Unemployment Insurance for Becoming Mothers: Protecting Newborn Health if Fathers Can´t?

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Abstract

It is well-documented that the unemployed who receive more generous unemployment insurance benefits (UIB) have better health than the unemployed with less generous benefits. Yet, three important aspects of the association between UIB and health remain unknown. First, even though such knowledge is crucial for policy design, it is unclear how much more generous benefits improve the health of the unemployed. Second, despite fundamental changes in household economics in the last decades, we know little about the extent to which UIB health effects depend on the economic resources of the partners of the unemployed. Third, the detrimental health consequences of unemployment are known to extend to family members. However, it is yet to be shown that UIB protects the health of those close to the unemployed.

Drawing on a quasi-experimental variation of the potential benefit duration (PBD) in Switzerland and uniquely tailored administrative data, this study addresses these knowledge gaps by asking a) whether shorter UIB reduce the birth weight of children of unemployed women and b) whether such effect are buffered by the fathers' income. We find that the reduction of PBD from 400 to 260 daily allowances reduced children's birth weight by more than 130 grams if their fathers had incomes below subsistence levels, but had no effect on all other newborns. The study shows that extended access to UIB protects the health of the unemployed and their children in households who lack alternative economic resources.

Introduction

Research on the welfare effects of unemployment insurance (UI) has generated abundant evidence on the moral hazard related to UI (e.g. Chetty, 2008). Because more generous UI benefits ease the financial consequences from unemployment, the unemployed reduce their search efforts and remain inactive for longer periods. Quasi-experimental evidence has confirmed the relationship between more generous UI systems and prolonged unemployment in diverse contexts (Schmieder and Wachter, 2016). Yet, finding the optimal design of UI not only requires understanding the negative implications of UI on labor supply, but also its benefits. Given the adverse impact of job loss on individuals' well-being and health (Brand, 2015), a growing body of research has therefore begun to consider the health effects of UI (O'Campo et al., 2015; Renahy et al., 2018).

The bulk of this literature suggests that more generous UI schemes improve the health of the unemployed. Unemployed who received more generous benefits reported elevated levels of self-rated health (Cylus and Avendano, 2017; Cylus et al., 2014a; Kuka, 2019; Shahidi et al., 2019), life satisfaction and mental health (Cylus et al., 2014b; Tefft, 2011; Voßemer et al., 2018; Wanberg et al., 2019; Young, 2012). Furthermore, as compared to unemployed that receive less generous benefits, they show improved health behaviors such as reduced drinking (Bolton and Rodriguez, 2009),[1] smoking (Fu and Liu, 2018) and increased physical activity (Cylus, 2017) and take-up of medical services (Kuka, 2019).

Despite the breadth of the literature on the health benefits of UI, we identify three persistent research gaps. First, conclusions largely rely on health differences between unemployed with more versus less generous UI benefits (Bolton and Rodriguez, 2009; Cylus and Avendano, 2017; O'Campo et al., 2015; Renahy et al., 2018; Shahidi et al., 2019; Wanberg et al., 2019; Young, 2012). On the one hand, it seems likely that unemployed in bad health conditions more often claim benefits given their greater difficulties in re-entering employment. On the other hand, it could be that certain characteristics (e.g. knowledge of social insurances) are related to both better health and more frequent take-up of benefits. Hence, whether previous studies just reflect selection into UIB receipt or whether more generous UI benefits remains really improve health remains an open question (Hillier-Brown et al., 2019).

Second, previous research has not sufficiently explored *who* actually benefits from UI (Renahy et al., 2018). It has suggested that lowly educated (Cylus, 2017; Fu and Liu, 2018) as well as unemployed in low income households (Shahidi et al., 2019) reap greater health benefits from UI, but effects have been found to not differ by age or gender (Cylus et al., 2014b). Given women's greater labor market involvement (cf. Gorbachev, 2016), changes in couples' resource sharing practices (Bennett, 2013) and increases in single households (Alm et al., n.d.; Seltzer, 2019), the extent to which unemployed can rely on the economic resources of partners to overcome the financial consequences of unemployment has

[1] Some studies find no relationship between UI generosity and risky behaviors (Kuka, 2019) and others even find a positive one (Lantis and Teahan, 2018).

become much more diverse. Understanding the extent to which UI health effects differ by level of partner economic resources is thus a major knowledge gap.

Third, it is well-known that the detrimental effects of unemployment spill over to the family members of the unemployed. For instance, job loss reduces mental health of spouses (Marcus, 2013) and the birth weight of children from affected families (Lindo, 2011). A crucial question is thus whether beneficial health effects of UI extend beyond the unemployed. Focusing on newborn health, Hoynes et al. (2015) have shown that increases in transfer incomes reduce the share of low weight births among low income families in the US. However, it remains unclear whether transfer incomes also improve birth weight among more socio-economically diverse populations - e.g. among the unemployed - and in contexts with widely accessible, publicly subsidized health care systems (cf. Hillier-Brown et al., 2019).

The present study addresses these research gaps by asking whether the 2011 reduction of the potential maximum duration of unemployment benefits (PBD) in Switzerland reduced the birth weight of children of unemployed mothers. We employ a difference-in-differences (DID) design where we compare the difference in birth weight between newborns affected by the reform and newborns in an unaffected control group before and after the legislative change. Focusing on the combined health of unemployed mothers and their newborns, our study thus situates itself in a recent literature that leverages quasi-experimental designs to estimate causal health effects of UI[2] (Cylus et al., 2014a, 2014b; Fu and Liu, 2018; Kuka, 2019; Tefft, 2011; Voßemer et al., 2018). Furthermore, by assessing the variance of effects by father income, we quantify the extent to which partner economic resources matter for health effects of UI. In view of the higher average household incomes, the higher minimum social benefits and universally subsidized health care among our Swiss study population as compared to the previously studied, low income populations in the US, we expect less detrimental health effects from a loss of UI benefits. Finding effects would therefore strongly corroborate earlier North american evidence on UI health effects and the role of public transfers in protecting newborn health.

We continue by describing the theoretical links between economic circumstances during pregnancy and fetal development, the specifics of the Swiss UI scheme, the 2011 reform and our study design. Then, we describe our data and methodology. Finally, we present and interpret our main results and derive conclusions for future research and policy.

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[2] Most of these studies draw on state-level changes in UI generosity in the US to estimate jurisdiction-fixed-effects of UI on health (Cylus et al., 2014a, 2014b; Fu and Liu, 2018; Kuka, 2019).

Theory and study design

Economic circumstances during pregnancy and fetal development

It is well-established that maternal socio-economic disadvantage is related to less favorable pre-natal child development. The assumed underlying mechanism is that greater economic resources (i.e. income or wealth) both reduce the fetus' exposure to harmful factors (e.g. certain hormones, toxins, noise) and increase the fetus' exposure to developmental promoters (e.g. nutrients) (Aizer and Currie, 2014). Drawing on linkable large-scale birth registers and quasi-experimental methods, the literature has now shown several of the mechanisms that make up the socio-economic gradient in newborn health. Given that low financial resources is strongly related to stress and that stress negatively affects fetal development, maternal stress is one such possible pathway (Aizer, 2011; Brown, 2018; Lima et al., 2018). Another set of mechanisms could be behaviors related to economic hardship such as smoking (Bharadwaj et al., 2014), drinking (Barreca and Page, 2015) or bad nutrition (Rossin-Slater, 2013). Other studies have suggested that the association could be established by adverse maternal health. For instance, socio-economically disadvantaged mothers are more likely to get influenza, which has detrimental effects on fetal development (Evans and Garthwaite, 2014; Schwandt, 2018). Ultimately, in contexts where environmental quality is strongly segregated along socio-economic lines, air or water pollution could further explain the association (Currie et al., 2013).

These results have motivated a number of studies that addressed the effectiveness of policy programs aimed at buffering the relationship between low SES and adverse birth outcomes. It has been established that targeted socio-medical programs such as the US Food Stamps program (SNAP) (Almond et al., 2010) and the Nurse Family Partnership (Eckenrode et al., 2010) have reduced the incidence of low weight births. So far, however, there is only limited evidence on the prenatal impact of general cash transfers. Using changes in the Earned Income Tax Credit scheme as a source of exogenous variation in income, Hoynes et al. (2015) have shown that a $1000 increase in cash transfers reduced the rate of low weight births by 2 to 3 percentage points (Hoynes et al., 2015). They interpreted their results as a mix between higher use of prenatal care and improved health behavior that resulted from pregnant women's greater financial resources. Brownell et al. (2018) have shown that an income supplement for pregnant mothers might have reduced socio-economic inequalities in pre-term births, birth weight and breast-feeding initiation. However, their cross-sectional estimates might be biased since their results also show that transfer recipients differ from non-recipients both in ways which are related to better (e.g. pre-natal risk screening) and worse child health (e.g. single parenthood).

Unemployment insurance in Switzerland and the 2011 reform

UI systems can be characterized in terms of access requirements, pre-unemployment income replacement levels and the potential maximum duration benefits can be claimed (PBD). In Switzerland, access to UI benefits depends on reasons for job loss, willingness to work and paid UI taxes in the two years prior to registration at public employment services (PES). Benefits are paid for both involuntary and voluntary job losses, yet after a waiting

period of up to one month in the latter case. Willingness to work is enforced with a minimum number of applications recipients need to prove and by obligation to participate in employment programs. To meet contribution requirements, workers need to have paid UI taxes during at least one year in the two years prior to claiming benefits[3]. Replacement levels reach 80% of insured pre-unemployment wages (capped at an annual CHF 148'200) if the unemployed has maintenance obligations towards children below age 25 and 70% otherwise. In terms of access requirements and replacement levels, Swiss UI can be classified as generous in the OECD comparison (OECD, 2018).

PBD in the Swiss UI scheme was lowered in a 2011 reform[4] (Devaud and Keller, 2012). The biggest sub-population affected by the reform were unemployed aged 25 to 54 with incomplete contribution histories (henceforth the *treated*). In contrast to unemployed aged 25 to 54 that paid contributions during 12 to 17 months and registered at PES between July 2003 and March 2009 (henceforth the *pre-reform period*), unemployed aged 25 to 54 that paid contributions during 12 to 17 months and registered at PES in April 2011 or later (henceforth the *post-reform period*) were entitled to a maximum of *only 260 instead of the 400 daily allowances in the pre-reform period*[5] (SECO, 2013). Unemployed who have contributed UI taxes during at least 18 months prior to registering at PES had a maximum of 400 daily allowances, both, before and after the reform (henceforth the *controls*). Unemployed are entitled to claim their maximum of daily allowances within the first 24 months after all access requirements are met for the first time (during the so called "Rahmenfrist Leistungsbezug", henceforth the *claiming period*). Under continuous reciept, benefits are exhausted after 12 months for the treated in the post-reform period and after 18 months for the treated in the pre-reform period and for the controls in both periods.

The expected effect of the reform on birth weight

We now restrict our focus to pregnant women among the treated and controls. Pregnant women are faced with two specific rules when claiming UI benefits. First, all pregnant women are exempted from job search requirements in months seven to nine of pregnancy, i.e. are entitled to benefits even if not actively looking for a job. Second, if claimants were

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[3] Workers are exempted from contribution requirements if they recently finished education, gave birth to a child, were sick, had a divorce or separation or finished a prison sentence.

[4] The reform was motivated with financial difficulties encountered by the Swiss UI scheme (Devaud and Keller, 2012).

[5] Unemployed that started their claiming between March 2009 and March 2010 were entitled to a maximum between 400 and 260. Unemployed that started their claiming period between April 2010 and March 2011 were entitled to 260 maximum benefits. We exclude them from our treated sample in the post-reform period, becauseat the start of their claiming period, they did not know that they would have only 260 daily allowances because the law change was only introduced in September 2010.

restricted in their employability by care obligation for children below 10, the maximum claiming period is extended by 24 months to a maximum of 48 months.

A first mechanism by which the reform has potentially affected fetal growth was by changing pregnant women's household incomes (henceforth the *income effect*). The income effect could have taken three forms. First, for those women still on job search after having exhausted 260 daily allowances, the reform led to a loss benefits. The loss of benefits took effect at earliest in month 12 until up to month 24 in the claiming period for women who did not claim their daily allowances continuously and who had *no other minor children* and at earliest after month 12 until up to month 48 in the claiming period for women who did not claim their daily allowances continuously but who had *other minor children*. Second, faced with a shortened PBD, it seems possible that pregnant women have increased their job search efforts and showed an increased tendency to accept job offers and to increase their incomes. Third, we expect that the children's fathers have reacted to the loss of benefits by increasing their work hours or by taking-up better paid jobs. By increasing household income, these maternal and paternal income effects thus could have counteracted the loss of benefits. However, if maternal and paternal income effects did not fully compensate the loss of benefits, the reform has led to a reduction of incomes of the households of treated pregnant women. In this case, we expect the reform to have resulted in *reduced consumption levels and increased financial difficulties* experienced by pregnant women, and hitherto, greater stress, health problems and negative health behaviors (cf. Kuka, 2019). These circumstances are likely to have slowed down fetal growth.

However, by affecting maternal and paternal labor supply, the reform could have also affected fetal growth due to increased stress from work pressure (henceforth the *employment effect*). Greater work hours by mothers might have increased the fetuses' exposition to stress hormones and toxins. Similarly, greater paternal work hours could have reduced emotional and practical support received by pregnant women, thereby increasing maternal stress and negative effects on fetal growth. In sum, it could be that even though the reform has not increased financial difficulties for unemployed women's households (or even if it has lowered them), it has still had negative effects on birth weight due to the detrimental effects of greater work pressure.

We expect that the size of income and employment effects to vary with the pregnant women's economic household situation (Shahidi et al., 2019). For women in households with abundant alternative economic resources, a loss of benefits does not require cuts in consumption expenses and does not lead to greater financial hardship. In consequence, neither pregnant women nor the children's fathers need to adapt their employment behavior. Conversely, if unemployed pregnant women cannot draw on alternative income or wealth, the loss of benefits will lead to cuts in consumption, increases in financial difficulties and induce labor market reactions by the pregnant women and the children's fathers. We expect that the main alternative economic resources available to unemployed, pregnant women are the children's fathers' incomes. Also, we expect that the critical level of father income is the income fathers need to provide for their own existence. Subsistence levels in Switzerland as defined by eligibility threshold of need-based social assistance (the social safety net of last resort) lay around CHF 2500/month. We take this benchmark as the level of father income below which pregnant women cannot count on economic support

from the children's fathers and above which we expect less detrimental effects of the reform on birth weight.

The study design

From these considerations we derive our basic study design. We apply a *difference-in-differences* methodology: we assess the average treatment effect of the reform with the pre-to-post-reform difference in the birth weight differences between the children of pregnant women among the treated and the children of pregnant women among the controls. The reduction of benefits for the post-reform treated took place at earliest in month 12 in the claiming period. We assume that most women who became pregnant during unemployment have exhausted 260 daily allowances after 24 months, given their difficulties at re-entering employment. On this backdrop, we define the critical time window in the claiming period as *month 12 to 24 in the claiming period (henceforth our main window)*. To preclude overlap of pre-reform claiming spells with the post-reform regime, we restrict pre-reform observations to claiming periods that started at least 24 months before the reform (at latest March 2009).

Previous studies have shown that fetal growth is most responsive to environmental influences in later stages of pregnancy (Almond et al., 2010; Currie and Maya, 2013), the so called sensitive fetal developmental stage. We define the *sensitive developmental stage as the last 3 months before birth*[6] and restrict our treatment and control groups to *children whose sensitive fetal developmental stage overlapped with the main window* during at least one month.

Methods

Data sources and study samples

To construct our study sample, we use individual-level data from several administrative data sources. Information on the unemployed is drawn from the unemployment insurance register (UIR) (Bundesrat, 2006). This data source provides information on starts of claiming periods and number of months with paid contribution and potential maximum duration, thereby allowing us to neatly define the treated and controls and check the validity of the treatment. Moreover, it includes information on pre-unemployment educational attainment, occupational class, income[7] and employment level of the

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[6] Our conclusions hold when we use the second trimester of pregnancy as the sensitive developmental stage but change (we find no effects) when we use the first trimester of pregnancy.

[7] The income that is considered by PES case workers when calculating the level of UI benefits.

unemployed, which allows us to account for potential divergences in pre-to-post-reform trends in these characteristics between treated and controls.

For information on birth timing, birth characteristics, further information on the studied couples (citizenship, marital status at birth) and to identify fathers, we merge information from UIR to national birth registers (BEVNAT) (FSO, 2019a). We use social security identification numbers (SSI) as keys for the merge. SSI are encrypted by the federal statistical office for research purposes. SSI are only directly available in births registered in BEVNAT 2010 or later. To identify parents of previous births, we link BEVNAT files to registers of population and households (STATPOP) of 2010 and later (FSO, 2019b), which contain SSI of all Swiss residents. We use unique combinations of maternal or paternal birth dates, marriage dates[8] and child birth dates[9] as pseudo-identifiers and exclude cases with non-unique combinations. With this method, we are able to identify SSI of both parents for all but 19.2 percent of all births registered in Switzerland between 2007 to 2009[10].

We measure the receipt of UI benefits by mothers and the couples' incomes with individual accounts from the Central Compensation Office (CCO) (CCO, 2018). CCO data covers all incomes from dependent employment, from self-employment and UI in the whole study period. To correct for trends in wages and prices, we adjust all monetary values to December 2014 price levels.

Using merged information from all these data sources, we construct our final analytical sample. For observations in the pre-reform period, we use mothers whose claiming period started between January 1st 2007 and March 31 2009 (see above). We restrict the pre-reform period to births after 2006, because birth dates of non-married fathers are only available from 2007 onward, thus allowing us to identify such fathers only afterwards. For the post-reform period we use an equally spaced interval of 26 months, including all starts of claiming periods between April 1st 2011 and June 30 2013. Since our treatment was restricted to individuals aged 25 to 54, we restricted observations to individuals in this age category at the start of the claiming period. To increase comparability between treated and controls, we restrict controls to individuals with 18 to 23 months contributions. This limits both treated and controls to cases with incomplete contribution histories.

We end up with 2143 control observations in the pre-reform period and 2742 observations in the post-reform period, as well as 1257 treatment observations in the pre-reform and 1109 observations in the post-reform period (see Table 1). All of them include mothers

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[8] For marital births. For non-marital births, we used combinations of maternal/paternal birth date and child birth date.

[9] Information that was available in both BEVNAT and STATPOP.

[10] For the post-reform period this figure amounts to 6.2 percent and likely covers births to single mothers.

with a child whose sensitive developmental stage overlapped with our main window in the claiming period and for whom we were able to identify fathers.

The divergent trends in sample sizes between treated (declining) and controls (increasing) could potentially be due to endogenous behaviors. First, it could be that individuals strategically adapted the timing of their job loss to the new rules such that more individuals reached 18 contribution months after the reform and are entitled to 400 maximum daily allowances. Detailed analyses of sample size trends by number of months with paid contributions (not reported) suggest that such behaviors were not the main drivers of the divergent trends in sample sizes. Rather than at 17 contribution months, declines in sample sizes were greatest at 12 months (indeed, sample size slightly increased at 17 contribution months). Furthermore, rather than at 18 contribution months, increases in sample sizes were greatest at 23 months contributions.

Second, it could be that these divergent trends in sample sizes were spurred by changed fertility behaviors. Women who were in their first year of the claiming period after the reform might have anticipated the harsher conditions after the earlier exhaustion of benefits in comparison to women in the first year of the claiming period before the reform. Potentially, these women could therefore have retreated from becoming pregnant. Additional analyses of the probability of conception among all unemployed women dismiss such hypothesis. The change in the probability of conceiving in the first 11 months after the start of the claiming period among the treated between the pre- and the post-reform regime paralleled the trend found among the controls. Hence, we are confident that endogenous behaviors have not affected our ability to reach causal conclusions.

Characteristics of treated and controls pre- and post-reform

To further explore the validity of our study design, we calculate mean values of all observed characteristics of treated and controls, both in the pre- and the post-reform period. By reporting DID estimates for each control characteristic, we test whether treated and controls had parallel trends in all characteristics but their maximum daily allowances, their receipt of benefits and the couples' incomes from employment. The coefficients reflect the extent to which the difference between the treated and controls changed between the pre- and the post-reform samples (cf. Analytical strategy for details on estimation strategy).

Most importantly, the table shows that there are no significant DID coefficients in our samples but for our treatment and employment income variables (see Table 1). We find the actual treatment effect to be slightly lower than the one we derived from regulatory changes (i.e. 128 daily allowances instead of 140). We explain this difference with more generous policies applied for the unemployed in the control group before the reform as compared to after the reform[11]. In total, the reform reduced the share of recipients in the

[11] Case workers could allow for an extended period of receipt if local unemployment rates exceeded 5 percent. We believe that this policy was more often applied to individuals with more continuous contribution histories.

main window by 19 percentage points or, in monetary terms, reduced benefits by an average amount of 505 CHF/month. The reduction in the share of benefit recipients is comparable to the estimate of a Swiss study that assessed the effect of a previous reform that introduced a similar reduction of PMD (cf. Cottier et al., 2019). The statistically significant and positive DID for father income suggest substantial compensatory employment effects by fathers. On average, the increase in father income surpasses the reduction of benefit income. Hence, while leaving mothers' income unchanged, the reform has, on average, not reduced the household incomes of unemployed women whose unborn child was in the sensitive development stage.

	Controls, pre	**Controls, post**	**Treated, pre**	**Treated, post**	**DID estimate**
Treatment					
PBD (daily allowances)	411.05	399.09	397.63	258.12	-127.55 ***
Share with UI benefits	0.34	0.35	0.41	0.23	-0.19 ***
UI benefits (CHF/month)	834.08	908.85	912.49	482.02	-505.25 ***
Employment incomes					
Mother income (CHF/month)	2322.13	2372.94	1826.65	1899.32	21.85
Father income (CHF/month)	6602.21	6391.19	5409.37	5848.5	650.14 ***
Control variables					
Pre-unemp. income (CHF/month)	3955.77	4162.05	3557.86	3725.99	-38.14
Pre-unemp. level of employment (100%=42h/week)	87.77	86.09	87.14	84.33	-1.13
Share married	0.79	0.76	0.8	0.77	0
Share managerial/professional (high)	0.22	0.23	0.18	0.22	0.02
Share with low profession (low)	0.18	0.17	0.23	0.2	-0.03
Share with tertiary education (high)	0.23	0.28	0.23	0.29	0.01
Share with less than vocational education (low)	0.23	0.21	0.3	0.31	0.03
Age of the mother	32.39	32.37	31.96	32.27	0.34
Age of the father	34.84	34.86	34.75	35.08	0.31
Swiss citizenship (mother)	0.59	0.57	0.47	0.43	-0.03
Swiss citizenship (father)	0.56	0.53	0.5	0.43	-0.04
Share of children female sex	0.49	0.48	0.46	0.49	0.03

Birth order	1.7	1.7	1.7	1.66	-0.04
Share singleton births	0.97	0.96	0.98	0.97	0.01
N (months)	**5520**	**7017**	**3247**	**2855**	
N (births)	**2143**	**2742**	**1257**	**1109**	

Month in sensitive fetal developmental stage = month in last three months of pregnancy. Main window in claiming period = month 12 to 23 in claiming period. Treated: 12 to 17 months contributions in the 24 months before start of the claiming period. Controls: 18 to 23 months contributions. Pre observations: registered between January 2007 and March 2009. Post: registered between April 2011 and June 2013. DID based on one observation per birth for PBD and control variables and for all months overlapping the sensitive fetal development stage and the main window in the claiming period for UI benefit share, UI benefit level and employment incomes. P-value thresholds DID: * = 5 percent, ** = 1 percent, *** = 1 promille.

Analytical strategy

We estimate the effect of the reform by including a multiplicative interaction term between the binary variable treatment versus control ($Treated$) group and the binary variable pre- versus post-reform ($Post$) in an OLS model which has birth weight as the outcome variable (Angrist and Pischke, 2008: 169).

We start with our basic model of the following form:

$$Birthweight = \alpha + \beta_{Treated} * Treated + \beta_{Post} * Post + \beta_{DID} * Treated * Post + \epsilon$$

where β_{DID} is the average effect of the reform. In this model, differences in pre-to-post changes between treatment and control groups in characteristics that are relevant for birth weight are unadjusted. In a second model, we therefore provide the same estimate but now include linear control variables for the women's pre-unemployment income, employment level and occupation, whether the couple was married or not at the moment of child birth, the mother's and the father's age in the year of the birth of the child, whether she and the father held a Swiss citizenship, whether the child had female sex, the birth order of the child and whether or not the child was a singleton birth. By capturing the mothers' socio-economic resources, their exposure to stress and biological determinants of birth weight, these dimensions are potentially related to treatment status and birth weight and might therefore bias β_{DID} in model 1.

$$Birthweight = \alpha + \beta_{Treated} * Treated + \beta_{Post} * Post + \beta_{DID} * Treated * Post + \beta_{Controlvariables} * Controlvariables + \epsilon$$

β_{DID} in model 2 provides a more reliable estimate of the effect of the reform, since it accounts for differences in the trends in these dimensions between treated and controls by linearly controlling for these dimensions. In the third kind of model (model 3), we additionally increase comparability between treated and controls by balancing distributions of all control variables prior to the DID-estimation. We use entropy balancing to generate weights that, separately for the pre- and the post-reform period, adjust means

of all control variables between treated and controls (Hainmueller, 2012). Table 1 (appendix) shows that after balancing, all DID estimates of control variables are 0. Hence, β_{DID} in model 3 expresses the effect of the reform on birth weight net of different trends in means of control variables between treated and controls.

To account for differences in DID by level of father income, we calculate model 3 separately by category of father income. We measure father income with the average income of the fathers in all months that are in the last three months of pregnancy and that overlap with our main window in the claiming period. We define three income categories: births from fathers with incomes of CHF 2500/month or below (below subsistence levels), births from fathers with incomes between CHF 2501/month and CHF 6000/month and births from fathers with incomes above CHF 6000/month. To improve our understanding of the impact of the reform by level of father income, we present a descriptive table (cf. Table 1) for each category of father income. This teaches us whether the reform had a similar impact on unemployed women whose partners had different levels of income. We then reweight treated and controls and calculate β_{DID} separately for each father income category and graphically present the coefficients.

We close our analyses with a robustness check. We compare β_{DID} with two *placebo estimates* of the reform. We obtain such estimates by varying the definition of the time window over the claiming period when selecting our sample. Definition a) includes women whose last trimester of the pregnancy overlapped with the time window of month 6 *before* to month 11 after the start of the claiming period; Definition b) includes women whose last trimester of the pregnancy overlapped with the time window of month 24 to month 42 after the start of the claiming period. Since the reform has taken full effect only after month 12 in the claiming period (see above), we expect β_{DID} with definition a) to not differ from 0. Similarly, we expect a β_{DID} which is close to 0 for women in definition b). These women were only marginally affected by the reform: both, the pre- and the post-reform regimes, at the time of their children's sensitive fetal developmental stage most of them have exhausted benefits (or claiming periods).

Results

Table 2 presents our DID estimates from the first three models. The first column reports the unadjusted DID coefficient. It shows that for the overall study sample there was no statistically significant reduction in birth weight caused by the reform. On average, birth weight increased by 12 grams. The second and third columns present the DID estimates when linear controls of covariates are added to the models and when control units are reweighted to match means of control variables by treated units prior to estimation. From these two models we learn that accounting for covariates leads to lower estimates of the effect of the reform and that balancing slightly improves the precision of our estimate (i.e. reduces standard errors).

	Without control variables	With control variables	Balanced control variables

DID	12.148	4.474	3.945
	(28.046)	(26.203)	(24.494)
Control variables linearly adjusted	no	yes	yes
Treated & Controls balanced	no	no	yes
N	7251	7251	7251
R2	0.000	0.132	0.115
logLik	-56141.389	-55631.435	-56095.515
AIC	112292.778	111300.870	112229.029

Sample: women whose last trimester of pregnancy overlapped with the second year of the claiming period. Control variables: women's pre-unemployment income, employment level, occupation, education, age and citizenship, father age and citizenship, child sex, parity and singleton status, marital status at birth. Balanced results based on entropy-balancing generated weights that adjust means in control variables of control group to means in control variables of treated group separately in pre- and post-reform period (cf. Table 1 appendix). Standard errors in parantheses. P-value thresholds: * = 0.05, ** = 0.01, *** = 0.001.

Did the effect of the reform differ by level of father income? Table 2 (appendix) provides us more information about how the reform affected unemployed women in different categories of father income. It shows that the reform has led to a identical loss of benefits among couples where fathers had income in the lowest and the middle income category, but has led to a lower loss of benefits among couples with high income fathers. The results also show us that income reactions to the reform differ strongly by father income. While we find no reactions or even negative reactions in the lowest father income category, fathers in the middle and the top income group have increased their incomes in reaction to the reform.

Figure 1 shows the estimates of β_{DID} (and 95% confidence intervals) for the samples defined by the category of father income. It shows that father income was a significant moderator of the effect of reduced PMD. On the one hand, the reform has led to a significant reduction of 132 grams in the birth weight of children whose fathers have incomes below CHF 2501/month. On the other hand, the reform has led to low and statistically insignificant increases in birth weight for children whose fathers earn above CHF 2500/month. Hence, the absence of an effect of the reform in the overall sample was thus due to the slightly positive effects among the majority of cases where fathers have income above subsistence levels (around 80%).

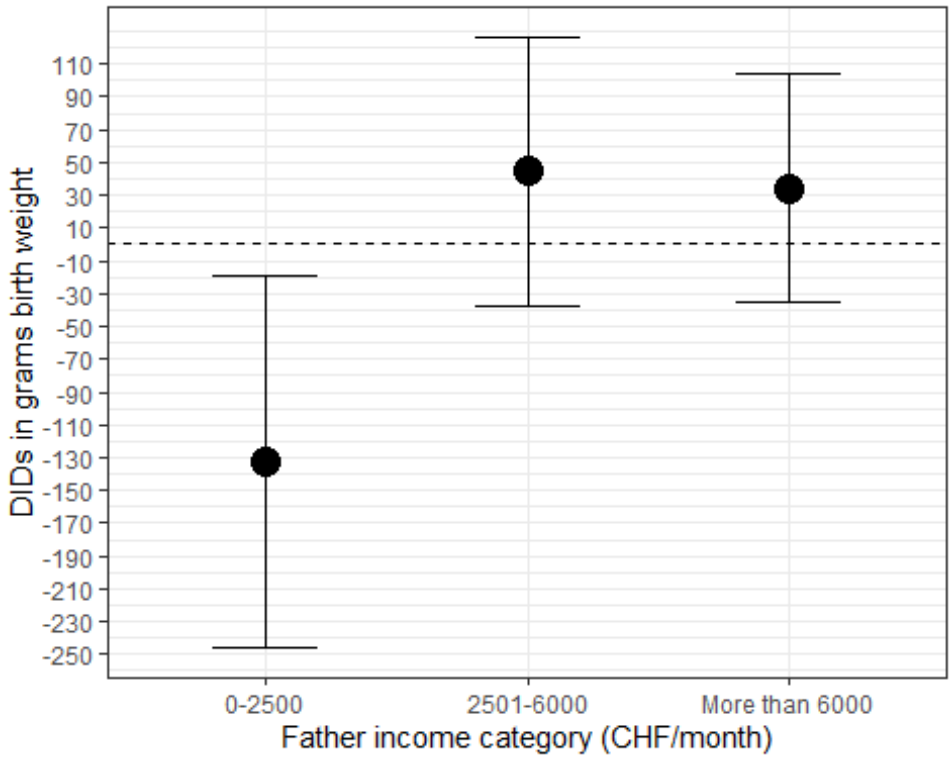

Figure 1: OLS DID coefficients of reform and 95% confidence intervals; dependent variable: birth weight (in grams), by father income category. Sample: women whose last trimester of pregnancy overlapped with the second year of the claiming period.

We check the robustness of our results for the subgroup of cases for whom we find an effect (where fathers have incomes below CHF 2501/month). Figure 2 presents the β_{DID} (and 95% confidence intervals) when using different definitions of the time window in the claiming period. As expected, we find a significant β_{DID} only where the reform took effect, i.e. in the second year after the start of the claiming period. The *placebo estimates* yield statistically insignificant results both if we shift the time window to the 18 months before our main window and if we shift the time window to the 18 months after our main window.

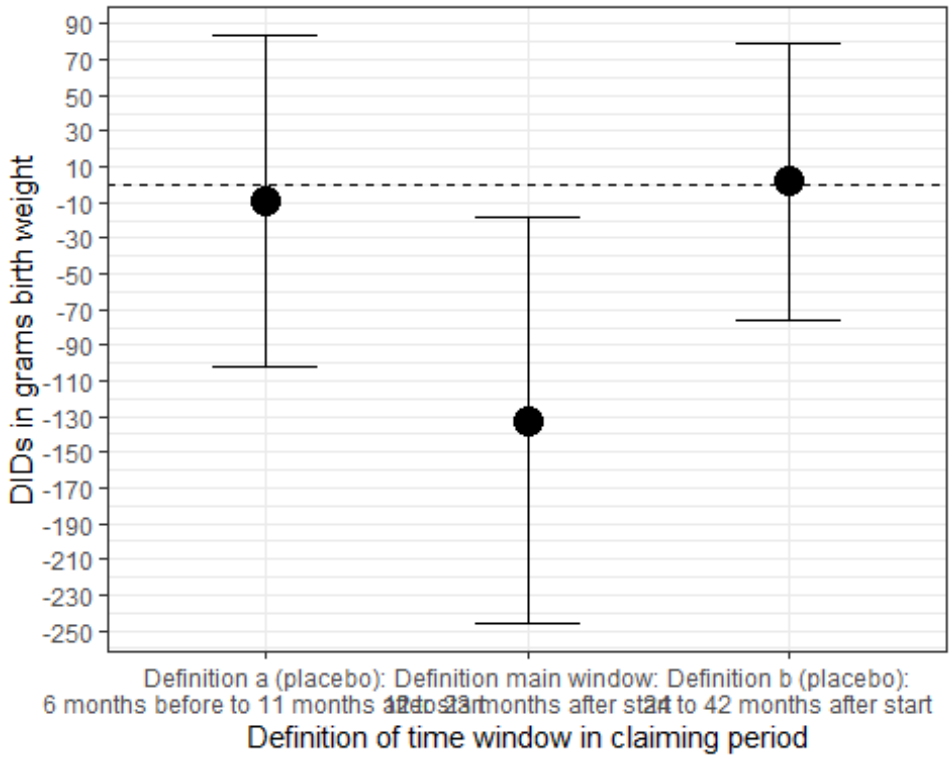

Figure 2: OLS DID coefficients of reform and 95% confidence intervals; dependent variable: birth weight (in grams), by definition of time window in the claiming period. Sample: women whose last trimester of pregnancy overlapped with the second year of the claiming period and whose children's fathers had incomes below CHF 2501/month.

Summary and conclusions

Does unemployment insurance improve the health of the unemployed and their family members, and does the level of partner resources matter for these protective effects? In this study, we first asked whether the 2011 unemployment insurance (UI) reform in Switzerland negatively affected the birth weight of children of unemployed women. Among unemployed women who were in the last trimester of their pregnancies in the second year after the start of their UI benefit claiming period, the reform caused a loss of benefits with an average value of CHF 500/month. Given that these women were close to giving birth, they were unable to compensate this loss of household income by increasing their own incomes. Conversely, the children's fathers substantially increased their incomes and, by that, compensated for the loss of benefits. It was therefore not surprising to find that the reform has *not reduced the average birth weight of all affected children*.

We argued that less benefits could have affected the birth weight of children by increasing economic hardship experienced by their mothers. Since such effects are absent in a situation in which women can draw on alternative economic resources, our second research question was whether the reform affected birth outcomes differently by level of father income. Indeed, we found that the overall analysis hid strong negative effects of the

reform for those women who could not draw on fathers' income to compensate the loss of benefits. For those *children whose fathers earned incomes around subsistence levels or below (CHF 2500/month), the loss of benefits by their unemployed mothers led to a reduction of their average birth weight of 132 grams*. To give this number a substantive interpretation: 132 grams equals 4% of the average birth weight, the estimated weight difference between newborn girls and newborn boys and about one third of the birth weight reduction related to regular smoking by the mother (Bharadwaj et al., 2014).

What conclusions can be drawn from these results? First, we interpret our results as robust evidence for the positive health effects of UI benefits. The most reasonable pathway through which the reform affected fetal development was by affecting unemployed women's stress levels, their health or their health behavior. We are confident that our estimates reflect causal effect of UI benefits, since we could not find such birth weight differences for women who were affected by the reform, but whose pregnancies preceded or followed the effects of the reform. Women who become pregnant during unemployment are a special group since they usually have strong difficulties in finding a job. Nevertheless, we believe that the positive health effects of UI might also be found among other populations who have job search difficulties. Example hypotheses that emerge from these findings are that health effects of UI are particularly salient for individuals with care obligations or individuals with pre-existent health problems.

Second, our results demonstrate the crucial role of household structure and couple relations for the welfare effects of UI benefits. We find that the greatest part of our sample of women was unaffected by the reform since the incomes of their partners were sufficient to compensate the loss of benefits. However, for those women whose partners were unable to provide financial assistance, transfer incomes from UI is a key health determinant. We believe that this finding also holds for the growing population of individuals in single households. In this study we were unable to explicitly account for them, since our data base lacked information on household structure in the pre-reform period. We therefore encourage future studies to shed more light on unemployed in single households (Alm et al., 2019; Seltzer, 2019) and re-test the role of household economics for the welfare effects of UI (Shahidi et al., 2019).

Finally, our study supplements earlier North American evidence (Brownell et al., 2018; Hoynes et al., 2015) on the positive impact of cash transfers on early-life development. On the one hand, we find that protective effects of additional income do not extend to populations who already have access to abundant economic resources. On the other hand, our results suggest that for pregnant women close to the poverty line, receiving less cash transfers is detrimental for their children's health - even in a context with a relatively generous social safety net of last resort and a subsidized health care system. In that sense, our results demonstrate the protective effect of cash transfers for the health of newborns when this money prevents material hardship where it would otherwise occur.

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Appendix

	Controls, pre	Controls, post	Treated, pre	Treated, post	DID estimate
Treatment					
PBD (daily allowances)	409.67	399	397.63	258.12	-128.83 ***

Share with UI benefits	0.36	0.39	0.41	0.23	-0.21 ***
UI benefits (CHF/month)	810.08	899.91	912.49	482.02	-520.3 ***
Employment incomes					
Mother income (CHF/month)	2050.97	2090.73	1826.65	1899.32	32.9
Father income (CHF/month)	6195.79	6099.65	5409.37	5848.5	535.27 ***
Control variables					
Pre-unemp. income (CHF/month)	3557.86	3726	3557.86	3725.99	0
Pre-unemp. level of employment (100%=42h/week)	87.14	84.33	87.14	84.33	0
Share married	0.8	0.77	0.8	0.77	0
Share managerial/professional (high)	0.18	0.22	0.18	0.22	0
Share with low profession (low)	0.23	0.2	0.23	0.2	0
Share with tertiary education (high)	0.23	0.29	0.23	0.29	0
Share with less than vocational education (low)	0.3	0.31	0.3	0.31	0
Age of the mother	31.96	32.27	31.96	32.27	0
Age of the father	34.75	35.08	34.75	35.08	0
Swiss citizenship (mother)	0.47	0.43	0.47	0.43	0
Swiss citizenship (father)	0.5	0.43	0.5	0.43	0
Share of children female sex	0.46	0.49	0.46	0.49	0
Birth order	1.7	1.66	1.7	1.66	0
Share singleton births	0.98	0.97	0.98	0.97	0
N (months)	**5520**	**7017**	**3247**	**2855**	
N (births)	**2143**	**2742**	**1257**	**1109**	

Month in sensitive fetal developmental stage = month in last three months of pregnancy. Main window in claiming period = month 12 to 23 in claiming period. Treated: 12 to 17 months contributions in the 24 months before start of the claiming period. Controls: 18 to 23 months contributions. Pre observations: registered between January 2007 and March 2009. Post: registered between April 2011 and June 2013. DID based on one observation per birth for PBD and control variables and for all months overlapping the sensitive fetal development stage and the main window in the claiming period for UI benefit share, UI benefit level and employment incomes. P-value thresholds DID: * = 5 percent, ** = 1 percent, *** = 1 promille.

	Controls, pre	Controls, post	Treated, pre	Treated, post	DID estimate
Fathers have low income (<2501)					
Treatment					
PBD (daily allowances)	413.92	399.8	400.82	258.77	-127.93 ***
Share with UI benefits	0.37	0.39	0.42	0.22	-0.23 ***
UI benefits (CHF/month)	819.25	911.4	952.9	444.59	-600.46 ***
Employment income					
Mother income (CHF/month)	1770.25	1781.53	1523.17	1368.96	-165.48
Father income (CHF/month)	538.26	596.02	602.36	576	-84.12
N (months)	*865*	*1158*	*763*	*582*	
N (births)	*341*	*448*	*293*	*227*	
Fathers have middle income (2501-6000)					
Treatment					
PBD (daily allowances)	407.69	398.63	397.08	259.78	-128.25 ***
Share with UI benefits	0.41	0.46	0.47	0.28	-0.25 ***
UI benefits (CHF/month)	814.22	975.29	992.49	554.43	-599.14 ***
Employment income					
Mother income (CHF/month)	1725.97	1644.01	1537.77	1506.95	51.14
Father income (CHF/month)	4860.57	4730.98	4628.35	4660.77	162.01 ***
N (months)	*1973*	*2420*	*1265*	*1104*	
N (births)	*767*	*949*	*493*	*437*	
Fathers have high income (>6000)					
Treatment					
PBD (daily allowances)	410.33	398.99	396.22	256.16	-128.73 ***
Share with UI benefits	0.31	0.31	0.34	0.18	-0.16 ***
UI benefits (CHF/month)	807.99	840.38	804.17	432.27	-404.29 ***
Employment income					
Mother income (CHF/month)	2471.58	2664.88	2316.39	2533.91	24.21

Father income (CHF/month)	9469.14	9411.79	9228.69	9595.14	423.8
N (months)	*2682*	*3444*	*1219*	*1169*	
N (births)	*1035*	*1348*	*471*	*445*	

Month in sensitive fetal developmental stage = month in last three months of pregnancy. Main window in claiming period = month 12 to 23 in claiming period. Treated: 12 to 17 months contributions in the 24 months before start of the claiming period. Controls: 18 to 23 months contributions. Pre observations: registered between January 2007 and March 2009. Post: registered between April 2011 and June 2013.. DID based on one observation per birth for PBD and for all months overlapping the sensitive fetal development stage and the main window in the claiming period for UI benefit share, level and alternative incomes. Treatment and control groups balanced separately in pre- and post-reform period. P-value thresholds DID: * = 5 percent , ** = 1 percent, *** = 1 promille.

Number	Task	Name of file
1	Within BFS: Generate id from pseudo social security number and prepare external export for SECO/ZAS	Export_ZAS_BSTV_SHS.R
2	Within BFS: Remove social security number and prepare for external export	Export_BFH.R
3	Create treatment and control groups using unemployment registers SECO	Experiment_2011_1217.R
4	Identify lacking social security numbers in BEVNAT Births using STATPOP	BEVNAT_STATPOP_Geburten_id_kld1.R
5	Prepare birth file	BEVNAT_births_long.R
6	Identify relevant cases	Final_data_prep.R
7	Prepare IK data	Income_birth.R
8	Prepare IV data	IV.R
9	Prepare SH data	SHS.R
10	Prepare tax data	Wealth_birth.R
11	Create final birth file with merged information	Final_data_prep.R
12	Test selection into treatment (Has treatment changed timing of conception?)	BEVNAT_births_long.R; Selection_into_treatment_health.R
13	Analyses	Paper 4 Health_V4.rmd