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Original Article

Cohort changes in the association between parental divorce and children's education: A long-term perspective on the institutionalization hypothesis

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The institutionalization hypothesis argues that in contexts where divorce is more common, its consequences will be less severe. An implication of this hypothesis is that the association between parental divorce and child outcomes will decline over time, parallel to the historical rise in divorce. Building on a handful of earlier tests of this idea, the current analysis provides a long-term cohort perspective with sufficient statistical power to detect possible trends. Data from 18 national surveys in the Netherlands were harmonized and pooled in order to obtain a large sample with sufficient numbers of children with divorced parents from a wide span of birth cohorts (N_{total} = 87,541, $N_{divorced}$ = 5,728). Using educational attainment as a dependent variable, and applying a set of relevant controls for key family background variables, there was no evidence that the association between parental divorce and education changed between 1930 and 1991. Multi-level models showed that there was no association between the prevalence of divorce and the magnitude of the parental divorce effect. The refutation of the institutionalization hypothesis for divorce is interpreted in terms of how the selection into divorce has changed, in combination with problems emerging in modern postdivorce relationships.

Introduction

It has long been known that there is a negative association between parental divorce and children's educational attainment (Härkönen, Bernardi and Boertien, 2017; Raley and Sweeney, 2020; De Leeuw, 2021). This association is not necessarily causal, although rigorous panel studies dealing with selection found adverse effects on children's school achievement as well (Kim, 2011; Amato and Anthony, 2014), suggesting that at least part of the association between divorce and educational attainment is causal. Although there is a large amount of research on 'divorce effects,' an important question that has occupied sociologists, demographers, and psychologists, is how divorce effects on child outcomes have changed over time.

Several theoretical arguments would suggest that the association has become weaker over time. The so-called institutionalization hypothesis argues that the impact of a divorce on children and parents is less severe in a context where divorce is more common (Härkönen, 2014; Kalmijn, 2017). One reason is that divorce is less stigmatized in a context where it occurs more often, reducing normative disapproval and making it easier for parents and children to seek and find support. Another reason is that when divorce is common, people can rely on existing behavioural patterns to solve the various social and practical problems connected to divorce. Finally, policy arrangements and care systems will become more adapted to the problems a divorce poses when there is more experience with these problems, possibly leading to a reduced impact of divorce on parents and children. Applying these ideas to trends is plausible since parents' divorce and the cumulation of education take place in a well-defined and demarcated part of the life course, thus making (birth or divorce) cohorts a relevant social context.

In recent review articles, it has been argued that despite the plausible logic of the underlying arguments, there is remarkable stability in divorce effects on children's outcomes over time, in contrast to the institutionalization hypothesis (Härkönen, Bernardi and Boertien, 2017:177). The starting point of this article is the observation that the evidence on trends for children's education and associated well-being measures is

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less widespread and not yet as convincing as it needs to be for reaching this—indeed surprising—conclusion. After reviewing this earlier evidence in detail below, the current article develops a novel design to study trends by harmonizing and pooling 18 large nationally representative surveys from the Netherlands collected between 1988 and 2017 that contained retrospective information on parental divorce. The pooled data contain more than 85,000 adult respondents who were born between 1930 and 1991, of whom 5,728 had parents who divorced when they were growing up. The design allows me to compare 20 cohorts with sufficient numbers of observations in each cohort. Cohort trends are described in the association between parental divorce and children's education and a two-stage multilevel approach is used in which the individual effect of divorce in each cohort is regressed on measures of the prevalence of divorce (Bryan and Jenkins, 2016).

The context of the study is the Netherlands, which has a moderate divorce rate compared to the high divorce rates in Northern Europe, the UK, and the US. The increase in divorce occurred from the late 1960s to the late 1980s and has cycled around a new plateau since then. Most authors have confirmed the adverse effects of parental divorce on child outcomes in the Dutch context, although there is no evidence from panel data (Fischer, 2004; Oldehinkel *et al.*, 2008; Westerman and Gaalen, 2015; De Leeuw, 2021). Increases in divorce coincided with a substantial liberalization of marriage and family values since the first measurements in the mid-1970s (Kraaykamp, 2002; Halman and van Ingen, 2015) along with long-term declines in church membership, church attendance, and traditional religious beliefs (De Graaf and Te Grotenhuis, 2008). Given the mutual causal relationship between norms and behaviour, changes in the normative acceptance of divorce are generally considered both cause and consequence of the rise in divorce (Lesthaeghe, 2014). The Netherlands is not different in this sense than other Western and Northern European countries (Halman, Luijkx and Van Zundert, 2005; Halman and Draulans, 2006).

Background and previous research

The institutionalization hypothesis

The institutionalization hypothesis is usually based on two mechanisms. The first explanation lies in the notion of stigmatization. A divorce can be followed by normative disapproval from friends, family, and the larger social network. Disapproval may not only be targeted at the husband or the wife. Especially when norms against divorce and single parenthood are strong, the children may also be faced with disapproval (Kung, Hung and Chan, 2004). Normative responses from the environment may negatively affect mental health, including self-esteem (Mak *et al.*, 2007) and indirectly affect school outcomes by reducing well-being.

The second explanation argues that social and institutional support mechanisms become stronger when divorce becomes more common. In a sense, society has learned to deal with the problem of divorced families. Schools can be more attuned to the problem, healthcare systems can better recognize the problems that a divorce may cause, and the welfare state may support divorced parents more strongly in terms of financial and housing needs. Better support mechanisms are believed to benefit children's well-being and may directly or indirectly prevent children from experiencing setbacks in their schooling career.

An appealing aspect of the institutionalization hypothesis is that it can be applied to different social contexts, not only cohorts but also countries, schools, and social groups. In principle, the hypothesis can also be applied to other kinds of transitions in the life course, e.g. unmarried cohabitation (Soons and Kalmijn, 2009).

Past evidence

The institutionalization hypothesis has been tested first and foremost to the country context, a logical design given the enormous variation in divorce rates across the world. Although tests have mostly been done in highly developed countries, thus missing part of the context variation, they clearly show that the effect of divorce on a range of child and adult outcomes is not associated with the aggregate divorce rate. In general, there appears to be little evidence that divorce has a weaker impact on children and adults in countries where divorce is more prevalent and accepted (Pong, Dronkers and Hampden-Thompson, 2003; Dronkers and Harkonen, 2008; Kalmijn, 2010b; Albertini and Garriga, 2011; Verbakel, 2012; Bernardi and Radl, 2014; Bernardi and Boertien, 2017; Kreidl, Stipkova and Hubatkova, 2017; Guetto, Bernardi and Zanasi, 2022).

Authors in the field of social stratification have applied the hypothesis to schools. Schools vary considerably in how many children come from divorced families and one would expect that the effects of parental divorce on children's school outcomes are weaker when divorce is more common at school. The studies that have tested this found only a main effect of parental divorce at the school level but not an interaction effect (Pong, 1998; Cavanagh and Fomby, 2012; De Lange, Dronkers and Wolbers, 2014). Students perform worse in schools where many students come from divorced families, even after controlling for associated school disadvantages and the socioeconomic status of schools, but the gap between children of divorced and married families is not smaller in those schools.

Studies have examined racial and ethnic differences in light of the institutionalization hypothesis. Divorce and single parenthood are more common and more accepted in several ethnic groups, including African Americans in the US and people with Caribbean and Latin American origins in Europe. European authors have found that the effects of parental divorce on children's outcomes are significantly weaker in ethnic groups where the prevalence of divorce was lower (Kalmijn, 2010a, 2017; Erman and Harkonen, 2017). Both classic and recent studies in the US showed that Black children were less strongly affected by parental divorce than White children (McLanahan and Bumpass, 1988; Amato and Keith, 1991; Cross, 2020) although there is also contrasting evidence (Sun and Li, 2007). In sum, the institutionalization hypothesis has so far been refuted for countries and schools but is generally confirmed for ethnic groups.

Given the historical increase in divorce in most western countries, the hypothesis also applies to the time periods. If the divorce of parents takes place in youth, and the outcomes measured pertain to children's youth or early adulthood as well, cohorts refer to specific, well-demarcated periods of time in which children live and experience the possible effects of divorce. One of the first tests came from a British panel study that compared two birth cohorts, i.e. 1958 and 1970 (Sigle-Rushton, Hobcraft and Kiernan, 2005). The study found no decline in the negative effect of parental divorce on children's well-being outcomes and the attainment of academic/vocational qualifications. Using the same data, Ely *et al.* (1999) had earlier come to the same conclusion when using a continuous measure of schooling. Two Swedish studies analyzed two cross-sectional surveys with retrospective data covering two broad groups of birth cohorts and found no difference in the effect of parental divorce on (young) adult's psychological problems (Gähler and Garriga, 2013) and educational attainment (Gähler and Palmtag, 2015).

Although these pioneering studies are important, there were also clear data limitations. First, the statistical power to detect trends has been limited. A detailed look at the studies suggests that the numbers of children from divorced parents were often limited ($n \approx 300$ and 900), an issue that will especially affect the reliability of estimates in older cohorts where divorce was uncommon. Second, one ideally needs a long period of time to test the hypothesis with sufficient numbers of observations spread in time. The British data covered two 1-year cohorts 12 years apart. The Swedish data covered a long period but could only compare two broad groups of cohorts (1924–1965 vs. 1966–2007), with the early group containing fewer than 100 cases of divorce. In both designs, there is a risk that trends could not be detected very well. Some large-scale cohort studies in Europe have included over-time variation in comparative designs, but these combined trends and country differences, making it difficult to assess how the divorce penalty changed (Bernardi and Radl, 2014; Guetto, Bernardi and Zanasi, 2022). All in all, more research is needed to assess if the divorce penalty has been stable.

Interesting in this context is that for trends in other types of child outcomes, the evidence was not negative for the institutionalization hypothesis. In studying the effects of parental divorce on children's own risk of divorce, one American study pooled cross-sectional data from the *General Social Survey* and found a significant decline in parental divorce effects between 1973 and 1996 (Wolfinger, 1999). This study was later criticized on methodological grounds (Li and Wu, 2008) but in a recent study Wolfinger addressed the problems and again found a declining effect of parental divorce (Wolfinger, 2011). Wolfinger's conclusion is in line with a German study which also found declines in the effect of parental divorce on children's own divorce risk over time (Engelhardt, Trappe and Dronkers, 2002). Studies of the negative effects of divorce on parent–child contact after divorce have also found considerable improvement over time (Van Spijker, Kalmijn and Van Gaalen, 2022).

Alternative arguments

Some authors have offered alternative ideas about how divorce effects might change over time. One argument is that the selectivity of divorce has changed. In this reasoning, the association between parental divorce and children's education is to a large extent due to the individual and/or marital problems that parents have, rather than to a causal effect of divorce (Amato and Cheadle, 2008; Yu *et al.*, 2010; Baxter, Weston and Qu, 2011). If the threshold to divorce declines, the couples who divorce will be a less negatively selected group today than in the past. In line with this, there is empirical evidence that more serious divorce motives such as domestic violence and substance abuse were more common in the past while 'soft' motives, such as not feeling affection for the partner, have become dominant (De Graaf and Kalmijn, 2006a). Although the logic of the selectivity argument is different, the implication is the same: a declining association between parents' divorce and children's education.

Another argument lies in the relief or escape hypothesis. Authors have argued that a divorce may benefit children if the marriage of parents is ridden with conflict. In line with this, some studies have found an interaction effect of parental divorce and interparental conflict on child well-being. Specifically, divorce appears to have a more negative impact on the child under conditions of low parental discord (Hanson, 1999; Yu *et al.*, 2010). In high-conflict marriages, the negative impact of divorce is counteracted by the positive effect of not being exposed anymore to the parents' problems; in low-conflict marriages, there only is the negative impact of separation. Although evidence is not entirely consistent on this interaction effect (Kalmijn and Monden, 2006), the implication for changes over time is clear. If marriages that end in divorce currently are less often characterized by conflict, the impact of divorce on children would increase over time, in contrast to what the institutionalization suggests (Kreidl *et al.*, 2017).

A final issue lies in the role of parental education. Studies on the 20th century have shown that the influence of parental status on children's education was originally quite strong but declined significantly over time, a trend that is commonly interpreted as a shift from 'ascription to achievement' (De Graaf and Ganzeboom, 1993; Tieben, De Graaf and De Graaf, 2010; Tolsma and Wolbers, 2010). The gradual long-term shift from ascription to achievement may affect changes in the parental divorce effect. In the period studied, divorce was more common among highly educated parents but this pattern later changed, with divorce becoming more common among the lower educated (De Graaf and Kalmijn, 2006b; Härkönen, 2014; Matysiak, Styrc and Vignoli, 2014). In other words, in the early period, there may have been an 'advantage' for children of divorced parents, not only because they came from more highly educated parents but also because the impact of parents' higher education on their own education was strong. The declining effect of parental education on children's education and the changing educational gradient in divorce may thus have made the effect of parental divorce on children's education increasingly negative. This leads to what we can call the suppressor hypothesis: a decline in the adverse effect of divorce across cohorts, as predicted by the institutionalization hypothesis, may only become visible when taking into account the role of parental education.

The current study

This article tries to solve some of the problems that have complicated the test of the institutionalization hypothesis as it applies to trends. This study harmonized and pooled 18 large nationally representative surveys from the Netherlands that were collected between 1988 and 2017 and that contained retrospective information on parental divorce. There has been a long and vivid tradition of survey data collection in the Netherlands and in this tradition, there was much emphasis on combining themes of demography and social stratification. As a result, the surveys had many important similarities in design and measurement, making them well-suited for pooling. All surveys asked about parental divorce in a similar manner and they included good data on key family background characteristics. Citations to these datasets can be found in Appendix A.

Based on the pooled data set, 20 birth cohorts were constructed for adult respondents born between 1930 and 1991. Children of parents who were married during youth were compared with children whose parents divorced when they were growing up. Distinctions based on the child's age at divorce were also made. Two approaches were developed to test the institutionalization hypothesis. The first approach was time based and tested cohort trends in the effects of divorce on children's education using interaction effects of cohort and divorce in regression models that controlled for key demographic determinants of both education and divorce (see below). The second approach was based on a two-stage multilevel framework in which the individual effects of divorce were first estimated in each cohort and subsequently linked in a meta-regression to measures of the prevalence of divorce in each cohort (Sharp, 1998; Harbord and Higgins, 2008). This approach has been suggested as an alternative to direct multilevel models when the number of units at the macro level is small (Bryan and Jenkins, 2016). Given the high correlation between time and the prevalence of divorce, it is unlikely that the two approaches will produce very different results, but the multilevel approach is conceptually more direct than descriptions of cohort trends.

The data also allowed me to include a number of key demographic variables in the models that may play a role in the trends. As mentioned earlier, parental education is an important variable because it is a strong predictor of children's education and because of the (changing) educational gradient in divorce. Second, a control for religiosity was included. Religiosity is correlated with divorce (Vaaler, Ellison and Powers, 2009; Wright, Rosato and O'Reilly, 2017) and is often found to be positively associated with children's schooling (Stokes, 2008). The association between religion and divorce may also have changed over time, something that will be tested. Third, a control for sibship size was used. There is a well-known negative association between sibship size and education (Steelman *et al.*, 2002; Kalmijn and van de Werfhorst, 2016) and sibship size is also important for divorce as families with more children are generally less likely to divorce (Diekmann and Schmidheiny, 2004; Kaplan, Endeweld and Herbst-Debby, 2020). It was expected that including sibship size and religion in the model will generally reduce parental divorce effects and possibly also attenuate trends in the effects of parental divorce.

Data and measures

Details of the 18 national surveys that were combined can be found in Appendix A. Descriptive statistics are presented in Table 1. The pooling strategy was inclusive in the sense that a survey was not excluded when it missed one or more control variables. All the surveys were national probability surveys, although the age range varied. In some surveys, specific groups were oversampled, but in these cases, a random sample of the oversampled groups was taken to ensure that the sample was representative again. In the regression models, a control was included for the survey (using 17 indicator variables), thereby allowing for possible design effects on educational attainment (e.g. variation in non-response, variation in measurement). Important to note is that cohorts were compared and not surveys; each cohort contained respondents from multiple surveys. People born in a non-European foreign country were excluded to avoid the trend from being affected by the rise of international migration. The effective sample size was N = 87,541.

Measures

Parental divorce was measured by questions of whether the parents were divorced/separated and, if so, at what age this occurred. The central independent variable was whether parents divorced at or before age 18. Additional analyses were done using the age of the child at divorce and making a distinction between divorced during ages 0–9 and 10–18. A control was used for whether parents were widowed when or before the respondent was 18 years. The comparison group thus consists of parents who were married when the respondent was growing up. The small group of respondents who experienced both transitions was coded as both divorced and widowed (n = 137). No distinction was made between divorce and separation and no distinction was made between marriage and cohabitation. In the four datasets that included information on parental marriage and cohabitation, 1.2 per cent of the respondents reported that their parents were not married.

Educational attainment was measured with somewhat different sets of categories across surveys. To make these comparable, two strategies were used. The first approach was to recode categories to a common metric (i.e. International Standard Level of Education or ISLED) without merging categories beforehand. This approach is attractive in that (i) it prevents a loss of information from merging categories, and (ii) it allows for a linear outcome variable (Schröder and Ganzeboom, 2014). Especially in a comparative perspective, finding a common categorization that is the same in all surveys can lead to a crude outcome variable. The second approach followed the opposite strategy by taking a loss of information for granted and ensuring exactly equivalent outcome variables across surveys. To make this possible, a distinction was made between tertiary education on the one hand and less than tertiary education on the other. The ISLED outcome was analyzed with a linear (OLS) regression model, the categorical outcome was analyzed with a logit model. The mean and standard deviation of ISLED are 55 and 20 (with a range of 17–95). Appendix B contains the details on the coding of education.

Four control variables were measured. *Father's and mother's education* were measured separately. Both variables were recorded in each survey to an ISLED score. In one survey, only the mother's education was included and in one survey, no information was present on parental education. Because reports on parental education contain missing values, the missing values were (initially) imputed using information on the parent's occupational status at age 14–16, if available. This was possible in nine surveys. Parental occupational status had fewer missing values because it is easier for children to know and remember their parents' occupation than their parents' education (Engzell and Jonsson, 2015). The correlation between parental education and occupational status was r = 0.64 for fathers and r = 0.59 for mothers, which shows that occupation is an attractive variable for imputing missing values on education. Using parental occupation instead of education was not an option since it was available in only nine surveys. For this reason, occupational status was only used as an auxiliary variable.

A measure of *religious background* was included. In most surveys, it was asked if the father and mother were church members when the respondent was growing up. The two dichotomous variables were combined to construct the variable (in other cases, only one parent was used), leading to a variable with codes of zero (no parent religious) or one (one or both parents religious). In a few cases, I relied on the question of whether the respondent ever belonged to a church or denomination. Ever belonging to a church is a reasonable proxy for church membership during youth since few people switch from not being a church member in youth to being a church member in adulthood (Voas and Storm, 2012). Sensitivity checks showed that the effect of religious background did not change when excluding the surveys with the alternative measure. The respondent's current religiosity was not used as it may be affected by events occurring after finishing school. *Sibsize* was measured in the majority of the surveys and was coded from 0 to 10. The main effect of cohort (categorical) and an interaction of gender and cohort were included to allow for the fact that education increased across cohorts (and more strongly for women).

Setup of the analyses

The data allowed me to compare 20 3-year birth cohorts. The first cohort was 1930–1932, the last cohort was 1988–1991.[2] The analyses consist of two parts. In the first part, I estimated regression models with interactions of cohort and parental divorce in three ways: an interaction with a linear cohort variable (coded from 1 to 20; Model 1), interactions with a linear and a quadratic cohort variable (Model 2), and interactions with categorical cohort (Model 3). The fit of the three models was compared using the Bayesian Information Coefficient or BIC measure. All models were estimated with (Models 1a, 2a, and 3a) and without (Models 1b, 2b, and 3b) control variables in order to see if trends were suppressed or confounded by (changes in) the association of parental divorce with the control variables. The models are presented in Table 2 (for ISLED) and Table 3 (for tertiary education). The categorical specifications are also presented in figures (Figures 3 and 4). In the interactions, cohort was centred around 1960 so the main effects of divorce applied to the 'middle' cohort. Selected models were estimated again using distinctions based on the age at divorce (Table 4).

In the second part, a multilevel framework was used. In view of the small number of macro units (20 cohorts), a two-stage procedure was adopted (Bryan and Jenkins, 2016). In the first step, interactions of categorical cohort and divorce were used to obtain the marginal divorce effects and their standard errors separately for each cohort. This was done twice, once for ISLED and once for the probability of completing tertiary education. Control variables were held constant at the mean. The marginal effects were subsequently used in a random effects meta-regression in which the prevalence of parental divorce in each cohort was used as a predictor (Harbord and Higgins, 2008). Two measures of divorce prevalence were used: (i) the proportion of children with divorced parents in each cohort as estimated by the data, and (ii) the official divorce rate during youth as published by Statistics Netherlands.[3] The (inverse of the) standard errors served as weights in the regression model. The meta-regressions are presented in Table 5 and the bubble plots for these models are presented in Figure 4. Each bubble represents a cohort, and each bubble's size is inversely related to the standard error of the divorce effect.

Respondents with missing values on the dependent and the key independent variables (divorce and cohort) were excluded beforehand (0.9 per cent) so that only the control variables had missing values in the analyses. Missing values on control variables were due to design effects (a survey not including the measure) or to partial missing values within a survey (11.5 per cent for father's education, 11.0 per cent for mother's education, 9.8 per cent for parents' religiosity, and 15.5 per cent for sibship size). To solve the missing value problem, I used multiple imputation based on chained regression models and Rubin's rules to combine the (20) imputations (Royston, 2005; Von Hippel, 2009). I also estimated models where missing values for the control variables were assigned to their cohort-specific means. The effects and standard errors in these models were virtually identical to the results with multiple imputation (Appendix C). The multiple imputation procedure was used for the main regression tables. The cohort-mean imputation procedure was used for the calculation of marginal effects and fit statistics (in these cases, applying multiple imputation was not straightforward). Since all models include survey effects, possible remaining levels of selectivity of the missing values will be adjusted implicitly and the effect of the control variables will not be biased by the fact that a specific survey did not contain a specific control variable.

Findings

Figure 1 shows that there was a clear and steady increase in the share of children whose parents divorced. Among children born in the 1930s, about 2–3 per cent had divorced parents and this increased to nearly 20 per cent for children born in the 1990s. There was no clear shift in the ages at which children experienced a parental divorce.

Before turning to the main set of results, I briefly explored the differences between married and divorced parents in terms of the control variables.[4] I found that compared with children of married parents, children of divorced parents had more highly educated fathers ($d = 2.77$, $p < 0.01$), more highly educated mothers ($d = 3.74$, $p < 0.01$), and fewer siblings ($d = -0.46$, $p < 0.01$). Divorced parents were less religious ($d = -0.16$, $p < 0.01$). These analyses showed that children of divorced parents were, in general, a positively selected group in terms of parental status. It was further explored if the differences in parental education changed across cohorts using interaction effects of cohort and divorce. In Figure 2, the predicted difference in parental education between married and divorced families is plotted by cohort. For father's education, we observed a gradual decline in the advantage of the divorced group; for mother's education, this was also observed but later in the period.

The main regression models for the level of completed education in Table 2 show that there was a negative and significant association between parental divorce and children's education (Model 1a). The

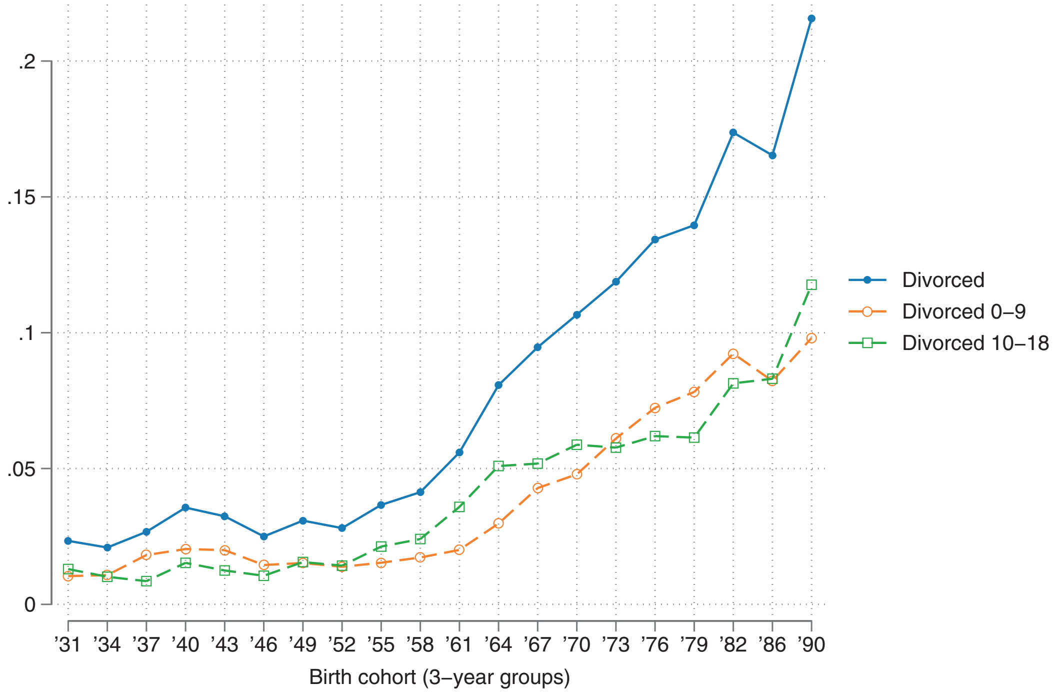

Figure 1 Parental divorce experience in youth by birth cohort in the Netherlands.

Note: Percentage of children experiencing the divorce of their parents between ages 0 and 18 based on merged life-history surveys from the Netherlands.

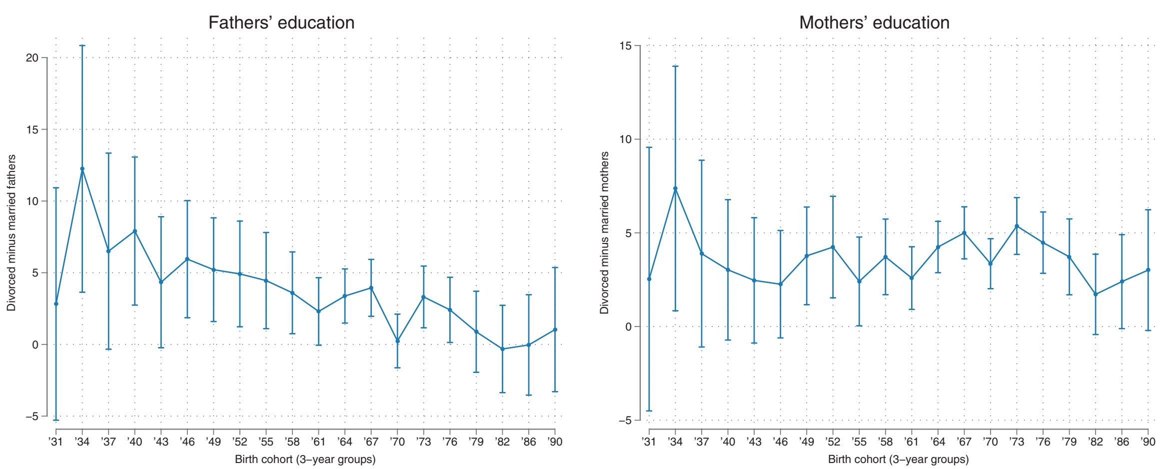

Figure 2 The parental educational 'advantage' of divorced parents.

Note: Marginal effects of parental divorce on parental ISLED scores are obtained from a model with cohort, divorce, and the interaction of cohort and divorce.

effect was −3.68, which amounts to 18 per cent of a standard deviation in education (SD_{ISLED} = 20). In the logit model in Table 3, the effect of parental divorce was −0.252 (Model 1a). Hence, children of divorced parents had a 22 per cent lower odd of achieving a tertiary education than children of married parents ($1 - e^{-0.252}$). The effects of widowhood were also negative and significant.

The interaction between cohort and divorce was included in Model 1a as well. There was a negative interaction between divorce and cohort for completed education ($b = -0.163$, $p = 0.02$), showing that the negative association between parental divorce and education became somewhat more negative over time, in contrast to the institutionalization hypothesis. There was no significant interaction

Table 1 Summary statistics of variables by survey

	Age			Birth year			Parental divorce			Means X and Y variables				
Survey	Mean	Min	Max	Mean	Min	Max	Number with divorced parents	Proportion divorced	Number respondents	Resp education	Father education	Mother education	Sibsize	Religion
og1988	31.3	25	38	1957	1950	1963	152	0.035	4,304	50.5			3.31	
psin1991	27.9	25	31	1963	1961	1965	56	0.070	803	56.6	44.6	36.60	2.63	0.75
hin1995	37.7	25	65	1957	1930	1970	214	0.068	3,144	54.1	42.3	35.16	3.03	0.71
sin1998	47.6	31	68	1950	1930	1967	29	0.048	598	53.7		32.60	3.51	0.81
og1998	37.6	25	53	1960	1945	1973	521	0.067	7,761	56.5	41.8	35.97	2.90	0.83
og1993	38.7	25	68	1957	1930	1973	541	0.048	11,376	52.7	40.0	33.07	3.37	0.83
fnb2000	45.4	25	70	1954	1930	1975	72	0.050	1,438	51.2	38.5	33.01	3.26	0.80
og2003	43.6	25	63	1959	1940	1978	464	0.067	6,922	55.1	41.4	33.86	3.08	0.85
fnb2003	47.0	25	73	1956	1930	1979	243	0.043	5,666	50.1	38.9	33.75	3.36	0.83
nkps2003	47.2	25	73	1955	1930	1978	1001	0.060	16,791	54.5	42.1	35.26	3.22	0.86
og2008	44.5	25	63	1964	1945	1983	595	0.090	6,646	55.6	44.4	36.34		0.84
evs2008	53.2	25	78	1955	1930	1983	67	0.053	1,274	55.8	43.3	38.48		0.37
nells2009	36.1	25	47	1973	1963	1985	261	0.126	2,067	63.0	48.6	41.00		0.58
fnb2009	49.5	25	79	1960	1930	1984	179	0.058	3,099	55.4	41.7	35.51		0.81
liss2012	53.0	25	82	1959	1930	1988	325	0.065	4,965	57.0	43.1	35.14	2.67	0.98
og2013	51.9	25	79	1961	1934	1988	653	0.077	8,468	57.3	43.7	36.38	2.89	0.83
okin2017	34.2	25	46	1982	1971	1991	355	0.160	2,219	61.9	50.8	46.80	1.69	0.64
Total	43.8	25	82	1959	1930	1991	5728	0.065	87,541	54.9	42.2	35.41	3.08	0.82

Source: See appendix A. Age truncated at 25. Birth year truncated at 1930.

Table 2 Linear regression models of respondents' level of education (*p*-values in parentheses)

	1a. Linear	2a. Quadratic	3a. Categorical	1b. Linear	2b. Quadratic	3b. Categorical
Divorced 0–18	−3.680**	−3.816**	−2.302	−5.175**	−5.273**	−5.887**
	(0.000)	(0.000)	(0.545)	(0.000)	(0.000)	(0.001)
Cohort × divorce	−0.163*	−0.174*	NP	−0.019	−0.027	NP
	(0.020)	(0.016)		(0.770)	(0.687)	
Cohort squared × divorce		0.009			0.006	
		(0.506)			(0.604)	
Parents widowed	−2.969**	−2.970**	−2.978**	−2.137**	−2.138**	−2.143**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Cohort × widowed	−0.056	−0.056	−0.056	0.097~	0.097~	0.097~
	(0.375)	(0.373)	(0.374)	(0.093)	(0.094)	(0.093)
Fathers' education				5.043**	5.043**	5.044**
				(0.000)	(0.000)	(0.000)
Cohort × father education				−0.273**	−0.273**	−0.273**
				(0.000)	(0.000)	(0.000)
Mothers' education				3.476**	3.476**	3.474**
				(0.000)	(0.000)	(0.000)
Cohort × mother education				−0.036~	−0.036~	−0.037~
				(0.063)	(0.062)	(0.059)
Number of siblings				−1.548**	−1.548**	−1.550**
				(0.000)	(0.000)	(0.000)
Cohort × sibsize				−0.026	−0.026	−0.027
				(0.180)	(0.176)	(0.164)
Parent religious				2.572**	2.572**	2.573**
				(0.000)	(0.000)	(0.000)
Cohort × religiosity				−0.105**	−0.105**	−0.105**
				(0.009)	(0.009)	(0.009)
N	87,541	87,541	87,541	87,541	87,541	87,541
BIC	768,865	768,875	769,043	752,570	752,581	752,752

Note: Controlled for survey, cohort, gender, and cohort × gender. Multiple imputation of missing values on control variables. NP is not printed (see figures).
~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

between cohort and widowhood. The logit model in Table 3 also reveals a significant and negative interaction between cohort and divorce ($b = -0.021$, $p = 0.01$), pointing to a growing gap in tertiary school completion between children of married and divorced parents.

Two additional ways of specifying cohort were considered. In Model 2a, parental divorce was interacted with a linear and a quadratic cohort variable. Using the BIC as a criterion, this model had a poorer fit to the data than the linear interaction model. This applied to both the model for completed education (Table 2) and the model for tertiary education (Table 3). In line with this, the interaction between quadratic cohort and divorce was not significant. In Model 3a, parental divorce was interacted with categorical cohort. This model had a higher BIC value and hence, a poorer fit to the data in both Table 2 and Table 3.

In sum, without control variables, there was evidence of an increasingly negative association between parental divorce and children's education across cohorts. This was supported by the marginal divorce effects obtained from Model 3a, as presented in Figure 3. The figure showed a gradually growing gap in ISLED and in the proportion with tertiary education between children with married and divorced parents across cohorts, with children of divorced parents achieving on average less education.

In the next set of models, control variables were added. In all three versions of the cohort specification, the BIC-values were lower—indicating better fit—with control variables than without control variables. This again applied to both educational outcomes (completed education and tertiary education). As expected, there were strong positive effects of father's and mother's education on respondent's education. Moreover,

Table 3 Logit regression models of respondents' tertiary education (*p*-values in parentheses)

	1a. Linear	2a. Quadratic	3a. Categorical	1b. Linear	2b. Quadratic	3b. Categorical
Divorced 0–18	−0.252**	−0.245**	−0.368	−0.454**	−0.449**	−0.627**
	(0.000)	(0.000)	(0.554)	(0.000)	(0.000)	(0.005)
Cohort × divorce	−0.021*	−0.020*	NP	−0.009	−0.008	NP
	(0.010)	(0.025)		(0.294)	(0.387)	
Cohort squared × divorce		−0.001			−0.000	
		(0.742)			(0.812)	
Parents widowed	−0.276**	−0.276**	−0.276**	−0.231**	−0.231**	−0.231**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Cohort × widowed	−0.007	−0.007	−0.007	0.007	0.007	0.007
	(0.342)	(0.343)	(0.350)	(0.365)	(0.365)	(0.362)
Fathers' education				0.497**	0.497**	0.498**
				(0.000)	(0.000)	(0.000)
Cohort × father education				−0.018**	−0.018**	−0.018**
				(0.000)	(0.000)	(0.000)
Mothers' education				0.350**	0.350**	0.350**
				(0.000)	(0.000)	(0.000)
Cohort × mother education				0.002	0.002	0.002
				(0.521)	(0.520)	(0.519)
Number of siblings				−0.125**	−0.125**	−0.125**
				(0.000)	(0.000)	(0.000)
Cohort × sibsize				−0.004	−0.004	−0.004
				(0.131)	(0.133)	(0.128)
Parent religious				0.234**	0.234**	0.234**
				(0.000)	(0.000)	(0.000)
Cohort × religiosity				−0.006	−0.006	−0.006
				(0.308)	(0.309)	(0.305)
N	87,541	87,541	87,541	87,541	87,541	87,541
BIC	103,095	103,106	103,287	92,957	92,968	93,149

Note: Controlled for survey, cohort, gender, and cohort × gender. Multiple imputation of missing values on control variables. NP is not printed (see figures).
~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

there were significant negative interactions between father's and mother's education and cohort. The interactions showed that the influence of parental education on children's education declined over time, in particular for father's education. These findings are in line with previous studies of educational attainment in the Netherlands (Korupp, Ganzeboom and Van der Lippe, 2002; Tieben, De Graaf and De Graaf, 2010). Having religious parents was associated with a higher level of education. This effect declined across cohorts but only in the model for completed education (Table 2), not in the model for tertiary education (Table 3). The number of siblings had the expected negative association with children's education but this was a stable effect, given the small and insignificant cohort interactions.

To what extent were the interaction effects of parental divorce and cohort affected by including the control variables? The linear interaction term of cohort and divorce declined to −0.019 (p = 0.770) in Model 1b (Table 2). A similar result was obtained in the logit model: a decline to −0.009 (p = 0.294) in Model 1b (Table 3). In other words, the association between parental divorce and children's education did not become more negative anymore after including the control variables. The same result was found when looking at the categorical cohort interactions graphically across models in Figure 3. In Figure 3 (without control variables), there was a slightly increasing gap between children with divorced parents and children with married parents. This was no longer the case in Figure 4, which had control variables held constant at the means. In sum, the increase in the divorce penalty over time reported in Figure 3 fades away in the final model in Figure 4. Interesting to see is that the adjusted marginal effect of divorce on tertiary education fluctuates around 10 per centage points, similar to what

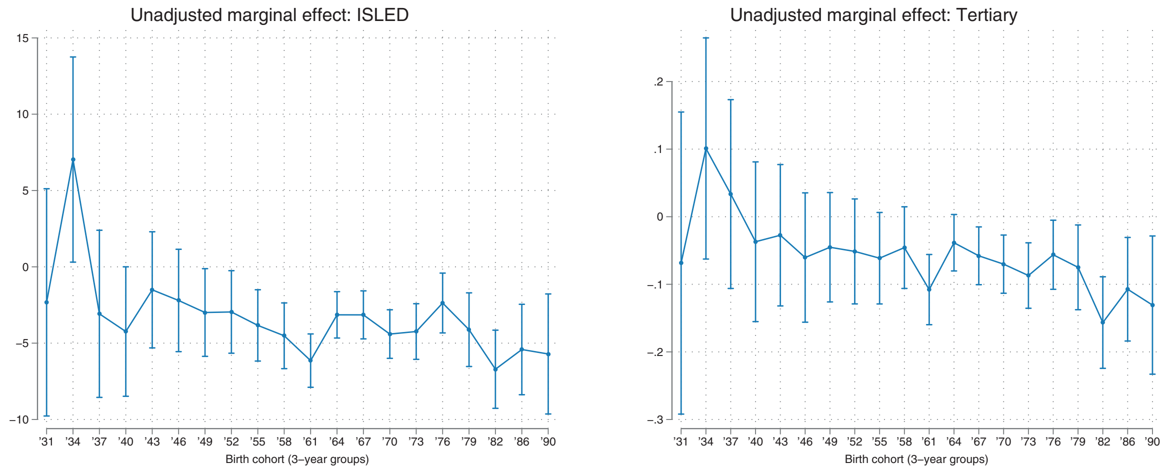

Figure 3 Marginal effects of parental divorce on education by cohort.

Note: The predicted gap in ISLED scores (left) and proportions completing tertiary education (right) between children of divorced and married parents with predictors. Based on Model 3a.

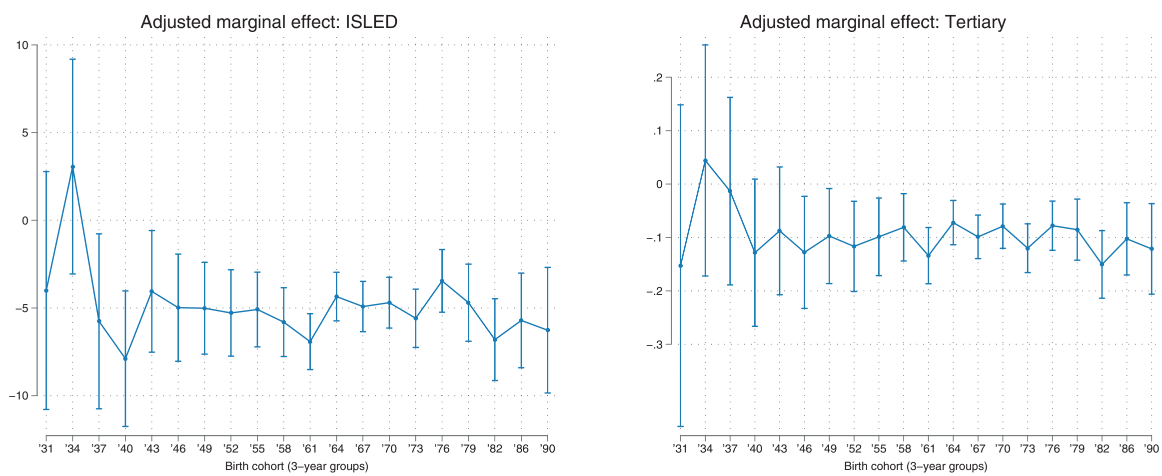

Figure 4 Marginal effects of parental divorce on education by cohort (control variables held constant).

Note: The predicted gap in ISLED scores (left) and proportions completing tertiary education (right) between children of divorced and married parents with predictors. Based on Model 3b.

was reported for other countries (Bernardi and Radl, 2014).

That the trend in the 'divorce penalty' was reduced after control variables were added is in line with our expectations about the changing relationships between divorce and parental education and between parental education and children's education. This was partial evidence for the suppressor hypothesis. However, the suppressor effect was not strong enough to turn the trend around: the remaining trend was flat.

In Table 4, the analyses were replicated by making a distinction based on the age at which children experienced the divorce. The main effects show that an early divorce had a slightly stronger association with children's education than a late divorce. Without control variables, the interactions of cohort and divorce were negative for both early and later divorces. With control variables, all four interactions disappeared.

So far, the analyses have used time as the key variable. To test the institutionalization hypothesis more directly, I estimated meta-regression models in which the divorce effect was associated with the prevalence of divorce. The marginal divorce effects and their standard errors were obtained from Model 3b (with control variables). In a second-stage model, these marginal effects were analyzed with a random effects model that used the (inverse of the) standard errors as weights (Harbord and Higgins, 2008). The results of these models are presented in Table 5.

Table 4 Linear and logit regression models with distinction using the child's age at divorce (*p*-values in parentheses)

	ISLED without controls	ISLED with controls	Tertiary without controls	Tertiary with controls
Divorced 0–9	−4.728**	−6.151**	−0.357**	−0.570**
	(0.000)	(0.000)	(0.000)	(0.000)
Divorced 10–18	−2.768**	−4.333**	−0.166**	−0.360**
	(0.000)	(0.000)	(0.000)	(0.000)
Cohort × divorce 0–9	−0.129	−0.005	−0.017	−0.007
	(0.174)	(0.953)	(0.134)	(0.600)
Cohort × divorce 10–17	−0.196*	−0.030	−0.024*	−0.011
	(0.049)	(0.741)	(0.037)	(0.391)
N	87,541	87,541	87,541	87,541

Note: Controlled for survey, cohort, gender, and cohort × gender. Multiple imputation of missing values on control variables.
~ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 5 Random effects meta-regression of the marginal divorce effects (Y) on the prevalence of divorce (X)

	Effect on ISLED		Effect on tertiary	
	Coefficient	*P*-value	Coefficient	*P*-value
Divorce prevalence	−0.941	0.903	−0.010	0.946
Constant	−5.088	0.000	−0.082	0.000
N cohorts	20		20	
	Coefficient	*P*-value	Coefficient	*P*-value
Divorce rate	0.036	0.340	0.001	0.280
Constant	−5.368	0.000	−0.086	0.000
N cohorts	20		20	

Note: Divorce prevalence is the proportion of divorced parents in the pooled data. Divorce rate is based on official statistics from Statistics Netherlands (see the text).

The findings in Table 5 show in an even clearer fashion that there was no support for the institutionalization hypothesis. The effects of the cohort-specific prevalence of divorce, as calculated from the data, on the effect of divorce were trivial in magnitude and statistically insignificant with very high *p*-values ($p = 0.90$ for ISLED and 0.95 for tertiary). The same finding was obtained when using the official divorce rate in society at the time the cohorts of children were raised ($p = 0.34$ and 0.28). The bubble plots in Figure 5 confirmed that there was no association between the divorce penalty on the one hand and these macro-level indicators of the prevalence of divorce on the other.

Conclusion

This analysis concludes that despite enormous changes in education and a substantial transformation of the life course during the 21st century, the 'divorce penalty' in the Netherlands was stable. The conclusion of the current analysis is in line with two previous studies that looked at educational outcomes, a study of the UK (Ely *et al.*, 1999; Sigle-Rushton, Hobcraft and Kiernan, 2005) and a study of Sweden (Gähler and Palmtag, 2015). The present article adds a long-term perspective, a larger number of cohorts, and a statistically more powerful set of tests. Similar evidence was found when looking directly at the prevalence of divorce. Using 20 cohorts in a two-step multilevel analysis, there was no association between the rate of divorce on the one hand and the effect of parental divorce on children's education on the other hand.

There was some evidence for an increasing association between parental divorce and children's education, in contrast to the institutionalization hypothesis, but this was to a large extent due to the selectivity of divorce with respect to family background variables. In the past, divorced parents had more 'favourable' traits, something that benefited the children of divorce. Over time, these benefits became smaller and less influential, leading to an increasing gap between children of married and divorced families. After adjusting for family background effects and cohort changes in these effects, the net result was stability.

How can the negative evidence for the institutionalization hypothesis be interpreted? In this conclusion, I offer a number of possible reasons why the 'divorce penalty' may have been stable over time. One of the elements of the institutionalization thesis is that the acceptance of divorce and single parenthood has increased over time. While there is much evidence for changing norms and values surrounding divorce and single parenthood (Halman and van Ingen, 2015), perhaps normative disapproval has little to do with the effects of parental divorce on children. Children may feel less comfortable when their home situation is not supported by their peers and their school, but the

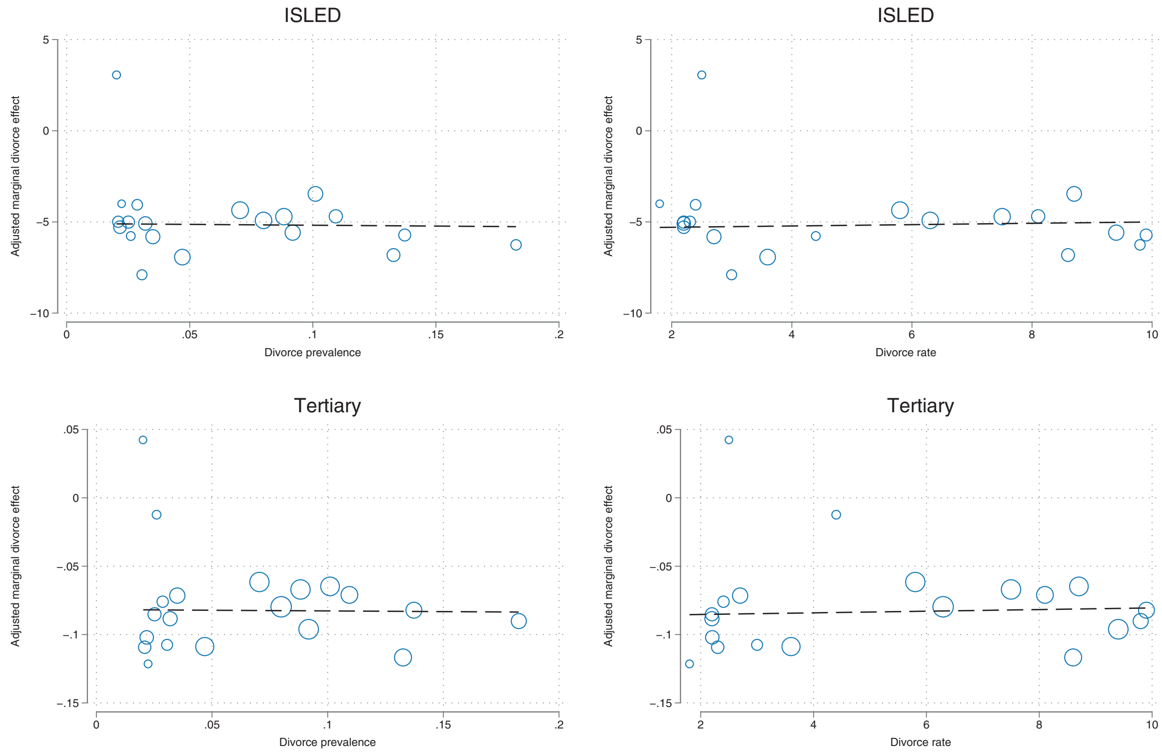

Figure 5 Meta analysis of divorce effects.
Note: The predicted gap in ISLED scores (top) and proportions completing tertiary education (bottom) between children of divorced and married parents, plotted against the prevalence of divorce in a cohort and the crude divorce rate in the cohort years (see the text). The size of bubbles is inversely proportional to the number of cases in the cohort. The marginal effects are from Model 3b. The line is the slope predicted in the meta-regression.

question is whether such effects also translate into more emotional problems. If children do experience emotional problems as a result of a divorce, which is plausible, these problems may not be reduced if disapproval declines. The social context could be supportive or not supportive in the process, but this may not depend as much on social norms as is commonly assumed.

Another way to interpret the findings is by considering how the nature of the divorce process has changed. Two different trends may have occurred that worked against each other. A first trend lies in the selectivity of divorce. There is evidence that in the early days of the divorce revolution, a divorce was often preceded by more serious problems between parents, such as violence and addiction (De Graaf and Kalmijn, 2006a). The more serious divorces that occurred in the past will have been harmful to children's well-being since children were exposed to these problems for a number of years before the divorce. In a sense, the group of divorced families was a more negative selection in the past than currently, and this by itself would lead to a weakening of the association between divorce and child outcomes over time.

An offsetting trend lies in what happens after divorce. In the past, divorce often meant an end to the parents' problems. For example, children in earlier divorce cohorts often lost contact with the non-resident parent altogether, suggesting that ex-partners were no longer in contact with each other (Van Spijker, Kalmijn and Van Gaalen, 2022). This seems in contrast to the contemporary situation. In contemporary divorces, fewer conflicts may occur but there is also a continuation of conflicts after divorce. Studies have shown that ex-partners often maintain a relationship after divorce, especially when there are children, and that couples who had conflicts during marriage had more conflicts after divorce, often with serious fights between ex-partners (Fischer, De Graaf and Kalmijn, 2005). Such lingering conflicts after divorce may have negative effects on child outcomes, leading to a strengthening of the association between divorce and child outcomes over time. Obviously, there is evidence that more post-separation contact with non-resident fathers is beneficial to children (Adamsons and Johnson, 2013) but there is also evidence that these effects are negative for children in the case of interparental conflict (Kalmijn, 2016). In other words, there may have been a shift toward a less negative selection into divorce over time, counteracted by a shift from 'clean breaks' to 'lingering problems' in the modern era. Without direct evidence on each of the two processes, this reasoning remains speculative, but it is a possibility that deserves further study, especially in the context of the rise of co-parenting (Sodermans, Matthijs and Swicegood, 2013; Vanassche *et al.*, 2013).

Other mechanisms and outcomes must also be considered. Because the outcome variable in the present article is children's educational attainment, part of the parental divorce effect will not work via the social, cultural, and psychological mechanisms that were central in the theoretical approach. Several studies have shown that parents' economic resources, and in particular their income, is also influential for children's educational attainment. Although traditionally, there was little evidence for economic mechanisms in the case of the Netherlands (De Graaf, De Graaf and Kraaykamp, 2000), more recent evidence suggests that income does play a role, on top of parental education (Westerman and Van Gaalen, 2015). How the disadvantaged income position of divorced mothers vis-à-vis married mothers has changed across cohorts is not known for the Netherlands. Research on cohort change in the economic consequences of divorce and separation in other countries shows that divorced women do not fare much better today than a few decades ago (Smock, 1993; Bröckel and Andreß, 2015; Mortelmans, 2020). It is therefore important to study cohort changes in the divorce penalty in connection to historical changes in the income position of divorced mothers. In a similar vein, it is important to study changes in child outcomes that are more directly connected to the social, cultural, and psychological mechanisms in the institutionalization hypothesis. It would be particularly interesting to study aspects of psychological well-being for testing the hypothesis. However, such measures cannot be tackled very well in life history surveys and this reduces the historical breadth of the design. The advantage of education as an outcome variable is that it can be measured well and that it is linked to an early age that is close to the divorce of the parents.

In closing, a number of limitations of the current study need to be considered. The most obvious limitation is that no panel data were used that contain outcome measures before and after divorce. Such panel data have been used in the past, in particular for children's emotional well-being, and have yielded similar conclusions about trends, although for a much shorter period of time (Sigle-Rushton, Hobcraft and Kiernan, 2005). Moreover, no measures were available for characteristics of the parents' marriage (e.g. conflict, marital satisfaction) and for behavioural–emotional problems of fathers and mothers (e.g. depression, child neglect, health behaviours). The data did allow me to control for key background variables such as parental education, religion, and sibsize. While these are limitations, it is also clear that they are the result of a trade-off between the level of detail in the analysis on the one hand, and the long-term cohort perspective on the other. Finally, this study focussed on one outcome. Although education is a key variable for a broad range of social, cultural, and economic differences, it remains important to assess how effects of parental divorce on other measures of children change. This may not be possible to assess over a long period of time, but it is possible and important to track such changes in contemporary times.

Notes

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2 The last 8 years were divided in two four-year cohorts because of sample size considerations.
3 The divorce rate during youth was calculated by taking the rate at age 10, which was the median age at divorce. More subtle measures (e.g. averages of rates at ages 6, 10, and 14) were also used but did not yield different results.
4 This was done using regression models as a descriptive tool to compare married and divorced parents after controlling for cohort as a covariate.

Supplementary Data

Supplementary data are available at *ESR* online.

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