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2023 **Geghetsik Afunts**

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Abstract

The first chapter of this dissertation investigates whether the introduction of unilateral divorce legislation (UDL), which started in the late 1960s affected the educational structure of marriage. Based on marriage and divorce certificate data covering 1970-1988, we (Štěpán Jurajda and I) provide new evidence on the evolution of the educational structure of marriage inflows (newlyweds) and outflows (divorces). We estimate a difference-in-differences model to gauge the impact of UDL on both of these flows. While UDL did not contribute to rising homogamy (the tendency towards married partners having the same level of education), it did affect the educational structure of marriage: it made generally unstable hypogamous couples (women marrying less educated partners) less likely to divorce, and it made homogamous couples more stable than hypergamous ones (women marrying more educated partners).

The second chapter argues that the changes in family regulations that affect marriage stability could also affect fertility decisions and thereby the fertility differentials of homogamous couples (with the same education level) versus non-homogamous couples. It has been shown that the introduction of joint custody laws (JCLs) in the US affected family decisions, including overall fertility, but there is little research on whether these reforms' effects differ between homogamous and non-homogamous couples. I leverage the staggered introduction of JCLs across US states and employ large administrative data from birth certificates to investigate the reforms' effects on family structure at birth. I find that marginal-free measures of educational assortative matching of parents - the odds of homogamy - increase in states adopting JCLs. The channel that increases the odds of homogamy of parents is a relative increase in births for married homogamous couples rather than a change in the incentive to marry for pregnant couples.

In the third chapter (authored jointly with Misina Cato and Tobias Schmidt) we study the new challenges that Russia's invasion of Ukraine is posing to the global economy, namely the impact on the inflation expectations of individuals, which in turn impacts the inflation rate. We aim to quantify the effect of the invasion on short- and long-term inflation expectations of individuals in Germany. We use microdata from the Bundesbank Online Panel - Households (BOP-HH), for the period from February 15th to March 29th, 2022. Treating the unanticipated start of the war in Ukraine on the 24th of February 2022 as a natural experiment, we find that both short- and long-term inflation expectations increased as an immediate result of the invasion. Long-term inflation expectations increased by around 0.4 percentage points, while the impact on short-term inflation expectations was more than twice as large - around one percentage point. Looking into the possible mechanisms of this increase, we suggest that it can be partially attributed to individuals' fears of soaring energy prices and increasing pessimism about economic trends in general. Our results indicate that large economic shocks can have a substantial impact on both short and long-term inflation expectations.

Abstrakt

První kapitola této disertační práce zjišťuje, zda zavedení unilaterální rozvodové legislativy (UDL), které začalo koncem 60. let, ovlivnilo vzdělanostní strukturu manželství. Na základě údajů z oddacích a rozvodových listů za roky 1970-1988 poskytujeme (já a Štěpán Jurajda) nové důkazy o vývoji vzdělanostní struktury přílivu manželství (novomanželé) a odlivu (rozvody). Odhadujeme model difference-in-difference", abychom změřili dopad UDL na oba tyto toky. I když UDL nepřispěla k nárůstu homogamie (sklon k sezdaným partnerům mívat stejnou úrovneň vzdělání), ovlivnila vzdělávací strukturu manželství: snížila pravděpodobnost rozvodu obecně nestabilních hypogamních párů (ženy, které si vzaly méně vzdělané partnery), a přispěla k větší stabilitě homogamních párů oproti hypergamním (ženy, které si vzaly vzdělanější partnery).

Druhá kapitola tvrdí, že změny v rodinných předpisech, které ovlivňují stabilitu manželství, mohou ovlivnit i rozhodování o plodnosti a tím i rozdíly v plodnosti homogamních párů (se stejnou úrovní vzdělání) oproti nehomogamním párům. Ukázalo se, že zavedení zákonů o společné péči (JCL) v USA ovlivnilo rodinná rozhodnutí, včetně celkové plodnosti, ale existuje jen málo studií, které ověřují, zda se účinky těchto reforem liší mezi homogamními a nehomogamními páry. Využívám postupné zavádění JCL ve všech státech USA a využívám velké administrativní údaje z rodných listů ke zkoumání účinků reforem na rodinnou strukturu při narození. Zjišťuji, že ve státech, které přijímají JCL, se zvyšují bezmezná opatření vzdělávacího různorodého párování rodičů, tedy se zvyšuje pravděpodobnost homogamie. Kanálem, který zvyšuje pravděpodobnost homogamie rodičů, je spíše rela-

tivní nárůst porodů u sezdaných homogamních párů než změna motivace ke sňatku u těhotných párů.

Ve třetí kapitole (napsané společně s Misinou Cato a Tobiasem Schmidtem) studujeme nové výzvy, které ruská invaze na Ukrajinu představuje pro globální ekonomiku, konkrétně dopad na inflační očekávání jednotlivců, což má zase dopad na míru inflace. Naším cílem je kvantifikovat vliv invaze na krátkodobá a dlouhodobá inflační očekávání jednotlivců v Německu. Využíváme mikrodata z Bundesbank Online Panel – Housholds (BOP-HH), za období od 15. února do 29. března 2022. Neočekávaný začátek války na Ukrajině 24. února 2022 považujeme za přirozený experiment, a dokumentujeme, že jak krátkodobá, tak dlouhodobá inflační očekávání se v důsledku invaze zvýšila. Dlouhodobá inflační očekávání se zvýšila zhruba o 0,4procentního bodu, zatímco dopad na krátkodobá inflační očekávání byl více než dvojnásobný – kolem jednoho procentního bodu. Podíváme-li se na možné mechanismy tohoto nárůstu, navrhujeme, že jej lze částečně přičíst obavám jednotlivců z prudce rostoucích cen energií a rostoucímu pesimismu ohledně ekonomických trendů obecně. Naše výsledky naznačují, že velké ekonomické šoky mohou mít podstatný dopad na krátkodobá i dlouhodobá inflační očekávání.

Keywords

Homogamy, unilateral divorce, marriage, fertility, Joint custody, inflation expectations, Russian invasion of Ukraine, survey, natural experiment.

Klíčová slova

Homogamie, Jednostranný rozvod, Manželství, Plodnost, Společná péče, Inflační očekávání, Ruská invaze na Ukrajinu, Průzkum, Přirozený experiment

Declaration

1. I hereby declare that I have compiled this thesis using the listed literature and resources only.
2. I hereby declare that my thesis has not been used to gain any other academic title.
3. I fully agree to my work being used for study and scientific purposes.

In Prague on 19.05.2023. Geghetsik Afunts

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Introduction

The first two articles of this dissertation investigate how changes in family legislation in the US affected the marriage market and fertility patterns. The third article analyzes how people in Germany adjusted their inflation expectations after learning about Russia's invasion of Ukraine. The commonality between the three articles is that they use large sets of microdata and econometric methods to study human behavior.

The first two studies revolve around the trend towards educational homogamy - the tendency towards partners having the same level of education. In the first chapter, Štěpán Jurajda and I ask whether the introduction of unilateral divorce legislation (UDL) affected the educational structure of marriage. Before UDL, it was only possible to divorce when both partners agreed to end the marriage or one partner committed a serious violation (fault-based divorce). UDL lifted these restrictions to divorce. While there is a large body of literature that studies the effect of UDL on divorce rates (Wolfers, 2006), and it is established that homogamous marriages are less likely to divorce (Schwartz, 2010), there is no previous research on whether UDL affected the educational structure of divorces.

We rely on precise measures of marriage inflows and outflows by state and year based on under-utilized administrative data from marriage and divorce certificates. We use a difference-in-differences approach to study how the distribution of marriages between three marriage types is affected - homogamous marriages (where the education level of the wife is equal to that of the husband, W=H), hypogamous marriages (women marrying less educated partners, W>H), and hypergamous marriages (women marrying more educated partners, H>W). We find no evidence that UDL affects the educational matches at marriage inflow (newlyweds). Regarding marriage outflow (divorces), we find that UDL did affect the educational structure of marriage. It decreased the relative likelihood of hypogamous (W>H) and homogamous (W=H) couples to divorce, compared to hypergamous marriages (H>W). Additionally, we provide novel evidence that much of the stability advantage of homogamy plays out within the first two years of marriage. In order to understand the potential underlying mechanisms, as a final part of our analysis we examine match quality changes in these couples.

The second chapter investigates whether changes in child custody legislation affected fertility decisions and thereby the fertility differentials between homogamous and non-homogamous couples. This study is the first to analyze the effect of an aspect of family legislation, joint custody laws (JCLs), on the fertility sorting on education. Between the early 1970s and the 2000s most federal states of the US introduced JCLs, which meant they switched from a practice of sole custody after a divorce to joint custody. This meant that ex-partners were granted a say regarding important decisions in the children's upbringing and the right to spend a certain amount of time with them.

JCLs might discourage partners in unstable relationships from having children, because they create the necessity to continuously interact with an ex-partner after a divorce. Partners who are certain of their relationship would be less affected by this rationale, because this scenario is less likely to materialize for them. Since homogamous relationships are more stable on average (Schwartz, 2010), JCLs should, therefore, increase the odds of homogamy of parents, as non-homogamous couples are more likely to be deterred from having children than homogamous couples.

The analysis is based on administrative data from US birth certificates, which contain detailed information on newborns and their parents and have a large coverage in terms of states and years. In a preliminary step I find that even before the JCLs homogamous couples were three to four times more likely to have a child than non-homogamous ones. This finding is in line with the finding that higher marriage stability increases the willingness to have children (Becker, 1981; Becker et al., 1977).

In the main analysis I utilize the gradual introduction of JCLs across different states to estimate a difference-in-differences model. The results are evidence that JCLs increase the odds of homogamy of parents. In the final step, I divide the analysis into subgroups to gain insights into the mechanisms underlying the increase in homogamy. These analyses indicate that the channel which increases the odds of homogamy of parents is a relative increase in births of married homogamous couples rather than a change in the incentive to marry for pregnant couples.

In the last chapter, Misina Cato, Tobias Schmidt and I study the impact of Russia's invasion of Ukraine on the inflation expectations of German residents. We treat the invasion as a natural experiment and utilise the microdata from a monthly online survey that collects information on people's expectations regarding several economic indicators in Germany. To confirm that we can treat the invasion as a natural experiment we check that it was not anticipated by the survey respondents. We can show that major preceding events, i.e., US President Biden's announcement on the probability of a war in Ukraine and Russian President Putin's assertion that Donetsk and Luhansk are independent republics, had no effect on inflation expectations.

In our main analysis we find a large - 1 percentage point, immediate increase in short-term inflation expectations (regarding the following 12 months), caused by the invasion. This finding is robust to various robustness checks, including the control for individual-level fixed effects. The increase of the inflation expectations for longer horizons (5 and 10 years), is smaller and less robust. Additionally, we look into potential determinants discussed in the literature, which suggest that possible mechanisms of these shifts are individuals' fears of increasing fuel prices, higher unemployment, and lower economic growth.

Chapter 1

Who Divorces Whom: Unilateral Divorce Legislation and the Educational Structure of Marriage [1]

1.1 Introduction

A large body of work in demography, economics, and sociology suggests that educational homogamy, the tendency to assortatively match into marriage and cohabitation based on one's education level, has increased significantly in the US since the 1960s (Schwartz & Mare, 2005; Siow, 2015).[2] This period has also been characterized by a dramatic increase in divorce rates and a decline in marriage rates, which has been partly attributed to the adoption of unilateral divorce legislation (UDL) that made divorce easier and affected marriage (inflow) decisions through anticipated welfare from marriage (Gruber, 2004; Rasul, 2006; Wolfers, 2006). Marriage outflow (divorce) structure clearly contributed to the rise in educational homogamy (henceforth, homogamy) in marriage stocks because homogamous marriages are less likely to divorce (Schwartz, 2010). This is driven by hypogamous couples (women marrying less educated partners) being most

[1] **Co-authored with Štěpán Jurajda (CERGE-EI).**

[2] Rising homogamy contributed to growing household income inequality (Greenwood et al., 2014; Dupuy & Weber, 2018; Gonalons-Pons et al., 2021) and intergenerational inequality, as investments in the human capital of children are affected by educational assortative matching (Chiappori et al., 2017).

likely to divorce of all educational marriage types (Tzeng, 1992; Tzeng & Mare, 1995).[3]

Despite the evidence on the importance of UDL for overall divorce rates, there is no research on whether UDL affected the *educational structure* of marriage outflows. Similarly, little is known about UDL effects on the educational structure of marriage inflows (newlyweds). In this paper, we ask whether UDL contributed to the increase in educational homogamy by quantifying UDL impacts on the educational structure of newlyweds and divorces using a difference-in-differences approach prevalent in the UDL literature. We rely on precise measures of marriage inflows and outflows by state and year based on under-utilized administrative data from certificates of marriages and divorces,[4] and study three marriage types—homogamous (where the education level of the wife is equal to that of the husband, W=H), hypogamous (W>H), and hypergamous (H>W).

The availability of easier divorce could affect marriage and/or divorce decisions particularly strongly for marriage types that are generally less stable (more likely to divorce), i.e., hypogamous couples. Education provides a signal about future earnings as well as values and attitudes. One of the explanations for the lower stability of non-homogamous couples is that they may have more value-driven disagreements in marriage (e.g., in decisions on raising children). If the availability of easier divorce thanks to UDL makes the generally risky hypogamous marriages even more likely to end in divorce (unstable), educational homogamy in the stock of prevailing marriages will increase. Next, fewer couples may enter risky non-homogamous marriages under UDL, and there can be adjustments to compensate for lower stability by improving match quality in other dimensions.[5] We find no evidence that UDL affected homogamy at marriage inflow, but we find that the unstable hypogamous marriages (W>H) as well as homogamous ones (W=H) become *less* likely to divorce thanks to UDL, relative to hypergamous marriages (H>W). We provide a discussion and tantalizing evidence on the potential mechanisms behind these effects.

Our analysis proceeds in four steps. We first confirm that the marriage and divorce certificate data we use, which include marriages formed during 1970-1988, display similar levels of educational homogamy in the stock of marriages to that measured using Current

[3]The share of hypogamous couples among newlyweds started rising during the 1970s (Appendix Figure 1.3), as changing social norms made them increasingly acceptable (Schwartz & Han, 2014).

[4]Hence, this paper focuses on marriage as opposed to cohabitation patterns.

[5]Such an adjustment is one of the equilibrium consequences of the introduction of UDL in a marriage market model based on imperfectly transferable utility (Reynoso, 2022). Similar mechanisms have been suggested in the literature on cohabitation (Schoen & Weinick, 1993; Brines & Joyner, 1999).

Population Survey (CPS) data. Second, we provide evidence on the evolution of the educational structure of marriage outflows (divorces) and inflows (newlyweds): It is well established from survey data that homogamous marriages are less likely to end in divorce (Schwartz, 2010; Goldstein & Harknett, 2006). We confirm this marriage stability gap by combining marriage and divorce certificates, and highlight that it is due to hypogamous marriages (W>H), while hypergamous marriages are about as stable as homogamous ones. These differences in divorce risk across educational marriage types are stable over our sample frame.[6] We provide novel evidence that much of the stability advantage of homogamy plays out within the first two years of marriage.[7]

Turning to newlyweds, we find that, while homogamy among newlyweds did not increase relative to non-homogamy,[8] there were significant changes in the educational structure at marriage inflow in the US during our sample frame: The odds of hypogamy (W>H) increase relative to hypergamy ('traditional' H>W newlyweds), and so do the odds of homogamy relative to hypergamy.

Third, we examine the role of unilateral divorce legislation (UDL) for educational sorting in marriage inflows and outflows. We use our state-year measures and employ a difference-in-differences identification strategy similar to that used in the literature studying the effects of UDL on overall divorce and marriage rates (Alesina & Giuliano, 2006; Wolfers, 2006).[9] Using data covering 1970 to 1988, we find little evidence that making divorce easier increased homogamy at marriage inflow, but we uncover robust evidence that UDL lowers the stability of hypergamous marriages relative to homogamous ones, and that it reduces the large stability disadvantage of hypogamous couples. Our estimates paint a picture of a marriage market in which the tendency to form newlywed couples in which women are more educated than their spouses has increased, these marriages are generally less stable, but their stability disadvantage has been reduced thanks to UDL.

[6]Schwartz & Han (2014) find that homogamous and hypogamous marriages became more stable relative to hypergamous marriages for marriage cohorts spanning 1950 to 2004 in PSID and NSFG data. Similar to our certificate-based evidence, they find these stability gaps to be stable from 1970 to 1988.

[7]This is relevant to the literature studying marriage inflows using survey data in which newlyweds are identified as those recently married, because such samples are already affected by survival bias. For example, Reynoso (2022) studies newlyweds by relying on CPS data on first marriages that occurred at most two years before the survey interview.

[8]This finding is in line with that of Gonalons-Pons & Schwartz (2017) who rely on PSID data to conclude that economic homogamy among newlyweds has not substantially increased.

[9]Gruber (2004) asks whether the timing of UDL is related to marriage market fundamentals affecting outcomes, and argues that UDL was introduced primarily to avoid the burden to states that resulted from lengthy divorce cases. For evidence of lack of geographical correlation in UDL, see Reynoso (2022).

In the final part of our analysis, we explore match quality changes driven by UDL in the first steps towards understanding the underlying mechanisms.

Our analysis brings two types of novel findings to the literature. First, we extend the homogamy literature (Schwartz, 2010; Siow, 2015) by exploring administrative data, which are larger and more precise than previously employed surveys. This allows us to not only study inflows and outflows jointly, but also to offer new state-by-state descriptive evidence on homogamy among newlyweds. Second, we extend the UDL literature (Gruber, 2004; Wolfers, 2006) by showing that it is not only the rates of divorce, but also the educational structure of divorce that is affected by UDL, which supports the empirical case against marriage market models based on fully transferable utility (Chiappori et al., 2015). The estimates we provide can form an input for the study of UDL within equilibrium marriage market models based on imperfectly transferable utility (Reynoso, 2022).

1.2 Educational Homogamy and Unilateral Divorce

A secular increase in homogamy and a dramatic rise in divorce rates, together with declining marriage rates, occurred simultaneously on the US marriage market starting in the 1960s.[10] The study of these two trends is only partially connected. Most of the literature studying educational homogamy, including our analysis, relies on log-linear models to capture the supply-free tendency to match assortatively on education.[11] Estimates of homogamy's rise in marriage stocks are typically based on estimating these models on Census or survey data such as the Current Population Survey. Rising homogamy has been linked to various causes (Schwartz, 2013) including increasingly shared culture and values (DiMaggio & Mohr, 1985; Kalmijn, 1994), lower partner search costs for college educated (Bicakova & Jurajda, 2016; Pestel, 2021), and shared interest in investing in the human capital of children (Chiappori et al., 2017). Easier divorce, where UDL makes it possible (in absence of a fault) to leave marriage without the partner's agreement, can affect the importance of these factors for divorce as well as marriage decisions, to the extent these are based on anticipation of marriage stability. Reynoso (2022) builds an equilibrium marriage market model suggesting that the marriage stability disadvantage

[10] A third simultaneous trend is the rise in the share of women among college graduates. It is now well established that marriage market returns to college are higher for women than for men (Zhang, 2021).

[11] A disadvantage of these models is that they ignore those who do not marry (Choo & Siow, 2006).

of marrying someone with a different level of education can be compensated under UDL in newly formed non-homogamous marriages by improving the quality of the match on other dimensions.

As a matter of accounting, the increase in homogamy in prevailing marriages (marriage stocks) since the 1970s was brought about by a changing educational structure of entry into marriage (newlyweds) and/or by selective exits (divorces). Using the National Longitudinal Survey of Youth (NLSY79), Schwartz (2010) finds that homogamous marriages are significantly less likely to end in divorce, while Schwartz & Han (2014) rely on multiple surveys to conclude that the relative stability of homogamous marriages has increased. Several CPS analyses attempt to focus on newlyweds by studying marriages that are at most two years old (e.g., Mechoulan, 2006; Reynoso, 2022). With the exception of Reynoso (2022), whose empirical analysis focuses on marriage inflows, this literature does not connect the changing education structure of marriage inflows and outflows to UDL.

From the end of the 1960s, US states began to change the grounds for divorce, moving from fault-based to unilateral and no-fault divorce. Within two decades, 29 states changed their marriage dissolution laws to a unilateral system, which allows one to divorce without the agreement of one's spouse even in absence of a fault. A body of research based on the difference-in-differences design quantifies the impact of unilateral divorce laws (and no-fault divorce laws, which are similar to unilateral divorce legislation) on divorce and marriage rates, as well as several other outcomes.[12] There is consensus that UDL led to higher divorce rates. In an influential early analysis, Friedberg (1998) controls for state specific time trends and concludes that UDL explains 17 percent of the increase in US divorce rates between 1968 and 1988, and that this effect is permanent.[13] Wolfers (2006) extends the sample frame to 1956-1988 and concludes that adoption of UDL increases divorce rates immediately, but that this effect dissipates within a decade of the legislative reform. The evidence on the entry side of marriage is mixed: Rasul (2004) implies that UDL lowers marriage rates, Drewianka (2008) finds no UDL effect on family formation,

[12]Other outcomes explored in the literature include investments in marriage-specific capital (Stevenson, 2007), domestic violence (Stevenson & Wolfers, 2006; Dee, 2003; Parkman, 1992), the family-formation behavior of children affected by UDL (Gruber, 2004), and labor supply of spouses and intra-household bargaining (Voena, 2015; Stevenson, 2007; Mechoulan, 2006; Chiappori et al., 2002; Gray, 1993).

[13]For similar US findings see Nakonezny et al. (1995), Rodgers et al. (1999, 1997), and Gruber (2004). González & Viitanen (2009) and Kneip & Bauer (2009) find the UDL effect to be quantitatively large in the EU, explaining one fifth of the total EU increase in divorce rates, and to be highly persistent.

and Alesina & Giuliano (2006) use the same administrative data we employ in this paper to suggest that UDL increases marriage rates.

The literature is yet to consider potential differences across educational marriage types in UDL effects on marriage inflows and outflows simultaneously within one analytical framework. This is likely due to a lack of large individual-level data spanning these legislative changes. The administrative data we employ allow for such analysis, and bring to the analysis of UDL effects key advantages as well as some disadvantages we discuss below.

Studying potential UDL effects on the educational structure of marriage inflows and outflows is important not only for understanding the sources of the rise in homogamy. It is also useful as a test of workhorse marriage market models based on an assumption of fully transferable utility within couples (Chiappori et al., 2015). The Becker-Coase theorem (Becker, 1991) implies that, so long as couples contemplating a divorce can easily bargain with each other, i.e., easily transfer utility, whether or not mutual consent is required for divorce to occur should not affect divorce decisions. Evidence that UDL affects marriage and divorce rates, as well as any evidence on differential UDL effects by homogamy type, thus lends support to models based on imperfectly transferable utility within couples.

1.3 Data

To study the effects of UDL on the educational composition of marriage inflows and outflows, one would ideally rely on longitudinal data following the duration of a large sample of inflowing marriages sorted by state and year, including information on the mobility of couples across state borders. Homogamy analyses of divorces and newlyweds (e.g., Schwartz, 2010; Schwartz & Han, 2014; Reynoso, 2022) are typically based on longitudinal surveys such as the Panel Study of Income Dynamics (PSID), which are well suited for US-level analyses, but contain few divorce or newlywed observations for typical state-year cells. The number of available observations is limited even in the substantially larger CPS, because most states are grouped for the years in which UDL was introduced (1968-1976 in CPS March), and because the year (or age) of (first) marriage is available only for some years. Only few recent marriages are available by state and year to approximate newlyweds. If sampling error renders many of the state-year average outcomes uninformative, it reduces the effective number of clusters employed in the difference-in-differences research design (Carter et al., 2017; Brewer et al., 2018). Furthermore, one does not observe newlyweds in the CPS, but only recent marriages that survived a certain time window, such that inflow proxies can be affected by early-marriage-outflow effects.

We use the CPS primarily for comparison purposes and in our main analysis, we rely on administrative data from the National Vital Statistics System of the National Center for Health Statistics (NCHS), which report characteristics that couples provide when applying for a marriage or divorce: residency, education, race and age of bride and groom, date of marriage (divorce), and the number of previous marriages.[14] The NCHS database covers all divorces and marriages from small states and provides a 10 to 50% sample in large states. However, the number of states with data on the education of spouses varies across years. Appendix Table 1.7 compares available sample sizes and state-year coverage in the CPS and the certificate data. The certificate data cover fewer states than the CPS, but they provide large annual samples by state and year, facilitating the measurement of marriage inflow and outflow structures by homogamy type, typically measured using a 5x5 educational-category match matrix. In contrast, in four-fifths of states, fewer than 10 newlywed couples are observed annually in the CPS.[15] The certificate data cover 1970-1988, such that, unlike survey data, they cannot be used to study the effect of UDL on the stability of marriages that began before 1970, i.e., before the first US introduction of UDL. However, the underutilized certificates cover the introduction period of most UDL, and allow for study of marriage inflows in the 1970s and 1980s without survival biases. They provide precise measures of both inflows into and outflows from marriage by educational type, order of marriage, and state and year of registration—a key characteristic supporting state-level panel-data analyses exploring the importance of legislative reforms.

We observe the state and year of marriage for divorced couples, which allows us to combine marriage and divorce certificates to study divorce rates. Our divorce analysis focuses on the share of marriages registered in a given year and state (by educational type) lasting more than two (four) years. Appendix 1.C provides details of these calculations. The NCHS data code the education of spouses by years of schooling. We divide this scale into 5 educational categories following Schwartz & Mare (2005).[16]

[14]The data used in this paper were downloaded in Sep 2017 from its NBER archive at https://www.nber.org/research/data/marriage-and-divorce-data-1968-1995.

[15]The CPS also does not allow one to approximate the educational structure of recent divorces, because divorced respondents are not asked about the education level of their ex-spouse.

[16]We also used a categorization based on Acemoglu & Autor (2011). The dynamics of marginal-free measures of homogamy and our estimated UDL effects were not affected (Appendix Tables 1.13 - 1.17).

The certificate data reproduce key features of educational assortative matching found in surveys. During our study period, gender gaps in education narrow and reverse as the share of educated women (and wives) increases faster than that of men. In 1970, 34% of newlywed wives had either some college or 4+ years of college, compared to 40% of husbands, in 1988 the corresponding shares are 49% and 47%, respectively (Appendix Table 1.8). The evolution of the educational structure of newlyweds is similar in CPS March and the certificate data.[17] Among newlywed men with 4+ years of college in 1970, 46% marry women with the same education; the share increases to 60% by 1988 (Appendix Table 1.9; again, this pattern is similar in the CPS, Appendix Figure 1.5).

To compare the odds of homogamy[18] between the CPS and the certificate data, we mimic the certificate data and focus on the CPS stock of marriages formed after 1970; we can perform a consistent comparison for the 1980 stock that had 10 years to build.[19] We apply the Schwartz & Mare (2005) log-linear estimation strategy (presented in the next section) to the 1980 CPS June sample and to our 1980 pseudo stock of marriages generated from the certificate data (see Appendix 1.C). As in the rest of the analysis, we analyze marriages in which the women entering the marriage were aged 16-40. In both data-sets, we uncover near-identical homogamy levels: for first marriages (from the wife's perspective), the 1980 log-odds level is 3.3; for higher-order marriages, it is 2.2.[20]

While the certificate database does not provide an ideal data source to study the US-wide evolution of homogamy in the 1970s and 1980s, due to its focus on recent marriages and incomplete coverage of states and years, it reproduces basic features of the US marriage market and allows one to simultaneously consider the educational structure of marriage inflows and outflows by state and year. Our main analysis conditions on state fixed effects and thereby aims to minimize the impact of state composition on our findings.

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[17]Appendix Figure 1.4 shows this for two key education categories—high school and 4+ years of college.

[18]The odds of educational homogamy measure positive assortative matching as the likelihood to be matched with someone with the same education rather than someone with a different education level.

[19]Several CPS June supplements from the 1970s and 1980s include information on the year of first marriage, but only the 1980, 1990 and 1995 June supplements also report the number of times a person has been married.

[20]We additionally compared the homogamy levels for all years between 1980 and 1988, where we cannot distinguish prevailing first marriages starting after 1970 in the CPS. In none of these years were log-odds homogamy levels different by more than 0.19 points (or 5.8 %) between the CPS and the NCHS data.

1.4 Homogamy in Marriage Inflow and Outflow

When the educational composition of the marriage market is changing, it is not clear to what extent changes in the share of homogamous couples are driven by assortative matching and to what extent they are a result of changes in supply structure (marginal distributions). The widely used log-linear homogamy model generates marginal-free homogamy measures by controlling for shifts in marginal distributions. Let i and j denote education levels of husbands and wives in observed ij marriage matches, where $i, j = 1, 2, ..., 5$ are education categories corresponding to a 5x5 match-type matrix. The log-linear model (e.g., Schwartz & Mare, 2005) explains the counts of these matches by year t as:

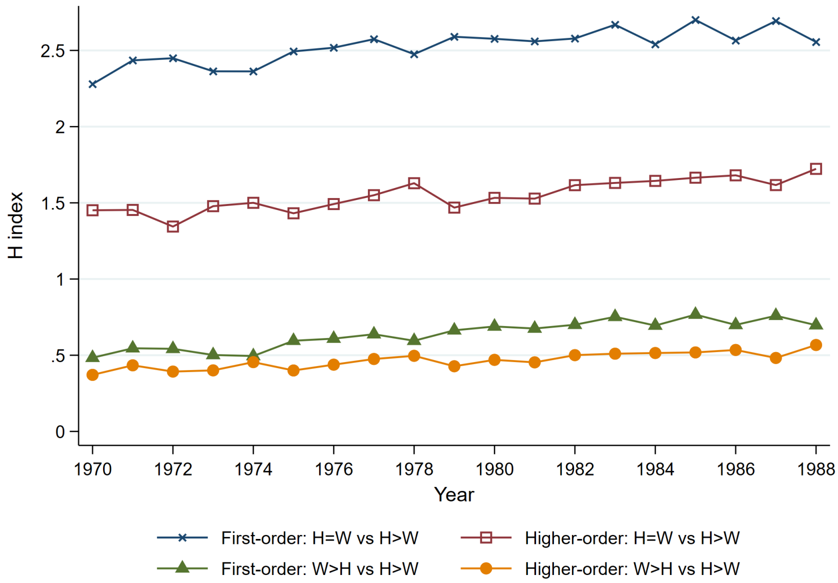
$$ln\mu_{ijt} = \lambda + \lambda_{ij} + \sum_{n=t,s} (\lambda_{in} + \lambda_{jn}) + \gamma_t^D + \epsilon_{ijt}, \tag{1.1}$$

where $t = 1, ..., T$, λ_{ij} is the fixed effect for ij match-type pairs, λ_{in} and λ_{jn}, $n = t, s$, are a set of (marginal) fixed effects for each year and state. In addition to these fixed effects, the simple model controls for diagonal (homogamy) elements of the match matrix by year γ_t^D. The evolution of γ_t^D over time speaks to the trajectory of homogamy (the H homogamy index following Schwartz & Mare, 2005).[21] In our preferred specifications, we separate the selective non-homogamous couples in which wives are more educated (hypogamous marriages or W>H) and the larger group of 'traditional' non-homogamous couples in which husbands are more educated (hypergamous marriages, H>W). Following Schwartz & Han (2014), we use hypergamy as the base; in terms of Equation 1.1, we thus add an above-diagonal coefficient γ_t^{AD}, leaving the below-diagonal elements in the base case.

The evolution of the log odds of homogamy against the base case of hypergamy in our *newlywed* data is shown in Figure 1.1, which also tracks the log odds of hypogamy against the base case of hypergamy. Homogamy (W=H) at marriage inflow is increasing against the 'H>W' benchmark for both first and higher-order marriages. There is a similar upward trend in the marriage inflow for the smaller group of 'W>H' couples.

[21] An H value of 3 means that a person is 3 times more likely to be married to someone with the same level of education rather than to someone with a different level.

Figure 1.1: Homogamy in Marriage Inflows for First and Higher-Order Marriages, NCHS Data



Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. We control for state fixed effects and state specific time trends.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Appendix Figure 1.6 then shows the evolution of the log odds of homogamy (H) among newlyweds against the combined base case of both non-homogamous marriage types. Overall homogamy at marriage inflow is *decreasing* among first marriages and stable for higher-order marriages.[22]

Next, we ask about trends in the relative stability of homogamous and non-homogamous marriages. We study survival rates of homogamous and non-homogamous couples, i.e., shares of marriage types (binary h index based on the 5x5 educational-type matrix) who are still married after two (four) years of marriage:

$$ln(Share\ of\ survived\ marriages_{hst}) = \beta Homogamous_{hst} + \lambda_s + \lambda_t + \lambda_s t + \epsilon_{hst}. \quad (1.2)$$

[22]Marriage order is measured from the wife's perspective. The finding is not driven by the changing state coverage of the certificate data, as it is replicated in most US states (Appendix Figures 1.7 and 1.8), and it is not materially affected when we focus only on first marriages of the homogenous group of white couples or when we restrict the age gap between the spouses to 5 years at the most.

Here, *Homogamous* (W=H) is an indicator corresponding to homogamous vs. non-homogamous couples, λ_s are state fixed effects, λ_t are year fixed effects and $\lambda_s t$ are state-specific time trends. Again, in our preferred specification we add a coefficient for the selective W>H marriages and keep only 'H>W' in the base case. Hypogamous 'W>H' marriages are particularly unstable (risky), while more 'traditional' hypergamous marriages (H>W) are at least as stable as the homogamous ones (Table 1.1).[23]

Our findings highlight that stability gaps between hypogamous marriages and other marriage types open early, within two years of marriage. Another novel finding is that homogamous marriages are less stable than hypergamous ones for higher-order marriages, but not for first marriages. The combination of these patterns implies that the share of homogamous marriages that survive more than two (four) years is higher than the share of surviving non-homogamous ones (Appendix Table 1.10). These findings based on the certificate data are thus in line with the survey-based evidence (Schwartz, 2010; Schwartz & Han, 2014); they imply that the stability gap between homogamous and hypogamous marriages mechanically contributes to rising homogamy levels in marriage stocks. Figure 1.2 also suggests that the stability advantages across educational marriage types (at 2 or 4 years of marriage) do not change much over our sample frame.

[23]In higher order marriages, hypergamous couples (H>W) are the most stable.

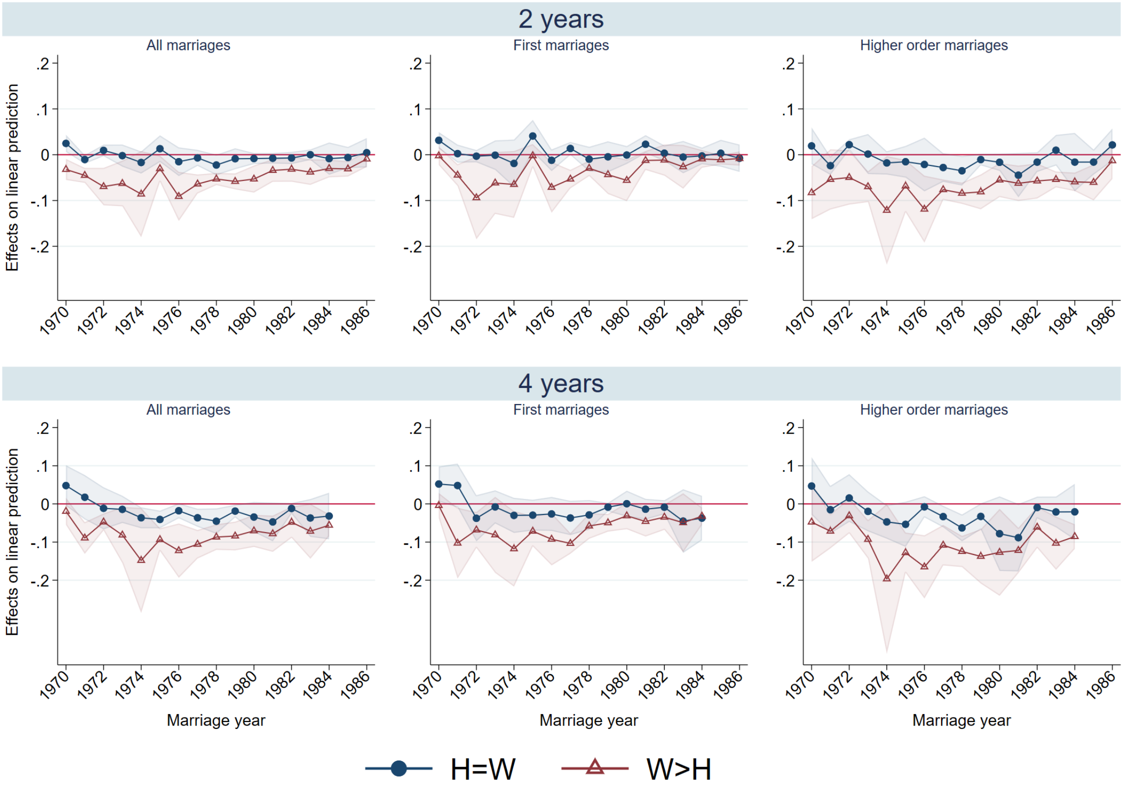
Table 1.1: Survival of hypogamous (W>H) and homogamous (W=H) couples relative to hypergamous (H>W)

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
			Up to 2 year old marriages			
H = W	-0.005	-0.005	0.003	0.003	-0.012^{*}	-0.013^{*}
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)
W > H	-0.046^{***}	-0.046^{***}	-0.032^{***}	-0.032^{***}	-0.067^{***}	-0.067^{***}
	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)
Observations	28,331	28,331	16,122	16,122	12,209	12,209
Marriages from	1970 – 1986	1970 – 1986	1970 – 1986	1970 – 1986	1970 – 1986	1970 – 1986
Number of states	15	15	15	15	15	15
			Up to 4 year old marriages			
H = W	-0.025^{**}	-0.025^{**}	-0.017	-0.017	-0.033^{***}	-0.033^{***}
	(0.010)	(0.010)	(0.013)	(0.013)	(0.008)	(0.008)
W > H	-0.079^{***}	-0.080^{***}	-0.059^{***}	-0.059^{***}	-0.109^{***}	-0.109^{***}
	(0.016)	(0.016)	(0.015)	(0.015)	(0.018)	(0.018)
Marriages from	1970 – 1984	1970 – 1984	1970 – 1984	1970 – 1984	1970 – 1984	1970 – 1984
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. The specifications also control for age groups of wives, marriage order and an indicator for inter-race marriages. Standard errors are clustered at the state level. Regressions are weighted by state-year population. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. All of the estimated W>H coefficients remain statistically significant at the 1% level based on wild bootstrap inference following Cameron & Miller (2015); the statistical significance of the H=W coefficients declines based on wild bootstrap.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Figure 1.2: Year x Homogamy coefficients for marriage survival: homogamous (W=H) and hypogamous (W>H) couples relative to hypergamous (H>W)



Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. The specifications also control for age groups of wives, marriage order, and an indicator for inter-race marriages. Standard errors are clustered at the state level, and regressions are weighted by state-year population. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. We control for state, year, and month of marriage fixed effects, and state-specific time trends.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

1.5 UDL and Homogamy

The legislative change from divorce based on mutual agreement (or fault-based divorce) to UDL makes divorce easier and more likely to occur (Wolfers, 2006). Since hypogamous marriages are generally the least stable, does the introduction of UDL curb the rise of hypogamy (and thus support the rise of homogamy) at marriage inflow due to the additional riskiness of these marriages? To investigate this, we introduce a UDL indicator into the log-linear equation for marriage inflows (Equation 1.1), together with the interaction term between UDL and marriage type.[24] Table 1.2 coefficients for the interaction of UDL with homogamy and hypogamy (with the base case of hypergamy) are all statistically insignificant and close to one, i.e., they signal no UDL impact on the educational structure of newlyweds.[25] We obtain the same conclusion when we combine the two non-homogamous marriage types in Appendix Table 1.11 and when we use the economics classification of education types in Appendix Table 1.12.

Reynoso (2022) measures homogamy as the within-couple similarity in years of education, and finds in linear specifications that UDL increases homogamy. We rely on educational categories and marginal-free measures of homogamy based on the log-linear model and find no effect of UDL on homogamy. In Appendix 1.D, we provide estimates based on the Reynoso (2022) approach and estimated off the CPS and the certificate data after we apply highly similar sample definitions. Based on both datasets, we obtain statistically indistinguishable and insignificant positive UDL coefficients; the CPS-based one is close to that of Reynoso (2022). It could be that UDL leads to changes in the educational similarity of couples measured in years of education that do not lead to changing structures measured in educational categories.

[24] We use the timing of divorce legislation changes from Voena (2015); nevertheless, we receive similar results when we rely on the year of legislative changes defined by Wolfers (2006) and Friedberg (1998).

[25] In a log-linear homogamy model $\mu_{ijst} = \beta_h X + \beta_u U_{st} + \epsilon_{ijst}$, the impact of unilateral divorce (U) on the dependent variable is $ln\left(\frac{\mu_{ijst}|U_{st}=1}{\mu_{ijst}|U_{st}=0}\right) = exp(\beta_u)$. Therefore, states with unilateral divorce laws have $exp(\beta_u)$ times as many homogamous marriages, as those with no UDL. The closer $exp(\beta_u)$ is to 1, the smaller is the impact of UDL. In Table 1.2, we have already calculated $exp(.)$s of coefficients.

Table 1.2: The impact of UDL on marriage inflow structure

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
UDL	1.070	0.929	1.044	0.936	1.240	0.911
	(0.126)	(0.065)	(0.100)	(0.068)	(0.228)	(0.085)
UDL x H = W	1.007	1.010	1.006	1.006	0.972	0.980
	(0.038)	(0.038)	(0.044)	(0.044)	(0.028)	(0.029)
UDL x W > H	0.867	0.871	0.866	0.867	0.860	0.874
	(0.093)	(0.093)	(0.100)	(0.100)	(0.083)	(0.080)
H = W	2.359***	2.353***	2.727***	2.725***	1.585***	1.573***
	(0.080)	(0.080)	(0.110)	(0.109)	(0.035)	(0.036)
W > H	0.694***	0.692***	0.774***	0.773***	0.526***	0.524***
	(0.048)	(0.048)	(0.066)	(0.066)	(0.016)	(0.017)
Observations	17,101	17,101	8,611	8,611	8,490	8,490
Marriages from	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988
Number of states	24	24	24	24	24	24
State FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Exponentiated coefficients; Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation (1). Only marriages that women entered when they were between the ages of 16 and 40 are included. Standard errors are clustered at the state level. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. All of the statistically significant coefficients remain significant at the 1% level when we alternatively rely on wild bootstrap inference (Cameron & Miller, 2015).
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

In the second part of our UDL analysis, we focus on marriage outflow structure and introduce a UDL indicator with its homogamy and hypogamy interactions (with hypergamy as the base case) into Equation 1.2 estimated at 2 and 4 years of marriage duration in Tables 1.3 and 1.4, respectively. While we detect no effect of UDL on the educational structure of marriage inflows, we do find statistically significant and sizeable effects of UDL on the educational structure of marriage outflows. First, UDL makes homogamous first marriages more likely to survive, i.e., less likely to divorce (relative to first hypergamous marriages). In other words, for first marriages (from the wife's perspective), UDL opens a stability gap between homogamous and more 'traditional' hypergamous marriages. Second, much of the survival disadvantage of hypogamous marriages (relative to hypergamous and homogamous) disappears thanks to UDL for first marriages. UDL has no impact on the outflow structure of higher-order marriages, but it changes the survival structure of first marriages considerably.

These findings are not sensitive to several robustness checks. First, while the marriage inflow log-linear model (Equation 1.1) is estimated using the Poisson model (as in Schwartz & Mare, 2005), the marriage outflow regressions (Equation 1.2) are linear difference-in-differences with a logarithmic outcome variable. To address the concerns raised in Silva & Tenreyro (2006), we therefore alternatively estimate the outflow specifications using the Poisson model.[26] The results are fully robust, as attested by Appendix Tables 1.14 and 1.15. Similarly, the estimates are robust to alternatively relying on the educational categories used in Acemoglu & Autor (2011) (see Appendix Tables 1.16 and 1.17).

[26] We use the `ppmlhdfe` package in Stata.

Table 1.3: The impact of UDL on 2-year marriage survival: hypogamous (W>H) and homogamous (W=H) couples relative to hypergamy (H>W)

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
		Up to 2 year old marriages				
UDL	−0.001	−0.043	−0.032*	−0.055**	0.046	−0.023
	(0.028)	(0.026)	(0.016)	(0.025)	(0.032)	(0.025)
H = W	−0.009	−0.009	−0.003	−0.003	−0.013*	−0.014*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
W > H	−0.053***	−0.053***	−0.042***	−0.042***	−0.068***	−0.068***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
UDL x H = W	0.016*	0.015*	0.024**	0.023**	0.004	0.004
	(0.008)	(0.008)	(0.010)	(0.010)	(0.011)	(0.011)
UDL x W > H	0.029**	0.029**	0.038***	0.038***	0.002	0.002
	(0.011)	(0.011)	(0.009)	(0.009)	(0.019)	(0.019)
Observations	28,331	28,331	16,122	16,122	12,209	12,209
Marriages from	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. The specifications also control for the age group of wifes, higher order and inter-race marriages. Standard errors are clustered at the state level, and regressions are weighted by state-year population. TT stands for time trends. We do not use the states where we have less than 3 years of coverage, and state-year pairs with fewer than 50 observations. All of the estimated W>H and W>H x UDL coefficients that are statistically significant at the 1% level or the 5% level based on clustered standard errors remain statistically significant at least at the 5% level based on wild bootstrap inference (Cameron & Miller, 2015).
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.4: The impact of UDL on 4-year marriage survival: hypogamous (W>H) and homogamous (W=H) couples relative to hypergamy (H>W)

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
		Up to 4 year old marriages				
UDL	−0.012	−0.058	−0.061	−0.097	0.068	0.003
	(0.066)	(0.065)	(0.052)	(0.064)	(0.070)	(0.057)
H = W	−0.032***	−0.032***	−0.029**	−0.029**	−0.034***	−0.034***
	(0.010)	(0.010)	(0.013)	(0.013)	(0.008)	(0.008)
W > H	−0.089***	−0.090***	−0.072***	−0.072***	−0.111***	−0.111***
	(0.018)	(0.018)	(0.016)	(0.016)	(0.022)	(0.022)
UDL x H = W	0.031*	0.031*	0.048***	0.048***	0.005	0.005
	(0.015)	(0.015)	(0.016)	(0.016)	(0.021)	(0.021)
UDL x W > H	0.041*	0.041*	0.052***	0.053***	0.011	0.010
	(0.020)	(0.020)	(0.015)	(0.015)	(0.027)	(0.027)
Observations	27,548	27,548	15,649	15,649	11,899	11,899
Marriages from	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. The specifications also control for the age group of wife, higher order and inter-race marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use the states where we have less than 3 years of coverage, and state-year pairs with fewer than 50 observations. All of the coefficients that are statistically significant at the 1% level or the 5% level based on clustered standard errors remain statistically significant at least at the 5% level based on wild bootstrap inference (Cameron & Miller, 2015), aside from the H=W coefficients for first marriages.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

We also explore the sensitivity of our baseline estimates to the choice of the control group following Sun & Abraham (2021); specifically, we use the never-treated states as the control group.[27] The difference between the hypogamy and homogamy UDL effect (against the base case of hypergamy) is again statistically significant at the 1% level for first marriages; further, the estimated coefficients are quantitatively similar to (within 8% of the size of the corresponding) interaction terms in Tables 1.3 and 1.4. Finally, in a companion paper, Afunts (2023) studies whether the impact of joint custody laws (JCLs) and UDL on fertility differs between homogamous and non-homogamous couples. JCLs require that decisions about children should be made jointly by both parents after a divorce. Our UDL estimates are again fully robust to additionally including JCL controls.

1.6 Mechanisms

Our results paint a picture of a marriage market on which more 'traditional' hypergamous (H>W) marriages are losing ground among newlyweds to both homogamy (H=W) and hypogamy (W>H), and where hypogamous marriages are more likely to end in divorce. These patterns hold for both first and higher-order marriages. With the introduction of unilateral divorce, this picture changes substantially for first marriages, but not for higher-order ones (with marriage order defined from the wife's perspective). Among first marriages, UDL makes hypogamy almost as stable as hypergamy, while homogamy begins to enjoy a stability advantage over hypergamy. What underlying mechanism could be responsible for these findings?

A growing literature asks why hypogamous couples are relatively unstable. One possibility is that a more educated (higher earning) wife may feel or be perceived as a threat to her husband's gender identity as primary breadwinner.[28] Under UDL, the increasing instability of marriage may be perceived particularly strongly among those contemplating a hypogamous marriage, which may in turn lead to compensating behavior where hypogamous newlyweds become better matches on dimensions other than education to improve their expected marriage stability. A smaller within-couple age gap is an indi-

[27] We rely on the `eventstudyinteract` Stata command (Sun, 2021) as it can handle unbalanced panels. To ask about the treatment effect difference across sub-samples (for H=W vs. H>W and W>H vs. H>W), we estimate these separately and then asses the statistical significance of their difference (using `lincom`.)

[28] E.g., Tichenor (2005, 1999); Kaukinen (2004). This may lead to lower marriage satisfaction (Bertrand et al., 2015), and higher infidelity and domestic violence risks (Munsch, 2010; Atkinson et al., 2005).

cator of more egalitarian and stable unions (e.g., Van de Putte et al., 2009). Second, higher-order marriages are more risky, and this includes marriages that are first for the wife, but second or higher-order for the husband. Third, inter-racial marriage is also more risky.[29] Finally, the unilateral nature of divorce may also give more agency to the less happy side of the marriage, affecting whether wives or husbands end up applying for divorce.

To explore these potential mechanisms, we rely on our newlywed certificates. First, we ask whether UDL affects (i) the probability that a woman entering her first union marries a man who has already been married, (ii) the share of inter-racial marriages, and (iii) the within-couple age gap (in years). For each of these outcomes, we calculate averages by state, year, and our three educational marriage types, and we estimate linear difference-in-differences UDL effects. We focus on first marriages (from the wife's perspective), as we have no evidence on UDL affecting higher-order marriages. Columns (1) and (2) of Table 1.5 imply that UDL lowers the share of hypogamous first-marriage wives marrying husbands who have previously been married. In the specification with state-specific time trends in column (2), the decline corresponds to about half of the base case hypogamy effect of 4 percentage points. UDL has no such effect on the other two educational marriage types. In columns (3) and (4), the dependent variable is the share of inter-racial marriages. UDL increases this share for all marriage types by 1 to 2 percentage points, but this effect is not statistically significant in the specification with state-specific time trends. Finally, the estimates in the last two columns suggest that, for homogamous first marriages and, even more, for hypogamous first marriages, there is more similarity in terms of age under UDL. The evidence is thus consistent with the notion that hypogamous couples compensate for the (perceived) higher riskiness of marriage under UDL by forming better (closer) matches and that this is responsible for much of the reduction in stability gaps.[30]

[29]Using the certificate data from state-year combinations before the introduction of UDL, a couple who are 4 years apart in age (corresponding to one standard deviation of the within-couple absolute age gap of 4.2 at marriage inflow) is 16 percentage points more likely to end up divorced within 4 years of marriage than a couple who are equal in age. An inter-racial marriage is 13 percentage points more likely to end in divorce within four years than a same-race marriage. A wife's marriage is 4 percentage points more likely to end in divorce if she is married to a husband who was previously married rather.

[30]In Appendix Table 1.18, we find no evidence of compensation on match quality for higher-order marriages. Perhaps younger couples entering first marriages are better able to adjust match quality.

Table 1.5: UDL and match quality among newlyweds: homogamous (W=H) and hypogamous (W>H) wives' first marriages relative to hypergamy (H>W)

	Husband's higher order marriage, %		Inter-racial marriage, %		Absolute within-couple age diff.	
	(1)	(2)	(3)	(4)	(5)	(6)
UDL x H=W	−0.235	−0.232	−0.180	−0.185	−0.160***	−0.160***
	(0.240)	(0.241)	(0.237)	(0.236)	(0.042)	(0.043)
UDL x W>H	−2.668***	−2.668***	−0.032	−0.031	−0.335***	−0.335***
	(0.618)	(0.618)	(0.263)	(0.264)	(0.087)	(0.087)
UDL	2.446*	0.806	0.985**	1.980	0.235	0.083
	(1.184)	(0.477)	(0.455)	(1.550)	(0.153)	(0.130)
H=W	−2.905***	−2.905***	−0.545***	−0.543***	−0.839***	−0.839***
	(0.176)	(0.176)	(0.081)	(0.081)	(0.026)	(0.026)
W>H	4.088***	4.087***	−0.438***	−0.437***	−0.410***	−0.410***
	(0.375)	(0.375)	(0.120)	(0.120)	(0.049)	(0.049)
Observations	25,064	25,064	23,694	23,694	25,064	25,064
Marriages from	1970 − 1988	1970 − 1988	1970 − 1988	1970 − 1988	1970 − 1988	1970 − 1988
Number of states	24	24	23	23	24	24
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The share of first-marriage wives marrying higher-order-marriage husbands and the share of inter-racial marriages are expressed in percentage points. The within couple age gap is defined in years. The specifications also control for the age group of wives. Only marriages in which women are entering when they were between the ages of 16 and 40 are included. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. The number of observations is smaller for specifications in columns (3) and (4) because for some states the information on race is missing.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

In the second step, we employ information on who files for divorce that is provided on divorce certificates in 20 states starting in 1974. In Table 1.6, we regress the share of first-marriage divorces that were initiated by wives on the UDL indicator and interactions with educational marriage types. Hypergamous marriages are again the base case.

Table 1.6: The impact of UDL on the share of wife-applied divorces: homogamous (W=H) and hypogamous (W>H) first marriages relative to hypergamy (H>W)

	(1)	(2)
UDL	0.063***	0.059***
	(0.011)	(0.009)
UDL x H = W	−0.033***	−0.033***
	(0.008)	(0.008)
UDL x W > H	−0.053***	−0.053***
	(0.013)	(0.013)
H = W	0.128***	0.127***
	(0.005)	(0.005)
W > H	0.256***	0.256***
	(0.009)	(0.009)
Observations	27,590	27,590
Marriages from	1970 − 1988	1970 − 1988
Divorces from	1974 − 1988	1974 − 1988
Number of states	20	20
State FEs	✓	✓
Marriage year FEs	✓	✓
Marriage month FEs	✓	✓
State Specific TT		✓

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. The specifications also control for the age group of wives and inter-race marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. We use divorce certificates starting in 1974, because information on who applied for divorce is not available from earlier years. We study only divorces from marriages that began during 1970-1988.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

The estimates imply that UDL increases the share of wife-initiated divorces by more than 10% (by about 6 percentage points relative to the control mean of 54%) for hypergamous marriages, which have higher divorce risks under UDL. This is consistent with less educated wives who could not leave a marriage without UDL taking advantage of unilateral divorce. In contrast, the share of divorces filed by wives increases little with UDL for homogamous marriages (by about 3 percentage points relative to the control mean of 65%), while there is almost no change for hypogamous marriages.[31] This is consistent with UDL having little effect on egalitarian homogamy unions, and with hypogamy newlyweds compensating on match quality to lower divorce risks and to leave unchanged the degree of the wife's need to file for divorce.

1.7 Conclusion

By employing administrative data, we are able to study the educational structure of marriage inflows and outflows at US state-year level within one analytical framework. We confirm several existing findings and show that the substantial marriage stability disadvantage of hypogamous couples (where wives are more educated than their husbands), relative to other marriage types plays out strongly within the first two years of marriage. We also find that homogamy among newlyweds (the tendency to form marriages in which spouses are equally educated) was decreasing (relative to non-homogamy) from 1970 to 1988, implying that the secular rise in homogamy in marriage stocks is due to the higher stability of homogamous marriages. We then provide the first study of the joint effect of unilateral divorce legislation on the educational structure of both marriage inflows and outflows.

Our findings depict a marriage market where, among newlyweds, more 'traditional' hypergamous marriages (in which husbands are more educated) are losing ground to both homogamy and hypogamy, and where hypogamous marriages are more likely to end in divorce. These patterns hold for both first and higher-order marriages. With the introduction of unilateral divorce legislation (UDL), the picture changes substantially for first marriages, but not for higher-order ones. Among first marriages, under UDL, hypogamy becomes almost as stable as hypergamy, while homogamy begins to enjoy a stability advantage over hypergamy. Our tantalizing evidence on potential underlying mechanisms is consistent with UDL allowing wives to leave hypergamous marriages they would not leave without UDL (with no adjustment on match quality at marriage entry), and with hypogamous newlyweds compensating for the higher (perceived) riskiness of marriage implied by UDL by improving their marriage stability through forming better matches in other respects.

The evidence that unilateral divorce introduction affects divorce rates (Wolfers, 2006)

[31] We find similar effects on wife-initiated divorces for higher-order marriages in Appendix Table 1.19.

as well as the evidence provided here on its effects on who divorces whom is not consistent with the predictions of marriage market models based on fully transferable utility (Chiappori et al., 2015), and lends support to models based on imperfectly transferable utility (limited bargaining) within couples (Reynoso, 2022).

1.A Appendix: Tables

Table 1.7: Annual averages of the number of observations (number of years covered) in the CPS and in the certificate data

State	CPS Newlyweds	Marriage Certificates	Divorce Certificates	UDL Year
Alabama	6 (14)	.	1,177 (11)	1971
Alaska	8 (13)	.	2,179 (18)	pre-1967
Arizona	5 (15)	.	.	1973
Arkansas	5 (14)	.	.	.
California	36 (20)	7,105 (19)	3,533 (8)	1970
Colorado	6 (14)	.	.	1972
Connecticut	4 (18)	4,672 (8)	3,223 (18)	1973
Delaware	5 (12)	.	.	1968
D. of Columbia	2 (19)	.	.	.
Florida	15 (20)	.	.	1971
Georgia	9 (20)	.	1,993 (7)	1973
Hawaii	3 (13)	5,115 (19)	2,571 (18)	1972
Idaho	6 (14)	.	.	1971
Illinois	16 (20)	55,296 (19)	29,117 (18)	.
Indiana	9 (20)	.	.	1973
Iowa	6 (16)	4,281 (6)	3,472 (17)	1970
Kansas	8 (15)	4,336 (19)	2,796 (19)	1969
Kentucky	6 (18)	3,219 (5)	.	1972
Louisiana	8 (19)	3,395 (19)	.	.
Maine	4 (12)	10,364 (11)	.	1973
Maryland	7 (19)	.	.	.
Massachusetts	9 (16)	.	.	1975
Michigan	14 (16)	.	1,238 (15)	1972
Minnesota	6 (15)	165 (6)	.	1974
Mississippi	7 (15)	2,879 (10)	.	.
Missouri	10 (20)	34,035 (14)	15,799 (15)	.
Montana	13 (13)	6,962 (13)	2,593 (11)	1973

Nebraska	6 (13)	11,688 (19)	3,997 (18)	1972
Nevada	5 (12)	.	.	1967
New Hampshire	6 (14)	8,420 (19)	3,482 (10)	1971
New Jersey	13 (20)	.	.	.
New Mexico	5 (14)	.	.	pre-1967
New York	21 (20)	.	36,814 (18)	.
North Carolina	13 (16)	4,344 (19)	.	.
North Dakota	6 (12)	.	.	1971
Ohio	16 (20)	.	.	1992
Oklahoma	7 (16)	.	.	pre-1967
Oregon	4 (18)	.	.	1971
Pennsylvania	16 (20)	.	.	.
Rhode Island	3 (14)	6,533 (19)	2,100 (17)	1975
South Carolina	4 (15)	3,513 (3)	.	.
South Dakota	5 (13)	.	.	1985
Tennessee	8 (20)	3,244 (19)	1,053 (18)	.
Texas	23 (20)	.	.	1970
Utah	8 (14)	4,200 (19)	1,519 (18)	1987
Vermont	4 (12)	4,728 (19)	1,482 (17)	.
Virginia	7 (15)	54,837 (19)	15,385 (18)	.
Washington	5 (15)	.	.	1973
West Virginia	4 (18)	.	.	1984
Wisconsin	7 (16)	29,253 (11)	2,247 (11)	1978
Wyoming	5 (13)	2,521 (19)	1,761 (16)	1977

Note: Annual averages of marriage observations with education of spouses available for women entering their marriage aged 16-40. We combine March&June CPS samples and proxy newlyweds as marriages that are less than one year old at the time of the survey. The CPS annual averages cover 1962, 1965, 1967-1971, 1976-1977, 1979-1983, 1986-1988, 1990, 1992 and 1994. NCHS data cover 1970-1988. In NCHS, as in the rest of the paper, we do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. The number of years covered with necessary variables available in each data-set for each state is in brackets next to the average number of observations per year. The 'UDL year' column gives the year of the introduction of unilateral divorce legislation based on Voena (2015).
Source: CPS March&June and National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.8: The % share of newlywed wives and husbands with different education levels

Education levels	Wife 1970	1988	Husband 1970	1988
0-9 years of education	7.14	3.38	9.02	3.91
10-11 years of education	15.56	8.35	11.50	8.12
High school degree	42.86	39.38	39.76	40.75
Some college	22.95	25.97	24.66	22.61
4+ years of college	11.49	22.92	15.06	24.61
Total	100.00	100.00	100.00	100.00
N	58,451	292,846	58,451	292,846

Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations.

Sources: CPS March&June and National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.9: The education structure (in %) of *partners* of newlyweds with 4+ years of college

Newlyweds with 4+ years of college:	Husband 1970	1988	Wife 1970	1988
Education of their spouses:				
0-9 years of education	0.27	0.32	0.97	0.40
10-11 years of education	1.43	0.79	1.13	1.02
High-school degree	17.33	14.47	13.05	14.75
Some college	35.26	24.83	24.94	19.84
4+ years of college	45.71	59.60	59.91	63.99
Total	100.00	100.00	100.00	100.00
N	8,802	72,069	6,715	67,121

Note: Only marriages that women entered when they were between the ages of 16 and 40 are included. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations.

Sources: CPS March&June and National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.10: Survival rates of non-homogamous (i.e., hypogamous (W>H) and hypergamous (W<H)) couples relative to homogamy (W=H)

	All marriages		First marriages		Higher order marriages	
(1)	(2)	(3)	(4)	(5)	(6)	
		Up to 2 year old marriages				
Non-Homogamous	-0.017^{***}	-0.017^{***}	-0.019^{***}	-0.019^{***}	-0.019^{**}	-0.019^{**}
	(0.004)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
Observations	28,331	28,331	16,122	16,122	12,209	12,209
Marriages from	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986
Number of states	15	15	15	15	15	15
		Up to 4 year old marriages				
Non-Homogamous	-0.013^{**}	-0.013^{**}	-0.012^{*}	-0.012^{*}	-0.018^{*}	-0.017^{*}
	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)
Observations	27,548	27,548	15,649	15,649	11,899	11,899
Marriages from	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages, where women are entering their marriage aged 16-40, are included. The specifications also control for the age group of wife, higher order and inter-racial marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use the states where we have less than 3 years of coverage, and state-year pairs with fewer than 50 observations.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.11: The impact of UDL on marriage inflow structure

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
UDL	1.082	0.941	1.053	0.945	1.214	0.898
	(0.132)	(0.060)	(0.102)	(0.059)	(0.225)	(0.080)
UDL x Non-Homogamy	0.928	0.928	0.928	0.928	0.961	0.960
	(0.050)	(0.050)	(0.053)	(0.053)	(0.034)	(0.034)
Non-Homogamy	0.354***	0.355***	0.323***	0.323***	0.463***	0.465***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.005)	(0.005)
Observations	17,101	17,101	8,611	8,611	8,490	8,490
Number of states	24	24	24	24	24	24
Marriages from	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988
State Specific TT		✓		✓		✓

Exponentiated coefficients; Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation (1). Only marriages, where women are entering their marriage aged 16-40, are included. Standard errors are clustered at the state level. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.12: The impact of UDL on marriage inflow structure - economics categorization

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
UDL	1.064	0.920	1.046	0.934	1.210	0.884
	(0.124)	(0.064)	(0.098)	(0.067)	(0.218)	(0.083)
UDL x H = W	1.014	1.016	1.002	1.002	1.005	1.012
	(0.036)	(0.036)	(0.040)	(0.040)	(0.029)	(0.029)
UDL x W > H	0.891	0.894	0.880	0.882	0.921	0.930
	(0.086)	(0.087)	(0.090)	(0.090)	(0.086)	(0.084)
H = W	2.120***	2.116***	2.442***	2.441***	1.406***	1.396***
	(0.063)	(0.063)	(0.087)	(0.087)	(0.029)	(0.029)
W > H	0.573***	0.572***	0.634***	0.633***	0.422***	0.422***
	(0.036)	(0.036)	(0.049)	(0.049)	(0.013)	(0.013)
Observations	$16,977$	$16,977$	$8,583$	$8,583$	$8,394$	$8,394$
Marriages from	$1970-1988$	$1970-1988$	$1970-1988$	$1970-1988$	$1970-1988$	$1970-1988$
Number of states	24	24	24	24	24	24
State FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Exponentiated coefficients; Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation (1). Only marriages, where women are entering their marriage aged 16-40, are included. Standard errors are clustered at the state level. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.13: The impact of UDL on marriage inflow structure - economics categorization

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
UDL	1.083	0.937	1.050	0.938	1.218	0.895
	(0.130)	(0.059)	(0.100)	(0.058)	(0.224)	(0.080)
UDL x Non-Homogamy	0.935	0.935	0.940	0.940	0.962	0.960
	(0.044)	(0.044)	(0.045)	(0.045)	(0.036)	(0.035)
Non-Homogamy	0.359***	0.359***	0.327***	0.327***	0.467***	0.471***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.006)	(0.006)
Observations	16,977	16,977	8,583	8,583	8,394	8,394
Marriages from	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988	1970 – 1988
Number of states	24	24	24	24	24	24
State FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓	✓	✓

Exponentiated coefficients; Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation (1). Only marriages, where women are entering their marriage aged 16-40, are included. Standard errors are clustered at the state level. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.14: The impact of UDL on 2-year marriage survival: hypogamous (W>H) and homogamous (W=H) couples relative to hypergamous (H>W) - Poisson model

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
			Up to 2 year old marriages			
UDL	-0.001	-0.015	-0.028	-0.023^{***}	0.046	-0.001
	(0.030)	(0.012)	(0.019)	(0.009)	(0.034)	(0.019)
H = W	-0.002	-0.002	0.006^{*}	0.006^{*}	-0.012^{**}	-0.012^{**}
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)
W > H	-0.054^{***}	-0.054^{***}	-0.047^{***}	-0.047^{***}	-0.065^{***}	-0.065^{***}
	(0.009)	(0.009)	(0.011)	(0.011)	(0.010)	(0.010)
UDL x H = W	0.009	0.008	0.015^{**}	0.015^{**}	-0.001	-0.002
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
UDL x W > H	0.023^{**}	0.023^{**}	0.037^{***}	0.037^{***}	-0.010	-0.010
	(0.011)	(0.011)	(0.011)	(0.012)	(0.019)	(0.019)
Observations	28952	28951	16403	16402	12549	12548
Marriages from	1970 – 1986	1970 – 1986	1970 – 1986	1970 – 1986	1970 – 1986	1970 – 1986
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages, where women are entering their marriage aged 16-40, are included. The specifications also control for the age group of wife, higher order and inter-racial marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use the states where we have less than 3 years of coverage, and state-year pairs with fewer than 50 observations.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.15: The impact of UDL on 4-year marriage survival: hypogamous (W>H) and homogamous (W=H) couples relative to hypergamous (H>W) - Poisson model

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
			Up to 4 year old marriages			
UDL	−0.001	−0.017	−0.041	−0.041	0.072	0.025
	(0.067)	(0.041)	(0.052)	(0.035)	(0.072)	(0.040)
H = W	−0.021**	−0.021**	−0.014	−0.015*	−0.030***	−0.029**
	(0.010)	(0.010)	(0.009)	(0.009)	(0.011)	(0.011)
W > H	−0.084***	−0.084***	−0.072***	−0.072***	−0.101***	−0.101***
	(0.019)	(0.019)	(0.018)	(0.017)	(0.022)	(0.022)
UDL x H = W	0.023*	0.022*	0.030***	0.030***	0.011	0.008
	(0.012)	(0.012)	(0.011)	(0.012)	(0.018)	(0.018)
UDL x W > H	0.033	0.032	0.045**	0.045**	−0.000	−0.001
	(0.021)	(0.021)	(0.018)	(0.018)	(0.027)	(0.027)
Observations	28403	28403	16013	16013	12390	12390
Marriages from	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984	1970 − 1984
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages, where women are entering their marriage aged 16-40, are included. The specifications also control for the age group of wife, higher order and inter-racial marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use the states where we have less than 3 years of coverage, and state-year pairs with fewer than 50 observations.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.16: The impact of UDL on 2-year marriage survival: hypogamous (W>H) and homogamous (W=H) couples relative to hypergamous (H>W) - economics categorization

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
		Up to 2 year old marriages				
UDL	−0.003	−0.047*	−0.024	−0.052*	0.032	−0.037
	(0.027)	(0.024)	(0.017)	(0.025)	(0.031)	(0.023)
H = W	−0.018*	−0.018*	−0.019	−0.018	−0.014**	−0.014**
	(0.009)	(0.009)	(0.012)	(0.012)	(0.006)	(0.006)
W > H	−0.053***	−0.053***	−0.040***	−0.040***	−0.073***	−0.072***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.013)	(0.012)
UDL x H = W	0.014	0.013	0.021	0.021	0.004	0.004
	(0.010)	(0.011)	(0.014)	(0.014)	(0.013)	(0.013)
UDL x W > H	0.031***	0.031***	0.030***	0.030***	0.018	0.018
	(0.009)	(0.009)	(0.008)	(0.008)	(0.021)	(0.021)
Observations	28,221	28,221	16,130	16,130	12,091	12,091
Marriages from	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986	1970 − 1986
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages, where women are entering their marriage aged 16-40, are included. The specifications also control for the age group of wife, higher order and inter-racial marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use the states where we have less than 3 years of coverage, and state-year pairs with fewer than 50 observations.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.17: The impact of UDL on 4-year marriage survival: hypogamous (W>H) and homogamous (W=H) couples relative to hypergamous (H>W) - economics categorization

	All marriages		First marriages		Higher order marriages	
	(1)	(2)	(3)	(4)	(5)	(6)
		Up to 4 year old marriages				
UDL	-0.016	-0.063	-0.058	-0.088	0.056	-0.019
	(0.066)	(0.063)	(0.052)	(0.063)	(0.071)	(0.057)
H = W	-0.042^{**}	-0.043^{**}	-0.043^{**}	-0.044^{**}	-0.040^{**}	-0.040^{**}
	(0.017)	(0.017)	(0.019)	(0.019)	(0.015)	(0.015)
W > H	-0.092^{***}	-0.093^{***}	-0.075^{***}	-0.075^{***}	-0.117^{***}	-0.117^{***}
	(0.018)	(0.018)	(0.013)	(0.013)	(0.026)	(0.026)
UDL x H = W	0.029	0.029	0.040^{*}	0.040^{*}	0.013	0.012
	(0.021)	(0.021)	(0.021)	(0.021)	(0.028)	(0.028)
UDL x W > H	0.048^{**}	0.048^{**}	0.045^{***}	0.046^{***}	0.037	0.036
	(0.019)	(0.019)	(0.013)	(0.013)	(0.029)	(0.029)
Observations	27,507	27,507	15,689	15,689	11,818	11,818
Marriages from	1970 – 1984	1970 – 1984	1970 – 1984	1970 – 1984	1970 – 1984	1970 – 1984
Number of states	15	15	15	15	15	15
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages, where women are entering their marriage aged 16-40, are included. The specifications also control for the age group of wife, higher order and inter-racial marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use the states where we have less than 3 years of coverage, and state-year pairs with fewer than 50 observations.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.18: UDL and match quality among newlyweds: homogamous (W=H) and hypogamous (W>H) wives' higher-order marriages relative to hypergamy (H>W)

	Husband's higher order marriage, %		Inter-racial marriage, %		Absolute within-couple age diff.	
	(1)	(2)	(3)	(4)	(5)	(6)
UDL x H=W	0.335	0.388	−0.029	−0.030	−0.019	−0.019
	(0.601)	(0.611)	(0.291)	(0.292)	(0.074)	(0.075)
UDL x W>H	−1.017	−1.093	0.564	0.564	−0.089	−0.090
	(1.394)	(1.385)	(0.511)	(0.513)	(0.116)	(0.116)
UDL	3.043	0.530	0.117	1.254	0.163	0.073
	(2.793)	(1.078)	(0.540)	(0.899)	(0.139)	(0.129)
H=W	−0.012	−0.031	−0.318***	−0.316***	−0.524***	−0.523***
	(0.543)	(0.545)	(0.056)	(0.056)	(0.070)	(0.070)
W>H	3.073***	3.089***	−0.243***	−0.246***	0.019	0.020
	(0.923)	(0.926)	(0.048)	(0.047)	(0.109)	(0.109)
Observations	23,592	23,592	22,297	22,297	23,596	23,596
Marriages from	1970 − 1988	1970 − 1988	1970 − 1988	1970 − 1988	1970 − 1988	1970 − 1988
Number of states	24	24	23	23	24	24
State FEs	✓	✓	✓	✓	✓	✓
Marriage year FEs	✓	✓	✓	✓	✓	✓
Marriage month FEs	✓	✓	✓	✓	✓	✓
State Specific TT		✓		✓		✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The share of higher-order-marriage wives marrying higher-order-marriage husbands and the share of inter-racial marriages are expressed in percentage points. The within couple age gap is defined in years. The specifications also control for the age group of wives. Only marriages, where women are entering their marriage aged 16-40, are included. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. The number of observations is smaller for specifications in columns (3) and (4) because for some states the information on race is missing.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 1.19: The impact of UDL on the share of divorces applied for by wives: homogamous (W=H) and hypogamous (W>H) higher-order marriages relative to hypergamy (H>W)

	(1)	(2)
UDL	0.062***	0.059***
	(0.010)	(0.011)
UDL x H = W	−0.049***	−0.049***
	(0.010)	(0.010)
UDL x W > H	−0.050***	−0.050***
	(0.016)	(0.016)
H = W	0.128***	0.128***
	(0.004)	(0.004)
W > H	0.244***	0.243***
	(0.012)	(0.012)
Observations	22,675	22,675
Marriages from	1970 − 1988	1970 − 1988
Divorces from	1974 − 1988	1974 − 1988
Number of states	20	20
State FEs	✓	✓
Marriage year FEs	✓	✓
Marriage month FEs	✓	✓
State Specific TT		✓

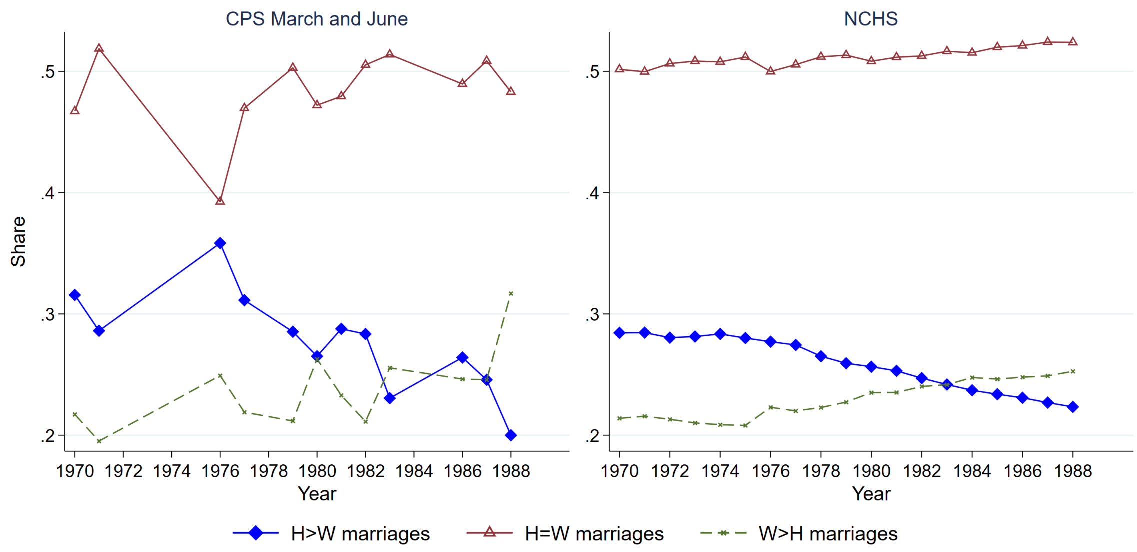
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Only marriages, where women are entering their marriage aged 16-40, are included. The specifications also control for the age group of wife and inter-racial marriages. Standard errors are clustered at the state level, regressions are weighted by state-year population. TT stands for time trends. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. We use divorce certificates starting from 1974 since information on which spouse applied for divorce is not available from earlier years. We study only divorces from marriages that began during 1970-1988.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

1.B Appendix: Figures

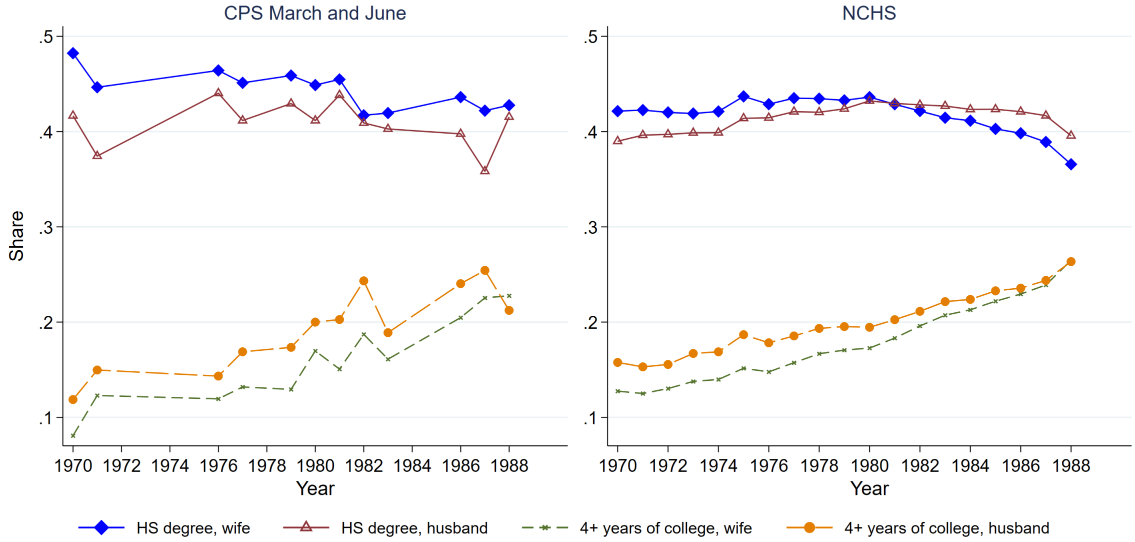
Figure 1.3: The educational structure of newlyweds



Note: Only marriages, where women are entering their marriage aged 16-40, are included from both CPS and NCHS. In NCHS, as in the rest of the paper, we do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. In CPS data, we proxy newlyweds using marriages that are less than one year old at the time of the survey. Here NCHS is also restricted to first order marriages for the graphs to be comparable. The sample sizes are as follows: total number observations N=4,319 in CPS and N=2,545,052 in NCHS.

Sources: CPS March&June and National Vital Statistics System of the National Center for Health Statistics (NCHS).

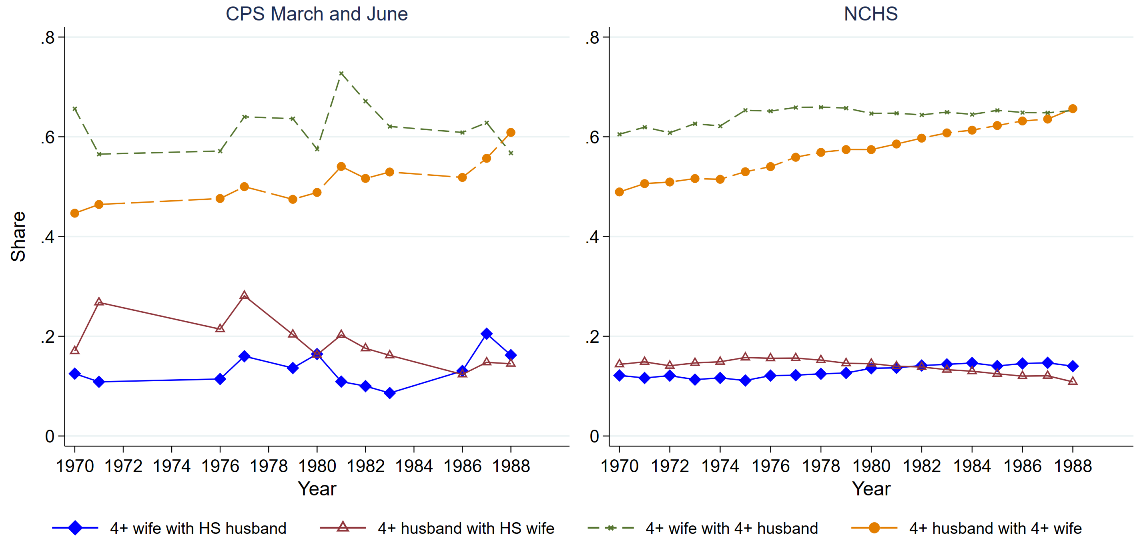
Figure 1.4: The share of newlywed wives (husbands) by their education level by year of marriage



Note: Only marriages, where women entering first marriage are aged 16-40, are included from both CPS and NCHS. In NCHS, as in the rest of the paper, we do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. In CPS data, we proxy newlyweds using marriages that are less than one year old at the time of the survey. Here NCHS is also restricted to first order marriages for the graph to be comparable to CPS. The sample sizes for the two displayed educational categories are as follows: for wives N=2,603 in CPS and N=1,546,877 in NCHS; for husbands N=2,589 in CPS and N=1,598,934 in NCHS.

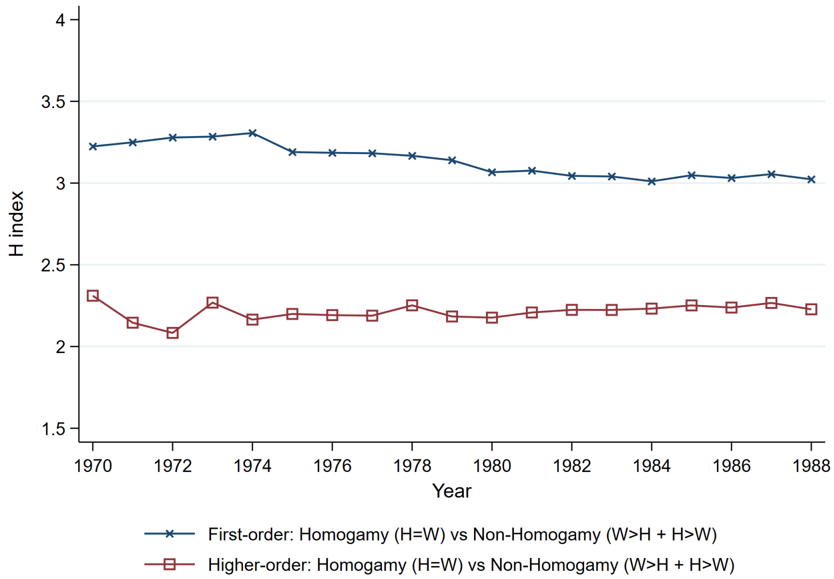
Sources: CPS March&June and National Vital Statistics System of the National Center for Health Statistics (NCHS).

Figure 1.5: The educational structure of partners of newlyweds with 4+ years of college



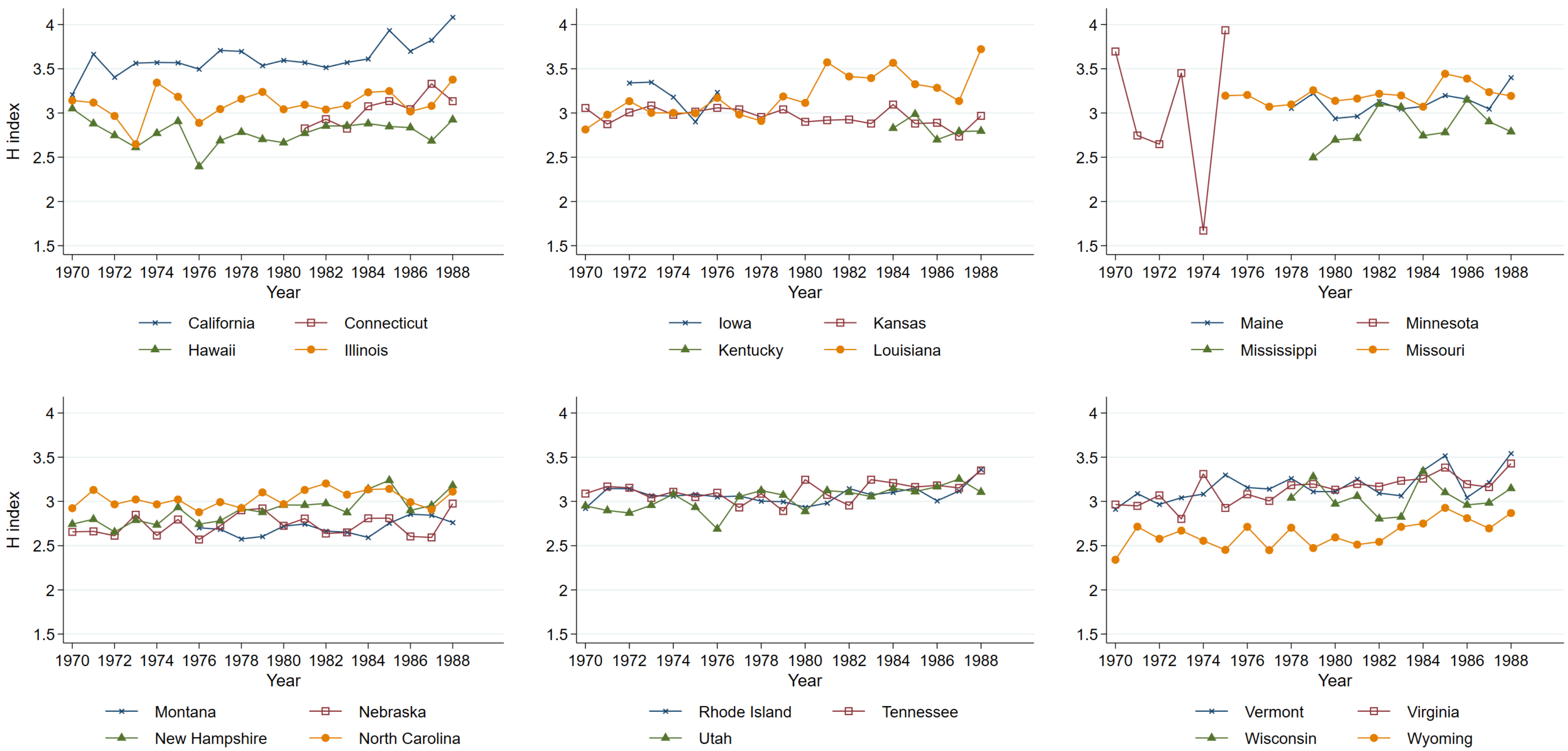
Note: Only marriages, where women are entering their marriage aged 16-40, are included from both CPS and NCHS. In NCHS, as in the rest of the paper, we do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. In CPS data, we proxy newlyweds using marriages that are less than one year old at the time of the survey. Here NCHS is also restricted to first order marriages for the graph to be comparable to CPS. The sample sizes are as follows: total number of wives with 4+ years of college N=684 in CPS and N=490,685 in NCHS; total number of husbands with 4+ years of college N=825 in CPS and N=534,462 in NCHS.
Sources: CPS March&June and National Vital Statistics System of the National Center for Health Statistics (NCHS).

Figure 1.6: Homogamy Evolution in Marriage Inflow (Newlyweds) for First and Higher-Order Marriages, NCHS Data



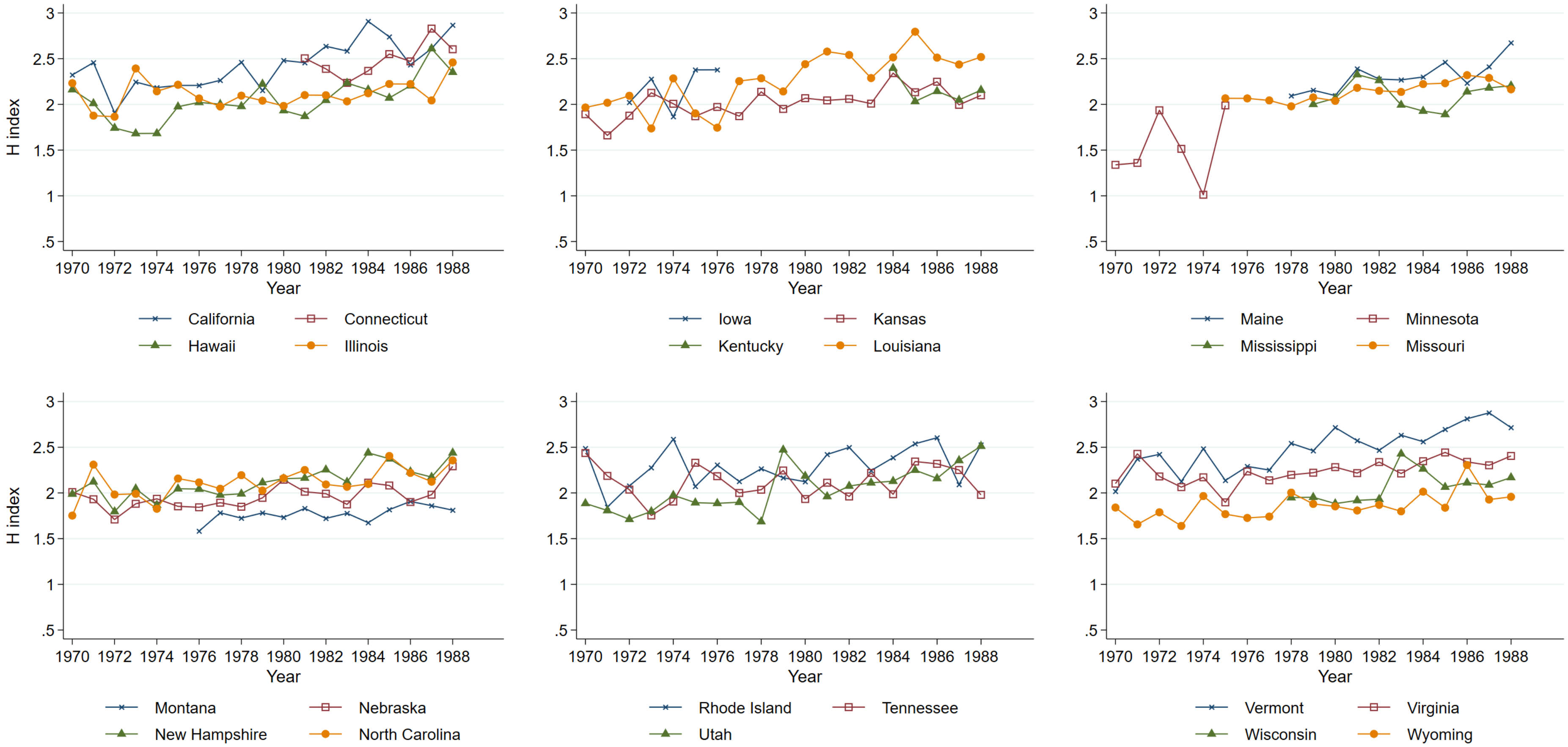
Note: Only marriages, where women are entering their marriage aged 16-40, are included. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations. We control for state fixed effects and state specific time trends.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Figure 1.7: Homogamy Evolution in Marriage Inflows (Newlyweds) for First-Order Marriages, NCHS Data



Note: Only marriages, where women are entering their marriage aged 16-40, are included. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Figure 1.8: Homogamy Evolution in Marriage Inflows (Newlyweds) for Higher-Order Marriages, NCHS Data



Note: Only marriages, where women are entering their marriage aged 16-40, are included. We do not use data from states where we have less than 3 years of coverage, or state-year pairs with fewer than 50 observations.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

1.C Appendix: NCHS Data

The National Vital Statistics System of the National Center for Health Statistics (NCHS) certificate data include information that couples report in order to apply for marriage or divorce during 1968-1995: residency, education, race and age of bride and groom, date of marriage/divorce, and the number of previous marriages. The education of spouses is measured in years of schooling. To check for sensitivity as suggested in Gihleb & Lang (2020), we use two different categorizations of education, one from sociology (Schwartz & Mare, 2005), the other from economics (Acemoglu & Autor, 2011). The sociology literature defines 5 categories of education as: < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. The economics literature separates education categories differently: < 12 years, 12 years/high school, some college, 4 years of college, 4+ years of college. Our marginal-free measures of homogamy are not sensitive to the specification of educational categories. We report baseline estimates based on the 'sociology' classification; Appendix A shows selected estimates based on the alternative categorization.

The NCHS database covers all marriages and divorces in small-population states; in large-population states the data correspond to 10-50% random samples. In all calculations, we thus adjust for NCHS sampling rates. Our analysis focuses on 1970-1988, because information on the education level of partners is only available during this period in the certificate data. The number of states with available education information grows over time. In total, 24 states provide education information in marriage certificates (with the average number of observations across state-year cells being 31,149), while 22 states provide this information in divorce certificates (on average, 20,346 divorces are observed across state-year cells). Table 1.7 in Appendix 1.A shows details of the certificate data coverage.

We code the state of the UDL right-hand-side variable based on the year of divorce. While marriage inflow measures can be directly constructed from marriage certificate data, constructing outflow (divorce) measures requires us to combine information from marriage and divorce certificates. Consider measuring the share of marriages formed in year t in state s that survive more than two (four) years. In the absence of data on the cross-border mobility of recently married couples, we calculate the divorce rate (one minus the survival rate) as follows: The numerator is the number of separations (of a given educational type) registered in state s occurring at most two (four) years (measured in months) after the year of marriage t. The denominator equals the number of newlywed couples (of a given educational type) in state s and year t. We apply this approach based on combined certificate data to generate state-year pseudo stocks of first marriages registered during 1970-1988.

We cannot link marriage and divorce certificates for specific couples. This creates the potential for measurement error driven by unbalanced mobility of couples across state borders. The numerator of our divorce rate is measured without any error, but it includes couples divorcing in state s who married in states other than s. In our sample, the share of such 'cross-border' divorces within both two and four years of marriage is 19%. The true denominator of the divorce rate, which we do not observe, is affected by net cross-border mobility of recently married couples, including those who never divorce, such that their mobility is not observable in certificate data.[32] However, if the net cross-border mobility of recently married couples is independent of unilateral divorce legislation, mobility generates measurement error in our outcome variable that need not affect the consistency of the estimated effects of the key causal variable—the UDL indicator. Below we present CPS-based evidence supporting such independence.

Measuring the cross-border mobility of recently married couples is difficult even in the large CPS samples from the 1970s and 1980s due to data issues discussed in Section 3. Specifically, the state of residence as of one year prior is only available in CPS March, and only from 1982 onward (except in 1985), when the age of first marriage in CPS March is not available. Instead of studying the mobility of recently married couples, we therefore study the mobility of married couples who are similarly young as couples who married after 1970, which form the basis of our main analysis. Specifically, in the first two columns of Table 1.20, we focus on the 4,494 married couples in CPS March from 1982 to 1988 where the age of the wife is below 40, who moved across state borders in the prior year.

The second two columns offer evidence based on 5,486 such married couples aged up to 59. We form net annual migration flows across state pairs and ask whether these flows depend on the gap across the two states in employment rates and on the change in UDL status. The estimated coefficients from regressions controlling for year fixed effects and state-pair fixed effects, clustered at state-pair level, are shown in Table 1.20. The employment-gap coefficient has the expected sign and is significant in some of the

[32] A similar issue with cross-state mobility arises in survey data, such as the CPS, where one does not observe the state of marriage. Existing studies do not detail how they deal with cross-border mobility.

specifications, but the difference in UDL status (compared to no difference) across state borders does not significantly affect the migration flows of young couples, and has the opposite sign than expected; we conclude that measurement error is not a major threat to our main regression analysis of divorce behavior.

Table 1.20: Cross-state-border gross mobility of married couples

	age up to 40 years		age up to 59 years	
	(1)	(2)	(3)	(4)
Employment rate gap	-0.020^{**}	-0.014	-0.025^{***}	-0.007
	(0.009)	(0.021)	(0.009)	(0.021)
From NUDL to UDL	-0.005	-0.077	-0.165	-0.114
	(0.181)	(0.217)	(0.174)	(0.221)
From UDL to NUDL	-0.011	0.096	0.014	0.036
	(0.121)	(0.267)	(0.091)	(0.202)
Observations	2,901	2,901	3,279	3,279
Number of state pairs	897	897	944	944
Year FEs	✓	✓	✓	✓
Pair FEs	✓	✓	✓	✓
Both State Specific TT		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the natural logarithm of flows between state pairs within the prior year. The corresponding employment gap in percentage points is defined as the unemployment rate of the sending state minus the employment rate of the receiving state (the average employment gap in our sample is-0.8% with a 5.5 standard deviation). The residuals are clustered at the state-pair level.
Source: CPS Match, year coverage 1982-1984, 1986-1988.

1.D Appendix: Replicating the Reynoso (2022) Approach with CPS and NCHS Data

To study the composition of newlyweds, we also estimate the Reynoso (2022) equation

$$\begin{aligned} Ed^w_{cts} =& \beta_0 + \beta_1 U_{ts} + \beta_2 Ed^h_{cts} U_{ts} + \beta_{3t} Ed^h_{cts} + \beta_{4s} Ed^h_{cts} + \beta_{5s} t + \\ & + \gamma_t + \gamma_s + \epsilon_{cts}, \end{aligned}$$

where Ed^w_{cts} (Ed^h_{cts}) are years of education of wife (husband) for couple c in state s at time t (coded to at most 17 years) and U_{ts} is a dummy equal to one if state s at time t has unilateral divorce legislation in place. Next, γ_t and γ_s are time and state specific effects, respectively, and β_s and β_t control for the state- and year-specific association of spouses' education levels. In Table 1.21 we estimate this Reynoso (2022) specification on the certificate NCHS data and on the CPS, using a sample frame that maximizes comparability of year coverage.

Table 1.21: UDL impact on newlywed composition (education in years): CPS vs NCHS

	CPS		NCHS	
	(1)	(2)	(3)	(4)
$Educ^h$UD (β_2)	0.0972	0.0919	0.0211	0.0184
	(0.104)	(0.104)	(0.0404)	(0.0407)
Observations	8,276	8,276	1,184,891	1,184,891
Number of states	51	51	29	29
State Specific TT		✓		✓

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: We follow Reynoso (2022) sample definition. In columns (1)-(2) the sample consists of CPS couples entering first marriage within less than a year of the survey year (we use a one-year window rather than two years as in Reynoso (2022) to minimize survival biases), and in which the husband is at most 25 years old. We use the years 1970,1971, 1976,1977, 1979-1983, 1986-1988 in both datasets to match with CPS March coverage. In columns (3)-(4) we use marriage certificates. All specifications are for first marriages. Standard errors are clustered at the state level.
Source: CPS March&June and National Vital Statistics System of the National Center for Health Statistics (NCHS).

Chapter 2

Joint Custody Laws and Fertility Sorting on Education

2.1 Introduction

It is well documented development that since the late 1960's there has been a trend in the US for people to marry partners with the same education level - educational homogamy (Schwartz & Mare, 2005; Siow, 2015; Schwartz, 2013). According to Mare & Schwartz (2006) the trend towards having the same education level is even stronger among parents, since homogamous couples have more children than non-homogamous ones. Homogamous marriages are more stable (Schwartz, 2010; Schwartz & Han, 2014), and higher marriage stability increases the willingness to invest in marriage specific capital - children (Becker, 1981; Becker et al., 1977). If the decision to have children is highly dependent on marriage stability, then changes in the environment that affect marriage stability should also affect fertility decisions and thereby the fertility differentials of homogamous versus non-homogamous couples.

Educational assortative matching, its determinants and role in economic income inequality have been studied extensively by sociologists as well as by economists (Greenwood et al., 2014; Dupuy & Weber, 2018; Gonalons-Pons et al., 2021). In the context of intergenerational income inequality, educational matching of parents and its dynamics would be of great interest, since the educational composition of parents has a strong influence on investments in the human capital of children (Chiappori et al., 2017), and the children's educational assortativeness when they marry (Mare, 2016).[1] Yet, the large literature on changes in educational assortative matching typically studies all couples (cohabiting or married) (Schwartz & Mare, 2005; Siow, 2015; Schwartz, 2013), instead of parents in particular.

This paper contributes to the literature on educational assortative matching by being the first to analyze the effect of family legislation on fertility sorting on education. The legislation in question is joint custody laws (JCLs), which changed the impact of divorces on families. Before they were introduced, one parent (usually the mother) would have full custody of all mutual children. The other partner would have extremely limited rights to interact with his or her children. Over a period of decades, starting from 1973, many federal states of the US introduced JCLs, which meant that ex-partners were granted a say in important decisions in the children's upbringing, and the right to spend a certain amount of time with them. In the context of fertility, Halla (2013) finds that JCLs increase overall fertility, but he does not analyze the impact on fertility sorting or the odds of homogamy of parents.

In terms of fertility decisions, such legislation might deter couples in unstable relationships from having children together. For a parent that would otherwise receive sole custody (usually the mother), JCLs create the necessity to agree with an ex-partner and continuously interact with them after the relationship is over. Stable relationships would be less or not affected at all by this rationale, because the threat of this scenario is less probable. Since homogamous relationships are more stable on average (Schwartz, 2010) the introduction of JCLs should increase the odds of homogamy of parents, as non-homogamous couples are more likely to be deterred from having children than homogamous couples.

This paper analyzes if the hypothesis that JCLs increase the odds of homogamy of parents is valid. To do this I take advantage of the process of how JCLs were introduced in the US: gradually in more and more states, while some never adopted them (Table 2.5). This provides a source of quasi-experimental variation that enables a difference-in-differences identification strategy.[2] I rely on administrative data from US birth certificates. The certificates give detailed information on newborns and their parents. Among

[1]More recent studies also find a link between educational matching of parents and children's health outcomes (Rauscher, 2020; Abufhele et al., 2021; Pesando, 2022).

[2]According to Jacob (1988)[Ch.8] JCLs were passed in relative obscurity. For evidence of arbitrary assignment of JCLs across states and lack of geographical correlation see Halla (2013).

other things, they contain information on the parents' education and marital status. Since the data records the marriage status of parents only at the time of birth, I cannot distinguish whether the decision to have a child was made outside or inside of marriage. Therefore, most of the results are about all births together, rather than separately for in- and out-of-wedlock births.[3]

There are two challenges to address when trying to identify the effect of JCLs on the odds of homogamy of parents. Firstly, the changes in educational attainment of men and women over the period of interest and secondly the introduction of other reforms, the effect of which might confound my results. The first challenge comes from the fact that the education level in the US increased in the period the JCLs were introduced. Furthermore, this increase was much stronger for women (see Table 2.6, which concentrates on parents), which means the educational structure of relationships changed. To control for this change I implement the log-linear homogamy model from Schwartz & Mare (2005). The second challenge I address by adding an extensive list of dummies and interaction terms in my regressions, which control for all other family law reforms that might have impacted fertility patterns and assortative matching by education: Abortion laws, the Child Support Enforcement Amendments of 1984, the Family Support Act of 1988 and the introduction of unilateral divorce laws (UDL).

My analysis is structured in the following way. First, I calculate the marginal-free measure of educational assortative matching of parents - the odds of homogamy - by state and year. I find that homogamous couples are 3-4 times[4] more likely to have a child than non-homogamous ones. This is in line with the literature (Becker, 1981; Becker et al., 1977), which states that higher marriage stability increases the willingness to have children. Second, I calculate the odds of homogamy of parents separately for the states that do eventually introduce a form of JCL versus those that do not and compare their development over time in a graph. I find that the odds of homogamy of parents are generally higher in the states that adopt JCLs. The gap widens during the time period when most of the states introduced JCLs. As a third step, I combine the log-linear model of Schwartz & Mare (2005) with a difference-in-differences approach, utilizing the quasi-experimental setting of the gradual introduction of JCLs across different states. The

[3]Dividing the analysis between in- and out-of-wedlock births is problematic, because the marriage status is unavailable for some states for 1969-1979. Therefore, restricting to legitimacy status results in dropping these states. Nevertheless, I present the results of the divided analysis in parallel.

[4]The exact ratio depends on the order of birth and the year being analyzed (Figure 2.5).

estimates support the hypothesis that JCLs increase the odds of homogamy of parents. In the final step, I divide the analysis by different subgroups as a first insight into possible underlying mechanisms.

This paper contributes to several strands in the literature. First, I bring new evidence on family reforms affecting fertility decisions (Halla, 2013; Alesina & Giuliano, 2006). By focusing on the possible differentiated impact by educational match, I show that these reforms affect the educational structure of fertility. To my knowledge, my paper is the first to examine this differential impact. I also extend the literature on educational homogamy and assortative matching of couples (Schwartz & Mare, 2005; Siow, 2015) by focusing on parents.[5] By increasing the understanding of how legislation can influence family decisions, my results could support policy-makers in their deliberations when making legislation that influences fertility, children's outcomes and intergenerational income inequality.

The remainder of this paper is organized as follows. Section 2 gives a short overview of JCL reforms. Section 3 describes the birth certificate data. Section 4 introduces the log-linear homogamy model from Schwartz & Mare (2005), the identification strategy and the main results. Section 5 provides a first insight into possible mechanisms and Section 6 concludes.

2.2 JCL Reforms in the US

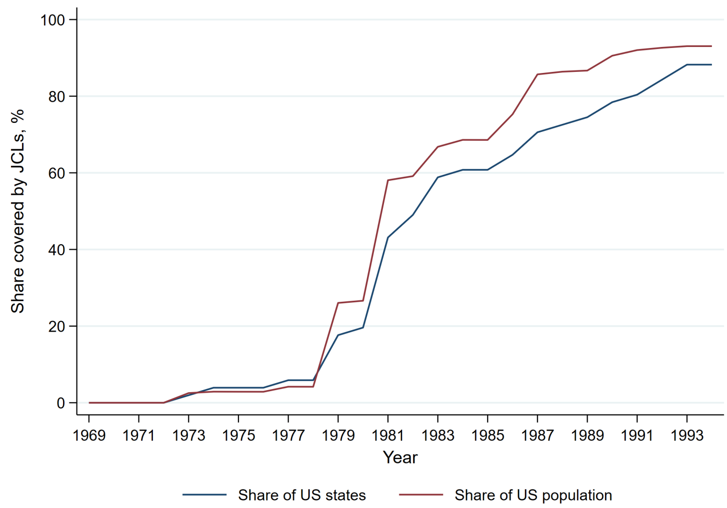
In the second half of the 20th century the legal framework governing family matters in the US underwent several substantial changes. Some changes, like the legalization of abortion and the introduction of unilateral divorce laws, have been intensively scrutinized for their effects in the demographic and economic literature (Alesina & Giuliano, 2006; Olivetti & Petrongolo, 2017; Wolfers, 2006; Stevenson & Wolfers, 2007). Another major change, the introduction of joint custody laws (JCLs), has received relatively little academic attention so far.

JCLs changed the rights of interaction of parents with their children after a divorce. Before the introduction of JCLs, after a divorce it was the norm that one parent would get sole custody over the mutual children. The other parent only had very limited visiting and

[5]Mare (2016) provides preliminary evidence on the assortative matching of parents, but the analysis has several drawbacks. He uses a small set of survey data and is constrained to make assumptions on the age of married parents when they have a child.

decision-making rights. Officially, the regime was gender-neutral. Courts had to decide which of the parents they deemed more fit to take care of the children. In practice, in the 20th century usually the mother received full custody (Brinig & Buckley, 1998). Indiana was the first state to change this practice of sole custody in 1973. The newly instated JCLs stipulated that decisions concerning children should be made jointly by both parents after a divorce and they extended visitation rights. Between 1973 and 2003 all but two states introduced JCLs in some form. Figure 2.1 illustrates the coverage of JCLs in the US over time. There are some differences in JCL regulations between states. The biggest is that in most of the states judges received the full power to mandate joint custody if they consider it to be in the children's best interest. Only six states deviate from this practice - they require agreement between parents for joint custody.[6]

Figure 2.1: Coverage of JCL reforms by year (%)



Note: The figure shows the percentage share (i) of reform states relative to the total number of states and (ii) of the population in reform states relative to the total US population by years. JCL years are according to Halla (2013).
Source: US Census Bureau, Population Estimates.

As more and more states adopted JCLs, joint legal custody became the new normal. By the late 1990s joint legal custody was granted in around half of all US divorces (Seltzer, 1998). Higher income and education are strong predictors of joint custody arrangements (Cancian & Meyer, 1998; Meyer et al., 2017; Cancian et al., 2014), because to share custody both parents have to be able to provide a sufficiently sized living area

[6]Dropping these states does not affect the main results in any meaningful way.

for the child(ren) and highly educated parents are more likely to value the involvement of both parents in a child's development (Juby et al., 2005). Halla (2013) analyzes the impact of JCLs on family structure and outcomes. His empirical evidence suggests that with JCLs female labor market participation, male suicide rates and domestic violence decreased, while the divorce rates of older couples, marriage- and fertility rates increased. In this paper I ask whether JCLs affected fertility differently depending on the educational composition of couples. To answer this question, I utilize the state and year variation in the implementation of JCLs as a source of quasi-experimental variation. Appendix Table 2.5 lists all states and if and when they introduced a JCL.

JCLs might impact the educational composition of couples who have children, because they can deter couples in unstable relationships from having children together. JCLs create the necessity to agree with an ex-partner and continuously interact with them, even after a divorce. Partners in stable relationships should not be negatively affected by this prospect - they are not concerned that it could materialize. Homogamous relationships are more stable on average (Schwartz, 2010) and also more congruent in their value systems, cultural background and lifestyle (DiMaggio & Mohr, 1985; Kalmijn, 1994). So even if they divorce, agreeing on how to raise their children should on average be easier for them than for non-homogamous couples. Therefore, as JCLs are more likely to deter non-homogamous couples from having children than homogamous couples, the introduction of JCLs should increase the odds of homogamy of parents.

As mentioned above, there were other major changes in US-family legislation during the period this paper studies. In 1973 abortion was legalized nationwide[7] and between the late 1960's and early 1990's a majority of states introduced unilateral divorce legislation (UDL), which made a divorce possible when only one of the spouses wants to get divorced. In the absence of fault, both spouses had to agree on a divorce prior to the introduction of UDL (see Afunts & Jurajda (2022)). Two further pieces of legislation changed the situation of families after a divorce in the 1980's. The 1984 Child Support Enforcement Amendments required states to have exact formulas and guidelines that judges could use to determine child support obligations, and to withhold child support payment from wages and/or other income of non-residing parents. Nevertheless, these amendments were just guidelines that judges could and did ignore (Garfinkel & Lanahan, 1990). Adherence to these guidelines was strengthened by the 1988 Family Support Act.

[7]After the 1973 Supreme Court decision on Roe v. Wade. Before that, abortion was legal in only five states since 1970.

Afterwards judges could deviate only with a special justification in written form. Other than JCLs and UDLs, both of these regulations were introduced nationwide at the same time. Throughout my analysis I control for the potential confounding effects of the mentioned law changes by including dummies that represent when and where the law changes were introduced and interaction terms of these dummies with the homogamy indicator.

2.3 Data

This analysis is based on administrative data - Natality Data from the National Vital Statistics System of the National Center for Health Statistics (NCHS).[8] The great advantage of using this data source is the wide coverage of states and number of observations per state and year. As the subject matter necessitates grouping couples into cells of a five-by-five education matrix, a large number of observations per state and year is vital to attain meaningful results. With many empty or scarcely filled cells in the matrix the regressions cannot yield reliable results at statistically significant levels. This is why longitudinal surveys covering a few thousand families across the entire US, e.g., PSID and NLSY, would not be suitable for my analysis.

The administrative data from the NCHS includes demographic and health characteristics of births in the US starting from 1968[9]. The sample coverage is large and increases over the years - starting with a 50% sample from each of the 50 states in the end of the 1960s. The number of states with 100% birth coverage increases from 6 states in 1972 to all states in 1985 (Figure 2.3). Additional to educational attainment, the data includes demographic variables including age, marital status, live-birth order, race and sex, and health and geographic variables including state, county, city etc.

I limit the analysis to mothers aged 15 to 49.[10] Outside these boundaries, fertility rates are extremely low. The NCHS data records parents' education in years of schooling. To define homogamy, I divide parents into 5 educational categories using approaches from

[8] The data used in this paper was obtained from the National Bureau of Economic Research (NBER) in September 2017 at https://www.nber.org/research/data/vital-statistics-natality-birth-data.

[9] The education levels of both parents are recorded from 1969.

[10] For the age range I follow Mare & Schwartz (2006) who also study educational homogamy of parents. In general, total fertility rates (TFR) are calculated using 5-year age groups in the age intervals 15-44 or 15-49, which are considered the fertile age range. The results displayed in the main part of the text are obtained using the wider age range, but running the analysis for the narrower age range does not change much about the results (Table 2.2 versus Table 2.7).

two different fields of literature: one from sociology (Schwartz & Mare, 2005) and one from economics (Acemoglu & Autor, 2011).[11] Gihleb & Lang (2020) argue that changes in homogamy are sensitive to these definitions of educational categories.[12] The main results I report are based on the categorization from the sociology literature. As a robustness check I re-ran all regressions using the economics categorization and the results are very similar (see Appendix 2.A).

There are two caveats about the NCHS data. The first is a limitation of the period of observation. Even though the last state only introduced JCL as late as 2003, the analysis is limited to 1994, because the NCHS stopped collecting information on father's educational attainment afterwards. This limitation of the time frame is only a minor drawback, though, as only four states introduced JCL after 1994 and the majority of states in terms of numbers and of US-population share had already introduced it by the late 1980's (see Figure 2.1).

The second caveat is that the records about parents' marital status have gaps and only relate to the time of birth, not the time of conception. JCL affects married couples, and therefore should mainly affect fertility decisions inside marriage. A detailed analysis by marital status at time of conception would be interesting. If the odds of homogamy of unmarried parents were not affected by JCL, while those of married parents were, this would corroborate the results. Unfortunately, there are significant coverage gaps for parents' marital status between 1969 and 1979 (see Appendix Table 2.5). These years are crucial for the analysis. Controlling for parents' marital status means dropping the states without that information. Therefore, for the main part of the paper I concentrate on all births together, rather than separately for in- and out-of-wedlock births. Nevertheless, I also perform the analyses by birth legitimacy status with the reduced data set and find no significant deviations from the main results.

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[11] In sociology the 5 categories of education are commonly defined as: < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. In economics authors usually separate education categories as: < 12 years, 12 years/high school, some college, 4 years of college, 4+ years of college.

[12] Gihleb & Lang (2020) show that the (unconditional) share of homogamous spouses increases in marriage stocks since the 1970s when based on the sociology categorization, but decreases when based on the economics one. Figure 2.4 displays that this is indeed the case when I use the (unconditional) share of homogamous parents.

2.4 Educational Homogamy of Parents by Regime

2.4.1 Controlling for Changes in the Distribution of Education

During the analysis-period of observation education levels in the US increased overall and even more drastically so for women. Thus, the distribution of education among parents changed considerably. Appendix Table 2.6 gives an overview of the development of fathers' and mothers' education levels over time. In the beginning of the period of observation there was a clear gap in education between the sexes in favor of fathers. By the end of this period the gap had almost closed completely, or mothers even overtook fathers, depending on how the education levels are categorized. For example, in 1970 only 23.9% of mothers had at least some college education compared to 31.5% of fathers. By 1990 these percentages had increased to 42.5% for mothers and 42.2% for fathers.

The closing gap in education naturally leads to an increase in homogamy, because when education is equally distributed between men and women it becomes more likely for everyone to meet an equally educated partner to have children with. Since I am interested in the effect of the introduction of JCL on the odds of homogamy I control for the closing gap in education. To do this I rely on the log-linear homogamy model by Schwartz & Mare (2005), which gives a marginal-free homogamy measure (odds of homogamy). Unlike the (unconditional) shares of homogamous couples, its results are the same, regardless of educational categorizations - economics or sociology.[13]

The model is based on the following regression:

$$ln\mu_{ijst} = \lambda + \lambda_{ij} + \sum_{n=s,t} (\lambda_{in} + \lambda_{jn}) + \gamma_t^D + \epsilon_{ijt}, \tag{2.1}$$

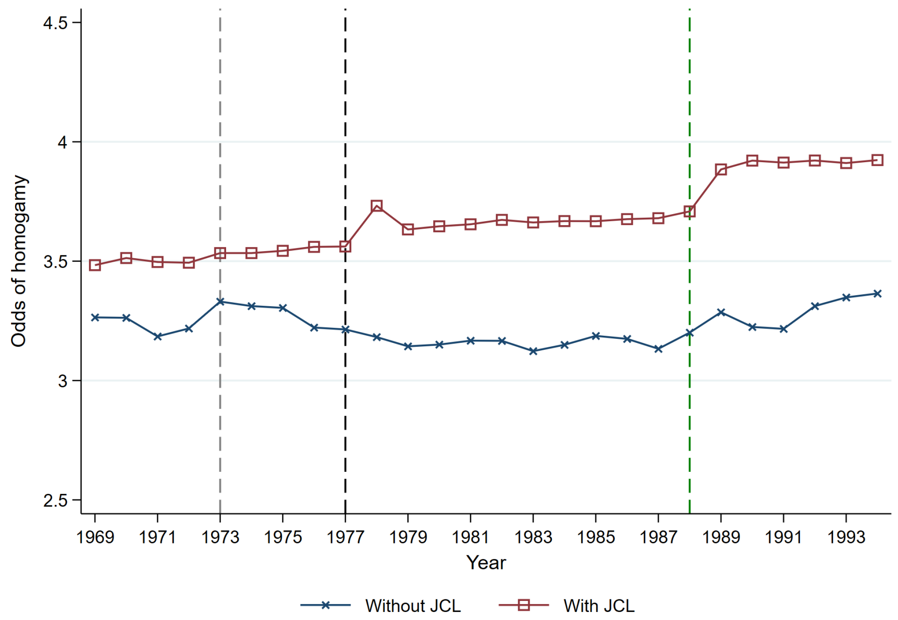
The subscripts i and j represent mothers' and fathers' educational categories, with $i, j = 1, 2...5$. The dependent variable μ_{ijst} counts the number of parents with a combination of educational categories ij in state s and year t. λ_{ij} is a set of fixed effects for each of the 5x5 ij match pairs. λ_{in} and λ_{jn}, $n = s, t$, are education level marginal fixed effects for each year and state. γ_t^D is a dummy for match pairs on the diagonal of the matrix - homogamous parents. The coefficient of this dummy represents the odds of homogamy when changes in the supply of education are controlled for.

[13] See footnote 11.

2.4.2 Odds of Homogamy over Time

Figure 2.2 shows the evolution of the coefficient of γ_t^D - the odds of homogamy - over time, separately for the states that eventually adopted JCLs (red line) and those that did not (blue line).[14] The dashed vertical lines represent important years for the JCLs and the development of homogamy of parents. 1973 (gray line) is the year the first state introduced JCLs. 1977 (black line) is the year the second state introduced JCLs, rapidly followed by more states (Appendix Table 2.5). 1988 (green line) marks the year the Family Support Act was passed nationwide.

Figure 2.2: Odds of homogamy evolution by regime



Note: The sample includes mothers aged 15-49. This figure uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

The states that adopted JCLs already have higher odds of homogamy compared to non-JCL states before the start of the reforms. In 1973 the first state, Indiana, introduced JCLs. The introduction starts a slight upward trend in homogamy of parents in the states that adopted JCLs, which becomes more pronounced when more states started

[14] If the odds of homogamy are equal to 3.5, it means that a person is 3.5 times more likely to have a child with someone with the same level of education rather than someone with different level of education.

adopting JCLs from 1977 onward. The states that did not adopt JCLs have a very slight downward trend in the odds of homogamy from 1973 to 1988. The early years show some more year-on-year variation, which can be explained by the vastly smaller sample size (44 vs. 6 states). The year 1988 marks an upward shift in levels of odds of homogamy for both groups.[15]

The upward trend in the group of states that adopted JCLs (which increases with the number of states that do so, with a simultaneously overall flat line for states that never adopted JCLs) is preliminary evidence for the hypothesis that JCLs increase the odds of homogamy. The upward shift in odds in 1988 can be explained by the introduction of the Family Support Act, which strengthened the enforcement of JCLs.

2.4.3 Difference-in-Differences Analysis by State and Year

The odds of homogamy of parents are higher and increase for all births in JCL states (Figure 2.2). I ask if, since JCLs require further cooperation on decisions concerning children even after a divorce, and homogamous marriages have fewer problems in this context, the introduction of JCLs explains some of the rise of homogamy. To answer this question, I introduce a JCL indicator and the interaction term between JCL and marriage type into the log-linear Equation 2.1, estimated first for all births and then for inside-marriage births separately. Table 2.1 columns (1)-(2) and (3)-(4) report these results, without and with state-specific time trends, respectively.[16] The coefficients indicate statistically significant, positive and sizeable effects of JCLs on the odds of homogamy of parents.[17] Furthermore, the coefficients of the JCL dummy and its interaction term with homogamy are larger and have smaller standard errors in the sample of only inside-marriage births. This is in line with the fact that JCL reforms should mainly affect inside-marriage births,

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[15]These dynamics are similar with economics categorization of education levels (Appendix Figure 2.6).

[16]Due to the small number of observations in 5x5 educational match matrices by state and year for out-of-wedlock births, the results on running the log-linear regression for only out-of-wedlock births would be unreliable.

[17]In our log-linear homogamy model (Equation 2.1 with JCLs)

$$ln\mu_{ijst} = \lambda + \lambda_{ij} + \sum_{n=s,t} (\lambda_{in} + \lambda_{jn}) + \gamma_t^D + \beta JCL_{st} + \epsilon_{ijst}$$

the impact of JCLs on the dependent variable is $ln\left(\frac{\mu_{ijst}|JCL_{st}=1}{\mu_{ijst}|JCL_{st}=0}\right) = exp(\beta)$. Accordingly, the states with JCL have $exp(\beta)$ times as many homogamous births as the states without JCL. Therefore, the closer $exp(\beta)$ is to one, the smaller is the impact.

since the JCL regulations do not change anything for unmarried couples.

Table 2.1: The impact of JCL on the odds of homogamy of parents

	All births		Inside-marriage births	
	(1)	(2)	(3)	(4)
JCL x Homogamy	1.103^{***}	1.102^{***}	1.107^{***}	1.108^{***}
	(0.038)	(0.039)	(0.036)	(0.036)
Homogamy	3.471^{***}	3.472^{***}	3.400^{***}	3.396^{***}
	(0.081)	(0.082)	(0.054)	(0.054)
JCL	0.891	0.910	0.955	0.947^{**}
	(0.067)	(0.056)	(0.027)	(0.025)
State FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
StatexTime		✓		✓
Observations	30,006	30,006	27,932	27,932
Births from	1969 – 1994	1969 – 1994	1969 – 1994	1969 – 1994
Number of states	51	51	51	51

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation 2.1. The sample includes mothers aged 15-49. This table uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

As a next step I add control variables for the other reforms happening during this time period that could affect my variable of interest. These reforms are the Child Support Enforcement Amendments 1984, the 1988 Family Support Act, UDL and the legalization of abortion. Table 2.2 summarizes these results. Controlling for other reforms shows that they confounded the baseline results (Table 2.1) to a considerable degree. Still, the effect of JCLs on the odds of homogamy of parents is statistically significant and positive.

The inclusion of most of the confounding reforms does not affect the JCL coefficients. The only exception are the dummy for the 1988 Family Support Act and its interaction term with homogamy. This act strengthened the JCLs by providing exact guidelines and enforcement mechanisms for joint child-care, especially the guidelines that determine the amounts of child support awards. It also strengthened the procedures of collecting the awards and increased the portion of children eligible to receive them (Garfinkel & Lanahan, 1990).[18] The coefficient of the 1988 Family Support Act and its interaction term with homogamy hint at an increase in the odds of homogamy of parents after 1988, in line with the corresponding jump in Figure 2.2. Nevertheless, the findings on the 1988 Family Support Act cannot be interpreted as a causal relationship, because the act was implemented simultaneously in all 50 states. This means I have no control group for a difference-in-differences identification strategy.

[18]Aside from child support provisions the 1988 Family Support Act also includes other reforms, such as provisions mandating that parents who receive welfare payments have to pursue employment.

Table 2.2: The impact of JCL on the odds of homogamy of parents - other reforms

	All births		Inside-marriage births	
	(1)	(2)	(3)	(4)
JCL x Homogamy	1.032**	1.031*	1.042**	1.042**
	(0.016)	(0.016)	(0.020)	(0.020)
Homogamy	3.305***	3.299***	3.273***	3.265***
	(0.064)	(0.063)	(0.054)	(0.054)
JCL	0.926	0.943	0.989	0.979
	(0.059)	(0.046)	(0.024)	(0.021)
1988 Family Support Act	1.281***	0.950	0.868**	0.801***
	(0.120)	(0.042)	(0.048)	(0.063)
1988 Family Support Act x Homogamy	1.118***	1.118**	1.101**	1.102**
	(0.048)	(0.050)	(0.044)	(0.043)
UDL	✓	✓	✓	✓
UDL x Homogamy	✓	✓	✓	✓
Child Support Enforcement Amendments 1984	✓	✓	✓	✓
Child Support Enforcement Amendments 1984 x Homogamy	✓	✓	✓	✓
Abortion	✓	✓	✓	✓
Abortion x Homogamy	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
StatexTime		✓		✓
Observations	30,006	30,006	27,932	27,932
Births from	1969 – 1994	1969 – 1994	1969 – 1994	1969 – 1994
Number of states	51	51	51	51

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation 2.1. The sample includes mothers aged 15-49. This table uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

As a robustness check I also run a linear regression version of Equation 2.1 to explore the sensitivity of the baseline estimates to the choice of the control group, following Sun & Abraham (2021). Specifically, I use the never-treated states as the control group.[19] As in the baseline regression, the coefficient of the interaction term between homogamy and JCL is statistically significant at the 1% level. The main findings are also not sensitive to several other robustness checks. The estimates are not affected by relying on the educational categories used in Acemoglu & Autor (2011) (economics categories, Tables 2.8 and 2.9), by dropping the 6 states that require parental agreement (Table 2.10) or by keeping only JCL states to make sure that the results are not due to some differences between JCL and non-JCL states (Table 2.11).

2.5 Mechanisms

In this section I want to inspect the mechanisms behind the increase in odds of homogamy for married couples caused by JCLs. Two separate channels for this effect

[19] I rely on the `eventstudyinteract` Stata command (Sun, 2021) as it can handle unbalanced panels.

are conceivable. First, the introduction of JCLs could affect already married couples' incentives to have children differently, depending on the couple's educational composition. Second, JCLs might change the incentive to marry or not, since they increase the chances of joint custody for married couples. Therefore, the adoption of JCLs might affect the educational structure of both fertility and new marriages.

The data does not permit me to see if a couple was married or not when a child was conceived. As a proxy approach, to see if rather the odds of homogamy of already-married parents increased or pregnant homogamous couples became relatively more likely to get married, I separate the previous analysis by birth order. If the effect of JCLs on the odds of homogamy of first time parents is higher than that of higher order parents, this would hint at the possibility that the incentives to marry were a stronger driver of the overall effect. The results in Table 2.3 imply that with JCLs homogamous married couples become more likely to have a 3+ child(ren) than non-homogamous couples, while there is no evidence of JCLs affecting the odds of homogamy of parents for first- and second-order births. These results are suggestive evidence for the first channel of increasing the relative likelihood of already married homogamous couples having children, compared to already married non-homogamous couples.

To corroborate the suggestive evidence from the regressions split by birth parity I extend the analysis by including marriage data. If, because of JCLs, homogamous pregnant couples became relatively more likely to marry than non-homogamous ones, this should show in the odds of homogamy of newlyweds. To check for the structural changes in marriage inflow caused by JCLs, I use the NCHS data on marriage certificates. This data reports all the characteristics that couples provide when applying for marriage, including education level and residency information.[20] I rely on the same log-linear regression as in Equation 2.1, but substitute the dependent variable with the number of newlyweds (instead of parents) with a combination of educational categories ij in state s and year t. Columns (1) and (2) of Table 2.4 show the results of these regressions, with and without state-specific time trends, respectively. There is no evidence of the introduction of JCLs affecting the odds of homogamy in marriage inflow. The channel that increases the odds of homogamy of parents is a relative increase in births of married homogamous couples rather than a change in the incentive to marry for pregnant couples.

[20]It has from 10 to 100% coverage of all marriages by state, which varies by years. For more detailed description of this data see Afunts & Jurajda (2022).

Table 2.3: The impact of JCL on the odds of homogamy of parents - inside-marriage and by the order of birth

	First child		Second child		3+ child	
	(1)	(2)	(3)	(4)	(5)	(6)
JCL x Homogamy	1.032	1.032	1.017	1.020	1.042^{***}	1.045^{***}
	(0.020)	(0.020)	(0.014)	(0.014)	(0.016)	(0.017)
Homogamy	3.258^{***}	3.252^{***}	3.251^{***}	3.245^{***}	3.135^{***}	3.126^{***}
	(0.048)	(0.049)	(0.066)	(0.066)	(0.069)	(0.068)
JCL	1.011	0.997	0.941	0.956	0.912	0.922^{*}
	(0.025)	(0.022)	(0.050)	(0.040)	(0.052)	(0.040)
1988 Family Support Act	0.898	0.805^{**}	1.312^{**}	0.975	1.143	0.849^{***}
	(0.076)	(0.083)	(0.141)	(0.053)	(0.112)	(0.033)
1988 Family Support Act x Homogamy	1.092^{**}	1.093^{**}	1.112^{**}	1.112^{**}	1.127^{**}	1.124^{**}
	(0.038)	(0.038)	(0.048)	(0.049)	(0.057)	(0.058)
UDL	✓	✓	✓	✓	✓	✓
UDL x Homogamy	✓	✓	✓	✓	✓	✓
Child Support Enforcement Amendments 1984	✓	✓	✓	✓	✓	✓
Child Support Enforcement Amendments 1984 x Homogamy	✓	✓	✓	✓	✓	✓
Abortion	✓	✓	✓	✓	✓	✓
Abortion x Homogamy	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
StatexTime		✓		✓		✓
Observations	27,641	27,641	29,697	29,697	29,845	29,845
Births from	1969 – 1994	1969 – 1994	1969 – 1994	1969 – 1994	1969 – 1994	1969 – 1994
Number of states	51	51	51	51	51	51

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to Equation 2.1. The sample includes mothers aged 15-49. This table uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 2.4: The impact of JCL on the odds of homogamy of marriage inflow

	(1)	(2)
JCL x Homogamy	0.986	0.985
	(0.015)	(0.015)
Homogamy	3.119***	3.103***
	(0.055)	(0.059)
JCL	0.956	1.023
	(0.037)	(0.028)
All reforms and their interactions as in Table 2.3	✓	✓
State FEs	✓	✓
Year FEs	✓	✓
StatexTime		✓
Observations	8,742	8,742
Marriages from	1970 − 1988	1970 − 1988
Number of states	24	24

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to Equation 2.1, but substitute the dependent variable with the number of newlyweds (instead of parents) with a combination of educational categories ij in state s and year t. The sample includes wives aged 15-49 as in the main part of the analyses of this paper. This table uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.

2.6 Conclusion

During the 1970's to 1990's newly introduced family reforms affected couples' decisions in many aspects, including the decision to have children. In this paper I use administrative data on births to analyze the effect of the introduction of joint custody laws (JCLs) on the educational structure of fertility. JCLs are sets of legislation that were introduced in most of the US from 1973 onward. Without this legislation, the norm was that one partner obtained sole custody of mutual children after a divorce. With JCLs the norm is that the divorcees share custody.

I investigate the hypothesis that the introduction of JCLs increased the odds of homogamy of parents (the relative likelihood of parents having the same level of education). The hypothesis is based on the reasoning that the prospect of continuously having to bargain with the divorced partner about decisions regarding the children might deter couples in unstable relationships from having children. Research has shown that homogamous couples are on average more stable (Schwartz, 2010; Schwartz & Han, 2014). Thus, they should on average be less concerned about the possibility of a divorce. Therefore, introducing JCLs should increase their likelihood of having children relative to non-homogamous couples, since those are more likely to divorce and therefore more affected by the introduction of JCLs. My results support the hypothesis that JCLs increased the odds of homogamy.

The results also suggest that the effect of JCLs on the odds of homogamy is driven by births in married couples. Therefore, an alternative explanation for the effect detected of JCLs on the odds of homogamy could be that the incentive to marry changes for pregnant couples. The first insights into possible mechanisms suggest that the homogamy of parents of third and higher-order children are affected, while that of first and second time parents is not. Furthermore, JCLs had no impact on the odds of homogamy of marriage inflows. Both findings indicate that the channel which increases the odds of homogamy of parents is a relative increase in births of married homogamous couples rather than a change in the incentive to marry for pregnant couples.

The empirical evidence that joint custody affects fertility rates (Halla, 2013) in combination with the evidence in this paper on its differential impact by educational composition of parents support the marriage market models with limited bargaining and imperfectly transferable utility (Galichon et al., 2019). I find that even though the educational structure of marriage inflows is unaffected by unilateral divorce legislation (UDL) (Afunts & Jurajda, 2022) or by JCLs, the educational structure of fertility is. The introduction of UDL, which came to most US states at least a few years (up to a few decades) earlier than JCLs (Table 2.1), increased the stability advantage of homogamous over non-homogamous marriages (Afunts & Jurajda, 2022). JCLs resulted in a relative increase in births of married homogamous couples.

2.A Appendix: Tables

Table 2.5: Reform years by states

State	Joint custody	UDL	Legitimacy status not reported in NCHS
Alabama	1997	1971	-
Alaska	1982	pre-1967	-
Arizona	1991	1973	-
Arkansas	2003	no	-
California	1979	1970	1968-1979
Colorado	1983*	1972	-
Connecticut	1981	1973	1968-1979
Delaware	1981	1968	-
D. of Columbia	1996	no	-
Florida	1979	1971	-
Georgia	1990	1973	1968-1979
Hawaii	1980	1972	-
Idaho	1982	1971	1968-1977
Illinois	1986	no	-
Indiana	1973	1973	-
Iowa	1977	1970	-
Kansas	1979	1969	-
Kentucky	1979	1972	-
Louisiana	1981	no	-
Maine	1981	1973	-
Maryland	1984	no	1968-1979
Massachusetts	1983	1975	1968-1977
Michigan	1981	1972	1978-1979
Minnesota	1981	1974	-
Mississippi	1983	no	-
Missouri	1983	no	-
Montana	1981	1973	1968-1979
Nebraska	1983^{+}	1972	-

Nevada	1981	1967	1971-1979
New Hampshire	1974	1971	-
New Jersey	1981	no	-
New Mexico	1982	pre-1967	1968-1979
New York	1981	no	1968-1979
North Carolina	1979*	no	-
North Dakota	1993	1971	-
Ohio	1981	1992	1969-1979
Oklahoma	1990	pre-1967	-
Oregon	1987^{+}	1971	-
Pennsylvania	1981	no	-
Rhode Island	1992	1975	-
South Carolina	1996	no	-
South Dakota	1989	1985	-
Tennessee	1986	no	-
Texas	1987	1970	1977-1979
Utah	1988	1987	-
Vermont	1992^{+}	no	1968-1977
Virginia	1987	no	-
Washington	no	1973	-
West Virginia	no	1984	-
Wisconsin	1979^{+}	1978	-
Wyoming	1993	1977	-

Note: Unilateral divorce law years are according to Voena (2015), joint custody years are according to Halla (2013). $^{+}$Both parents need to agree, i.e., parental agreement is required.
*Parental agreement is required until 1987.

Table 2.6: The % share of parents with different education levels

	Mother 1970	1980	1990	Father 1970	1980	1990
	General education shares by gender					
0-9 years of ed.	11.22	6.89	9.24	13.07	6.89	9.18
10-11 years of ed.	16.33	12.04	10.13	13.50	10.74	9.14
HS degree	48.57	45.57	38.08	41.96	41.60	39.46
Some college	14.41	19.42	22.01	15.02	18.47	18.42
4+ years of col.	9.47	16.08	20.53	16.45	22.29	23.79
Total	100.00	100.00	100.00	100.00	100.00	100.00
Observations	1,088,429	2,348,162	3,241,490	1,088,429	2,348,162	3,241,490

	1970	1980	1990
	Share with homogamous partners		
0-9 years of ed.	6.04	3.01	5.53
10-11 years of ed.	5.18	3.96	3.33
HS degree	27.94	27.20	23.72
Some college	4.85	7.06	8.24
4+ years of col.	7.13	11.81	14.52
Total homogamous	51.15	53.04	55.34
Total non-homogamous	48.85	46.96	44.66
Observations	1,088,429	2,348,162	3,241,490

Note: Our sample includes mothers aged aged 15-49.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 2.7: The impact of JCL on the odds of homogamy of parents - other reforms

	All births		Inside-marriage births	
	(1)	(2)	(3)	(4)
JCL x Homogamy	1.032**	1.031*	1.042**	1.042**
	(0.016)	(0.016)	(0.020)	(0.020)
Homogamy	3.304***	3.298***	3.271***	3.264***
	(0.064)	(0.063)	(0.054)	(0.054)
JCL	0.926	0.943	0.989	0.979
	(0.059)	(0.046)	(0.024)	(0.021)
1988 Family Support Act	1.282***	0.950	0.868**	0.801***
	(0.120)	(0.042)	(0.048)	(0.063)
1988 Family Support Act x Homogamy	1.118***	1.118**	1.101**	1.102**
	(0.048)	(0.050)	(0.043)	(0.043)
UDL	✓	✓	✓	✓
UDL x Homogamy	✓	✓	✓	✓
Child Support Enforcement Amendments 1984	✓	✓	✓	✓
Child Support Enforcement Amendments 1984 x Homogamy	✓	✓	✓	✓
Abortion	✓	✓	✓	✓
Abortion x Homogamy	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
StatexTime		✓		✓
Observations	30,005	30,005	27,931	27,931
Births from	1969 − 1994	1969 − 1994	1969 − 1994	1969 − 1994
Number of states	51	51	51	51

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation 1.1. The sample includes mothers aged 15-44. This table uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 2.8: The impact of JCL on the odds of homogamy of parents - economics categories

	All births		Inside-marriage births	
	(1)	(2)	(3)	(4)
JCL x Homogamy	1.107***	1.107***	1.115***	1.117***
	(0.032)	(0.032)	(0.032)	(0.032)
Homogamy	3.337***	3.336***	3.276***	3.271***
	(0.070)	(0.070)	(0.043)	(0.042)
JCL	0.888	0.909	0.954*	0.945**
	(0.066)	(0.053)	(0.026)	(0.024)
State FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
StatexTime		✓		✓
Observations	29,973	29,973	27,899	27,899
Births from	1969 – 1994	1969 – 1994	1969 – 1994	1969 – 1994
Number of states	51	51	51	51

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation 1.1. The sample includes mothers aged 15-49. This table uses economics categorization - < 12 years, 12 years/high school, some college, 4 years of college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 2.9: The impact of JCL on the odds of homogamy of parents - economics categories

	All births		Inside-marriage births	
	(1)	(2)	(3)	(4)
JCL x Homogamy	1.037**	1.037**	1.047**	1.047**
	(0.016)	(0.016)	(0.020)	(0.020)
Homogamy	3.162***	3.158***	3.125***	3.120***
	(0.063)	(0.062)	(0.044)	(0.043)
JCL	0.923	0.940	0.988	0.978
	(0.059)	(0.045)	(0.024)	(0.021)
1988 Family Support Act	0.996	0.790***	0.652***	0.602***
	(0.077)	(0.031)	(0.023)	(0.036)
1988 Family Support Act x Homogamy	1.099**	1.098**	1.089**	1.090**
	(0.041)	(0.041)	(0.038)	(0.038)
UDL	✓	✓	✓	✓
UDL x Homogamy	✓	✓	✓	✓
Child Support Enforcement Amendments 1984	✓	✓	✓	✓
Child Support Enforcement Amendments 1984 x Homogamy	✓	✓	✓	✓
Abortion	✓	✓	✓	✓
Abortion x Homogamy	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
StatexTime		✓		✓
Observations	29,973	29,973	27,899	27,899
Births from	1969 − 1994	1969 − 1994	1969 − 1994	1969 − 1994
Number of states	51	51	51	51

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation 1.1. The sample includes mothers aged 15-49. This table uses economics categorization - < 12 years, 12 years/high school, some college, 4 years of college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 2.10: The impact of JCL on the odds of homogamy of parents - without 6 states requiring parental agreement

	All births		Inside-marriage births	
	(1)	(2)	(3)	(4)
JCL x Homogamy	1.037**	1.036**	1.046**	1.046**
	(0.018)	(0.018)	(0.022)	(0.022)
Homogamy	3.321***	3.315***	3.284***	3.277***
	(0.072)	(0.071)	(0.062)	(0.062)
JCL	0.921	0.940	0.995	0.990
	(0.065)	(0.053)	(0.027)	(0.023)
1988 Family Support Act	1.268**	0.938	0.845***	0.789***
	(0.123)	(0.043)	(0.049)	(0.067)
1988 Family Support Act x Homogamy	1.121***	1.121**	1.104**	1.105**
	(0.049)	(0.051)	(0.045)	(0.045)
UDL	✓	✓	✓	✓
UDL x Homogamy	✓	✓	✓	✓
Child Support Enforcement Amendments 1984	✓	✓	✓	✓
Child Support Enforcement Amendments 1984 x Homogamy	✓	✓	✓	✓
Abortion	✓	✓	✓	✓
Abortion x Homogamy	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
StatexTime		✓		✓
Observations	26,106	26,106	24,258	24,258
Births from	1969 − 1994	1969 − 1994	1969 − 1994	1969 − 1994
Number of states	45	45	45	45

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation 1.1. The sample includes mothers aged 15-49. This table uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Table 2.11: The impact of JCL on the odds of homogamy of parents - only JCL states

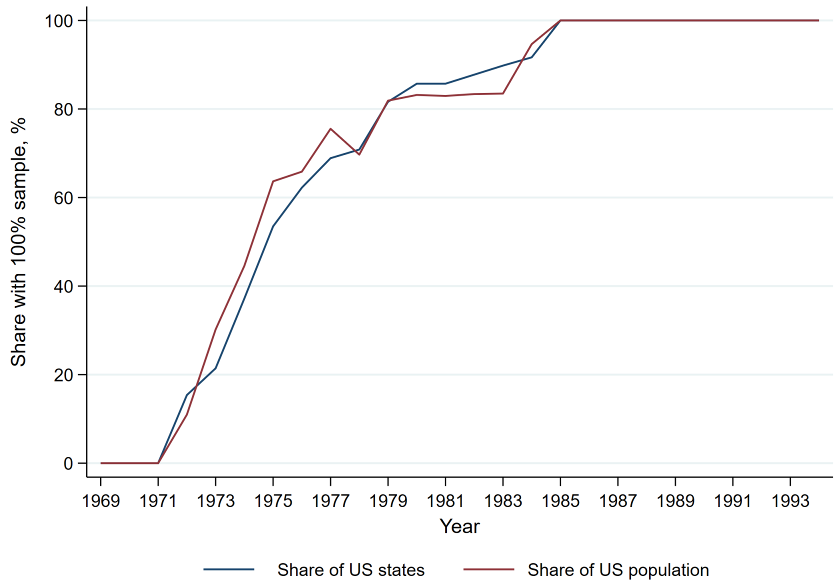
	All births		Inside-marriage births	
	(1)	(2)	(3)	(4)
JCL x Homogamy	1.022*	1.020*	1.035*	1.035*
	(0.012)	(0.011)	(0.019)	(0.018)
Homogamy	3.305***	3.299***	3.271***	3.264***
	(0.067)	(0.066)	(0.057)	(0.057)
JCL	0.901	0.945	0.984	0.980
	(0.063)	(0.044)	(0.026)	(0.022)
1988 Family Support Act	1.304***	0.955	0.884**	0.812**
	(0.119)	(0.044)	(0.051)	(0.067)
1988 Family Support Act x Homogamy	1.123**	1.123**	1.106**	1.106**
	(0.051)	(0.053)	(0.047)	(0.047)
UDL	✓	✓	✓	✓
UDL x Homogamy	✓	✓	✓	✓
Child Support Enforcement Amendments 1984	✓	✓	✓	✓
Child Support Enforcement Amendments 1984 x Homogamy	✓	✓	✓	✓
Abortion	✓	✓	✓	✓
Abortion x Homogamy	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
StatexTime		✓		✓
Observations	27,181	27,181	25,108	25,108
Births from	1969 − 1994	1969 − 1994	1969 − 1994	1969 − 1994
Number of states	45	45	45	45

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The estimates correspond to the log-linear model, Equation 1.1. The sample includes mothers aged 15-49. This table uses sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends. Standard errors are clustered at the state level. I do not use the state and year pairs that have observations for less than half of the 5x5 educational matrix, though including them does not affect the main results.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

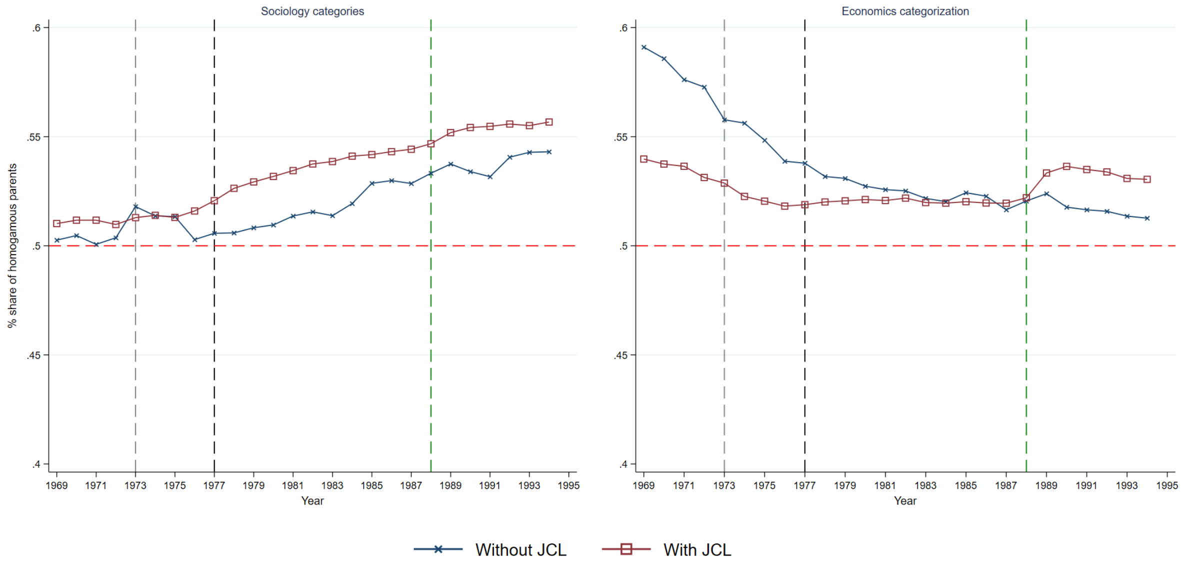
2.B Appendix: Figures

Figure 2.3: 100% birth coverage by year (%)



Note: The figure shows the percentage share (i) of states covering 100% of births in NCHS and (ii) of the population in these states covering 100% of births relative to the total US population by years.
Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

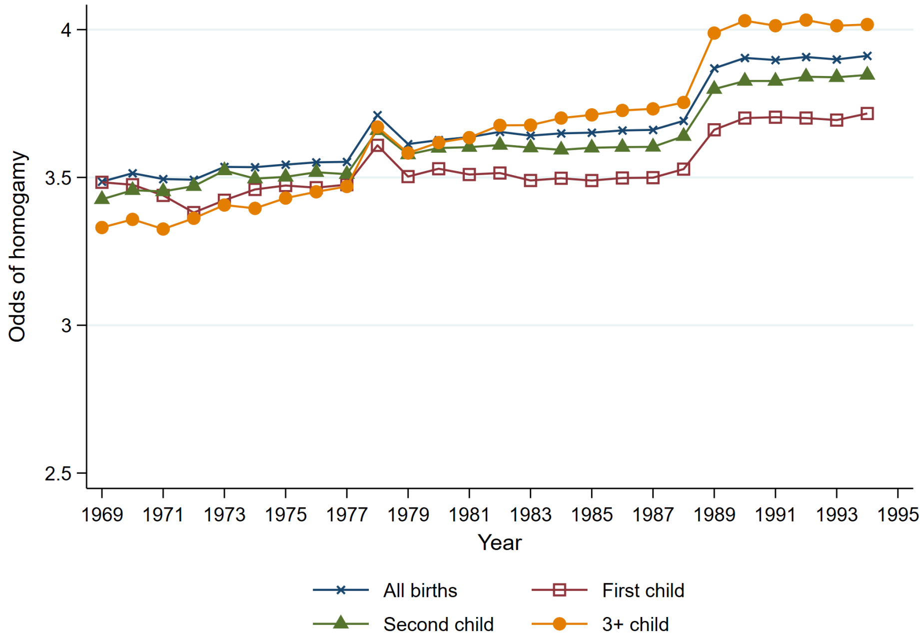
Figure 2.4: The share of homogamous parents by regime and educational categorization



Note: The sample includes mothers aged 15-49. Education categories are defined as: sociology categories - < 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college; economics categorization - < 12 years, 12 years/high school, some college, 4 years of college, 4+ years of college.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

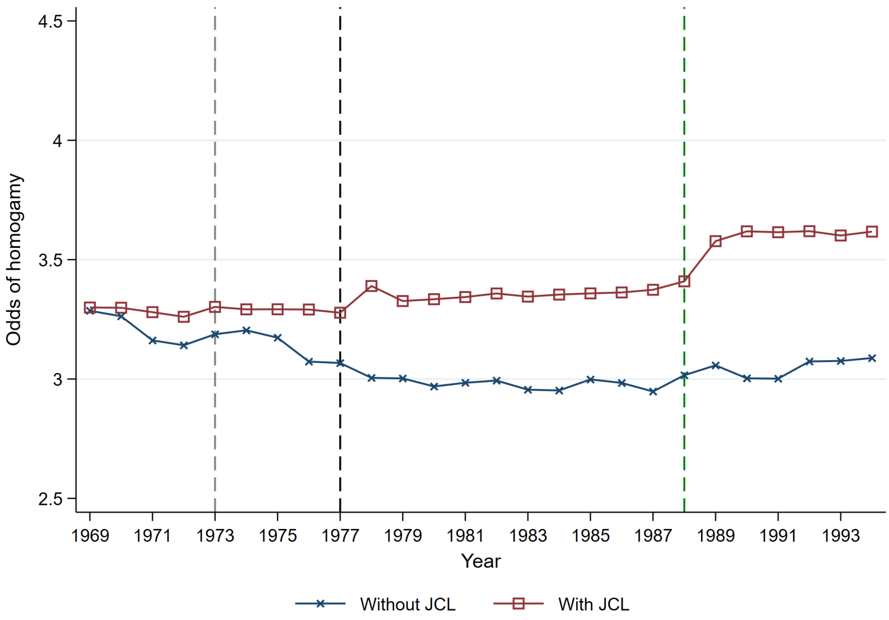
Figure 2.5: Odds of homogamy evolution by order of birth



Note: The sample includes mothers aged 15-49. This figure uses sociology categories - $<$ 10 years, 10-11 years, 12 years/high school, some college, 4+ years of college. I control for state fixed effects and state-specific time trends.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Figure 2.6: Odds of homogamy evolution by regime



Note: The sample includes mothers aged 15-49. This figure uses economics categorization - $<$ 12 years, 12 years/high school, some college, 4 years of college, 4+ years of college. I control for state fixed effects and state-specific time trends.

Source: National Vital Statistics System of the National Center for Health Statistics (NCHS).

Chapter 3

Inflation Expectations in the Wake of the War in Ukraine[1][2]

3.1 Introduction

Russia's invasion of Ukraine is making the post-COVID recovery of the global economy more challenging. From its outset, the war has affected energy prices and inflation rates, which had already started to increase in mid-2021 in the later days of the COVID pandemic. Additional supply chain disruptions, price increases of goods imported from Ukraine and Russia, and climbing energy prices make inflation a very volatile aspect of the post-COVID period. If individuals anticipate high inflation and act accordingly by adjusting their consumption and/or demanding wage increases, and if companies simultaneously adjust their prices in anticipation of growing costs, rising inflation expectations can drive up real inflation. Studying changes in individuals' expectations can be useful for central banks and policymakers deciding future actions related to anchoring inflation and global economic recovery in general.

We use the start of the war in Ukraine as a natural experiment to document the impact of a large geopolitical shock on inflation expectations of individuals in Germany. The microdata for our study come from the Bundesbank Online Panel - Households (BOP-HH)

[1] **Co-authored with Misina Cato (Goethe University Frankfurt, Deutsche Bundesbank) and Tobias Schmidt (Deutsche Bundesbank).**

[2] **Disclaimer: The views expressed in this paper are those of the authors and do not necessarily reflect the views of the Deutsche Bundesbank or the Eurosystem.**

- a monthly online survey that collects information on individuals' expectations regarding several economic indicators in Germany. The survey's field phase of February wave began on the 15th of February, several days before Russia invaded Ukraine, and continued until March 1st. The next wave of the survey included March 15th-29th. Because we have information on the exact date when respondents filled in the survey, we can determine causally and in real time how the onset of war in Ukraine affected individuals' short- and long-term inflation expectations. To determine causality, we perform an OLS regression with an indicator variable for the start of the war. The main identifying assumption in this setting is that the war was an unanticipated event that is exogenous to the time at which individuals chose to fill in the survey. Hence, there are no systematic differences in terms of individual characteristics between those respondents that completed the survey before and after the invasion of Ukraine. The results from this analysis demonstrate an immediate upwards shift in both short- and long-term inflation expectations. We find that short-term inflation expectations (for the following 12 months) increase by around 1 percentage point as an immediate response to the invasion. For longer horizons (5 and 10 years), the increase in inflation expectations is smaller - around 0.4 percentage points.

We find that the results for short-term inflation expectations are robust to various approaches addressing the issue of outliers, and to different econometric strategies to tackle unobserved individual heterogeneity. Our results on long-term inflation expectations are unaffected in approximate size and direction by our robustness checks, but we lose significance for some specifications due to smaller sample sizes. To rule out any concerns regarding comparability of the control and treatment groups, we report the difference in means between the groups, rely on a difference-in-differences approach, perform a placebo regression with data from one year earlier, and most importantly - due to the panel component - add individual-level fixed effects to control for further unobserved heterogeneity in a fixed-effects regression.

We demonstrate that major preceding events, i.e., US President Biden's announcement on the probability of a war in Ukraine and Russian President Putin's assertion that Donetsk and Luhansk are independent republics had no effect on inflation expectations. Hence, the war was indeed unexpected by individuals and was an important factor in their inflation expectations. To understand why individuals in Germany associate the start of the war in Ukraine with rising inflation, we look into two potential determinants discussed in the literature. First, one of the main implications of the war has been increasing energy prices. If individuals anticipated that the war would result fuel prices soaring further, they may have adjusted their expectations regarding inflation upwards (Istiak & Alam, 2019; Kilian & Zhou, 2022). Second, Binder (2020) and Kamdar (2019) find that households tend to associate bad economic outcomes with both high unemployment (or low economic growth) and with high inflation. Our analysis demonstrates that these two aspects can contribute at last partially to the rising inflation expectations that we observe in the wake of the Russian invasion of Ukraine.

This paper contributes to several strands of literature. First, our findings are related to the large literature studying the expectation formation of market participants (D'Acunto et al., 2023; Weber et al., 2021; Coibion et al., 2018; Coibion & Gorodnichenko, 2015).[3] We contribute to this literature by exploring a large exogenous shock to individuals' inflation expectations - the start of the war. Our findings suggest that the war influenced individuals' expectations. We document that individuals saw the start of the war in Ukraine as a large shock to energy prices, which they expected would increase even further going forward. Our finding is consistent with the insights from Verbrugge & Higgins (2015), who document that unusual changes in energy-prices influence movements in individuals' inflation expectations.

We also contribute to recent emerging literature on the economic implications of the war in Ukraine (Bachmann et al., 2022; Ferrara et al., 2022; Pestova et al., 2022; Berninger et al., 2022). The paper by Dräger et al. (2022a) is closely related to our study. The authors find that the war shifted experts' inflation expectations considerably, with the main channel also being fear of further energy price hikes, which is consistent with our findings regarding individuals' expectations. On the firm side, Seiler (2022) finds that the war increased agents' long-term inflation expectations. We contribute to these studies by providing evidence on the inflation expectations of individuals in Germany.

In terms of empirical methodology, our study is related to the literature on event studies and natural experiments, which rely on unanticipated shock episodes for causal identification (see also DiNardo (2010); Fuchs-Schündeln & Hassan (2016); Cantoni & Yuchtman (2021) for a general literature review). We use the invasion of Ukraine on the 24th of February 2022, a large geopolitical event, which was unexpected to individuals living in Germany, as a natural experiment. We argue that the outbreak of the war was a relatively unanticipated event that was not correlated with individuals' characteristics

[3] D'Acunto et al. (2023) classify the main determinants of individual expectations regarding inflation into four main categories: i) prices they observe in daily life ii) lifetime experiences, iii) cognition, iv) news and information.

and behaviour.

The rest of the paper is organised as follows. Section 2 describes the BOP-HH survey design and simple statistics on the data. In Section 3, we describe our identification strategy and report the main regression results. In Section 4, we examine the robustness of our main identifying assumption and examine whether the war was somehow anticipated by individuals. Section 5 describes the possible mechanisms of the rising inflation expectations, and Section 6 concludes.

3.2 Data and Event Description

The outbreak of the war in Ukraine in February 2022 took the world by surprise. Although many had previously discussed potential scenarios for such an event, few anticipated its occurrence, and certainly not the exact day of its onset. We use the timing as a natural experiment to identify whether this major unanticipated event played a decisive role in shaping individual sentiment.

3.2.1 The Survey and Timeline

To causally assess how Russia's invasion of Ukraine affected individuals' inflation expectations in Germany, we use microdata from the Bundesbank Online Panel - Households (BOP-HH). The BOP-HH is an online survey conducted on a monthly basis, which started collecting information on individuals' expectations regarding economic indicators in Germany prior to the onset of the COVID-19 pandemic. The survey includes individuals who are at least 16 years old and have used the internet at least once in the past months. It contains information on individuals' expectations regarding inflation, interest rates, and other macroeconomic variables, individuals' socio-demographic characteristics, and the time of the interview, amongst other things (Beckmann & Schmidt, 2020). For most of our analyses, we use the BOP-HH waves from February and March 2022. For our robustness checks, we make use of previous waves of the survey.

In this paper, we primarily concentrate on survey's questions about short- and long-term inflation expectations.[4] For short-term expectations, the BOP-HH includes a quantitative question about how individuals expect the inflation rate to perform over the upcoming 12 months. To identify their expectations about longer-term inflation, the

[4]See the exact wording of the question in Appendix C.

survey sample is randomly divided into two groups. Half of the sample is asked to state their point estimate of inflation over the next five years, and the other half states their expectations for ten years. The survey has had a rotating panel component since January 2021, and, in some waves, the latter questions on long-term inflation expectations are addressed only to "refreshers", i.e., respondents participating in the survey for the first time. Hence, we have a much smaller sample size for the longer-term expectations. We truncate the measures for expected inflation between the interval [-12%, 12%] both for long- and short-term expectations.[5]

Our sample includes data from around 7,000 respondents[6] surveyed in February and March 2022. The interviews were carried out from February 15th to March 1st, 2022 (the February wave), and from March 15th to March 29th, 2022 (the March wave). We use the information on the time individuals completed the survey to compare how short- and long-term inflation expectations differ between the group of individuals who responded to the questionnaire between the 15th and 23rd of February (control group) and those who responded after the invasion, in February and March 2022 (treatment group).

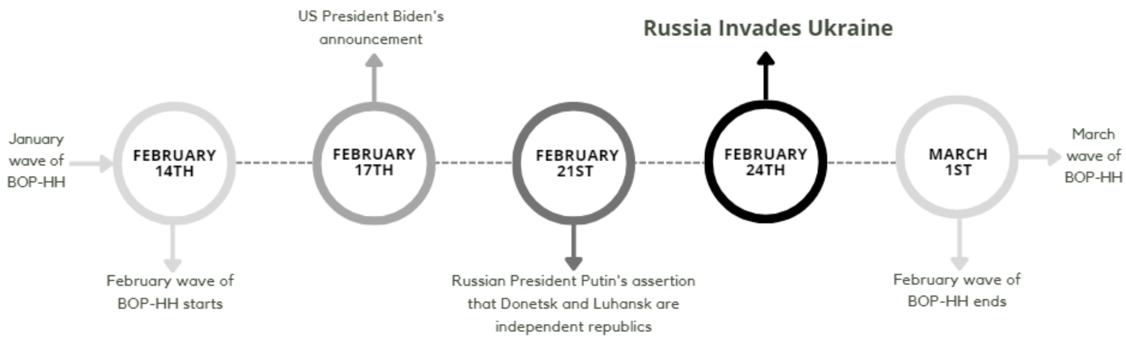
We reason that participants in the survey could not possibly have anticipated the invasion, or at least not the exact day it would begin. Nonetheless, there may have been some anticipation effects preceding the event, such as US President Biden's announcement of the possibility of a war in Ukraine and Russian President Putin's assertion that Donetsk and Luhansk are independent republics. Because our survey covers the time periods of both events preceding the invasion we can consider them in further analyses. In Figure 3.1, we display the timeline of these events and interview periods (waves) of the BOP-HH survey.

For the descriptive section and the OLS analysis, we do not use the panel respondents, meaning that in case February respondents are also surveyed in March, we do not use their March responses. This leaves us with 4,442 observations in the control group, and 2,558 in the treatment group for short-term inflation expectations. Because the question on long-term inflation expectations is only addressed to refreshers in some waves, the number of observations is comparably small. For the long-term, we have around 930

[5]We truncate the data between the interval [-12%, 12%], following the design of probabilistic questions used in the New York Fed Survey of Consumer Expectations. The outer bins of this question end at -12 and +12 (Van der Klaauw et al., 2008). In Section 3, we report and discuss the results for alternative trimming procedures.

[6]In some cases, the total number of observations changes depending on the regression specification and inclusion of additional controls.

Figure 3.1: Timeline of events



Note: The figure displays the timeline of our survey, the invasion of Ukraine and preceding events. For the main part of our analyses, the control group is defined as respondents who filled in the questionnaire from February 15th to 23rd. The treatment group comprises the respondents who filled in the questionnaire from February 24th to March 1st and from March 15th to 29th. In the figure, we also indicate other major events that preceded the beginning of Russia's invasion of Ukraine.

observations in the control and around 1,226 in the treatment groups for 5 and 10-year inflation expectations together.

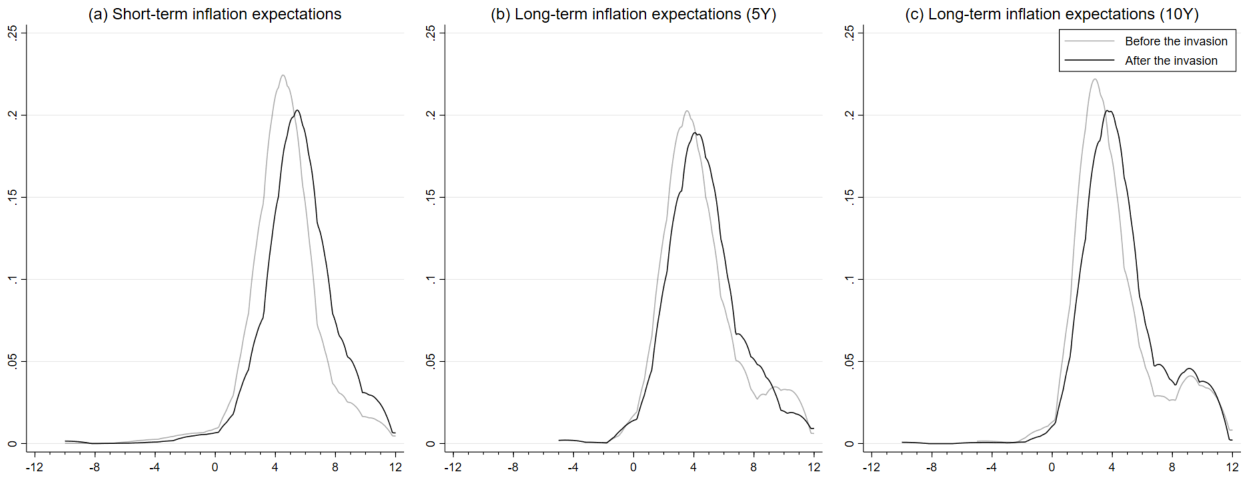
3.2.2 Descriptive Results

Figure 3.2 depicts the distribution of inflation expectations among individuals surveyed before and after the invasion of Ukraine.[7] In panel (a) we plot the distribution for their short-term inflation expectations (12 months ahead). The distribution for the individuals who completed the questionnaire on or after the 24th of February, the treatment group, is considerably further to the right than that of the control group. The average expectations regarding inflation for the next twelve months is 4.7% before the invasion. After the invasion, this average increases to 5.6% (see Appendix A, Table 3.7). This difference in means is about 0.9 percentage points. Aside from the increase in the mean, the median increased from 5% (control group) to 5.5% (treatment group).

Long-term inflation expectations, for the upcoming five and ten years on average, were also affected by the start of the war in Ukraine, but to a lesser extent than the short-term. Panels (b) and (c) show that the distribution of long-term expectations also shifted to the right and that there is more mass at higher inflation rates. When asked about their expectations for the next five years, individuals who had not yet experienced the start

[7]Analytical weights are used. The weights correct for the marginal distribution of age, gender, education, and region to be representative of the German (online) population aged 16 and older.

Figure 3.2: The distribution of individuals' inflation expectations



Note: The figure plots the distribution of inflation expectations for the next twelve months (panel (a)), five years (panel (b)) and ten years (panel (c)). The control group (before the invasion) is defined as respondents who filled in the questionnaire from February 15th to 23rd. The treatment group (after the invasion) are the respondents who filled in the questionnaire from February 24th to March 1st and from March 15th to 29th. Analytical weights are used. Inflation expectations are measured as a point prediction and truncated between [-12%, 12%].

of the war indicated an average (median) of 4.5% (4%). Those who responded after the war began, in contrast, reported an average (median) of 4.8% (5%). A similar structure is observed for the very long-term inflation expectations (panel (c)). Individuals in the control group had considerably lower expectations (mean: 4.2%) than the treatment group (mean: 4.6%). These differences between means are statistically significant at 1% (12 months and 5 years) and 5% (10 years) level (see Appendix A, Table 3.7).

3.3 Empirical Framework

The descriptive results show a large shift in the expectations regarding inflation of individuals living in Germany, with the effect being particularly strong for short-run expectations. In this section, we extend our analysis beyond descriptive results. To examine the size and significance of the effect of the war in Ukraine on inflation expectations, we estimate the following regression model:

$$E_{it}(\pi_{t+1,t+5,t+10}) = \alpha + \beta Treatment_t + \gamma X_{it} + \epsilon_{it}, \quad (3.1)$$

where $E_{it}(\pi_{t+1,t+5,t+10})$ is the inflation expectation of an individual i who responded to the survey in time period t - *before* or *after* the invasion. *Treatment* is a dummy variable, that is equal to one from February 24th onwards and zero before. X_{it} are individual level characteristics, including age, employment status, gender, education, income, region of residence, and household and city size. The regression results rely on the identifying assumptions that the war was unexpected by the agents, and that the group who responded before the invasion is similar to the group that responded after. In Section 4, we perform further analyses to test whether our identifying assumptions hold.

Table 3.1: Before and after regression results

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
	Panel A		
Before vs after invasion	1.055***	0.423***	0.393**
	(0.054)	(0.141)	(0.154)
Control mean	4.669	4.501	4.184
Individual and household level controls			
R-squared	0.054	0.008	0.006
Observations	7,000	1,112	1,044
	Panel B		
Before vs after invasion	1.074***	0.418***	0.389**
	(0.055)	(0.141)	(0.157)
Control mean	4.669	4.501	4.184
Individual and household level controls	✓	✓	✓
R-squared	0.066	0.062	0.057
Observations	6,696	1,059	1,002

Note: In panel A, we report the results from an OLS regression with a time dummy indicating the start of the war in Ukraine. We include only observations from February and March 2022. In panel B, we use the same specification, but add individual and household level controls. The treatment group (after the invasion) are respondents who had learned about the start of the war (from February 24th onwards). The control group (before the invasion) are respondents who received the questionnaire in February 2022, but before the beginning of the war. Regression results include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The findings from an OLS regression in Panel A of Table 3.1 confirm the descriptive results. Short-term inflation expectations are the most affected by the outbreak of the war. Long-term inflation expectations also increase, but the magnitude is considerably smaller, which is in line with Dräger et al. (2022a).[8]

Without controlling for individual and household-level characteristics, we find that, after Russia's invasion of Ukraine, inflation expectations over the upcoming 12 months increased by around 1.1 percentage points. The increase in the average expected inflation over the next 5 and 10 years is around 0.4 percentage points. All regression coefficients are statistically significant at the 1% or 5% levels. In panel B, we add controls including age, employment status, income, gender, education, region of residence of the respondent, and household and city size. The coefficient sizes remain roughly the same and stay highly statistically significant.

The size, direction and significance of the coefficients for short-term inflation expectations do not change when we vary the list of controls or when we use analytical weights (see Appendix A, Table 3.8). When we use the February wave only, the coefficient for short-term inflation expectations drops to 0.4-0.5 percentage points and remains statistically significant at the 1%-level, which indicates that, over time, the effect of the war on inflation expectations becomes stronger (see Appendix A, Table 3.9). The decrease in the size of the coefficient is expected, because, in this specification, the "after period" includes only responses within 5 days after the invasion day. To put these results into perspective, the change in the 12 months ahead inflation expectations from one month to the next was higher only in two months since the survey started in April 2020. The coefficients of long-term inflation expectations change and are non-significant in this setting (Appendix A, Table 3.9), potentially due to the small sample size.

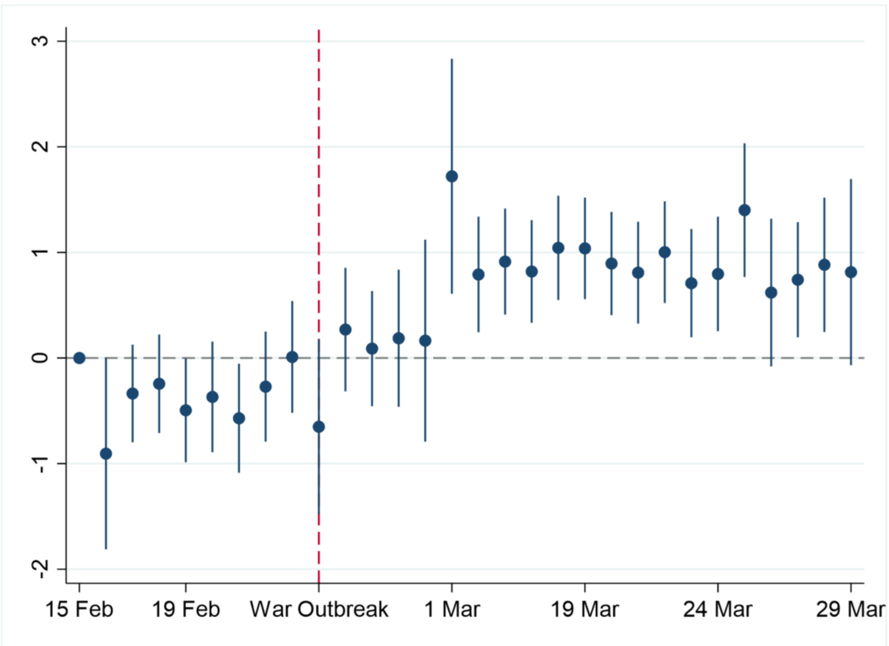
In the main specification, we addressed outliers by trimming the responses of individuals who reported inflation expectations of less than -12% and more than 12%. Unfortunately, there is no unified approach in the literature of expectation formation on how to address the problem of outliers and unreasonable answers. Therefore, we repeat the main analysis reported in Table 3.1 by choosing three alternative approaches. In Appendix A, Table 3.10, we report the results with the main dependent variable trimmed at the interval [-5, 30] following the Survey of Consumers from the University of Michigan; in Table 3.11 we report the results for the main dependent variable trimmed at [-5, 25] as in Dräger et al. (2022b); in Table 3.12 we trim inflation expectations at the interval [-2, 15] as in Candia et al. (2021). As reported previously, the results remain unaffected for the short-term measure of inflation expectations. In only a few cases, the coefficient for

[8]The authors find results similar to ours for short- and long-term inflation expectations from surveying economics professors in Germany.

the very long-term inflation expectations measure (10 years) is affected. As noted earlier, this might be due to the very small sample size we have for the longer term inflation expectation measures.

In addition to our main analysis, the daily dynamics for short-term inflation expectations[9] reported in Figure 3.3 reinforce our finding in Table 3.1 that the longer the war progresses, the more convinced individuals are that the war will result in higher inflation in the coming year. The figure also rules out the possibility that any other event that preceded the day of the invasion could have already elevated inflation expectations.[10]

Figure 3.3: The daily pattern of inflation expectations



Note: The figure shows the coefficients plot graph for individuals' inflation expectations. The x-axis shows the dates, where "War Outbreak" represents the date of the invasion of Ukraine, the 24th of February. The plot is based on the results of an event study regression of inflation expectations on daily dummies. The base or omitted category is the 15th of February.

To address any remaining concerns about unobserved heterogeneity, rule out that there are any pre-existing differences between the control and the treatment groups, and further strengthen our statement that it was mainly the outbreak of the war that elevated inflation expectations, we perform several complementary analyses in Appendix B. In

[9]We cannot show similar graphs for the average expected inflation within the next 5 and 10 years due to the small sample sizes.

[10]In addition to the major events we examine in Section 4, on 23 February 2022 the Federal Cabinet approved raising the minimum wage in Germany to 12 euros per hour from 1 October 2022.

Table 3.14 and Table 3.15, we show the results from a regression with individual fixed effects and a difference-in-difference regression. The results remain similar for the short - (12 months ahead) and long-term (5 years) inflation expectations. In some robustness checks the treatment coefficient is not statistically significant for the very long-term (10 years) inflation expectations, but the size of the coefficient stays the same in this case. Moreover, the results from a placebo regression, where we use data from the previous year (2021), suggest that the difference in inflation expectations is not driven by differences in characteristics between late and early respondents (Table 3.13), but by the major geopolitical event that happened on the 24th February (Table 3.16).

Overall, in this section we document that the onset of the war affected individuals' inflation expectations in Germany. We observe the strongest effect on short-term expectations. We can also confirm that individuals' long-term inflation expectations do not remain completely unaltered. However, it is important to emphasize that the effect on the long-term is not as large in magnitude as the estimated effect on the short-term expectations, and is not statistically significant in all cases.

3.4 Was the War an Unexpected Event?

Our main analysis relies on the argument that the invasion of Ukraine came as a rather unexpected event that most people believed would not actually happen.[11] Nonetheless, two significant instances preceded the invasion that could have led to some anticipation effects. First, we consider an announcement made by US President Biden regarding the possibility of Russia attacking Ukraine. Second, we analyse a crucial signal that occurred before the invasion, the moment Russian President Putin signed a decree to recognize the Donetsk and Luhansk regions of Ukraine as independent republics.

The President of the United States announced on the 17th of February 2022 "...we have reason to believe the Russian forces are planning to and intend to attack Ukraine in the coming days." (Biden, 2022). This was the first announcement made by a government official of a western country on the elevated threat of an invasion by Russia. Therefore, to examine whether individuals used this information to update their inflation expectations before the actual invasion occurred, we divide the sample into more than two periods:

[11] In the news, there was also a considerable amount of coverage of the events preceding the invasion and there were many articles pointing to the direction of a full scale invasion not happening (e.g., BBC article titled "Ukraine crisis: Five reasons why Putin might not invade").

1) pre-announcement, 2) announcement period, but before invasion, and 3) the invasion period. The "pre-announcement" period includes February 15th and 16th. The "announcement period" spans from the day that President Biden made the announcement until February 23rd. The "invasion" period is defined as the period after the invasion of Ukraine, i.e., from February 24th to the end of March.

Table 3.2: Summary statistics: President Biden's announcement

	Before		Announcement		Invasion	
	(1) Mean	(2) N	(3) Mean	(4) N	(5) Mean	(6) N
Expected inflation, point prediction	4.60	111	4.67	4,331	5.60	2,558
Expected inflation (5 years)	.	.	4.48	464	4.77	638
Expected inflation (10 years)	.	.	4.12	444	4.64	588

Note: This table summarizes the average inflation expectations during three periods. In columns (1) and (2), we include observations from respondents who were interviewed during the days before President Biden announced that there was a high risk of a Russian invasion of Ukraine (i.e., February 17th). The announcement period (columns 3 and 4) spans from the day President Biden made the announcement, i.e., February 17th until the day before the invasion on February 24th. The invasion period (columns 5 and 6) includes observations from February 24th to the end of March. The columns with odd numbers contain the average expected inflation of individuals. The results are weighted.

In Table 3.2, we show summary statistics for each of the above-mentioned periods. There is no evidence for a significant effect of the announcement on the average inflation expectations of individuals for the next twelve months. The post-announcement pre-war expectations, at 4.7%, are only marginally higher than the pre-announcement expectations or expectations on the day of the announcement (4.6%). However, short-term inflation expectations rose to 5.6% between February 24th and March 29th.

For long-term inflation expectations, we do not have enough observations to report reliable statistics for the period prior to President Biden's announcement. However, we still see a difference between average long-term inflation expectations of individuals during the announcement and the invasion periods. Expected inflation over the next five years increased from 4.5% to 4.8% on average between the announcement and the invasion periods. The inflation expectations for the next ten years increased from 4.1% to 4.6% on average.

The second event we look into is the day President Putin approved a decree to recognise the Donetsk and Luhansk regions in Ukraine as independent republics (February

21st). This development escalated tensions further and increased the chance that an invasion would actually take place. Therefore, we group the observations according to the date individuals filled in the questionnaire. We define three comparison groups: 1) before the recognition of the Donetsk and Luhansk regions as independent republics (February 15th-20th), 2) after the Russian decree, but before the invasion (February 21st-23rd) and 3) the invasion period (February 24th - March 29th).

Table 3.3: Summary statistics: Donetsk and Luhansk declaration

	Before		Donetsk and Luhansk		Invasion	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	N	Mean	N	Mean	N
Expected inflation, point prediction	4.68	3,600	4.64	842	5.60	2,558
Expected inflation (5 years)	4.64	282	4.32	192	4.77	638
Expected inflation (10 years)	4.37	270	3.87	186	4.64	588

Note: This table summarizes the average inflation expectations during three periods. In columns (1) and (2), we include observations from respondents who were interviewed February 15th -20th. In columns (3) and (4) we report the results for individuals who were interviewed during the period February 21st -23rd. In columns (5) and (6) we report the results for individuals interviewed during the invasion (February 24th - March 29th).

In Table 3.3, we can confirm that there was no anticipation effect on inflation expectations, even when Russia declared that the Donetsk and Luhansk regions in Ukraine were independent republics. Short-run inflation expectations of individuals were very similar before and after the declaration, at 4.7% and 4.6%, respectively. For long-term inflation expectations within the next five and ten years, we can confirm what we previously documented in Table 3.1. The effect is much weaker, but again we can exclude any anticipation during the period when the Donetsk and Luhansk regions were declared independent.

Regression results for short-term inflation expectations in Table 3.4 that include the preceding events are in line with our previous findings. The coefficients for both Biden's announcement and the declaration about Donetsk and Luhansk are small and non-significant, while the coefficient for the after invasion period maintains both magnitude and statistical significance, as previously reported.

Table 3.4: Regression results for expected inflation in the next 12 months

	(1)	(2)
	Panel A: Biden Announcement	
Announcement	−0.087	−0.069
	(0.206)	(0.209)
Invasion	0.969***	1.000***
	(0.208)	(0.211)
Observations	7,000	6,710
R-squared	0.054	0.063
Individual and household level controls		✓
	Panel B: Declaration of the Donetsk and Luhansk as independent republics	
Donetsk	−0.022	−0.017
	(0.087)	(0.088)
Invasion	1.051***	1.064***
	(0.055)	(0.056)
Observations	7,000	6,710
R-squared	0.054	0.063
Individual and household level controls		✓

Note: The main dependent variable in the regression is the short-run inflation expectations of individuals in the next 12 months. The comparison (excluded) group is the period before the event happened. In column 2, we include the following controls: age, income, gender, education, employment status, region of residence of the respondent, and household size and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.5 Why Do Individuals Associate the War with Higher Inflation?

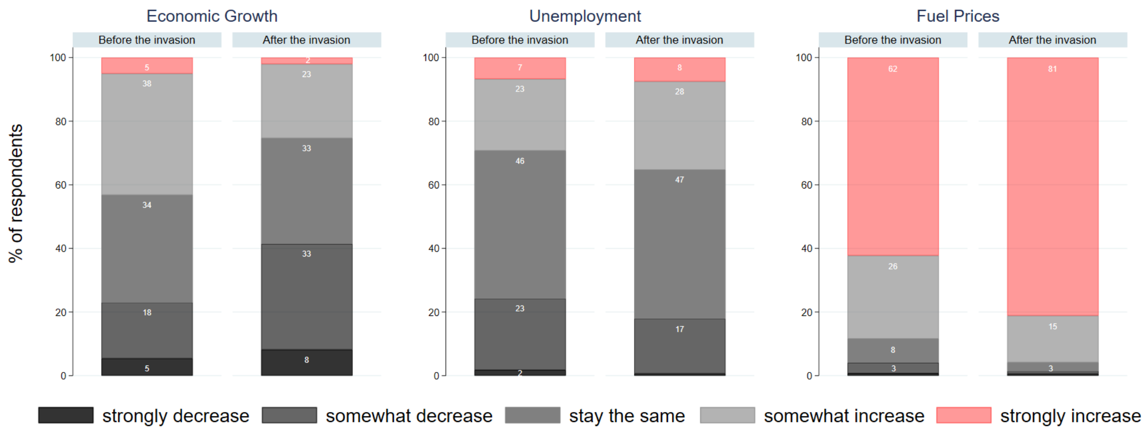
We documented in the previous sections that the war caused individuals to raise their expectations of upcoming inflation. It is important to understand why individuals reacted so strongly to this event in terms of their inflation predictions. We explore two channels that are widely discussed in the expectations formation literature.

The first aspect has to do with individuals' general sentiments about the future economic outlook. Several studies have shown that individuals tend to associate high inflation with bad economic outcomes, so they perceive a positive co-movement between inflation and unemployment rates (Kamdar, 2019; Binder, 2020). In the current context, the war created further global supply chain distortions that aggravate the effect inherited from the COVID-19 pandemic. Hence, it is possible that the war made individuals more pessimistic about the future economic outlook and unemployment, leading to higher inflation expectations as well.

Another aspect that could elevate inflation expectations is anticipation of further energy price hikes. D'Acunto et al. (2021) show that individuals' inflation expectations are strongly influenced by the price signals they most often observe, and fuel prices were very salient when the invasion started. Additionally, Verbrugge & Higgins (2015) document that unusual changes in energy-prices influence movements in individuals' inflation expectations.

To explore these two channels we use several questions from the survey that ask individuals about their expectations on the development of the three aspects: 1) economic growth, 2) unemployment rates and 3) fuel prices.

Figure 3.4: Macro expectations of individuals, qualitative



Note: The figure depicts the results from the following question in the February 2022 BOP-HH wave: "What developments do you expect with regard to economic growth/unemployment/fuel prices over the next twelve months?" Each split in the bars represents the share of respondents choosing a specific category from: 1 decrease significantly, 2 decrease slightly, 3 stay roughly the same, 4 increase slightly, 5 increase significantly. In each of the three panels, we split the sample between individuals who answered before the invasion (February 15th - 23rd) and those who answered immediately after the invasion (February 24th - March 1st).

The results from Figure 3.4 indicate that individuals became more pessimistic in terms of economic growth and the unemployment rate for the coming 12 months. For example, before the war started only 18% of respondents believed that economic growth in Germany would decline. After the war started, this share increased to 33%. Respondents also report significant concerns about soaring energy prices, which were already a matter of worry even before the war began. The share of respondents expecting a "significant increase" in fuel prices over the next twelve months is 81% immediately after the invasion began, about 20 percentage points higher than the share in the pre-invasion period.

We attempt to quantify the possible shift in expectations of economic outcomes or fuel prices by using these indicators as a dependent variable in the following regression model:

$$E_{it}(Y_{t+1}) = \alpha + \beta Treatment_t + \gamma X_{it} + \epsilon_{it}, \tag{3.2}$$

where $E_{it}(Y_{t+1})$ is a matrix of binary variables indicating the expectation of an individual i about unemployment, economic growth, or fuel prices for the period $t+1$. In other words, we use the equation 3.2 with 3 different dependent variables, which are equal to one if an individual i expects 1) an increase in the unemployment rate (slight or significant), 2) a decrease in economic growth (slight or significant), and/or 3) an increase in fuel prices (slight or significant) over the next 12 months - $t+1$. *Treatment* is a dummy variable, that is equal to one from February 24th onwards and zero before. X_{it} are individual level characteristics, including age, employment status, gender, education, income, region of residence, and household and city size. We use both a linear probability model (LPM- Table 3.5, Panel A) and a logit model (Table 3.5, Panel B).

Table 3.5: Mechanisms - before and after regression results

	Increase in Unemployment	Decrease in Economic Growth	Increase in Fuel Prices
	(1)	(2)	(3)
		Panel A: LPM	
Before vs after invasion	0.047^{**}	0.165^{***}	0.084^{***}
	(0.023)	(0.024)	(0.011)
Individual and household level controls	✓	✓	✓
R-squared	0.028	0.037	0.018
Observations	4,821	4,821	4,821
		Panel B: Logit	
Before vs after invasion	1.263^{**}	2.270^{***}	3.488^{***}
	(0.140)	(0.246)	(0.880)
Individual and household level controls	✓	✓	✓
Pseudo R-squared	0.024	0.034	0.028
Observations	4,821	4,821	4,821

Note: In panel A, we report the results of a LPM regression with a time dummy indicating the beginning of the war. In panel B, we use the logit specification and report the computed odds ratios. In both specifications we have individual and household level controls. The treatment group (after the invasion) comprises respondents who heard about the start of the war (from February 24th to March 1st). The control group (before the invasion) comprises respondents who received the questionnaire in February 2022, but before the war began. Regression results include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results confirm that the probability and odds of expecting an increase in the unemployment rate (column 1), a decrease in economic growth (column 2) and/or an increase in fuel prices (column 3) are significantly higher after the invasion. The computed odds ratios reported in Panel B show that the odds that a respondent will expect an increase in fuel prices are 3.5 higher after the invasion. For an expected decrease in economic growth and increase in the unemployment rate, the odds are also higher in the period post-invasion (2.3 and 1.3, respectively). The results with LPM are both highly significant and in line with the results of the logit model.

Determining precisely which of the channels matters most is challenging, because individual respondents can simultaneously associate the war with multiple aspects that have direct implications for the economy and inflation. We attempt to quantify the effect arising from general pessimism about the economic outlook and the effect originating from interpreting the war as a signal related to energy prices by including measures for each in a regression specification similar to equation 3.1. We include three indicator variables that equal one if individuals expect: 1) an increase in the unemployment rate, 2) a decrease in economic growth, and/or 3) an increase in fuel prices. The results shown in Table 3.6 cannot be interpreted causally, because of the endogeneity arising from the simultaneity between the outcome and control variables related to economic developments.

Table 3.6: Before and after regression results - channels

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
	(1)	(2)	(3)
Before vs after invasion	0.753***	0.095	0.208
	(0.059)	(0.150)	(0.165)
Exp. Incr. Unemp	0.530***	0.440***	0.521***
	(0.060)	(0.159)	(0.181)
Exp. Decr. Growth	0.758***	0.844***	0.205
	(0.065)	(0.160)	(0.183)
Exp. Incr. Fuel Prices	0.684***	0.290	0.145
	(0.072)	(0.211)	(0.178)
Individual and household level controls	✓	✓	✓
R-squared	0.129	0.109	0.071
Observations	6,687	1,058	1,000

Note: The treatment group (after the invasion) comprises respondents who heard about the start of the war (from February 24th onwards). The control group (before the invasion) comprises respondents who received the questionnaire in February, but before the war began. Regression results include the following controls: age, income, gender, education, employment status, region of residence of the respondent, and household size and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Nevertheless, we can confirm a strong positive (negative) correlation between expected unemployment (economic growth) and the expected inflation rate, which indicates that the first channel matters in individuals' expectation formation. Furthermore, Table 3.6 confirms that individuals associate the war in Ukraine with a negative shock to the economy in Germany, which they anticipate could result in lower economic growth, higher unemployment rates, and/or soaring inflation. In addition, Table 3.6 confirms a positive correlation between expected fuel prices and inflation expectations for the upcoming twelve months.

Overall, we can conclude from this section that the fear of greater supply side shocks and increases in energy prices play an important role in elevating individuals' inflation expectations.

3.6 Conclusion

It is well established in the literature that individuals' inflation expectations can be an important influence on the real inflation rate. Inflation expectations can influence individuals' consumption and saving behaviour, affecting the current level of inflation and making it more difficult for central banks to achieve their price stability goals. Therefore, understanding how (large) shocks such as Russia's 2022 invasion of Ukraine influence individuals' expectations is crucially important.

In this study, to assess how the invasion affected individuals' inflation expectations, we treat its timing as an unanticipated event. We find that both short- and long-term inflation expectations increase with the invasion. The increase of short-term inflation expectations is around 1 percentage point, and survives all the robustness checks. When we widen the window of expectations to the upcoming 5 and 10 years, the increase in inflation expectations is only around 0.4 percentage points. Using the panel component and fixed-effects model instead of OLS does not affect the significance of our results on short- (12 months) and long-term (5 years) inflation expectations.

Possible mechanisms of these shifts in inflation expectations are individuals' fear of increasing fuel prices, higher unemployment, and lower economic growth. Our results are in line with the existing literature and with concerns that, in the current economic setting, large-scale political shocks can contribute to de-anchoring tendencies of inflation expectations. This study and further research on how persistent the increase in inflation expectations is could be useful for policymakers deciding on future action plans and policies for minimizing inflation and global economic stability in general, in the face of ongoing shocks to the system.

3.A Appendix: Tables

Table 3.7: Summary statistics - before and after the invasion

	Before the Invasion			After the Invasion			Difference in means (t-test)
	Mean	Median	N	Mean	Median	N	
Expected inflation (12 months)	4.67	5.00	4,442	5.60	5.50	2,558	1.05***
Expected inflation (5 years)	4.50	4.00	474	4.77	5.00	638	0.42***
Expected inflation (10 years)	4.18	3.00	456	4.64	4.00	588	0.39**

Note: The treatment group (after the invasion) comprises respondents who learned of the start of the war (from February 24th onwards). The control group (before the invasion) comprises respondents who received the questionnaire in February 2022, but before the war began. We use analytical weights. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8: Before and after results with analytical weights

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
	Panel A		
Before vs after invasion	0.926***	0.267	0.457*
	(0.095)	(0.225)	(0.263)
Control mean	4.669	4.501	4.184
Individual and household level controls			
R-squared	0.034	0.003	0.007
Observations	7,000	1,112	1,044
	Panel B		
Before vs after invasion	0.996***	0.174	0.350
	(0.090)	(0.211)	(0.231)
Control mean	4.669	4.501	4.184
Individual and household level controls	✓	✓	✓
R-squared	0.051	0.059	0.109
Observations	6,696	1,059	1,002

Note: In panel A, we report the results from an OLS regression with a time dummy indicating the beginning of the war. We include only observations for February and March 2022. In panel B, we use the same specification, but add individual and household level controls. The treatment group (after the invasion) comprises respondents who heard about the start of the war (from February 24th onwards). The control group (before the invasion) comprises respondents who received the questionnaire in February 2022, but before the war began. Regression results include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Analytical weights are used. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.9: Before and after results for February 2022 only

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
	Panel A		
Before vs after invasion	0.478***	−0.220	0.078
	(0.107)	(0.230)	(0.261)
Control mean	4.669	4.501	4.184
Individual and household level controls			
R-squared	0.004	0.001	0.000
Observations	4,887	575	554
	Panel B		
Before vs after invasion	0.422***	−0.331	−0.077
	(0.110)	(0.233)	(0.272)
Control mean	4.669	4.501	4.184
Individual and household level controls	✓	✓	✓
R-squared	0.018	0.099	0.060
Observations	4,657	544	525

Note: In panel A, we report the results from an OLS regression with a time dummy indicating the beginning of the war. We include only observations for the February wave. In panel B, we use the same specification, but add individual and household level controls. The treatment group comprises respondents who heard about the start of the war (from February 24th onwards). The control group comprises respondents who received the questionnaire in February 2022, but before the beginning of war. Regression results include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.10: Before and after results [-5,30]

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
	Panel A		
Before vs after invasion	1.220***	0.502**	0.017
	(0.079)	(0.228)	(0.288)
Control mean	5.159	5.326	5.717
Individual and household level controls			
R-squared	0.034	0.004	0.000
Observations	7,146	1,155	1,104
	Panel B		
Before vs after invasion	1.240***	0.508**	0.147
	(0.080)	(0.224)	(0.272)
Control mean	5.159	5.326	5.717
Individual and household level controls	✓	✓	✓
R-squared	0.061	0.069	0.081
Observations	6,835	1,098	1,058

Note: In panel A, we report the results from an OLS regression with a time dummy indicating the beginning of the war. We include only observations for February and March 2022. In panel B, we use the same specification, but add individual and household-level controls. The treatment group (after the invasion) comprises respondents who heard about the start of the war (from February 24th onwards). The control group (before the invasion) comprises respondents who received the questionnaire in February 2022, but before the war began. The main dependent variable is restricted to the interval between -5 and 30. Regression results include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.11: Before and after results [-5,25]

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
	Panel A		
Before vs after invasion	1.196***	0.411**	0.216
	(0.069)	(0.202)	(0.243)
Control mean	5.042	5.283	5.367
Individual and household level controls			
R-squared	0.043	0.004	0.001
Observations	7,116	1,148	1,093
	Panel B		
Before vs after invasion	1.213***	0.439**	0.257
	(0.070)	(0.200)	(0.240)
Control mean	5.042	5.283	5.367
Individual and household level controls	✓	✓	✓
R-squared	0.069	0.073	0.073
Observations	6,808	1,092	1,049

Note: In panel A, we report the results from an OLS regression with a time dummy indicating the beginning of the war. We include only observations for February and March 2022. In panel B, we use the same specification, but add individual and household level controls. The treatment group (after the invasion) comprises respondents who heard about the start of the war (from February 24th onwards). The control group (before the invasion) comprises respondents who received the questionnaire in February 2022, but before the war began. The main dependent variable is restricted to the interval between -5 and 25. Regression results include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.12: Before and after results [-2,15]

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
	Panel A		
Before vs after invasion	1.067***	0.285*	0.344**
	(0.054)	(0.159)	(0.169)
Control mean	4.872	4.959	4.547
Individual and household level controls			
R-squared	0.055	0.003	0.004
Observations	7,005	1,126	1,056
	Panel B		
Before vs after invasion	1.089***	0.298*	0.331*
	(0.054)	(0.156)	(0.171)
Control mean	4.872	4.959	4.547
Individual and household level controls	✓	✓	✓
R-squared	0.076	0.077	0.067
Observations	6,699	1,070	1,014

Note: In panel A, we report the results from an OLS regression with a time dummy indicating t the beginning of the war. We include only observations for February and March 2022. In panel B, we use the same specification, but add individual and household level controls. The treatment group (after the invasion) comprises respondents who heard about the start of the war (from February 24th onwards). The control group (before the invasion) comprises respondents who received the questionnaire in February 2022, but before the war began. The main dependent variable is restricted to the interval between -2 and 15. Regression results include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.13: Difference in means between treatment and control

	Before the war	After the war	Difference	P-values
Age	58.02	56.75	1.27 * **	(0.00)
Employment				
Employed	0.53	0.57	−0.03 * *	(0.01)
Not employed	0.03	0.03	0.00	(0.36)
In training	0.01	0.02	−0.00	(0.28)
Retired	0.42	0.39	0.03 * *	(0.01)
Income				
Less than 2,500	0.27	0.25	0.02	(0.07)
2,500-4,000	0.35	0.35	0.00	(0.98)
more than 4,000	0.37	0.39	−0.02	(0.10)
Gender	0.42	0.41	0.00	(0.92)
Education				
High-school or less	0.56	0.57	−0.01	(0.39)
Bachelor or equivalent	0.17	0.17	0.01	(0.57)
Higher than bachelor	0.26	0.26	0.01	(0.63)
Region				
East	0.18	0.17	0.00	(0.99)
North-West	0.17	0.17	−0.00	(0.89)
South-West	0.39	0.39	0.01	(0.65)
West-West	0.26	0.27	−0.00	(0.69)
HH size	2.12	2.21	−0.09 * **	(0.00)
Region size (inhabitants)				
< 5,000	0.14	0.12	0.01	(0.12)
5,000 - 20,000	0.26	0.25	0.01	(0.36)
20,000 - 100,000	0.27	0.30	−0.02*	(0.03)
100,000 - 500,000	0.15	0.16	−0.00	(0.92)
500.000	0.17	0.17	0.00	(0.82)
Observations	4,388	2,584	6,972	

Note: The table shows the difference in means between the average characteristics of households before and after the start of the war in Ukraine. Columns (1) and (2) report the average age; share of respondents employed, not employed, in training or retired; share of individuals who have a net household income of less than 2,500 EUR per month, between 2,500 EUR - 4,000 EUR or more than 4,000 EUR; share of respondents who are women; share of respondents with less than a high-school degree, bachelor or equivalent, higher than bachelor degree; share of individuals who live in the East, North-West, South-West or West-West Germany; average household size; and the share of individuals living in a region with fewer than 5,000 inhabitants, between 5,000 and 20,000, between 100,000 and 500,000, or with more than 500,000 inhabitants. Column (3) reports the difference in the average or share between the treatment and control groups. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.B Appendix: Additional Analyses

3.B.1 Fixed-Effects Analysis

To address any remaining concern about unobserved heterogeneity between the treatment and control groups, we use the panel component of the survey in an individual fixed-effects specification. To be able to add individual-level fixed effects, of course we use the panel respondents, unlike in the OLS regressions reported in the main part of the paper. The results in Table 3.14 indicate that the impact of the war on both short - (12 months) and long-term (5 years) inflation expectations remains highly significant, and is even larger than with a simple OLS specification. This rules out the concern that our OLS results are biased upwards as a result of the unobserved systematic differences across individual respondents in the treatment and control groups. We lose significance for the main coefficient of interest only for the very long-term (10 years) inflation expectations, but the size of the coefficient stays the same.

Table 3.14: Before and after results with individual fixed effects

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
Before vs after invasion	1.310***	0.874***	0.392
	(0.042)	(0.272)	(0.278)
Control mean	4.669	4.501	4.184
Individual Fixed Effects	✓	✓	✓
R-squared	0.248	0.143	0.021
Observations	9,988	1,825	1,800

Note: We report the results from an FE regression using the panel component and implementing individual-level FEs, with a time dummy indicating the start of the war. We include only observations for February and March 2022. The treatment group (after the invasion) comprises respondents who heard about the start of the war (from February 24th onwards). The control group (before the invasion) comprises respondents who received the questionnaire in February 2022, but before the war began. Standard errors are clustered at individual level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.B.2 Difference-in-Differences Analysis

In Section 3, we documented that the unexpected outbreak of war in Ukraine negatively affected individuals' inflation expectations. Although we argued that the day of the invasion was unexpected and not correlated to respondents' characteristics, the concern remains that the sample that responded later during the survey is inherently different from the sample that responded early on. We address this concern by drawing on the panel-observations of the survey participants who were interviewed in January 2022.[12] Using the longitudinal component of the data in conjunction with the day of the invasion as a natural experiment, we implement a difference-in-differences analysis with two groups and two periods.

We estimate the following specification:

$$E_{it}(\pi_{t+1,t+5,t+10}) = \beta_0 + \beta_1 Treatment_{war} + \beta_2 POST + \beta_3 Treatment_{war} XPOST + \gamma X_{it} + \epsilon_{it}, \tag{3.3}$$

Where $E_{it}(\pi_{t+1,t+5,t+10})$ is the reported inflation rate for the 12 months, 5 years and 10 years horizon, respectively, for individual i at time t. $Treatment_{war}$ is a dummy variable that takes the value of one if the respondent filled in the questionnaire on or after February 24th (treatment group). It takes a value of zero if they responded before the war started in February (control group). $POST$ indicates the survey periods, and is equal to zero if the survey was conducted in January 2022 and one if it took place in February or March 2022, which is the period of the invasion of Ukraine in our study. The coefficient of interest is β_3 which captures the causal effect of the war on inflation expectations of respondents.

We report the results from specification 3.3 in Table 3.15. The results on the main coefficient of interest, β_3 of the interaction term $Treatment_{war}XPOST$ corroborate the results from the OLS regression (Table 3.1). The reaction of respondents is particularly strong for short-term inflation. The results show that average expected short-term inflation increased by approximately 0.9 percentage points for the treatment group as compared to the control group (column 1). The results remain the same if we control for age, income, gender, education, employment status, region of residence, and household and city size (column 2). The magnitude of this impact decreases as the prediction

[12] The survey has a rotating panel design. Therefore, we cannot track the full sample of respondents in January 2022, but only a sub-sample.

horizon lengthens. The start of the war in Ukraine led to an increase in inflation expectations over the next 5 and 10 years, by respectively around 0.4 and 0.6 percentage points (columns 3 to 6). However, the estimated coefficients are not statistically significant. This can be partially attributed to the small sample size of respondents who reported their long-term inflation expectations.[13]

Table 3.15: Difference-in-differences results

	Exp. Infl. 12M		Exp. Infl. 5Y		Exp. Infl. 10Y	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (War) X Post	0.944^{***}	0.982^{***}	0.411	0.285	0.616	0.600
	(0.165)	(0.166)	(0.474)	(0.471)	(0.392)	(0.405)
Post	0.191^{***}	0.176^{***}	0.382^{**}	0.371^{**}	0.095	0.047
	(0.056)	(0.057)	(0.179)	(0.175)	(0.197)	(0.198)
Treatment (War)	0.111	0.092	0.012	0.116	−0.223	−0.211
	(0.156)	(0.157)	(0.452)	(0.449)	(0.361)	(0.371)
Individual and household level controls		✓		✓		✓
R-squared	0.053	0.066	0.018	0.065	0.008	0.057
Observations	9,249	8,864	1,414	1,351	1,343	1,292

Note: In columns (1), (3) and (5), we report the results from the difference-in-differences regression without controls. In the other columns, we use the same specification, but add individual and household level controls. Treatment (War) is a dummy variable that takes the value of one if the respondents heard about the start of the war (from February 24th onwards). It takes a value of zero if s/he responded before the war started in February (control group). POST indicates the survey periods and it is equal to zero if the survey was conducted in January 2022 and one if it took place in February or March. Regressions in (2), (4) and (6) include the following controls: age, employment status, gender, education, income, region of residence, and household and city size. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[13]In BOP-HH the question on short-term inflation expectations is asked every month to the full sample of respondents. For long-term inflation expectations the sample is split in two groups in every wave where one group is asked about their point prediction in five years and the other for the prediction in ten years. Furthermore, in some waves this question is asked only of new survey participants.

3.B.3 Placebo Regression

Finally, to ensure that the samples from before and after the start of the war are not systematically different in other waves, for example, showing differences along unobserved heterogeneity between early and late respondents to the survey, we rely on a placebo regression from the previous waves of BOP-HH (February and March 2021). In this setting, we repeat the main regression analysis, with the placebo event date being the 24th of February, but for the year 2021. When we repeat the regression specification 3.1, but with the data from 2021, we find no significant effect on inflation expectations for each of the three prediction horizons (Table 3.16). This finding reinforces our main result: it was the start of the war that caused the divergence of inflation expectations between the control and the treatment groups, and it is not driven by unobserved differences between individuals asked before or after the 24th of February, i.e., early versus late respondent characteristics.

Table 3.16: Placebo regression for February and March 2021

	Exp. Infl. 12M	Exp. Infl. 5Y	Exp. Infl. 10Y
Placebo	0.046	0.034	−0.019
	(0.070)	(0.102)	(0.119)
Control mean	2.805	3.371	3.745
Individual and household level controls			
R-squared	0.000	0.000	0.000
Observations	5,908	2,367	2,292

Note: The table reports the results for a placebo regression similar to the one reported for specification (1). We report the results from an OLS regression with a placebo time for the 24th of February 2021, one year before the start of the war in Ukraine. We include observations for February and March 2021. The placebo treatment group comprises the respondents who filled in the questionnaire on or after the 24th of February 2021. The placebo control group comprises respondents who received the questionnaire in February 2021, but before the 24th of February. The main dependent variable is restricted at the interval between -12 and 12. Robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.C Appendix: Survey Questions

Short-term inflation expectations qualitative

Respondent group: all

Question: Do you think inflation or deflation is more likely over the next twelve months?
Note: Inflation is the percentage increase in the general price level. It is mostly measured using the consumer price index. A decrease in the price level is generally described as "deflation".

Please select one answer.

1. Inflation more likely
2. Deflation more likely

Short-term inflation expectations quantitative

Respondent group: all

If inflation:
Question: What do you think the rate of inflation in Germany will roughly be over the next twelve months?
If deflation:
Question: What do you think the rate of deflation in Germany will roughly be over the next twelve months?
Note: Inflation is the percentage increase in the general price level. It is mostly measured using the consumer price index. A decrease in the price level is generally described as "deflation".

Please enter a value in the input field (values may have one decimal place).
Input field percent

Long-term inflation expectations quantitative - 5 years on average

---

Respondent group: refresher only

We would now like to ask you to consider what developments you expect in the long term.

Question: What value do you think the rate of inflation or deflation will take on average over the next five years?

Note: Please enter a value in the input field (values may have one decimal place). If you assume that prices will fall (deflation), please enter a negative value.

Input field percent

Long-term inflation expectations quantitative - 10 years on average

---

Respondent group: refresher only

We would now like to ask you to consider what developments you expect in the long term.

Question: What value do you think the rate of inflation or deflation will take on average over the next ten years?

Note: Please enter a value in the input field (values may have one decimal place). If you assume that prices will fall (deflation), please enter a negative value.

Input field percent

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