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Health and Unemployment during Macroeconomic Crises

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ABSTRACT

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This paper shows that health is an important determinant of labor market vulnerability during large economic crises. Using data on adults during Sweden's unexpected economic crisis in the early 1990s, we show that early and later life health are important determinants of job loss after the crisis, but not before. Adults who were born with worse health (proxied by birth weight) and those who experience hospitalizations (and especially so for mental health related issues) in the pre-crisis period, are much more likely to lose their jobs and go on unemployment insurance after the crisis. These effects are concentrated in the private sector that happened to be more affected by the crisis. The results hold while controlling for individual education and occupational sorting prior to the crisis, and for controlling for family level characteristics by exploiting health differences within twin pairs. We conclude that poor health (both in early life and as adults) is an important indicator of vulnerability during economic shocks.

JEL Classification: I10, I18, J65, E32

Keywords: early life, birth weight, economic crises, shocks, unemployment

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1 Introduction

A large literature in economics has examined the causes and consequences of macroeconomic fluctuations. Given the importance of health human capital for labor market outcomes, an important facet of the literature on consequences of economic fluctuations has examined whether and how events like recessions, job displacements and business cycles affect health outcomes (Ruhm 2000, Stillman and Thomas 2008, Sullivan and Von Wachter 2009, Currie and Tekin 2011). Some of this work has focused on how such events affect early childhood health or even health at birth (see for example Chay and Greenstone (2003), Dehejia and Lleras-Muney (2004), and Paxson and Schady (2005)); this research is especially important given the recent work highlighting the long term economic implications of health *in utero* and during infancy (Heckman 2007, Almond and Currie 2011).

While examining the consequences of macroeconomic shocks on health is extremely important, it is also critical to understand whether people with poorer health *ex ante* are more vulnerable to job loss during a crisis. The research examining *who* is impacted by economic fluctuations has largely examined how business cycles and recessions affect labor market outcomes across a wide range of demographic characteristics such as age, gender, sex, race and education (Clark and Summers 1981, Bound, Holzer, et al. 1995, Engemann and Wall 2009, Cho and Newhouse 2012, Hoynes, Miller, and Schaller 2012). However, despite the large body of important work in this area, there appear to be few studies examining whether pre-determined health, such as health at

birth, dictate the degree to which one is affected during economic downturns. In this paper, we build on the literature examining who is affected during a crisis to show that *pre-crisis health* (both, health at birth and health in adulthood) is an important marker for labor market vulnerability during economic downturns.

We study the effects of health on job loss before and after an arguably exogenous and dramatic increase in unemployment in Sweden in the early 1990s, when unemployment went from 2% to 8% in less than 2 years. This increase in unemployment was largely the result of layoffs rather than voluntary quits (Skans, Edin, and Holmlund 2009). This crisis is referred to as one of the "Big Five" downturns along with that of Spain, Norway, Finland, and Japan according to Reinhart and Rogoff (2008). Many observers of the Great Recession in 2008 compared it to the Swedish crash of the 1990s, and they especially noted the ways in which Sweden recovered from the crisis (New York Times, September 22, 2008; Time, September 24, 2008). 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 the unexpected crisis was the result of a combination of various factors including, monetary policies in the 1980s, budget deficits, financial deregulation, and collapse of trade. We make a crucial distinction here by examining the effects of the crisis in the public and private sector. Prior work has shown that the effects of such economic crises differ across the public and private sectors (Kopelman and Rosen 2015), and the Swedish case was no exception. Our

own data and the work of others (Lundborg 2001) show that a larger share of workers were displaced from the private sector than the public sector.

We use two measures of health, observed at two very different points during an individual's life, to highlight the wide reaching consequences of pre-determined health. Using birth weight as an indicator of health at infancy,[1] we examine how adults who were born with lower birth weight fare during the Swedish crisis. We find that adults who were born with poorer health at birth were much more likely to face job loss and go on unemployment insurance (UI) during the crisis. While this result is true for individuals who work in the private sector, it does not hold for individuals who work in the public sector (despite the fact that the public sector also experienced job reductions during this period). This suggests that the private sector is able to respond to macroeconomic shocks by laying off ostensibly weaker individuals (those with lower birth weight) more so than the public sector. Recognizing that birth weight likely represents nutritional inputs and other attributes of the mother and the family that might

[1] 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 we examine birth weight differences within twins in this setting, the variation in birth weight is more likely due to fetal nutritional intake rather than maternal behaviors (Royer 2009). While other measures of health at infancy are sometimes used (APGAR scores, for example), given the historical nature of the data, we only have birth weight data available to us.

confound such long term analysis, we examine plausibly exogenous variation in birth weight within twin pairs similar to prior studies (Almond, Chay, and Lee 2005, Black, Devereux, and Salvanes 2007, Royer 2009).

We use the same setting (twins comparisons, and analyzing public and private sector employees separately) to examine the role of *adult* health before and after a crisis. While the identifying assumptions in this instance (relative to the assumptions required when examining twin differences in birth weight) are stronger, at the very least, it provides a useful way of controlling for time invariant family level characteristics (we expand on these assumptions in Section 3). Using information on individual hospitalizations, we show that individuals with poorer adult health prior to the crisis were significantly more likely to face job loss after, but not before, the crisis. This is again largely true for individuals working in the private sector, and for workers who were hospitalized for mental health conditions prior to the crisis. Hence, we show that both early life health and adult health matter for job loss during a crisis.

We then explore *why* poorer health might make individuals more susceptible to job loss during crises. We find that the relationship between pre-crisis health and UI take up during the crisis is not mediated via factors like educational attainment or pre-crisis selection into occupations. For example, since the private sector and the manufacturing industry were hit extensively by the crisis, one hypothesis might be that individuals with worse health select into sectors and occupations that just happened to be more affected by the crisis. However, our results hold when we examine twin pairs who worked in the *same*

sector, 3 digit or 5 digit occupation code (while magnitudes are similar for all three, we lose statistical significance due to smaller samples when restricting the data to same occupation codes), and for twins who have the same level of education.[2] While job tenure is argued to be a determinant of hiring/firing decisions in the Swedish context, we unfortunately do not observe job tenure in the data. However, we can confirm that our results are not driven by relatively younger adults who might be more likely to lose their jobs under a "last in-first out" policy.[3]

Examining the relationship between pre-determined health and unemployment, before and after economic shocks, requires rather unique data. Most electronic birth records, even in countries known for their excellent administrative records (for example Norway), start in the late 1960s. For this reason, examining how pre-determined health endowments affect job attachment during major crises has been under-explored since subjects for whom we have reliable birth data are generally too young to be observed for a substantial period in the labor market before and after the crisis. In the case of Sweden, we use a unique source of twin birth records collected for nearly the entire population of births between 1926-1958. These unique birth records are then matched to individual yearly income (including income from sources such as unemployment insurance, disability, sickness etc) records from 1981-2005 and to hospitalization records starting in 1987. Hence, most of our sample is ob-

[2]While birth weight itself might be a factor that determines adult health, educational attainment, and occupational sorting, we find that these interlinkages are not first order in out setting. We discuss this in greater detail in Section 5.

[3]We provide additional information on the weak enforcement of employment protection laws in Sweden in the Appendix.

served while they were active in the labor market for several years before and after the crisis.

This paper underscores the importance of health in determining labor market outcomes via the notion that health matters more for job attachment during economic crises. Our paper documents that better health at infancy and in adulthood can be particularly protective during periods of economic fluctuations. Recent work has shown 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 2012). We add to these papers 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 more broadly during a crisis. 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 and variation in adult health not explained by family level unobservables, we are able to show that social assistance, at least in the case of Sweden, appears to come to the aid of those who have a health disadvantage.

2 Background

2.1 The 1990s crisis in Sweden

Unlike most European countries, unemployment in Sweden remained low during the 1980s and fluctuated between 2 to 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 Swedish 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 level until it started to fall in 1997. The decrease in employment occurred in both the private and the public sector, with the private sector being more affected (Lundborg 2001). The sectoral spread of UI take up in our twins sample confirms these findings and is shown in Figure 1. We describe the roots of the Swedish crisis, relying heavily on Englund (1999) and Holmlund (2011), in the Appendix.

2.2 The UI System in Sweden

The basic rules that regulate the right to reimbursement from unemployment funds have largely been the same since the 1930s.[4] The government subsidies

[4]One has to be at least 16 years of age, able to work, and had to have reported as seeking a job at the Swedish Public Employment Service. In addition to these, between 1973 and 1994, there was an employment requirement in place. This required an individual to have been a paying member of the unemployment fund for at least 12 months prior to becoming unemployed. For full compensation, it has also been required that the reason for unemployment is due to involuntary unemployment. Unemployment benefits could still be paid to workers who quit their job and become unemployed or to workers who get fired due

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). The monthly membership fees, which are typically small, cover 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 Arbetsmarknadsstod in Swedish) from the government, but the benefits paid out was much lower than those of the unemployment funds and the entitlement period substantially shorter.

By international standards, the replacement rate in the Swedish unemployment insurance has historically been generous. Whereas the 80s and early 90s saw replacement rates of about 90 percent of earnings, there was a ceiling on the benefit level. This meant that the actual replace rate may have been much lower than 90 percent, and especially so for high-earning workers. In 1996, it was for instance estimated that 75 percent of employees had monthly earnings exceeding the ceiling. From 1974 and 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 and then further reduced to 75% in 1996 but then increased to 80 percent again in 1997 (Carling et al, 2001). In 1994 the working requirement was also changed such that one

to misbehavior, but the rules then become less generous. In such cases, the rules allow the unemployment funds to subtract days of compensation to the person. In 2007, for instance, a worker who voluntarily quit his job, lost 45 days of unemployment benefits.

needed to have worked for at least 75 hours per month during a five month period, or alternatively, for 65 hours per month during a 10 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.

In summary, although it became more difficult to qualify for UI during the period after the crisis, it is important to reiterate that our twins based methodology implies that both twins face the exact same labor market conditions and rules regarding UI. Moreover, any effects on UI that we do find, would be *despite* the fact that it became more difficult to qualify for UI.

3 Theoretical Framework

In this section we write down a simple framework where employers observe and make hiring, firing, and compensation related decisions based on a composite index (I) of an employee's characteristics (we think of these as being a "productivity" index of the individual as in Heckman (1998)). This index, in our simplified framework, depends on health (H) and other factors such as education (Ed). Since the focus of the paper is on examining the role of health, we ignore the interlinkages between health and education for the time being and think of current health as a function of past health.

Hence at time t, we formalize the above as follows (to be precise, since we typically observe individuals over the age of 30 we can also assume that all

education related investments have already taken place by the time we observe them in the labor market; i.e. education stops at an age k, where $k < t$):

$$I_t = q(H_t, Ed_k) \tag{1}$$

$$H_t = f(H_0 \dots H_{t-1}) \tag{2}$$

Health at time t is a function of health at birth H_0 as well as health at all points since, until the previous period, H_{t-1}. A simple, linear representation of equation 1 results in the following expression for productivity at time t:

$$I_t = \alpha_0 H_0 + \sum_{n=1}^{n=t-1} \alpha_n H_n + \tau Ed_k + \epsilon_t \tag{3}$$

We consider employers making hiring and firing decisions based on cutoffs of the productivity index I. In particular, we assume that employers fire employees if $I_t < \underline{c}$, where $\underline{c}$ is some minimum level of productivity necessary to obtain and/or maintain a given job. During an economic crisis, standards for keeping workers might become more stringent, and therefore employers fire individuals whose productivity is below c' where $c' > \underline{c}$. In our case, hiring and firing decisions are captured by the individual's observed take up of unemployment insurance (UI), and we can estimate for each given point in time t (also, we only observe one measure of post birth health, so we further simplify equation 3 from above), under different hiring/firing conditions:

$$UI_t = \beta_t H_0 + \gamma_t H_{t-1} + \zeta_t Ed_k + \epsilon_t \qquad when \qquad I_t < \underline{c} \tag{4}$$

$$UI_{t+1} = \beta_{t+1} H_0 + \gamma_{t+1} H_{t-1} + \zeta_{t+1} Ed_k + \epsilon_{t+1} \qquad when \qquad I_{t+1} < c' \tag{5}$$

The above equations represents our main equations of interest: the impact of health at birth and health in adulthood on unemployment before (t) and after ($t+1$) the requisite exit conditions for work change (from $\underline{c}$ to c'). In other words, our goal is to compare β_t to β_{t+1}, and γ_t to γ_{t+1}. The underlying hypothesis is that when employment conditions become more strict (i.e. under condition c'), those with poorer health ex ante (implying lower overall productivity indices) are more likely to lose their jobs and take up UI.

We wish to highlight a few aspects about estimating equations 4 and 5. One main concern is that for any given individual, there are aspects hidden in the unobserved component ϵ that drive both, health at various points in time, as well as unemployment. These unobserved aspects could be family specific or individual specific. Our methodology of using twin fixed effects is crucial for purging from equation 4 and 5, all family specific time invariant characteristics. These would include aspects such as parental education and health, which one could easily claim as affecting the health of the child and subsequent employment opportunities. *Individual* specific attributes, such as general ability, however, are not purged while using twins fixed effects.

It should be noted that the assumptions required when examining adult health

differences within twin pairs are particularly stronger relative to examining twin differences in birth weight. Twin variation in birth weight is due to causes beyond those that the children concerned or the mother can control, and hence, considered largely exogenous. Adult health differences within a twin pair, however, could well be the result of individual level behaviors and actions, which could also affect the outcome variable of interest. Hence, while twins fixed effects go some distance towards controlling for family specific characteristics, we cannot rule out that there could be other factors that are correlated with health differences and labor market outcomes that might be driving the results. This worry however, is mitigated when we compare twin fixed effects estimates from the pre-crisis period to the post-crisis period, similar to a difference-in-difference design. In that instance, we need the assumption that the individual and time varying drivers of health and labor market outcomes would have led each twin to have the same trends in job attachment before the crisis. Unfortunately the data on hospitalizations during the pre-crisis period exist for too short a time period to examine parallel trends.

Second, there are several possible interlinkages that the current specification glosses over. For example, as stated earlier, it is easy to imagine that education is also a function of health. Hence, for most of our analysis we present results not controlling for education and allowing the reduced form impacts of health to reflect health and education impacts (although we show results including education as well). Third, adult health (captured by H_{t-1} above) can also be a function of early life health (H_0). Hence, we present results where we separately include H_0 and H_{t-1} and also when we include them jointly. It

turns out that both these concerns are not first order.

4 Data and Econometric Specification

4.1 Data

We use data from a number of administrative registers. Data on birth weight comes from the BIRTH register, which collects data on birth outcomes of all twins born in Sweden between 1926-1958. The data originates 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 are/were required by law to collect and preserve birth information, the researchers where able to obtain data for a high fraction of twins. The data includes essential birth information, such as birth weight, sex, geographical markers, birth length (but lack information typically included in modern registers, such as APGAR scores), and personal identifiers, where the latter means that the data can be merged to other administrative registers in Sweden.

Due to the way in which the birth data was collected, the sample of twins only includes twins that survived up to 1972. The reason is that in 1972, an extensive survey on the twin cohorts born 1926-1958 was conducted. Since the data from this survey contained variables deemed important for twins research, the surveyors set out to collect birth data only for twins participating in the survey. Fortunately, the response rate was high (86%). Since we do not have

access to the universe of twins born in 1926-1958, we are unable to construct weights or assess attrition in any systematic manner.[5]

For our measure of adult health, we use data on hospitalizations from the Swedish National Patient Register (NPR). The register covers all hospitalizations from 1987 and onwards and contain detailed data on diagnoses (ICD codes) and length of stay. Information to NPR is delivered to the Centre for Epidemiology (EpC) at the National Board of Health and Welfare from each of the 21 county councils in Sweden. In our analyses, our main measure is a binary indicator of having any hospitalization in the pre-crisis period. Since the hospitalization data is collected after 1987, we use any hospitalizations during 1987 and 1988 as the basis for examining the role of adult health on labor market outcomes during 1989 and 1990 (pre-crisis). For the post-crisis period, we use the full data on any hospitalizations between 1987-1990.

With the use of the personal identifiers, the BIRTH data was linked to both the NPR and the Income and Taxation register (IoT). The income (labor market earnings plus all taxable benefits such as unemployment benefits, sickness pay and welfare pay) records we have access to start in 1968 and end in 2007 and are present at the yearly level. We lose less than 1 percent of the data due to matching issues across the twins data and the income register. The labor market earnings records come from the equivalent of W2 records in the United States, in that the income is reported by employers and is not based on self reports. Taxable benefit income is reported directly by the administrative

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

agency. Hence, combined, we consider income measures in this data to be accurate. All of our income data is adjusted by the 2007 CPI measure to make them comparable across years.

We use two primary measures to capture an individual's job loss status before and after the crisis. First, we create a binary variable indicating take up of any unemployment insurance in a given year (this is an "extensive" measure of UI). Second, we measure the fraction of income coming from unemployment insurance out of total income (we consider this as an "intensive" measure of UI). In order to shed light on possible mechanisms through which health affects unemployment, we use information on schooling and occupation. We obtain information on individual years of schooling from the education register (utbildningsregistret, UREG) from 1990 (or from 2007 for those individuals missing in the 1990 data), where years of schooling has been imputed based on obtained degree. We use data on occupation from the censuses in 1985 and 1990. These data contain 4-digit codes on occupation and sector of employment (public or private).

4.2 Summary Statistics

In our analyses, we impose a number of necessary restrictions that affect the sample size (Table 1). First, from the BIRTH register, we select twin pairs where both twins have non-missing records on birth weight. This reduces the sample size from 46,618 (23,309 twin pairs) to 35,318 individual twins (17,659 twin pairs). Second, we restrict our sample to same-sex twin pairs, further

reducing the sample size to 26,418 individual twins. Third, since we are interested in estimates by sector of work, we select twin pairs where both twins are in the labor force before the crisis and where data on occupation is non-missing. This further reduces the sample to 20,190 individual twins (10,095 twin pairs) when conditioning on non-missing data on sectoral employment in 1990 (the comparable number conditioning on non-missing data on sectoral employment in 1985 is 20,738 individuals, or 10,369 twin pairs). The sample sizes conditioning on *both* twins working in the same sector brings the sample size down to 7,077 twin pairs (using sectoral classification in 1985) and 6,816 twin pairs (using sectoral classification in 1990). Appendix Table 1 shows descriptive statistics for the twin samples. The twins are approximately 44 years old, have between 10-12 years of schooling (based on sector of employment)[6] and have an average birth weight between 2,593-2,666 grams (again, depending on sector of employment). It is also important to note that in our sample only 20% of the employees in the public sector are male, while around 75% of the employees in the private sector are male. Hence, there are significant sectoral differences based on gender composition of the workforce in Sweden (in line with the findings in Rosen (1997)).

In order to shed light on the external validity of our results, we compare the characteristics of twins to that of the general population born in the same time period. The sample of twins look very similar to the non-twin population

[6]That the average education for twins in the public sector is about two years higher than the one for twins employed in the private sector is something we find also for the full population. When calculating the same numbers for the full population using the 1990 Census, and using the same cohorts (1926-1958), average years of education is 12.2 for those employed in the public sector and 10.6 for those employed in the private sector.

(Table 2) along important observable characteristics. Columns 1 and 2, show for example, that the full population and the twin population born in the decade between 1926-38 are quite similar in terms of years of schooling and income. Twins and non-twins born in other cohorts (born 1939-48, or born 1949-58) also appear similar along these margins.

Another way to examine how twins differ from the full population is to compare the returns to schooling among twins and non-twins. In the lower panel of Table 2, we estimate Mincerian returns to schooling.[7] Again, the twin and non-twin samples appear similar (in fact there appears to be a general decline in returns to schooling across cohorts, in both the twin and non-twin samples) with the exception that for cohorts born 1949-1958, we observe lower returns to schooling in the twin sample.

4.3 Econometric Specification

We follow other papers that have used twins fixed effects as the basis for our empirical specification. For a given outcome Y (take up of unemployment insurance for instance) for person i belonging to family j in year t, we estimate the following relationship in the case of birth weight as the main independent variable of interest:

$$Y_{ijt} = \beta_t H_{ij0} + \xi_t X_{ijt} + \mu_j + \epsilon_{ijt} \tag{6}$$

[7]The Mincerian income/earnings regressions are estimated by OLS and include years of schooling, age, age squared, and an indicator for male.

In this equation H_0 is log birth weight measured in grams or a measure of low birth weight (less than 2500 grams for example, or less than some specific threshold) and X's are individual specific variables, which in our case includes years of education, occupation categories, and sector of employment. η_j is the twin or family fixed effect. In other words, β_t can be interpreted as the coefficient on the difference in birth weight within twins in a given calendar year t. We estimate equation 7 for years before and after the crisis for the regression tables (our "pre-crisis" period covers 1986-1990 and our "post-crisis" period covers 1993-1997)[8] and for each year for the graphs. We cluster standard errors at the family level. This equation is estimated separately for twins working in the private and public sector.

In the case of adult health as the main independent variable of interest, we estimate a variant of equation 6:

$$Y_{ijt} = \gamma_t H_{ijt-1} + \eta_t X_{ijt} + \mu_j + v_{ijt} \tag{7}$$

Here, H_{t-1} captures the health of the individual in adulthood (measured as any hospitalization event) prior to the crisis. Other inputs in equation 7 have the same interpretation as the inputs in equation 6 (see above). The only difference is that our pre-crisis period in this instance covers 1989-1990 and post-crisis period covers 1993-1997; and hence to examine pre-crisis labor mar-

[8]Our choice of the 5 year period between 1993-1997 in the post-crisis era is motivated by the fact that the crisis affected the public sector later (compared to the private sector). Note that our headline private sector results are *not* sensitive to the choice of examining just these five years post-crisis. Results using 1993-1994, 1994-1995 and 1995-1996 as our post-crisis years yield very similar results (available upon request).

ket outcomes we use information on hospitalizations from 1987-1988 and for post-crisis labor market outcomes use information on hospitalizations from 1987-1990.[9]

5 Results

5.1 Early life health

We begin by examining the relationship between unemployment insurance payments (UI) as a fraction of total income (TI) and birth weight in the years leading up to and after the crisis, by sector of employment. Figures 2 and 3 show the twins fixed effects estimates of estimating equation 6 for each year between 1983 and 2005, by sector. The independent variable of interest in this case is the natural log of birth weight.

Figure 2 very clearly shows the main point of this paper: adults who were relatively higher birth weight than their twin counterparts in the private sector have lower UI payments relative to total income (hereafter referred to as UI/TI) after the crisis. Birthweight does not seem to play an important role in determining UI payments as a fraction of total income in the public sector after the crisis (Figure 3). Figure 2 also shows that the birth weight-UI/TI relationship is persistent after the crisis. Those that happened to go on UI after the crisis appear to stay on it for many years. While the standard errors

[9]The main results are similar when using the 1987-1988 pre-crisis period for examining post-crisis labor market outcomes.

in this figure seem large, pooling pre and post-crisis years improves precision. The estimates in Tables 3 and 4 show this relationship by combining a few years before the crisis (1986-1990) and few years after the crisis (1993-1997).[10] The years 1991 and 1992 are transitionary years before the full effect of the crisis hit, and while the figures include it, we omit them in the regressions since it is unclear whether they should be included in the pre or post-crisis years.

Table 3 shows in regressions that birth weight matters significantly for UI/TI after the crisis but only in the private sector. As noted earlier, the private sector was more affected during the crisis than the public sector. Table 3 shows that a 10% increase in birth weight reduces the fraction of total income coming from UI by 6% in the post-crisis period in the private sector (the OLS results, presented in Appendix Table 2, underestimate these impacts suggesting an important role for controlling for unobserved family characteristics). The difference-in-difference estimate (comparing the pre and post-crisis effect within sectors) shows a statistically significant post-crisis effect for the private sector, but not for the public sector. This pattern is reinforced when examining the results for discordant twins (twins whose birth weight difference is more than 10%). Note that our sample in the post-crisis period consists of different individuals, mainly due to people switching across occupational sectors or retiring from the workforce. However, a balanced sample analysis presented in Appendix Table 3 shows similar results in magnitude, albeit with

[10]Since UI is only available to people who were previously employed, we condition the "pre" years on being employed in 1985 and the "post" years on being employed in 1990. Note that we only have direct employment and occupational data from 1985 and 1990.

less precision.

We also examine the extensive margin of UI take up, since UI/TI could also reflect the fact that lower birth weight decreases the *ceiling* of UI payments eligible before the crisis (if lower birth weight weight individuals worked fewer hours or earned less pre-crisis). Table 4 presents the results from examining the relationship between birth weight and UI take up (a binary variable indicating *any* income from UI during the pre and post-crisis periods). The results are presented in the same format as Table 3 and imply while birth weight has a larger effect on UI take up in the post-crisis period, the difference in effects across sectors are not as stark as in the case of UI/TI (the difference-in-difference coefficient is -0.0491 in the private sector and -0.0372 in the public sector). Examining discordant twins in Table 4, we see significant differences in post versus pre-crisis take up of UI in the private sector and smaller effects in the public sector (not statistically significant). The difference in sectoral effects across Tables 3 and 4 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 after the crisis. Finally, while sickness benefits before the crisis was sometimes used in lieu of unemployment benefits, our analysis shows that both UI and sickness benefits (calculated as the share of total income from *both* UI and sickness benefits (SB)) has no correlation with birth weight before the crisis (see Figures 4 and 5, regression results available upon request). Hence, our main UI results are not simply the result of misclassifying the type of benefit prior to the crisis.

Appendix Table 4 examines whether there are any non linearities in the birth weight-UI/TI relationship before and after the crisis. While most of the coefficients are not significant at the conventional levels, the magnitudes indicate some strong non-linearities in this relationship especially in the private sector post-crisis. Most of the effects appear concentrated in the below 2000 gram range. For example, being less than 1500 grams (Very Low Birth Weight) increases the fraction of income coming from UI after the crisis by nearly 71% (coefficient of 0.048 off a base of 0.067). Appendix Table 5 shows that birth weight measurement error issues that are discussed in Bharadwaj, Lundborg, and Rooth (2015) are not a concern in this context. Even if we mechanically introduce measurement error by rounding all birth weight data to the nearest 50 gram, our results are unchanged.

5.2 Adult health

Turning to the effects of adult health on job loss, we see very similar patterns to what we observed for birth weight in Table 3. As mentioned earlier, our measure of adult health is a binary variable indicating ever having been hospitalized. Table 5 shows that in the public sector, ever having been hospitalized has no impact on UI as a fraction of income either before or after the crisis. In the private sector, however, the impacts are quite large after the crisis. In the post-crisis period, ever having been hospitalized in the *pre-crisis* period (1987-1990) increases the UI/TI ratio by 1.4 percentage points. Off a base of 6.7%, this is a magnitudinally meaningful increase of 21%. The

difference-in-difference coefficient in the private sector is of similar magnitude and statistically significant. Turning to Table 6, we find similar results for UI take up. Again, there are small effects in the public sector, but any hospitalizations in the pre-crisis period results in a 2.5 percentage point increase in the probability of UI take up in the post-crisis period.[11] This is a 17% increase from the mean take up of UI during this period. Hence, the results confirm that adult health is an important factor of job vulnerability in the private sector.

Table 7 shows that a major factor in the determination of job vulnerability is hospitalization for mental illnesses.[12] Once again, this table shows that in the public sector, mental illness hospitalizations in the pre-crisis period do not matter for UI/TI in the post-crisis period. However, this is not true in the private sector. Hospitalization for mental illnesses pre-crisis leads to a significant increase in the fraction of income coming from UI post-crisis. Appendix Tables 7a and 7b show broad categories of hospitalization causes that we observe in the data. While the point estimates for UI/TI and UI take up for these other diagnosis are positive and magnitudinally meaningful, none are statistically significant. Finally, we can also examine an alternative measure of adult health – number of hospitalizations, instead of whether *any* hospitalization occurred. These results shown in Appendix Table 8, are consistent with Table 5.

As mentioned in Section 2, we need to consider the extent to which early life

[11] A balanced sample analysis is presented in Appendix Table 6 with similar results.

[12] Although, if we exclude hospitalizations due to mental illness from our main specifications in Table 5, our results are still statistically significant.

health itself affects later life health. Since the regressions in Table 5 do not control for early life health, we present estimates where both, hospitalization incidence and birth weight are included in the same regression. Appendix Table 9 reveals nearly identical results to that in Tables 3 and 5. Hence, it appears that the impact of birth weight on the component of adult hospitalizations that matter for UI take up is minimal.

5.3 Mechanisms

Table 8 examines whether the effect of pre-crisis health on UI related payments after the crisis is explained by intermediate factors such as educational attainment and occupational sorting *prior* to the crisis. For example, if individuals with lower birth weight attain less education and if the less educated are more vulnerable to job loss during economic crisis, then the effects observed in Table 3 would simply proxy for education rather than a broad measure of early childhood health. Alternatively, the less educated could have worse adult health and hence, the results in Table 5 could again reflect fewer educational investments. Similarly, if individuals with worse health are likely to sort into occupations that are more likely to be hit by the crisis, then the effects are driven purely by the relationship between pre-crisis health and occupational sorting, rather than health and on the job vulnerability.[13]

Columns 1 and 2 in Table 8 control for education linearly and then non-linearly. The magnitudes of the coefficients remain largely unchanged suggesting that

[13]The effects of birth weight on education and occupational sorting, and hospitalizations on occupational sorting are statistically significant; these results are available on request.

education is not a first order intermediary factor. Columns 3, 5 and 6 control for various aspects of occupational choice such as sector of employment (there are 5 sectors of employment defined even within the private sector), and detailed 3 and 4 digit occupation codes. The results are quite stable across these difference specifications; hence, it does not appear that birth weight specific educational sorting, and overall health specific occupational sorting explains much of the results seen in Tables 3 and 5.

To examine this idea further, columns 4 and 7 in Table 8 restrict the sample to twins who share the same sector of employment (5 categories within the private sector), or 3 digit occupation code. Restricting the sample to twins in the same sector results in larger magnitudes; for twins in the same 3 digit occupation code (Column 7), the birth weight and adult health effect is statistically insignificant (this is likely due to the small number of observations where both twins are in the same occupation). The overall results of the this table suggest that the effect of birth weight and adult health on unemployment after the crisis is not operating through the channels of pre-crisis investments in education, the effect of education on adult health, or via pre-crisis occupational sorting.

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 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 have 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 rendered unclear practice governing dismissals. While we unfortunately do not observe job tenure in our data, in Appendix Table 10 we show that effects of birth weight and hospitalizations are not statistically different across older cohorts and younger cohorts – if the employment protection issues were driving our results, we might have expected to see 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).

Finally we examine results by zygosity in Appendix Table 11. Prior work examining the relationship between birth weight and labor market outcomes has found little heterogeneity in the effects by zygosity or twin gender (a proxy for zygosity as used in Royer 2009 and Black, Devereaux and Salvanes 2007). Our results are inconclusive about the role of zygosity or gender in determining the health-UI relationship. Examining just the private sector results, Appendix Table 11 shows that our main effects for birth weight and hospitalizations, and birth weight and UI/TI are of similar magnitude for monozygotic female twins and dizygotic male twins post-crisis. Another reading of this table reveals that our results are also inconclusive by gender as it is not obvious whether the ef-

fects are concentrated among males or females.

5.4 Role of the safety net

In Table 9 we examine, in the same framework as Table 3 and Table 5, the effects of health on total income (income inclusive of labor and benefit payments) across sectors, before and after the crisis. Table 9 shows that despite the large increase in UI take up in the private sector after the crisis, the effect of birth weight and hospitalizations on total income before and after the crisis are nearly identical (the difference in difference estimates shows that these are not statistically different). 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. This suggests the importance of a social safety net in mitigating the effects of poorer health on labor market outcomes during economic downturns.

6 Conclusion

A growing literature has shown the deleterious effects of major economic crises on health. However, no prior work has examined whether pre-existing health, such as health at birth, is a determinant of *who* is affected during large recessions. This paper shows that health at birth, as proxied by birth weight,

and adult health as proxied by hospitalizations, are important sources of job vulnerability during macroeconomic crises. Using data on Swedish twins to control for family level unobservables that might affect both health (as infants and as adults) and subsequent job attachment, we find that individuals with worse health are more likely to become unemployed after the crisis. These effects are concentrated in the private sector, which was more affected by the crisis.

While education and occupational sorting are factors behind who becomes unemployed, these variables do little to mediate the health impacts. Hence, it is likely, that factors such as cognitive development (which is linked to birth weight in studies such as Figlio, Guryan, Karbownik, and Roth (2014) and Bharadwaj, Eberhard, and Neilson (2013)), or non-cognitive development (also linked to birth weight in the work of Conti, Heckman, Yi, and Zhang (2010)) might play an important role in addition to health in determining job vulnerability during recessions.

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Figure 1: Take up of UI by year and sector (Twins Sample)

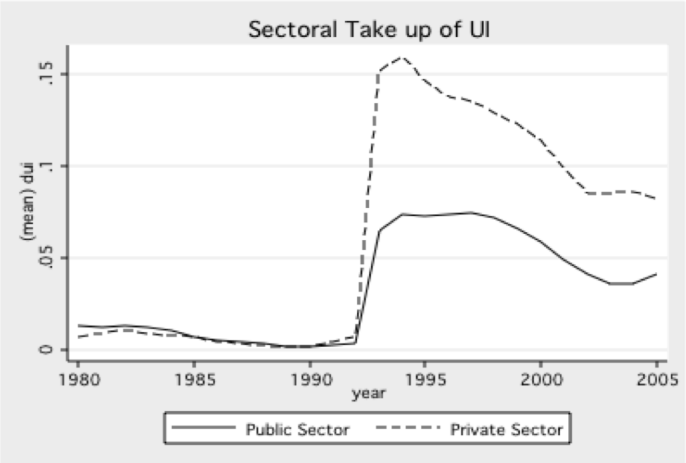

Figure 2: Effects of Log Birth Weight in the Private Sector

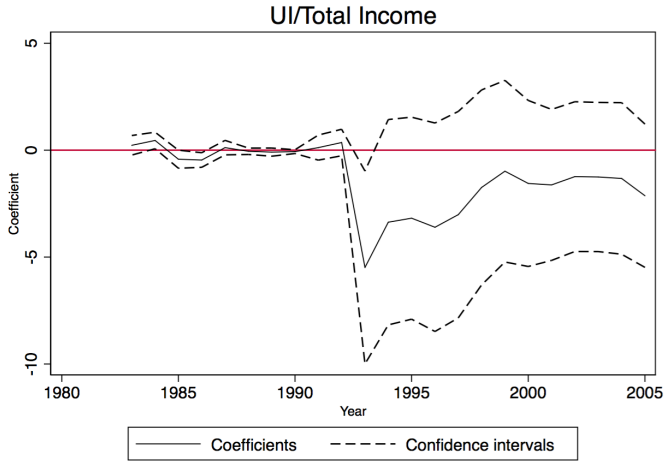

Figure 3: Effects of Log Birth Weight in the Public Sector

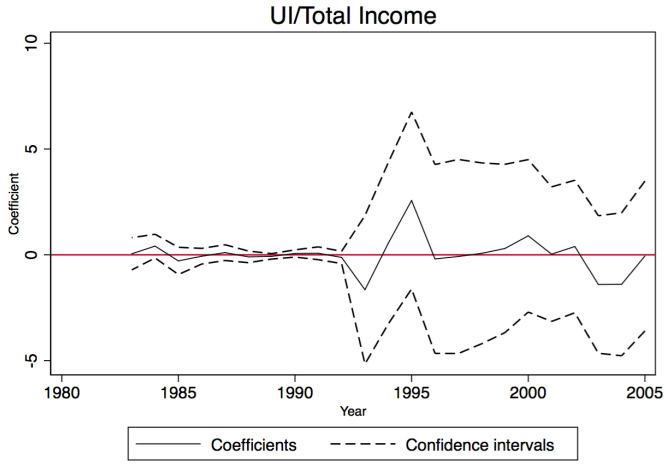

Figure 4: Effects of Log Birth Weight in the Private Sector

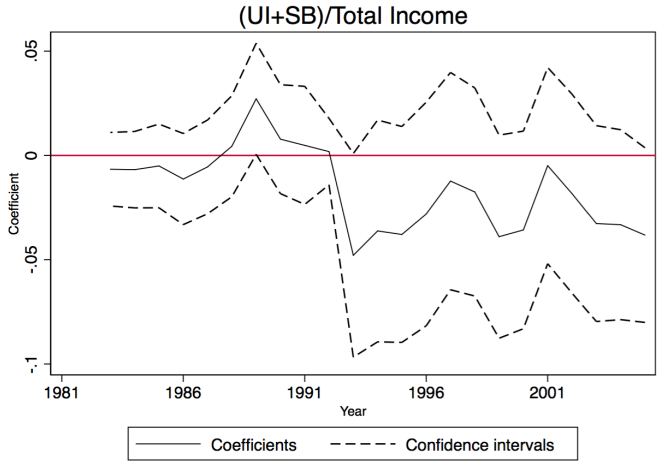

Figure 5: Effects of Log Birth Weight in the Public Sector

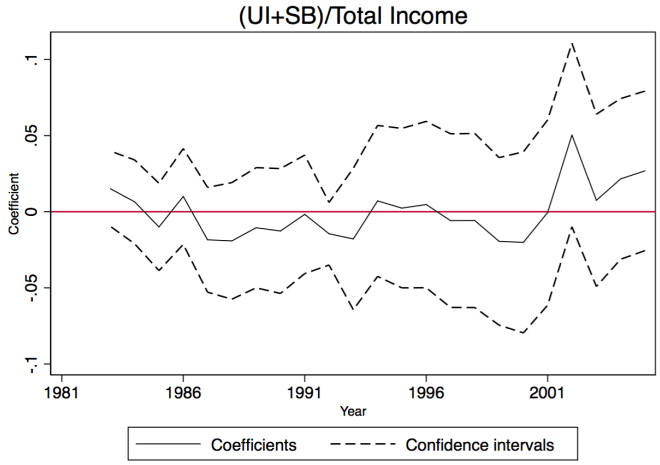

Table 1. Sample Size Table

Sample	Observations	Twin pairs
A. Raw BIRTH Data	46,618	23,309
B. with information on birthweight (and only keeping pairs where information on *both* twins is available)	35,318	17,659
C. only same sex twins	26,418	13,209
D. Information on sector of employment in 1985	20,738	10,369
E. Information on sector of employment in 1990	20,190	10,095
F. Both twins employed in public or private sector in 1985	14,154	7,077
G. Both twins employed in public or private sector in 1990	13,632	6,816
H. Data from hospitalizations	13,632	6,816

Table 2. Comparison of the twin sample with the full population

	1926-1938		1939-1948		1949-1958	
	1	2	3	4	5	6
	Full pop	Twins	Full pop	Twins	Full pop	Twins
A. Descriptive statistics						
Male	49.5	48.0	51.0	48.3	51.3	50.0
Age	58.5	57.3	46.2	46.1	36.6	36.6
	(3.5)	(3.8)	(2.8)	(2.8)	(2.9)	(2.9)
Years of schooling	10.5	10.7	11.3	11.4	11.6	12.0
	(2.2)	(2.4)	(2.4)	(2.5)	(2.3)	(2.6)
Ln Income	7.13	7.16	7.29	7.27	7.18	7.18
	(.72)	(.68)	(.66)	(.63)	(.68)	(.67)
B. Return to education						
Years of schooling	.103***	.084***	.078***	.065***	.067***	.044***
	(.000)	(.003)	(.000)	(.002)	(.000)	(.002)
Nr of observations	908,269	7,949	1,078,529	13,354	1,031,995	13,133

Notes: The comparison in descriptive statistics between the full population and twin sample is made using information from the 1990 Census. Both samples contain the population born 1926-1958. The Mincer type earnings regressions are estimated by OLS and include, other than years of schooling, also age, age squared and an indicator for male. * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors in parentheses.

Table 3. Birth weight and UI/Total Income, 1986-1990 vs 1993-1997. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1986-1990)	Post Crisis (1993-1997)	Pre Crisis (1986-1990)	Post Crisis (1993-1997)
All twins				
Log birth weight	-.0002	-.0039	-.0002	-.0367**
	(.0004)	(.0165)	(.0004)	(.0187)
Mean Outcome	.000	.026	.000	.067
No of twin pairs	2,405	2,346	4,672	4,470
DiD	-.0037		-.0365**	
	(.0165)		(.0187)	
			p=.051	
Discordant twins				
Log birth weight	0.0001	-0.0015	-0.0001	-0.0395**
	(0.0003)	(0.0171)	(0.0004)	(0.0192)
No of twin pairs	1,142	1,099	2,337	2,227
DiD	-.0014		-.0394**	
	(.0171)		(.0192)	
			p=.041	

Notes: This table shows regressions of the share of unemployment insurance payments of total income (UI/TI) on birth weight for the private and public sector, before (1986-1990) and after (1993-1997) the crisis. All coeffcients are from a twin fixed effects model using same sex twins, including both men and women. Cohorts are born 1926-1958. The table also shows the difference-in-difference estimates calculated using the estimates from before/after the crisis. "Discordant twins" only include twin pairs which differ more than 10%, that is, 264g, in birthweight. * $p < .10$, ** $p < .05$, *** $p < .01$. Clustered standard errors in parentheses.

Table 4. Birth weight and UI Take Up, 1986-1990 vs 1993-1997. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1986-1990)	Post Crisis (1993-1997)	Pre Crisis (1986-1990)	Post Crisis (1993-1997)
All twins				
Log birth weight	-.0025	-.0397	-.0045	-.0536*
	(.0037)	(.0340)	(.0036)	(.0317)
Mean Outcome	.002	.068	.002	.139
No of twin pairs	2.405	2.346	4,672	4,470
DiD	-.0372		-.0491	
	(.0342)		(.0319)	
			p=.11	
Discordant twins				
Log birth weight	-0.0021	-0.0316	-0.0043	-0.0621*
	(0.0037)	(0.0352)	(0.0037)	(0.0327)
No of twin pairs	1.142	1.099	2.337	2.227
DiD	-.0295		-.0578*	
	(.0354)		(.0329)	
			p=..079	

Notes: This table shows regressions of UI take up (an indicator for having received any UI payments during the period) on birth weight for the private and public sector, before (1986-1990) and after (1993-1997) the crisis. For additional information, see the notes to Table 3. * p < .10, ** p < .05, *** p < .01. Clustered standard errors in parentheses.

Table 5. Adult health and UI/Total Income, 1989-1990 vs 1993-1997. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1989-1990)	Post Crisis (1993-1997)	Pre Crisis (1989-1990)	Post Crisis (1993-1997)
All twins				
Ever Hospitalized pre crisis	-.0001	-.0009	.0003**	.0144**
	(.0002)	(.0055)	(.0001)	(.0061)
Mean Outcome	.000	.026	.000	.067
No of twin pairs	2,382	2,346	4,601	4,470
DiD	-.0008		.0141**	
	(.0055)		(.0061)	
			p=.021	

Notes: This table shows regressions of the share of UI payments of total income (UI/TI) on whether having been hospitalized pre crisis for the private and public sector, before (1989-1990) and after (1993-1997) the crisis. Ever hospitalized pre crisis is measured for the years 1987-1988 for the 1989-1990 analysis, and during 1987-1990 for the 1993-1997 analysis, see section 4 for details. For additional information, see the notes to Table 3. * p < .10, ** p < .05, *** p < .01. Clustered standard errors in parentheses.

Table 6. Adult health and UI Take Up, 1989-1990 vs 1993-1997. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1989-1990)	Post Crisis (1993-1997)	Pre Crisis (1989-1990)	Post Crisis (1993-1997)
All twins				
Ever Hospitalized pre crisis	-.0019	.0006	.0053***	.0255**
	(.0023)	(.0111)	(.0019)	(.0105)
Mean Outcome	.002	.068	.002	.139
No of twin pairs	2,382	2,346	4,601	4,470
DiD	.0025		.0202*	
	(.0113)		(.0107)	
			p=.060	

Notes: This table shows regressions of UI take up (an indicator for having received any UI payments during the period) on whether having been hospitalized pre crisis for the private and public sector, before (1989-1990) and after (1993-1997) the crisis. Ever hospitalized pre crisis is measured for the years 1987-1988 for the 1989-1990 analysis, and during 1987-1990 for the 1993-1997 analysis, see section 4 for details. For additional information, see the notes to Table 3. * p < .10, ** p < .05, *** p < .01. Clustered standard errors in parentheses.

Table 7. Adult mental health and UI/Total Income, 1989-1990 vs 1993-1997. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1989-1990)	Post Crisis (1993-1997)	Pre Crisis (1989-1990)	Post Crisis (1993-1997)
All twins				
Ever Hospitalized for mental health pre crisis	-.0012	.0019	.0004	.0468*
	(.0012)	(.0242)	(.0003)	(.0276)
Mean Outcome	.000	.026	.000	.067
No of twin pairs	2,382	2,346	4,601	4,470
DiD	.0031		.0464*	
	(.0242)		(.0276)	
			p=.093	

Notes: This table shows regressions of the share of UI payments of total income (UI/TI) on whether ever been hospitalized for mental health diagnosis pre crisis for the private and public sector, before (1989-1990) and after (1993-1997) the crisis. Ever hospitalized for mental health diagnosos pre crisis is measured for the years 1987-1988 for the 1989-1990 analysis, and during 1987-1990 for the 1993-1997 analysis, see section 4 for details. For additional information, see the notes to Table 6. * p < .10, ** p < .05, *** p < .01. Clustered standard errors in parentheses.

Table 8. Examining mediating factors for the relationship between health and UI/Total Income. Private sector, 1993-1997. Twin fixed effects.

	1	2	3	4	5	6	7
Log birth weight	-.0374**	-.0386**	-.0378**	-.0503**	-.0402**	-.0330*	-.0150
	(.0187)	(.0186)	(.0186)	(.0223)	(.0186)	(.0190)	(.0374)
Ever Hospitalized (1987-1990)	.0143**	.0139**	.0140**	.0131*	.0141**	.0140**	.0152
	(.0061)	(.0061)	(.0061)	(.0071)	(.0061)	(.0062)	(.0122)
Controls	Years of schooling (linearly)	Years of schooling (indicators)	Sector of employment (5 cat.)	Both twins have the same sector of employment	Occupation 3 digits (105 cat.)	Occupation 4 digits (287 cat.)	Both twins have the same 3 digit occupation 3 digit
No of twin pairs*	4,470	4,470	4,470	3,209	4,470	4,470	961

Note: This table shows regressions of the share of UI payments of total income (UI/TI) on birthweight and whether having been hospitalized pre crisis exlusively for the private sector and the post crisis years (1993-1997). Coefficients on birth weight and ever hospitalized are obtained from separate regressions. The first column adds a linear years-of-schooling variable, while the second column add indicators for each year of schooling. Column 3-7 instead adds information on pre-crisis employment. Column 3 adds indicators measuring what sector (within the private sector) the person worked in in 1990, while column 4 forces both twins to be working in the same sector of employment in 1990. Column 5 and 6 add indicators for the occupation held in 1990 (3 and 4 digit level, respectively), while column 7 forces both twins to be working in the same type of occupation in 1990. For additional information see notes to Table 3. * $p < .10$, ** $p < .05$, *** $p < .01$. Clustered standard errors in parentheses.

Table 9: Health and Total income (including income from benefits), pre and post crisis for the private sector. Twin fixed effects.

	Pre Crisis[a,b]	Post Crisis (1993-1997)
All twins		
Log birth weight[a]	0.0949*	0.0907
	(0.0495)	(0.0579)
DiD	-.0042	
	(.0762)	
Ever Hospitalized pre crisis[b]	-.0551**	-.0445**
	(.0223)	(.0178)
DiD	.0106	
	(.0285)	
No of twin pairs (different sample sizes since pre crisis defined differently for birth weight and hospitalizations, see footnotes a and b)	4.672/4,601	4,470

Note: Coefficients on birth weight and ever hospitalized are obtained from separate regressions. This table shows regressions of total income on birthweight and whether ever been hospitalized pre crisis exlusively for the private sector, analysing the years before (see a and b below) and after (1993-1997) the crisis. Total income include annual earnings and all work-related benefits, e.g., unemployment insurance, sickness benefits and parental leave benefits. For additional information see notes to Table 3 and 5. * p < .10, ** p < .05, *** p < .01. Clustered standard errors in parentheses.

a) The pre crisis period for analysing the effect of birthweight uses data from 1987-1990.

b) Ever hospitalized for the pre crisis analysis uses hospitalization data from 1987-1988, and UI/TI data from 1989-1990. We use hospitalization data from 1987-1990 for the post crisis analysis.

Appendix Table 1. Descriptive statistics of the analysis sample.

	Public employees		Private employees	
	Pre-crisis sample	Post-crisis sample	Pre-crisis sample	Post-crisis sample
Birth weight	2,593	2,605	2,666	2,665
	(499)	(499)	(507)	(499)
Male	.20	.18	.75	.75
Age in 1990	44.2	43.9	44.9	44.0
	(8.0)	(7.7)	(8.4)	(7.8)
Years of schooling	12.3	12.2	10.2	10.3
	(3.0)	(3.0)	(2.6)	(2.6)
UI/TI	.000	.026	.000	.067
UI take up	.002	.068	.002	.139
Indicators of some hospitalization: 1987-1988				
Any cause	.140	.222	.110	.175
Mental and Behavioral conditions (ICD-8 and ICD-9: 290-319, ICD-10: F00-F99)	.011	.013	.011	.012
Diseases in the Nervous system (ICD-8 and ICD-9: 320-359 ICD-10: G00-G99)	.003	.005	.001	.003
Heart disease (ICD-8: 390-444.1, 444.3-458, 782.4. ICD-9: 390-459. ICD-10: I00-I99)	.011	.016	.012	.019
Accident	.012	.020	.015	.027
All other causes	.103	.168	.071	.114
Nr of twin pairs	2,405	2,346	4,672	4,470

Appendix Table 2. Health and UI/Total Income. OLS Estimates.

	Public sector		**Private sector**	
	Pre Crisis[a,b]	Post Crisis (1993-1997)	Pre Crisis[a,b]	Post Crisis (1993-1997)
All twins				
Ln Birthweight[a]	.0002	-.0017	.0001	-.0133
	(.0002)	(.0084)	(.0002)	(.0094)
Ever Hospitalized pre crisis[b]	.0000	.0063	.0003	.0089*
	(.0001)	(.0042)	(.0002)	(.0050)
No of observations (birth weight/hospitalizations) see footnotes a and b	4,810/4,764	4.692	9,344/9,202	8,940

Notes: Coefficients on birth weight and ever hospitalized are obtained from separate regressions. * p < .10, ** p < .05, *** p < .01. Clustered standard errors in parentheses.

a) The pre crisis period for analysing the effect of birthweight uses data from 1987-1990.

b) Ever hospitalized for the pre crisis analysis uses hospitalization data from 1987-1988, and UI/TI data from 1989-1990. We use hospitalization data from 1987-1990 for the post crisis analysis.

Appendix Table 3. Birth weight and UI/Total Income, 1986-1990 vs 1993-1997. Balanced panel.

	Public sector		Private sector	
	Pre Crisis (1986-1990)	Post Crisis (1993-1997)	Pre Crisis (1986-1990)	Post Crisis (1993-1997)
All twins				
Log birth weight	-.0001	.0093	-.0004	-.0244
	(.0004)	(.0169)	(.0003)	(.0194)
No of twin pairs	1,821	1,821	3,661	3,661
DiD	-.0094 (.0174)		-.0240 (.0194) p=.108	

Notes: See notes to Table 3.

Appendix Table 4. Birth weight categories and UI/Total Income. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1986-1990)	Post Crisis (1993-1997)	Pre Crisis (1986-1990)	Post Crisis (1993-1997)
Birth weight < 1500g	.0002	.0269	.0000	.0480*
	(.0002)	(.0267)	(.0000)	(.0256)
Birth weight < 2000g	.0000	-.0051	.0006**	.0168*
	(.0000)	(.0076)	(.0003)	(.0095)
Birth weight < 2500g	.0002	.0007	-.0001	.0029
	(.0002)	(.0062)	(.0001)	(.0063)
Birth weight < 3000g	.0000	.0005	-.0001	.0002
	(.0003)	(.0068)	(.0001)	(.0067)
No of twin pairs	2,405	2,346	4,680	4,470

Notes: See notes to Table 3. Coefficients are obtained from running separate regressions for being born with a birthweight below each cutoff in the table. * p < .10, ** p < .05, *** p < .01. Clustered standard errors in parentheses.

Appendix Table 5. Birth weight and UI/Total Income, 1986-1990 vs 1993-1997. Measurement error analysis. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1986-1990)	Post Crisis (1993-1997)	Pre Crisis (1986-1990)	Post Crisis (1993-1997)
All twins				
Log birth weight	-.0002	-.0038	-.0002	-.0371**
	(.0004)	(.0162)	(.0004)	(.0184)
Mean Outcome	.000	.026	.000	.067
No of twin pairs	2,405	2,346	4,680	4,470
DiD	-.0037		-.0369**	
	(.0165)		(.0184)	
			p=.046	

Notes: See notes to Table 3. Birthweight is recoded into 50g bins for the full data.

Appendix Table 6. Adult health and UI/Total Income, 1989-1990 vs 1993-1997. Balanced panel. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1989-1990)	Post Crisis (1993-1997)	Pre Crisis (1989-1990)	Post Crisis (1993-1997)
All twins				
Ever Hospitalized pre crisis	-.0001	-.0027	.0002*	.0147**
	(.0003)	(.0052)	(.0001)	(.0066)
Mean Outcome	.000	.017	.000	.061
No of twin pairs	1,821	1,821	3,661	3,661
DiD	-.0026		.0145	
	(.0052)		(.0066)	
			p=.028	

Notes: See notes to Table 5.

Appendix Table 7a. Hospitalization Event and UI/Total Income. Only post crisis, 1993-1997, 1993-1997. Twin fixed effects.

	Ever hospitalized (1987-1990) for:					
	Any cause	Mental Health	Diseases of the Nervous system	Heart disease	Accidents	All other causes
Private Sector						
Twin Fixed effect	.0144**	.0468*	.0359	.0237	.0211	.0026
	(.0061)	(.0276)	(.0448)	(.0173)	(.0137)	(.0070)
Mean Outcome			.067			
No of twin pairs			4,470			
Public Sector						
Twins Fixed Effect	-.0009	.0019	-.0561	.0040	-.0126	.0029
	(.0055)	(.0242)	(.0389)	(.0104)	(.0135)	(.0060)
Mean Outcome			.026			
No of twin pairs			2,346			

Notes: See notes to Table 5 and Appendix Table 1 for ICD classifications used for each column.

Appendx Table 7b. Hospitalization Event and UI Take Up. Only post crisis, 1993-1997, 1993-1997. Twin fixed effects.

	Ever hospitalized (1987-1990) for:					
	Any cause	Mental Health	Diseases of the Nervous system	Heart disease	Accidents	All other causes
Private Sector						
Twin Fixed effect	.0255**	.0810*	.0759	.0474	.0397	.0043
	(.0105)	(.0430)	(.0739)	(.0300)	(.0261)	(.0121)
Mean Outcome			.139			
No of twin pairs			4,470			
Public Sector						
Twins Fixed Effect	.0006	.0202	-.0957	-.0028	-.0308	.0077
	(.0111)	(.0522)	(.0649)	(.0283)	(.0254)	(.0123)
Mean Outcome			.068			
No of twin pairs			2,346			

Notes: See notes to Table 6 and Appendix Table 1 for ICD classifications used for each column.

Appendix Table 8: Adult health (number of hospitalizations) and UI/Total Income, 1989-1990 vs 1993-1997. Twin fixed effects.

	Public sector		Private sector	
	Pre Crisis (1989-1990)	Post Crisis (1993-1997)	Pre Crisis (1989-1990)	Post Crisis (1993-1997)
All twins				
Number of hospitalizations pre crisis	.0000	-.0044	.0000	.0035*
	(.0000)	(.0037)	(.0001)	(.0019)
Mean number of hospitalizations	.233	.384	.194	.318
No of twin pairs	2,382	2,346	4,601	4,470
DiD	-.0044 (.0037)		.0035 (.0019) p=.066	

Notes: This table shows regressions of UI/TI (the share of unemployment insurance payments of total income) on the *number of hospitalizations* for the private and public sector, before (1986-1990) and after (1993-1997) the crisis. See also notes to Table 5.

Appendix Table 9. Health and UI/Total Income. Birth weight and hospitalizations jointly estimated. Only post crisis, 1993-1997. Twin fixed effects.

	Public sector		Private Sector	
All twins				
Log birth weight	-.0039	-0.0038	-.0367**	-.0365*
	(.0165)	(0.0165)	(.0187)	(.0187)
Ever Hospitalized pre crisis		-0.0009		.0144**
		(0.0055)		(.0061)
Mean Outcome	.026	.026	.067	.067
No of twin pairs	2,346	2,346	4,470	4,470

Notes: This table shows regressions of UI/TI (the share of unemployment insurance payments of total income) on birth weight and hospitalizations for the private and public sector post crisis. Column 1 and 3 includes only the estimate for birthweight (as in Table 3), while columns 2 and 4 shows the estimates for birthweight and ever hospitalized pre crisis (1987-1990) when being jointly estimated. * $p < .10$, ** $p < .05$, *** $p < .01$. Clustered standard errors in parentheses.

Appendix Table 10. Health and UI/Total Income by cohort groupings. Private sector, 1993-1997. Twin fixed effects.

Post crisis analysis only	Born 1926-1942	Born 1943-1958
Log birth weight	-.0648*	-.0243
	(.0341)	(.0223)
Ever Hospitalized (1987-1990)	.0102	.0167**
	(.0101)	(.0076)
No of twin pairs	1,392	3,078

Notes: See notes to Table 8

Appendix Table 11. Health and UI/Total Income in the private sector, pre and post crisis. Zygosity by gender. Twin fixed effects.

MZ Twins	**Males**		**Females**	
	Pre Crisis[a,b]	Post Crisis (1993-1997)	Pre Crisis[a,b]	Post Crisis (1993-1997)
Log birth weight[a]	-.0007	.0486	-.0007	-.1059**
	(.0007)	(.0334)	(.0009)	(.0500)
Ever Hospitalized[b]	na	.0036	.0004*	.0311**
		(.0090)	(.0002)	(.0139)
No of twin pairs[a,b]	1,361/1,340	1,300	501/498	500
DZ Twins	**Males**		**Females**	
	Pre Crisis	Post Crisis (1993-1997)	Pre Crisis	Post Crisis (1993-1997)
Log birth weight[a]	.0001	-.0937***	-.0017	-.0081
	(.0005)	(.0286)	(.0017)	(.0493)
Ever Hospitalized[b]	.0003	.0209**	.0009	.0001
	(.0003)	(.0100)	(.0006)	(.0185)
No of twin pairs[a,b]	2,079/2,051	1,996	647/633	610

Notes: Coefficients on birth weight and ever hospitalized are obtained from separate regressions. See notes to Table 3 and 5 for birthweight and ever hospitalized, respectively. Due to perfect collinearity (must be right??) we are not able to arrive at an estimate of ever hospitalized for MZ males in the pre crisis period.

a) The pre crisis period varies for when analysing the effect of birthweight (using 1987-1990) and ever hospitalized (using 1989-1990).

b) Ever hospitalized pre crisis is measured during 1987-1988 for the pre crisis analysis and during 1987-1990 for the post crisis analysis.