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ABSTRACT

Birth Weight in the Long-Run*

We study the effect of birth weight on long-run outcomes, including permanent income, income across various stages of the lifecycle, education, social benefits take-up, and adult mortality. For this purpose, we have linked a unique dataset on nearly all Swedish twins born between 1926-1958, containing information on birth weight, to administrative records spanning nearly entire life time labor market histories. We find that birth weight positively affects permanent income and income across large parts of the life cycle, although there is some evidence of a fade out after age 50. Our results indicate that lower birth weight children are more likely to avail of social insurance programs such as unemployment and sickness insurance and that birth weight matters for adult mortality. We supplement our main analysis with more recent data, which enables us to study how the impact of birth weight on income and education of young adults has changed across cohorts born almost 50 years apart.

JEL Classification: I10, I18

Keywords: birth weight, early life, permanent income, unemployment sickness absence, life-cycle, mortality

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1 Introduction

Economic research in the last decade has shown considerable consensus on the importance of early childhood health in determining various short and long term outcomes (Almond and Currie 2011; Heckman 2007). Birth outcomes, and in particular birth weight, has been shown to matter for educational achievement in school, completed years of education, IQ and labor market outcomes such as employment and earnings. Not only have the economic outcomes examined been numerous, but the settings just as varied, with studies using data from developed countries like the United States, Canada, and Norway to middle income countries like Chile, China and Taiwan and even in less developed countries like South Africa.[1]

One of the key reasons to examine the role of birth weight over the life cycle is to answer questions about the persistence of health inequalities at birth. However, despite the rich number of studies and geographical settings, data limitations have largely prevented an examination of the role of birth weight in determining long-term outcomes, such as permanent income, income across each point in the life-cycle, take up of various social assistance programs, and adult mortality.[2] For example, the income differences due to birth weight found by Black, Devereux, and Salvanes (2007) are measured around age 30; while this is an important result, it is also useful to examine whether these differences persist, increase, or fade with age. The recent literature on in utero assaults (see Almond (2006), Almond and Mazumder (2011) and Roseboom et al (2001)) has found persistent long run impacts of harm to the fetal environment.[3]

Our paper adds to this important literature by addressing the long term effects of low birth weight. First, we study the impact of birth weight on *permanent* income. Previous studies have been limited to studying the effect of

[1]A sample of these studies in Economics include: Almond, Chay, and Lee (2005), Royer (2009), Black, Devereux, and Salvanes (2007), Behrman and Rosenzweig (2004), Oreopoulos, Stabile, Walld, and Roos (2008), Bharadwaj, Eberhard, and Neilson (2014), Lin and Liu (2009), Cooper and Sandler (1997) and more recently Figlio, Guryan, Karbownik, and Roth (2015). The literature in medicine and public health on the links between birth weight and short and long term outcomes is also equally large. Relevant to our study however, it appears that this literature has not been able to examine birth weight and old age outcomes within a causal framework.

[2]While other birth outcomes such as gestational age are extremely important in this context, we only examine birth weight in this study since a natural experiment providing us with arguably exogenous variation in gestational age is not possible in this setting.

[3]There are many more papers that look at long run effects of in utero assaults. A small sampling of other papers of interest in this area are: Almond, Edlund, and Palme (2009), Mazumder, Almond, Park, Crimmins, and Finch (2010), Lin and Liu (2012) and Van Ewijk (2011).

birth weight on income at relatively young adult ages.[4]

Second, our data allows us to study the impact of birth weight on income at different stages of the lifecycle. In the fetal origins literature, poor early life environment, as reflected by low birth weight, affects long-run health through programming changes (Almond and Currie 2011). These changes makes people more susceptible to heart disease and metabolic syndrome and since these outcomes often occur later in life one can expect a stronger effect of birth weight on labor productivity as people age.[5] With our data, we can study if the impact of birth weight increases by age due to the fetal origins effect.

Third, we study the effect of birth weight on social assistance takeup. By doing so, we provide yet another way of quantifying the long term benefits of programs that are aimed at improving birth outcomes (for example see Bitler and Currie (2005)).[6] Recent work has suggested that social assistance programs not directly intended to improve early childhood health appear to have important spillovers (Hoynes, Schanzenbach, and Almond 2012); hence, understanding whether better infant health itself leads to lower social assistance take up is important.

Fourth, we study the effect of birth weight on adult mortality. While there are many studies in epidemiology that address the link between birth weight and adult mortality, these studies largely compute correlations rather than causal estimates.[7] Particularly in the instance of examining birth weight and mortality, we know from studies like Almond, Chay, and Lee (2005) that ordinary least squares estimates tend to be severely overestimated.

[4]For example, Black, Devereux, and Salvanes (2007) examine the relationship between birth weight and income for people in the age range of 25-35 years, Behrman and Rosenzweig (2004) use income as observed around age 45 and only for women and Oreopoulos, Stabile, Walld, and Roos (2008) examine the take up of social assistance between the ages of 18-22. A recent paper by Bhuller, Mogstad, and Salvanes (2011) suggests that life cycle bias in returns to education is minimized around age 32 in Norway. Hence, the paper by Black, Devereux, and Salvanes (2007) is fairly consistent with life cycle earnings estimates. Specific to our context in Sweden, Böhlmark and Lindquist (2006) show that life cycle bias is an issue when analyzing Swedish earnings data.

[5]It is also possible that there are latent effects on productivity that manifest themselves before the actual health shock occurs.

[6]Although in the Swedish context the term "assistance" refers exclusively to welfare, we use it interchangeably with "benefits" to imply take up of unemployment insurance, welfare etc.

[7]A recent meta-analysis of the relationship between birth weight and adult mortality found that birth weight mattered for mortality, and particularly so for mortality due to cardiovascular reasons (Risnes et al 2011). However, none of the studies discussed in this meta-analysis can claim to get close to causal estimates. However, the upside to these studies is that they analyze large population based samples, whereas studies in Economics (and this paper included) examine this question using twins who constitute a smaller fraction of overall births.

Finally, we supplement our main analysis (which uses cohorts born between 1926-1958) with more recent data (cohorts born 1973-1982, similar to that used by Black et al (2007)). Hence, we are able to study how the impact of birth weight on income and education of young adults has changed across cohorts born almost 50 years apart. In so doing, we are also able to confirm the findings of Black et al. (2007) in a different context.

To deal with the endogeneity of birth weight, the predominant solution in Economics has been to use twins based estimates, taking as random the variation in birth weight within twin pairs (Almond, Chay, and Lee 2005, Royer 2009, Black, Devereux, and Salvanes 2007, Behrman and Rosenzweig 2004). We follow the same strategy in this paper. To answer the main questions we posed earlier, we need outcomes collected over a very long period of time to allow us to observe mortality outcomes and life time income, but also a data set that allows us to use empirical strategies such as twins fixed effects to obtain arguably causal estimates.

Our unique data allows us to do precisely this. Our database contains birth records matched with mortality and yearly income records (including income by source) at the individual level for a vast majority of all twins born between 1926-1958 in Sweden. On average, we have 35 years of labor market data for each twin. The *average* age of these twins by 2010 (the last year for which we have mortality records) is 68; the oldest alive twin pairs are 84 and the youngest alive twin pairs are 54. Since life expectancy in 2010 was around 80 for the entire population (presumably it was less for the cohorts we examine), we expect a moderate level of mortality in our sample; indeed we observe approximately 11% mortality in this sample.

In line with Black, Devereux, and Salvanes (2007) and others who have examined the relationship between birth weight and income in early adulthood, we find birth weight to have a positive and statistically significant effect on income. We find positive effects on both permanent income as well as on income over large parts of the life cycle. After age 50, however, the effect seems to fade out. Interestingly, we find that the impact of birth weight on income has remained rather constant across cohorts born almost 50 years apart.

In terms of social assistance program take up and mortality, our results show that lower birth weight twins are more likely to avail of social insurance programs like unemployment insurance and and sickness pay. The effects are strongest during the ages at which the income effects are the largest, suggesting that the income losses partly works through unemployment and sickness episodes. We find a significant effect of birth weight in mortality among males but not among females. Males with higher birth weight are less likely to die at each period of time.

We next introduce our empirical strategy. In Section 3, we describe the data used for our empirical analysis and Section 4 presents the results. Section 5 concludes.

2 Empirical Strategy

The empirical strategy used in this paper is a straightforward application of the traditional twins fixed effects approach used in prior work. What is different is that in some regressions we use repeated observations over time to obtain a "birth weight effect" at different ages. Our baseline specification is as follows:

$$Y_{ijt} = \beta_t BW_{ij} + \mu_j + \gamma_t X_{ij} + \epsilon_{ijt} \tag{1}$$

Y_{ijt} is log income of individual i, belonging to twin pair j at time t; X_{ij} are time invariant individual specific characteristics like education or sex. The coefficient of interest is β_t which is the birth weight effect at time t. The above equation is estimated at different ages (t's) providing us with the life cycle pattern of the birth weight effect. The key empirical strategy is the inclusion of the twin pair fixed effect (μ_j). Including this fixed effect controls for all twin-pair specific factors that might be relevant for birth weight formation as well as income, such as family background, parental income during pregnancy, nutritional intake of the mother etc. To see this more clearly, we can express Equation 1 in terms of within twin differences (where the other twin is i'):

$$Y_{ijt} - Y_{i'jt} = \beta_t(BW_{ij} - BW_{i'j}) + \gamma_t(X_{ij} - X_{i'j}) + (\epsilon_{ijt} - \epsilon_{i'jt}) \tag{2}$$

The operating assumption for β_t to be interpreted as the birth weight effect at a given point in time t is that $BW_{ij} - BW_{i'j}$ is uncorrelated with $\epsilon_{ijt} - \epsilon_{i'jt}$. In some regressions, we replace the birth weight variable with a dummy variable taking the value of 1 if the twin's birth weight is less than certain treshold values.[8] In our main specifications, we only include birth weight indicators as controls but in subsequent regressions, we add education and income as controls

[8] For this, we use some of the common classifications of birth weight, such as low birth weight (below 2500 grams) and very low birth weight (below 1500 grams).

in some specifications. In our regressions, the standard errors are clustered at the twin pair level.

The question of whether parents react to birth weight or initial endowments is a subject of recent research (for a recent review see see Almond and Mazumder (2012)).[9] In the framework of Bharadwaj, Eberhard, and Neilson (2014) and Conti, Heckman, Yi, and Zhang (2010) (both these papers explicitly use a framework that involves twins), the bias in OLS estimates arise largely from parental investments that compensate or reinforce initial health differences. Under their framework, OLS estimates will tend to overstate initial differences if parents act in reinforcing ways, and understate the role of initial differences if parents act in compensating ways. To the extent that parents in developed countries like Sweden might act in more compensatory ways, we interpret our estimates of the long run effects of birth weight that arise *despite* parents' best efforts to compensate.[10]

3 Data

We use two different sets of data in our empirical analyses. Our most important source of data, BIRTH, originates from the Swedish Twin Registry. BIRTH is part of a project where researchers at the twin registry in Sweden went out to get the birth records of all twins born in Sweden between 1926-1958. These birth records were kept in paper form at local delivery archives around Sweden. Collecting and preserving birth information is/was enforced by law and people at the twin registry set out to digitize them. The BIRTH sample only includes twins that survived up to 1972. In 1972, there was a large twin survey conducted for the twin cohorts born 1926-1958, which formed the basis for the collection of birth data. Unfortunately we do not have access to the universe of twins used to obtain the sample interviewed in 1972. Hence we are unable to construct weights or assess attrition in any systematic manner. The information we have on twins interviewed in 1972 include information on birth weight, sex, year of birth and year of death (if after 1972).[11]

[9]Some papers that tackle this issue are: Aizer and Cunha (2012), Conti, Heckman, Yi, and Zhang (2010), Adhvaryu and Nyshadham (2014), and Bharadwaj, Eberhard, and Neilson (2014).

[10]Another underlying aspect of twins fixed effects estimates is the role of sibling peer effects or how one twin can affect the other twin's choices. We recognize this as something that might be occurring in the background, but we are not aware of papers that explicitly address this issue or how this might drive our results.

[11]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

Our second data source on birth outcomes is the Swedish medical birth register. From this, we get information on birth outcomes for cohorts born 1973 and onwards. The register was founded in 1973 and includes data on the universe of deliveries in Sweden. The information in the register is collected from medical records from prenatal, delivery, and neonatal care units. Evaluations have shown the register to be of high quality; attrition through the period range between 0.5-3 percent (Socialstyrelsen 2014). The register covers information such as weight and height at birth, duration of pregnancy, APGAR score, mode of delivery, infant diagoses, infant mortality, and maternal diagnoses and lifestyle factors. By exploiting information on the mothers ID in the register, we are able to link children born to the same mothers. Since we also have information on the month and year of birth, we can also identify twins.

It should be noted that prior to 1973, there exists no digitized birth register in Sweden. The BIRTH sample is an exception, where researchers went out to collect birth data on twins. Since the BIRTH data only includes data on twins, and since no other digitized birth data exists prior to 1973, we can only run analyses on singletons and siblings for the cohorts born 1973 and onwards.

We use the data from the medical birth register for two purposes. First, with these data, we can check to what extent the effect of birth weight on outcomes such as education and income have changed across cohorts born many years apart. By combining the BIRTH sample with the data from the medical birth register, we are in a unique situation to study changes across cohorts born as far apart as 1926-1982. Second, we can replicate the findings in Black et al. (2007) for Norway. While the Swedish medical birth register does not entirely overlap with its Norweigan counterpart, which starts in 1967, it is close enough to make for an interesting replication.

For both the young and older cohorts, we merge data on individual years of schooling from the education register (utbildningsregistret, UREG) from 1990 or 2007, where years of schooling has been imputed based on obtained degree.[12] For the overwhelming majority of people, we thus have register-based information

births we certainly capture fewer twins than perhaps expected from earlier cohorts. The fraction of twins (compared to total live births) observed in the data stabilizes around 1940. Unfortunately, since we do not observe the original universe of twins, it is difficult the assess the degree to which the earlier cohorts suffer from selective survival. As robustness checks, we test if there have been important changes in the impact of birth weight across early and late cohorts.

[12]If the information differs between 1990 and 2007, we use the information on highest attained schooling. Few individuals change their highest obtained schooling between those years, however.

on schooling. If the person died before 1990, schooling information was taken from the census of 1970.

Income records at the individual level are available starting in 1968 and ending in 2007. The income 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. For people with multiple jobs, only the total income is reported. This same data source also provides information on income by source, such as unemployment benefits, sickness pay and welfare pay. All of our income data is adjusted by 2007 CPI measures to make them comparable.[13]

While we have income data for the BIRTH sample from 1968-2007, there have been some changes in what is included in our main income measure. Different income sources (labor income, income from social safety nets etc) exist in the register because they are taxable. Fortunately, starting in 1974, most income sources, including the various social benefits, were taxable and are included separately in the data. Between 1968-1974 we only have access to income from labor market participation and self employment.[14]

The main income measure we use in this paper is a measure that includes several sources of income. For total income, we use the definition by Statistics Sweden, where it consists of income from work and income from self-employment. Income from work includes, besides wage income, income from pensions, sickness benefits, and other taxable benefits. As previously mentioned, this means that the income measure before and after 1974 are not strictly comparable, as the pre-1974 period excludes benefits. Even though many benefits became taxable in 1974, most are not separately shown until later. Pensions are shown from 1974, unemployment benefits from 1978 and sickness insurance benefits from 1981.[15]

It should be noted that it seems common to include social safety net income in the total income measure in papers using Scandinavian data.[16] It is also

[13]For the cohorts we examine, there does not appear to be much by way of changing variances in income over time.

[14]Recent work by Bjorklund, Jantti, and Lindquist (2009) suggests that this change in definition is not of primary concern for our analysis since benefits constituted less than 5 percent of total earnings in years prior to 1974. In any case, we can reproduce our main analysis restricting the sample to years after 1974 and the basic pattern of results remains unchanged.

[15]Sickness benefits reporting underwent a large change in 1992. Starting in 1992, only sickness benefits where sick leave was taken for more than 2 weeks were reported in the tax registry. Hence, for the analysis of sickness benefits, we restrict our analysis to the 1992-2005 period.

[16]See for instance Black et al. (2007) and Böhlmark and Lindqvist (2006).

important to understand that most benefits are related to having worked and to one's earnings. Sickness benefits and unemployment benefits are related to one's labor income and typically 80-90 percent of one's labor earnings are paid out, although with a cap at some level, which has varied over time. So in some sense, labor income plus benefits paid out will better reflect one's *contracted* labor earnings that year compared to a measure that only includes labor income.

As an alternative income measure, we focus on "pure" labor income as an outcome in some regressions, consisting only of wage income from work or self-employment, i.e. net of taxable benefits. It can be of interest to contrast the findings using total income with those using labor income, since the latter does not include compensation paid by the welfare state. If birth weight has a negative effect on productivity and labor supply, we expect to see a stronger effect of birth weight when using pure labor income as an outcome.[17]

In some of our main analyses, we will use a measure of permanent income as an outcome. This is defined as average earnings across ages 35-45. We base this on the paper by Böhlmark and Lindquist (2006), where they show that average income across these ages is a good proxy for permanent income.

The income data was matched to all twins in the BIRTH sample who were contacted and alive during the 1972-73 survey. Since the data from the medical birth register comes from a separate database, the linkage to income is also slightly different. Here, we have the same type of income data as for the BIRTH sample, but this time from 1990 and onwards. The more limited time period is not of concern, however, since the oldest cohorts in the medical birth register would still only be 17 years of age in 1990. We lose less than 1% of the data due to matching issues across the vital statistics and income registries.

In order to study the effect of birth weight on mortality, we use data from the Swedish causes of death register. The register started in 1961 and covers all deaths among individuals who were permanently residing in Sweden, irrespective of whether the death took place in or outside Sweden. The register includes information on the date and cause of death, as well as information on where the death took place.

For our replication of Black et al. (2007), we follow them as closely as possible when we construct our outcome measures. We define high school completion as having at least 12 years of schooling and require that the person was at least

[17]While the benefits received in general are related to a person's labor earnings, it is not a perfect relationship. The cap in unemployment and sickness benefits means that high-earners lose more than low-earners.

twenty-one when measured, meaning that we include the cohorts born 1973 to 1982, since the last year of measurement of education is 2003. For income, we have data until 2006 for the medical birth register sample. The closest we can get to the analysis in Black et al. (2007), where income at the age range 25-35 is used, is thus to use the age range 25-33, born between 1973-1981. The yearly earnings measure used by Black et al. (2007) includes labor earnings, unemployment insurance payments, sickness benefits, parental leave benefits, and pensions. The measure of yearly earnings we use includes labor earnings, unemployment benefits, sickness benefits, but does not include parental benefits and pensions. Black et al. (200) also restrict their analysis to full-time employees, based on register information. The Swedish registers do not contain such information and we are thus unable to impose the same restriction.

The other outcomes we study in our replication are infant mortality and five-minute APGAR scores. The former is measured as mortality during the first year of life and is taken directly from the birth register. The APGAR score is a summary measure of health at birth, ranging from 0-10, including measures of activity, pulse, grimace, appearance, and respiration (Black et al. 2007). For the analyses of infant mortality and APGAR we use data for the entire birth cohorts 1973-2007.

Table 1 provides the essential statistics on the BIRTH sample and the sample from the medical birth register. For the BIRTH sample, we have 12,888 male twins and 13,266 female twins in complete twin pairs with non-missing data on birth weight and education. Note that the number of observations used in the regressions will differ according to the outcome studied and the ages considered. Since the income register starts in 1968, we cannot study earnings between ages 35-45 for cohorts born earlier than 1933, for instance. For some of our main outcomes, we will show results both for unbalanced and balanced panels. The statistics in the table are shown for the samples used in studying education as an outcome, where we need to put no restrictions on the cohorts included. From the medical birth register, we observe 4,992 and 5,050 male and female twins, respectively, born between 1973-1982.

From Table 1, we learn that the average birth weight of twins has remained rather constant over time. The average is slightly higher for the older cohorts, which reflects the fact that the fraction born with very low birth weight has more than doubled in the younger cohorts. This has occurred due to improvements in the medical technology to treat low-birth infants. We can also observe that twins on average have lower birth weight than singletons. The difference amounts to almost 1 kilogram on average and is consistent with data from other countries (Black et al. 2007). The average discordance in birth weight among twins has decreased somewhat over time but the difference is quite small. The average birth weight and discordance across the cohorts born 1926-1958 and 1973-1982 are also illustrated in Figure 1. Figure 2 illustrates the life expectancy at birth for our cohorts. The average life expectancy increased from 63 to 76 between 1926 and 1982 and the gap between males and females appears to have widened across cohorts.

Since we are using birth weight data from a time when measurement methods were perhaps less refined, it is likely that birth weight and subsequently birth weight differences are measured with more error for the earlier cohorts than for later cohorts. While we have no direct way of assessing the extent to which measurement error plays a role, we can use rounding as a proxy for poor measurement. We do this by examining the fraction of births that are rounded to 50 gram intervals in Figure 3. The first panel shows very clearly that the number of children where birth weight was a multiple of 50 has reduced considerably between 1926-1982. This is likely due to improvements in birth weight measurements over time. In addition, the second panel shows that in the early 1930's a much larger fraction of twins were noted to have the same birth weight. This number falls from around 6-8% in the early 1930's to around 2% in the 1940's and 1% in later cohorts. Because of these differences across cohorts, we will check the robustness of some of our main results when rounding all the birth weight data. In general, we find no difference in results when we conduct these robustness checks.

Table 1 shows that we observe about 14 percent and 10 percent of males and females in the BIRTH sample dying during the sample period. Among those observed to die, the average age of death is about 60.

4 Results

4.1 Replication

We start our empirical analysis by studying the effect of birth weight on short- and medium-run outcomes for the "young" cohorts born 1973 and onwards. We then compare some of these estimates with those obtained for the cohorts born 1926-1958. These analyses serve as a replication of the results in Black et al. (2007) but also allows us to study to what extent the impact of birth weight have changed across cohorts. For the purpose of the replication, we closely follow Black et al. (2007) and include both male and female twins (and both same-sex and mixed-sex pairs) in the regressions and control for gender. The estimates are shown in Table 2, where each coefficient represents an estimate from a separate regression. For the young cohorts, we show results for both singletons and twins and run both OLS and family fixed effects regressions. For the sibling fixed effects regressions, we also show estimates when we restrict the birth weight range to be the same as in the twin sample. In the OLS regressions, we control for year and month of birth, mother's education, mother's year of birth, birth order, and sex of the child. In the sibling fixed effects regressions, we use the same controls except for mother's education and mother's year of birth, which do not vary across siblings. The twin fixed effects regressions only control for birth weight and sex of the child.[18]

We present our analyses in chronological order and first examine short-term outcomes for the young cohorts, including infant mortality and five-minute APGAR scores. We focus first on our sample of main interest, the twins, and as also found in Black et al. (2007) our pooled twins estimates of -180 in Panel A suggest that higher birth weight decreases infant mortality. The estimates for one-year mortality are interpreted as a reduction in the number of deaths per 1000 and the magnitude of the effect means that a 10 percent increase in birth weight would lead to 18 fewer deaths per 1000 births. The twin fixed effect estimate is substantially smaller, at -55, but is statistically significant and not far from the estimate of -41 in Black et al. (2007).

For APGAR scores, our pooled twins and fixed effects estimates of 1.53 and 0.47 are also quite similar to those of Black et al. (1.46 and 0.35). The estimates are significant, again suggesting that higher birth weight leads to improved health outcomes in the short-term.

Moving on to educational outcomes, we find that birth weight is positively related to the probability of high school completition, meaning having at least 12 years of schooling. Like Black et al. (2007), we find that the twin fixed effects estimates are somewhat greater in magnitude compared to the pooled OLS twins estimates. The former estimate implies that a 10 percent increase in birth weight increases the probability of high school completion by 7 percentage

[18]We do not include the birth order of the twins in the regressions, since we lack this information for the older cohorts and since we want to have comparable results. When we include the birth order of twins as a control variable in the regressions on the young cohorts, the results are practically unchanged (results available on request).

points, which is similar to Black et al's estimate of 9 percentage points.

For income at ages 25-33 our twin FE estimates are substantially greater in magnitude, around 9 percent, compared to the pooled twins estimates, which are not significant. Our twin FE estimate is close to the one obtained for ages 25-35 in Black et al. (2007) and they also found the FE estimate to be greater in magnitude than the OLS estimate, although the difference in estimates was smaller in their case. As our OLS estimate is rather imprecise, however, we cannot reject that the estimates are in fact the same.

In Appendix Tables A and B, we show the estimates broken down by gender. Note that the sum of the observations in the male and female samples of siblings and twins do not add upp to the numbers in Table 2, since mixed-sex siblings and twins are now not included. For most of the outcomes, the estimates are similar across genders. The estimate for log earnings increase somewhat to 0.12 and 0.14 for males and females, respectively, whereas the estimate for high school completion stays roughly the same.

Overall, our results are similar to those obtained by Black et al. (2007). This should not come as a surprise since the countries share many characteristics and since we use roughly the same birth cohorts. As in their case, our twin fixed effects estimates are smaller than the corresponding OLS estimates for short-term outcomes, while the estimates are more similar for the medium-run outcomes. The difference in the patterns across short and medium-run results may reflect that compensating parental investments by birth weight affect the medium-run estimates but not the short-run estimates.

4.1.1 Comparison with singletons

Table 2 also shows the corresponding estimates for our sample of singletons, with and without controlling for family fixed effects. The first thing to note is that for our short-term measures, the pooled OLS estimates for the twin sample are substantially greater in magnitude that the corresponding estimates for the singleton sample. Second, for the same outcomes, the sibling FE are greater in magnitude than the twin FE estimates. Both these patterns were obtained in Black et al. (2007) as well.

For high school completion and income, our OLS estimates on the siblings and twins sample are much more similar. Moreover, the twin FE estimates are in this case greater in magnitude than the corresponding sibling FE estimates. Again, the same pattern was found in Black et al. (2007).

Summing up, the siblings FE estimates are greater for short-term outcomes whereas the twin FE estimates are greater for our medium- and long-outcomes.

4.1.2 Results for the BIRTH cohorts: education and income

We next move back in time and analyze the effect of birth weight on various outcomes for our sample of main interest; the cohorts born 1926-1958. We focus first on education and income as outcomes, as these are the outcomes we are able to measure for both the old and young cohorts.

Panel B of Table 2 shows the effect of birth weight on educational outcomes and Panel B ofTables A and B in the Appendix show the same results by gender.[19] For the sake of replicating Black et al. (2007), we show results for mixed-sex couples in Table 2, but the results are similar when including only same-sex pairs (results available on request).

The estimates suggest that higher birth weight has a positive effect on high school completion, where a ten percent increase increases the probability of high school completion by 10 percentage points. This estimate is somewhat larger than that of the younger cohorts, where the corresponding number was 7 percentage points. The estimate is also quite similar across genders.

Table 2 also includes estimates for years of schooling. Although this outcome was not included when we ran regressions on the young cohorts, it is of interest to include for the old cohorts, as we can be quite sure that they have completed their schooling at the ages when we measure it, which was not the case for the young cohorts. In addition, in our analyses of income, welfare-takeup, and mortality, we will run specifications including years of schooling as a control variable. Our estimates for the pooled sample suggest that a 10-percent increase in birth weight leads to 0.67 additional years of schooling and the estimate is similar for males and females.

4.1.3 Income

In Panel B of Table 2, we also report results for income between ages 25-33.[20] The estimated twin FE effect of birth weight on income during these

[19] In the OLS regressions for the old cohorts, we control for the sex of the child and birth year. The reason is that we lack information on parents' education, month of birth, birth order, and mothers' year of birth.

[20] Since the income register starts in 1968, the analyses on income use different cohorts than the ones we use for educational outcomes. For the analyis of income between ages 25-33, the analyses was restricted to cohorts born 1943 and onwards and for permanent income, analyzed in section 4.2, the corresponding restriction was those born 1933 and onwards. We have checked if the estimates for education are the same when using the same cohort restrictions. We found this to be the case; in the analysis when we pool same-sex male and female twin pairs, the effect of birth on years of schooling is 0.60 for the cohort born 1943 and onwards

ages, 0.09, is remarkably similar to the estimate we obtained for the younger cohorts. It thus appears that the impact of birth weight on earnings as a young adult is rather constant across cohorts born almost 50 years apart. As we show in Appendix Tables A and B, the estimate is similar across genders, although less precisely measured for females. We again find the OLS estimate to be insignificant and smaller in magnitude compared to the FE estimate. There is no statistically significant difference between the estimates, however

4.2 Permanent income

We next turn to our measure of permanent income, i.e average income between ages 35-45. As shown in the second column of Table 3, the twin FE estimate for this outcome is more or less the same as the ones we obtained in our earlier analysis on the young and old cohorts, the estimate suggesting that a 10 percent increase in birth weight leads to a 1 percent increase in permanent income. The estimate is quite similar between genders and is robust to instead using labor income as an outcome, as shown in Panel B of Table 3. Controlling for years of schooling only slightly reduces the magnitude of the estimate. The corresponding OLS estimates are about half in magnitude but the differences are again not statistically significant.

While using the log of birth weight in our income regressions allows for some non-linearity in the effect, we can also employ more flexible specifications. Figures 4 and 5 plot estimates from specifications where we have constructed 100 grams bins of birth weight. We then run OLS and FE regressions including these bins as dummy variables, the reference category being twins with a birth weight below 1500 grams. For males, the graph shows an increase in permanent income by birth weight up until roughly 2500 grams after which the curve flattens out. The corresponding graph for females shows a similar pattern. Note that the estimates at the end-points of the graph are quite noisy, which reflects that fewer observations are found at those levels of birth weight.

Another way to explore potential non-linearities is to replace our birth weight measure with various birth weight tresholds, as shown in Table C in the Appendix. Here, the coefficients displayed are from separate regressions, using various tresholds. Some of these point estimates are quite large where, for instance, being below 2500 grams results in 2.5 percent reduction in perment income. The estimates for weighting less than 1500 grams are sometimes large

and 0.59 for cohorts born 1933 and onwards.

but suffer from low precision. The estimates are quite similar for permanent income and permanent labor income.

We can also study if the effect of birth weight on permanent income changes across cohorts in the BIRTH sample. For this, we divided the data into three groups, twins born 1933-1939, 1940-1949, and 1950-1958 and interacted the birth weight variable with the cohort indicators. To gain sufficient power, and since results were fairly similar across genders, we pool males and females. The results, shown in Table D in the Appendix, show that there are no significant differences in the effect across cohorts. The point estimate for the oldest cohort suggests a smaller effect that could perhaps reflect more measurement error in the data for the oldest cohorts. To investigate this, the third and fourth columns show results where we rounded the birth weight measure to the nearest 50 gram bin.[21] This does not affect the estimates in any important way, showing that the more prominent rounding of the birth weight measure in the older cohorts is not a problem.

4.3 Income across the lifecycle

We next ask whether the effect of birth weight on income varies across the lifecycle. This would for instance be suggested by the Barker hypothesis, where poor early life environment leads to programming changes that makes people more susceptible to heart disease and metabolic syndrome as people age.

Figures 6 and 7 show the estimates of the relationship between log birth weight and log income at each point in the life cycle using both fixed effects and OLS estimation methodologies. The coefficients can be interpreted as the effect on 4 year moving averages since at each age, we use 4 years of data to smooth the graphs. To be precise, the coefficient shown for age 50 uses data from ages 50-53, the coefficient for age 51 uses data from ages 51-54 and so on. These regressions control for calendar year of observation and nothing else. In addition, the figures show the difference in OLS and FE estimates and the associated confidence intervals around this difference.

For males, the figure suggests that higher birth weight seems to lead to higher income early in the life cycle, but that the effect declines at older ages. In panel A of Table 4, we show regression estimates for an unbalanced sample of male twins where the outcome is 5-year average earnings.[22] The coefficient for age

[21] Recall that part of the reason for more measurement error in the data for the old cohorts was that such rounding was more prominent.

[22] The reason that the number of observations change across specifications is that the income

30, for instance, shows the impact of birth weight on average earnings between 28-32.[23] At these ages, a 10 percent increase in birth weight (approximately 250 grams) leads to a 1.25 percent increase in total income among males. These estimates are quite similar to our earlier estimates on permament income and to the ones in Black, Devereux, and Salvanes (2007). This birth weight effect seems to last until around age 40-50 after which it appears to fade out. For parsimony, these regressions do not control for schooling but the results are similar when doing so.

In order to check if the change in results across the lifecycle reflects changes in the sample compostion, we can restrict the analysis to a balanced sample, as in Table E. While the point estimates at higher ages are reduced in magnitude, the precision is also lower due to the smaller sample size and we cannot rule out that there are large effects also at high ages. The results do not provide any evidence, however, that the impact of birth weight on income increase by age due to the fetal programming effect.

The fact that the birth weight effect appears to fade out after age 50 is perhaps unexpected if we believe that initial labor market differences tend to persist. Many studies have found that labor market conditions at the time of entry are incredibly important for long term outcomes (see for example Kahn (2010) and Beaudry and DiNardo (1991)). The declining result is not a feature of the sample itself changing since the results from the balanced panel for ages 30-55 in Table E showed a remarkable decline in the birth weight effect by age 50 and nearly disappearing at older ages. Examining just the results for income earned from labor market activity, shown in Panels B and D of Table 4 results in similar results where, if anything, the point estimates at ages 50 an above are smaller in magnitude.

For females, the patterns across the lifecycle are quite different. Figure 7 suggests that birth weight has a positive effect early on and late during the lifecycle. This is also suggested in Table 4, where the effects are largest at ages 30-35 and 50-60. These results hold up less well when running the same

register starts in 1968 and ends in 2007 and that our cohorts are born 1926-1958. For income at age 30, for instance, we can only include cohorts born 1938 and onwards. The number of observations increase until age 45 after which it declines again. The reason is that at age 50 and onwards fewer and fewer of the younger cohorts can be included. At age 55, for instance, the youngest cohorts included are born in 1952.

[23] In these regressions, we require that both twins are present for the same number of years when calculating the average. If income is available only for a subset of the years of the 5-year intervals, we base the calculation on those years. The number of observations in the table is the number of unique individuals.

regression on the balanced panel. The estimates at ages 30 and 35 increase in magnitude but at older ages the estimates become smaller and imprecise.

Tables F and G explore non-linearity in the birth weight-income relationship across the lifecycle, where the different rows show estimates from separate regressions with different tresholds indicated and where Table F show results for total income and Table G for labor income. While most coefficients in these tables are not significant, the general pattern suggests some non-linearity in the birth weight - income relationship. The coefficients on very low birth weight (VLBW) tend to be larger than the effects for higher thresholds, although the coefficients are rarely statistically significant.

4.4 Social Benefits

Having established that birth weight affects both permanent income and income at different stages of the lifecycle, we next set out to understand the effect of birth weight on the uptake of various social benefits. Table 5 show regressions where the dependent variable is the fraction of time spent earning income from social assistance sources (we examine these in 10 year intervals). The results suggest that a 10 percent decrease in birth weight is associated with a 0.0065 increase in the fraction of time between the ages of 35-45 that males spent on sickness pay. Given a base of 0.22, this suggests an increase of nearly 3 percent. Likewise, between the ages of 45-55, a 10 percent decrease in birth weight can lead to an increase in unemployment insurance take up of 0.003 - which translates to a 3.6 percent increase in the fraction of time spent on UI. These estimates are almost unchanged when we control for education. Note that we do not show specifications controlling for income here, as the level of the benefits is mechanically related to one's earnings.

Among females, few of the point estimates show up as significant and most are small in magnitude. The exception is time spent on sickness benefits during ages 55-65, where higher birth weight leads to significant and rather strong decline in the uptake of sickness benefits.

We can relate our findings on social benefits to our earlier findings on income. Among males, birth weight is significantly related to the use of sickness benefits during the same ages at which we obtained significant effects on permanent income, i.e. at ages 35-45. This suggests that part of the negative effect on permanent income works through increased use of sickness benefits. This, in turn, suggests that some of the effect of birth weight on income works through health-related problems. The fading out of the effect on social benefits by age is also consistent with the fading out of the income effect.[24]

Among females, our findings on social benefits do not explain the effect on permanent income to the same degree. Looking at the lifecycle patterns, however, we found strong income effects late in the lifecycle, which is also where we found that birth weight affects sickness benefits, i.e. at ages 55-65. It thus appears that part of the late-life income effect works through poor health, as expressed through being on sick-leave.

4.5 Mortality

Given the negative effects of birth weight on economic outcomes that we have found so far, it is natural to ask if this these effects also translate into reduced longevity. In Figure 8 we illustrate such potential patterns by plotting the mortality hazard rates of light versus heavy twins, while allowing for the censoring in the data. These Kaplan-Meier estimates show greater hazard rates for the lighter twin, suggesting a possible mortality gradient in birth weight. The pattern is most pronouced for males, where the difference in hazard rates start already in the 40s.

In order to obtain more precise estimates of the mortality gradient, we run Cox proportional hazard models, where we account for twin fixed effects by allowing for twin-pair specific baseline hazards. These models account for the censoring in mortality and exploit differences in the timing of death within twin pairs. In Table 6, we first show the Cox estimates with only birth weight as control and we then subsequently add education and income. These regressions are based on the entire cohorts born 1926-1958.

For males, we obtain a significant hazard ratio, where higher birth weight leads to lower mortality. The hazard rate is 0.51, suggesting that a 10 percent increase in birth weight halves the instantaneous hazard of dying. The estimate does not change much when we add education and average income at ages 38-42 to the regression, suggesting that income and education are not important mechanisms through which the effect of birth weight on mortality arises.[25,26]

[24]In order to shed more light on the reasons for the fading out of the income effect by age, we have checked if individuals with higher birth weight retire earlier. This would be the case if such individuals hold more attractive jobs that allow for earlier retirement. We found the opposite though; birth weight was negatively and significantly related to the probability of being retired at ages 50, 55, 60, and 65. These results are available on request.

[25]We chose the age range 38-42 for income in order to maximize the sample size. Including incomes at other ages, or permanent income, leads to smaller sample size but similar results.

[26]We also ran regressions including controls for welfare use at the different age ranges used in Table 5. This did not change the birth weight coefficient in any meaningful way. Welfare use was in itself linked to increased mortality. We also investigated if the birth weight effect

In contrast to the results for males, we find no significant effects of birth weight on mortality for females.

In order to shed more light on what variation across the birth weight distribution that is generating the significant results for males, Table 6 also shows specifications using birth weight cutoffs. Here, the coefficients shown are obtained from separate regressions. From these, we see that the hazard ratio for males is largest for the specification using the 1500 gram cutoff, although the estimate is only significant at the 10 percent level. At the bottom row, we also show results comparing the lighter twin to his heavier counterpart. The results mirror those in Figure 8 and show that the hazard of dying significantly increases by between 15 to 18 percent for the lighter twin, depending on the covariates included. Again, we obtain no significant estimates for females.[27]

5 Conclusion

This paper adds to the emerging literature on the long lasting impacts of birth outcomes by examining the relationship between birth weight and long-run outcomes, including permanent income, income across the lifecycle, education, social benefits take-up, and adult mortality. Moreover, we add to the literature by examining how the impact of birth weight has changed across cohorts born almost 50 years apart and by replicating findings from another Scandinavian country. We find that birth weight is a predictor of permanent income and that most of the effects on income appear concentrated in early adulthood. Our results indicate that lower birth weight children are more likely to avail of social insurance programs such as unemployment and sickness insurance. We also find important effects on longevity among males but not among females. The effects on income and education at young adulthood are surprisingly constant across cohorts and similar to the effects found in a previous study in Norway.

Our results are important in highlighting how inequalities at birth play out over the life cycle and support the idea that part of the health-income gradient

on mortality was different for people that used welfare. The interaction between birth weight and welfare use at different age ranges was always insignificant however. These results are available on request.

[27] We have also ran linear probability regressions on the effect of birth weight on the probability of dying first within a twin pair (on the subsample of twin pairs where we observe at least one twin dying). For males, a ten percent increase in birth weight lowered the probability of dying first by 44 percentage points. The estimate was significant at the 5 percent level and did not change much when adding education and income to the regressions. Among females, the corresponding estimate was 28 percentage points but it did not reach statistical significance.

in adulthood can be explained by health in childhood (Case, Lubotsky, and Paxson 2001). The extent to which policies or parental investments can remedy initial deficiencies is an important question for researchers to tackle. A first step towards this goal is to understand whether initial differences have long lasting impacts and then to examine intermediary channels that might drive the long run results. We recognize that differences in early childhood can lead to differences in the medium/long term for various reasons and pinning down which one of these intermediary channels best explains the long run results is beyond the scope of this paper. We therefore view our results as arising from a combination of health deficiencies at birth and subsequent behavioral responses that might be aimed at mitigating or exacerbating these initial differences.

We acknowledge that twins form a small fraction of the overall population and hence are sympathetic to concerns about the generalizability of these findings. However, because twins are smaller at birth than singletons, if there are non-linearities in the *true* effect of birth weight on various outcomes, then we can take some comfort in that our effects are over the relevant range of birth weight. Indeed, as Bharadwaj, Eberhard and Neilson (2014) show, the effects of birth weight on test scores at least, are quite similar across twins and sibling fixed effects for siblings close together in age. In related work, Oreopoulos, Stabile, Walld, and Roos (2008) examine the effects of birth weight using both sibling and twin fixed effects models; while for social assistance take up it appears that twins and siblings estimates are quite different, the effects of birth weight appear quite similar for schooling related outcomes. Since we lack data on siblings for our main sample, we are unable to make such comparisons with our own data. We do not wish to claim that variation in twin birth weight is generalizable to the larger population, but rather that there is at least *some* indication that this variation might be useful for making broader inferences. Ultimately, variation in birth weight using twins fixed effects is the closest we have to randomly assigned birth weight; however, the extent to which this variation in relevant for policy is still an open question.

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Table 1: Descriptive statistics. Means and SDs.

	Old cohorts (1926-1958)		Young cohorts (1973-1982)		Young cohorts (1973-1982)	
	Twins		Twins		Siblings	
	Males	Females	Males	Females	Males	Females
Birth weight	2704.5	2582.1	2641.0	2548.7	3578.1	3476.7
	(510.7)	(500.8)	(535.7)	(514.2)	(532.4)	(513.6)
Less than 1500 grams	0.00667	0.0127	0.0252	0.0313	0.00213	0.00232
	(0.0814)	(0.112)	(0.157)	(0.174)	(0.0461)	(0.0481)
Less than 2500 grams	0.337	0.422	0.375	0.442	0.0251	0.0295
	(0.473)	(0.494)	(0.484)	(0.497)	(0.156)	(0.169)
Birth weight diff.	337.5	325.8	313.4	307.4	412.3	394.9
	(278.3)	(273.7)	(274.7)	(268.9)	(350.1)	(341.7)
High school completion	0.388	0.328	0.794	0.845	0.784	0.827
	(0.487)	(0.469)	(0.404)	(0.362)	(0.411)	(0.378)
Years of schooling	10.85	10.81				
	(3.064)	(2.938)				
Permanent income	254442.6	154084.5				
	(125801.8)	(69114.5)				
Welfare use 45-55*	0.0602	0.0884				
	(0.191)	(0.235)				
Unemploment 45-55*	0.0585	0.0589				
	(0.166)	(0.165)				
Sickness benefits 45-55*	0.161	0.230				
	(0.230)	(0.261)				
Fraction observed deaths	0.142	0.0991				
	(0.349)	(0.299)				
Age at death**	59.84	60.78				
	(12.92)	(11.93)				
Observations	12888	13266	4992	5050	159873	143362
# twin pairs (families)	6444	6633	2496	2525	70702	63161

Notes: The table shows descriptive statistics for (1) the "old" cohorts born 1926-1958 and (2) the "young" cohorts born 1973 and onwards. The statistics are for the twin sample in the "old" cohorts and for the twin and sibling samples in the "young" cohorts. In the old sample, the statistics for birth weight, education outcomes, and mortality are based on the entire cohorts born 1926-1958. For permanent income, welfare use, and unemployment benefits, and sickness benefits, the sample size and the cohorts included vary, depending on the data available for the different outcomes, see data section for details. The sample sizes are as follows for males and females, respectively: Permanent income: 5,607 and 5,687. Unemployment benefits: 12,250 and 12,802. Welfare use: 11,842 and 12,366. Sickness benefits: 10,060 and 10404. * Measured as the the fraction of time during ages 45-55 using the benefit. ** The statistics for age at death are for those who are observed to die during the sample period. Permanent income is measured as average earnings during ages 35-45. High school completion is measured as having 12 or more years of schooling. For the siblings sample, the average difference in birth weight is based on the difference in birth weight between the two observed youngest siblings of the same gender in a family. The statistics for the young cohorts are based on individuals born between 1973-1982.

Table 2: Effects of birth weight on selected outcomes for cohorts born 1973 and onwards and cohorts born 1926-1958.

	Singletons			Twins	
Dependent variable	OLS	Sibling FE	Sibling FE restricted	OLS	Twins FE
	Panel A: Cohorts born 1973-				
One-year mortality	-83.066	-132.561	-122.316	-179.617	-54.961
	(1.056)***	(1.662)***	(1.693)***	(6.156)***	(7.728)***
N	2,379,386		2,237,795	64,976	
Five minute APGAR score	0.665	1.033	1.033	1.523	0.470
	(0.007)***	(0.010)***	(0.010)***	(0.037)***	(0.059)***
N	2,153,559		2,153,544	59,448	
High school completion	0.048	0.033	0.033	0.042	0.070
	(0.004)***	(0.006)***	(0.006)***	(0.017)**	(0.029)**
	509,427		484,038	13,972	
ln(earnings) Ages 25-33	0.038	0.041	0.042	0.025	0.090
	(0.004)***	(0.007)***	(0.007)***	(0.017)	(0.035)**
N	287,970		273,409	8,352	
	Panel B: Cohorts born 1926-1958				
High school	-	-	-	0.0551***	0.101***
completion	-	-	-	(0.0136)	(0.0225)
N	-	-	-	35,046	
ln(earnings)	-	-	-	-0.00894	0.0916**
Ages 25-33	-	-	-	(0.0224)	(0.0416)
N	-	-	-	16,400	
Years of	-	-	-	0.322***	0.671***
schooling	-	-	-	(0.0804)	(0.126)
N	-	-	-	35,046	

Notes: The table shows regressions on the relationship between birth weight and selected outcomes for the cohorts born 1973 and onwards and for cohorts born 1926-1958. The number of cohorts included depend on the outcome studied, see data section for detailed information. In the OLS regresssions for cohorts born 1973 and onwards, we control for year and month of birth, mother's education, birth order, mother's year of birth, and sex of the child. Sibling fixed effect regressions control for all of the above except for mother's year of birth and mother's education. Twin fixed effect regressions control for sex. In the OLS regressions for the cohorts born 1926-1958, we control sex of the child and birth cohort. The third column shows sibling fixed effect results when restricting the birth weight range to that in the corresponding twin sample. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Birth weight and permanent income. Cohorts born 1933-1958.

	Pooled			Males		Females	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	FE	FE	FE	FE	FE	FE
	Panel A: Permanent income (age 35-45)						
Ln Birth weight	0.0509***	0.104***	0.0736**	0.102***	0.0695*	0.106*	0.0790
	(0.0188)	(0.0344)	(0.0339)	(0.0391)	(0.0381)	(0.0560)	(0.0555)
Observations	22,588	22,588	22,588	11,214	11,214	11,374	11,374
# twin pairs	11,294	11,294	11,294	5,607	5,607	5,687	5,687
	Panel B: Permanent labor income (age 35-45)						
Ln Birth weight	0.0646***	0.125***	0.0893**	0.100**	0.0621	0.148**	0.116*
	(0.0216)	(0.0400)	(0.0394)	(0.0487)	(0.0477)	(0.0629)	(0.0622)
Observations	22,376	22,200	22,200	11,060	11,060	11,140	11,140
# twin pairs	11,100	11,100	11,100	5,530	5,530	5,570	5,570
Control for schooling	No	No	Yes	No	Yes	No	Yes

Notes: This table shows regressions on birth weight and permanent income for the cohorts born 1933-1958. Columns (1) to (3) show results for the pooled sample of males and females. Columns (4) and (5) show results for males and columns (6) and (7) for females. The coefficients in columns (2) to (7) are from twin fixed effects models, whereas the coefficient in column (1) is from an OLS regression. Panel A show reults for permanent total income and Panel B show results for permanent labor income. Income is defined as labor income from employment and self-employment plus taxable benefits, whereas labor income excludes the latter. Permanent income is calculated as the average income between the ages 35 and 45. Years of schooling is included in columns (3), (5), and (7) and is controlled for using discrete categories. The regressions on permanent income include cohorts born 1933-1958. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Birth weight across the lifecycle.

	Age						
	30	35	40	45	50	55	60
	Panel A: Income, males						
Log birth weight	0.125***	0.123***	0.107**	0.101**	0.0996*	0.0605	0.0696
	(0.0451)	(0.0448)	(0.0447)	(0.0489)	(0.0524)	(0.0580)	(0.0679)
Observations	10,636	11,632	12,554	12,368	12,044	9,886	7,292
Number of twin pairs	5,318	5,816	6,277	6,184	6,022	4,943	3,646
	Panel B: Labor income, males						
Log birth weight	0.137***	0.153***	0.0597	0.173***	0.155*	-0.0207	0.160
	(0.0473)	(0.0498)	(0.0560)	(0.0667)	(0.0803)	(0.111)	(0.217)
Observations	10,518	11,400	12,126	11,712	11,058	8,662	5,840
Number of twin pairs	5,259	5,700	6,063	5,856	5,529	4,331	2,920
	Panel C: Income, females						
Log birth weight	0.211*	0.180*	0.0761	0.00323	0.131**	0.163**	0.152**
	(0.112)	(0.0971)	(0.0796)	(0.0641)	(0.0593)	(0.0637)	(0.0752)
Observations	9,656	11,064	12,228	12,484	12,434	10,428	8,126
Number of twin pairs	4,828	5,532	6,114	6,242	6,217	5,214	4,063
	Panel D: Labor income, females						
Log birth weight	0.173	0.215**	0.0732	0.0460	0.153	0.251**	0.270
	(0.114)	(0.100)	(0.0875)	(0.0764)	(0.103)	(0.126)	(0.216)
Observations	9,426	10,618	11,526	11,504	10,984	8,666	5,964
Number of twin pairs	4,713	5,309	5,763	5,752	5,492	4,333	2,982

Notes: This table shows regressions on birth weight and income across the lifecycle for the cohorts born 1926-1958. All coefficients are from twin fixed effects models. Panels A and B show regressions on average 5-year income and average 5-year labor income among males. Panels C and D shows the corresponding regressions for females. Income is defined as labor income from employment and self-employment plus taxable benefits, whereas labor income excludes the latter. The ages in the table refer to the midpoints of the 5-year averages. No controls for schooling. The cohorts included in the regressions vary, see the text for details. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Birth weight, social insurance, and welfare

	Age								
	35-45			45-55			55-65		
	UI	SI	Welfare	UI	SI	Welfare	UI	SI	Welfare
				Panel A: Male twins					
A. Log birth weight	-0.0120	-0.0652*	-0.0455	-0.0323**	-0.0244	-0.00508	-0.0341	0.00659	-0.0144
	(0.0112)	(0.0347)	(0.0299)	(0.0159)	(0.0241)	(0.0188)	(0.0234)	(0.0282)	(0.0201)
B. Log birth weight	-0.0130	-0.0626*	-0.0386	-0.0318**	-0.0215	-0.00132	-0.0330	0.00629	-0.0119
(control for schooling)	(0.0111)	(0.0347)	(0.0298)	(0.0159)	(0.0241)	(0.0188)	(0.0233)	(0.0282)	(0.0201)
Observations	11,200	6,390	9,904	12,250	10,060	11,842	8,890	8,750	8,890
Number of twin pairs	5,600	3,195	4,952	6,125	5,030	5,921	4,445	4,375	4,445
				Panel B: Female twins					
A. Log birth weight	0.0117	-0.0120	-0.0111	0.00211	-0.0272	0.0117	0.00195	-0.0678**	-0.0151
	(0.0115)	(0.0375)	(0.0334)	(0.0157)	(0.0262)	(0.0221)	(0.0227)	(0.0285)	(0.0235)
B. Log birth weight	0.00971	-0.0150	-0.00710	0.000129	-0.0291	0.0145	0.00159	-0.0694**	-0.0108
(control for schooling)	(0.0115)	(0.0375)	(0.0334)	(0.0157)	(0.0262)	(0.0221)	(0.0227)	(0.0285)	(0.0235)
Observations	11,464	6,384	10,124	12,802	10,404	12,366	9,720	9,620	9,718
Number of twin pairs	5,732	3,192	5,062	6,401	5,202	6,183	4,860	4,810	4,859

Notes: This table shows regressions on birth weight and use of social benefits across the lifecycle for the cohorts born 1926-1958. All coefficients are from twin fixed effects models. The regressions show the effect of birth weight on the fraction of time spend using different social insurance systems in 10-year intervals. Years of schooling is controlled for using discrete categories. The cohorts included in the regressions vary, see the text for details.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Birth weight and adult mortality. Cox proportional hazard models.

		Males			*Females*	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Log birth weight	0.506**	0.513**	0.470**	0.843	0.863	0.896
	(0.148)	(0.155)	(0.150)	(0.282)	(0.296)	(0.340)
B. Less than 1500 grams	2.750*	2.807*	2.571	0.818	1.027	0.945
	(1.606)	(1.748)	(1.640)	(0.368)	(0.481)	(0.466)
C. Less than 2000 grams	1.114	1.194	1.248	0.985	0.960	1.010
	(0.183)	(0.203)	(0.225)	(0.170)	(0.170)	(0.192)
D. Less than 2500 grams	1.124	1.122	1.139	1.064	1.046	1.025
	(0.116)	(0.119)	(0.128)	(0.118)	(0.120)	(0.132)
E. Lighter twin	1.152***	1.150***	1.176***	0.968	0.959	0.946
	(0.0585)	(0.0602)	(0.0650)	(0.0566)	(0.0578)	(0.0634)
Observations	13,022	13,022	12,554	13,392	13,392	12,228
Number of twin pairs	6511	6511	6277	6696	6696	6114
Control for schooling	NO	YES	YES	NO	YES	YES
Control for income	NO	NO	YES	NO	NO	YES

Notes: This table shows regressions on birth weight and mortality for the cohorts born 1926-1958. All coefficients are from Cox proportional hazard models with twin fixed effects (twin-pair specific baseline hazards). The coefficients represent hazard ratios. The coefficients presented in the different rows come from separate regressions. In the regressions controlling for income, we use average income during ages 38-42, see text for details.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A: Effects of birth weight on selected outcomes for cohorts born 1973 and onwards and cohorts born 1926-1958. Males.

	Singletons			Twins	
Dependent variable	OLS	Sibling FE	Sibling FE restricted	OLS	Twins FE
	Panel A: Cohorts born 1973-				
One-year mortality	-106.668	-160.822	-160.836	-214.389	-68.971
	(1.777)***	(2.599)***	(2.600)***	(10.360)***	(15.569)***
N	965,444		965,438	22,456	
Five minute	0.757	1.133	1.133	1.666	0.466
APGAR score	(0.012)***	(0.017)***	(0.017)***	(0.062)***	(0.107)***
N	861,546		861,541	20,522	
High school	0.055	0.025	0.026	0.061	0.067
completion	(0.007)***	(0.011)**	(0.011)**	(0.030)**	(0.050)
N	159,873		159,872	4,992	
ln(earnings)	0.060	0.047	0.047	0.039	0.121
Ages 25-33	(0.007)***	(0.013)***	(0.014)***	(0.028)	(0.054)**
N	86,365		80,739	3,180	
	Panel B: Cohorts born 1926-1958				
High school	-	-	-	0.0492**	0.111***
completion	-	-	-	(0.0232)	(0.0375)
N	-	-	-	12,888	
ln(earnings)	-	-	-	0.0165	0.0827**
Ages 25-33	-	-	-	(0.0208)	(0.0356)
N	-	-	-	8,232	
Years of	-	-	-	0.114	0.554***
schooling	-	-	-	(0.139)	(0.210)
N	-	-	-	12,888	

Notes: The table shows regressions on the relationship between birth weight and selected outcomes for the cohorts born 1973 and onwards and cohorts born 1926-1958. The number of cohorts included depend on the outcome studied, see data section for detailed information. In the OLS regresssions, we control for year and month of birth, mother's education, birth order, mother's year of birth, and sex of the child. Sibling fixed effect regressions control for all of the above except for mother's year of birth and mother's education. Twin fixed effect regressions control for sex. The third column shows sibling fixed effect results when restricting the birth weight range to that in the corresponding twin sample. In the OLS regressions for the cohorts born 1926-1958, we control sex of the child and birth cohort. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B: Effects of birth weight on selected outcomes for cohorts born 1973 and onwards and cohorts born 1926-1958. Females.

	Singletons			Twins	
Dependent variable	OLS	Sibling FE	Sibling FE restricted	OLS	Twins FE
	Panel A: Cohorts born 1973-				
One-year mortality	-100.228	-156.437	-156.462	-172.516	-29.857
	(1.851)***	(2.760)***	(2.760)***	(10.721)***	(12.733)**
N	877,941		877,935	21,754	
Five minute APGAR score	0.704	1.132	1.132	1.499	0.547
	(0.012)***	(0.017)***	(0.017)***	(0.067)***	(0.099)***
N	780,130		780,125	19,750	
High school completion	0.058	0.029	0.029	0.016	0.075
	(0.007)***	(0.010)***	(0.010)***	(0.025)	(0.040)*
N	143,362		143,360	5,050	
ln(earnings) Ages 25-33	0.027	0.037	0.037	0.001	0.141
	(0.008)***	(0.014)***	(0.015)**	(0.028)	(0.060)**
N	66,937		64,618	2,864	
	Panel B: Cohorts born 1926-1958				
High school	-	-	-	0.0543***	0.0867***
completion	-	-	-	(0.0204)	(0.0325)
N	-	-	-	13,266	
ln(earnings)	-	-	-	-0.0287	0.100
Ages 25-33	-	-	-	(0.0378)	(0.0753)
N	-	-	-	8,168	
Years of	-	-	-	0.374***	0.604***
schooling	-	-	-	(0.120)	(0.184)
N	-	-	-	13,266	

Notes: The table shows regressions on the relationship between birth weight and selected outcomes for the cohorts born 1973 and onwards and cohorts born 1926-1958. The number of cohorts included depend on the outcome studied, see data section for detailed information. In the OLS regresssions, we control for year and month of birth, mother's education, birth order, mother's year of birth, and sex of the child. Sibling fixed effect regressions control for all of the above except for mother's year of birth and mother's education. Twin fixed effect regressions control for sex. The third column shows sibling fixed effect results when restricting the birth weight range to that in the corresponding twin sample. In the OLS regressions for the cohorts born 1926-1958, we control sex of the child and birth cohort. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C: Birth weight and permanent income (ages 35-45). Non-linear specifications.

	Pooled		Males		Females	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Permanent income (age 35-45)					
A. Less than 1500 grams	-0.0334	-0.0229	-0.0842	-0.0603	-0.00186	-0.00119
	(0.0466)	(0.0459)	(0.0595)	(0.0579)	(0.0695)	(0.0688)
B. Less than 2000 grams	-0.0319*	-0.0283	-0.0414*	-0.0439**	-0.0257	-0.0182
	(0.0179)	(0.0176)	(0.0225)	(0.0219)	(0.0270)	(0.0267)
C. Less than 2500 grams	-0.0252**	-0.0206*	-0.0174	-0.0133	-0.0324*	-0.0285
	(0.0117)	(0.0115)	(0.0133)	(0.0130)	(0.0191)	(0.0189)
Observations	22,588	22,588	11,214	11,214	11,374	11,374
# twin pairs	11,294	11,294	5,607	5,607	5,687	5,687
		Panel B: Permanent labor income (age 35-45)				
A. Less than 1500 grams	-0.0819	-0.0688	-0.0853	-0.0567	-0.0798	-0.0784
	(0.0540)	(0.0531)	(0.0735)	(0.0719)	(0.0780)	(0.0771)
B. Less than 2000 grams	-0.0375*	-0.0328	-0.0209	-0.0229	-0.0484	-0.0386
	(0.0208)	(0.0205)	(0.0281)	(0.0275)	(0.0303)	(0.0300)
C. Less than 2500 grams	-0.0339**	-0.0286**	-0.0193	-0.0142	-0.0476**	-0.0430**
	(0.0136)	(0.0134)	(0.0166)	(0.0162)	(0.0214)	(0.0211)
Observations	22,200	22,200	11,060	11,060	11,140	11,140
# twin pairs	11,100	11,100	5,530	5,530	5,570	5,570
Control for schooling	No	Yes	No	Yes	No	Yes

Notes: This table shows regressions on the effect of having a birth weight below different tresholds on permanent income for cohorts born 1933-1958. Columns (1) to (2) show results for the pooled sample of males and females. Columns (3) and (4) show results for males and columns (5) and (6) for females. All coefficients are from twin FE models. Panel A show reults for permanent total income and Panel B show results for permanent labor income. Income is defined as labor income from employment and self-employment plus taxable benefits, whereas labor income excludes the latter. Permanent income is calculated as the average income between the ages 35 and 45. Years of schooling is included in columns (2), (4), and (6) and is controlled for using discrete categories. The regressions on permanent income include cohorts born 1933-1958. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D: Effect of birth weight on permanent income by birth cohort categories

	(1)	(2)	(3)	(4)
	Model A		Model B	
Log birth weight	.106***	0.111*	.105***	.109*
	(.035)	(.058)	(.035)	(.057)
Log birth weight*C_1940-1949		.008		.013
		(.078)		(.077)
Log birth weight*C_1933-1939		-.053		-.051
		(.102)		(.100)
Observations	22,588	22,588	22,588	22,588
Number of twin pairs	11,294	11,294	11,294	11,294

Notes: This table shows regressions on the effect of log birth weight on permanent income (ages 35-45) across cohorts born 1933-1939, 1940-1949, and 1950-1958. All coefficients are from twin fixed effects models. Permanent income is calculated as the average income between the ages 35 and 45. Column 1 shows the estimate for the combined sample of men and women. Column 2 interacts birth weight with the birth cohort categories. Column 3 and 4 repeats the analysis in Column 1 and 2, but recodes the birth weight data into 50g bins, i.e., analyzes whether measurement error matters. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E: Birth weight across the lifecycle. Balanced panels.

	Age						
	30	35	40	45	50	55	60
Balanced panels	*Panel A: Income, males*						
Log birth weight	0.123**	0.112**	0.106**	0.0762	0.0551	0.0515	-
	(0.0485)	(0.0473)	(0.0494)	(0.0542)	(0.0623)	(0.0677)	-
Observations	7,722	7,722	7,722	7,722	7,722	7,722	-
Number of twin pairs	3,861	3,861	3,861	3,861	3,861	3,861	-
	Panel B: Labor income, males						
Log birth weight	0.122**	0.116**	0.110**	0.0994	0.0212	-0.0733	-
	(0.0484)	(0.0506)	(0.0539)	(0.0766)	(0.0867)	(0.108)	-
Observations	6,668	6,668	6,668	6,668	6,668	6,668	-
Number of twin pairs	3,334	3,334	3,334	3,334	3,334	3,334	-
	Panel C: Income, females						
Log birth weight	0.358***	0.239**	0.0819	-0.00162	0.0610	0.0894	-
	(0.138)	(0.109)	(0.0773)	(0.0581)	(0.0559)	(0.0605)	-
Observations	7,722	7,722	7,722	7,722	7,722	7,722	-
Number of twin pairs	3,861	3,861	3,861	3,861	3,861	3,861	-
	Panel D: Labor income, females						
Log birth weight	0.284*	0.193	0.0412	-0.0353	-0.00902	-0.00384	-
	(0.153)	(0.122)	(0.0872)	(0.0730)	(0.0908)	(0.135)	-
Observations	5,536	5,536	5,536	5,536	5,536	5,536	-
Number of twin pairs	2,768	2,768	2,768	2,768	2,768	2,768	-

Notes: This table shows regressions for a balanced panel on birth weight and income across the lifecycle for the cohorts born 1926-1958. All coefficients are from twin fixed effects models. Panels A and B show regressions on average 5-year income and average 5-year labor income among males. Panels C and D shows the corresponding regressions for females. Income is defined as labor income from employment and self-employment plus taxable benefits, whereas labor income excludes the latter. The ages in the table refer to the midpoints of the 5-year averages. No controls for schooling. The cohorts included in the regressions vary, see the text for details. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F: Birth weight and *income* across the lifecycle. Non-linear specifications. No control for schooling.

	Age						
	30	35	40	45	50	55	60
Male twins, unbalanced							
A. Less than 1500 grams	-0.0430	-0.0424	-0.0827	-0.156**	-0.127	-0.0371	-0.0328
	(0.0685)	(0.0684)	(0.0704)	(0.0770)	(0.0835)	(0.0914)	(0.111)
B. Less than 2000 grams	-0.0604**	-0.0376	-0.0246	-0.0237	-0.0539*	-0.0453	-0.0194
	(0.0260)	(0.0257)	(0.0257)	(0.0280)	(0.0301)	(0.0335)	(0.0388)
C. Less than 2500 grams	-0.0342**	-0.0291*	-0.0224	-0.0182	-0.0166	0.00564	-0.0204
	(0.0153)	(0.0153)	(0.0153)	(0.0168)	(0.0180)	(0.0199)	(0.0234)
Observations	10,636	11,632	12,554	12,368	12,044	9,886	7,292
Number of twin pairs	5,318	5,816	6,277	6,184	6,022	4,943	3,646
Male twins, balanced							
A. Less than 1500 grams	-0.0319	-0.0806	-0.0869	-0.0493	-0.127	0.0119	-
	(0.0740)	(0.0722)	(0.0753)	(0.0826)	(0.0949)	(0.103)	-
B. Less than 2000 grams	-0.0404	-0.0321	-0.0434	-0.0340	-0.0746**	-0.0808**	-
	(0.0283)	(0.0276)	(0.0288)	(0.0316)	(0.0363)	(0.0394)	-
C. Less than 2500 grams	-0.0293*	-0.0193	-0.0113	-0.00627	0.0164	0.0142	-
	(0.0164)	(0.0160)	(0.0166)	(0.0183)	(0.0210)	(0.0228)	-
Observations	7,722	7,722	7,722	7,722	7,722	7,722	-
Number of twin pairs	3,861	3,861	3,861	3,861	3,861	3,861	-
Female twins, unbalanced							
A. Less than 1500 grams	-0.0626	0.197	0.105	-0.0346	-0.0998	-0.127	-0.288***
	(0.138)	(0.121)	(0.1000)	(0.0803)	(0.0744)	(0.0808)	(0.104)
B. Less than 2000 grams	-0.0338	-0.0608	-0.0564	0.0251	0.00187	-0.0590*	-0.0195
	(0.0538)	(0.0467)	(0.0385)	(0.0311)	(0.0288)	(0.0307)	(0.0370)
C. Less than 2500 grams	-0.0177	-0.0485	-0.0174	-0.00937	-0.0420**	-0.0417*	-0.0570**
	(0.0382)	(0.0332)	(0.0272)	(0.0218)	(0.0203)	(0.0216)	(0.0252)
Observations	9,656	11,064	12,228	12,484	12,434	10,428	8,126
Number of twin pairs	4,828	5,532	6,114	6,242	6,217	5,214	4,063
Female twins, balanced							
A. Less than 1500 grams	-0.166	0.172	0.106	0.0302	0.0124	-0.00505	-
	(0.175)	(0.137)	(0.0976)	(0.0733)	(0.0705)	(0.0764)	-
B. Less than 2000 grams	-0.0801	-0.0991*	-0.0459	0.0372	0.00232	-0.0109	-
	(0.0654)	(0.0513)	(0.0365)	(0.0274)	(0.0264)	(0.0286)	-
C. Less than 2500 grams	-0.0187	-0.0206	0.0206	0.0335*	0.0153	-0.00516	-
	(0.0469)	(0.0368)	(0.0262)	(0.0197)	(0.0189)	(0.0205)	-
Observations	6,980	6,980	6,980	6,980	6,980	6,980	-
Number of twin pairs	3,490	3,490	3,490	3,490	3,490	3,490	-

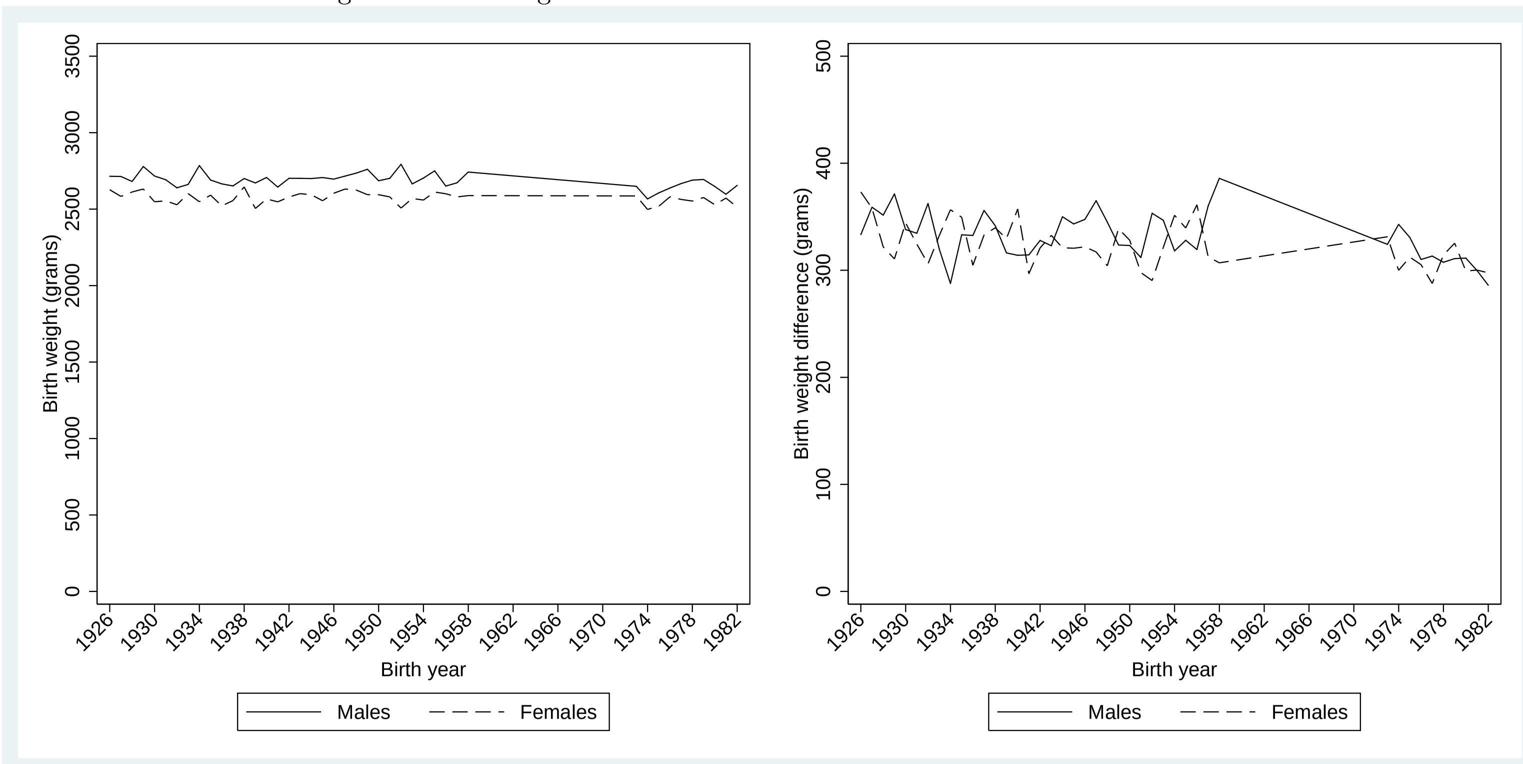
Notes: This table shows regressions on the effect of having a birth weight below different tresholds on average 5-year income at different ages. All coefficients are from twin fixed effects models. The coefficients presented in the different rows come from separate regressions. The ages in the tables refer to the midpoints of the 5-year averages. No controls for schooling. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G: Birth weight and *labor* income across the lifecycle. Non-linear specifications.

	Age						
	30	35	40	45	50	55	60
Male twins, unbalanced							
A. Less than 1500 grams	-0.0368	-0.0469	-0.0969	-0.307***	-0.427***	-0.0223	-0.0371
	(0.0719)	(0.0761)	(0.0871)	(0.104)	(0.130)	(0.181)	(0.358)
B. Less than 2000 grams	-0.0620**	-0.0410	0.00460	-0.0283	-0.129***	-0.0847	-0.125
	(0.0273)	(0.0286)	(0.0323)	(0.0383)	(0.0462)	(0.0642)	(0.123)
C. Less than 2500 grams	-0.0447***	-0.0327*	-0.00386	-0.0474**	0.00593	0.0558	-0.0527
	(0.0160)	(0.0170)	(0.0191)	(0.0228)	(0.0274)	(0.0375)	(0.0736)
Observations	10,518	11,400	12,126	11,712	11,058	8,662	5,840
Number of twin pairs	5,259	5,700	6,063	5,856	5,529	4,331	2,920
Male twins, balanced							
A. Less than 1500 grams	-0.0177	-0.107	-0.0772	-0.212*	-0.107	0.0496	-
	(0.0756)	(0.0789)	(0.0841)	(0.119)	(0.135)	(0.168)	-
B. Less than 2000 grams	-0.0425	-0.0269	-0.0534*	-0.0560	-0.0863*	-0.0838	-
	(0.0286)	(0.0298)	(0.0318)	(0.0452)	(0.0511)	(0.0634)	-
C. Less than 2500 grams	-0.0290*	-0.0163	-0.0183	-0.0194	0.0108	0.0357	-
	(0.0160)	(0.0167)	(0.0178)	(0.0253)	(0.0286)	(0.0356)	-
Observations	6,668	6,668	6,668	6,668	6,668	6,668	-
Number of twin pairs	3,334	3,334	3,334	3,334	3,334	3,334	-
Female twins, unbalanced							
A. Less than 1500 grams	-0.0710	0.0935	0.0570	-0.127	-0.0379	-0.176	-0.565**
	(0.142)	(0.124)	(0.110)	(0.0966)	(0.130)	(0.157)	(0.285)
B. Less than 2000 grams	-0.0175	-0.0632	-0.0464	0.0277	0.0136	-0.0707	-0.158
	(0.0550)	(0.0480)	(0.0426)	(0.0374)	(0.0504)	(0.0618)	(0.110)
C. Less than 2500 grams	-0.0175	-0.0575*	-0.0457	-0.0168	-0.0666*	-0.0819*	-0.107
	(0.0391)	(0.0342)	(0.0298)	(0.0257)	(0.0347)	(0.0420)	(0.0713)
Observations	9,426	10,618	11,526	11,504	10,984	8,666	5,964
Number of twin pairs	4,713	5,309	5,763	5,752	5,492	4,333	2,982
Female twins, balanced							
A. Less than 1500 grams	-0.0980	0.235	0.141	0.0699	0.112	0.0554	-
	(0.189)	(0.150)	(0.108)	(0.0901)	(0.112)	(0.167)	-
B. Less than 2000 grams	-0.0572	-0.0550	-0.0332	0.0292	0.00661	0.000876	-
	(0.0735)	(0.0584)	(0.0419)	(0.0351)	(0.0436)	(0.0649)	-
C. Less than 2500 grams	-0.0209	-0.0347	0.0194	0.0102	0.0253	-0.0172	-
	(0.0513)	(0.0408)	(0.0292)	(0.0245)	(0.0304)	(0.0453)	-
Observations	5,536	5,536	5,536	5,536	5,536	5,536	-
Number of twin pairs	2,768	2,768	2,768	2,768	2,768	2,768	-

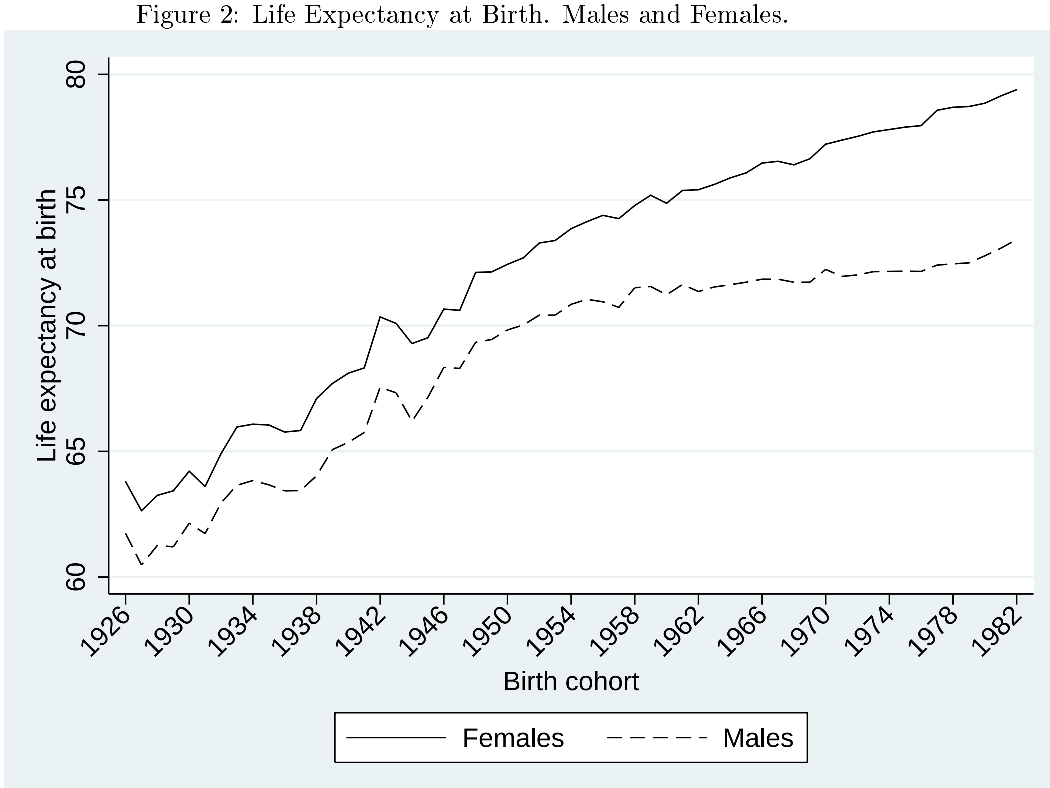
Notes: This table shows regressions on the effect of having a birth weight below different tresholds on average 5-year labor income at different ages. All coefficients are from twin fixed effects models. The coefficients presented in the different rows come from separate regressions. The ages in the tables refer to the midpoints of the 5-year averages. No controls for schooling. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Birth weight and Discordance across Cohorts Born 1926-1982.



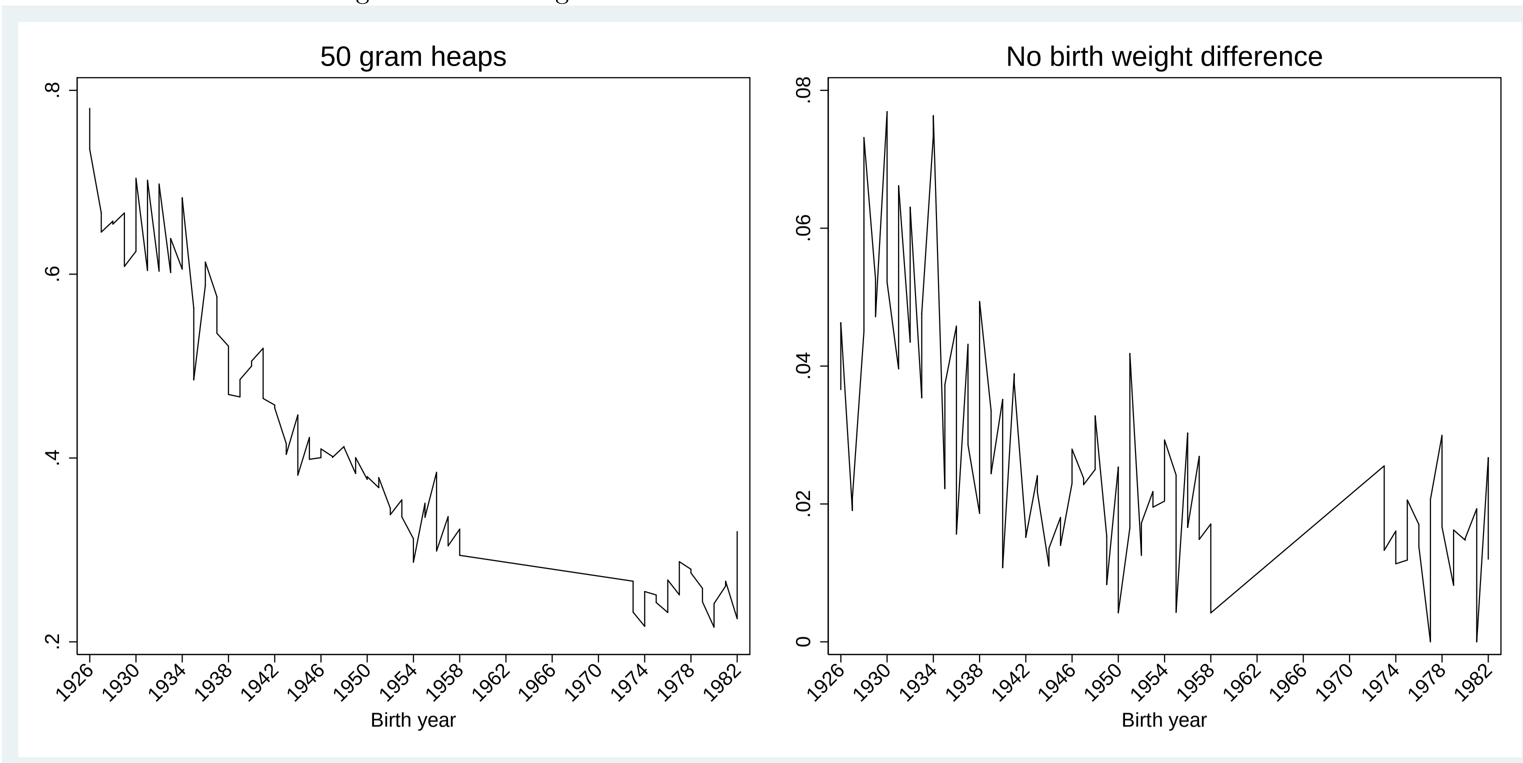
Notes: These graph plot average birth weight and average discordance in grams across cohorts born 1926-1958 and 1973-1982.

Figure 2: Life Expectancy at Birth. Males and Females.



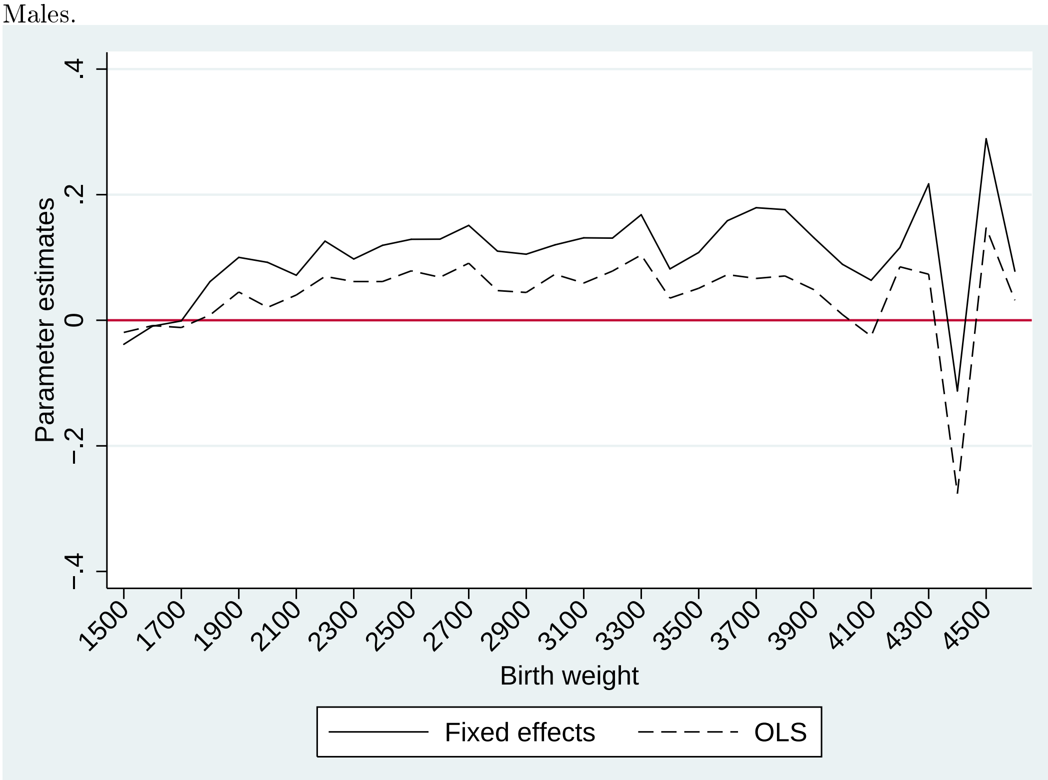
Notes: This graph plots average life expectancy at birth for the cohorts born 1926-1958. Source: www.mortality.org.

Figure 3: Birth weight Measurement across Cohorts Born 1926-1982.



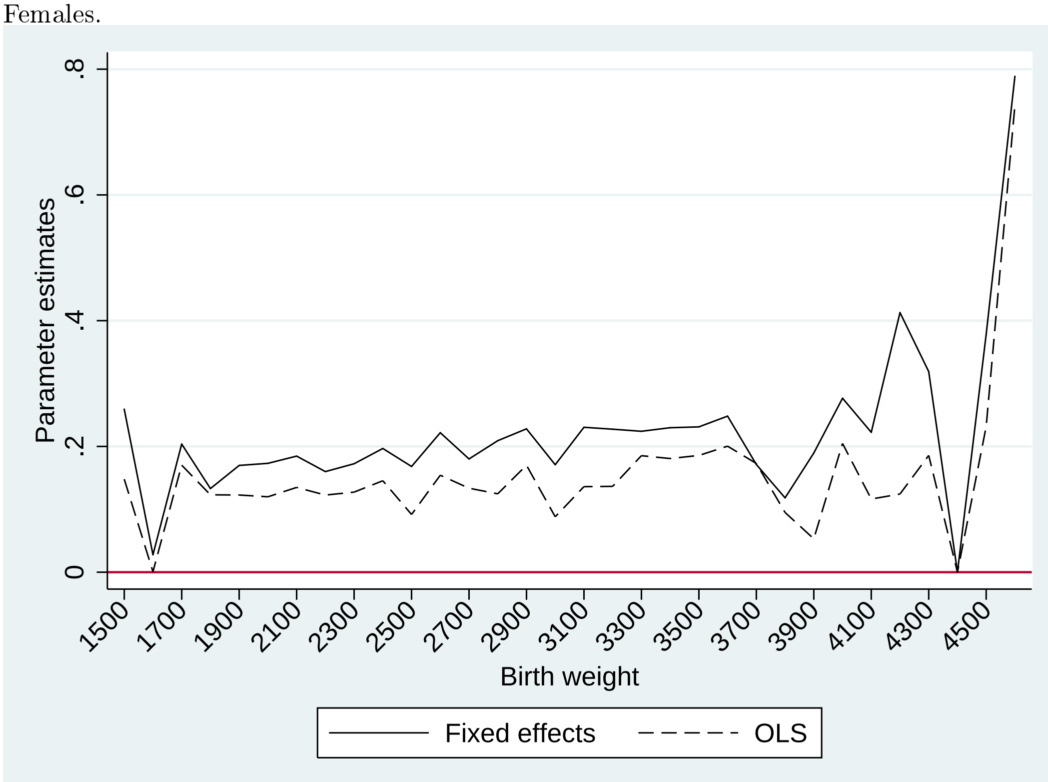
Notes: These graph plot fraction of twins where birth weight is measured in 50 gram bins (left panel) and fraction of twin pairs with zero difference in birth weight. Cohorts born 1926-1958 and 1973-1982.

Figure 4: Birth Weight and Permanent Income. Non-Parametric Estimates. Males.



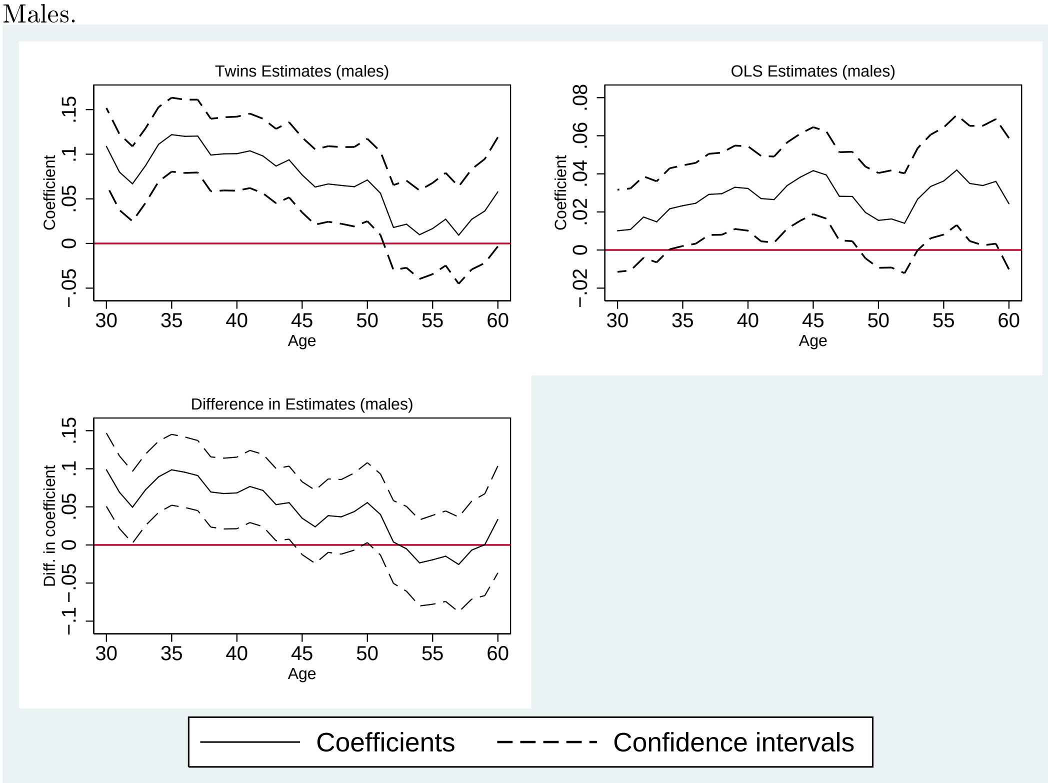
Notes: This graph plots estimates from OLS and twin fixed effects regressions on the effect of birth weight on permanent income among males, where birth weight is measured in 100 grams bins. The reference category is weighting below 1500 grams. Permanent income is calculated as the average income between the ages 35 and 45. Cohorts born 1933-1958 are included in the regressions.

Figure 5: Birth Weight and Permanent Income. Non-Parametric Estimates. Females.



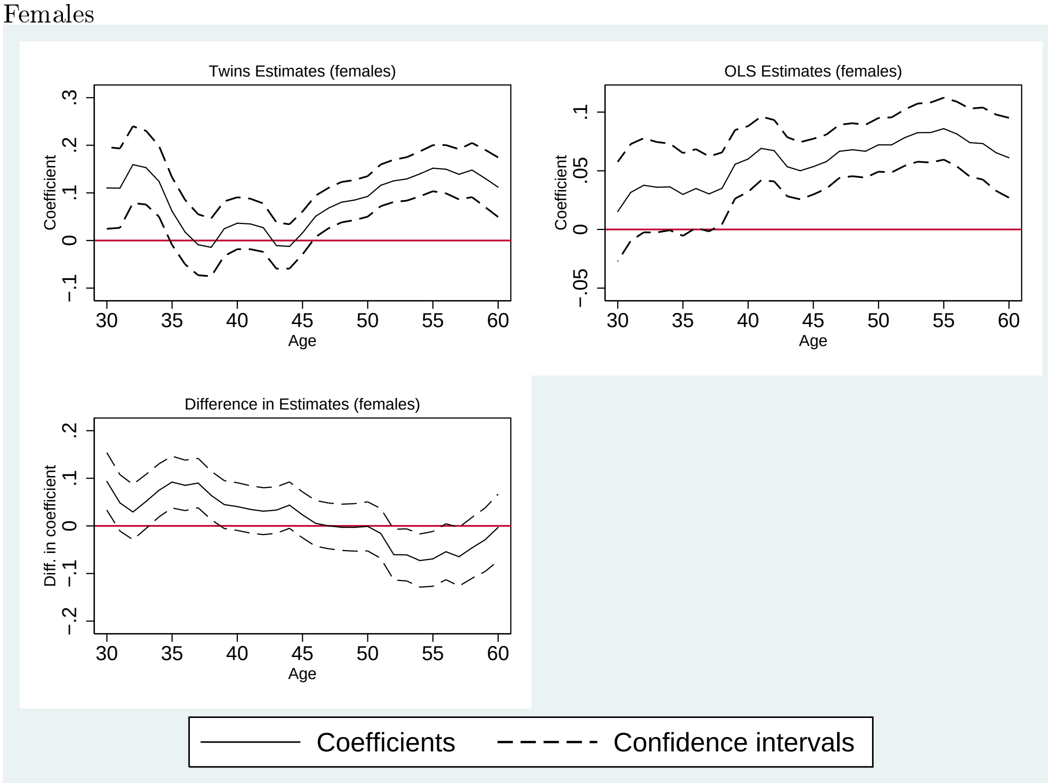
Notes: This graph plots estimates from OLS and twin fixed effects regressions on the effect of birth weight on permanent income among females, where birth weight is measured in 100 grams bins. The reference category is weighting below 1500 grams. Permanent income is calculated as the average income between the ages 35 and 45. Cohorts born 1933-1958 are included in the regressions.

Figure 6: Birth Weight and Income over the Lifecycle. OLS and Twin FE. Males.



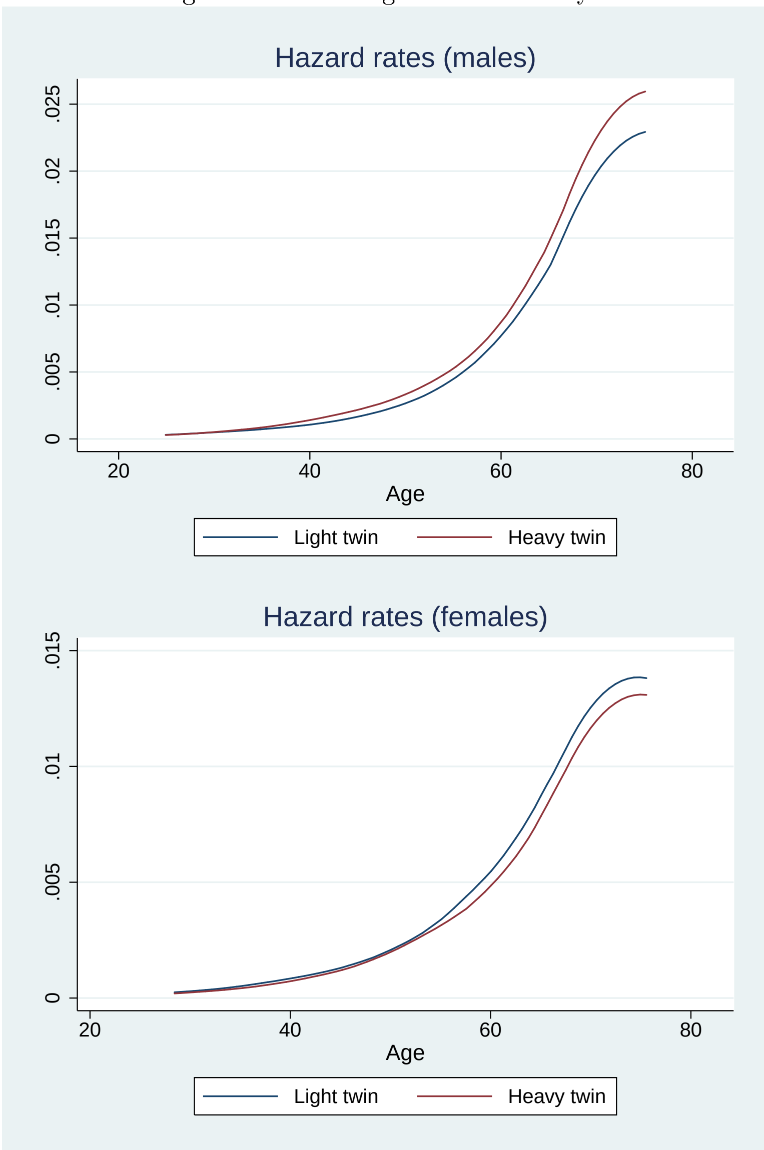
Notes: The upper panel of the graph plots estimates from twin FE (left) and OLS (right) regressions on the effect of birth weight on income across the lifecyle among males. Coefficients are interpreted as the effect on 4 year moving income averages since at each age, 4 years of data are used. The coefficient shown for age 50 uses data from ages 50-53, the coefficient for age 51 uses data from ages 51-54 and so on. The OLS regressions control for calendar year of observation and nothing else. The lower panel plots the difference in OLS and FE estimates and the corresponding confidence intervals of the difference.

Figure 7: Birth Weight and Income over the Lifecycle. OLS and Twin FE. Females



Notes: The upper panel of the graph plots estimates from twin FE (left) and OLS (right) regressions on the effect of birth weight on income across the lifecyle among females. Coefficients are interpreted as the effect on 4 year moving income averages since at each age, 4 years of data are used. The coefficient shown for age 50 uses data from ages 50-53, the coefficient for age 51 uses data from ages 51-54 and so on. The OLS regressions control for calendar year of observation and nothing else. The lower panel plots the difference in OLS and FE estimates and the corresponding confidence intervals of the difference.

Figure 8: Birth weight and Mortality. Hazard rates.



Note: This figure plots Kaplan-Meier mortality hazard rates for the ligher versus the heavier twin for cohorts born 1926-1958.