Who Reacts to Less Restrictive Divorce Laws?

Objective: To study how divorce behavior in Denmark changed following a July 2013 reform that removed previous restriction on immediate divorce by repealing mandatory 6-month separation periods for uncontested divorces, instead allowing for immediate administrative divorce.

Background: Most countries have mandatory separation periods that couples undergo before they can divorce. Separation allows couples a grace period, during which they may reconcile and stay together. Yet, the impact of separation periods on divorce risk remains understudied.

Methods: Using monthly time series data on divorce rates from 2007 to 2018 ($T = 144$), the research brief estimates the size and shape of the policy impact of the July 2013 reform. Using monthly administrative population data on all ever-married couples ($N*T = 11,304,566$), the study further calculates the average characteristics of married couples in Denmark who would have remained together absent the reform.

Results: After an initial spike in the divorce rate driven by couples divorcing earlier, the divorce rate settled at a 10% higher level compared to pre-reform. Couples who divorced because of the reform had been married for fewer years compared to other divorced couples, were ethnic Danish, and had high school degree as the highest educational level.

Conclusion: Mandatory separation periods have a dampening effect on divorce rates.

**INTRODUCTION**

Divorce is life-altering and causes individuals to face new economic, social, and emotional challenges (Amato, 2000; Leopold & Kalmijn, 2016; McManus & DiPrete, 2006). Because of the radical way divorce alters the life course, most Western countries have mandatory periods of legal separation that couples undergo before they can finalize their decision (see Moore, 2016; Smith, 2009 for overviews). The periods serve two purposes. First, they allow couples time to sort out the termination of their joint life. Second, in the case of rushed decisions, mandatory separation periods may allow for cooling of tempers and settling of emotions, so couples may resolve the conflicts that created the immediate want for a divorce (Binstock & Thornton, 2003; Plauche et al., 2016). If a mandatory time-out allows couples to resolve differences that seemed irresolvable while tempers were running hot, mandatory separation may cause couples who otherwise would have divorced to reconsider the decision.

In this study, we examine what characterizes couples who increase divorce risk following repeal of mandatory separation. We study Denmark, a vanguard country, in terms of access to divorce (Hussain & Kangas, 2009; Rosenbeck, 2017; Sandström & Garðarsdóttir, 2018;
Smith, 2009). Halfway through 2013, Denmark made voluntary a former mandatory 6-month separation period for uncontested divorces, allowing couples to instead divorce immediately (Rosenbeck, 2017). We use an interrupted time series design to estimate the impact of the reform on divorce rates. Further, we calculate mean characteristics of couples who would have remained together and had the mandatory separation period remained in place. Our study makes two contributions. First, because mandatory separation periods remain the norm in Western countries, the study offers insight into how less restrictive laws governing separation affect relationship processes. Second, describing the traits of people that change behavior when no longer forced to undergo a 6-month separation period can inform theories on who reconsiders after initializing divorce proceedings, as well as highlight the understudied role of legal separation as part of the divorce process (Amato, 2010).

**BACKGROUND**

Across most of the 20th century, Western countries liberalized the legal access to divorce (see, e.g., González & Viitanen, 2009; Stevenson & Wolfers, 2007), which together with demographic and socioeconomic developments drastically increased the prevalence of divorce. Denmark was a legal trailblazer in this regard. The Danish Marriage Act of 1922 (Danish Parliament, 1922) introduced modern divorce (Rosenbeck, 2017), obtainable in cases of “family violence, unfaithfulness, venereal disease, mental illness, or imprisonment (Hussein & Kangas, 2009, p. 102)” or following a 1.5- to 2.5-year-long separation. In 1969, unilateral divorce filings became legal and couples could divorce after 12 months apart (Hornslet et al., 1970). The year 1989 saw the separation period shortened to 6 months for uncontested divorces (outlined in Danish Ministry of Justice, 1987).

The 2013 reform dispensed with the mandatory period for uncontested divorces (Danish Parliament, 2013). Further, couples could obtain divorce online. Thus, for couples where both partners agreed to the divorce, the reform removed the need for a process where couples first had to file for separation and then only 6 months later could file for divorce. The reform also increased the fee for obtaining a divorce from €68 to €122 (2020 values). Subsequently in 2016, the fee was decreased to €56 and in 2018 to €46. For an average couple, the highest fee (€122) corresponded to 0.2% of annual disposable income, and for couples with the lowest income (both on social assistance, no children), it corresponded to 0.8% of annual disposable income. In total, the 2013 reform continued a century-long transition toward less restrictive divorce laws in Denmark, this time allowing fast divorce without mandatory separation if both partners agreed (Rosenbeck, 2017).

**Divorce Law, Predictors of Divorce, and the Role of Separation**

Previous work that has studied consequences of divorce law has mainly focused on how changing access to divorce affects outcomes of divorcees (e.g., Fella et al., 2004; Genadek et al., 2015; Kneip et al., 2014; Stevenson, 2008; van Poppel & de Beer, 1993). Part of this research demonstrated that legal changes in divorce law at least partly reflected already changed social practices within couples (Fella et al., 2004; Kneip & Bauer, 2009), but that legal reform still can have effects in itself. For example, unilateral divorce increased divorce risk in Europe (Kneip et al., 2014), although with transitory effects. Lee (2013) studied the introduction of 3-month long cooling-off periods using data from divorce courts in South Korea and found a decrease in the number of effectuated divorces but no change in the number of divorce filings. In the Netherlands, the recent introduction of administrative divorces not requiring court appearances increased the individual risk of divorce around the time of the reform (Kabátek, 2019).

With changing divorce legislation comes also changing motives for divorce. de Graaf and Kalmijn (2006) documented for the Netherlands that as laws expanded the access to divorce, the threshold for when couples chose to divorce lowered. Personal beliefs about, investments in, and wants of the relationship surpassed previous periods more problematic causes (e.g., domestic violence, incarceration, marital desertion) as the main motives for divorce.

Denmark saw a similar development as the Netherlands. Thus, divorce motives occurring at lower thresholds may interact differently with the presence (or lack) of a mandatory separation period. Individuals with more severe causes to divorce (e.g., adultery or domestic violence) may
be less likely to their change of mind during a separation than, say, a divorce filing caused by differences in individuals’ wants from the relationship.

Furthermore, relationship-specific investments, such as joint children (Becker et al., 1977; Brüderl & Kalter, 2001), have been found to not keep couples from divorcing when accounting for selection (Svarer & Verner, 2008). Yet, couples may make their decision on a more considered and informed basis in the presence of joint children (or these couples are just on average better matched, and divorce proceedings more considered [Brüderl & Kalter, 2001; Svarer & Verner, 2008]), and thus be less affected by repeal of mandatory separation.

In terms of personal beliefs, previous work has argued that as relationship duration increases, beliefs about the quality/compatibility of the relationship stabilize (Brüderl & Kalter, 2001; Fallesen & Breen, 2016). Couples who have been together for a shorter period have more volatile beliefs about their relationship, which in turn may cause couples to be more likely to make a divorce decision they would later reconsider given the possibility to do so (Fallesen & Breen, 2016 pp. 1381–2).

We study what characterized couples who increased divorce risk when legal divorce access remained unchanged, but divorces were expedited faster. One possibility is that the increase occurred among those who already had the highest divorce risk. Lyngstad and Jalovaara (2010) provided an extensive review of general predictors of divorce and union dissolution. Summing up the literature, they reported that for the Scandinavian context also considered in this study, divorce risk had a negative educational gradient, as well as a negative gradient for age at marriage. Higher age at marriage may also imply better match quality of the couple, because spouses may have undergone a longer search process before marrying, thus marrying on a more informed basis with a better match (cf. Becker et al., 1977; Kneip et al., 2014; Oppenheimer, 1988). Less clear was the differences in risk among immigrant, native, and mixed background couples. Migrants often bring the divorce culture from their country of origin with them (Furtado et al., 2013). With Denmark as a vanguard country in terms of divorce access, it implies that the average migrant likely brings a more restrictive view of divorce that could be less affected by changes to separation periods.

We expect then that divorce finalization for migrant (and to a lesser extend mixed) couples are affected less by whether separation periods are mandatory or not than the divorce decision are for native Danish couples.

In total, the literature leads us to suspect that the reform likely had heterogeneous impact. Relationship length, the presence of children, age at marriage, education, and ethnic background may all play a role in shaping who the reform impacted. If the reform increased the divorce rate, it is also important to ask for whom it did so.

**Data and Method**

**Data**

We use two data sets to study the impact of the repeal of mandatory separation. The first data set is a monthly time series of all effectuated divorces in Denmark 2007–2018 supplied by Statistics Denmark. To obtain divorce rates, we divide the monthly number of divorces with the annual average number of married couples. We thereby end up using a time series of the Danish divorce rate measured as the number of monthly divorces per 100,000 marriages. The data and accompanying R program are available in supplementary materials.

Figure 1a shows the monthly divorce rate for all married opposite- and same-sex couples for the period 2007–2018, including all finalized divorces. The dashed line indicates the legal introduction of the reform in July 1, 2013. The Danish government introduced the reform jointly with a set of other bureaucratic restructurings and IT changes, which created a 3-month backlog (Nilsson, 2013), causing a 3-month delay in the actual possibility to obtain an uncontested divorce without pre-trial separation (the dotted line). Thus, the sharp drop in divorce risk in July 2013 was driven by problems at introduction of the reform, and the increase of 3 months later represents the actual change in practice. Divorce rates were higher after the reform came into effect. Further, sharp 1-month drops also occurred in January 2015 and December 2017. The 2015 drop was due to the introduction of a new IT system, and the 2017 drop was due to a substantially lower number of cases referred to the family courts that month likely because of new guidelines and legal acts issued in the middle of the month coinciding with fewer workdays because of the
Figure 1. Monthly Number of (a) Divorces per 100,000 Marriages in Denmark, January 2007—December 2018; (b) Marriages per 100,000 Unmarried Adult Dyads, January 2007—December 2018; (c) the Dissolutions per 100,000 Unmarried Unions, January 2007—December 2018.

Notes: Dashed Line Indicates the Introduction of the Reform, and the Dotted Line Indicates When the Reform Came into Effect.
holiday season (information on these incidents obtained from direct communication with the Danish State Administration). These 1-month drops will be considered in the interrupted time series design as “interesting/anomalous” outliers. Figure 1b shows the monthly marriage rate and Figure 1c shows the monthly dissolution rate among unmarried unions (available until 2017 end). Neither of these changed around the time of the reform, indicating that there was no other structural change in the married population nor in relationships in general that could affect the divorce rate in lieu of the reform.

To study who then reacted to the change in divorce law, we use a second data set that includes individual-level population data from Statistics Denmark’s population database on the full stock of married and divorced couples in Denmark. We cannot make the individual data publicly available due to privacy concerns. The access to the data is governed by Statistics Denmark and granted to affiliates of approved Danish research institutions. We included as supplementary material program files used to build the sample and generate the result, together with instructions on how to obtain data access. Due to the nature of the reform effect, we compare monthly data on the stock of married and divorced couples for the period January 2013 to June 2013 (pre-reform) to monthly data for January 2014 to June 2014 (post-reform). We leave out the period immediately following the reform, because our interrupted time series model showed that a substantial amount of divorces was moved forward in time because of the reform, leading to bunching of divorces in the months following the reform. Second, our time series model shows that there is substantial seasonality in divorce. By comparing the same time of year, we take variation in characteristics due to seasonality into account.

We obtain information on couples’ mean age at marriage and their ethnic origins (first- or second-generation immigrant background grouped together due to the second generation making up a minute portion of the data). We include time-varying information on whether the couples have children living with them (following divorce, children living in any one of the households) and time since marriage. The latter we right censor at time of divorce for those who divorce. From the Danish education registry that links directly to the population database through a unique personal identifier, we obtain the highest level of education for both partners at each year of marriage, and group couples into six joint educational categories: (a) both only having less than high school; (b) less than high school with high school; (c) less than high school with college; (d) high school with college; (e) both high school; and (f) both college. Using a gendered grouping did not lead to different results (see Table A1 in online appendix), and an ungendered grouping of education allow us to retain same sex couples in the data. We present descriptive statistics for the sample in the results section.

Estimating the Policy Impact

To estimate the policy impact, we use an interrupted time series design (ITSD) with transfer functions (also known as intervention analysis [Box & Tiao, 1975]). It involves modeling the monthly divorce rate in Denmark as a time series, allowing for the reform to interrupt the time series, and then estimate the shape and impact of the interruption through modeling. Given the shape seen from Figure 1a and the discussion of different types of interruptions to time series provided in Box and Tiao (1975), there appear to be two ways the reform could affect divorce rates, both building on an understanding of the reform representing a pulse intervention (cf. Cryer & Chan, 2008 p. 249ff): (a) The reform may simply hasten time to divorce for couples who would have divorced anyway, now removing the 6-month separation period as well as any period that included temporary reconciliation for couples who would later end up divorcing anyway. Thus, the divorce rate sores at intervention time (i.e., the pulse), but then settles back to pre-reform level over time as couples’ divorce times are moved forward. In ITSD terms, this would be equivalent of the reform representing a pulse whose influence decayed over time through a first-order autoregressive (AR[1]) process. (b) Beyond hastening time to divorce, the reform may also induce divorce among couples who would have stayed together absent the repeal of mandatory separation. In ITSD terms, this would be the equivalent of combining a decaying pulse with a stable increase in the divorce rate (a step-function). Because we use monthly data, there also could be seasonal variation in the divorce rate. To account for seasonal correlation in divorce rates, we include a seasonal autoregressive term as well. In the end,
we compare the two models to see if the effect of the reform decays completely or some effect remains, which is as follows:

$$Y_t = \mu + \theta Y_{t-12} + m^1_t + IO_t + z_t \quad (1)$$

where $Y_t$ is the log of the divorce rate and $m^1_t = \omega_1 B p^{(T)}_t$ is the pulse response function, which increases divorce rate with $\omega_1$ following the reform and decays over time through the AR(1) term $\delta$. $P^{(T)}_t$ is a pulse indicator capturing the periods where $T > t$ indicating the reform has occurred, and $B$ is a backshift operator, so that, for example, $BP^{(T)}_t = P^{(T)}_{t-1}$. $IO_t$ is the vector of the three outliers evident from Figure 1a and discussed earlier. $z_t$ is the white-noise error term. We use the log-transformed divorce rate because it allows ease of interpretation of coefficients as semi-elasticities and stabilizes the variance of the time series. To establish whether the reform merely accelerated time to divorce or also increased the divorce rate, we compare the estimates from the model from Equation (1) to:

$$Y_t = \mu + \theta Y_{t-12} + m^2_t + IO_t + z_t \quad (2)$$

where $m^2_t = \omega_1 B p^{(T)}_t + \omega_2 B p^{(T)}_{t-1}$, and the second term of $m^2_t$ represent the stable increase ($\omega_2$) in the divorce rate once the pulse has decayed. To decide between the two models, we choose the model with the smallest AIC$_C$-value that also passes the diagnostic tests outlined in Cryer and Chan (2008). AIC$_C$ penalizes additional parameters more than AIC does, and is recommended when the observation to parameter ratio is smaller than 40:1 (Anderson & Burnham, 1998). As we will demonstrate in the Results section, the model based on Equation (2) fits the data best, indicating that the reform likely did have a lasting impact on divorce rates. Thus, it makes sense to investigate what characterizes the couples who drove the increase in divorce rate.

Calculating Characteristics for Couples Who Respond to the Policy Change

To identify what characterized the couples who drove the increase in the divorce rate following the July 2013 policy, we draw on an insight from instrumental variable regression. The population stock of ever married couples across the policy change can be thought of as consisting of three groups: (a) those who are divorced no matter if it is before or after the policy change (always-divorced); (b) those who are married no matter if it is before or after the policy change (never-divorced); and (c) those who are divorced if the policy is in effect, but not if the policy is not in effect (reform-shifters). If we consider a sample of couples captured monthly in the period right before to right after the reform, we can divide the three groups into four combinations as evidenced in Table 1. The average of any characteristic (e.g., relationship length or the couple’s joint educational levels) is decomposable into the sum of weighted conditional averages for the three groups. Yet, as seen from Table 1, reform-shifters cannot be isolated as an individual group, which means that their characteristics cannot readily be compared to the general sample or the two other subgroups.

Nevertheless, Abadie (2003, theorem 3.1) demonstrated that under a set of assumptions, it is possible to estimate sample characteristics for the reform-shifters (in Abadie’s (2003) terminology, these are called compliers). First, the reform had to be as good as randomly assigned to couples, meaning that the only difference in divorce probability between couples before and after the reform is driven by the reform. We will show evidence for this in the Reform section. Second, the reform had to increase the risk of being divorced. We also demonstrate this in the reform section. Finally, the reform should not decrease any couple’s risk of divorce. Because the reform merely made 6-month separation voluntary, but still obtainable, we believe this assumption met as well, because couple’s who only divorced if they could have a separation first could still do that.

Abadie (2003) developed a weighting scheme $\kappa$ that “locates” reform-shifters in the sample. The weighting scheme does so by using the fact that when conditioning on being divorced or not [$Divorced_i = \{0, 1\}$], and if the reform had occurred [$Reform_i = \{0, 1\}$] we can directly observe always-divorced (lower left quadrant in Table 1) and never-divorced (upper right quadrant in Table 1). Thereby, we can for any characteristic $X$ calculate a “weight” $\kappa$ expressed as one minus the weights for always-divorced and never-divorced for each individual observation:

$$\kappa_i = 1 - \frac{Divorced_i(1 - Reform_i)}{1 - P(Reform_i = 1|X_i)} - \frac{(1 - Divorced_i) Reforms_i}{P(Reform_i = 1|X_i)}, \quad (3)$$
and use that weight to estimate the average of any characteristic $X$ for reform-shifters by

$$E[X_i|\text{Reform shifter}] = \frac{E[k_iX_i]}{E[k_i]}.$$  \hfill (4)

Following Abadie (2003), we use a probit model to estimate $P(\text{Reform}_i = 1|X_i)$. We can then compare $E[X_i|\text{Reform shifter}]$ to $E[X_i|\text{Always divorced}]$, and $E[X_i|\text{Never divorced}]$ to examine how the couples who reacted to the reform differ from the other parts of the sample.

**RESULTS**

**Impact of 2013 Reform on Divorce Rates**

Table 2 reports the findings from the interrupted time series models based of Equations (1) and (2), as well as from a model without any interventions (Model 0) to serve as a baseline comparison. Both models include a seasonal autoregressive term and dummies for the three outliers that depressed the divorce rate in July 2013, January 2015, and December 2017. Model 1 provides estimate of the policy effect as a pulse function that decays back to the previous divorce rate, whereas Model 2 also includes a step function allowing the divorce rate to stable itself at a new level. Model 0 contains no intervention functions. Because the dependent variable is the log of the monthly divorce rate, changes in independent variables represent percentage change divided by 100. All three models are without notable presence of sequential patterns in residuals (Independence of sequence test). We also cannot reject that residuals follow a standard normal distribution (Shapiro–Wilk test) for Models 1 and 2, or that we do not have independence in the overall group of residuals for Models 1 and 2 (Ljung-Box test). In addition, the augmented Dickey-Fueller test rejects non-stationarity. Further diagnostics shown in Figure A1 in Appendix also corroborate that Models 1 and 2 are stationary and the residuals are well behaved.

In both Models 1 and 2, the reform pulse was highly significant and decayed through an AR(1) process. The sizes of the AR(1) parameters $\delta$ are nevertheless markedly different between the two models. For Model 1, the reform’s half-life occurred 12.4 months after the pulse month, whereas the half-life of the reform pulse in Model 2 occurred after 4.5 months. The faster decay of the pulse in Model 2 was due to the presence of a significant step function, where the effect of the reform caused the divorce rate to decay toward a new stable divorce rate that was 9.7% higher than prior to the reform (95% confidence interval [2.3%; 17.1%]). Using the AICc-value as the criteria for choosing our preferred model, we see that including a step function provided a better fit for the data taking the cost of the additional parameter into account, although the AICc values are close in size. Compared to the baseline Model 0, Model 1 had an AICc that was 84.5% lower, whereas Model 2 had an AICc that was 86.2% lower. At the same time, an LR-test between Models 1 and 2 rejects the null at $p = .07$ and the pulse effect is significant at a 5% level. Overall, our results suggest that the repeal of mandatory separation periods for uncontested divorces appeared to have led to a higher monthly divorce rate in Denmark.

**Characteristics of Couples Who Respond to the Policy Change**

Table 3 presents monthly sample characteristics for ever-married couples in the periods...
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Table 2. Interrupted Time Series Model of Impact of Reform on log(Monthly Divorces per 100,000 Marriages)

| Parameters                        | Model 0          | SE     | Estimate | SE     | Model 1          | SE     | Estimate | SE     | Model 2          | SE     | Estimate | SE     |
|-----------------------------------|------------------|--------|----------|--------|------------------|--------|----------|--------|------------------|--------|----------|--------|
| Seasonal AR(1) \( \theta \)      | 0.25***          | 0.07   | 0.21***  | 0.06   | 0.23***          | 0.06   |          |        |                  |        |          |        |
| Reform pulse intervention \( \omega_1 \) | 0.52***          | 0.08   | 0.59***  | 0.11   |                  |        |          |        |                  |        |          |        |
| AR(1) of reform pulse intervention \( \delta \) | 0.94***          | 0.02   | 0.85***  | 0.06   |                  |        |          |        |                  |        |          |        |
| Reform step-intervention \( \omega_2 \) | 0.10**           | 0.04   |          |        |                  |        |          |        |                  |        |          |        |
| IO (July 2013)                    | -0.79***         | 0.18   | -0.75*** | 0.15   | -0.74***         | 0.15   |          |        |                  |        |          |        |
| IO (January 2015)                 | -1.14***         | 0.18   | -1.21*** | 0.15   | -1.12***         | 0.15   |          |        |                  |        |          |        |
| IO (December 2017)                | -0.62***         | 0.18   | -0.60*** | 0.15   | -0.65***         | 0.15   |          |        |                  |        |          |        |
| Time series mean \( \mu \)       | 4.85***          | 0.02   | 4.79***  | 0.02   | 4.78***          | 0.02   |          |        |                  |        |          |        |
| AIC_C                             | -68.43           |        | -126.27  |        | -127.39          |        |          |        |                  |        |          |        |
| Log-likelihood                    | 40.43            |        | 71.55    |        | 73.23            |        |          |        |                  |        |          |        |
| Independence of sequence-test     | \( p = .21 \)    |        | \( p = .53 \) |        | \( p = .50 \)   |        |          |        |                  |        |          |        |
| Shapiro–Wilk normality test       | \( p < 0.01 \)   |        | \( p = .58 \) |        | \( p = .62 \)   |        |          |        |                  |        |          |        |
| Ljung-Box test, 24 lags           | \( p < 0.01 \)   |        | \( p = .25 \) |        | \( p = .23 \)   |        |          |        |                  |        |          |        |
| Augmented-Dickey-Fueller test, 5 lags | \( p = 0.10 \)  |        | \( p < .01 \) |        | \( p < .01 \)  |        |          |        |                  |        |          |        |
| \( T \)                          | 144              |        | 144      |        | 144              |        |          |        |                  |        |          |        |

Note: See supplementary materials for data and code. LR test between Models 1 and 2 rejects the null at \( p = .07 \). *\( p < .1 \); **\( p < .05 \); ***\( p < .01 \). Source: Own calculations on data from Statistics Denmark.

January 2013 to June 2013 (pre-reform) and January 2014 to June 2017 (post-reform). We leave out the 6 months after the reform to not conflate always-divorced hastening their divorce with reform-shifters induced to divorce by the reform. The sample does appear slightly imbalanced on relationship length and whether the couples have children. The likely cause of the imbalance in marriage length can be glimpsed from Figure 1b. Around 2009, the marriage rate declined, possibly as a reaction to the Great Recession. The Great Recession also had substantial impact on fertility in Denmark causing long-term decrease in the probability of transitioning to parenthood (Comolli et al., 2019). The imbalances should be kept in mind when interpreting the results on reform-shifter characteristics.

Table 3 also reports calculations on average reform-shifter characteristics using the formulas from Equations (3) and (4) for couples who divorced because of the repeal of the separation period. For reform-shifters, we obtain \( SE \)s on the estimates through bootstrapping. We compare reform-shifter characteristics to the full sample, those who always are divorced no matter the reform (always-divorced) and couples never divorced no matter the reform (never-divorced). Because the three other groups are identified for the full population, we treat their values as fixed.

Compared to the all three other groups, reform-shifters had been married for a shorter period. They were less likely than always-divorced couples to be on their second marriage, but more likely to than the average couple was. Reform-shifters married younger than the general sample, but older than the always divorced. In terms of ethnicity, the reform-shifter group was more likely to be ethnic Danish, and better educated than the always-divorced group, but less educated than the full sample. Reform-shifters were also less likely to have children than the always-divorced, although difference was only borderline significant. When we compare reform-shifters to couples who remain married (never-divorced), we see the same differences as with the full sample.

Conclusion

In this study, we presented the effect of repeal of mandatory separation periods prior to effectuation of a divorce on the Danish divorce rate. The repeal substantially increased risk of being divorced. We then investigated who would have stayed together absent the reform.
Reform-shifters were more likely to be recently married compared to couples who always divorce, as well as more likely to have low and medium levels of education compared to the average population of couples. What we designated reform-shifter couples were thus not those with the strongest risk factors, but “the next in line”—compared to couples who always divorced (mandatory separation period or not) reform-shifters had better education, but were still less well educated compared to couples who always remained married. In this sense, it does indicate that the reform did change a threshold. It also points to differences in response to bureaucratic changes—reform-shifters are generally those that we would expect to have an easier time navigating bureaucratic machinery through some level of education and a native majority background.

The presented research comes with limitations. First, the model that allowed the divorce rate to settle at a new, higher level provide a better fit of the time series data, whereas the preferred model only improved the AICC with 1.7 percentage points more than the alternative model when measured against a baseline model. Thus, another year or two of data would have been preferable. Alas, the

Table 3. Average Characteristics of (a) Individual Monthly Sample Pre- and Post-Reform; and (b) Reform-shifters, Full Sample, Always-Divorced, and Never-Divorced

| Covariate                                | Sample balance | Group characteristics |
|------------------------------------------|----------------|-----------------------|
|                                          | Pre-reform | Post-reform | Reform-shifter | Full Sample | Always-divorced | Never-divorced |
|                                          | Mean | SD | Mean | SD | Mean | SE | Mean | Mean |
| Divorced                                 | 0.22  | 0.41 | 0.24  | 0.43 | 0.23*** | 0.32 | 0.20*** |
| Time since marriage (right censored at divorce): |         |         |         |         |         |         |         |
| ≤5 years                                 | 0.24  | 0.43 | 0.23  | 0.42 | 0.33 | 0.01 | 0.23*** | 0.32 |
| >5 years and ≤10 years                   | 0.25  | 0.43 | 0.25  | 0.43 | 0.41 | 0.01 | 0.25*** | 0.36*** |
| >10 years                                | 0.51  | 0.50 | 0.53  | 0.50 | 0.35 | 0.01 | 0.52*** | 0.32*** |
| Mean age at marriage ≤25                 | 0.11  | 0.31 | 0.11  | 0.31 | 0.11 | 0.01 | 0.11 | 0.18*** |
| >25, ≤30                                 | 0.32  | 0.47 | 0.32  | 0.47 | 0.37 | 0.01 | 0.32*** | 0.37 |
| >30, ≤35                                 | 0.25  | 0.43 | 0.25  | 0.43 | 0.27 | 0.01 | 0.25*** | 0.23*** |
| >35, ≤40                                 | 0.13  | 0.34 | 0.13  | 0.34 | 0.14 | 0.01 | 0.13 | 0.12*** |
| >40, ≤45                                 | 0.08  | 0.27 | 0.08  | 0.27 | 0.07 | 0.00 | 0.08* | 0.06** |
| >45                                      | 0.06  | 0.24 | 0.06  | 0.24 | 0.03 | 0.00 | 0.06*** | 0.02*** |
| Second+ marriage                         | 0.12  | 0.33 | 0.12  | 0.32 | 0.22 | 0.01 | 0.12*** | 0.34*** |
| Ethnic origin: Both native               | 0.04  | 0.20 | 0.04  | 0.21 | 0.01 | 0.00 | 0.04*** | 0.04*** |
| Native with nonnative                    | 0.08  | 0.27 | 0.08  | 0.27 | 0.05 | 0.01 | 0.08*** | 0.10*** |
| Both nonnative                           | 0.88  | 0.32 | 0.88  | 0.33 | 0.94 | 0.01 | 0.88*** | 0.86*** |
| Education:                               |         |         |         |         |         |         |         |         |
| Both less than HS                        | 0.13  | 0.34 | 0.13  | 0.34 | 0.09 | 0.01 | 0.13*** | 0.21*** |
| Less than HS w. HS                       | 0.21  | 0.41 | 0.21  | 0.41 | 0.24 | 0.01 | 0.21*** | 0.28*** |
| Less than HS w. college                  | 0.06  | 0.24 | 0.06  | 0.24 | 0.06 | 0.00 | 0.06 | 0.05 |
| HS w. college                            | 0.19  | 0.39 | 0.19  | 0.39 | 0.21 | 0.01 | 0.19*** | 0.13*** |
| Both HS                                  | 0.26  | 0.44 | 0.25  | 0.44 | 0.33 | 0.01 | 0.26*** | 0.25*** |
| Both college                             | 0.15  | 0.36 | 0.15  | 0.36 | 0.12 | 0.01 | 0.15*** | 0.07*** |
| Children                                 | 0.61  | 0.49 | 0.58  | 0.49 | 0.60 | 0.01 | 0.60 | 0.61* |
| Number of observations                   | 5,586,055 | 5,718,531 | 11,304,566 | 1,228,928 | 4,346,084 |

Note: The terms reform-shifter, never-divorced, and always-divorced do not refer to time-constant characteristics of couples but refer instead to time-specific couple characteristics. HS = high school. Reform-shifter characteristics for categorical variables does not necessarily sum to 1. Source: Own calculations on data from Statistics Denmark. Standard errors on reform shifter characteristics obtained from 100 bootstrap repetitions. *p < .1; **p < .05; ***p < .01.
Danish Government reintroduced mandatory separation periods of 3-month duration for divorcing parents per April 1, 2019 (Danish Parliament, 2018), thereby effectively creating a new time series intervention. Similarly, the kappa-weighting scheme used to generate average reform-shifter characteristics assumed that the post-reform group would have had similar divorce rates as the pre-reform group had the reform not occurred. Yet, the groups appeared a little unbalanced on length of relationship and whether children were present—the latter likely caused by the former, because we saw fewer shorter relationships post-reform as well as fewer couples with children. Children were roughly as likely among reform-shifter couples as among other couples, and short relationship length more likely among reform-shifters even though we observed fewer of these couples post-reform.

These empirical and methodological limitations aside, the study demonstrates an important empirical point. When studying the impact of repeal of mandatory separation on divorce rates, it is important to consider that divorce rates will be substantially higher right after a reform, due to (a) hastening of divorce among couples who would have divorced after the separation period, had the separation period still been in place; and (b) couples who divorce following their first divorce filing, who otherwise would have reunited, then separated again, and then divorced. Failing to account for this bunching effect may lead to overestimation of the impact of repeal or introduction of mandatory separation periods.

Perspective

The laws and motives governing divorce saw substantial change across the 20th century. Legal reforms changed access to divorce and shortened periods of mandatory separation. Most divorce reforms, and thereby also most studies of divorce reforms, have focused on changes in access to divorce. As more and more countries adopt unilateral no-fault divorce laws, repeal of mandatory separation and the introduction of administrative divorces are the obvious next frontiers if the trend toward a more liberal divorce policy like those found in Denmark is to continue.

Yet, the complete repeal of mandatory separation is qualitatively different than previous reforms that predominantly changed the accessibility of divorce. Removing separation periods leaves the opportunity to divorce unchanged. Instead, the divorce itself happens sooner. In normative terms, mandatory separation periods borders a space between (paternalistically) preserving relationships between people who may make rash decision they could later regret, and a liberalization of family life where the decision to divorce is viewed similarly to the decision to marry—that is, something the state grants when people meet a minimum set of requirements, but does not otherwise seek to regulate or delay. This study has joined the sparse ranks of research that consider how the presence versus absence of mandatory separation affect divorce rates (Kabátek, 2019; Lee, 2013), and have thereby at least made plausible that mandatory separation periods do serve to keep some couples together. The study also adds very timely knowledge from a national policy perspective. As of April 1, 2019, the Danish government reintroduced a 3-month mandatory separation period for married couples with children (Danish Parliament, 2018). The re-introduction of a new, shorter separation period was part of a general overhaul of Danish family law that also disbanded the previous agency managing family law (the State Administration) and introduced a new governing body (the Agency of Family Law). The new 3-month period (called a “reflection period”) was demanded by the traditionalist Danish Peoples’ Party with the specific stated aim of lowering divorce rates (Friis, 2017). As of June 19, 2020, the reflection period was repealed by a majority of Danish parliament after public discussions about adverse consequences the mandatory period may have on children (Lessel, 2020).

From a theoretical perspective, the study has sought to inform the literature on how couples reach the decision to divorce. Previous work has focused on, among other things, the role of information and marital-specific capital (Brüderl & Kalter, 2001; Fallesen & Breen, 2016). Here, we instead aimed to study what characterizes couples who increased divorced risk after repeal of mandatory separation periods, who could have reversed their decision if given a mandatory period of separation. Denmark, as a country with high gender equality that has moved from high partnership instability in the 1980s (cf.
Espin-Andersen & Billari, 2015), now has the divorce risk concentrated among lower educated strata (Grow et al., 2017). From this, we might expect a Matthew-like effect of divorce risk by the repeal of mandatory separation periods, with the lowest educated more likely to be reform-shifters. Yet, the results indicated a slightly different story—couples who had been together for shorter periods, who thereby knew each other less well (cf. Fallesen & Breen, 2016), were more likely to reconcile rather than divorce if subject to mandatory separation, but so were those with upper secondary education as well. Thus, divorce decisions that would more likely be resolved if couples were forced to have a trial separation appear more common in the middle strata of the educational distribution and among the more recently wed.

**Supporting Information**

Additional supporting information may be found online in the Supporting Information section at the end of the article.

**Appendix S1:** Supporting Information

**Appendix S2:** Supporting Information

**References**

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