HEALTH AND UNEMPLOYMENT DURING MACROECONOMIC CRISES

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ABSTRACT.

This paper shows that early-life health is an important determinant of labor market vulnerability during macroeconomic downturns. Using data on twins during Sweden's crisis of the early 1990s, we show that birth weight differentially affects job loss after the crisis as compared to before it. In particular, a 10 percent increase in birth weight differentially reduces take-up of unemployment insurance benefits by 0.3 percentage points (about 2 percent of the post-crisis mean) and decreases the share of total income coming from such benefits by 0.2 percent (3 percent of the post-crisis mean) after the crisis. While differences in pre-determined health thus lead to increased inequality in employment outcomes, we also show that there is no differential effect of birth weight on total income after the crisis. This suggests that in the context of Sweden, the social safety net is able to mitigate the effects of poorer health on labor market outcomes during economic downturns.

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1. Introduction

A large and growing literature in economics studies the individual-level consequences of macroeconomic fluctuations. Given the importance of health human capital for labor market outcomes, one strand of this literature investigates whether and how events like recessions, job displacements and business cycles affect health outcomes.[1] A second important strand of research examines *who* is affected by macroeconomic fluctuations and focuses mostly on heterogeneous impacts by demographic characteristics, such as age, gender, sex, race and education.[2] However, despite the large amount of work in this area, only very few studies investigate whether pre-determined health, such as health at birth, dictates the degree to which individuals are affected during recessions. In this paper, we contribute to this literature by showing that pre-crisis health, as measured by birth weight, is an important marker for labor market vulnerability during macroeconomic downturns.

There are at least two reasons why examining how pre-determined health moderates the impacts of crises is important. First, understanding the returns to health is crucial for both individuals and governments, and one understudied aspect of these returns is whether better health makes individuals more resilient to economic shocks. If having better *ex ante* health helps to weather economic fluctuations, then this could be yet another reason for individuals to invest more in their own health human capital. Second, investigating the importance of pre-determined health during recessions provides important insights into the role of social insurance in helping to smooth shocks. While it might not be possible to equalize health across the population, social safety nets could ensure that individuals subjected to worse health for exogenous reasons are able to smooth income during periods of economic crises.

We study the effects of health on job loss before and after an unexpected and dramatic increase in unemployment in Sweden in the early 1990s, when unemployment went from

[1] See, for example, Ruhm (2000), Stillman and Thomas (2008), Sullivan and Von Wachter (2009), and Currie and Tekin (2015).

[2] See, for example, Clark and Summers (1981), Bound, Holzer, et al. (1995), Engemann and Wall (2009), Cho and Newhouse (2013), Hoynes, Miller, and Schaller (2012).

2% to 8% in less than three years. This crisis has been referred to as one of the "Big Five" postwar economic downturns in Reinhart and Rogoff (2008), and many observers of the Great Recession in 2008 have compared it to this Swedish crash.[3] While much has been written about the causes and consequences of the crisis in the Nordic countries during the early 1990s (Englund 1999, Jonung, Kiander, and Vartia 2009, Gorodnichenko, Mendoza, and Tesar 2012), the main import from these studies appears to be that it was the result of a combination of factors, including monetary policy, budget deficits, financial deregulation, and a collapse of trade. Prior work has shown that the effects of such economic crises differ between the private and the public sector (Kopelman and Rosen 2016), and the Swedish case was no exception (Lundborg 2001). In our study, we therefore examine the impact of health on labor market outcomes before and after the recession separately for each sector.

In the empirical analysis, we estimate the differential impact of health on job loss after the crisis in the early 1990s as compared to before it. Our two main outcome variables are an indicator for receiving any unemployment insurance (UI) benefits, and the fraction of total income coming from such benefits.[4] We measure health using birth weight, which is a widely-used proxy for health at infancy. Recognizing that birth weight captures both nutritional intake and maternal behaviors such as smoking, which might confound our analysis, we follow prior studies (Almond, Chay, and Lee 2005, Black, Devereux, and Salvanes 2007, Royer 2009, Bharadwaj, Lundborg, and Rooth 2018) and identify effects using only exogenous variation within twin pairs. Since we compare the impacts of birth weight across two time periods – before and after the crisis – and between two twins, our empirical strategy intuitively resembles a difference-in-differences model.

The results show that adults who were born with better health were significantly less likely to face job loss and go on unemployment insurance during the crisis. This is especially true

[3]Observers especially noted the ways in which Sweden recovered from the crisis; see, for example, *New York Times*, September 22, 2008, and *Time*, September 24, 2008.
[4]UI benefit receipt is the only measure of job loss/separation available in our data. While we discuss this issue in detail in Section 3.1, it is important to note here that the main source of UI take-up during the crisis was layoffs rather than quits (Skans, Edin, and Holmlund 2009).

for individuals working in the private sector, for whom we find that a 10 percent increase in birth weight differentially reduces UI take-up by 0.3 percentage points (about 2 percent of the post-crisis mean) and decreases the fraction of total income due to UI benefits by 0.2 percent (3 percent of the post-crisis mean) after the crisis. In contrast, the estimates for the public sector are smaller in absolute value and not statistically different from zero, despite the fact that this sector also experienced job reductions during the downturn.[5] One potential explanation for this pattern is that the private sector responds to macroeconomic shocks by laying off ostensibly weaker individuals (those with lower birth weight) and holding on to the stronger (higher-birth-weight) ones more so than the public sector does.

We then explore *why* better health might make individuals less susceptible to job loss during crises. Our analysis focuses on three likely channels: educational investments, occupational sorting, and the role of labor market institutions. For example, the manufacturing industry was particularly hard-hit during the crisis, and thus one hypothesis might be that individuals with higher birth weight selected into other, less-affected industries and occupations already before the crisis. However, our results hold even when we control for industry of employment, or for 4-digit occupation, suggesting that occupational sorting is not the main mechanism at work. Similarly, there is no evidence that the differential impact of birth weight after the crisis is mediated by educational attainment. Moreover, while job tenure is often argued to be an important determinant of hiring and firing decisions in the Swedish context, we find no support for the hypothesis that our results are driven by "last-in-first-out" policies, which might differentially affect lower-birth-weight workers. Finally, we also show that birth weight does not have a differential effect on *overall* income, including income from welfare and unemployment insurance payments, after the crisis. While individuals with poor health have lower incomes in general, the presence of a strong welfare system in our setting thus moderates the effects of job loss on total income during the crisis.

[5] As we discuss later on, our estimates do not allow us to conclude that the effects in the two sectors are statistically different from each other.

This paper underscores the importance of health in determining labor market outcomes by showing that health, and in particular health at birth, matters more for job attachment during economic crises. Recent work has documented the importance of social assistance programs in improving early-childhood health, as well as the long-run effects of early exposure to social safety nets (Bitler and Currie 2005, Hoynes, Schanzenbach, and Almond 2016). We add to this literature the idea that there could be early childhood health-related spillovers of safety net programs, as children born with better health are themselves *less* likely to take up social assistance later in life. This study is also important for highlighting the role of social assistance during a crisis more generally. One of the fundamental questions about the design of optimal insurance policy is the extent to which it can mitigate morally arbitrary misfortunes of nature. By exploiting random variation in birth weight, we are able to show that social assistance, at least in the case of Sweden, appears to come to the aid of those who are born with a health disadvantage.

2. Background

2.1. **The 1990s crisis in Sweden.** Unlike in most European countries, unemployment in Sweden remained low during the 1980s and fluctuated between 2 and 4 percent. In the later part of the decade, the Swedish economy experienced a boom which pushed unemployment further down to a low of 1.5 percent in 1989. This exceptionally good period in the labor market was followed by the worst recession since the 1930s as unemployment increased from 2 percent in 1990 to 8 percent in 1993. The open unemployment rate then remained at this high level until it started to fall again in 1997. The decrease in employment occurred in both the private and the public sector, with the private sector being more affected (Lundborg 2001). Figure 1 shows that the timing and sectoral spread UI take-up in our twins sample, which is described in detail below, follow these same patterns. We outline the roots of the Swedish crisis, relying heavily on Englund (1999) and Holmlund (2011), in the Appendix.

2.2. **The unemployment insurance system in Sweden.** The basic rules that regulate the right to reimbursement from unemployment funds have largely been the same since the 1930s.[6] The government subsidies to the unemployment funds are substantial; in the early 1990s, the subsidies covered about 95 percent of all unemployment benefits paid out (Carling et al. 2001). In contrast, the monthly membership fees were typically low and covered only a small part of the benefits paid out. During the same period, about 80 percent of the recorded unemployed workers were members of an unemployment fund. Unemployed non-members could, between 1976 and 1997, receive a so-called "cash assistance" (Kontant Arbetsmarknadsstöd in Swedish) from the government, but the benefits paid out were much lower than those of the unemployment funds and the entitlement period was substantially shorter.

By international standards, the replacement rate of the Swedish unemployment insurance funds has historically been generous. The 1980s and early 1990s saw replacement rates of about 90 percent of earnings, but there was a ceiling on the benefit level. This meant that the actual replace rate could be much lower than 90 percent, in particular for high-earning workers. In 1996, for example, it was estimated that 75 percent of employees had monthly earnings exceeding the ceiling. From 1974 onwards, unemployed workers could receive unemployment benefits for a total of 300 days; however, workers aged 55 and above could receive benefits for 450 days.

The unemployment insurance system became somewhat less generous in 1993. On July 1st, 1993, the replacement rate was first reduced to 80 percent. It was then then further reduced to 75 percent in 1996 but increased again to 80 percent in 1997 (Carling et al, 2001). In 1994, the working requirement was also changed such that one needed to have worked for

[6]One has to be at least 16 years of age, able to work, and report as seeking a job at the Swedish Public Employment Service. In addition to these requirements, between 1973 and 1994, individuals were only eligible to receive UI benefits if they had been a paying member of an unemployment fund for at least 12 months prior to becoming unemployed. For full compensation, the reason for unemployment has to be involuntary unemployment. Unemployment benefits can still be paid to workers who quit their job and become unemployed or to workers who get fired due to misbehavior, but the compensation then becomes less generous. In such cases, the rules allow the unemployment funds to subtract days of compensation. In 2007, for example, a worker who voluntarily quit his job lost 45 days of unemployment benefits.

at least 75 hours per month during a five-month period, or alternatively for at least 65 hours per month during a ten-month period. This had the effect that part-time workers and youths found it more difficult to qualify for unemployment benefits. The duration of unemployment benefit payments was, however, not changed during this period.

To summarize, the unemployment insurance system in Sweden has historically been generous, but qualifying for UI benefit receipt became somewhat more difficult in the aftermath of the crisis in the early 1990s. As stated in the introduction, the empirical analysis studies the effect of birth weight on UI benefit dependence before and after the crisis using a within-twin design. Because twins face the exact same labor market conditions and rules regarding UI benefits in a given year, this design can account for these institutional changes. Moreover, the effects on UI benefit receipts that we do find are *despite* the fact that it became more difficult to qualify for them after the crisis.

2.3. **Birth weight as a measure of health.** A large literature has examined the associations between birth weight and various health and labor market outcomes. Birth weight is the result of both maternal nutritional intake and maternal behaviors, such as smoking and prenatal care visits, and is therefore the focus of many policy efforts in developing and developed countries. In an excellent summary of some of this literature on the impacts of birth weight, Hack, Klein, and Taylor (1995) conclude that, "Although the vast majority of low birth weight children function within the normal range, they have higher rates of subnormal growth, health conditions, and inferior neurodevelopmental outcomes than do normal birth weight children." Moreover, at least since Barker, Osmond, and Law (1989), the idea that fetal growth restrictions due to nutritional deficiencies in early life have long term health impacts (i.e. the "fetal origins hypothesis") has been popular among various disciplines and the subject of many research studies. Since in this paper we examine birth weight differences within twins, the variation in birth weight is more likely due to fetal nutritional intake rather than maternal behavior (Royer 2009).

3. Data and econometric specification

3.1. **Data.** Examining the relationship between pre-determined health and unemployment before and after macroeconomic shocks requires rather unique data. Most electronic birth records, even in countries known for their excellent administrative data (such as Norway), only start in the late 1960s. This implies that for most major crises, individuals for whom reliable birth data exist are too young to be observed in the labor market for a substantial period of time before the crisis, which complicates the study of the effect of health endowments on job attachment during macroeconomic downturns. Here, we overcome this challenge by exploiting unique data on birth records for nearly the entire population of twins born in Sweden between 1926 and 1958. We match these data to yearly income records for 1978 to 2007, which allows us to observe individuals' labor market outcomes, including income from unemployment insurance benefits, for several years before and after the crisis of the early 1990s. In what follows, we describe our data sources in more detail.

We start our data construction from the BIRTH register, which collects data on birth outcomes for all twins born in Sweden between 1926 and 1958. The data originate from a project at the Swedish twin registry, where researchers set out to digitize birth records that were kept in paper form at local delivery archives around Sweden. Since municipalities were required by law to collect and preserve birth information, the researchers were able to obtain data for a high fraction of twins. The records include essential birth information such as birth weight, sex, and birth length, but lack information typically included in modern registers, such as APGAR scores. They also include personal identifiers, which means that the data can be linked to other administrative registers in Sweden at the individual level.

Due to the way in which the data were collected, the BIRTH register only includes twins who survived up to 1972. In particular, in 1972, an extensive survey on the twin cohorts born between 1926 and 1958 was conducted. Since the data from this survey contained variables that were deemed important for twins research, the surveyors set out to collect birth data only for twins who participated in the survey. Fortunately, the response rate was high (86%).

Since we do not have data on the universe of twins born in 1926-1958, we are unable to construct weights for non-response or to assess attrition in any systematic manner.[7]

With the use of the personal identifiers, we linked the BIRTH data to the Income and Taxation (IoT) register, which contains information on labor market earnings and all taxable benefits, including unemployment insurance benefits and sickness and welfare pay. The labor market earnings records in this register come from the equivalent of W2 forms in the United States, in that the income is reported by employers and is not based on self reports. Similarly, taxable benefit income is reported directly by the responsible administrative agency. Hence, we consider the income measures in these data to be highly accurate. The records we have access to contain individual-level yearly data between 1968 and 2007. Note, however, that receipt of unemployment insurance benefits was only recorded from 1978 onward. To make variables comparable over time, we adjust all income measures using the 2007 CPI. When merging the records from the BIRTH and IoT registers, we lose less than one percent of the data due to various matching issues.

Based on the income data from the IoT register, we construct our two main outcome variables. First, we create a binary variable indicating take-up of any unemployment insurance benefits in a given year ("UI take-up"); this is an "extensive" measure of UI dependence akin to job loss. Second, we measure the fraction of total income coming from unemployment insurance benefits ("UI/total income"); we consider this as an "intensive" measure of UI dependence. Note that UI benefit receipt is the only measure of job loss available in our data. We acknowledge that generally, this is not an ideal measure as UI take-up could partly be driven by supply-side responses, rather than only changes in labor demand. In the case of this particular macroeconomic shock of the 1990s in Sweden, however, we argue that UI take-up is a good proxy for job loss, at least in the immediate aftermath of the recession. As Figure 1 shows, the take-up of UI is sudden, large in magnitude, concentrated in the

[7]Since we only capture twins where both individuals were alive as of 1972, we expect to find fewer twins from the 1930s as compared to twins from the 1950s. As a fraction of overall live births we certainly capture fewer twins than expected from earlier cohorts.

private sector, and timed so tightly with the onset of the recession that it is unlikely to be driven by supply-side responses. Moreover, work on the Swedish labor market specifically suggests that this period was dominated by layoffs rather than quits: "The importance of layoffs increased substantially during the slump of the 1990s, but separate data on quits and layoffs are not available after 1988. Other evidence, such as information on unemployment inflow and advance notification of layoffs, indicates sharply rising layoff rates in the early 1990s" (Skans, Edin, and Holmlund 2009). In the long run, however, it is admittedly harder to argue that UI take-up reflects purely demand-side forces, as there could be supply-related responses that lead workers to stay on UI long after the crisis.

Finally, in order to shed light on the possible mechanisms through which health at birth affects later unemployment, we link our data to several further administrative records. Thus, we obtain information on years of schooling from the Education Register from 1990 (or from 2007 for those individuals missing in the 1990 data), where years of schooling has been imputed based on the highest degree obtained. We also link data on sector of employment (private versus public) and 4-digit occupation from the 1985 and 1990 censuses. Finally, we use data on hospitalizations from the National Patient Register to measure individuals' health as adults. For a more detailed description of these data sources, we refer to Bharadwaj, Lundborg, and Rooth (2018).

3.2. **Sample selection and summary statistics.** The empirical analysis contrasts the impact of birth weight on UI benefit dependence before and after the crisis of the early 1990s. Our main results use the years from 1986 to 1990 as the pre-crisis period, and the years from 1993 to 1997 as the post-crisis period. We take 1993 as the starting point of the post-crisis period because of the sudden increase in UI benefit take-up in that year (see Figure 1), and we use a five-year window in order to capture the medium-run effects of the shock. We exclude 1991 and 1992 from the analysis as these were transitory years before the full effect of the crisis was realized (later on, we also present results using alternative definitions of the pre- and post crisis periods which confirm our main findings). Finally, because of the differential degree of severeness of the crisis in the private and the public sector, we examine the effect of birth weight on UI benefit dependence separately for each sector.

The analysis plan laid out in the previous paragraph, and the within-twins regression design described in detail below, mean that we have to impose a number of necessary sample restrictions. First, from the BIRTH register, we select only those twin pairs where both twins have non-missing records on birth weight. Second, to ensure that our results are not driven by gender differences, we restrict our sample to same-sex twins. Third, since we are interested in estimates by sector of employment, we only select observations where both twins are working in the same sector. Because we observe sector of employment only in the census data, we condition on both twins working in the same sector in 1985 for observations in the pre-crisis period, and on both twins working in the same sector in 1990 for observations in the post-crisis period. The resulting final analysis sample includes (across both periods) 5,481 twin pairs in the private sector and 2,930 twin pairs in the public sector. Appendix Table 1 provides further details on how each of the restrictions mentioned in this paragraph reduces our sample size to arrive at these numbers.

Table 1 shows summary statistics for the main analysis samples in the private and the public sector. The twins are approximately 44 years old, and they have an average of 10 (12) years of schooling in the private (public) sector.[8] The average birth weight is around 2,600 grams. Note also that in our samples, only 19% of the employees in the public sector are male, while 73% of the employees in the private sector are male. These sectoral differences in the gender composition of the workforce are in line with the findings for the wider Swedish labor market in Rosen (1997).

In order to shed light on the external validity of our results, we compare the socio-economic characteristics of our twins sample in 1990 to those of the general population

[8]We confirmed that average educational attainment for public sector employees is about two years higher than that for private sector employees even in the full population of Sweden. In particular, using the 1990 census and the same cohorts as in our twins sample (1926-1958), the average years of education is 12.2 for those employed in the public sector and 10.6 for those employed in the private sector.

using data from the 1990 census. Panel A of Table 2 shows that in terms of age, education, and income, the twins look very similar to the general population born during the same time period. However, the fraction of males is larger among twins, likely due to our sample restriction that both twins need to work in the labor market. Panel B reports estimates from Mincer-type regressions of the returns to schooling for both samples. The returns are somewhat lower among twins, but overall in the same ballpark as those found in the census data. We conclude that the twins in our sample are broadly comparable to the general population in Sweden born during the same time period.

3.3. **Econometric specification.** The empirical analysis aims to measure the differential impact of health on UI take-up and UI/total income after the crisis in the early 1990s as compared to before it. Like all other studies investigating the impacts of health on labor market outcomes, the key challenge we face is that of bias due to unobserved confounders: workers' health is influenced by a myriad of (unobserved) factors, many of which exert a direct effect on these outcomes. For example, if mothers' education affects both birth weight and later labor market success, a naive regression of UI take-up on birth weight that does not control for mothers's education will yield biased results. In this paper, we address this challenge by following the recent literature on the effects of birth weight (e.g., Bharadwaj et al. 2018, Black et al. 2007) and focusing on differences between twins. This allows us to hold constant a wide range of potential confounding factors, including mothers' education. While this research design cannot account for the impact of individual-level correlates of birth weight that still vary between twins, such as cognitive ability, the same is true for all papers that examine the effects of birth weight on later life outcomes using twin fixed effects.

Our main OLS estimates are based on the following regression model:

$$Y_{ijt} = \beta BW_i + \gamma POST_t + \delta BW_i \times POST_t + \mu_j + \epsilon_{ijt} \tag{1}$$

Here, i denotes individuals, j denotes families, and t denotes years. Y is one of our two outcomes (UI take-up or UI/total income), BW is log birth weight, and $POST$ is an indicator taking value 1 if $t \in [1993, 1997]$ and 0 otherwise. The main parameter of interest is δ, which measures the differential impact of birth weight on the outcome in the years after the crisis. The twin fixed effects μ_j control for any determinants of BW and Y that do not vary between twins, such as family background characteristics. Moreover, by focusing on same-sex twins, we ensure that our results are not driven by gender differences. Finally, note that the regression model in equation 1 intuitively corresponds to a difference-in-differences model: it compares how birth weight differences within twin pairs (first difference) influence their labor market outcomes before and after the crisis (second difference). A key assumption of our empirical strategy is thus that the outcomes of low- and high-birth weight twins would have followed a parallel trend in the absence of the crisis, and we provide evidence supporting this assumption further below.

4. RESULTS

4.1. **Main results.** Table 3 shows the main results of the paper. Focusing first on the private sector, there are small negative effects of birth weight on UI take-up and UI/total income already before the crisis, which are however not statistically significant at conventional levels. Strikingly, the size of these impacts increases significantly after the crisis: for example, a 10 percent increase in birth weight differentially reduces UI take-up by 0.3 percentage points (about 2 percent of the post-crisis mean) and decreases UI/total income by 0.2 percent (3 percent of the post-crisis mean) during the mid-1990s. Thus, inequalities in early-life health lead to unequal employment outcomes mainly after, as opposed to before, the crisis. Turning to the estimates for the public sector, we find qualitatively similar but smaller effects. This more muted response is likely due to the fact that individuals in the public sector were quicker to move out of UI after an initial period of being on UI benefits after the crisis.[9]

[9]Note that partly due to our small sample size, the effects in the two sectors are not statistically different from each other. To establish this, we stacked the private and public data and estimated regressions which included a

Figure 2 recasts our main results in terms of a difference-in-differences event-study framework. To construct the graphs in this figure, we estimate versions of the specification in equation 1 that include a full set of year dummies instead of the post dummy. The graphs plot the estimated coefficients on the interactions between these dummies and log birth weight for each year. Confirming the results in Table 3, there are small negative effects of birth weight on both outcomes in the private sector already in the 1980s. Importantly, however, these impacts stay constant throughout the pre-crisis period, showing that there are no differential trends in these outcomes by birth weight. The coefficients drop only after the crisis hits in the early 1990s, which corroborates our finding of a differential impact of health endowments after the downturn. Finally, the public sector results mirror those for the private sector, but as in Table 3 the differential impact of birth weight after the crisis is less pronounced.

4.2. **Sensitivity checks and further results.** We conduct a range of additional analyses to assess the sensitivity of our main results to various restrictions. First, Appendix Table 2 shows estimates from specifications which do not control for twin fixed effects. These regressions underestimate the birth weight impacts, suggesting that controlling for unobserved family characteristics is important in our context. Second, our main analysis sample includes different individuals before and after the crisis, mainly due to people switching across occupational sectors or retiring from the workforce. In Appendix Table 3, we show that estimates are similar, though somewhat less precise, when we focus on a balanced panel of twins. Third, Appendix Table 4 shows that the birth weight measurement error issues discussed in Bharadwaj, Lundborg, and Rooth (2018) are not a concern in this context. Even if we mechanically introduce measurement error by rounding all birth weight data to the nearest 50 grams, our results are unchanged. Fourth, one might worry that the effects on UI/total income reflect a purely mechanical relationship: for example, if income from other sources

full set of interactions of all variables with a dummy for private sector. The p-values for the interaction between this dummy and the difference-in-differences coefficient were 0.599 and 0.163 for UI take-up and UI/total income, respectively. The differences in results across sectors should therefore be interpreted as suggestive.

falls after the crisis hits, UI/total income will increase even if UI benefits themselves do not change. Addressing this issue, Appendix Table 5 shows that results hold even if we flexibly control for total income in our regressions.

Fifth, we test the sensitivity of our results to various re-definitions of the sample periods. One potential concern is that unemployment in the pre-crisis years was simply "too low" for birth weight to show an effect on UI benefit receipt. This raises the question whether our results in fact capture a *change* in the extent to which ill-health is penalized, or whether this effect only *becomes visible* after the crisis. In panel B of Table 4, we address this concern by taking the years from 1978-1982 as our pre-crisis period (panel A reproduces our main results for convenience). While still low, UI take-up was substantially higher in those years compared to the late 1980s (0.8 percent vs 0.2 percent in the private sector). Our results are virtually unchanged by this modification, suggesting that they indeed capture a change in the penalization of ill-health after the crisis. In panel C of Table 4, we test whether the differential impact of birth weight after the crisis fades out over time. We extend our sample to 2007 (the last year observed in our data) and run regressions that include an additional interaction of birth weight with an indicator for observations in these later years. Strikingly, there appears to be only little fade-out, with most of the effect persisting into the 2000s.[10]

Finally, we investigate whether our main results hide any non-linearities in the effect of birth weight on UI benefit receipt, and whether impacts differ by gender and zygosity. In Appendix Table 7, we show estimates from regressions in which log birth weight is replaced by dummies for falling below certain birth weight thresholds. The effects on both UI take-up and UI/total income appear to be concentrated in the below 2,000g range, pointing towards strongly non-linear effects. In Appendix Table 8, we split our samples by gender and zygosity. Prior work examining the relationship between birth weight and labor market outcomes

[10]Appendix Table 6 shows that results are robust to various further re-definitions of the samples, including extending the pre-crisis period and letting the post-crisis period start in 1992 rather than 1993. In a further robustness check, we also confirmed that estimating the impacts of birth weight on UI take-up using a probit specification rather than a linear probability model as done here yields qualitatively similar results.

has found little heterogeneity in the effects by zygosity or twin gender (a proxy for zygosity used in Royer 2009 and Black, Devereaux and Salvanes 2007). In contrast, our results show that effects appear to be concentrated among both female monozygotic twins and male dizygotic twins, with more muted responses among the remaining groups.

4.3. **Mechanisms.** We now examine the potential mechanisms behind our results. We focus on three main channels, education, occupational sorting, and adult health, and also discuss the role of Swedish labor market institutions (in particular, the Swedish Employment Protection Act). Starting with education, panel B of Table 5 shows results from regressions which control for years of education (panel A again reproduces our main estimates for convenience). To the extent that less educated workers are more vulnerable to job loss during economic crises, the small positive effect of birth weight on education documented in Bharadwaj, Lundborg and Rooth (2018) could be driving the negative impacts on UI benefit dependence found above. The estimates in Table 5 suggest this is not the case, however, as controlling for education leaves these impacts virtually unchanged.

Turning to occupational sorting, panel C of Table 5 shows results from regressions that control for five indicators for industry of employment. Similarly, in panel D, we control for 287 individual (4-digit) occupation dummies. The estimates are hardly affected by the inclusion of these controls, suggesting that occupational sorting by birth weight is not driving our results. Finally, we investigate whether differences in adult health are responsible for the increased propensity to take up UI benefits after the crisis among lower birth weight individuals. For this purpose, panel E of Table 5 reports estimates from regressions which control for the number of hospitalizations between 1987 and 1990 as a proxy for adult health. Again, the estimates are almost unchanged. Overall, the results in Table 5 therefore suggest that the effect of birth weight on UI benefit dependence after the crisis is not operating through the channels of differential investments in education, differential occupational sorting, or differences in adult health.[11]

An important concern while examining unemployment in Sweden is the possibility that our effects are purely driven by the Swedish Employment Protection Act (SEPA), rather than health per se. For example, a prominent feature of the Swedish employment law is the idea of "last-in-first-out," according to which employers dismiss people based on job tenure rather than productivity or other considerations (Von Below and Skogman Thoursie 2010). This affects our interpretation if individuals with worse health enter the labor force later than healthier twin counterparts. The strength of these employment protection acts has been debated in the Swedish context and we refer the reader to the Appendix for an in-depth discussion of these issues. The main take away from our examination of the literature surrounding SEPA is that the "last-in-first-out" principle basically has lost its initial intentions and has rendered unclear the practice governing dismissals. While we unfortunately do not observe job tenure in our data, in Appendix Table 9 we show that effects of birth weight and hospitalizations look broadly similar for older and younger cohorts. If the employment protection issues were driving our results, we might have expected to see instead that the main results are driven by job loss in the younger cohorts (since they presumably start their jobs later than people in the older cohorts).

Overall, the evidence suggests that education, occupational sorting, adult health, and the Swedish labor market institutions are not the main mechanisms behind the differential effect of birth weight on UI benefit dependence after the crisis. While we are unable to investigate this here due to a lack of data, it is therefore likely that alternative factors such as cognitive development (which is linked to birth weight in studies including Figlio, Guryan, Karbownik,

[11]We confirmed that also in our sample, birth weight strongly predicts years of education within twins. Our results are robust to controlling for separate dummies for number of years of education, rather than linear years of education as in Table 5. We also find that birth weight affects industry of employment. Conditioning on twins being in the same industry of employment does not change our main estimates much either. Finally, hospitalization data are available only from 1987, which is why in panel E of Table 5 we control for hospitalizations between 1987 and 1990. We confirmed that birth weight has a negative effect on hospitalizations within twins, which is however not statistically significant at conventional levels.

and Roth (2014) and Bharadwaj, Eberhard, and Neilson (2018)), or non-cognitive development (also linked to birth weight in the work of Yi, Heckman, Zhang, and Conti (2015)) play an important role in mediating the effects of birth weight during recessions.

4.4. **The role of the safety net.** In Table 6 we examine the effects of health on total income, including labor market earnings and benefit payments. Interestingly, the results show no differential impact of birth weight on income after the crisis. This is an important finding as it suggests that despite the high level of unemployment during this period and the new structural level of unemployment reached after the crisis, those with worse health did not see a differential drop in their *total* income, but rather just a differential increase in the fraction of income coming from UI benefits. This points to the importance of a social safety net in mitigating the effects of poorer health on labor market outcomes during economic downturns.

5. Conclusion

This paper shows that pre-determined health is an important marker for labor market vulnerability during macroeconomic downturns. Our empirical analysis focuses on the differential effect of birth weight on UI benefit dependence after the Swedish crisis of the early 1990s as compared to before it. In order to establish a causal effect, we focus on random variation in birth weight between twins. The results show that individuals with higher birth weight are less likely to take up UI benefits, and receive a lower share of their total income from UI benefits, after the crisis. These effects are especially pronounced in the private sector, potentially because employers there are more likely to lay off ostensibly weaker (lower-birth-weight) individuals in response to economic downturns. While differences in pre-determined health thus lead to increased inequality in employment outcomes, we also show that there is no differential effect of birth weight on total income after the crisis. This suggests that in the context of Sweden, the social safety net is able to mitigate the effects of poorer health on labor market outcomes during economic downturns.

Appendix

The Swedish crisis. In this subsection, we summarize the roots of the Swedish crisis, relying heavily on Englund (1999) and Holmlund (2011). At the beginning of the 1980s the Swedish economy was characterized by a regulated credit market, a fixed exchange rate, and fiscal policies that aimed at full employment. Inflation, to a large extent driven by rapidly increasing wages, was consistently higher than in the neighboring economies and reached a high of over 10 percent in 1990 (Holmlund 2011). In order to protect its export industry from increasing costs, Sweden devalued the Swedish krona on six occasions between 1973 and 1982.

Despite high inflation, the real interest rate was extremely low, and sometimes even negative, as a result of a tax system with high marginal tax rates combined with generous opportunities for interest deductions. The Swedish credit market had been tightly regulated since World War II, but was deregulated during the first half of the 1980s. The increased ability to borrow, combined with a tax system that made loans cheap, created a price bubble in the real estate sector. Further, and as discussed earlier, unemployment was low throughout the decade, and extremely low in the second half, and probably lower than equilibrium level of unemployment (Holmlund 2011). Overall, these circumstances led to sharp increases in prices and wages in the Swedish market in the late 1980s.

Then a series of factors - mostly policy-driven - interacted to create a sharp contraction of the Swedish economy. We make no statement about which factors were most important and only aim to describe them. First, in 1991 a new tax system with lower marginal tax rates and reduced opportunities for interest deductions was introduced. This implied an increase in real interest rates, resulting in a sharp fall in property prices. In downtown Stockholm, the price of real estate decreased by 35 percent in 1991 (Englund, 1999, pp. 90). Between 1988 and 1992 household savings increased by 12 percentage points, which constituted an important reason for the sharp decline in domestic demand between 1990 and 1993 (Holmlund, 2011, pp. 4).

Second, the central bank decided to defend a fixed exchange rate. This implied that devaluations of the Swedish currency were no longer going to be used to compensate for the negative effect of wage inflation on the competitiveness of the export industry. In the end of the 1980s, production and employment in the export industry started to fall rapidly. The central bank defended the fixed exchange rate until November 1992 when they finally decided to float the Swedish Krona, which in practice led to a devaluation of the currency. The defense of the fixed exchange rate also led to increased interest rates, but internationally higher interest rates as a result of the German unification and the introduction of the new tax system also played a role in this increase (Englund, 1999, pp. 89).

Third, the crises coincided with a dramatic reduction in labor demand in the public sector. This was caused by large deficits in public finances during this period, leading to cuts in public spending. Instead of compensating for the fall in demand for labor in the private sector, as was often done in the past, the reduction in public employment instead contributed to the fall in overall employment during the crisis.

The crisis lasted until the late 1990s. The reason for this prolonged period of the crisis was a desire to keep fiscal and monetary policies restrictive. Monetary policy had to be restrictive in order to create credibility for the new low-inflation regime, while fiscal policy had to deal with the budget deficit by increasing taxes and cutting costs. During the late 1990s both fiscal and monetary policy became less restrictive, while at the same time the international economy improved.

Employment Protection Laws in Sweden. Numerous theses and articles have been written in the field of law during the last ten years concerning the Swedish Employment Protection Act (SEPA).The consensus in this literature seems to be that SEPA has gradually, since its start in 1982, lost its original intention on how to protect employees in the case of dismissal. The intent was to force employers to use objective standards (so called “turordningsregler" in Swedish) when deciding on whom to dismiss, but cases/practice in court has turned to increasingly meet employer’s interest in choosing subjectively whom to fire.

The SEPA actually consists of two criteria: dismissals made for personal reasons and dismissals made due to a redundancy of labor. We start by discussing the latter, since it is likely to be the more common one implemented during the crisis. The SEPA dictates that a shortage of work ought to be the main justification for laying off workers and that a dismissal by the employer must be made on objective grounds. When a firm decides to lay off some of its employees for this reason it is not allowed to choose at will, instead the protection of employees is met by implementing a seniority rule, the so called “last in - first out" principle.

However, the SEPA contains a number of possibilities to circumvent this principle, making it possible for employers to subjectively choose whom to dismiss. For example, if the firm is bound by collective agreements, and a clear majority of firms in Sweden are, the workforce at the firm can be divided into smaller units based on their union affiliation and work task, and the “last-in-first-out” principle can then be applied to each such unit separately. This implies that during a crisis, layoffs can be directed towards a specific unit within the firm, and hence, make it possible to keep those workers that are important to the firm, and dismiss those that are not (see von Below and Skogman Thoursie (2010) for more details).

Furthermore, the SEPA also allows employers to discriminate based on personal reasons when deciding whom to dismiss, for example that a worker’s education or another type of qualification is deemed insufficient. The employer can even be allowed to dismiss workers based on personal characteristics, if these same characteristics can be motivated as being important for doing the job. Wilhelmsson (2001) presents a large number of cases that have been ruled in the Labor Court in line with the view of the employer. A worker’s low performance, insufficient customer focus and results orientation has been ruled by the Labor Court as acceptable for a termination due to incompetence or lack of professional skills, a worker’s lack of judgment as a basis for a dismissal because of negligence, and a worker’s poor health or inadequate body constitution forms the basis for a dismissal because of reduced work capacity. However, after reading a few of these court cases ourselves it is fair to say that the Labor Court sometimes rule in line with the employer, but also in line with the employee being dismissed. For example, in case AD 1993:42 a company was allowed to dismiss two employees who due to work related injuries could no longer perform some common work tasks. In another case, AD 1994:115, an employee had undergone rehabilitation for a long time and could only work part-time. The employer dismissed him due these factors, but this was turned down by the court. To summarize, Glavå (1999), Rönnmar (2001), Calleman (2000) and Wilhelmsson (2001) all argue that the “last in - first out" principle basically has lost its initial intentions and rendered unclear practice governing dismissals in the Swedish labor market.

Surprisingly, given the amount of political debate over SEPA in Sweden there has been very little work on the causal effect of the SEPA on hiring and dismissal strategies of firms; hence it is hard to answer the question of whether the seniority rule is truly binding or not. However, we have found one study for Sweden looking exactly at whether the separation strategies of firms changes when SEPA was reformed. In 2001 there was a reform of the SEPA targeted at smaller firms, making it possible for firms with ten employees or fewer to withdraw two of its employees from the ranking list of who to dismiss. Hence, the rules governing dismissals with respect to seniority became more lenient after the reform. von Below and Skogman Thoursie (2010) use this reform in a difference in difference framework and analyze whether the reform changed the dismissal due to seniority differently for small (2-10 employees) and large (11-15 employees) firms. They find that the effect of the reform was smaller for workers with long tenure (5 years or longer, making up around 15-18 percent of the data) compared to workers with short tenure (0-4 years, see Panel C in their Table 3). Since the exemption rule was expected to make it *easier* for firms to layoff workers with long seniority, one interpretation of this result is that the seniority rule was not in effect even before the reform.

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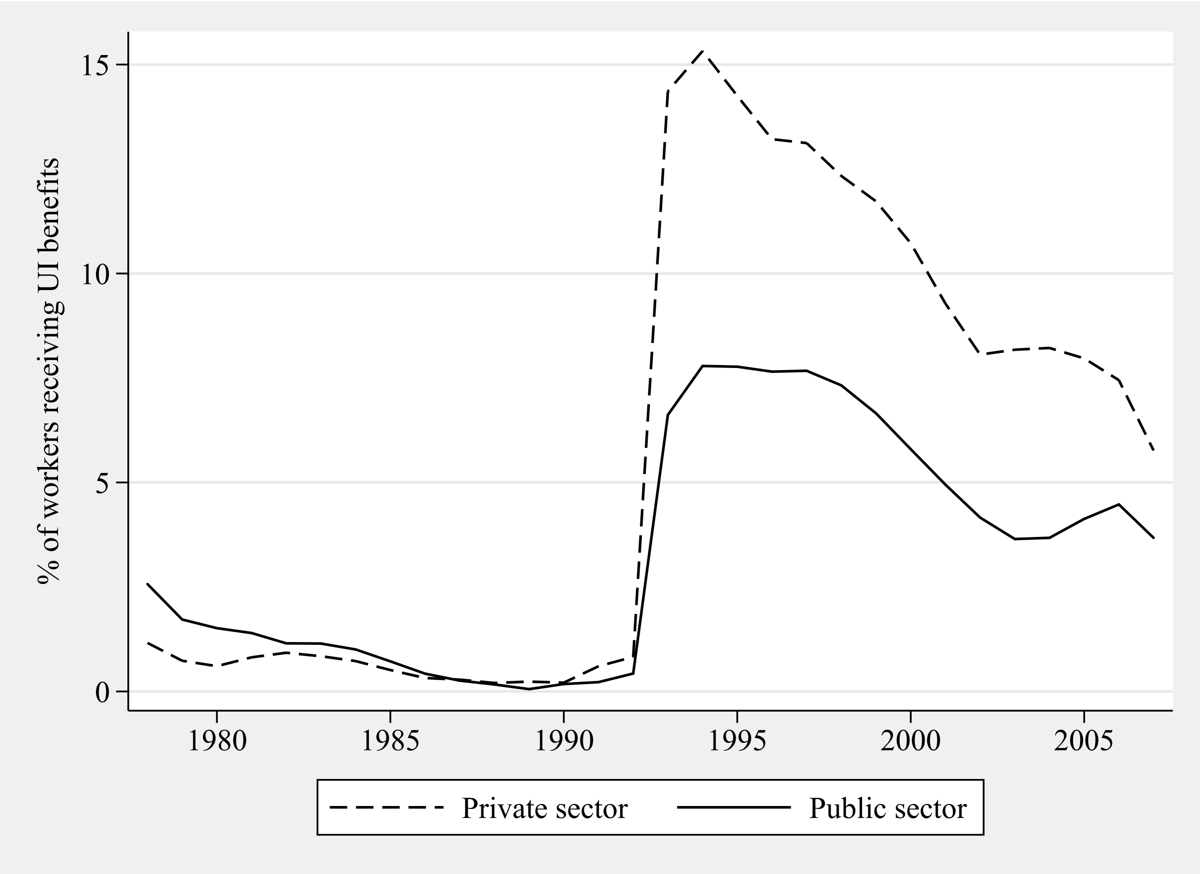
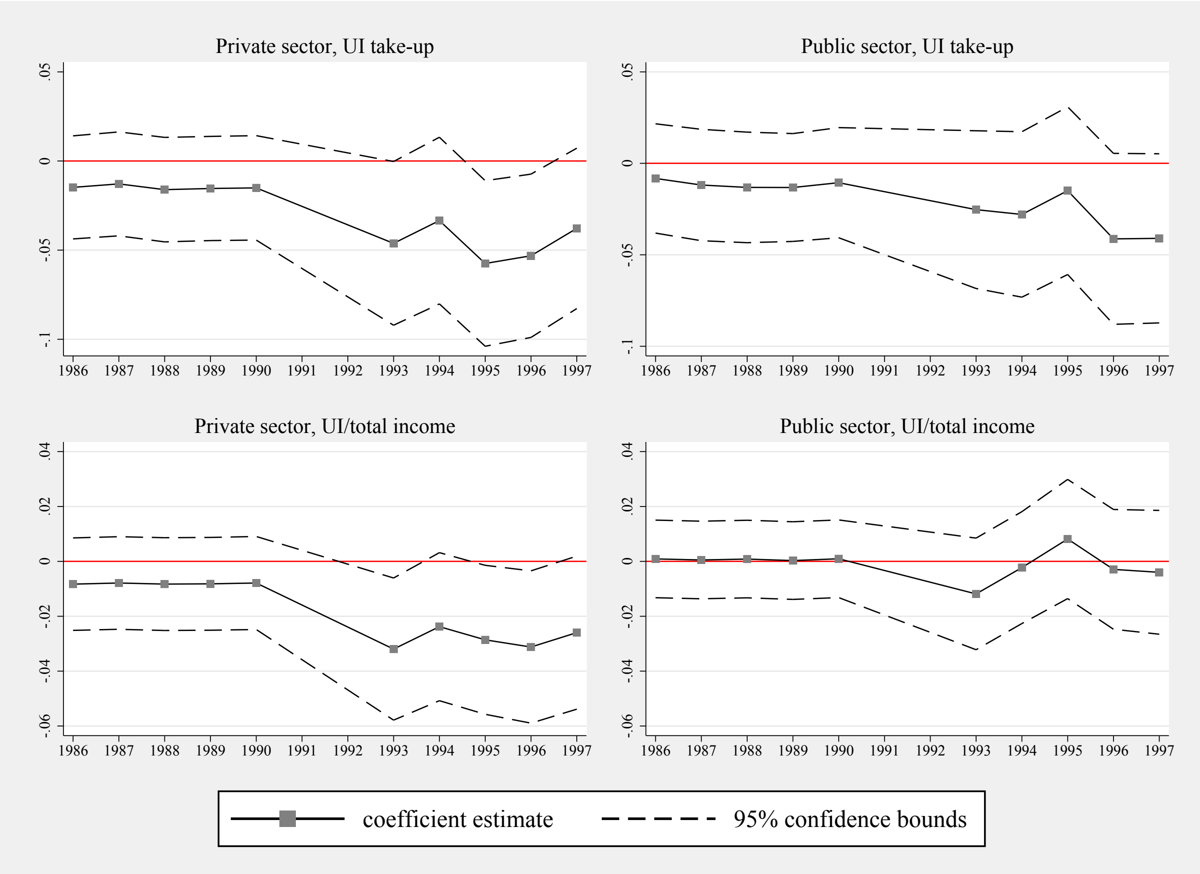
Figure 1
UI take-up by sector of employment (twins sample)



Notes: The figure shows the fraction of workers among twins in the main analysis sample who receive unemployment insurance benefits by sector of employment and year.

Figure 2

Year-by-year impacts of birth weight on UI take up and UI/total income



Notes: The figure shows estimates of the impact of birth weight on UI take-up and the share of total income due to UI, by sector of employment and year. The estimates are based on the regression models in Table 3, with the difference that the post dummy is replaced by a full set of year dummies. Standard errors are clustered at the twin pair level.

Table 1

Summary statistics for the main analysis samples

	Private sector	Public sector
Birth weight	2,666.684	2,602.284
	(507.940)	(499.969)
Male	0.730	0.193
	(0.444)	(0.395)
Age in 1990	44.484	43.880
	(8.292)	(8.006)
Years of schooling	10.278	12.181
	(2.657)	(2.976)
UI take-up		
1986-1990	0.002	0.002
	(0.027)	(0.025)
1993-1997	0.139	0.068
	(0.292)	(0.215)
UI/total income		
1986-1990	0.000	0.000
	(0.004)	(0.003)
1993-1997	0.067	0.026
	(0.173)	(0.110)
No. of twin pairs	5,481	2,930
No. of individual twins	10,962	5,860
No. of individual-year obs.	89,858	46,994

Notes: The table shows means and standard deviations (in parentheses) of key variables, separately for the private sector sample and the public sector sample. UI take-up is an indicator for receiving any unemployment insurance benefits during a given year. UI/total income is the fraction of total income (including labor market earnings, sickness and welfare pay, etc.) coming from unemployment insurance benefits. The number of individual-year observations is lower than the number of twins multiplied by the number of years (10 years in total from 1986-1990 and from 1993-1997) because not every twin pair is observed in every year in the sample, see Appendix Table 1 for details.

Table 2
Comparison of the twin sample with the full population

	Cohorts 1926-1938		Cohorts 1939-1948		Cohorts 1949-1958	
	Full pop. (1)	Twins (2)	Full pop. (3)	Twins (4)	Full pop. (5)	Twins (6)
	Panel A: Means of socio-economic variables					
Male	49.5	55.2	51.0	54.5	51.3	58.3
Age	58.5	57.1	46.2	46.0	36.6	36.5
Years of schooling	10.5	10.7	11.3	11.4	11.6	11.9
Log income	7.1	7.3	7.3	7.4	7.2	7.3
	Panel B: Returns to schooling					
Years of schooling	.103***	.081***	.078***	.062***	.067***	044***
	(.000)	(.004)	(.000)	(.003)	(.000)	(.003)
No. of observations	908,269	2,862	1,078,529	5,582	1,031,995	5,392

Notes: Panel A shows means of socio-economic variables for the full population of Swedish workers, based on data from the 1990 census, and for the combined private and public sector twins analysis samples, separately for three groups of cohorts. The twins sample is restricted to twin pairs observed in the year 1990 (hence the slightly lower number of observations in this table compared to Table 1). Log income and years of schooling in the twins sample are recoded to match the definitions in the census for this table only. Panel B shows estimates from regressions of log income on years of schooling, which additionally control for an indicator for male, age, and age squared. Robust standard errors in parentheses. *** $p<0.01$.

Table 3

Birth weight, UI take-up, and UI/total income, 1986-1990 vs 1993-1997

	UI take-up		UI/total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
log birth weight	–0.0148	–0.0114	–0.0081	0.0007
	(0.0147)	(0.0150)	(0.0086)	(0.0072)
post	0.3686***	0.1914*	0.2197***	0.0419
	(0.1392)	(0.1154)	(0.0813)	(0.0505)
log birth weight × post	–0.0307*	–0.0186	–0.0202**	–0.0033
	(0.0176)	(0.0147)	(0.0103)	(0.0064)
No. of observations	89,858	46,994	89,858	46,994
No. of twin pairs	5,481	2,930	5,481	2,930

Notes: The table shows estimates from regressions of an indicator for receiving any UI benefits (columns 1 and 2) and the share of total income due to UI benefits (columns 3 and 4) on log birth weight, an indicator for post, the interaction between these variables, and twin fixed effects. Standard errors clustered at the twin pair level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 4

Birth weight, UI take-up, and UI/total income, different sample periods

	UI take-up		UI / total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
	Panel A: 1986-1990 vs 1993-1997 (main results)			
log birth weight × post	–0.0307*	–0.0186	–0.0202**	–0.0033
	(0.0176)	(0.0147)	(0.0103)	(0.0064)
No. of observations	89,858	46,994	89,858	46,994
Mean outcome (pre)	0.002	0.002	0.000	0.000
Mean outcome (post)	0.139	0.068	0.067	0.025
	Panel B: 1978-1982 vs 1993-1997			
log birth weight × post	–0.0301*	–0.0176	–0.0204**	–0.0038
	(0.0177)	(0.0148)	(0.0103)	(0.0064)
No. of observations	89,560	46,376	89,560	46,376
Mean outcome (pre)	0.008	0.016	0.001	0.002
Mean outcome (post)	0.139	0.068	0.067	0.025
	Panel C: 1986-1990 vs 1993-1997 vs 1998-2007			
log b. weight × 93-97	–0.0308*	–0.0117	–0.0186*	–0.0015
	(0.0170)	(0.0148)	(0.0099)	(0.0070)
log b. weight × 98-07	–0.0226*	–0.0109	–0.0176***	–0.0011
	(0.0118)	(0.0118)	(0.0067)	(0.0049)
No. of observations	171,928	91,120	171,928	91,120
Mean outcome (pre)	0.002	0.002	0.000	0.000
Mean outcome (93-97)	0.139	0.068	0.067	0.025
Mean outcome (98-07)	0.088	0.046	0.038	0.016

Notes: The table shows estimates from regressions like in Table 3 for samples with different definitions of the pre- and post-crisis period as indicated in each panel heading. Standard errors in parentheses clustered at the twin pair level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 5
Mediating factors for birth weight effects

	UI take-up		UI/total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
	Panel A: main results			
log birth weight × post	–0.0307*	–0.0186	–0.0202**	–0.0033
	(0.0176)	(0.0147)	(0.0103)	(0.0064)
	Panel B: control for years of schooling			
log birth weight × post	–0.0304*	–0.0166	–0.0201*	–0.0026
	(0.0176)	(0.0147)	(0.0103)	(0.0064)
	Panel C: control for industry of employment (5 cat.)			
log birth weight × post	–0.0325*	–0.0182	–0.0206**	–0.0035
	(0.0176)	(0.0147)	(0.0103)	(0.0064)
	Panel D: control for occupation (287 cat.)			
log birth weight × post	–0.0291*	–0.0124	–0.0163	–0.0014
	(0.0170)	(0.0151)	(0.0101)	(0.0067)
	Panel E: control for hospitalizations during 1987-1990			
log birth weight × post	–0.0313*	–0.0185	–0.0205**	–0.0031
	(0.0177)	(0.0148)	(0.0103)	(0.0065)
No. of obs. (all panels)	89,858	46,994	89,858	46,994
No. of twin pairs	5,481	2,930	5,481	2,930

Notes: The table shows estimates from regressions like in Table 3 which additionally control for potential mediating factors. Panel A reproduces the main results from Table 3. Panel B adds controls for linear years of schooling. Panel C adds five indicators for industry of employment. Panel D adds 287 indicators for occupation. Panel E adds indicators for the number of hospitalizations during 1987-1990. For all control variables, interactions with the post dummy are additionally included in the regressions. Missing values on the control variables are imputed at the variable mean, and dummies for missing values for the variable are added to all regressions. Standard errors in parentheses clustered at the twin pair level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 6

Birth weight and total income (including income from benefits)

	Private (1)	Public (2)
log birth weight	0.0902**	0.0816*
	(0.0448)	(0.0458)
post	–0.0097	0.0439
	(0.1748)	(0.1889)
log birth weight × post	0.0004	0.0038
	(0.0222)	(0.0240)
No. of observations	89,858	46,994
No. of twin pairs	5,481	2,930

Notes: The table shows estimates from regressions in which the outcome is total income (including labor market earnings and income due to unemployment and sickness benefits). Standard errors in parentheses clustered at the twin-pair level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 1
Sample selection

Sample	No. of individuals	No. of twin pairs
A. Raw BIRTH Data	46,618	23,309
B. Within sample A, only those with information on birth weight	35,318	17,659
C. Within sample B, only same sex twins	26,418	13,209
D. Within sample C, only those with information on sector of employment in 1985	20,738	10,369
E. Within sample D, only those where both twins are employed in the same sector in 1985	14,154	7,077
F. Within sample C, only those with information on sector of employment in 1990	20,190	10,095
G. Within sample F, only those where both twins are employed in the same sector in 1990	13,632	6,816
H. Number of unique twin pairs across samples E and G combined (main analysis sample)		
total	16,822	8,411
in the private sector	10,962	5,481
in the public sector	5,860	2,930

Notes: The table shows how subsequent restrictions of the sample reduce the number of observations from the raw BIRTH data (sample A) to the sample used in the main analysis (sample H, which combines samples E and G). At each step, only the twin pairs where the indicated information is available for both twins are retained. Next to conditioning on both twins being employed in the same sector (private versus public), samples E and G also require both twins to earn a positive income and to have information on UI benefit receipt. Note that in the final sample, not all twin pairs are observed in all years, with some pairs only observed in the pre-crisis period and some pairs only observed in the post-crisis period.

Appendix Table 2

Birth weight, UI take-up, and UI/total income, 1986-1990 vs 1993-1997, estimates without twin fixed effects

	UI take-up		UI/total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
log birth weight	–0.0000	0.0010	0.0001	0.0002
	(0.0017)	(0.0024)	(0.0002)	(0.0002)
post	0.3257**	0.1070	0.1720**	0.0358
	(0.1312)	(0.1283)	(0.0775)	(0.0704)
log birth weight × post	–0.0241	–0.0053	–0.0134	–0.0014
	(0.0166)	(0.0163)	(0.0098)	(0.0090)
No. of observations	89,858	46,994	89,858	46,994
No. of twin pairs	5,481	2,930	5,481	2,930

Notes: The table shows estimates from regressions like in Table 3, with the only difference being that twin fixed effects are not controlled for. Standard errors in parentheses clustered at the twin pair level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 3

Birth weight, UI take-up, and UI/total income, 1986-1990 vs 1993-1997, balanced panel of twins

	UI take-up		UI/total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
log birth weight	–0.0180	0.0014	–0.0066	0.0052
	(0.0165)	(0.0171)	(0.0096)	(0.0079)
post	0.3522**	0.1346	0.1994**	0.0265
	(0.1443)	(0.1182)	(0.0841)	(0.0509)
log birth weight × post	–0.0287	–0.0114	–0.0177*	–0.0013
	(0.0183)	(0.0151)	(0.0107)	(0.0065)
No. of observations	69,120	35,260	69,120	35,260
No. of twin pairs	3,456	1,763	3,456	1,763

Notes: The table shows estimates from regressions like in Table 3, with the only difference being that the sample is restricted to twins observed in every year between 1986-1990 and 1993-1997. Standard errors in parentheses clustered at the twin pair level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix Table 4

Birth weight, UI take-up, and UI/total income, 1986-1990 vs 1993-1997, measurement error analysis

	UI take-up		UI/total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
log birth weight	–0.0148	–0.0117	–0.0081	0.0006
	(0.0145)	(0.0147)	(0.0085)	(0.0071)
post	0.3732***	0.1827	0.2230***	0.0389
	(0.1373)	(0.1129)	(0.0802)	(0.0494)
log birth weight × post	–0.0314*	–0.0176	–0.0207**	–0.0029
	(0.0174)	(0.0144)	(0.0102)	(0.0063)
No. of observations	89,858	46,994	89,858	46,994
No. of twin pairs	5,481	2,930	5,481	2,930

Notes: The table shows estimates from regressions like in Table 3, with the only difference being that the continuous birth weight variable is recoded into 50g bins before taking logs. Standard errors in parentheses clustered at the twin pair level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix Table 5

Birth weight and UI/total income, 1986-1990 vs 1993-1997, controlling for total income

	Private (1)	Public (2)
log birth weight × post	–0.0222**	–0.0033
	(0.0099)	(0.0064)
No. of observations	89,858	46,994
No. of twin pairs	5,481	2,930

Notes: The table shows estimates from regressions of UI/total income like in Table 3, with the only difference being that indicators for 10 deciles of total income are additionally included as controls. Standard errors in parentheses clustered at the twin pair level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 6

Birth weight, UI take-up, and UI/total income, further different sample periods

	UI take-up		UI / total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
	Panel A: 1986-1990 vs 1991-1992 vs 1993-1997			
log b. weight × 91-92	0.0018	0.0036	–0.0009	0.0005
	(0.0042)	(0.0030)	(0.0011)	(0.0004)
log b. weight × 93-97	–0.0313*	–0.0179	–0.0203**	–0.0032
	(0.0176)	(0.0147)	(0.0103)	(0.0064)
No. of observations	106,002	55,734	106,002	55,734
	Panel B: 1986-1990 vs 1992-1996			
log birth weight × post	–0.0304*	–0.0112	–0.0181*	–0.0017
	(0.0175)	(0.0144)	(0.0099)	(0.0066)
No. of observations	90,130	47,128	90,130	47,128
	Panel C: 1978-1990 vs 1993-1997			
log birth weight × post	–0.0314*	–0.0195	–0.0202*	–0.0037
	(0.0176)	(0.0145)	(0.0103)	(0.0064)
No. of observations	163,572	84,558	163,572	84,558
	Panel D: 1988-1990 vs 1993-1995			
log birth weight × post	–0.0312	–0.0110	–0.0218**	–0.0015
	(0.0193)	(0.0147)	(0.0106)	(0.0063)
No. of observations	53,938	28,234	53,938	28,234

Notes: The table shows estimates from regressions like in Table 3 for samples with different definitions of the pre- and post-crisis period as indicated in each panel heading. Standard errors in parentheses clustered at the twin pair level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 7

Birth weight, UI take-up, and UI/total income, 1986-1990 vs 1993-1997, non-linear effects

	UI take-up		UI/total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
bw $\leq$ 1500g $\times$ post	0.0398	0.0111	0.0196	0.0089
	(0.0331)	(0.0225)	(0.0212)	(0.0115)
bw $\leq$ 2000g $\times$ post	0.0332***	0.0147	0.0192***	0.0033
	(0.0121)	(0.0096)	(0.0070)	(0.0041)
bw $\leq$ 2500g $\times$ post	0.0107	0.0033	0.0075*	–0.0003
	(0.0069)	(0.0059)	(0.0040)	(0.0027)
bw $\leq$ 3000g $\times$ post	–0.0057	0.0079	–0.0008	0.0007
	(0.0077)	(0.0074)	(0.0045)	(0.0034)
No. of observations	89,858	46,994	89,858	46,994
No. of twin pairs	5,481	2,930	5,481	2,930

Notes: The table shows estimates from regressions like in Table 3, with the only difference being that the continuous birth weight variable is replaced by indicators for falling below a specific birth weight threshold. Each coefficient in the table comes from a different regression. Standard errors in parentheses clustered at the twin pair level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 8

Birth weight, UI take-up, and UI/total income, 1986-1990 vs 1993-1997, split by gender and zygosity

	UI take-up		UI/total income	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
	Panel A: Male monozygotic twins			
log birth weight × post	0.0278	–0.0972	0.0135	–0.0403
	(0.0330)	(0.0591)	(0.0196)	(0.0278)
No. of observations	26,164	4,282	26,164	4,282
No. of twin pairs	1,541	275	1,541	275
	Panel B: Male dizygotic twins			
log birth weight × post	–0.0438*	–0.0489	–0.0262*	–0.0071
	(0.0249)	(0.0366)	(0.0148)	(0.0146)
No. of observations	40,072	4,426	40,072	4,426
No. of twin pairs	2,387	284	2,387	284
	Panel C: Female monozygotic twins			
log birth weight × post	–0.1706***	–0.0152	–0.1130***	–0.0044
	(0.0573)	(0.0245)	(0.0325)	(0.0103)
No. of observations	9,874	16,670	9,874	16,670
No. of twin pairs	634	1,014	634	1,014
	Panel D: Female dizygotic twins			
log birth weight × post	–0.0008	0.0132	–0.0129	0.0089
	(0.0557)	(0.0223)	(0.0287)	(0.0101)
No. of observations	12,332	21,178	12,332	21,178
No. of twin pairs	826	1,330	826	1,330

Notes: The table shows estimates from regressions like in Table 3 for different sub-samples as indicated in each panel heading. Standard errors in parentheses clustered at the twin pair level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 9

Birth weight, UI take-up, and UI/total income, 1986-1990 vs 1993-1997, split by cohorts

	Private		Public	
	Born 1926-1942 (1)	Born 1943-1958 (2)	Born 1926-1942 (3)	Born 1943-1958 (4)
	Panel A: UI take-up			
log birth weight	–0.0345	–0.0056	–0.0194	–0.0070
	(0.0245)	(0.0183)	(0.0236)	(0.0190)
post	0.5098**	0.3070*	0.0668	0.2596*
	(0.2559)	(0.1657)	(0.1475)	(0.1536)
log birth weight × post	–0.0494	–0.0226	–0.0047	–0.0264
	(0.0324)	(0.0210)	(0.0188)	(0.0196)
No. of observations	29,518	60,340	14,402	32,592
No. of twin pairs	1,821	3,660	876	2,054
	Panel B: UI/total income			
log birth weight	–0.0202	–0.0025	–0.0194	–0.0070
	(0.0145)	(0.0107)	(0.0236)	(0.0190)
post	0.2516	0.2046**	0.0668	0.2596*
	(0.1630)	(0.0915)	(0.1475)	(0.1536)
log birth weight × post	–0.0240	–0.0184	–0.0047	–0.0264
	(0.0207)	(0.0116)	(0.0188)	(0.0196)
No. of observations	29,518	60,340	14,402	32,592
No. of twin pairs	1,821	3,660	876	2,054

Notes: The table shows estimates from regressions like in Table 3 for two different sub-samples, which cover different cohorts. Standard errors in parentheses clustered at the twin pair level. * p<0.10, ** p<0.05, *** p<0.01.