

exclusion-restriction concerns. They support a conditional identification claim: after absorbing state-year shocks, predetermined district characteristics, and observed pre-reunification divorce dynamics, the historical-share-by-female-push instrument predicts young East–West migration exposure and is associated with lower divorce rates. At the same time, the loss of precision under district-specific linear trends means the results should be interpreted as evidence from a historical-share exposure design, not as proof from quasi-random assignment of historical settlement shares.

6 Conclusion

This paper studies how young East–West migration after German reunification affected marital stability in receiving West German districts. The empirical setting is useful because reunification generated a large and demographically distinctive internal migration episode. The inflow was female-skewed among young adults, and receiving districts differed in their predicted exposure because of historically rooted East-linked settlement patterns. I combine district-level administrative divorce outcomes with an imputed measure of cumulative young-female East–West migration exposure and a historical-share-by-push instrument based on 1961 East-linked settlement shares.

The main empirical result is that districts with greater predicted exposure to young East–West migration, expressed in imputed young-female units, experienced lower district-level divorce rates in the preferred controlled IV specification and across several robustness checks. In the preferred controlled IV specification, the dependent variable is the annual district divorce count scaled by fixed 1987 population, and the control set includes the district's 1987 divorce rate. The estimated coefficient is negative, statistically significant, and economically meaningful: an additional unit of lagged cumulative young-female migration exposure is associated with about 0.066 fewer annual divorces per 1,000 residents in the fixed 1987 population. The first stage is strong, and the negative sign is robust to a pre-period-normalized outcome, distance-bin-by-year fixed effects, two-way clustering, population weighting, and a long-difference specification.

The sign of the effect is substantively important. The results do not support a simple destabilization story in which female-skewed inflows mechanically increase divorce by expanding men's outside options. The preferred estimates point in the opposite direction: districts more exposed to predicted young East–West migration experienced lower divorce rates. This pattern is consistent with a receiving-market marriage-market interpretation in which female-skewed inflows changed local partner-market conditions in ways that lowered the expected gains from marital dissolution. At the same time, the treatment is measured in imputed young-female units, and the design does not separately identify a purely female-specific effect.

The heterogeneity results are suggestive rather than definitive. The district-type split suggests that the negative response is more visible in rural-type districts than in urban-core districts. Urban-core estimates are too imprecise to be informative, while rural-type districts show a negative response in both the controlled state-year specification and the district-fixed-effects specification. The distance-to-border split is more informative: the negative effect is most credible among districts closer to the former inner-German border, where migration frictions were likely lower and historical East–West settlement ties may have been more relevant. However, these subgroup results should be read as descriptive evidence, not as independent proof of the mechanism.

The mechanism evidence is likewise limited. The filing decomposition does not support the interpretation that the divorce decline is concentrated in unilateral filings. In the preferred filing-sample IV specification, the man-filed response is essentially zero, and the woman-filed response is negative but statistically insignificant. The clearest negative filing component is instead joint-filed divorces. Marriage outcomes also do not show a robust positive response. These results weaken a narrow one-sided bargaining interpretation. The mechanism evidence is therefore best summarized as follows: the aggregate divorce decline is robust, but the available filing and marriage data do not identify the precise behavioral channel.

The main caveat remains identification. The first stage is strong, and the reduced-form relationship has the expected sign in the controlled and district-fixed-effects specifications. However, the reduced form should not be interpreted as a test of the exclusion restriction. It shows that the component of migration exposure predicted by the instrument is associated with divorce outcomes, but the validity of the instrument still depends on the assumption that historical East-linked settlement shares, interacted with the East German female-weighted labor-market push, affect divorce only through migration exposure. The pretrend checks and additional baseline-trend controls reduce this concern, but they do not eliminate it. The district-specific linear-trend specification is particularly important: once every district is allowed to follow its own linear trend, the estimate becomes close to zero and statistically insignificant. This does not necessarily invalidate the design, since cumulative exposure measures leave limited identifying variation after district-specific trends are absorbed, but it is an important limitation.

The paper therefore makes a conditional and design-based claim. Given state-year shocks, predetermined district characteristics, baseline divorce levels, and observed pre-reunification divorce dynamics, districts with greater predicted exposure to young East–West migration experienced lower post-reunification divorce rates. The estimates should be interpreted as local effects of predicted exposure generated by the historical-share-by-push instrument, not as individual-level effects of each migrant and not as the average effect of all migration flows.

The broader implication is that internal migration can shape family outcomes through demographic composition and partner-market adjustment, not only through wages, employment, or aggregate population size. In the German reunification setting, East–West migration did not merely reallocate labor across space. It also changed the demographic structure of receiving localities, and this demographic margin appears to have mattered for marital stability. Even with the identification caveats, the evidence points to a stabilizing association between predicted exposure to young, female-skewed East–West migration and divorce rates in receiving West German districts.

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A Additional Tables and Figures

A.1 Expanded regression tables

This appendix reports expanded versions of the compact regression tables shown in the main text. The purpose is transparency. The main-text tables emphasize the coefficient of interest, fixed-effects structure, and weak-instrument diagnostics. The appendix tables report the full coefficient vector for the predetermined district controls where applicable. These control coefficients are useful for documenting the conditional comparisons underlying the preferred specification, but they should not be interpreted causally.

The expanded tables follow the same specification logic as the main text. The preferred controlled specification includes state-by-year fixed effects and a predetermined district control vector. District-fixed-effects specifications are reported in the main text as demanding robustness checks, but they do not include the time-invariant baseline controls separately because those controls are absorbed by district fixed effects. The appendix therefore focuses on the specifications in which the control coefficients are estimable and interpretable as conditioning variables.

Table 10 confirms that the instrument is strongly predictive both of observed annual East–West inflow intensity and of the lagged cumulative young-female migration exposure used in the second stage. The first-stage coefficient is positive and statistically significant across all specifications. Adding the predetermined district control vector does not weaken the first stage; the first-stage diagnostics remain comfortably above conventional weak-instrument thresholds. The control coefficients describe conditional allocation patterns and should not be interpreted as causal effects of baseline district characteristics on migration.

Table 11 makes the preferred controlled specification fully transparent. In OLS, the association between lagged young-female migration exposure and divorce activity is close to zero once state-by-year fixed effects and predetermined district controls are included. In 2SLS, the uncontrolled state-year specification is positive, but the preferred controlled specification is negative and statistically significant. This sign reversal is important because it shows that districts with stronger historical East-linked exposure differ in baseline characteristics that matter for divorce activity. The preferred interpretation therefore relies on the controlled specification, which compares districts within the same state-year after conditioning on pre-reunification divorce levels and other predetermined district characteristics. The control coefficients should be read only as conditioning relationships, not as causal estimates.

Table 12 shows the source of the sign change in the IV estimates. With state-by-year fixed effects only, the reduced-form relationship between the instrument and divorce activity is positive. After adding predetermined district controls, the reduced form becomes

Table 10: Expanded first-stage estimates with full baseline controls

	Construction check		Main first stage	
	Annual total East–West inflow intensity		Lagged cumulative young-female exposure	
	State-year FE	+ controls	State-year FE	+ controls
	(1)	(2)	(3)	(4)
Instrument	11.324***	14.126***	30.222***	34.820***
	(1.435)	(1.783)	(4.685)	(5.072)
1987 divorce rate		342.890**		581.614
		(146.040)		(352.539)
Share single, 1987		-0.082		6.383
		(3.928)		(7.727)
Log population density, 1987		-0.480***		-0.873***
		(0.140)		(0.272)
In-commuting rate, 1987		0.872		1.153
		(0.715)		(1.704)
Sex ratio among young adults, 1987		-0.355		0.623
		(1.300)		(3.391)
Unemployment rate, 1987		0.003		6.430
		(4.687)		(8.249)
Catholic share, 1970		-0.394		-1.989***
		(0.311)		(0.716)
Share of young adults in population, 1987		5.360		13.983
		(4.406)		(8.876)
Female employment share, 1970		5.962***		11.958***
		(2.093)		(4.317)
Constant	1.742***	0.606	2.682***	-4.246
	(0.104)	(2.011)	(0.323)	(4.777)
Baseline district controls		✓		✓
State × year fixed effects	✓	✓	✓	✓
Observations	5,120	5,120	5,120	5,120
District clusters	320	320	320	320
R^2	0.4450	0.4953	0.7015	0.7324
Instrument F statistic	62.23	62.76	41.61	47.14

Notes: Columns (1)–(2) report construction-check first-stage regressions in which the dependent variable is annual total East–West inflow intensity per 1,000 baseline 1988 residents. Columns (3)–(4) report the main first-stage regressions in which the dependent variable is lagged cumulative imputed young-female East–West migration exposure per 1,000 baseline 1988 residents, the endogenous regressor used in the 2SLS specification. The instrument is the one-year-lagged historical-share-by-push predictor formed by interacting each district's historical East-linked settlement share with the lagged cumulative East German female-sector push component. Columns (2) and (4) additionally include the predetermined district control set shown in the table. All specifications include state-by-year fixed effects and use district-clustered standard errors. The reported F-statistics are cluster-robust tests of the excluded instrument. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

negative and statistically significant. This supports the decision to use the controlled specification as the preferred benchmark and highlights why raw historical exposure variation should not be interpreted without conditioning on baseline district characteristics.

Table 11: Expanded OLS and 2SLS estimates with full baseline controls

	OLS		2SLS	
	State-year FE (1)	+ controls (2)	State-year FE (3)	+ controls (4)
Lagged young-female exposure	0.006	-0.004	0.056***	-0.066***
	(0.007)	(0.006)	(0.017)	(0.014)
1987 divorce rate		431.357***		479.918***
		(54.743)		(63.563)
Share single, 1987		-2.139*		-2.616*
		(1.248)		(1.375)
Log population density, 1987		-0.014		-0.038
		(0.031)		(0.035)
In-commuting rate, 1987		-0.690***		-0.420
		(0.252)		(0.318)
Sex ratio among young adults, 1987		-1.597***		-1.548***
		(0.454)		(0.521)
Unemployment rate, 1987		-3.929***		-4.703***
		(1.163)		(1.291)
Catholic share, 1970		-0.004		-0.151
		(0.077)		(0.094)
Share of young adults in population, 1987		1.693		2.938*
		(1.476)		(1.611)
Female employment share, 1970		-2.502***		-1.453**
		(0.649)		(0.656)
Constant	2.487***	5.166***		
	(0.038)	(0.713)		
Baseline district controls		✓		✓
State × year fixed effects	✓	✓	✓	✓
Observations	5,120	5,120	5,120	5,120
District clusters	320	320	320	320
Model F-statistic	0.960	31.590	10.850	28.140
R^2	0.342	0.542	-0.076	0.202
Kleibergen–Paap F			41.61	47.14

Notes: The dependent variable is annual district divorce activity scaled by fixed 1987 population, $1000 \times D_{ct}/P_{c,1987}$. Columns (1)–(2) report OLS estimates. Columns (3)–(4) report 2SLS estimates in which lagged cumulative imputed young-female East–West migration exposure is instrumented with the one-year-lagged historical-share-by-push instrument. All specifications are estimated on the common post-reunification sample, include state-by-year fixed effects, and use district-clustered standard errors. Columns (2) and (4) additionally include the predetermined district control set shown in the table. The Kleibergen–Paap rk Wald F-statistic is reported as the weak-instrument diagnostic under clustered standard errors. The centered second-stage R^2 reported for IV columns can be negative and is not directly comparable to OLS R^2. Constants are not reported in the IV columns because they are partialled out in the absorbed 2SLS specification. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.2 Dynamic reduced-form event-study estimates

This appendix reports the coefficient path underlying the dynamic reduced-form figure. The specification is a reduced-form event study in which annual district divorce activity

Table 12: Expanded reduced-form estimates with full baseline controls

	State-year FE (1)	+ controls (2)
Instrument	1.703***	-2.287***
	(0.454)	(0.409)
1987 divorce rate		441.716***
		(53.188)
Share single, 1987		-3.036**
		(1.254)
Log population density, 1987		0.020
		(0.031)
In-commuting rate, 1987		-0.496*
		(0.267)
Sex ratio among young adults, 1987		-1.589***
		(0.444)
Unemployment rate, 1987		-5.126***
		(1.140)
Catholic share, 1970		-0.021
		(0.074)
Share of young adults in population, 1987		2.020
		(1.446)
Female employment share, 1970		-2.238***
		(0.635)
Constant	2.394***	5.322***
	(0.038)	(0.703)
Baseline district controls		✓
State × year fixed effects	✓	✓
Observations	5,120	5,120
District clusters	320	320
R^2	0.3529	0.5540

Notes: The dependent variable is annual district divorce activity scaled by fixed 1987 population, $1000 \times D_{ct}/P_{c,1987}$. The regressor of interest is the one-year-lagged historical-share-by-push instrument. Both specifications include state-by-year fixed effects and use district-clustered standard errors. Column (2) additionally includes the predetermined district control set shown in the table. The reduced form reports the relationship between the instrument-predicted component of migration exposure and divorce activity; it is not by itself a test of the exclusion restriction. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

scaled by fixed 1987 population is regressed on interactions between the historical East-linked settlement share and year indicators, with 1990 omitted as the reference year. The model includes district fixed effects, state-by-year fixed effects, and the full baseline control set interacted with year indicators. Standard errors are clustered at the district level.

The dynamic reduced-form pattern is broadly consistent with the main IV interpretation. The pre-reunification coefficients are imprecise and jointly insignificant. After reunification, the coefficients become predominantly negative, especially from the mid-

Table 13: Dynamic reduced-form event-study estimates relative to 1990

Year	Coefficient	Std. error
1985	-0.035	(1.932)
1986	1.537	(1.662)
1987	-0.730	(1.498)
1988	-2.074	(1.497)
1989	-0.371	(1.558)
1990	0.000	(omitted)
1991	-0.500	(1.373)
1992	-1.422	(1.391)
1993	-2.170	(1.860)
1994	-4.771***	(1.767)
1995	-5.423***	(1.614)
1996	-3.780*	(2.019)
1997	-4.562**	(2.312)
1998	-2.361	(2.386)
1999	-2.337	(1.816)
2000	-2.761	(2.493)
2001	-2.807	(2.139)
2002	-6.754**	(2.650)
2003	-7.686***	(1.949)
2004	-5.075**	(2.002)
2005	-7.351***	(2.332)
2006	-2.355	(2.428)
2007	-5.437**	(2.639)
Pre-period joint test (1985–1989)	$F = 0.907,\ p = 0.4765$	
Post-period joint test (1991–2007)	$F = 2.274,\ p = 0.0030$	
Observations	7,360	
District clusters	320	
District fixed effects	Yes	
State × year fixed effects	Yes	
Baseline controls × year	Yes	

Notes: The dependent variable is annual district divorce activity scaled by fixed 1987 population, $1000 \times D_{ct}/P_{c,1987}$. Each reported coefficient is the interaction between the historical East-linked settlement share and a year indicator in a regression that omits 1990 as the reference year. The specification includes district fixed effects, state-by-year fixed effects, and the full baseline control set interacted with year indicators. Standard errors are clustered at the district level. The 1990 coefficient is normalized to zero by construction. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

1990s onward. This timing is consistent with the cumulative and lagged nature of the preferred migration exposure measure. The event study should nevertheless be interpreted as supporting evidence on timing and pre-trends, not as a substitute for the main IV estimates.