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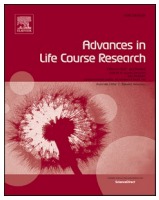

The effects of unemployment on fertility☆

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ABSTRACT

We analyse the effects of unemployment on the likelihood of having a first and second birth in Denmark. The existing studies on this topic have generated contradictory results, and have made a weak case for the exogeneity of unemployment to fertility. We suggest that firm closures constitute an exogenous source of unemployment, and adopt firm closures as an instrument for estimating individuals' fertility responses. Using a life-course approach, we exploit unique administrative data from Denmark that include *all* Danish residents born in 1966 and followed between 1982 and 2006. The data contain monthly information about each individual's employment status, type of employer, relationship status and partner's characteristics; as well as very detailed fertility information, including on stillbirths and registered miscarriages. We find that unemployment has a positive effect on motherhood transitions and a negative effect on fatherhood transitions, although the latter is not robust to the inclusion of controls. We find no significant effect of unemployment on second births.

1. Introduction

Although the relationship between unemployment and fertility has been investigated by demographers and economists for almost a century (see Currie & Schwandt, 2014), most studies have focused on how fertility outcomes respond to changes in unemployment rates (or female employment rates) (e.g. Adsera, 2004; Ahn & Mira, 2002; Brewster & Rindfuss, 2000; Currie & Schwandt, 2014; Dehejia & Lleras-Muney, 2004; Engelhardt & Prskawetz, 2004; Esping-Andersen, 2009; Kravdal, 2002; Sobotka, Skirbekk, & Philipov, 2011).

A growing number of studies went beyond the aggregate data to analyse how an individual's unemployment experience and fertility behaviour are related in various country contexts (e.g. Kravdal, 2002; Kohler & Kohler, 2002; Tölke & Diewald, 2003; Adsera, 2005; González & Jurado-Guerrero, 2006; Kreyenfeld, 2009; Ozcan, Mayer, & Luedicke, 2010; Adsera, 2011; Kreyenfeld & Andersson, 2014; Inanc, 2015). However, the findings of this literature are inconclusive (see Kreyenfeld & Andersson, 2014 for a review). Some studies found either no association between unemployment and women's fertility timing (e.g. Kreyenfeld, 2009; Kravdal, 2002; Rindfuss, Morgan, & Swicegood, 1988; Kohler & Kohler, 2002) or a positive association for women with lower levels of education (Hoem, 2000; Inanc, 2015; Kreyenfeld, 2009). Others reported a negative association between unemployment and transition to motherhood (e.g. Hoem, 2000; Adsera, 2005; Gonzalez & Jurado 2006). The few studies that focused on men have reported similarly contradictory findings (e.g. Tölke & Diewald, 2003; Kravdal, 2002; Ozcan et al., 2010; Schmitt, 2012).

In this paper, we revisit the relationship between individuals' unemployment experience and their two specific fertility outcomes: the transitions to parenthood and the probability of second conceptions. We use (unexpected) firm/plant closures in Denmark as an exogenous source of individuals' unemployment, and analyse how unemployment that results from firm closure affects the timing of the first and second births. Our modelling strategy combines an explicit life-course approach, which considers complete work, fertility and relationship history of all Danish residents born in 1966, followed over 24 years between 1982 and 2006, and a causal framework, which relies on a well-established instrumental variable to ensure that the unemployment experience does not correlate with the observed or unobserved characteristics of the individuals, which make them more likely to have children.

In addition to the inconclusive findings, this paper is particularly motivated by our observation that the literature on unemployment and fertility often lacks a causal approach. Demographers typically use reduced form duration models to estimate fertility timing, and a measure of employment status as the main explanatory variable, while

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controlling for several other factors such as age, education, socio-economic status, region, partnership status, etc. The inconsistency of findings led scholars to consider whether the meaning and importance of unemployment are different for different population subgroups. As a result, recent studies increasingly paid attention to the interactions between subgroup indicators, such as education, region, socio-economic status, age-groups and the employment status indicator (e.g., Kreyenfeld, 2009; Ozcan et al., 2010; Schmitt, 2012; Kreyenfeld & Andersson, 2014). Although showing how the association between unemployment and fertility varies within these groups is informative, some of these associations could be spurious because, at the very least, individuals could sort themselves into specific subgroups based on employment prospects (and preferences for family size).

These studies typically set the time of conception at around nine months before the birthdate of the child, as a common practice to avoid reverse causation, whereby fertility influences the likelihood of becoming unemployed. Although this procedure breaks the time order, it fails to fully eliminate the endogeneity problem: i.e., that a series of events and preferences interlinked across the life course determine both fertility outcomes and the likelihood of being unemployed (e.g., Angrist & Evans 1998). For example, first, planning to become a parent, and strong preferences for motherhood or fatherhood might affect an individual's career aspirations, work performance and attachment, and subsequently could affect the probability that he or she will become unemployed. Even in the context of Nordic countries, Datta Gupta, Smith, and Verner (2008) document that preferences for motherhood and children lead women to opt for jobs that are less steep in career progressions, relatively low paying and predominantly in the public sector. These jobs are likely to have different levels of unemployment risk than average jobs in the private sector. Second, an explanation from the labour demand side suggests that mothers or those planning to become mothers might be perceived by the employers as less attached to careers, making these women more vulnerable to unemployment. The opposite might be true for men based on the implications of the vast literature on fatherhood premium. Smith, Smith, and Verner (2011) found evidence of statistical discrimination in Denmark and report that the variables related to children, maternity leave had differential effects for men and women on their promotion rates to high-rank positions. In a very recent study, Becker et al. (2019) provide evidence of employment discrimination experienced by women in Germany, Austria and Switzerland also for "potential fertility". We claim that it is very plausible that such effects are likely to be found not only for promotion and hiring but also for experiencing unemployment and job loss. Thus, a careful assessment of the causal relationship between unemployment and fertility must be based on an exogenous source of unemployment (i.e. independent of the individuals' observed or unobserved characteristics).

A much smaller group of studies with similar concerns have used job displacements due to firm closures to predict fertility outcomes in Austria (Del Bono, Weber & Winter-Ebmer 2012 and Del Bono et al., 2014), in the US (Lindo, 2010), in Germany (Hofmann, Kreyenfeld, & Uhlendorff, 2017) and Finland (Huttunen & Kellokumpu, 2016). Our paper, which uses Danish data, contributes to this literature, but departs from them in a number of ways:

First, most of these studies did not focus on "unemployment" explicitly. For example, Lindo (2010) used the husband's job loss[1] as a negative shock to the family's income to estimate the wife's fertility response but did not consider whether the wife was unemployed. Huttunen and Kellokumpu (2016) explored the job displacement of both partners during the recession in Finland in 1991−1993. Still, their emphasis was on non-economic channels that may affect completed fertility, such as divorce and the probability of re-employment (which they examined by grouping unemployment with inactivity in the reference category). Del Bono, Weber, and Winter-Ebmer (2012) analysed the effects of all career interruptions due to job displacements (irrespective of unemployment experience) on individuals' completed fertility levels. Only, in their follow-up study (2014) they focused on unemployment and completed fertility levels. While this study is the closest to ours, our approach differs considerably from the one used in this study because we analyse the timing of first and second births by taking into account complete life histories.

Our study focuses exclusively on unemployment, and we use firm closures to instrument unemployment. Regarding fertility outcomes, we primarily focus on two: the timing of the decisions for the first and the second births as distinct but sequential transitions. We include all first and second births, including out-of-wedlock births, which constitute almost 40 per cent of all live births in Europe and nearly half of all live births in Denmark (Eurostat 2014).[2] Unlike most other studies, we do not limit our sample to married couples only.

Our analyses benefit from our use of the best possible data source: administrative panel data for all residents of Denmark born in 1966, followed between 1982 and 2006. These data have several advantages compared to the data used in earlier studies. First, because the data are monthly, we can measure precisely the timing of conception and unemployment. Second, the data allow us to identify accurately all of the firm closures that affected all of the women and men employed by private sector companies in Denmark and their partners. By contrast, Del Bono, Weber and Winter-Ebmer (2012), Del Bono et al. (2014) only considered "white collar" individuals who were working in the private sector, and excluded people who were working in specific industries, such as tourism, construction, and agriculture. Third, and perhaps most importantly, the data help us overcome many of the measurement issues that have arisen in the broader literature. For instance, data limitations have led some studies to derive fertility histories from the age of cohabiting children in a given year (e.g., Kravdal, 2002; González & Jurado-Guerrero, 2006; Adsera, 2005). This approach provides an incomplete picture of birth events, as it fails to count children who have left home or are living with the other parent or with relatives.[3] Studies that use available fertility histories (e.g., Ozcan et al., 2010) are also limited by the tendency of men to underreport early births in retrospective surveys, and especially non-marital births (Joyner et al., 2012), and by their reliance on a selected sample of "live" births. Ideally, the data would provide information about all types of conception decisions[4]; i.e., those that result in live births as well as in abortions, miscarriages and stillbirths. There is evidence from Denmark that these events correlate with unemployment and income loss (Bruckner, Mortensen, & Catalano, 2016). As a result, previous studies on how unemployment experience affects conceptions are likely to underestimate the true effect of unemployment.

Importantly, the theoretical arguments often refer to the "conception decisions" taken while being unemployed rather than to "live births". To our knowledge, no existing studies have considered all conceptions. To

[1] His study considered job losses due to various factors in addition to firm closures, including "being fired".

[2] Statistics Explained: http://epp.eurostat.ec.europa.eu/statistics_explained/

[3] González and Jurado-Guerrero (2006) and Adsera (2005) attempted to address this problem by setting the age of women to 40. They argued that the percentage of women under this age who do not live with their children is very small. Schmitt (2012) used fertility histories available in ECHP for all women who were present in the first wave, but relied on children living in the household for women who joined the panel later (a small subset of his sample) for whom fertility history was not available.

[4] Ideally, the data for a study on "transition to parenthood" should include unsuccessful attempts to conceive, adoption decisions and step-parenthood. The failure to take such information into account may be consequential, particularly in comparative studies, as the prevalence of these events varies between countries. For example, transitions to fatherhood via adoptions and step-fatherhood are about twice as prevalent in East Germany as in West Germany (Ozcan et al., 2010).

capture the broadest possible range of conceptions, we include in our analyses stillbirths and registered miscarriages. Moreover, to ensure that we capture early deliveries, we measure the exact duration of each pregnancy. This is also important for any studies on the timing of first birth given the previous evidence that unemployment may also be correlated with pre-term births (Rodrigues & Barros, 2008).

2. Theoretical background and previous literature

Individuals' unemployment experiences may affect their fertility outcomes; either directly by influencing their childbearing decisions, or indirectly by changing their health, partnership formation and dissolution processes, which may in turn influence their fertility outcomes (e.g. Eliason & Storrie, 2009; Huttunen & Kellokumpu, 2016). Here, we ignore the indirect channels[5] and focus only on the direct relationship between unemployment and childbearing decisions. We start with the neoclassical (economic) fertility model developed by Willis (1973) and Becker (1960, 1981), and its extensions (see Hotz, Klerman, & Willis, 1997; Kravdal, 2002; Adsera, 2004, 2011), as a framework to link unemployment experience to fertility decisions. However, we are aware that this theoretical framework remains very limited and outdated especially in the context of Denmark, which we describe in Section 2.1.

The standard (static) microeconomic models of fertility build on three assumptions. First, it is assumed that children are consumption goods and that parents derive utility from having and raising offspring. This implies that there is a positive relationship between income and the demand for children. Second, it is assumed that children are costly and time-consuming, especially during the months immediately after birth. Thus, for parents with budget constraints, having children is understood to involve a trade-off between quality and quantity. Third, as these models implicitly assume that traditional gender roles are common and persistent even in advanced societies,[6] researchers generally consider only the woman's time investment in childbearing and rearing. The neoclassical model, which does not consider the man's time investment, predicts that unemployment will have gender-specific effects on the couple's fertility outcomes. Overall, the prediction is negative for the man, and is directly related to the negative effect of unemployment on the family's total income and resources. This is the income effect. In addition to the income effect, the same model suggests that there is a substitution effect for the woman, whereby unemployment reduces the woman's cost of having children by providing her with additional time for childbearing and childcare. This implies that there is a positive association between unemployment and fertility decisions for women. In sum, the neoclassical model predicts that unemployment will have a negative effect on men's fertility decisions, but that the overall effect on women's decisions is ambiguous. The static model has been argued to better explain completed fertility rather than fertility timing (Hotz et al., 1997) even though it is commonly applied in many empirical studies on fertility decisions.

This model has provided the foundation for most empirical research on fertility decisions not only for studies that focused on unemployment mentioned before, but also for those that examined the effects of other types of economic uncertainty, such as job insecurity/instability due to short-term contracts (e.g. Bernardi, Klärner, & Von der Lippe, 2008; De la Rica, 2005), general economic and institutional uncertainty (e.g. Kohler & Kohler, 2002), and subjective and expected financial uncertainty (e.g. Krayenfeld 2009; Bhaumik & Nugent, 2005).[7]

In addition, there is a large body of research on the effects of recessions and economic crises on fertility (see the review by Sobotka et al., 2011). Many of these studies sought to explain the pro-cyclical nature of fertility (e.g. Butz & Ward, 1979; Adsera, 2005). More recently, a number of researchers have taken advantage of the opportunity to study large-scale firm closures during recessions (e.g. Ananat, Gassman-Pines, & Gibson-Davis, 2013[8]; Huttunen & Kellonkumpu, 2016). However, the large fluctuations in unemployment rates that occur during recessions might generate very different behavioural responses than unemployment experienced under stable macro-economic conditions. Recessions may lead people to postpone having children even in the absence of unemployment due to generalised feelings of long-term economic insecurity, rather than in response to actual income losses (Adsera, 2011). In such studies, it is hard to separate "the effect of unemployment" from "the effect of recession", where individuals experience more than just a higher risk of unemployment.

Some scholars have argued that dynamic (life-cycle) models might provide a more suitable framework for understanding the timing of births (e.g., Hotz et al., 1997). These models rely on a set of assumptions, such as the absence of uncertainty and the existence of perfect capital markets; and suggest that households maximise utility by choosing the timing of births and the wife's allocation of time over the life cycle. The models predict that a woman will prefer to have her children early in her life cycle so she can enjoy her offspring for a longer period of time. The models further imply that there is a dominant substitution effect: i.e., that a woman will prefer to give birth when her wages are low, such as during a phase of transitory unemployment (Hotz et al., 1997; Lindo, 2010).

According to sociological theories, substitution effects are stronger for the probability of first births than for the timing of higher order births, given the general social norm against childlessness (Kravdal, 2002). However, when there is uncertainty, and the capital markets are imperfect, transitory unemployment may still have equally important income effects especially on the probability of higher order births. Importantly, however, Adsera (2011) stressed that the substitution effect dominates only if unemployment is perceived as being truly temporary. When unemployment is persistent, a pregnancy might signal "a weaker commitment to the labour market", especially "if it happens early in the life course where human capital accumulation is crucial" (Adsera, 2011, p. 6). As a result, childbearing at younger ages combined with longer periods of unemployment may become "an unemployment trap" (Adsera, 2004, p. 22). Thus, if an unemployed woman is uncertain about whether the unemployment spell is temporary, she may prefer to postpone childbearing. In sum, the dynamic models also generate ambiguous predictions about the effect of women's unemployment on their timing of births.

Studies from the economics perspective and that relied on static models view completed fertility as the most important outcome as it is directly related to smaller family sizes and policies related to child well-being (e.g. Del Bono et al., 2012). The assumption is that as long as completed fertility is unaffected by unemployment, a delay in the timing of births would not matter. We believe this view missed the importance of considering timings of first and second birth outcomes sequentially because they both affect cohort sizes, available maternal resources, etc. which is essential for policy and planning, even if completed fertility (an outcome we cannot accurately look at with our current design) remains unchanged for each individual. The timing of first birth and the

[5] These channels may be less relevant in the Danish context, where marital status and fertility are less correlated and normatively ordered than in other industrialised societies (e.g. Esping-Andersen, 2007)

[6] See a critique of this assumption in Brodmann, Esping-Andersen, and Güell (2007), in which the authors find that fathers' involvement in childcare is high, and is one of the predictors of the second order births in Denmark.

[7] Although many of these studies rely on a similar theoretical framework, it can be argued that the substitution and income effects operate in the same way for other types of uncertainty.

[8] Ananat et al. (2013) also took a causal approach, although their study used county-level plant closures and birth rates rather than individual-level data.

probability of second birth is also correlated with the increase in completed fertility, as well. However, the sequential and interdependent nature of birth outcomes, especially regarding second births, is under-theorized in formal models (both static and dynamic models) mentioned above.

Moreover, the theoretical predictions about fertility outcomes and most empirical research on the topic usually refer to within-marriage births and tend to ignore out-of-wedlock births, including births to cohabiting couples (Hotz et al., 1997; Lindo, 2010; Huttunen & Kellokumpu, 2016). However, in many European countries, including in Denmark, many births are non-marital, and it is likely that unemployment has different implications for men and women in different relationship contexts. In our analysis, we include all marital and non-marital births and control for the mother's relationship status as well as for the partner's characteristics.

2.1. The Danish context

The theoretical framework described above (i.e. Becker's static model itself, as well as follow up work by Hotz et al. (1997), etc. described in the previous section) traditionally refers to family and labour market dynamics in countries with weak welfare states and relatively low levels of labour protection, such as the US. These models are often silent regarding contextual factors. Therefore, previous empirical studies on unemployment and fertility link in European countries with stronger social protection, ended up formulating hypothesis that deviate from original predictions of Beckerian model under uncertainty. Our paper uses data from Denmark, where the welfare state is comprehensive, and trade unions are strong, influential and active. These conditions may influence the relationship between unemployment and fertility decisions through several channels. First, the majority (approximately 75 per cent) of Danish workers have unemployment insurance, and most of those who lack coverage are eligible for welfare benefits. Thus, during phases of unemployment, the overwhelming majority of Danes still have a monthly income that provides them with a certain standard of living.[9] Furthermore, welfare benefits are means-tested, and unemployed people with children get higher benefits. These measures reduce the income shock of unemployment and could attenuate the income effect. It is also important to note that Danish unemployment rate has been relatively low compared to many other European countries throughout much of the observation period for our cohorts. In early 1980s, the unemployment rates were about 9% and declined until 1990s. In 1993, following the crisis in Scandinavian area, unemployment rate peaked reaching to nearly 10 %. However, this rate continuously declined every year until 2006 reaching to a record low of 3.6 % (OECD, 2020). Youth unemployment (15–24) followed similar trends and were not particularly high, and long-term unemployed (>12 months) were less than %10 of all unemployed throughout the period, a rate lower than in all other European countries (OECD, 2020). Thus, the implication is that majority of the unemployment experience could be seen short-lived (i.e. shorter than 9 months), which may affect the fertility decisions of the Danes.

Second, the labour force participation rate of Danish women throughout the period has been almost as high as that of Danish men, even though more women work part-time (36 per cent of women vs. 17 per cent of men (2017)). High-quality childcare is heavily subsidised and has been widely available especially since 1990s, which implies that a) a woman does not need to stay home to take care of her children, and b) that the family benefits financially from the woman's labour market participation (since her income will always exceed the cost of childcare). Moreover, the childcare costs of low-income families are subsidised. Given these incentives, there are strong norms among Danish women against opting out of the labour force even for shorter periods of time. Furthermore, throughout our observation window, generous paternity leave quotas were available to Danish men (which are abolished in 2002), which may have contributed reinforcement of social norms against sacrificing labour market attachment and careers for childbearing (Gash, 2009).

Unlike many other European countries, Danish period fertility rate has been increasing since early 1980s, a trend which is commonly explained by the successful integration of mothers in the labour market and related welfare state reforms (Andersson, Kreyenfeld, & Mika, 2009). Denmark has enjoyed one of the highest full-time maternal employment rates in Europe especially throughout our study period.

However, despite the strong labour market attachment of women in Denmark and high rates of maternal full-time employment, the division of labour within the household in Denmark is still far from being perfectly egalitarian, especially throughout our study period, although there has been some convergence towards gender equality in recent years (Bonke & Jensen, 2012). Evidence from time-use datasets until early 2000s in Denmark show that the time spent in housework and childcare remains quite unequal between men and women (81 min p/week on average), despite the fact that women's labour force participation rate (84 %) has almost caught up with that of men (90 %) throughout the period of our study (e.g. Bonke, Gupta, & Smith, 2003). Furthermore, in Denmark participating in childcare and housework is more likely to penalize men's wages (at the higher ends of the distribution) than comparable women's wages, reinforcing the traditional norms within the marriage (Bonke et al., 2003). Finally, childbirth still implies substantial gender inequality in the labour market (i.e. earnings gap) between men and women in the long-run in Denmark, which is worsened by occupation, sector and firm choices of women (Kleven, Landais, & Søgaard, 2019) These choices are likely to be structurally shaped by gender norm identity, as Kleven and colleagues show using data from the International Social Survey Program (ISSP) that attitudes favoring part-time and flexible employment as opposed to full time employment following childbirth was still dominating Denmark in early 2000s, even in the presence of near universal coverage of free childcare. Overall, these features of Danish society may imply the predictions of the neoclassical model still be valid even in the Danish context.

However, the generosity and gender-egalitarian nature of the Danish welfare state may reduce both the income and the substitution effect of unemployment on fertility decisions. Hence, any effect of unemployment on fertility decisions found in the Danish data probably represents lower bound estimates of a comparative effect in other countries, where the implications of unemployment and women's labour market decisions are likely to be much greater.

3. Data and method

3.1. DATA and SAMPLE

All residents of Denmark have a unique personal number. This number identifies the individual in a range of transactions, such as interactions with the welfare system, the education system, and the workplace. Some transactions are recorded on a daily basis; others on a

[9] The replacement rate with unemployment insurance is 90 per cent of the previous wage, but with a relatively low upper limit (a wage income of DKK 21.330/€ 2,850 per month). In 2014, the welfare benefit levels were DKK 14,203 (€ 1893) per month for uninsured unemployed individuals over age 30 who were responsible for minor children, and DKK 10,689 for individuals who were not responsible for minor children. The benefit amounts were considerably lower for uninsured unemployed individuals under age 30.

weekly or monthly basis; and a few, such as annual income, on a yearly basis. Statistics Denmark collects the information registered with this personal number annually and makes the data available to researchers. The data are arranged in a panel that starts in 1982 and ends in 2006,[10] and that contains all Danes. The data for each individual can be linked to his or her partners (married or cohabiting) and children. Since we know from these data the birthday of each child and the length of the mother's pregnancy, we can calculate the time of conception for each child. We also have information on the months the mother spent in unemployment. In addition, the data include information on the conception of stillborn children[11] and registered miscarriages, which is especially useful for our analysis of the probability of conceiving a child.

We use the Danish 1966 cohort (N = 87,333), whom we follow in the registers during the years when they are most likely to have a child: i.e. from age 16[12] (1982) until age 40 (2006). We follow them on a monthly basis, record the month of conception for their first and second child, and construct two samples. Our first sample contains all person-month observations until the individual conceives his or her first child. We right-censor the individuals who do not conceive before age 40 at age 40. The second sample contains all person-month observations from the conception of the first child to the conception of the second child. This sample only contains individuals with one child, and we right-censor the individuals in this sample who do not conceive their second child before age 40 at age 40. We further restrict both samples to include only individuals who were employed in private sector firms with more than four employees. The exclusion of smaller firms follows the tradition in the literature on firm closures and helps to eliminate the potential feedback effect of fertility behaviour on firm closures. Put differently, in smaller firms (four or less employees) we run the risk that the unemployment of one of the workers (may or may not be due to the childbirth) may lead the firm to be closed. Focusing on larger firms eliminates the possibility that the unemployment of a single individual brings down the firm because the larger firms' fates are less likely to depend on individual workers. We exclude public firms, as their closures are difficult to identify in the Danish registers. Given these restrictions, we only include observations of each individual in the months when he or she was employed in a private firm with more than four employees. Hence, if an individual moved between public and private firms, or between smaller and larger firms, we only include him or her during the months he or she spent working in a private firm with more than four employees.

Our first sample consists of 6,501,531 person-month observations, representing 74,556 individuals (40,923 men and 33,633 women). Our second sample consists of 1,784,356 person-month observations, representing 41,432 individuals (22,079 men and 19,353 women). We provide additional summary statistics in Appendix, in Table A11 for both samples. On average, we observe an individual for approximately 16 years before first birth (191 months) and the minimum duration until first birth is 2 years (24 months) and maximum duration, which is when an individual is censored is 300 months (25 years).[13] Less than 10 % of all individuals have only worked in the small firms (which we exclude from the data) and transitions to and from small firms are small but transition to and from public to private sector is more considerable, with some implications related to our findings.

3.2. Method

To test the average effect of unemployment on our fertility outcomes, we start with the approach most commonly used in the literature: i.e. a standard reduced-form discrete-time duration model, estimated using a linear probability model (Yamaguchi, 1991). However, we expand the simple framework and apply a panel version of the standard two-step procedure (2SLS) in which we instrument unemployment. We do this in order to accommodate our desire to analyse single cohort from a life course perspective and to include time-varying covariates, while addressing the potential endogenous relationship between unemployment experience and the decision to conceive. Hence, we set our data as if we were estimating a standard discrete time duration model, as recommended in Jenkins (1995)[14]; but we estimate a linear probability model (LPM) instead of the commonly used logit link. We, then, apply a 2SLS procedure to this dataset (which is set to estimate discrete time logit model). We use the ivreg command in Stata,[15] and handle the panel structure by clustering the observations of each individual using the individual identifier. As a result, our coefficients regarding fertility timing are the best linear approximations of the hazard rates, therefore, we interpret them as such.

We present separate results for men and women. Our models include industry and region dummies, or fixed effects, to capture differential exposure to unemployment as a result of the non-random distribution of plant closures in certain industries and regions in Denmark.

3.2.1. Exogeneous variation: firm closure

Firm closure has recently been established as a valid instrument for exogenous variation in unemployment (e.g., Del Bono, Weber, & Winter-Ebmer, 2014; and for Danish applications, see Browning & Heinesen, 2012; Browning, Møller Danø, & Heinesen, 2006; Eliason & Storrie, 2009). We follow this tradition and instrument unemployed with firm closures in an IV model. The IV model estimates a local average treatment effect (the LATE), and our results then describe the behaviour of those affected by the instrument. Of the individuals who experience a firm closure, some will be never-takers, who will find another job easily and thus avoid unemployment experience; some will be always-takers, who would have been unemployed regardless of the firm's closure; and some will be compliers. Compliers are individuals who are so valuable that the firm will, if possible, keep them employed if the firm hasn't been closed, but who are not sufficiently skilled to be able to find other employment as soon the firm closes and thus, they become unemployed. The LATE only concerns the effect of unemployment for this last group. Importantly, causal inference is only possible if two assumptions are fulfilled.

First, the independence assumption implies that the instrument only affects the outcome through the endogenous variable. The assumption holds if employees do not anticipate the closure and if the unemployment that results from the firm closure is not correlated with employee

[10] Our data ends in 2006 because 2008 great recession might have changed the relationship between unemployment and conception decision and the data post-recession in Denmark (post 2012) became available at a later stage (i.e. note that we do have data from still births and hospital records).

[11] Before 1997, a stillbirth was defined as the birth of a non-living child after the 27th week of the pregnancy. In 1997, the cut-off was changed to the 20th week of the pregnancy.

[12] There are fewer births taking place before age 20 in Denmark, thus changing age limits to 20 and 40 does not change our results. However, the characteristics of those who give birth at younger ages are likely to be very different than older ages. In order to avoid selection, we opt for the broadest sample with the largest age window.

[13] Note that these are the durations observed in the final sample, where months where the individual did not work in the private sector or in a large firm have been excluded. Hereby a significant share of individuals observed with the maximum duration of 288 months are in fact right censored even if they are not represented in the sample with 300 months.

[14] Where we observe each individual until they become a parent (and censor the observations afterwards).

[15] Later, for robustness, we estimate our models with ivprobit estimation, which gives us similar findings.

Table 1
Summary Statistics of Main Explanatory Variables.

	First Birth		Second birth	
	Men	**Women**	**Men**	**Women**
Variable	Pct.(std.)	Pct. (std.)	Pct. (std.)	Pct. (std.)
Unemployment	6 (23)	6 (24)	4 (19)	10 (30)
First child	4 (6)	7 (8)		
Second child			1 (1)	1 (12)
Excl. res.: firm closure (dummy var.)	1 (10)	1 (11)	09 (9)	10 (10)
Married (dummy var.)	8 (27)	8 (26)	50 (50)	38 (49)
Cohabiting (dummy var.)*	35 (47)	40 (49)	89 (31)	83 (38)
In education (dummy var.)	22 (42)	27 (44)	3 (17)	4 (20)
Previous unemployment**	5 (8)	4 (8)	7 (10)	8 (11)
Level of education (dummy variables, mutually exclusive categories)				
Elementary school	37 (48)	33 (47)	19 (40)	20 (40)
High school	10 (30)	21 (41)	5 (23)	11 (31)
Vocational	40 (49)	33(47)	54 (49)	50 (50)
Intermediate	3 (18)	3 (17)	6 (24)	5 (21)
College or above	8 (27)	9 (29)	15 (36)	15 (35)
Missing information	0 (6)	0 (3)	0 (6)	0 (4)
County (dummy variables, mutually exclusive categories)				
Copenhagen	23 (42)	31 (46)	20 (40)	26 (44)
Fredensborg	6 (24)	7 (25)	7 (26)	8 (27)
Roskilde	4 (20)	5 (21)	5 (22)	6 (24)
West Zealand	5 (22)	4 (20)	6 (23)	5 (22)
Storstrøm	4 (20)	3 (18)	4 (20)	3 (18)
Bornholm	1 (8)	1 (7)	1 (7)	1 (7)
Funen	8 (28)	7 (26)	8 (28)	7 (26)
Southern Jutland	5 (21)	4 (19)	4 (20)	4 (19)
Ribe	4 (21)	4 (19)	5 (21)	4 (19)
Vejle	7 (25)	6 (24)	7 (26)	7 (25)
Ringkjøbing	6 (23)	5 (22)	5 (22)	5 (22)
Århus	12 (32)	12 (32)	11 (32)	10 (30)
Viborg	4 (20)	4 (19)	4 (20)	4 (20)
Northern Jutland	9 (28)	8 (26)	9 (28)	7 (26)
Industry (dummy variables, mutually exclusive categories)				
Extraction of raw mater.	3 (3)	1 (12)	2 (13)	1 (10)
Production	32 (47)	22 (41)	31 (46)	26 (44)
Construction	12 (33)	2 (15)	11 (32)	3 (16)
Trade	29 (45)	29 (46)	24 (43)	25 (43)
Hotel and restaurant	2 (15)	7 (25)	1 (10)	3 (17)
Knowledge	12 (32)	19 (40)	12 (33)	19 (39)
Service	2 (15)	9 (29)	3 (17)	11 (31)
Art and craft	2 (12)	3 (17)	1 (12)	3 (17)
Service, other	0 (6)	1 (12)	0 (4)	1 (10)
No industry	6 (24)	6 (23)	13 (34)	9 (28)
Partner characteristics				
Unemployed (dummy variable)	6 (23)	3 (17)	13 (33)	5 (22)
In education (dummy variable)	7 (25)	5 (22)	7 (26)	4 (20)
Level of education*** (dummy variables, mutually exclusive categories)				
Elementary school	9 (29)	9 (29)	19 (39)	17 (38)
High school	6 (23)	4 (20)	8 (27)	6 (23)
Vocational	13 (34)	19 (40)	41 (49)	41 (49)
Intermediate	1 (11)	2 (14)	4 (19)	5 (21)
College or above	4 (24)	5 (22)	17 (38)	13 (34)
Missing information	0 (6)	0 (7)	1 (7)	1 (9)
N, Observations	**4,093,613**	**2,192,288**	**1,085,999**	**698,357**
N, individuals	**40,923**	**33,633**	**22,079**	**19,353**

*Note that the two categories "married" and "cohabiting" are not mutually exclusive, as some married individuals are not cohabiting in specific months.
**we define previous unemployment as the share of months observed in the data until the current month in which the individual has been unemployed.
***does not aggregate to 100 because individuals do not have partners in all observation months.

Table 2
Reduced-Form Models on the Timing of *First Births.*

Variable	Men Coefficient (std.)		Women Coefficient (std.)	
Unemployment *(Ref: Employed)*	−0.001(0.000)***	0.000 (0.000)	−0.005 (0.000)***	−0.005 (0.000)***
In education		0.000 (0.000)		−0.002 (0.000)***
Previous unemployment		−0.001 (0.001)		0.003(0.001)**
Married *(Ref: Single)*		0.008 (0.000)***		0.009 (0.000)***
Cohabiting		0.008 (0.000)***		0.009 (0.000)***
Level of education *(Ref: elementary school)*				
High school		0.000 (0.000)**		−0.002 (0.000)***
Vocational		0.000 (0.000)*		−0.000 (0.000)*
Intermediate		0.001 (0.000)***		−0.000 (0.000)
College or above		0.001 (0.000)**		0.000 (0.000)
Missing information		−0.000 (0.001)		0.006 (0.002)
Partner characteristics				
Unemployed *(Ref: Employed)*		−0.003 (0.001)**		0.000 (0.000)
In education		−0.005 (0.000)***		−0.004 (0.000)***
Level of education *(Ref: elementary school)*				
High school		0.001 (0.000)***		−0.000 (0.000
Vocational		0.002 (0.000)***		0.001 (0.000)*
Intermediate		0.004 (0.001)***		0.001 (0.001)
College or above		0.006 (0.000)**		0.003 (0.000)***
Missing information		−0.009 (0.000)***		−0.001 (0.001)
Intercept	0.001 (0.000)**	−0.001 (0.000)***	0.002 (0.000)***	0.001 (0.000)**
F-test	375.68***	275.30***	307.69***	223.73***
N, observations	**4,093,613**	**4,093,613**	**2,192,288**	**2,192,288**
N, individuals	**40,923**	**40,923**	**33,633**	**33,633**

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; † $p < 0.1$ These models are linear probability models applied to typical time-to-event data. This is a linear approximation of a typical standard discrete time duration model, which is commonly used in the literature. Thus, the OLS coefficients represent hazard rates. and they also include 26 time dummies, 13 region dummies and nine industry dummies. The standard errors are clustered at the level of the individual. The unemployment indicator covers all of the unemployed, *i.e.* not only those who were unemployed due to a firm closure. Very few number of cells where an individual is out of labour force is included in the reference category of employed in these models.

characteristics. This assumption, in turn, has two components. First, the assumption implies that regardless of whether the employees are subsequently unemployed, experiencing a firm closure does not directly affect fertility decisions. It may be assumed that our instrument has a direct effect on our outcome variables if the fertility outcomes do not differ between the two groups, both of whom experience the firm closure, but who differ regarding whether they are subsequently unemployed. Table A8 in the appendix tests this possibility crudely by comparing the fertility outcomes of those who became unemployed as a result of the firm closure and those who did not. The observed difference between the two groups regarding fertility outcomes indicates that unemployment resulting from firm closure, and not the firm closure per se, lowers fertility rates. The second component is related to the anticipation problem. In addition to the three groups specified above (never-takers, always-takers, and compliers), we now have a fourth group, the early-leavers, who foresee the closure months in advance and leave the firm. We usually fail to consider these departures as part of the firm-closing process, because they occur before we observe the firm closure in the data. The interpretation of our results will be affected if the early-leavers are systematically different from other employees. Schwerdt (2011) has suggested that this problem can be solved by including in the treatment group the individuals who left the firm during the two quarters preceding the firm closure. Table A9 reports the results from such a robustness check, and they are consistent with our main results.

Second, the monotonicity assumption implies that the instrument always moves individuals in the same direction. Hence, it may not be the case that the instrument pushes some individuals from employment to unemployment (these are the compliers), and others from unemployment to employment. This last group of individuals are the defiers, who would have become unemployed in the absence of the instrument, but who now, after being exposed to a firm closure, are more likely to find a job. We cannot rule out this mechanism entirely. If a large local firm closes, the local job centre may make an exceptional effort to help the affected workers, including the defiers. Moreover, because the job loss is seen as unrelated to worker characteristics, the firm closure may protect workers from the stigma of becoming unemployed, and improve their reemployment chances relative to those of workers with a job loss unrelated to firm closure. Still, both situations imply that potential new employers only observe whether the worker has experienced a firm closure and no other characteristics. This is not very realistic, and we claim that our IV model complies with the monotonicity assumption.

Eqs. 1 and 2 show the IV model.

$$unemployment_{it} = \alpha_{it} + \delta_1 x_{it} + \theta firm\ closure_{it} + r_{it} \quad (1)$$

$$fertility\ outcome_{it} = \alpha_2 + \delta_2 x_{it} + \beta \widehat{unemployment}_{it} + u_{it} \quad (2)$$

Table 3
Reduced-Form Models on the Timing of *Second Births*.

Variable	Men Coefficient (std.)		Women Coefficient (std.)	
Unemployment *(Ref: Employed)*	−0.003 (0.000)***	−0.000 (0.000)	−0.011 (0.000)***	−0.010 (0.000)***
In education		−0.001 (0.001)		−0.003 (0.001)***
Previous unemployment		0.001 (0.001)		0.007 (0.001)***
Married *(Ref: Single)*		0.003 (0.000)***		0.006 (0.000)***
Cohabiting		0.007 (0.000)***		0.006 (0.000)***
Level of education *(Ref: elementary school)*				
High school		0.002 (0.001)***		0.000 (0.0001)
Vocational		0.000 (0.000		−0.000 (0.000)
Intermediate		0.002 (0.000)***		0.001 (0.001)†
College or above		0.004 (0.000)***		0.003 (0.001)***
Missing information		0.000 (0.002)		−0.004 (0.003)
Partner characteristics				
Unemployed. *(Ref: Employed)*		−0.010 (0.000)***		0.001 (0.001)
In education		−0.004 (0.000)***		−0.001 (0.001)
Level of education *(Ref: elementary school)*				
High school		0.003 (0.000)***		0.001 (0.001)*
Vocational		0.001 (0.000)***		0.001 (0.000)**
Intermediate		0.005 (0.001)***		0.004 (0.001)***
College or above		0.006 (0.000)***		0.004 (0.001)***
Missing information		−0.001 (0.001)		−0.002 (0.001)
Intercept	0.005 (0.002)**	−0.004 (0.002)**	0.004 (0.001)**	−0.002 (0.001)
F-test	145.21***	120.24***	105.64***	95.44***
N, observations	**1,085,999**	**1,085,999**	**698,357**	**698,357**
N, individuals	**22,079**	**22,079**	**19,353**	**19,353**

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; † $p < 0.1$. These models are linear probability models applied to typical time-to-event data. This is a linear approximation of a typical standard discrete time duration model, which is commonly used in the literature. Thus, the coefficients represent hazard rates. The models also include 26 time dummies, 13 region dummies and nine industry dummies The standard errors are clustered at the individual level. The unemployment indicator covers all of the unemployed, *i.e.* not only those who were unemployed due to a firm closure. Very few number of cells where an individual is out of labour force is included in the reference category of employed in these models.

In both equations, it is the individual i (i = 1,…,N) at a specific point in time t (t = 1,…M). Eq. 1 is the first-stage equation, in which the endogenous variable ($unemployment_{it}$) is regressed on the vector of exogenous controls x_{it} and the instrument. The second stage uses the predicted rather than the actual value of the endogenous variable to predict the outcomes, along with the vector of controls, x_{it}. The random error terms are r_{it} and u_{it}. If the instrument is valid, the predicted value of the endogenous variable and the error term in the second-stage equation is uncorrelated, and the model produces consistent results. We cluster the standard errors at the individual level, reflecting the data structure where we observe each individual each month.

We identify firm closures following the standard definition in the Danish firm closure literature. The registers provide reliable yearly information on all private sector Danish firms, which allows us to determine whether each uniquely identified firm exists in November of each year. We can generally assume that a firm has closed down if it disappears from the data from one year to the next. However, the following changes are not considered firm closures: 1) the firm changes address, but has the same owner and continues to operate in the same industry; 2) the firm changes address, but has the same owner and the same employees; 3) the firm changes owner, but has the same employees and the same address; or, 4) the firm changes owner, but has the same employees and operates in the same industry. The registers define "same employees" as the continued engagement of at least 30 percent of employees from one year to the next.

However, to identify the causal effect of unemployment on conception, we need information on the month as well as the year in which the firm closed. We determine the month of closure by identifying the month in which the unemployment rate among the firm's employees increased by 50 percent or more relative to the preceding month. Finally, we restrict our investigation to closures of private firms with more than four employees.

Based on this definition, 11,778 of the 74,556 individuals (15.8 percent) in our first sample and 2992 of the 41,432 individuals (7.2 percent) in our second sample experienced one or more firm closures, and the majority (10,405 and 2811 individuals, respectively) experienced only one closure. While more of the individuals in our sample experienced firm closures than the individuals in the samples of previous studies,[16] we included more worker types in our sample. For example, Del Bono et al. (2014) only focused on "white collar women", and excluded specific industries such as tourism, agriculture and construction; and Browning and Heinesen (2012) only used information on full-time, high-tenure male workers with more than three years of firm-specific experience. Both groups are likely to experience fewer firm closures than the average employee. The average number of employees of the firms in our sample that closed is 222 (std. deviation 645.2).

3.3. Variables

3.3.1. Fertility measures

Our first outcome variable is a monthly indicator of the individual's timing of the first-child conception that may result in a miscarriage, live birth or still birth. While this indicator is not perfect—it does not, for example, provide us with information about abortions or intentions to conceive (both of which may be influenced by unemployment)—it is far

[16] In the sample used in previous studies, the shares of the individuals who experienced firm closures were as follows: Del Bono et al. (2012), 7.2 per cent; Huttunen and Kellokumpu (2016), 3.7 per cent; Browning and Heinesen (2012), around 5.6−8.4 per cent; and Browning et al. (2006), around 4.6 per cent.

Table 4
2SLS Models on First Births. *Second Stage, Outcome: First Births.*

	Men		Women	
Variable	**Coefficient (std.)**		**Coefficient (std.)**	
Endogenous regressor				
Unemployment	−0.015 (0.219)*	−0.011 (0.009)	0.105 (0.030)**	0.143 (0.043)**
Controls				
In education		−0.000 (0.000)		−0.004 (0.000)***
Previous unemployment		0.011 (0.008)		−0.161 (0.048)**
Married *(Ref: Single)*		0.008 (0.000)***		0.009 (0.001)***
Cohabiting		0.007 (0.000)***		0.010 (0.001)***
Level of education *(Ref: elementary school)*				
High school		0.000 (0.000)		0.001 (0.001)
Vocational		0.000 (0.009)		0.002 (0.002)*
Intermediate		0.001 (0.000)*		0.002 (0.001)*
College or above		0.000 (0.000)*		0.003 (0.001)**
Missing information		0.000 (0.001)		0.005 (0.002)*
Partner characteristics				
Unemployed *(Ref: Employed)*		−0.008 (0.000)***		−0.004 (0.001)**
In education		−0.005 (0.000)***		−0.004 (0.000)***
Level of education (ref: elementary school)				
High school		0.001 (0.000)***		0.000 (0.001)
Vocational		0.002 (0.000)***		0.001 (0.000)**
Intermediate		0.004 (0.001)***		0.002 (0.001)†
College or above		0.006 (0.000)***		0.003 (0.001)***
Missing information		−0.002 (0.001)**		−0.003 (0.002)
Intercept	0.001 (0.000)**	−0.001 (0.000)	0.003 (0.000)***	0.001 (0.000)**
F-test of model/centred R^2	364.26***/0.002	275.52***/0.009	146.22***/−0.106	121.32***/−0.167
N, observations	**4,093,613**	**4,093,613**	**2,192,288**	**2,192,288**
N, individuals	**40,923**	**40,923**	**33,633**	**33,633**

The standard errors are clustered at the individual level. Both the first and the second stage equations also include time, region and industry dummies. The first stage equation also includes the partner's characteristics. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; † $p < 0.1$. The results of the first stage equation can be found in Appendix Table A1.

more precise than the measures previously used in the literature. In particular, we have information about pregnancy duration, which allows for a precise estimate of the timing. In our sample, 32,104 individuals (43.06 percent) conceived their first child between ages 16 and 40, which corresponds to an average monthly conception rate of 0.004 for men and 0.006 for women.

Our second outcome is the timing of the second conception. In our sample, 20,885 individuals (50.4 per cent) conceived their second child between ages 16 and 40, which corresponds to an average monthly conception rate of 0.010 for men and 0.014 for women.

3.3.2. Explanatory variables and controls

Our key explanatory variable indicates whether the individual was unemployed in a given month. We created this indicator using information from unemployment benefit registers, and do not distinguish between those who were insured and uninsured. In the sample used for estimating the effect of unemployment on first births, 38,007 individuals (51.4 per cent) experienced shorter or longer unemployment spells during our observation period, and the total number of months of unemployment was 376,031.[17] In the sample used for estimating the effect of unemployment on second births, 13,250 individuals (31.9 per cent) experienced unemployment, and the total number of months of unemployment was 109,415. Of the 11,778 (first sample) or 2992 (second sample) individuals who experienced a firm closure in our two samples, only 10.1 per cent and 11.9 per cent, respectively, became unemployed as a result (the compliers).

Table 1 shows the mean values of all of the variables. Our control variables represent factors that we believe could affect the probability of conceiving a child, such as age, cohabitation, marital status, previous unemployment, educational participation (whether the individual was in school during any given month) and educational level.[18] We also control for the partner's unemployment status, educational participation, and educational level. Controlling for a partner's employment status may matter, since there is growing evidence of couple-simultaneity in unemployment (Gregg & Wadsworth, 2001), and such dual-joblessness may be correlated with childbearing decisions (Härkönen, 2011). Table 1 shows that in any given month, between

[17] The extent of unemployment in the two samples corresponds to average monthly unemployment rates of 5.98 per cent.

[18] We did not separately include a variable measuring the months in which the respondent/partner was "out of labor force" because in Danish context, being out of labor force is rare even for women. Denmark boasts one of the highest female labor force participation rates in the World. According to Statistics Denmark, less than 5% of a cohort are out of labor force at any given point in time. However, this figure becomes even smaller in our cohorts because we focus on the fertility window, as being out of labor force is rarer in younger ages. Including a separate category for such a small number of months would result in a sparse matrix to estimate its coefficient thus they are counted in the reference category.

Table 5
2SLS Estimations on Second Birth. *Second Stage, Outcome: Second Birth.*

	Men		Women	
Variable	**Coefficient (std.)**		**Coefficient (std.)**	
Endogenous regressor				
Unemployment	0.062 (0.016)	−0.012 (0.042)	0.047 (0.038)	0.059 (0.044)
Controls				
Married		0.003 (0.000)***		0.006 (0.000)***
Cohabiting		0.006 (0.001)***		0.007 (0.001)***
In education		−0.001 (0.001)		−0.004 (0.001)***
Previous unemployment		0.008 (0.023)		−0.051 (0.037)*
Level of education (ref: elementary school)				
High school		0.002 (0.001)**		−0.001 (0.001)
Vocational		0.000 (0.001)		−0.000 (0.000)
Intermediate		0.001 (0.001)		0.001 (0.001)†
College or above		0.004 (0.001)***		0.003 (0.001)***
Missing information		0.001 (0.002)		−0.007 (0.004)†
Partner characteristics				
Unemployed		−0.009 (0.000)***		−0.002 (0.002)
In education		−0.004 (0.000)***		−0.001 (0.001)
Level of education (ref: elementary school)				
High school		0.003 (0.000)***		0.000 (0.001)
Vocational		0.001 (0.000)***		0.001 (0.000)*
Intermediate		0.005 (0.001)***		0.003 (0.001)**
College or above		0.006 (0.000)***		0.002 (0.001)*
Missing information		−0.001 (0.001)		−0.002 (0.002)†
Intercept	−0.012 (0.009)	−0.004 (0.002)	0.007 (0.002)	−0.002 (0.002)
F-test of model/R2	67.15***/0.015	116.78***/0.004	73.82***/-0.018	60.72***/-0.0223

The standard errors are clustered at the individual level. Both the first and the second stage equations also include time, region and industry dummies. The first stage equation also includes the partner's characteristics. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; † $p < 0.1$. The results of the first stage equation can be found in Appendix Table A2.

three and 13 per cent of the partners were unemployed, while between four and seven per cent of the partners were in school. Most of the partners had vocational training as their highest educational attainment.[19]

To account for industry and county fixed effects, we include dummy variables for the industry[20] and the county of residence. All of these variables are time-varying at a monthly level.[21] Age captures our duration dependence, and we recode this into 24 binary variables, one for each year between age 16 and age 40. This means that we specify a flexible piecewise constant duration dependence.

4. Results

Although our main analyses rely on 2SLS models, we first present the results from the reduced-form models to enhance the comparability of our results with those from the previous literature, and to provide a benchmark for our preferred specifications (the 2SLS models).

4.1. Results from reduced-form models

Table 2 shows four linear probability models (LPM) of the association between our first outcome, the timing of first births, and unemployment. This specification resembles the model specification commonly used in the previous literature on fertility timing (e.g. Adsera, 2004; or for the Danish case Kreyenfeld & Andersson, 2014), i.e. the linear version of a typical discrete-time duration model estimated using logit link. Put differently, these OLS models are estimated on a data that is set in specific way which is typically used to estimate logistic regression –which follows every individual until they have a birth event or censored – thus the coefficients are interpreted similar to the hazard rates estimated using logistic regression. As can be seen in the table, the model without controls for men and both models for women show a negative association between unemployment and first births. However, there is no significant correlation for men when we include controls.

Table 3 below shows the results of the LPMs for the relationship between the timing (the probability) of a second birth and unemployment.

We again find a negative correlation between unemployment and fertility timing. Overall, there are indications of a negative association between unemployment and the probability of conceiving both a first and a second child for women as well as for men. However, the changes in the results for men that occur when we include controls illustrate the importance of considering the conditions of this correlation. Once adjusted for a set of observed characteristics the effect is not statistically significant anymore. Thus, we find no clear evidence of an effect for men, but we do find a negative effect for women; i.e., that women tend to delay having a first child when unemployed. Similar results were reported in the previous literature: e.g., for Danish women for the second birth, and for men for both the first and the second birth in Kreyenfeld and Andersson (2014).

These reduced-form models do not allow us to make any causal claims regarding the effect of unemployment on fertility behaviour, but serve mainly as a benchmark. Thus, we continue with the results from the 2SLS models, in which we instrument unemployment using firm closures.

4.2. Results from 2SLS MODELS

Table 4 reports the second stage results of 2SLS estimates. Again, we show four models: two without controls and two with controls. Table A1 in the appendix shows the first stage results. As we can see, our exclusion restriction, firm closures, increases the probability of unemployment for both men and women. The instrument is strong in all of the models.

[19] Note that all of the information on the partners is set to zero in the months in which the individual studied has no partner.
[20] For unemployed individuals, we assign the last known industry.
[21] The industry dummies are coded monthly, but they only vary at a yearly level in the registers.

Table 4 shows the coefficient of interest in our four models or the causal effect of unemployment on first births. Our findings align with the predictions of the static theory of fertility: i.e., for men, the coefficient of interest is negative and significant at a five per cent level when we do not include controls. While we may interpret this as being a negative income effect, this effect disappears when we include controls, suggesting that unemployment does not have an effect of first child conception. This is very similar to the reduced form results presented in the previous section. In contrast, we get a positive effect of unemployment on women's first births in the models with and without controls, which support the hypothesis on substitution effect. The sign of the coefficient of unemployment for women is the opposite of the sign we observed in the reduced-form models, which is not surprising, given that the 2SLS coefficients reflect the LATE.

Both the dynamic models and the sociological theories indicate that the substitution effect is stronger for first births than for second or higher order births. We now test this finding by focusing on second births. According to Table A2 in the appendix (first stage results), the instrument has reasonable power. Table 5 shows the second stage results from the 2SLS models: we find no effect of unemployment on the likelihood of conceiving of a second child for men or women.[22] This finding is robust to the in-/exclusion of controls. Thus, neither the income effect for men nor the substitution effect for women affects the conception of a second child.

Tables 4 and 5 also report the coefficients of the controls, which all follow the expected pattern: married and cohabiting individuals are more likely to experience a first and a second birth, and being in school has no effect for men but a negative effect for women. The partner's unemployment decreases the likelihood that both men and women will conceive, and this probability increases with the level of education. Finally, previous unemployment is irrelevant for men, but reduces the probability of conception for women.

4.3. Robustness checks

We apply a number of checks to test the robustness of our findings. First, since partner characteristics are potentially endogenous to unemployment, we tested the robustness of our results to the exclusion of partner variables. Appendix Table A3 shows that our findings are robust to this re-specification. Second, since fertility decisions are likely different for individuals with no partner, we tested the robustness of our results to the exclusion of months in which the individual is single. Appendix Table A4 shows that our findings are reinforced when we only rely on months in which the has a partner. Given that now, we have specifications on i) complete spells (single + married + cohabiting), ii) only on spells where there is a partner, as well as specifications on complete cells with and without partner characteristics, we can confidently claim and show that these variations do not change the sign or size of our coefficients. Our results remain robust to the selection of spells and specifications based on partnership status and characteristics.

Third, we present models with lags of up to three months to our instrument and to the month of unemployment in order to account for the potential time difference between the decision to conceive and the actual conception and to properly match the timing of unemployment to these events.[23] Tables A5 and A6 show the models for the timing of the first and the second birth, respectively. Each table reports only the coefficient of interest from the six models, with various lags on the instrument and on unemployment. Although the sizes vary, the coefficients have similar signs for "first birth" across all models. We also find consistent results for the second birth, except for women when we lagged our instrument and unemployment by three months. In this case, the coefficient is significant. In total, our results remain mostly immune to the inclusion of lags in our models.

The fourth robustness check addresses the potentially endogenous nature of previous unemployment. As we can see in Table A7 in the appendix, our models are robust to the exclusion of previous unemployment. Furthermore, after restricting our sample to include only individuals with one firm closure experience and then re-estimating models, our results, and especially the sign of our coefficients, remained the same (Table A8).

Fifth, firm closures may have effects on individual fertility behaviour other than through unemployment. But while the IV setup does not enable us to perform a straightforward test of whether the individuals who experience a firm closure without becoming unemployed exhibit fertility behaviour that differs from that of other individuals, we address this concern by re-estimating our OLS models on a reduced sample that only contains the 11,778 (first sample) and 2992 (second sample) individuals who experienced a firm closure. If the coefficients from unemployment in these models turn out to be insignificant, we can assume that the firm closure, rather than the unemployment, is likely to drive our results. Table A9 in the appendix reports these results, which show significant correlations between unemployment and both of our outcomes for women, and significant correlations between unemployment and first births for men. Overall, this is a good indication that our results are driven not only by the firm closures, but also the subsequent unemployment.

Finally, as we mentioned above, we test whether the presence of early-leavers compromises our results. Table A10 in the appendix shows the results when we expand our treatment group to include individuals who left the firm in the six months before the firm closed. First, it should be noted that this inclusion weakens our instrument, probably because only a few of the early-leavers experienced unemployment. For women, the instrument is so weak that it compromises the reliability of our results; thus, we refrain from interpreting them. However, for men, the instrument is sufficiently strong and produces results that are relatively similar to our original results; with the exception that the negative coefficient for first births is now significant at the five per cent level. In light of the other robustness checks discussed above, this is not surprising; and we conclude that the presence of early-leavers does not jeopardise our results.

5. Discussion and conclusions

Our study shows that experiencing unemployment has a positive causal effect on first birth conceptions for women. We did not find statistically significant effect for men on the first birth conceptions, additionally, no effect on second birth conceptions for men or women. These findings are consistent with the predictions of the static neoclassical model, which assumes that people follow traditional gender roles, and thus that the time costs associated with childbearing and childcare only apply to women.

Our results may seem surprising given the similarities in the labour market behavioural and life course patterns of Danish men and women (e.g., Esping-Andersen, 2007). Yet sociological arguments about the social norms against childlessness and the dynamic models of fertility predict that women will transition to motherhood more quickly during phases of unemployment. Moreover, it is possible that since Denmark had a low unemployment rate, and a small share of those unemployed were long term unemployed overall during the period covered, most of

[22] The coefficients in the Table 5 models represent marginal effects expressed as hazard rates.

[23] This strategy does not change the composition of the treated group (i.e. We do not expand the treatment group to include individuals who conceived up to three months before the firm closed.).

the Danish women we studied saw their unemployment status as temporary, as suggested by Adsera (2011). In light of these factors, and given the generosity of unemployment benefits, it seems plausible that Danish women would consider having a child while unemployed following a firm closure. After all, in the Danish context, the low cost of childrearing may attenuate any negative income effect of unemployment. This interpretation seems to be further supported by previous research that showed that among Danish women, income losses due to childbirth tend to be small (Gupta & Smith, 2002).

We acknowledge that we are unable to distinguish the effect of income loss from the effect of uncertainty induced by unemployment. This is because our main focus has been assessing the causal effect of unemployment as a first step, and our design and data did not allow us distinguishing these two effects. One could consider including income as a control variable in the modelling strategy, however, we are uncertain that this would help us separating income effect from uncertainty effect completely, as income is an endogenous variable. There is a need for explicitly and separately modelling income uncertainty, which may only be partially correlated with "current income". The definition of income changes during and before unemployment and "current income" as a measure would be a poor proxy for resources when it comes to affect time decisions involving future outcomes. Separately modelling income uncertainty from employment uncertainty would be an interesting and fruitful direction for future research.

It is reassuring that we do not find any statistically significant effect of unemployment on the likelihood of conceiving a second child, as this finding confirms the predictions of the theoretical arguments, and highlights the importance of distinguishing between the probability of having a first or a second birth, rather than using the total number of births in the years immediately following a firm closure (e.g. Del Bono et al., 2014). Moreover, these results have been proven robust to a number of alternative specifications and checks.

These findings contribute to broader literature that describes the relationship between labour market uncertainty and fertility behaviour. Unemployment is the most common and the most relevant type of labour market uncertainty, especially in European countries over the past decade, especially after the Great recession and now entering to post-pandemic recession period. Although many studies have explored the association between unemployment and fertility at the individual level, they did not use a causal approach, and their findings have been mixed at best. To explain these mixed findings, some studies have explored group differences in the association between unemployment and fertility, by education, age, etc. (e.g. Kreyenfeld, 2009; Ozcan et al., 2010; Kreyenfeld & Andersson, 2014). These studies have found that the coefficient for unemployment often changes sign across different education groups. Although this is a useful observation that illustrates the importance of the heterogeneity of the unemployed, these reduced-form models may be of limited utility, since the endogeneity of unemployment itself may vary by education. For example, it is possible that low educated women are less attached to the labour market, and are more likely to become unemployed when they plan to start a family. Thus, we believe the research should carefully consider type of heterogeneity and embed it in causal designs rather than providing heterogenous associations.

Nevertheless, to provide a benchmark for this relationship, we performed a similar reduced-form analysis with our data. The signs of the coefficients for women changed when we moved from average associations to our causal specifications that rely on an instrumental variable strategy, supported by industry- and county-level fixed effects. Yet this result is not surprising, as our conclusions can be generalised only to those who were unemployed specifically due to a firm closure. In other words, we restrict the interpretation of our results to the limits of the local average treatment effect (LATE). The validity of our instrument has been tested on other outcome variables from the previous literature; it is well-accepted and has been previously applied in the Danish context.

Our findings are overlap with some of the findings in the previous literature. First, in Danish context, our associations are similar to one reported for high educated women in Denmark by Kreyenfeld and Andersson (2014). However, once we consider causal relationship the effect of unemployment turns positive (similar to their findings for low educated women). It is plausible that unemployment due to firm closures more likely to occur among low educated women or their associations for high educated women are driven by unobserved characteristics of these women in Denmark, which make them more likely to be unemployed and also become mothers. Among the studies with more causal designs, the results for men in our study are consistent with Del Bono et al. (2014) and Huttunen and Kellokumpu (2016), etc. Regarding women, the former finds positive (though not significant) effect of unemployment due to firm closures on Austrian women's fertility, which is largely consistent with our findings. They suggest that any effect on fertility could be attributable to employability (a factor related to human capital levels). Huttunen and Kellokumpu (2016) finds that unemployment during recession has a negative effect on Finnish women's fertility however, this effect is moderated by the parnter's characteristics. There are stark differences in Denmark, Finland and Austria and the sample of men and women considered in these studies making the comparison difficult. However, we contribute to this growing literature, which provides causal evidence from different settings to better understand how unemployment, an ever important phenomennon will further effect family formation processes.

While doing so, our data provide the most precise measures of "fertility timing" and "conceptions" in the literature, as they incorporate previously ignored birth outcomes such as stillbirths, and they allow us to identify pre-term births through the use of information on the exact duration of pregnancy. Importantly, reducing the measurement error for conception timing in our empirical analysis brings us a few steps closer to the theoretical arguments, which are concerned with "conception decisions" during phases of unemployment, rather than with "live birth outcomes". Still, it is possible that our treatment sample of individuals who became unemployed after their firm closed is a heterogeneous group. Future research can extend our analyses to explore such heterogeneity by differentiating the effect of unemployment due to a firm closure by, for example, educational level, specific worker type or industry sector.

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Appendix A

Table A1
First stage results of the 2SLS models on first births (reported in Table 4).

First Stage, Outcome: The Likelihood of Unemployment				
	Men		**Women**	
Variable	**Coefficient (std.)**		**Coefficient (std.)**	
Instrument				
Firm closure	0.037 (0.003)***	0.032 (0.002)***	0.022 (0.003)***	0.017 (0.003)***
Controls				
Married		0.005 (0.001)***		−0.004 (0.002)†
Cohabiting		−0.011 (0.001)***		−0.007 (0.002)**
In education		−0.002 (0.001)*		0.008 (0.001)***
Previous unemployment		0.934 (0.009)***		1.110 (0.014)***
Level of education (ref: elementary school)				
High school		−0.018 (0.001)**		−0.021 (0.002)***
Vocational		−0.007 (0.001)**		−0.015 (0.002)***
Intermediate		−0.028 (0.002)***		−0.017 (0.003)***
College or above		−0.010 (0.002)***		−0.016 (0.003)***
Missing information		0.028 (0.007)***		0.011 (0.011)
Partner characteristics				
Unemployed		0.017 (0.001)***		0.030 (0.003)***
In education		−0.004 (0.001)**		0.001 (0.002)
Level of education (ref: elementary school)				
High school		0.003 (0.002)		−0.003 (0.003)
Vocational		−0.001 (0.001)		−0.004 (0.002)*
Intermediate		0.007 (0.003)**		−0.005 (0.004)
College or above		0.008 (0.002)***		0.004 (0.003)
Missing information		0.019 (0.007)**		0.010 (0.010)
Intercept	−0.000 (0.002)	0.005 (0.002)*	−0.007 (0.002)**	−0.004 (0.002)
F-test of excluded instruments	205.68***	173.71***	44.06***	30.13***
F-test of model/R^2	376.98***/ 0.0308	691.22***/ 0.130	245.49/ 0.031	396.92**/ 0.142

*** p < 0.001; ** p < 0.01; * p < 0.05; † p < 0.1. Note: The specification includes.

Table A2
First stage results of the 2SLS models on second births (reported in Table 5).

First Stage, Outcome: The Likelihood of Unemployment				
	Men		**Women**	
Variable	**Coefficient (std.)**		**Coefficient (std.)**	
Instrument				
Firm closure	0.031 (0.005)***	0.026 (0.005)***	0.041 (0.008)***	0.036 (0.008)***
Controls				
Married		−0.000 (0.001)		−0.003 (0.002)
Cohabiting		−0.019 (0.003)***		−0.008 (0.004)†
In education		0.018 (0.004)***		0.019 (0.006)**
Previous unemployment		0.548 (0.016)***		0.845 (0.016)***
Level of education (ref: elementary school)				
High school		−0.006 (0.003)**		0.011 (0.005)**
Vocational		−0.006 (0.002)**		0.001 (0.004)
Intermediate		−0.024 (0.003)***		−0.018 (0.006)
College or above		−0.016 (0.002)***		−0.008 (0.004)†
Missing information		0.020 (0.016)		0.041 (0.026)*
Partner characteristics				
Unemployed		0.009 (0.001)***		0.039 (0.005)***
In education		0.000 (0.002)		−0.002 (0.005)
Level of education (ref: elementary school)				
High school		−0.001 (0.002)		0.014 (0.005)**
Vocational		−0.000 (0.002)		0.003 (0.003)*
Intermediate		0.001 (0.003)		0.011 (0.005)*
College or above		0.000 (0.002)		0.018 (0.004)***
Missing information		0.000 (0.009)		0.010 (0.013)
Intercept	−0.012 (0.009)	−0.018 (0.009)*	−0.042 (0.015)**	−0.001 (0.014)
F-test of excluded instruments	39.17***	32.62***	25.21***	22.19***
F-test of model/R^2	67.15***/ 0.0146	52.13***/ 0.098	136.25/ 0.067	139.034**/ 0.159

*** p < 0.001; ** p < 0.01; * p < 0.05; † p < 0.1.

Table A3
Excluding partner variables (F is the value of the F-test of the excluded instrument).

	Men	**Women**
First birth	−0.011 (F = 173.33***)	0.141** (F = 30.52***)
Second birth	0.059 (F = 22.19***)	0.009 (F = 32.45***)

The standard errors are clustered at the individual level. Both the first and the second stage equations also include time, region and industry dummies. The first stage equation also includes the partner's characteristics. *** p < 0.001; ** p < 0.01; * p < 0.05; † p < 0.1.
***: p < 0.001 **: p < 0.01, *: p < 0.05, †: p < 0.10.

Table A4
Excluding months in singlehood (F is the value of the F-test of the excluded instrument).

	Men	N, obs.	N, ind.	Women	N, obs.	N, ind.
First birth	−0.033 (F = 55.62***)	1,430,518	29,414	0.179** (F = 21.55***)	8,81,932	21,506
Second birth	−0.017 (F = 28.93***)	972,838	23,371	0.066 (F = 23.46***)	5,82,743	18,170

The standard errors are clustered at the individual level. Both the first and the second stage equations also include time, region and industry dummies. The first stage equation also includes the partner's characteristics. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; † $p < 0.1$.

Table A5
First birth (F is the value of the F-test of the excluded instrument).

	Men	Women
Lagged instrument (1 month)	−0.017†(F = 157.93***)	0.119**(F = 37.91***)
Lagged instrument (2 months)	−0.022*(F = 131.96***)	0.105**(F = 46.55***)
Lagged instrument (3 months)	−0.014(F = 108.69***)	0.056*(F = 53.76***)
Lagged instrument and lagged unemployment (1 month)	−0.016†(F = 179.87***)	0.134**(F = 31.77***)
Lagged instrument and lagged unemployment (2 months)	−0.019*(F = 187.06***)	0.122** (F = 37.81***)
Lagged instrument and lagged unemployment (3 months)	−0.010*** (F = 196.12***)	0.066* (F = 44.11***)

***: $p < 0.001$, **: $p < 0.01$, *: $p < 0.05$, †: $p < 0.10$. The standard errors are clustered at the individual level. All specifications include the same set of control variables as indicated in Table 4. Both the first and the second stage equations also include time, region and industry dummies.

Table A6
Second birth.

	Men	Women
Lagged instrument (1 month)	−0.047 (F = 31.48***)	−0.020 (F = 29.33***)
Lagged instrument (2 months)	−0.046 (F = 30.66***)	−0.023 (F = 32.40***)
Lagged instrument (3 months)	−0.016 (F = 26.74***)	−0.056 (F = 34.57***)
Lagged instrument and lagged unemp (1 month)	−0.045 (F = 33.62***)	−0.023 (F = 24.49***)
Lagged instrument and lagged unemp (2 months)	−0.043 (F = 35.06***)	−0.027 (F = 27.15***)
Lagged instrument and lagged unemp (3 months)	−0.013 (F = 35.99***)	−0.063* (F = 30.84***)

***: $p < 0.001$, **: $p < 0.01$, *: $p < 0.05$, †: $p < 0.10$. The standard errors are clustered at the individual level. All specifications include the same set of control variables as indicated in Tables 5. Both the first and the second stage equations also include time, region and industry dummies.

Table A7
Excluding previous unemployment.

	Men	Women
First birth		
OLS		
Unemployment	0.000 (0.000)	−0.005 (0.000)***
IV		
Firm closure	0.036 (0.003)***	0.021 (0.003)***
F-test for instrument	197.69***	38.54***
Unemployment	−0.010 (0.008)	0.116 (0.034)***
Second birth		
OLS		
Unemployment	−0.000 (0.000)	−0.010 (0.000)***
IV		
Firm closure	0.030 (0.009)***	0.040(0.008)***
F-test for instrument	39.62***	24.48***
Unemployment	−0.010 (0.035)	0.053 (0.039)

***: $p < 0.001$, **: $p < 0.01$, *: $p < 0.05$, †: $p < 0.10$ All specifications include the same set of control variables as indicated in Tables 4 and 5.

Table A8
Re-estimating the models on a sample that excludes observations of individuals who experienced more than one firm closure.

	Men	Women
First birth		
OLS		
Unemployment	−0.000 (0.000)	−0.005 (0.000)***
IV		
Firm closure	0.035 (0.003)***	0.021 (0.004)***
F-test for instrument	143.68***	32.65***
Unemployment	−0.021 (0.009)*	0.141 (0.041)**
Second birth		
OLS		
Unemployment	−0.000 (0.000)	−0.011 (0.000)***
IV		
Firm closure	0.026 (0.006)***	0.040 (0.010)***
F-test for instrument	19.98***	17.07***
Unemployment	−0.013 (0.050)	0.030 (0.045)

The specifications include same controls as in Table 3 and 4 respectively. ***: $p < 0.001$, **: $p < 0.01$, *: $p < 0.05$, †: $p < 0.10$.

Table A9
Comparing the outcomes of individuals who did and did not become unemployed after experiencing a firm closure, OLS.

	Men	Women
First birth	−0.002 (0.001)*	−0.004 (0.002)*
Second birth	−0.000 (0.004)	−0.008 (0.003)*

***: $p < 0.001$, **: $p < 0.01$, *: $p < 0.05$, †: $p < 0.10$. The estimated model specifications are the same as in Tables 2 and 3.

Table A10
Accounting for early-leavers (IV models only).

	Men	Women
First birth		
Firm closure	0.015 (0.002)***	0.007 (0.002)**
F-test for instrument	65.47***	8.75*
Unemployment	−0.029 (0.014)*	0.243 (0.101)*
Second birth		
Firm closure	0.010 (0.003)**	0.010 (0.006) †
F-test for instrument	10.06**	3.17†
Unemployment	0.065 (0.087)	0.179 (0.152)

***: $p < 0.001$, **: $p < 0.01$, *: $p < 0.05$, †: $p < 0.10$.

Table A11
Additional Summary Statistics of the Sample.

Sample, first births			
	Full cohort, right censored at first birth	Excluding months in public sector	Excluding months in small firms
Observations	16,719,719	8,713,793	6,285,901
Individuals	87,333	81,894	73,918
Min obs per indv.	24	1	1
Ave. obs pre indv.	1914	1064	85
Max obs per indv.	300	300	288
Sample, second births			
	Full cohort, right censored at second birth	Excluding months in public sector	Excluding months in small firms
Observations	4,104,918	2,131,561	1,784,356
Individuals	63,005	46,372	41,432
Min obs per indv.	1	1	1
Ave. obs pre indv.	652	46	431
Max obs per indv.	277	264	253

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