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Who Reacts to Less Restrictive Divorce Laws?*

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ABSTRACT

Objective: To study how divorce behavior in Denmark changed following a July 2013 reform that repealed mandatory separation periods for uncontested divorces, instead allowing for immediate administrative divorce.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 2007-2018 (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=40,431,848) 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 9.7 percent higher level compared to pre-reform. Couples who divorced because of the reform had been married for fewer years, were ethnic Danish, and had high school degree as highest educational level.

Conclusion: Mandatory separation periods keep a minor, but substantial, share of potential divorcees together.

INTRODUCTION

Divorce is life-altering and causes individuals to face new economic, social, and emotional challenges (Amato, 2000; Leopold, 2018; Leopold & Kalmijn, 2016; McManus & DiPrete, 2006; Smock, Manning, & Gupta, 1999). 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, they 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, Marks, & Hawkins, 2016). If cooler heads prevail, mandatory separation may cause couples who otherwise would have divorced in the heat of the moment to reconcile.

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 six-month separation period for uncontested divorces, allowing couples to instead divorce immediately (Rosenbeck, 2017). Using an interrupted time series design, we estimate that after an initial spike caused by couples who divorced faster, divorce rates settled at a nine percent higher rate than before the reform. Further, we calculate mean characteristics of couples who would have remained together, had the mandatory separation period remained in place. Our study makes two contributions. First, describing the traits of people that change behavior when no longer forced to undergo a six-month cooling off period can inform theories on who rushes into divorce proceedings and highlight the understudied role of legal separation as part of the divorce process (Amato, 2010). Second, because mandatory

separation periods remain the norm in Western countries, the study offers insight into how less restrictive divorce laws governing separation affect relationship processes.

BACKGROUND

Across most of the 20th century, Western countries liberalized the legal access to divorce (see, e.g., González & Viitanen, 2009; Schoen, Greenblatt, & Mielke, 1975; Stevenson & Wolfers, 2007; Wright & Stetson, 1978), which together with demographic and socioeconomic developments skyrocketed the prevalence of divorce. Denmark was a legal trailblazer in this regard. The Danish Marriage Act of 1922 (Danish Parlament, 1922) introduced modern divorce (Rosenbeck, 2017), obtainable in cases of: "family violence, unfaithfulness, venereal disease, mental illness or imprisonment (Hussain & 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 twelve months apart (Hornslet, Danielsen, & Hermann, 1970). 1989 saw the separation period shortened to six months for uncontested divorces (outlined in Danish Ministry of Justice, 1987). The 2013 reform dispensed with the mandatory period for uncontested divorces (Danish Government, 2013). Further, couples could obtain divorce online. As a result, the 2013 reform continued a century long transition towards less restrictive divorce laws in Denmark, this time allowing almost instantaneous divorce if both partners agreed (Rosenbeck, 2017).

Divorce Law and Predictors of Divorce

Previous work considering consequences of divorce law mainly studies how changing divorce access affects outcomes of divorcees (e.g., Genadek, Stock, & Stoddard, 2015; Giulio Fella, Paola Manzini, & Marco Mariotti, 2004; Kneip, Bauer, & Reinhold, 2014; Stevenson, 2008; van Poppel & de Beer, 1993). Legal changes at least partly reflectes already changed practices (Giulio Fella et al., 2004; Kneip & Bauer, 2009), but the introduction of, for example, unilateral divorce increased divorce risk in Europe (Kneip et al., 2014), although

effects appeared transitory. Recently, Lee (2013) studied the introduction three-month long cooling-off periods using data from divorce courts in South Korea and found a decrease in number of effectuated divorces but no change in number of divorce fillings. 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 comes also changing motives for divorce. De Graaf and Kalmijn (2006) documented for the Netherlands that across the second half of the 20th century, as laws expanded the access to divorce, divorce motives became less "problematic" (e.g., domestic violence) and more individualized (e.g., relational, psychological, and gender ideological). Denmark saw a similar legal development as the Netherlands. With the lower threshold, the absence of a legal separation period could induce divorce for couples who had less information about each other (Brüderl & Kalter, 2001; Fallesen & Breen, 2016), or who had less marriage specific capital (Brüderl & Kalter, 2001). That is, the absence of stabilizing factors, such as a firm belief about the quality of the relationship or the presence of children, could have made couples more likely to react to repeals of mandatory legal separation.

In the present paper, 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. Less clear was the differences in risk among immigrant, native, and mixed couples. Yet, we may expect non-natives to have reacted less to the reform. First, bureaucratic changes in divorce legislation that does not

change access to divorce may have changed non-natives divorce risk less, because their divorce threshold were higher from the outset (cf. Fallesen & Breen, 2016). Second, culture and language differences could have made non-native couples less responsive to legislative changes to mandatory separation periods.

In total, the literature leads us to suspect that the reform not necessarily affected all types of couples equally. 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. First, a monthly data set supplied by Statistics Denmark includes all effectuated divorces in Denmark 2007-2018. To obtain divorce rates, we divide the monthly number of divorces with annual average number of married couples taking the mean of the amounts at the beginning and end of each calendar year. The data and accompanying R-program are available in supplementary materials. To document who reacted to the reform, we use a restricted couple-level dataset that includes characteristics of all ever married couples in Denmark, including an indicator for whether a couple is divorced in a given month and that right-censor ever married relationships at death of a (former) spouse, annulment due to bigamy (only very few cases), or remarriage of either partner.

Figure 1(a) shows the monthly divorce rate for all married couples for the period 2009-2018. 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 three month backlog (Nilsson, 2013), causing a three 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 three month later represents the actual change in practice. Divorce rates were higher after the reform came into effect. Further, sharp temporary 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 likely due to a substantially lower number of cases referred to the family courts (information on these incidents obtained from communication with the Danish State Administration). These temporary drops will be considered in the interrupted time series design. Figure 1(b) shows the monthly marriage rate and Figure 1(c) shows the monthly dissolution rate among unmarried unions (only available until end-2017). 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.

[Figure 1 here.]

To study who then reacted to the change in divorce law, we use individual level data from Statistics Denmark's population database. Due to the nature of the reform effect, we compare monthly data on whether couples divorced for the period July 2011, to June 2013 (pre-reform) to monthly data for July 2014, to June 2017. We leave out the period following the reform, because as our interrupted time series model will show, 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.

We obtain information on couples' mean age at marriage and their ethnic origins (first- or second-generation immigrant background grouped together). We include time-varying information on whether the couples have children living with them (following divorce, children living in one of the households). From the Danish education registry, we obtain highest level of education at year of marriage, and group couples into two educational

categories with primary education as reference: 1) at least one finished high school; and 2) at least one finished college. We present descriptive statistics for the sample in the results section. We cannot make the individual data publicly available due to privacy concerns. Instead, we include as supplementary material program files used to build the sample and generate the result, together with instructions on how to obtain data access through Statistics Denmark.

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 modelling 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 modelling. Data on the monthly divorce rate is obtained from Statistics Denmark's publicly available website. Given the shape seen from Figure 1(a), as well as the discussion of different forms provided in Box & Tiao (1975), there appear to be two ways in which the reform could affect divorce rates, both building on an understanding of the reform representing a pulse intervention (cf. Cryer & Chan, 2008 p. 249ff). 1) The reform may simply hasten time to divorce for couples who would have divorced anyway, now removing the six-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. 2) 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, the two models we compare to see if the effect of the reform decays completely or some effect remains are:

$$Y_t = \mu + \theta \mathrm{Y}_{\mathrm{t}-12} + m_t^1 + \boldsymbol{IO_t} + z_t \qquad (1)$$

where Y_t is the log of the divorce rate, $m_t^1 = \frac{\omega_1 B}{1-\delta B} P_t^{(T)}$ is the pulse response function which increases divorce rate with ω_1 following the reform and decays over time through the AR(1) term δ. $P_t^{(T)}$ is a pulse indicator capturing the periods where T > t indicating the reform has occurred, and *B* is a backshift operator, so that, e.g., $BP_t^{(T)} = P_{t-1}^{(T)}$. $\boldsymbol{IO_t}$ is the vector of the three outliers evident from Figure 1(a). 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 eq. (1) to:

$$Y_t = \mu + \theta \mathrm{Y}_{\mathrm{t}-12} + m_t^2 + \boldsymbol{IO_t} + z_t \qquad (2)$$

where $m_t^2 = \frac{\omega_1 B}{1-\delta B} P_t^{(T)} + \frac{\omega_2 B}{1-B} P_t^{(T)}$, and the second term of m_t^2 represent the stable increase (ω_2) in the divorce rate once the pulse has decayed. To decide between the two models, we choose the model with the smallest AIC-value that also passes the diagnostic tests outlined in Cryer & Chan (2008). As we will demonstrate in the Results section, the model based on eq. (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 couples who the reform induced to divorce (i.e., complier couples).

Calculating Complier Characteristics

We use methodological insights from Abadie (2003) to obtain average characteristics of couples who divorced solely due to the reform. $D_i(1)$ indicates the divorce decision for couple *i* who is married in a post-reform period, and $D_i(0)$ indicates the divorce decision for couple *i* who is married in a pre-reform period. We define our population as consisting of three groups of couples: 1) never-takers, who in a given month never are divorced $[D_i(1) = D_i(0) = 0]$; 2) always-takers, who in a given month always are divorced $[D_i(1) = D_i(0) = 1]$; and 3) compliers, who are divorced in a given month if the reform has occurred $[D_i(1) > D_i(0)]$. Table 1 shows the distribution of the three groups across the reform and divorce outcomes (adapted from Angrist and Pischke 2009). The terms complier, never-taker, and always-taker refer to couples at specific months, not to a couple-constant trait. Never-takers in a given month can be compliers or always-takers in a later month.

[Table 1 about here.]

We cannot identify individual complier couples because they always coexist with either never-takers or always-takers (as seen from Table 1). Yet, the mean of any sample characteristic, such as ethnicity of the couple or average age at marriage, can be viewed as a weighted average of the characteristics of the three groups specified in Table 1. Abadie (2003) demonstrates that through the weighting-scheme κ we can obtain the mean characteristics of the complier group and compare these to the full sample, always-takers, and never-takers [see also Angrist and Pischke (2009)]. The mean of any characteristic *X* of the complier group can be calculated as:

$$E[X_i|D_i(1) > D_i(0)] = \frac{E[\kappa_i X_i]}{E[X_i]} \qquad (3)$$

where

$$\kappa_i = 1 - \frac{D_i(1-Reform_i)}{1-P(Reform_i = 1|X_i)} - \frac{(1-D_i)Reform_i}{P(Reform_i = 1|X_i)} \qquad (4)$$

and $Reform_i$ indicates if the period is after the 2013 reform, and D_i is the indicator of divorce. We can observe all elements in Eq. (4) in the data or calculate them under the assumption that pre- and post-reform groups are similar. As we show in the Results section this is likely a feasible assumption twelve months after the reform. Following Abadie (2003), we use a probit model to estimate $P(Reform_i = 1|X_i)$. $E[X_i|D_i(1) > D_i(0)]$ then expresses the mean of any characteristic *X* for the complier group. We can compare characteristics of reform compliers to the general sample of ever married couples, as well as to always- and never-takers. Thereby, we can examine how those who divorce when you remove mandatory legal separation are different from the general pool of ever married couples.

RESULTS

Impact of 2013 Reform on Divorce Rates

Table 2 reports the findings from the interrupted time series models based of eq. 1 and 2. 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. Because the dependent variable is the log of the monthly divorce rate, changes in independent variables represent percentage change divided by 100. Both models appear stationary without notable presence of sequential patterns in residuals (Independence of sequence test). We also cannot reject that residuals follows a standard normal distribution (Shapiro-Wilk test) or that we do not have independence in the overall group of residuals (Ljung-Box test). Further diagnostics shown in Figure A1 in appendix also corroborate that both models are stationary and well-behaved.

[Table 2 about here.]

In both models, the reform pulse was highly significant and decayed through an AR(1) process. The size of the AR(1) parameters δ 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 towards a new stable divorce rate that was 9.7 percent higher than prior to the reform (95% confidence interval [2.3%;17.1%]). Using the AIC-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 AIC values are close in size. Thus, the repeal of mandatory separation periods for uncontested divorces appeared to have led to a higher monthly divorce rate in Denmark.

Complier Characteristics

As demonstrated above, credibly the 2013 reform induced divorces. Therefore, we use information on couples on either side of the reform to calculate average complier characteristics. Table 4 presents monthly sample characteristics for ever-married couples in the periods January 2011 to July 2013 (pre-reform) and July 2014 to December 2017 (post-reform). We leave out the 12 months after the reform, to not conflate always-takers hastening their divorce with compliers 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, possible as a reaction to the Great Recession. The imbalances should be kept in mind when interpreting the results on complier characteristics. We return to this in the Discussion section.

Table 4 also reports calculations on average complier characteristics using the formulas from eq. (3) and (4) for couples who divorced because of the repeal of the separation period. We compare complier characteristics to the full sample, those who always are divorced no matter the reform (always takers) and couples never divorced no matter the reform (never takers). Compared to the full sample of married and divorced couples, compliers were more recently married and one or both spouses were more likely to be on their second or higher order marriage. Complies were also less likely to have married at above age 45 on average. In terms of ethnicity, the complier group had a higher share of ethnic Danish couples and couples with high school as highest level of education. Slightly fewer complier couples had college degrees as highest level of education, more had high school, and slightly more had children. When we compare compliers to couples who remain married (never takers), we see the same differences as with the full sample.

[Table 4 about here.]

Compared to couples who are always divorced no matter a six-month separation period or not, compliers again had been married for a shorter period, but were less likely to be on their second or higher order marriage. Compliers were also older at time of marriage, more likely to be ethnic Danes, and a higher share of compliers had college educations.

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. Mandatory separation periods kept recently married couples together, as well as couples with low and medium levels of education compared to the average population of couples. What we designated complier couples were thus not those with the strongest risk factors, but "the next in line" – they were better educated than couples who always divorced separation period or

not, but less well educated compared to couples who always remained married. In this sense, it does appear that the reform did change a threshold. It also points to differences in response to bureaucratic changes – compliers 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 certain limitations. First, whereas the model that allows the divorce rate to settle at a new, higher level does provide a better fit of the time series data, the gain in AIC is limited. Thus, another year or two of data would have been preferable. Alas, the Danish Government reintroduced mandatory separation periods of three-month duration for divorcing parents per April 1, 2019 (Danish Government, 2018), thereby effectively creating a new time series intervention. Similarly, the kappa-weighting scheme used to generate average complier characteristics assumes 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 see fewer shorter relationships post-reform as well as fewer couples with children. Because children roughly are as likely among complier couples than among other couples, and short relationship length more likely among compliers even though we observe fewer of these couples post-reform, indicating that the imbalance likely is not driving this finding.

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 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 is the obvious next frontier if the trends towards a more liberal divorce policy 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 evident 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 three month mandatory separation period for married couples with children (Danish Government, 2018).

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 studied what characterizes couples who have low discount rates in terms of relationship decision-making—that is, which couples reverse their divorce decision if given a cooling-off period? Denmark, as a country with high gender equality that has moved from high partnership instability in the 1980 to high partnership stability in the 2000s, (cf. Esping-Andersen & Billari, 2015), now has the divorce risk concentrated among lower educated strata (Grow, Schnor, & Van Bavel, 2017). From this we might expect a Matthew-like effect of divorce risk by the repeal of mandatory separation periods, with lowest educated more likely to be compliers. 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), benefitted from separation, but so did those with upper secondary education as well. Thus, divorce decisions driven by resolvable differences appear more common in the middle strata of the educational distribution and among the more recently wed.

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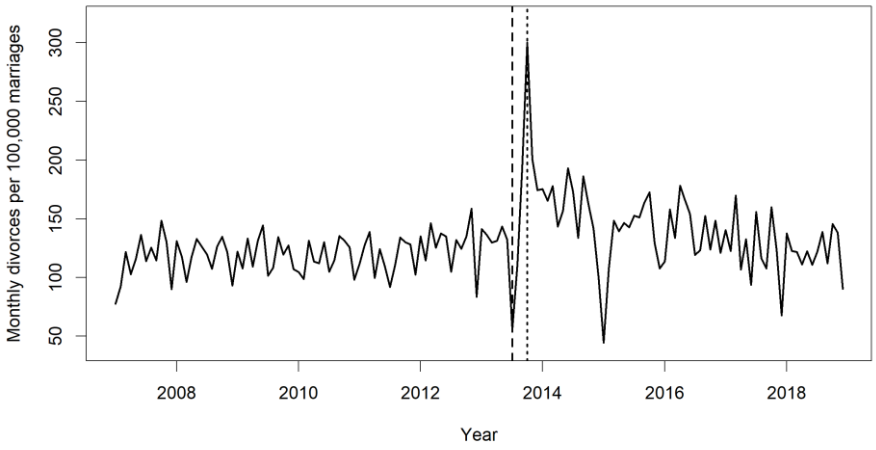
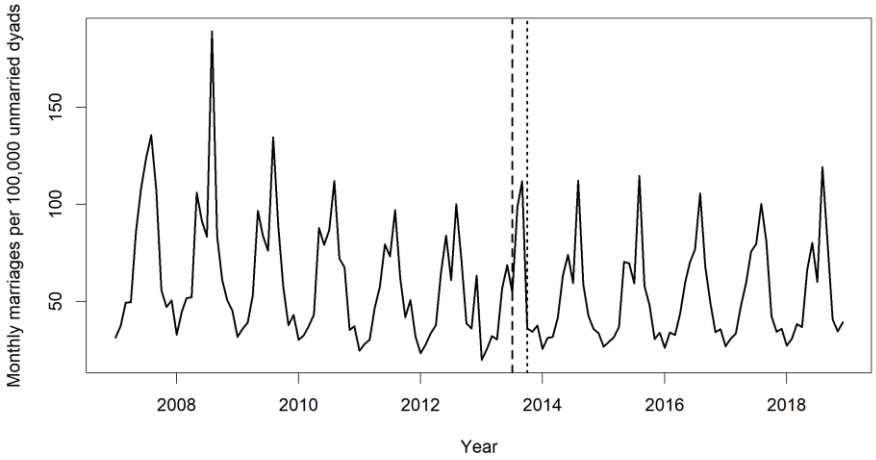
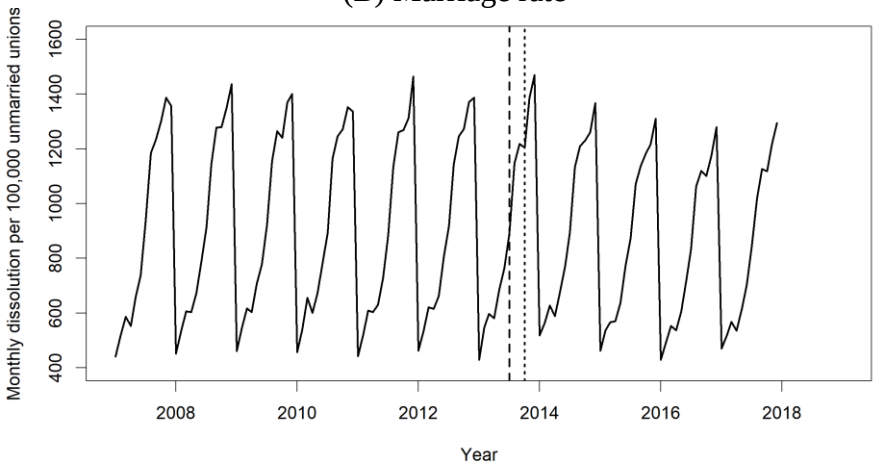
FIGURE 1. MONTHLY NUMBER OF (A) DIVORCES PER 100,000 MARRIAGES IN DENMARK, JANUARY 2007 – DECEMBER 2018; (B) MARRIAGES PER 100,000 UNMARRIED ADULT DYADS, JANUARY 2007 – DECEMBER 2018; (C) THE DISSOLUTIONS PER 100,000 UNMARRIED UNIONS, JANUARY 2007 – DECEMBER 2017



(A) Divorce rate



(B) Marriage rate



(C) Unmarried union dissolution rate

Notes: Dashed line indicates the introduction of the reform, and the dotted line indicates when the reform came into effect. Data on unmarried unons only available until end of 2017.

Table 1
Distribution of Groups Across Reform and Outcome

	Reform = 0	Reform = 1
Divorce = 0	Never-takers Compliers	Never-takers
Divorce = 1	Always-takers	Always-takers Compliers

Table 2
Interrupted Time Series Model of Impact of Reform on log(Monthly Divorces per 100,000 Marriages)

	Model 1		Model 2	
Parameters	Estimate	SE	Estimate	SE
Seasonal AR(1) θ	0.21***	0.06	0.23***	0.06
Reform pulse intervention ω_1	0.52***	0.08	0.59***	0.11
AR(1) of reform pulse intervention δ	0.94***	0.02	0.85***	0.06
Reform step-intervention ω_2			0.10**	0.04
IO(July 2013)	-0.75***	0.15	-0.74***	0.15
IO(January 2015)	-1.21***	0.15	-1.12***	0.15
IO(December 2017)	-0.60***	0.15	-0.65***	0.15
Time series mean μ	4.79***	0.02	4.78***	0.02
AIC	-129.1		-130.45	
Log-Likelihood	71.55		73.23	
Independence of sequence-test	p = .53		p = .50	
Shapiro-Wilk normality test	p = .58		p = .62	
Ljung-Box test, 24 lags	p = .25		p = .23	
T	144		144	

Note: See supplementary materials for data and code.
*p < .1; ** p < .05; *** p < .01.
Source: Own calculations on data from Statistics Denmark.

Table 3

Average Characteristics of (a) Individual Monthly Sample Pre- and Post-Reform; and (b) Compliers, Full Sample, Always-Takers, and Never-Takers

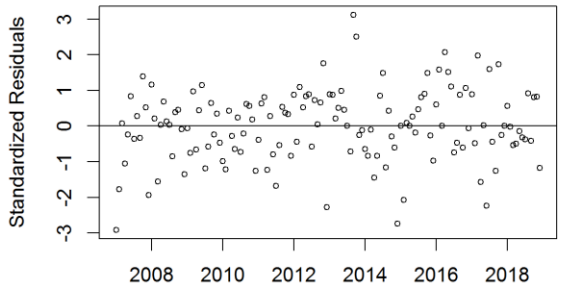
Covariate	Sample balance				Group Characteristics			
	Pre-reform		Post-reform		Complier	Full Sample	Always-takers	Never-takers
	Mean	S.D.	Mean	S.D.	Mean	Mean	Mean	Mean
Divorced	.22	.41	.25	.43				
Length <=5 years	.18	.39	.15	.36	.45	.17	.02	.20
Length > 5 years and <= 10 yrs	.20	.40	.18	.38	.15	.19	.12	.20
Mean age at marriage for couple:								
=< 25	.11	.31	.11	.31	.14	.11	.18	.09
> 25, =< 30	.32	.47	.32	.47	.40	.32	.37	.31
> 30, =< 35	.24	.43	.25	.43	.25	.25	.23	.25
> 35, =< 40	.13	.34	.13	.35	.10	.13	.12	.14
> 40, =< 45	.08	.27	.08	.27	.05	.08	.06	.08
> 45	.12	.32	.11	.31	.03	.06	.02	.08
Second+ marriage	.12	.32	.11	.31	.18	.12	.33	.05
Mix-native origin	.08	.27	.08	.27	.04	.08	.10	.07
Non-native origin (both)	.04	.20	.05	.22	.01	.04	.04	.05
High school as highest degrees	.44	.50	.43	.50	.55	.44	.49	.41
College as highest degree	.39	.49	.41	.49	.31	.40	.25	.45
Children	.63	.48	.56	.50	.61	.59	.64	.56
N*T	19497437		20934411					

Note: The terms compliers, never-takers, and always-takers do not refer to time-constant characteristics of couples but refer instead to time-specific couple characteristics.

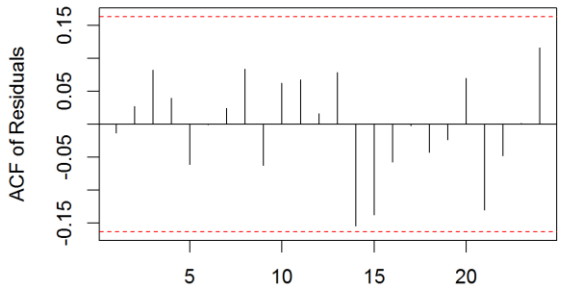
Source: Own calculations on data from Statistics Denmark.

FIGURE A1. DIAGNOSTIC PLOTS FOR INTERRUPTED TIME SERIES MODEL WITH ONLY DECAYING PULSE INTERVENTION (LEFT COLUMN) AND WITH ADDITIONAL STEP-FUNCTION (RIGHT COLUMN) SHOWING STANDARDIZED RESIDUALS (TOP PANEL), AUTOCORRELATION FUNCTION PLOT OF RESIDUALS (MIDDLE PANEL), AND PLOTTED LJUNG-BOX TEST STATISTICS ACROSS LAGS (BOTTOM PANEL).

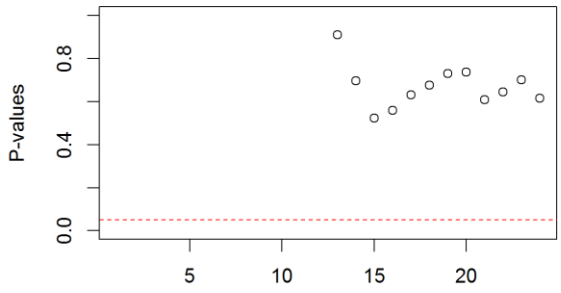
Model 1



(a) Standardized residuals

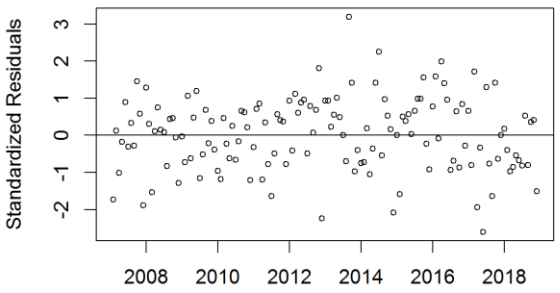


(c) ACF plot

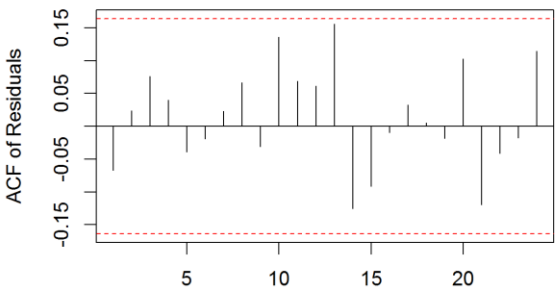


(e) Ljung-Box across lags

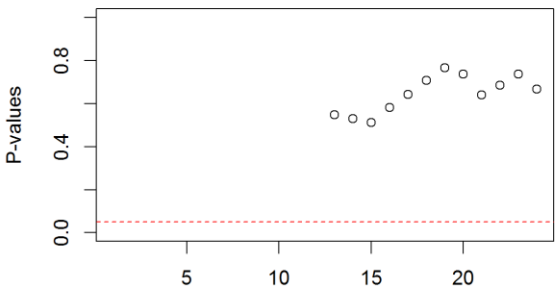
Model 2



(b) Standardized residuals



(d) ACF plot



(f) Ljung-Box across lags

Source: Own calculations on data from Statistics Denmark. Produced using the tsdiag() command in R. Dotted lines in middle and lower panels indicate the 5% significance levels. Absence of dotted lines in top panel is due to all residuals falling within the area expected under the null-hypothesis of no significant outliers in residuals.