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Weathering the storm together: Does unemployment insurance help couples avoid divorce?

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Abstract

This study examines whether unemployment insurance benefit generosity impacts divorce, drawing on full population administrative data and a Swiss reform that reduced unemployment insurance maximum benefit duration. We assess the effect of the reform by comparing the pre- to the post-reform change in divorce rates among unemployed individuals who were affected by the reform with the change in divorce rates among a statistically balanced group of unemployed individuals who was not affected by the reform. Difference-in-differences estimates suggest that the reform caused a 2.8 percentage point increase in divorce (a 25% increase). Effects were concentrated among low-income couples (+58%) and couples with an unemployed husband (+32%) though gender differences are attributable to men's breadwinner status. Female main breadwinners were more strongly affected (+78%) than male main breadwinners (+40%). Results confirm the 'family stress model' which posits that job search and financial stress cause marital conflict. Policymakers should consider a broad array of impacts, including divorce, when considering reductions in unemployment insurance generosity.

Keywords

Unemployment insurance, divorce, income, unemployment benefits, family stress, quasi-experiment

Introduction

Unemployment insurance was designed to alleviate the hardship of unemployment. Until the recent COVID-19 pandemic there was a trend towards

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reducing unemployment insurance generosity across the OECD (Biegert, 2017; Gschwind, 2021; OECD, 2018). This was partly premised on findings that generous unemployment benefits extend unemployment duration (Mortensen, 1976; for an overview see: Schmieder and Von Wachter, 2016).

In contrast, the literature on the effects of unemployment benefits has paid little attention to outcomes such as health (Kuka, 2020; Shahidi and Parnia, 2021) and subjective wellbeing (Carr and Chung, 2014; Rose, 2019; Wulfgramm, 2014). With respect to families, there is just one study examining divorce and fertility (Lindo et al., 2020). From the comparative social policy literature we know that other welfare state programmes impact family stability (Bitler et al., 2004; Halla et al., 2016; Van Winkle, 2020), suggesting a likely, though insufficiently explored, relationship between unemployment insurance and divorce.

This article takes an important step in filling this gap in our knowledge, using full population administrative data from Switzerland and a quasi-experimental design to examine whether a reduction in maximum unemployment insurance benefit duration impacted the likelihood of divorce. We exploit a Swiss policy reform which *reduced* maximum benefit duration from 1.5 years to one year. The reform increased pressure to find a job and shortened unemployment duration for individuals shortly before or after exhausting one year of benefits and reduced benefit income for those exhausting one year of benefits. We measured the effect of the reform on divorce by comparing the pre to post reform change in divorce rates among unemployed individuals affected by the reform (treated) with the change in divorce rates among a statistically balanced group of unemployed individuals who were not affected by the reform (controls).

Our study offers key improvements over the literature on the positive effects of unemployment insurance and the literature on social policy and family stability. First, Lindo et al. (2020), the only other study on the effects of unemployment benefits on divorce, examine the effect of changes in unemployment benefit generosity on divorce in the US, but compare changes in divorce among the unemployed to those without unemployment spells, a comparison that could be biased given different trends in family formation and dissolution by social strata (Cherlin, 2020). Second, a number of studies from the social policy literature examine policy changes that include simultaneous variations on multiple policy dimensions (for example, generosity and activation requirements) (Bitler et al., 2004; Halla et al., 2016; Van Winkle, 2020). For these studies, it is difficult to attribute effects to specific policy changes. Third, some studies use relatively small experiments (Goñalons-Pons and Calnitsky, 2021), finding increases in discord due to cash transfers but not divorce. This could simply be due to the small sample, as divorce is a rare event. In studies using survey data, sample size problems are exaggerated due to panel attrition (Vandecasteele and Debels, 2007).

The study improves on each of these shortcomings. First, both our treated group and control group include unemployed individuals, which allows for more reliable comparisons of changes in divorce rates than comparisons with continuously employed individuals. Second, our design examines a policy change in maximum benefit duration allowing us to isolate effects of benefit generosity. Third, we use full population linked register data that is not prone to panel attrition, allowing us to detect even small changes in divorce and examine heterogeneous effects by income group and gender. Given Switzerland's high levels of average wealth (Kuhn, 2020) and generous social assistance (Obinger, 1999), these results can be seen as a 'least likely' case, meaning that if unemployment benefit generosity affects divorce in this context, it is likely relevant in any context.

Income shocks and divorce

The literature on how income shocks, such as those caused by the reform, affect divorce can be broadly summarized into two groups: a) findings that are interpreted in support of the *family stress model* and b) findings that are interpreted in support of the *cost and independence hypotheses*.

The family stress model

First, there is the argument that economic stability contributes to marital stability. This is often

summarized as the *family stress model* and linked to the work of Rand Conger and Glen Elder (Conger et al., 1990, 1994, 2010). The central argument is that a lack of economic resources induces a chain of negative events for couples. With an income loss, households might reduce expenditures, have difficulties paying bills, or be unable to meet consumption needs. This economic stress then translates into psychological stress (Inanc, 2018) through conflicts related to finances (Goñalons-Pons and Calnitsky, 2021) eventually leading to low relationship satisfaction, and ultimately divorce (Blom et al., 2019a, 2019b; Conger et al., 2010; Di Nallo et al., 2022).

A gender-focused extension of this argument is that conflict increases not only in response to income loss but depending on *whose* income has been lost. Stress and conflict might be higher when gendered expectations are not met. An income loss of the male partner indicates a failure to follow the male breadwinner norm (Cherlin, 2020; Townsend, 2010; West and Zimmerman, 1987), whereas an income loss of the female partner is not at odds with prevalent norms. Thus male income loss, more than female income loss, translates into increased conflict and marital instability (Bertrand et al., 2015; Blom et al., 2019b; Goñalons-Pons and Gangl, 2021; Inanc, 2018; Killewald, 2016; Kim and Luke, 2020).

The family-stress model and its gender-focused extension are supported by some empirical evidence. Goñalons-Pons and Calnitsky (2021) show that cash transfers in the Manitoba Negative Income tax experiments reduced money-related conflict in couples. Several longitudinal studies have shown that reductions in men's employment incomes lower relationship satisfaction and increase divorce (Bertrand et al., 2015; Blom et al., 2019b; Di Nallo et al., 2022; Killewald, 2016), while women's incomes are less clearly related to divorce (Killewald, 2016; Özcan and Breen, 2012). This association is more pronounced in contexts with stronger gender norms (Goñalons-Pons and Gangl, 2021). Boertien (2012) finds that income from lottery wins increases relationship satisfaction and reduces the chances of separation, but only if the male partner won. Similarly, Lindo et al. (2020) showed that divorce rates of unemployed men decrease with increased unemployment benefit generosity, but not those of unemployed women.

The cost/independence hypothesis

A second set of arguments can be summarized as the *cost/independence hypothesis* which stresses the role of economic resources in a) helping couples bear the economic cost of separation and in b) fostering economic independence from a partner.

The high cost of divorce is one potential reason why a negative income shock might depress divorce. Divorce is costly as the dissolution of the household into two new households leads to the loss of economies of scale. Furthermore, divorce comes with considerable direct costs such as legal fees (Boertien and Lersch, 2021). A decrease in benefit income might thus lead couples who were considering divorce to delay it.

A second reason a negative income shock might depress divorce is explained by the economic independence hypothesis (Becker et al., 1977; Özcan and Breen, 2012; Sayer and Bianchi, 2000). The key argument is that marriage is attractive for those with limited individual resources as they can depend on the income support of a spouse. With no employment income, a decrease in benefit income could decrease the unemployed individuals' economic independence and reduce divorce (Bitler et al., 2004; Halla et al., 2016). However, it must be noted that, when seen from the perspective of a spouse who depends on the unemployed spouse, a decrease in benefits could also *increase* divorce as the attractiveness of marriage is reduced.

Several experimental and quasi-experimental studies are interpreted as supporting the cost and independence arguments. Analysing the impact of the US welfare reform in the 1990s, Bitler et al. (2004) concluded that the introduction of policies restricting access to benefits and promoting work (work requirements, sanctions, time limits, and lowered income tax rates while on welfare) reduced the risk of divorce in states adopting these policies. Groeneveld et al. (1980) showed that income transfers in the Seattle–Denver Negative Income Tax experiments led to an increase in the divorce rate. Bobonis (2011) analysed the conditional cash

transfer programme in Mexico (Progresa) and found small increases in divorce rates related to more transfer income for women. Francesconi et al. (2009) showed that working tax credits in the UK (in-work benefits) significantly increased divorce rates in couples with low-earning men. Finally, using an instrumental variable design, Halla et al. (2016) have shown that in OECD countries, increases in national social spending are related to higher divorce rates.

Hence, the literature on how income shocks impact divorce does not lend uniform support to either the family stress model or the cost/independence hypotheses (see Table 1A (appendix) for an overview of the most relevant studies). One possible explanation for these inconsistent findings could be that both mechanisms operate simultaneously with one dominant for specific subgroups. For example, a higher ability to bear the cost of divorce after a positive income shock likely only translates into more divorce among couples who were already unhappy (Goñalons-Pons and Calnitsky, 2021).

Unemployment insurance in Switzerland and the 2011 reform

Unemployment benefit systems can be described by various characteristics, including eligibility requirements, income replacement levels, and maximum benefit duration (Schmieder and Von Wachter, 2016). In Switzerland, eligibility depends on reason for job loss, willingness to work, and paid unemployment insurance contributions. Benefits are paid for both involuntary and voluntary job loss, with a waiting period in the latter case. Recipients must write a minimum number of job applications per month and potentially participate in employment programmes, both determined by the recipient's caseworker. Furthermore, the recipients must have been employed in a job that paid social contributions into the Swiss unemployment benefit system for a minimum of 12 of the last 24 months.[1] The replacement level is stable throughout the unemployment spell at 70% of the previous salary for those without dependents and 80% for those with dependents or an income below 3797 CHF/mo.[2] Those with annual earnings below 6000 CHF are not covered and income is insured up to 148,200 CHF/yr. In terms of eligibility requirements and replacement levels, the Swiss unemployment benefit system is rather generous compared to other OECD countries.

In a reform implemented in April 2011, the maximum benefit duration was reduced for the subpopulation of unemployed aged 25–55 with incomplete contribution histories.[3] Prior to March 2009 benefit recipients with contributions of 12–17 months in the 24 months before claiming benefits were entitled to a maximum of 400 daily allowances (~1.5 years of benefit receipt).[4] Following the reform, in April 2011, the same group was entitled to a maximum of only 260 daily allowances (~1 year of benefit receipt) (SECO, 2013). This means that the reform reduced maximum unemployment benefit entitlement by 6 months for prime age workers with incomplete contributions. Unemployed individuals in the same age group with longer contributions were entitled to 400 days of benefits (~1.5 years of benefit receipt), both before and after the reform.

The expected effect of the reform on divorce

The reform reduced benefit income for those exhausting one year of benefits and increased pressure to find a job even earlier than that. The family stress model predicts that these changes should lead to more marital conflict and, ultimately, divorce. Arguments on economic dependence also anticipate more divorce when taking the perspective of the spouse of the unemployed. Due to benefit cuts, previously dependent spouses can depend less on their unemployed partner. This reduces the attractiveness of the marriage relative to finding a new partner.

Hypothesis 1a: *The reform increased divorce.*

From the viewpoint of the economic dependence of the unemployed spouse, however, the reform should increase economic dependence, as he or she must rely on their spouse after their benefits are cut, reducing divorce. A reduction in divorce is also anticipated by the cost hypothesis as a reduction in benefit income makes it more difficult for the couple to afford a divorce.

Hypothesis 1b: *The reform reduced divorce.*

The literature also suggests testable hypotheses about how these mechanisms play out among certain subgroups. The family stress mechanism should be more pronounced for couples with lower household incomes who already suffer economic stress.

Hypothesis 2a: *The reform increased divorce more for couples with a low household income before unemployment.*

The cost hypothesis also suggests that couples with fewer economic resources were more strongly affected by the reform. A low-income household is more likely to drop below the threshold at which they can shoulder the cost of divorce and maintain two separate households. Thus, if the cost hypothesis holds, it should also be stronger for low-income couples.

Hypothesis 2b: *The reform decreased divorce more for couples with a low household income before unemployment.*

We also expect the reform's effect to differ according to the unemployed spouses' gender. On the one hand, there are arguments supporting the expectation *that increases in divorce caused by the reform are stronger for men than for women*. Gendered norms on breadwinning lead to differences in the relative income contributions to the household by unemployed men versus unemployed women. Unemployed men have more insured income, lose more when benefit eligibility is reduced, and can rely less on their partners' income – meaning more stress caused by the reform for men. In addition, seen from the perspective of a dependent spouse, the attractiveness of marriage is more strongly reduced by men's versus women's loss of benefits.

Beyond the fact that men contribute more income to the household, gender norms could exacerbate the stress caused by the reform. There is a strong cultural expectation that men contribute to the household. The stress associated with benefit loss is thus likely greater for unemployed men, *irrespective* of the amount of benefit they lose or how much their wives earn (Goñalons-Pons and Gangl, 2021; Killewald, 2016). This would mean that couples experience greater increases in conflict and divorce when husbands were affected by the reform, even after considering their greater income contribution to the household.

Hypothesis 3a: *The reform increased divorce more for couples in which the unemployed spouse was the husband, irrespective of men's greater relative income contribution.*

In contrast, the cost hypothesis would anticipate that the reform *suppresses* divorce more when the unemployed spouse is the *husband*, because of the greater income loss. From the viewpoint of cost, gender norms play no role, meaning that any gender effect should be explained by men's greater incomes.

Hypothesis 3b: *The reform decreased divorce more for couples in which the unemployed spouse was the husband.*

If changes in economic dependence are the dominant mechanism, however, the reform should suppress divorce more when the unemployed spouse is the *wife*. Given husbands' higher earnings, wives losing benefits become even more dependent. Their husbands, not relying on the unemployment benefits of their wives anyhow, likely won't react either way. In contrast, for an unemployed man losing benefits, their wives are less able to support them, implying that unemployed men do not become more dependent when they lose benefits. Their wives, however, as described earlier, are more likely to seek a new partner. Jointly these facts suggest that if the reform suppresses divorce, it should do so more for unemployed wives.

Hypothesis 3c: *The reform decreased divorce more for couples in which the unemployed spouse was the wife.*

Methods

Data

To test our hypotheses, we used linked full population register data from Switzerland. Civil status

register data provided us with information about marriages[5], births, deaths, and divorce from 2000 to 2018 (FSO, 2019).[6] Unemployment insurance register data (2000–2015) added information on the timing of unemployment spells, the characteristics of the unemployed (pre-unemployment income, working hours, education, occupation) and unemployment benefit receipt (contribution history, maximum number of benefits, sanctions, actual duration of benefit receipt) (Federal Council, 2006). Finally, social security data on individual accounts included earned employment income, self-employment, and social insurance income (unemployment benefits, maternity, wage replacement during mandatory military service) for both spouses from 2000 to 2016 (Central Compensation Office (CCO) 2018).

Study design

Measuring the effect of the reform by simply comparing divorce rates between those affected by the reform (*treated*, 12–17 of 24 months of contributions) versus those not affected by the reform (*controls*, 18–23 of 24 of months of contributions) would lead to biased estimates, as individuals with more interrupted employment histories have a higher likelihood of divorce.[7] Also, simply comparing the change in divorce rates of the treated before the reform to the treated after the reform would lead to biased estimates of the reform's effect, as the likelihood of divorce changes across cohorts, for example due to changing norms.

Given that the timing of the reform was unrelated to divorce (see Footnote 4), however, it seems plausible that the *pre-to-post-reform change* in divorce rates among the treated would have been identical to the change in divorce rates among the controls, *had the reform not taken place*. If this holds, any observed difference in the change in divorce rates between the two groups reflects the causal effect of the reform. We measure differences in the change using a *repeated cross-sectional difference-in-differences* research design, which, due to the random timing of the reform, can be called quasi-experimental. Figure 1 (upper panel) displays the definitions of treated and controls in the pre- and post-reform cohorts, based on contributions and corresponding maximum benefit durations. Due to the design of the reform, both treated and controls were limited to individuals aged 25 to 53 whose unemployment spells lasted at least nine months.[8]

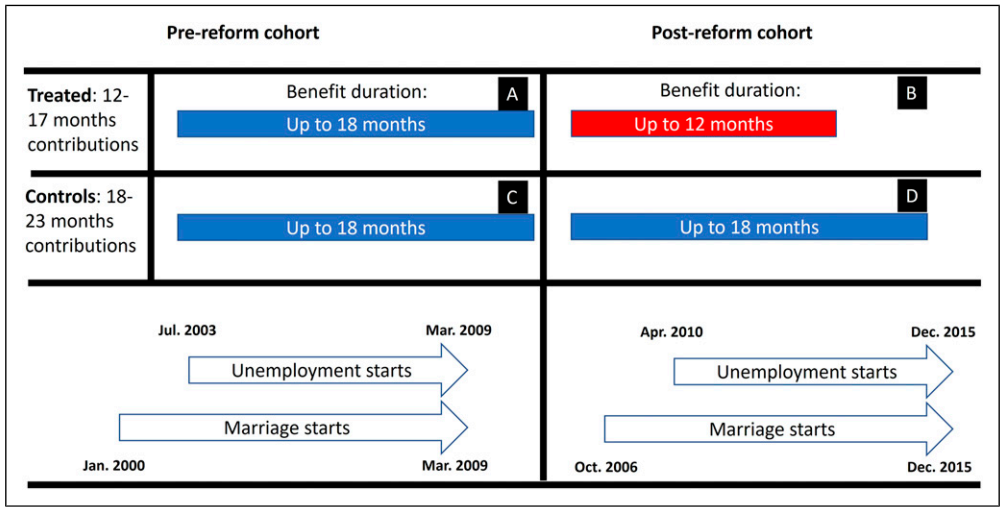

Figure 1. Study design and sample selection. Selection of treated and controls, benefit duration among treated and controls in the pre- and the post-reform period (upper panel), and selected unemployment spells and marriages (lower panel). The effect of the reform is measured with (B-A) – (D-C).

Adverse selection. A potential threat to the validity of the research design is adverse selection, that is, the possibility that following the reform more capable individuals postponed quits and layoffs until they had 18 months of contributions. Such strategic behaviour would create differences in divorce rates due not to the causal effect of shorter benefit eligibility, but due to compositional shifts between treated and controls.

To test for strategic behaviour, we plotted the share of individuals with a tertiary degree among all individuals registering for unemployment with at least 18 contribution months for all monthly cohorts between January 2009 and December 2011 (see Figure 1A (appendix)). If there was strategic behaviour among more capable individuals, then we would have seen a sudden increase in the share of individuals with a tertiary degree after the reform was accepted in the popular vote in September 2010 or after it was introduced in April 2011. The data does not show a sudden increase, but rather the continuation of a steady trend in increased tertiary degrees. There was similarly no sudden increase using alternative measures of selection such as earnings in the last job before unemployment (not reported).

Sample

We restricted observations to those individuals whose marriage took place prior to or in the first year of unemployment and remained married throughout the first year of unemployment. Among couples with multiple unemployment spells after marriage, we select the first spell (taking a random spell if both spouses registered for unemployment on the same day).

For the pre-reform sample, we selected all individuals married between January 2000 (the earliest date that we could measure unemployment after marriage) and March 2009 (the last date unemployment spells started that were not affected by the reform). Among these, we selected those for whom the first unemployment spell after marriage started between July 2003 (the first date unemployment spells started in the pre-reform regime)[9] and March 2009 (the last date unemployment spells started that were not affected by the reform, see Figure 1, lower panel). For the post-reform samples, we selected all individuals married between October 2006 (3 years and 6 months between the first marriages and the first unemployment spells, as for the pre-reform sample) and December 2015 (6 years and 3 months between first and last marriages, as for the pre-reform sample) and whose unemployment spells started after April 2010 (the first date unemployment spells started in the post-reform regime). With this definition, there were 21,269 couples. After removing the 5.2% with a missing value on at least one of the study variables, the sample was 20,161. (Due to the small amount of missing information, we simply dropped these observations). We ended up with 4456 treated and 6331 controls before the reform and 3124 treated and 6250 controls after the reform.

Table 1 presents the descriptive statistics for the four groups. By definition the treated group had been employed for fewer months in the 2 years prior to unemployment and had lower levels of education and occupational status. The last column of Table 1 presents the coefficient of the respective characteristics regressed on an interaction term between the treatment and period (pre-reform vs post-reform) variables with statistically significant effects of this DiD estimator for several variables, suggesting differences between treated and controls that changed following the reform. We adjust for these differences by including all variables in Table 1 as control variables in our multivariate models and by balancing the four comparison groups to match the characteristics of the treated post-reform (see the section on Analytical strategy).

Variables

The outcome variable is whether the couple was legally divorced. Using this measure, the effects of the reform will have a substantial lag. Using data from the Swiss Families and Generations Survey (Federal Statistical Office, 2021) we find an average lag of one year and 9 months between the year of self-reported separation and the year of divorce in the general population in our study period. Given that unemployment is associated with even longer time lags between separation and divorce (Tumin and Qian, 2017), this can be seen as a lower bound for our sample. Second, the

Table 1. Characteristics of treated and controls and trends from before to after reform.

	Treated		Controls		DiD
	Pre	Post	Pre	Post	
UI contribution months	14.01	14.18	21.58	21.48	0.27***
Education: Tertiary	0.23	0.28	0.24	0.32	−0.03*
Upper-secondary	0.45	0.35	0.52	0.41	0
Less than vocational	0.32	0.37	0.24	0.26	0.03*
Occupation: Managerial/professional	0.16	0.15	0.21	0.24	−0.04***
Clerical/Services/Sales/Skilled agricult	0.48	0.43	0.53	0.48	−0.01
Craft/Plant and machine operators/Elementary	0.35	0.42	0.26	0.27	0.05***
Citizenship: Swiss	0.29	0.2	0.47	0.39	−0.01
Foreign: Neighbouring countries	0.11	0.13	0.15	0.17	−0.01
Foreign: Non-neighbouring countries	0.6	0.67	0.38	0.44	0.02
Age at marriage	32.19	31.66	32.94	32.52	−0.11
Partner age at marriage	34.42	33.55	33.59	33.51	−0.79**
First marriage	0.73	0.77	0.72	0.77	0
Age at unempl. Start	33.98	33.81	35.05	34.98	−0.1
Employed y. Before unempl	0.96	0.96	0.97	0.97	0
Income y. Before unempl. (CHF 1000/yr.)	35.43	31.08	53.66	57	−7.69***
Working hours before unempl. (100 = 42 hrs./wk.)	89.61	86.5	88.91	88.47	−2.67***
Desired working hours (100 = 42 hrs./wk.)	92.33	92.89	88.47	89.48	−0.44
Sanctions	4.94	5.33	5.87	6.81	−0.56*
Waiting days	3.35	3.11	3.75	3.97	−0.45***
Partner employment y. Before unempl	0.79	0.8	0.81	0.8	0.02
Partner income y. Before unempl. (CHF 1000/yr.)	47.65	48.66	53.71	59.08	−4.36**
Number of children: < Age 4	0.4	0.42	0.5	0.55	−0.03
Age 4–6	0.12	0.13	0.14	0.16	−0.01
Age 7+	0.17	0.13	0.23	0.18	0
Hh. Income year. Before unempl. (CHF 1000/yr.)	83.08	79.74	107.37	116.08	−12.05***
Region: Lake geneva	0.3	0.31	0.29	0.33	−0.03*
Central lowlands	0.21	0.21	0.19	0.17	0.02
Northwest	0.1	0.1	0.11	0.12	−0.01
Zurich	0.19	0.19	0.2	0.18	0.01
East	0.08	0.09	0.09	0.1	0.01
Central	0.06	0.05	0.06	0.05	0
Ticino	0.05	0.05	0.05	0.05	0
N (couples)	4456	3124	6331	6250	—

Treated: 12–17 months contributions in the 24 months before start of the UI spell. Controls: 18–23 months contributions. Pre observations: Married between Jan 2000 and Mar 2009, start of unemployment between Jul 2003 and Mar 2009. Post: Married between Sep 2006 and Dec 2015, start of unemployment between Apr 2010 and Dec 2015. DiD = interaction term between treated versus controls and pre- versus post-reform. *p*-value thresholds DiD: * = 5%, ** = 1%, *** = 0.1%

divorce process itself (that is, the time between the moment couples file for divorce and the moment the divorce is legally granted) can take a long time. Analysing divorce register data, for divorces granted in 2008, there was a 9-month lag between filing for divorce and the divorce being legally granted.[10] Together, the total lag between separation and the legal enactment of divorce is

between 2 and 3 years. The effects of the reform would thus be expected in years 3 and 4 after the year the unemployment spell began. Given this time lag, we measure cumulative divorce up to 4 years after the start of unemployment.

We define household income as income from employment and social insurance from both partners in the calendar year before unemployment. In the presented analysis, we examine income terciles, but smaller categories (quartiles and quintiles) were also tested.[11]

We measure the relative income contribution of the unemployed spouse using the share of the total household income in the calendar year before unemployment. To estimate simultaneous differences by gender and relative income contribution (Hypothesis 3a), we used a *binary* indicator distinguishing between 'primary earners' (relative income contribution greater than 50%) versus 'secondary/non-earners' (50% or less).

Analytical strategy. To capture the effects of the reform on divorce, we use linear probability models (LPM) on the stock of divorced couples in each year after unemployment start with the dependent variable of whether a couple has had a divorce yet. Compared to event-history analysis of person-year data, our approach is less prone to statistical noise (present in annual separation rates). Given that divorce is a relatively rare event impacting about 10% of the couples in our sample, we also compare results using a logit model (presented in the Online appendix).

We measure the effect of the reform with an interaction term between a dummy variable for whether the unemployment spell took place in the pre-reform or post-reform period and a dummy variable for the unemployed spouse belonging to the treated or the control group. Our model is:

$$Divorced = \beta_0 + \beta_T \cdot X_T + \beta_P \cdot X_P + \beta_{DiD} \cdot X_T \cdot X_P$$

β_0 denotes the intercept, β_T the coefficient that estimates the effect of being in the treated versus control group in the pre-reform period and β_{Post} the coefficient that estimates the effect of the change from the pre- to the post-reform period in the control group. β_{DiD} expresses the difference between treated and control group in the change from the pre- to the post-reform period – the difference-in-differences estimate of the effect of the reform. To remove covariate imbalance, we use entropy balancing (see Table 2A (appendix)). Entropy balancing generates weights that adjust means in all comparison groups to those in the treated group post-reform. Compared to adjustment using linear control variables, entropy balancing reduces the dependence of treatment effect estimates on model specification (Hainmueller, 2012). We report results using entropy balancing in the main article and results using regression in the Online appendix. To further strengthen conclusions on the causal effect of the reform, we consider the trends in divorce among treated and controls before the reform: if divorce trends in the two groups were parallel before the reform, then a divergence in divorce trends between the two groups just when the reform happened suggests that this is due to the causal effect of the reform.

Results

Overall effect of the reform

Figure 2 displays the share of couples who were divorced in the years following an unemployment spell in the four groups (treated/control, pre/post-reform). The lower panel includes difference-in-differences estimates and 95% confidence intervals.

For all four groups, the share of couples who are divorced increases relatively steeply in the years after an unemployment spell. Also, there is a clear increase in divorce rates between the two periods, illustrating an increasing trend in divorce in Switzerland over this period. What is important is that divorce rates before the reform are identical for treated and control couples (gray lines) while after the reform divorce rates for the treated accelerate. The treated group's relatively higher divorce rate increases steadily in the years after unemployment starts, only reaching statistical significance in the fourth year. This confirms Hypothesis 1a, showing an increase in divorce due to the reform.

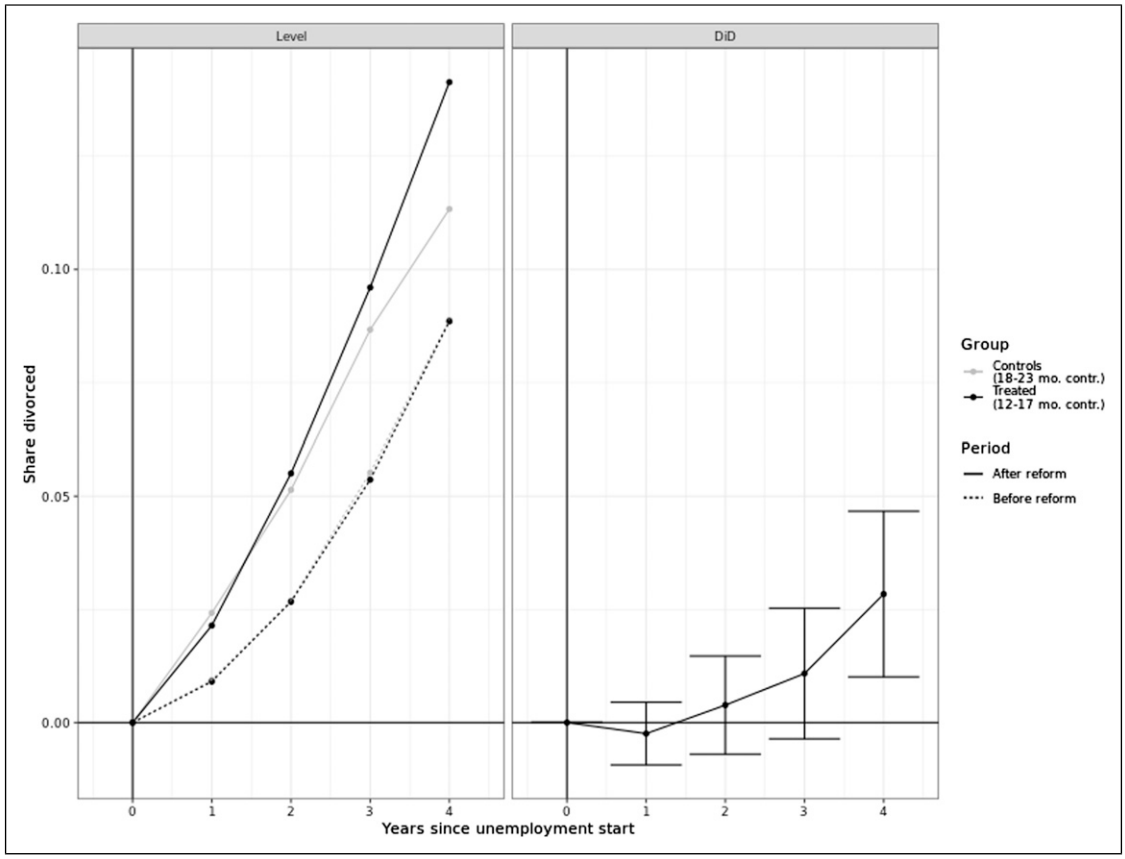

Figure 2. Effect of the reduction of unemployment benefit duration on divorce. Levels in treated/controls and pre/post-reform and cumulative difference-in-differences of the share of couples divorced by year after unemployment start (95% confidence intervals). Results based on entropy-balanced samples.

The estimated effect of the reform is a 2.8 percentage point increase in the share of couples who divorce (Figure 1, bottom panel). Relative to the counterfactual level of divorce in the treated group (the observed post-reform level of divorce minus the treatment effect = 14.1–2.8 percentage points = 11.3 percentage points) this is a 25% increase in divorce due to the reform.[12]

It is possible that the greater difference in divorce among the treated after the reform is not due to the causal effect of the reform, but due to diverging trends in divorce between the two groups that already existed before the reform. To test this, Figure 3 shows trends in divorce among the treated and the controls before the reform. Before the reform, trends in divorce were parallel between treated and controls (with covariates unbalanced, left hand panel) or even steeper among the controls (with covariates balanced, right hand panel). Hence, extrapolating this trend to the post-reform period without the reform's effect, one would expect a parallel increase in divorce between the two groups (resp. A stronger increase among the controls). Instead, we find stronger increases in divorce among the treated just after the reform (cohort 2010–2012), in line with the expectation that the difference between treated and controls in the post-reform sample is due to the reform.

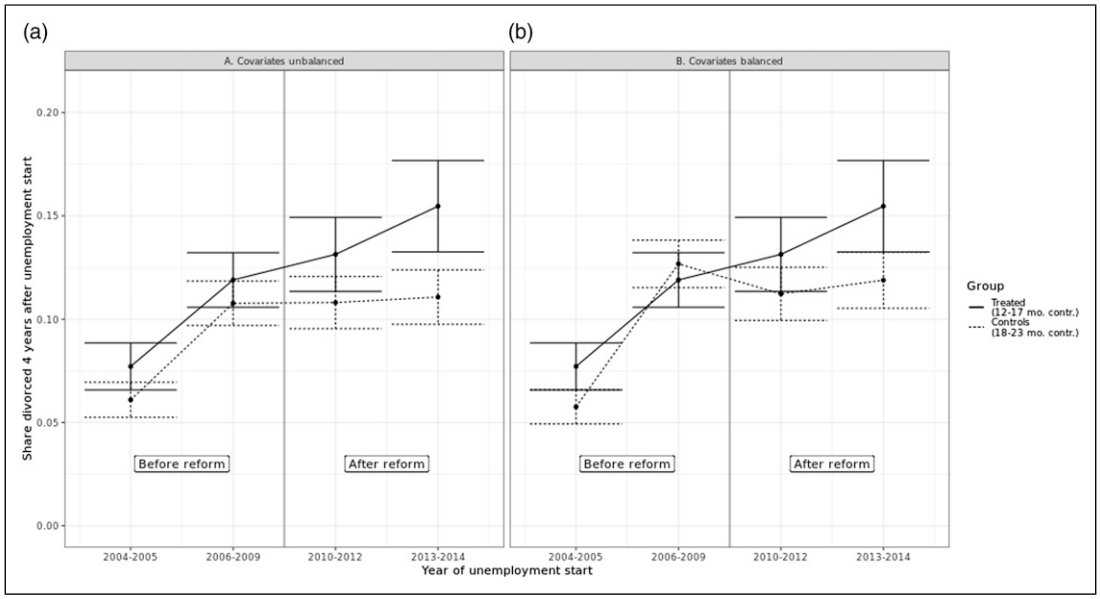

Figure 3. Trends in the level of divorce before and after reform, divorce stock measured in the fourth year after unemployment start, balanced and unbalanced control groups, 95% confidence intervals.

From a policy perspective, it is important to consider the effect of the reform on divorce (+25%) in the context of other reform effects including losses in benefits and increases in employment and income. Those reaching at least 9 months of unemployment received CHF 9770 less in benefits, which is a 23% loss (concentrated in the second year of unemployment, −55%). Benefit loss was partially compensated by faster reemployment (about 3 weeks, −4.3% in unemployment duration) and higher earnings (about CHF 2,880, +11.3%, see Table 2A (appendix)).

Heterogenous effects of the reform

In addition to main effects, we hypothesized differential effects across subgroups. Figure 4 presents the results from the stratified analyses by terciles of household income, gender and gender combined with relative income contribution.

Confirming Hypothesis 2a, we find a greater increase in divorce due to the reform for couples in the bottom income tercile (Figure 4, upper panel), with reform effects, and the difference in reform effects by income, significant only in the fourth year following unemployment. The divorce rate among couples in the lowest tercile increased by 5.8 percentage points (a 56% higher divorce rate than in the absence of the reform). In the middle tercile there is an increase in divorce of 2.8 percentage points that is not statistically significant, while the reform has no impact on divorce rates among the upper tercile.

Hypotheses 3a-c considered gender-specific effects. We do not find support for the gendered expectations of any of these hypotheses (Figure 4, middle panel). In the fourth year after unemployment, the reform had a statistically significant impact on both men's and women's divorce rates. While absolute effects for men (3.7 percentage points) are somewhat stronger than for women (2.8 percentage points), they are more similar when expressed relative to counterfactual levels of divorce (32% for men, 28% for women).

Considering the results for gender and relative income contribution simultaneously suggests that stronger effects for unemployed men are entirely due to their greater contribution to household income (Figure 4, bottom panel). The reform impacted

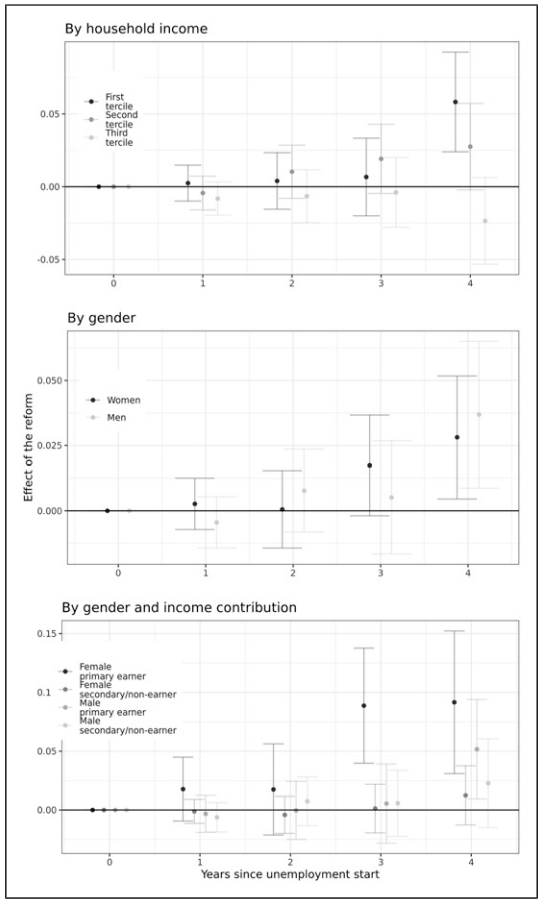

Figure 4. Difference-in-differences estimates of the effect of the reduction of unemployment benefit duration on cumulative divorce after unemployment start by household income, gender and gender combined with relative income contribution, 95% confidence intervals.

divorce only among breadwinners with no statistically significant effects for unemployed individuals contributing less than half of the couples' income before unemployment. It increased divorce among unemployed female breadwinners in the third year after unemployment start by + 8.9 percentage points which is a 117% increase; in the fourth year, it increased divorce by + 9.2 percentage points, a 78% increase. In contrast, for male breadwinners the reform's effect is only statistically significant in the fourth year, increasing divorce by + 5.2 percentage points, or 40%.

Conclusion

Although there is literature showing unemployment insurance generosity impacts health and that unemployment causes divorce (e.g. Di Nallo et al., 2022; Kuka, 2020), there is only one study examining unemployment insurance and divorce (Lindo et al., 2020). Further, from the social policy literature, we know that social programme generosity is associated with family stability, but these studies have not examined how unemployment insurance, the most important social safety net for the working age population, affects divorce. Using full-population administrative data and a policy change that reduced maximum benefit duration in Switzerland, in this study we provide new evidence for how unemployment insurance generosity impacts divorce.

We found that the reform, reducing entitlement by 6 months, was associated with a 2.8 percentage point increase in the share of couples who divorce, which is a 25% relative increase. As could be expected from the lag between marital conflict, household separation and formal divorce, we found statistically significant effects only 4 years after unemployment start. The reform's effect on divorce was mediated by income and employment effects, with the reform leading to a 23% drop in total benefit income, a small reduction in unemployment duration (−4.3%) and an increase in earned income (+11.3%) for those unemployed for more than nine months.

The increase in divorce was largely concentrated among poor households, with upper income households unaffected. Unlike Lindo et al. (2020), we also found substantial effects of benefit generosity on divorce rates of unemployed women. Slightly higher average effects for men are explained by men's higher relative contribution to household income before job loss. Among primary earners, the reduction in benefits had a stronger impact on divorce for unemployed women.

These results largely lend support for a gender-neutral version of the family stress model in which the stress caused by financial difficulties and increased pressure to find a job leads to marital conflict and divorce. Taking the viewpoint of the unemployed spouse, these findings could also be related to economic dependence: due to benefit loss, the spouse

can depend less on the financial support of their unemployed partner, reducing the attractiveness of this marriage.

In contradiction to interpretations put forward in many studies on the effect of income shocks on divorce (Bertrand et al., 2015; Goñalons-Pons and Gangl, 2021; Killewald, 2016), our results do not support the notion that the male breadwinner norm (which is still dominant in Switzerland) alters the interpretation of income loss. Rather, the high effects among (female and male) primary earners suggest that stress is greater when the income loss is more financially significant for a couple. It is possible earlier studies finding stronger effects for male income loss did not sufficiently account for the household's dependency on lost income. In fact, our results combining gender and relative income contribution suggest that women's income loss is more important than men's.

Results cannot, however, conclusively reject the cost/independence hypotheses. It is possible that unhappy couples or unemployed individuals who were considering divorce might have postponed it in the face of reduced benefits. Since our data did not allow us to identify dissatisfied couples (as compared to Goñalons-Pons and Calnitsky, 2021), we can only conclude that this was not the dominant mechanism in our overall sample, nor among the subgroups we explored.

The study also has several limitations. In principle, our results could reflect differences in divorce trends between more (treated) and less (controls) disadvantaged groups that existed independently of the reform. Or they could express adverse selection, with more able, and determined (and less divorce-prone) individuals self-selecting into the control group. However, our robustness checks reject these assumptions in favour of a causal interpretation of our findings: the higher divorce rate among treated versus controls emerges just after the reform, which could not be predicted looking at how divorce evolved among the two groups before the reform. Also, we find no evidence that individuals strategically adapted to the new eligibility rules after these became known to the public.

In addition, our results might have limited generalizability. We only studied long-term unemployed individuals and only a single dimension of benefit generosity, that is, benefit duration. Results might not be generalizable to the overall population receiving unemployment benefits, as most unemployment spells are short or might not apply to different changes in unemployment insurance, like a reduction in the replacement rate. More knowledge is needed on how other changes to unemployment benefits impact divorce (Lindo et al., 2020). Also, it might seem that the Swiss context might be very specific and not generalizable to other countries. That said, given high savings rates and wealth, as well as generous social assistance, it seems likely that Switzerland is a 'least-likely' case, that is, if unemployment insurance is key for marital stability in Switzerland, it is likely even more so in other countries. Still, additional studies from other countries are necessary to confirm the results.

Assuming these estimates reflect the causal effect of shortened maximum benefit duration on divorce, our findings have important policy implications. When deciding on shorter benefits, policymakers must not only consider the immediate savings from fewer benefits being paid out and small positive re-employment effects, but also potential impacts on other outcomes like divorce. Social insurance generosity has a broader impact than is often considered when policymakers cut programmes.

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Data availability statement

Due to Swiss data protection laws, authors are not allowed to share research data directly. Data can, however, be requested from the Swiss Federal Statistical Office (https://www.bfs.admin.ch/bfs/en/home/services/data-linkages/for-third-parties.html). Code for data preparation and analysis is available from authors upon request.

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Supplemental Material

Supplemental material for this article is available online.

Notes

1. Workers are exempted from contribution requirements if they recently finished education, gave birth to a child, were sick, had a divorce or separation, or finished a prison sentence.
2. Figure refers to the level in 2021.
3. The reform was motivated by budget shortfalls following an underestimate of the anticipated unemployment rate from 2004 to 2010 (Devaud and Keller, 2012).
4. Starting in July 2003, when a reform last changed benefit generosity.
5. The observation period of the study (2000–2018) refers to the period before the introduction of same-sex marriage in Switzerland (July 2022). Therefore, same-sex marriages could not be considered in the analysis even if the theoretical arguments apply equally to opposite-sex and same-sex marriages.
6. Data were linked with pseudonymized social security numbers. Social security numbers of individuals who married/divorced before 2010 were missing in the original data. Therefore, we identified them via matching with 2010 population registers using unique combinations of marriage and birth dates, leading to a loss of observations of individuals who were no longer resident in Switzerland in 2010. We do not see this data limitation as a threat to our study design since it affected all our study groups (see Study design) alike.
7. We excluded the unemployed who had paid 24 months of contributions to restrict the control sample to unemployed with disrupted employment histories, that is, to select controls that are more comparable to the treated, but were still not affected by the reform.
8. Due to the design of the reform (reducing benefits from 1.5 to one year), only those who were at the least approaching one year of benefits were affected by it. We assumed that anticipation of benefit exhaustion after 12 months already affects individuals after 9 months. We therefore restricted the sample to those with at least 9 months of unemployment duration.
9. There were different rules to benefit eligibility before July 2003.
10. Information on both divorce filing and the date the divorce was granted was last available in 2008 as the Federal Statistical Office stopped collecting this variable to reduce the administrative burden of the courts.
11. Overall conclusions remain unaffected using quartiles versus terciles.
12. Alternative estimates using logistic regression (see Figure 2A), illustrating effects as average marginal effects have similar results. Covariate adjustment with linear controls (versus entropy balancing) also leads to similar but less pronounced effects (statistically significant at the 10% level in the third year after unemployment).

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