Labor market consequences of growing up with a sibling with type 1-diabetes

Swedish Childhood Diabetes Study Group

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Abstract

Economic research on child health and future labor market outcomes has mainly focused on children with impaired health themselves, and only recently begun to assess spillover effects for siblings. Yet, the challenge to accommodate a family's routines within the requirements of a complex and time-consuming disease is most likely to spillover on siblings. While the burden of ill health and managing a disease may have adverse effects, living with a disease may still give families useful experiences and skills that favor future labor market outcomes. Therefore, the potential labor market impacts of growing up with a sick sibling could be both positive and negative. This study investigates differences in the progression of annual labor earnings between siblings of children with type 1-diabetes and population controls. The data is based on detailed Swedish longitudinal registers, covering annual labor earnings in the years 1990-2010 for 764 siblings of 764 children with diabetes and 5,506 population controls born in 1962-1971, and follow individuals between ages 19-48. The results indicate that brothers of children with type 1-diabetes have lower earnings growth than controls, while sisters' earnings growth appears unaffected. Consequently, spillovers from one family member to another might differ within a family.

Keywords: Sweden, siblings, spillovers, type 1-diabetes, early life health, earnings.

1. Introduction

Existing evidence shows that childhood health affects adult labor market outcomes (see, e.g., Almond & Mazumder, 2013; Currie, 2009; Currie & Almond, 2011). Only recently has economic research started to investigate whether or not health shocks in early life affect outcomes, not only for the sick child, but also for other members of the family. A few economic studies on educational outcomes confirm the existence of spillovers between siblings. While Breining et al. (2015) show positive spillovers from early-life medical interventions, Breining (2014) shows negative influence of ADHD on siblings' educational outcomes in ninth grade. Similarly, Fletcher et al. (2012) report that having a sibling with developmental disability or externalizing behavior is associated with lower math and language test scores. In addition, a related literature report negative long-run educational and labor market impacts of childhood peers (see, e.g., Carell et al., 2016). Building on the framework developed by Bolin et al. (2002) and Jacobson (2000), who model the family as a health-producing unit and assume interrelatedness in the health of all family members, this paper contributes to this recent strand of literature by investigating the earnings of siblings growing up with a brother or sister with type 1-diabetes.

Type 1-diabetes (hereafter referred to as diabetes) is a disease with a sudden onset and well-documented consequences on everyday life and future health. Parents describe onset as a time of crisis, as learning the child's daily management routines and accommodating family routines to the requirements imposed by the disease generally poses a great challenge to the entire family (Wennick & Hallström, 2006). Managing the diabetes is complex and time-consuming, involving several daily insulin injections and blood glucose check-ups, exercise, and dietary restrictions. The imposed focus on a healthier lifestyle with daily routines could have positive effects, fostering children to become more responsible adults. Moreover, caring for a child with diabetes imposes a host of long-term stressors (e.g., fear of hypoglycemia and increased insecurity about future health) (Sparud-Lundin et al., 2013). Therefore, the time and effort needed to manage the disease and the stress and insecurities that follows are likely to affect the entire family, even though some children with diabetes are relatively healthy during long periods.

Several studies find that childhood onset of diabetes has adverse educational and labor market consequences for the affected individual (see, e.g., Dahlquist et al., 2007; Fletcher & Richards, 2012; Lundborg et al., 2014; Minor, 2011, 2013; Persson et al., 2013; Steen Carlsson et al., 2010). Moreover, child health shocks have been shown to strain family resources (e.g., by reducing parents' working hours (Kvist et al., 2013)), change intra-household resource allocation (e.g., parents compensate for or reinforce differences in child endowments (Almond & Mazumder, 2013; Currie & Almond, 2011)), and affect the quality of interaction among family members (Heckman & Mosso, 2014).

Many families of children with diabetes spend time and effort to restore and maintain the child's health. This could cause the family to redefine its preferences towards, for example, a healthier lifestyle, but reduce the family resources available for other activities. Such changes may in turn affect the sick child's skill formation and labor market performance later in life. If this is the case, not only the sick child, but also its siblings, could be affected. If parents spend less time helping with homework due to changes in their time constraints, this may have negative consequences for the human capital formation of both the child with diabetes and his or her siblings. Alternatively, if, in caring for a child with diabetes, parents become more health- and family-oriented, parent-child interactions may improve and the children may learn skills (such as responsibility and foresightedness) that favor future labor market outcomes. Consequently, potential diabetes-related spillover effects on siblings may run through several channels and could either undermine or improve future labor market outcomes. The resulting impact on earnings is, therefore, an empirical question.

The spillover effects on siblings' outcomes may differ substantially between individuals. Heckman et al. (2006) argue that human capabilities (i.e., health, cognitive skills, and non-cognitive skills) are closely related and that behaviors and abilities have both a genetic and an acquired character. Therefore, unobservable individual-specific factors might affect both health behaviors and the capability of incorporating a sibling's diabetes into everyday life. These individual-specific factors may have both moderating and mediating effects on sibling outcomes. As moderators they may indicate heterogeneous effects across individuals with certain individual-specific characteristics. For example, inherited ability and/or preferences are likely to moderate sibling spillovers by influencing *if* and *how much* the individual's behavior is affected by having a sibling with diabetes.

On the other hand, child and adolescent abilities and preferences may themselves be influenced by having a sick sibling, thereby mediating the spillover effects of the diabetes. If the individual-specific factors are mediators, education and family formation are also likely to be affected by the sibling's diabetes, as cognitive and non-cognitive ability directly affects schooling, fertility, and other aspects of social and economic life (Heckman et al., 2006). Assessing observable mediator variables related to education and family formation may give us a clue to whether unobservable abilities contribute to the mechanisms of the potential sibling spillover effects. Individual-specific factors (e.g., inherited preferences favoring a healthy lifestyle) may also be important confounders, influencing the development of diabetes, which is a multifactorial disease, triggered by a partially unknown combination of environmental and genetic factors (Daneman, 2006). Therefore, we cannot rule out the possibility that individual-specific factors are correlated with both the presence of a diabetic sibling and future labor market outcomes.

Using detailed Swedish longitudinal registers, covering the years 1990-2010, I examine the progression of annual earnings for individuals with a sibling that was diagnosed with diabetes in age 6-15. Following the earnings trajectories of individuals aged 19-48 during this time period (hence born in 1962-1971), I take an exploratory approach to assess the potential influence of both unobservable individual-specific factors and mediator variables related to education and family formation.

Psychological research in diabetes and child health suggests that siblings of chronically ill children have contradictory feelings towards their sick brother or sister: a strong sense of responsibility (e.g., acting as protector and caregiver); resentment (e.g., being jealous of the sick sibling receiving extra attention); exaggerated sibling rivalry (e.g., fighting for parents' attention); and social and emotional isolation (e.g., being afraid to increase their parents' worries and evoke their anger by showing negative feelings for, or fail to protect, their sick sibling) (Wennick and Huus, 2012; O'Brien et al., 2009).

Hollidge (2001) finds that such feelings may interfere with psychological development and contribute to feelings of low self-esteem, anxiety and/or depressive and psychosomatic symptoms. However, similar studies focusing solely on siblings of children with diabetes are scarce and their results are inconclusive (Gendelman et al., 2009; Luyckx et al., 2010; Sleeman et al., 2010). Whereas some studies find increased risk of maladjustment (Adams et al., 1991), others find that siblings of children with diabetes function psychologically as well, or even better, than siblings of non-diabetic children (Hollidge, 2001; Jackson et al., 2008; Sleeman et al., 2010).

Despite the conflicting results, this literature suggests that boys and girls may respond differently when their sibling falls ill (Gendelman et al., 2009; Hollidge, 2001; O'Brien et al., 2009). Girls tend to show more internalizing symptoms (e.g., depression, withdrawal), while boys show more externalizing ones (e.g., hyperactivity, aggression). It is possible that these gender differences affect labor market responses to growing up with diabetes, as externalizing behaviors have been connected to adverse educational (McLeod & Kaiser, 2004; Miech et al., 1999) and labor market (Gregg & Machin, 2000) outcomes, whereas internalizing strategies appear to have less of an impact on future outcomes (McLeod & Kaiser, 2004; Miech et al., 1999).

2. Data

This study uses data from the Swedish Childhood Diabetes Register (SCDR), which has recorded incident cases of diabetes in children aged 0-14.9 years in Sweden since 1977 (see, e.g., Dahlquist et al., 1982). The SCDR data is collected in accordance with the Declaration of Helsinki. Informed consent was given by all parents of registered children. Present research on the database was approved by the Regional Research Ethics Board in Umeå (Dnr 071-69M). The Swedish Childhood Diabetes Study Group has added data to the SCDR as follows: for each individual, Statistics Sweden identified parents and siblings from the Multi-Generation Register and matched four non-diabetic controls from the Total Population Register to each individual with diabetes by age and municipality of residence at the time of diagnosis. Statistics Sweden also connected the population controls to their parents and added background characteristics and yearly earnings data for 1990-2010, for each individual from the Longitudinal Integration Database for Health Insurance and Labour Market Studies (Statistics Sweden, 2011).

The SCDR comprises 2,551 non-diabetic siblings of children who were born in 1962-1971 and diagnosed with diabetes between 1977-1986. I excluded 527 siblings born before 1962 and 846 siblings born after 1971 to prevent differing age distributions of affecting the results. The reason behind the differing age distributions is that the control group was originally designed to match the diabetes group. As the data, consequently, does not cover all siblings of the children with diabetes, this study focuses on the siblings who are most likely affected by diabetes-induced spillovers by including the sibling who is the closest in age to the child with diabetes and who is younger than 16 at the time of diagnosis. Siblings older than 15 were excluded (390 siblings) as they only share a relatively short period of their upbringing with a sick brother or sister. If the child with diabetes had two siblings with the same age difference, I included the older (24 siblings excluded). The resulting dataset comprise 764 siblings of 764 children with diabetes and 5,506 population controls. Consequently, the findings of this study may not be generalizable to all siblings, as they are likely to represent upper bounds of the spillover effects. The dataset contains no information on whether the individuals in the control group have any siblings. Therefore, the siblings of individuals with diabetes are compared to individuals both with and without siblings.

Because siblings are often born with only a few years apart and both the children with diabetes and their siblings were relatively old (between ages 6-15) at the time of diagnosis, the sample is unlikely to be skewed by the possibility of the onset of diabetes affecting parents' fertility choices. The siblings were on average 11.9 (standard deviation 2.4) and the mothers were 38.6 years old (standard deviation 5.7) at the time of diagnosis. In the late 1960s, Swedish women were on average 26.4 years old at childbirth (all births) and having children after age 40 was uncommon (less than four out of 1000 births) (Statistics Sweden, 2014). Moreover, the number of siblings of the diabetic children is similar to that of the general population: the average number of children per woman was 2.13 in 1962 and 1.96 in 1971 (Statistics Sweden, 2014), while the families with a diabetic child in SCDR have on average 1.96 children when including families without siblings and 2.4 children in the studied sample. 47.0% of the siblings of diabetic children are same-sex siblings and 56.8% are older than the child with diabetes. 45.8% of the sibling pairs are born within two years, and 94.9% are born within five years of each other.

This paper will hereafter refer 'sibling/s' to siblings of children with diabetes. 'Brothers' refer to men that have grown up with a brother or sister with diabetes and 'sisters' refer to women that have grown up with a brother or sister with diabetes.

Following earnings in the years 1990-2010, the dataset is a panel with 128,235 observations and an average of 20.5 observations per individual. The timeline of the sample and how each cohort contributes to the studied panel are shown in Supplementary Figure A [INSERT LINK TO ONLINE FILE A]. In order to exclude individuals with only short-term (holiday) jobs, I restrict the sample to the years that each individual is part of the labor force, defined as having annual earnings exceeding one price base amount (PBA), as did Lundborg et al. (2015). Fifteen siblings (18.1% of the sibling-year observations) and 127 controls (20.0%) fall below this threshold. The PBA is a measure based on changes in the general price level and is set by the Swedish government. It increased from SEK 29,700 (≈EUR 2,970) to SEK 42,400 (≈EUR 4,240) over the study period.

Table 1 shows sample means at age 30, when most individuals have finalized their education but are at an early career stage. The outcome variable, annual earnings, and the potential mediator variables related to education and family formation may be affected by the presence of a child with diabetes, whereas the other variables, representing family background, are measured pre-onset or are constant over time.

The siblings are on average slightly older than the control group. Brothers' mean annual earnings are SEK 241,000, while men in the control group earn SEK 259,000. The difference in earnings between sisters and women in the control group is small (SEK 175,000 for sisters, SEK 180,000 for controls). Siblings are similar to the control group in terms of civil status, but sisters are more likely to have one or more children than women in the control group (though significant only at 10%). No significant differences exist between the groups in own or parental level of education. Yet, a higher proportion of the parents of brothers have a university education than parents of men in the control group. Due to the relatively high prevalence of diabetes among native Swedes and Finnish immigrants, a higher proportion of controls have non-Nordic born parents. This explains why parents of the controls more often have missing educational data. Fathers of siblings belong to earlier cohorts than fathers of the controls.

3. Method

The empirical strategy is divided into two parts. First, to design a control group which matches the siblings, I use the Entropy Balancing (EB) method developed by Hainmueller (2012). Because the EB weights makes the controls more similar to the siblings in terms of observable characteristics, the matching procedure is assumed to also make the two groups more likely to be similar with regards to (time-variant and time-invariant) unobservable factors (Ho et al., 2007). Second, I assess potential earnings differences between the siblings and the weighted controls using regression models both with and without controls for individual-fixed effects and potential mediators to test (1) if time-invariant individual-specific factors appear to influence the studied relationship, and (2) if the earnings differentials are driven by post-onset differences in education and family formation.

3.1 Entropy balancing

Building on the propensity score matching technique, the EB method (Hainmueller, 2012) achieves covariate balance by constructing a weight for each control observation such that the sample moments of covariates are identical between the siblings and the weighted controls. More weight is given to the under-represented controls and less weight to over-represented controls. The weights are calculated to satisfy pre-specified balancing conditions (i.e., all conditioning variables having the same mean, variance, and skewness as in the siblings group). Thereby, this non-parametric weighting procedure directly secures covariate balance with maximum retention of information. In practice, the weights are chosen to make the weighted control group match the sibling group (for women and men separately) in terms of the observable background variables presented in Table 1.

By including interaction terms of the background variables, covariates are balanced across subsample groups, such as individuals with two university educated parents. The standardized differences in means of the variables are a quality measure for the matching process. Figure 1 shows that these differences ranged from -0.42 to 0.15 before EB and are reduced to zero afterwards, confirming that EB has improved balance for *all* conditioning variables (see also Table A.1 showing the mean of the conditioning variables before and after EB). I use the Ebalance package for Stata (Hainmueller & Xu, 2013).

3.2 Regression analysis

The effects of child health on adult earnings are likely to vary over time. Whereas Heckman et al. (2006) suggest that early health shocks accumulate over time so that even small health shocks could lead to adverse adult outcomes, Grossman (1972a,b) instead predicts that the effects of health shocks diminish over time. It is possible that spillover effects on siblings exhibit a similar behavior. To account for this possibility, I assess age-specific differences in earnings between siblings and weighted controls. I use the following specification for individual *i* in year *t*:

$$y_{it} = \alpha + \beta D_i + \sum_{Age} \gamma_{age} AGE_{it} + \sum_{Age} \delta_{age} D_i * AGE_{it} + \lambda_t + \varepsilon_{it} \quad (1)$$

The dependent variable, y_{it}, is the natural logarithm of annual labor earnings for individual *i* during year t, conditional on earnings>1PBA. This variable comprises all (gross) earnings from employment and self-employment reported to the Swedish Tax Agency. D_i is a dummy variable which takes on the value one for siblings. AGE_{it} is a vector of dummy variables representing the age categories 26-30, 31-35, 36-40, 41-45, and 46-48 years. The reference category is 19-25. The interaction terms, $D_i{*}AGE_{it}$, capture age-specific differences in average annual earnings (in percentage points) between the siblings and the weighted controls. The coefficient of D_i shows how much the average annual earnings differ between the siblings group and the weighted controls at age 19-25, and the coefficient on, for example, the second interaction term, $D_i{*}AGE_{i2}$, displays the additional difference between the groups at age 26-30. λ_t is a vector of year dummies (1990-2010), which control for aggregate changes in the economy over time and ε_{it} is an idiosyncratic error term.

$$y_{it} = \alpha + \beta D_i + \sum_{Age} \gamma_{age} AGE_{it} + \sum_{Age} \delta_{age} D_i * AGE_{it} + \theta X_{it} + \lambda_t + \varepsilon_{it} \quad (2)$$

The second specification adds X_{it}, which is a vector of observable potential mediators, including education (compulsory, upper secondary, or university) and indicator variables for having children and marital status (married, divorced, and widow(er)). These variables are added to test whether they are channeling the studied relationship by absorbing some of the effect of the sibling spillovers. If so, the coefficients of the mediator variables may not capture their causal effect and, more importantly, including these variables could bias the sibling estimates. Therefore, the results from equation (2) should not be interpreted as causal effects.

$$y_{it} = \alpha + \beta D_i + \sum_{Age} \gamma_{age} AGE_{it} + \sum_{Age} \delta_{age} D_i * AGE_{it} + \lambda_t + \mu_i + \varepsilon_{it} \quad (3)$$

The third specification adds the vector μ_i of individual-fixed effects, which absorb the effect of time-invariant individual-specific factors. I remove μ_i from the estimation problem by using mean differenced data.

Because the FE model relies on variation within individuals across time, the resulting estimates capture within-individual changes in earnings over time, and do not distinguish between high- and low-level earnings profiles. Consequently, the FE model captures only the effect on the earnings trajectory, whereas the OLS model assesses the total average earnings gap, including both level- and trajectory effects. Also, potential differences between siblings and controls that are caused by the diabetes, but do not vary with age (i.e., differences in the reference ages) will be captured by the individual-specific effects. For example, if the 'sibling' effect operates via an individual's level of earnings capacity, which is determined prior to the observed period (i.e., time-invariant during the observed period) it will not be identified by the FE model and the individual-fixed effects will be mediating the studied relationship.

Individual-fixed effects could also be moderating the spillover effect, so that some individual-specific factor, which is unaffected by having a sibling with diabetes, affects both who better handles life with a sibling with diabetes and who has a positive earnings trajectory. If this is the case, controlling for individual-specific effects implies that individuals who have a sibling with diabetes are compared only to others with the same level of that time-invariant individual-specific factor.

4. Results

4.1 Earnings differences

Table 2 displays estimates of age-specific sibling spillovers. Appendix B shows results for mediators. The results do not indicate any significant spillover effects for sisters, while brothers' earnings appear affected when using the FE estimator (column 8). The FE estimates for brothers are significantly lower for all age categories compared to those of the weighted controls (except for ages 46-48, probably due to the small number of observations in this category). These differences increase with age, from 4.8 percentage points lower earnings growth than the weighted controls at age 26-30 to 7.6 percentage points at age 41-45.

The OLS estimates show no statistically significant difference in earnings between siblings and weighted controls. The positive (but insignificant) estimate for brothers in the reference ages could suggest higher initial earnings, but the positive coefficient turns negative at older ages. This is likely to be the case if brothers, as a group, are less educated and therefore enter the labor market earlier than weighted controls. However, educational level does not appear to be a channel through which sibling spillovers affect earnings. Nor is marital status or having one or more children, as the estimates appear robust across specifications, with only small deviations in size when adding observable potential mediators. Appendix C, assessing potential mediators as outcome variables, shows no significant differences between siblings and weighted controls regarding their probabilities of having a university education, having children, and being married at age 30. These findings give us no reason to believe that the positive (but insignificant) estimate for brothers in the reference ages relates to level differences in earnings capacity.

The difference between the OLS and FE estimates is not surprising given that these models capture different aspects of the studied relationship. Still, it indicates that individual-fixed effects are influential and that the results are sensitive to the chosen estimation strategy. If the EB weights works correctly, a simple comparison of mean earnings should produce unbiased results. Comparing mean earnings at age 30 produces results consistent with the FE results (brothers have 5.2 percent lower mean earnings than weighted controls, Table C.1).

Intuitively, the OLS estimates ought to be larger than the FE estimates as the FE specification adds controls for time-invariant individual-specific effects and captures only the effect on trajectory in labor outcome, whereas the OLS model also captures differences in levels. Given that the net effect of the spillovers appear to be negative, it seems reasonable to expect that the OLS estimates would be larger also if individual-specific factors are confounding the studied relationship. This could be the case if some time-invariant unobservable factor (e.g., inherited ability and/or preferences favoring a healthy lifestyle) reduces the risk of diabetes onset in one's family member and also favors higher earnings. However, previous studies that use diabetes as a measure of child health describe its onset as exogenous (i.e., a health shock that individuals neither anticipate nor influence before onset) (Minor, 2011; Persson et al., 2013; Steen Carlsson et al., 2010), suggesting that confounding is unlikely to be the main reason behind the larger FE estimates.

More likely, the larger FE estimates are due to unobservable traits which positively affect both the individual's capacity of handling life with a diabetic sibling and his/her earnings trajectory. If individual-fixed effects moderate the effect of sibling spillovers, then the influence of high-ability individuals compensate for the negative spillovers within the sibling group, when individual-specific factors are not controlled for. If this is the case, differences due to sibling spillovers will become evident when conditioning on individual-specific factors, as high-ability individuals are compared only with each other and can no longer compensate for lower-ability peers.

Potential mediation via childhood ability may also arise if individual-specific factors are influenced by sibling spillovers. However, the OLS specification shows no significant differences between siblings and weighted controls in the reference ages 19-25, indicating no level differences in earnings capacity. Nor is there any indication of differences in levels of education or family formation driving the results. Also, most types of ability have been found to stabilize early in life (e.g., IQ generally manifests around age 10 (Heckman, 2007)). Therefore, sibling spillovers are more likely to affect the individual-specific factors (causing mediation) the younger the siblings are at diabetes onset. To test for the presence of mediation, I estimate model (1) excluding all individuals who were younger than eleven when their sibling was diagnosed. This does not change the main results, suggesting that mediation through childhood ability is not the driving force behind the larger FE estimates. Results are available on request.

Given that sibling spillovers could operate to either deter or favor both ability formation and earnings, we can only speculate to the mechanisms that are at play. However, it is clear that individual-specific factors are important in this setting, possibly by creating heterogeneous effects across individuals with certain individual-specific characteristics.

4.2 Sensitivity analysis

It is possible that omitted variables and unobservable between-group differences in characteristics that affect earnings, but are unrelated to having a sibling with diabetes could bias the results. Accounting for this possibility, I run a placebo test on (previously excluded) siblings who were older than 15 at diabetes onset. Because they only share a relatively short period of their upbringing with a sick sibling, their earnings are more likely to be unaffected by diabetes-related spillovers than the earnings of siblings who were younger at the time of onset. If this assumption is true, significant 'sibling' effects would indicate that some factor, other than diabetes-induced spillovers, is biasing the results. However, this does not seem to be the case, as the results (available on request) are insignificant in all specifications. Similarly, excluding all siblings that are born more than five years apart, in order to reduce the exposure to the diabetes, does not change the main results. Results are available on request.

A general concern when estimating earnings equations is selection into employment. However, individual-fixed effects may alleviate this issue by controlling for time-invariant unobservable factors (e.g., permanent ability) that might lead to self-selection into employment. Moreover, siblings and their weighted controls appear to be equally likely to have earnings>1PBA. Table D.1, reporting results from equations (1) and (3) but with the probability of having earnings>1PBA as outcome variable, shows no significant age categories except for sisters in ages 36-40.

Testing the results sensitivity to the 1PBA threshold, Table E.1 presents unconditional estimates and estimates conditional on having annual earnings above SEK 100,000. This higher threshold has previously been shown to yield results similar to those of hourly wage when studying the returns to education in Sweden (Antelius & Björklund, 2000). These results confirm the FE results, but the size of the estimates is sensitive to the chosen threshold, suggesting that spillovers influence earnings via both wages and labor supply. Therefore, we need to be aware that the estimates are conditional on earnings>1PBA when interpreting the results. Finally, Appendix F presents results for brothers and controls that is matched using propensity score weighting rather than entropy balancing. The results are similar to those obtained by the entropy balancing method.

5. Discussion

This study contributes to a recent strand of literature, which investigates how health shocks to one family member affect other family members, focusing on siblings of children with diabetes. Using detailed longitudinal register data, I find that brothers have lower annual labor earnings growth than peers when controlling for individual-fixed effects, while sisters' earnings trajectory appear to be unaffected.

The negative net-effect of sibling spillovers on brothers' earnings increases over time, from 4.8 percentage points lower earnings growth than peers at ages 26-30 to 7.6 percentage points at ages 41-45. This finding is likely an upper bound for the effect, as this study focuses on the siblings that are the closest in age, and thereby likely to be the most exposed to their sibling's disease. The results are insensitive to controlling for educational level and family formation, but selection into different occupations or fields of education might still be an important explanation driving the results. I leave this to future research to determine.

There are two major strengths of this study. First, diabetes is an interesting case to study as spillovers between siblings could potentially arise from and affect many aspects of life, as diabetes has diverse and well-documented impacts on the entire family (Wennick & Hallström, 2006). Second, this study tests whether individual-specific factors appear to influence if sibling spillovers affect earnings. The results indicate that such factors are influential and suggest further research to disentangle the mechanisms behind their importance. It is possible that individual-specific factors moderate the relationship, so that individuals with different inherited abilities respond differently to having a sibling with diabetes.

The reason behind the different findings for sisters and brothers remains to be explained. Potential explanations could relate to differences in childhood adjustment strategies. Boys have been shown to adopt externalizing behaviors, which have in turn been linked to adverse educational and labor market outcomes (Gregg & Machin, 2000; McLeod & Kaiser, 2004; Miech et al., 1999). Conversely, girls more often show internalizing symptoms, which have been reported as less important for future outcomes (McLeod & Kaiser, 2004; Miech et al., 1999). Another potential explanation could relate to differences in peer-effects and parent-child interactions between boys and girl. Previous results underline the importance of family interactions for early-life medical interventions (Breining et al. 2015) and developmental disabilities (Fletcher et al. (2012), suggesting that this type of potential differences may be an important channel of the gender differences in spillover effects. However, converse to the results of this study, Fletcher et al. (2012) show that girls are more negatively affected than boys by sibling spillovers from developmental disabilities. They explain their finding by women experiencing greater intimacy in the sibling relationship. This explanation may be an important explanation of the robustness of women's earnings to exposure to a diabetic sibling, as greater intimacy could counteract an exaggerated sibling rivalry and feelings of social and emotional isolation that are often reported by the psychological research in diabetes and child health (Wennick and Huus, 2012; O'Brien et al., 2009).

Future work should explore within-family interactions as a possible pathway of sibling spillovers also for diabetes. To present separate results by the siblings' gender composition, order of birth, and birth spacing could be informative in this respect. For example, Fletcher et al. (2012) find that younger siblings of children with externalizing behaviors tend to be more negatively affected than older siblings. For siblings of children with diabetes, feelings of responsibility for the diabetic sibling are common (Wennick and Huus, 2012) and possibly more so when protecting and caring for a younger sibling. To learn responsibility is often a good thing, but too much responsibility could be a stressor. A mis-match between insulin, food, and exercise leads to acute complications of the diabetes and, therefore, is a higher degree of behavioral regulation than is normal for a child of similar age required by children with diabetes and possibly also by their siblings. Furthermore, the spillovers might also operate through parents, as they may reduce their workhours to free time to care for their sick child. Such a response could have a negative effect on financial resources but a positive effect on parent-child interactions. Possibly, financial resources are of less importance in the Swedish setting with universal social insurance coverage (i.e., low cost of care, and free pediatric care).

This study underlines the importance of considering all family members when studying the consequences of childhood onset of chronic illness. These findings for siblings of individuals with diabetes suggest the importance of actions acknowledging a broader family impact when initiating further research and support children's diabetes management programs that target also siblings. However, the shown difference in spillovers for brothers and sisters indicates that spillovers from one family member to another might differ within a family. More research is needed to further assess diabetes-related spillovers between siblings and to disentangle its mechanisms.

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Appendices

A. The entropy balancing conditions

B. Extended results

C. Log(Earnings) and mediators as outcomes

D. Probability of earnings>1PBA

E. Alternative thresholds

F. Propensity Scores

To get the (probit) estimations of the propensity scores (PS) to converge, I use a more restricted set of constraints than for the EB weighting, excluding most of the interactions. Because we strive to find a matching procedure with a good balance on a large number of covariates to increase the similarity between the siblings and the controls, the more restricted set of constraints for the PS method speaks in favor of the EB method.

The PS are used to reweighting a control group's observations either by weights (as done here) or by weights that depend on PS distances to the treatment group's observations (as in nearest neighbor or kernel matching). Often, the time-consuming process of PS estimation, matching, and balance checking succeeds in improving the balance on one covariate at the cost of that of another (Ho et al., 2007). The EB technique is more efficient in reducing covariate imbalance as it directly secures balance by reweighting the controls observation is a way that satisfy pre-specified balancing conditions.

Tables

Table 1: Descriptive statistics at age 30

	Sisters	Women controls	t-test	Brothers	Men controls	t-test
	mean	mean	p-value	mean	mean	p-value
Year of birth	1967.23	1968.27	0.000	1967.43	1968.31	0.000
Annual earnings[a]	175132	179578	0.421	240851	258601	0.02
Compulsory[b]	0.07	0.08	0.655	0.10	0.10	0.695
Upper secondary	0.59	0.54	0.118	0.59	0.61	0.571
University	0.34	0.38	0.175	0.31	0.30	0.701
Married	0.34	0.34	0.859	0.28	0.24	0.177
Divorced	0.04	0.04	0.849	0.02	0.02	0.763
Child in household	0.41	0.35	0.079	0.24	0.24	0.94
Mothers						
Year of birth	1941.34	1941.74	0.276	1941.51	1941.91	0.237
Non-Nordic	0.02	0.05	0.083	0.01	0.04	0.008
Age at child's birth	25.89	26.54	0.058	25.92	26.40	0.126
Compulsory[b]	0.37	0.37	0.964	0.36	0.38	0.487
Upper secondary	0.40	0.40	0.997	0.39	0.40	0.601
University	0.22	0.19	0.264	0.23	0.19	0.072
Missing educational data	0.01	0.03	0.014	0.02	0.03	0.5
Fathers						
Year of birth	1938.05	1938.91	0.03	1938.06	1938.92	0.024
Non-Nordic	0.04	0.07	0.052	0.03	0.06	0.029
Compulsory[b]	0.43	0.37	0.056	0.42	0.40	0.571
Upper secondary	0.36	0.37	0.853	0.35	0.36	0.71
University	0.17	0.17	0.937	0.20	0.16	0.034
Missing educational data	0.03	0.09	0.001	0.03	0.08	0.001
Observations	369	2605		380	2774	

[a] SEK 2010 prices (SEK 10 ≈ EUR 1)
[b]Chi-2 test of differences in level of education are insignificant

Table 2: Age-specific estimates of earnings differences between siblings and weighted controls

	Sisters				Brothers			
	OLS	OLS	OLS	FE	OLS	OLS	OLS	FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sibling	-0.0130	-0.0113	-0.0125		0.0283	0.0314	0.0296	
	(0.0238)	(0.0245)	(0.0240)		(0.0232)	(0.0236)	(0.0237)	
*Sibling*26-30*	-0.00164	0.00123	0.00861	-0.00622	-0.0403	-0.0431*	-0.0422*	-0.0480*
	(0.0283)	(0.0281)	(0.0275)	(0.0291)	(0.0250)	(0.0246)	(0.0246)	(0.0257)
*Sibling*31-35*	0.0275	0.0332	0.0324	0.0180	-0.0312	-0.0360	-0.0351	-0.0515*
	(0.0331)	(0.0330)	(0.0324)	(0.0331)	(0.0302)	(0.0295)	(0.0292)	(0.0310)
*Sibling*36-40*	0.0232	0.0305	0.0285	-0.00173	-0.0525	-0.0566*	-0.0514	-0.0638*
	(0.0351)	(0.0349)	(0.0345)	(0.0344)	(0.0337)	(0.0327)	(0.0320)	(0.0333)
*Sibling*41-45*	0.0246	0.0308	0.0265	-0.00517	-0.0313	-0.0426	-0.0372	-0.0760**
	(0.0397)	(0.0395)	(0.0394)	(0.0366)	(0.0410)	(0.0397)	(0.0384)	(0.0365)
*Sibling*46-48*	0.0358	0.0262	0.0166	-0.00377	0.0575	0.0486	0.0576	-0.106
	(0.0763)	(0.0773)	(0.0796)	(0.0681)	(0.105)	(0.0966)	(0.0939)	(0.0778)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education	No	Yes	Yes	No	No	Yes	Yes	No
Family	No	No	Yes	No	No	No	Yes	No
Observations	47577	47577	47577	47577	55319	55319	55319	55319
Individuals				2974				3154
R2	0.293	0.317	0.332	0.387	0.349	0.372	0.384	0.490

Robust (clustered) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Year FE indicates controls or year-fixed effects. Education indicates controls for level of education. Family indicates controls for marital status and child(ren).

Table A.1: Descriptive statistics of own and parents' background factors for brothers and controls before and after entropy balancing

	Brothers mean	Controls mean before EB	Controls mean after EB	sdiff[a] before EB	sdiff[a] after EB
Mother_compulsory	0.37	0.39	0.37	-0.04	0.00
Mother_university	0.22	0.18	0.22	0.11	0.00
Mother_education_missing	0.02	0.03	0.02	-0.04	0.00
Father_compulsory	0.41	0.41	0.41	0.01	0.00
Father_university	0.20	0.15	0.20	0.11	0.00
Father_education_missing	0.03	0.08	0.03	-0.28	0.00
Year_of_birth	1967.43	1968.31	1967.43	-0.08	0.00
Mother_year_of_birth	1941.55	1941.85	1941.55	-0.06	0.00
Father_year_of_birth	1938.06	1938.90	1938.06	-0.13	0.00
Mother_foreign	0.01	0.04	0.01	-0.34	0.00
Father_foreign	0.03	0.06	0.03	-0.18	0.00
Age_at_child_birth*Mother_year_of_birth	25.82	26.37	25.82	-0.12	0.00
Mother_compulsory*Father_compulsory	50789.61	51911.86	50790.11	-0.12	0.00
Mother_compulsory*Father_university	0.24	0.22	0.24	0.04	0.00
Mother_university*Father_compulsory	0.02	0.02	0.02	-0.03	0.00
Mother_university*Father_university	0.03	0.03	0.03	0.01	0.00
Mother_upper_secondary*Father_compulsory	0.12	0.07	0.12	0.15	0.00
Mother_upper_secondary*Father_university	0.12	0.14	0.12	-0.06	0.00
Mother_compulsory*Mother_year_of_birth	0.05	0.05	0.05	0.03	0.00
Mother_compulsory*Father_year_of_birth	724.14	757.78	724.22	-0.04	0.00
Mother_compulsory*Year_of_birth	722.72	756.66	722.80	-0.04	0.00
Mother_compulsory*Mother_foreign	0.00	0.01	0.00	0.00	0.00
Mother_compulsory *Age_at_child_birth*year_of_birth	9.73	10.53	9.73	-0.06	0.00
Mother_university*Mother_year_of_birth	19147.25	20729.91	19149.77	-0.06	0.00
Mother_university*Father_year_of_birth	429.14	343.24	429.09	0.11	0.00
Mother_university*year_of_birth	428.39	342.81	428.34	0.11	0.00
Mother_university*Mother_foreign	0.00	0.00	0.00	-0.05	0.00
Mother_university*age_at_child_birth*year_of_birth	5.87	4.83	5.87	0.09	0.00
Father_compulsory*Father_year_of_birth	11553.84	9504.06	11552.55	0.09	0.00
Father_compulsory*Mother_year_of_birth	795.83	787.64	795.85	0.01	0.00
Father_compulsory*year_of_birth	797.50	789.08	797.52	0.01	0.00
Father_compulsory*Father_foreign	808.25	799.80	808.27	0.01	0.00
Father_university*Father_year_of_birth	0.01	0.02	0.01	-0.08	0.00
Father_university*Mother_year_of_birth	378.87	293.38	378.90	0.11	0.00
Father_university*year_of_birth	379.43	293.70	379.46	0.11	0.00
Father_university*Father_foreign	384.74	297.84	384.77	0.11	0.00
Mother_foreign*Father_foreign	0.00	0.02	0.00	-0.42	0.00

[a]sdiff refers to standardized differences in means. These are defined as the difference between the means in the two groups as a percentage share of the square root of the average variance in the two groups.

Table B.1: Estimated earnings differences (OLS), results for mediators

	Women		Men	
	(1)	(2)	(3)	(4)
Compulsory	-0.0925**	-0.106***	-0.0617**	-0.0580**
	(0.0388)	(0.0369)	(0.0244)	(0.0247)
University	0.183***	0.167***	0.184***	0.177***
	(0.0191)	(0.0186)	(0.0204)	(0.0199)
Married		-0.000423		0.106***
		(0.0179)		(0.0181)
Divorced		0.0366		0.0433
		(0.0310)		(0.0376)
Widow(er)		-0.330***		-0.221***
		(0.0499)		(0.0476)
Child(ren)		-0.164***		0.0695***
		(0.0164)		(0.0157)
Year_FE	Yes	Yes	Yes	Yes
Observations	47577	47577	55319	55319
Individuals				
R2	0.317	0.332	0.372	0.384

Robust (clustered) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Year_FE indicates controls for year-fixed effects.

Table C.1: Log(Earnings) and the probability of having university education, having child(ren), and being married at age 30

	Sisters				Brothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log (Earnings)	University	Child(ren)	Married	Log (Earnings)	University	Child(ren)	Married
Sibling	0.0311	-0.0365	0.0459	-0.0221	-0.0519*	-0.00269	-0.0102	0.0275
	(0.0332)	(0.0317)	(0.0318)	(0.0311)	(0.0296)	(0.0275)	(0.0256)	(0.0264)
Year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	2206	2206	2206	2206	2682	2682	2682	2682
R2	0.0516	0.0108	0.0260	0.0104	0.0444	0.0115	0.0211	0.0271

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Year_FE indicates controls for year-fixed effects.

Table D.1: Age-specific estimates of the probability of having earnings>1PBA

	Sisters		Brothers	
	OLS	FE	OLS	FE
	(1)	(2)	(3)	(4)
Sibling	0.00771		0.00921	
	(0.0199)		(0.0205)	
*Sibling*26-30*	0.0173	0.00864	0.0134	0.0189
	(0.0209)	(0.0208)	(0.0213)	(0.0208)
*Sibling*31-35*	-0.0208	-0.0280	0.00765	0.0140
	(0.0229)	(0.0228)	(0.0230)	(0.0226)
*Sibling*36-40*	-0.0473*	-0.0586**	0.0245	0.0323
	(0.0242)	(0.0239)	(0.0231)	(0.0222)
*Sibling*41-45*	-0.0142	-0.0354	-0.00485	0.0102
	(0.0287)	(0.0264)	(0.0280)	(0.0250)
*Sibling*46-48*	-0.0151	-0.0462	-0.0180	-0.0183
	(0.0596)	(0.0633)	(0.0700)	(0.0493)
Year FE	Yes	Yes	Yes	Yes
Observations	62522	62522	65713	65713
Individuals		3054		3216
R2	0.0270	0.0321	0.0336	0.0460

Robust (clustered) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Year FE indicates controls for year-fixed effects.

Table E.1: Estimated earnings differences (FE), using different thresholds

	Sisters		Brothers	
	(1)	(2)	(3)	(4)
	Full sample	Earnings>100'	Full sample	Earnings>100'
*Sibling*26-30*	-0.0522	-0.00250	0.0984	-0.0264
	(0.107)	(0.0190)	(0.101)	(0.0162)
*Sibling*31-35*	-0.176	0.000167	0.0271	-0.0373*
	(0.127)	(0.0215)	(0.117)	(0.0197)
*Sibling*36-40*	-0.295**	-0.00388	0.136	-0.0299
	(0.134)	(0.0232)	(0.116)	(0.0218)
*Sibling*41-45*	-0.234	-0.00252	-0.0119	-0.0565**
	(0.158)	(0.0267)	(0.149)	(0.0256)
*Sibling*46-48*	-0.302	-0.0493	-0.0572	-0.0579
	(0.425)	(0.0506)	(0.293)	(0.0508)
Year_FE	Yes	Yes	Yes	Yes
Observations	62522	36876	65713	48857
Individuals	3054	2884	3216	3098
R2	0.0613	0.535	0.0950	0.604

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Year_FE indicates controls for year-fixed effects.

Table F.1: Estimated earnings differences using PS weighting, brothers

	(1)	(2)
	OLS	FE
Sibling	0.0177	
	(0.0266)	
*Sibling*26-30*	-0.0254	-0.0260
	(0.0302)	(0.0317)
*Sibling*31-35*	-0.0253	-0.0371
	(0.0338)	(0.0357)
*Sibling*36-40*	-0.0791**	-0.0843***
	(0.0318)	(0.0313)
*Sibling*41-45*	-0.0409	-0.0843**
	(0.0396)	(0.0356)
*Sibling*46-48*	0.0791	-0.0960
	(0.0884)	(0.0671)
Year_FE	Yes	Yes
Observations	55319	55319
Individuals		3154
R2	0.364	0.497

Robust (clustered) standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1. Year_FE indicates controls for year-fixed effects.

Figure Captions

Figure 1: Covariate balance (standardized differences in means) for all moment conditions before (gray bars) and after (black bars) entropy balance (EB) weighting for brothers and controls.

Figures

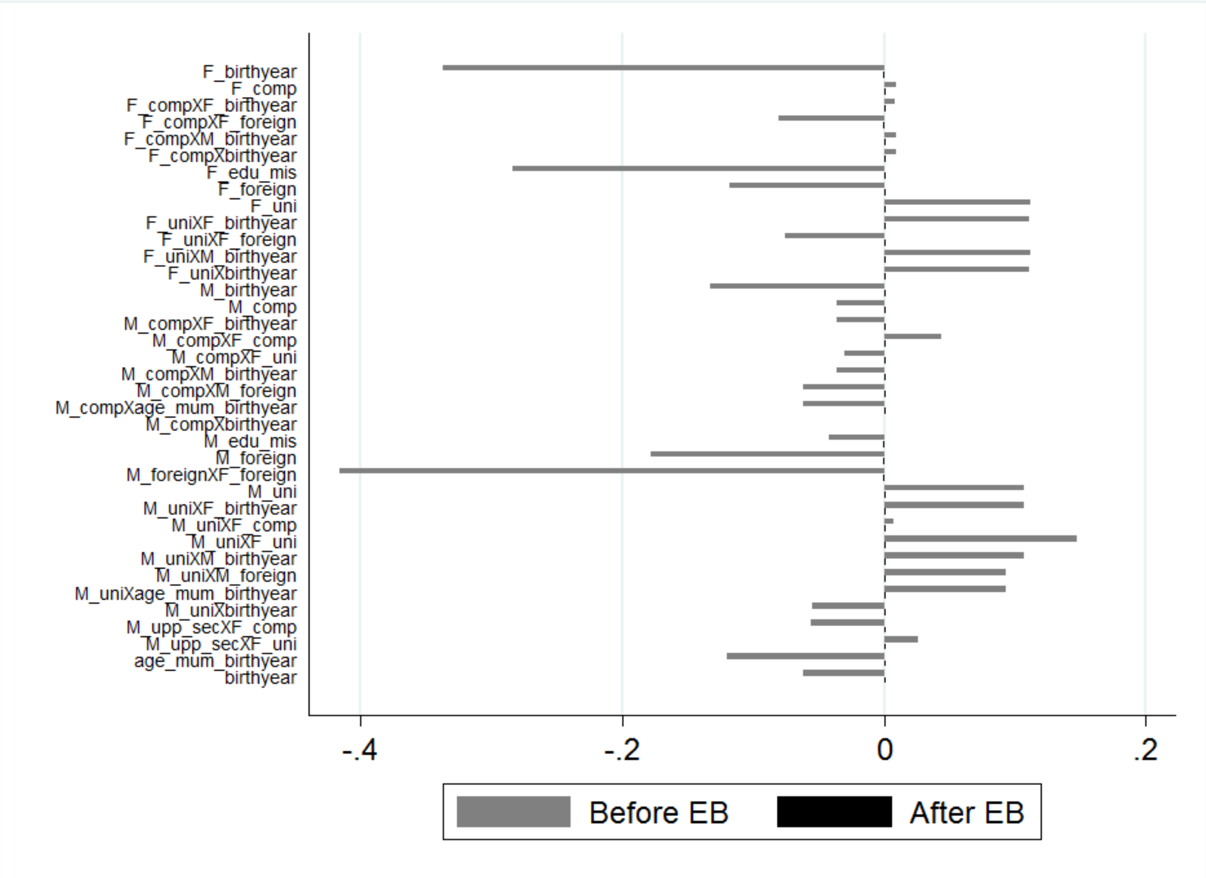
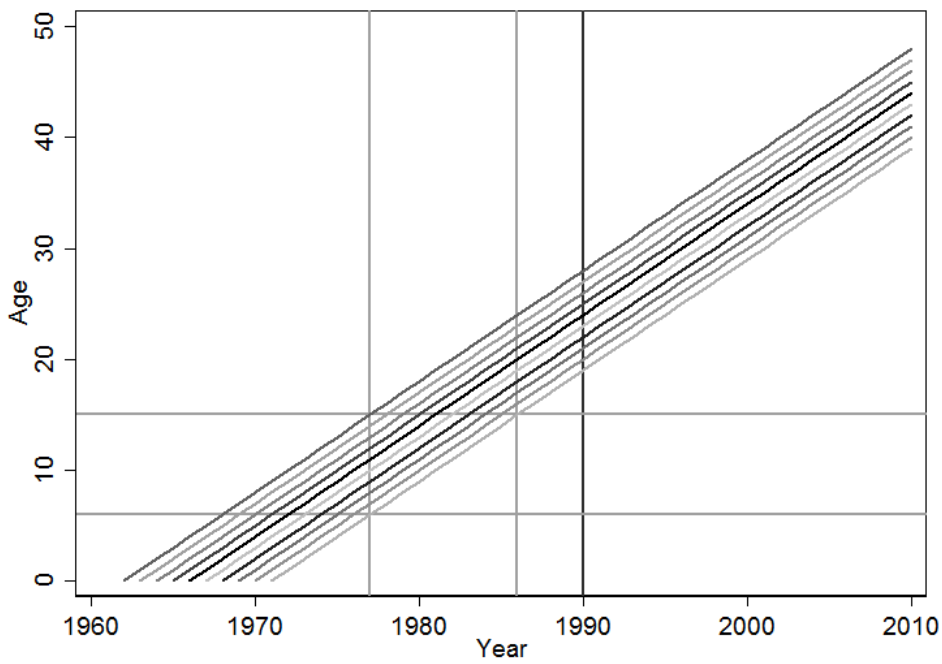


Figure 1: Covariate balance (standardized differences in means) for all moment conditions before (gray bars) and after (black bars) entropy balance (EB) weighting for brothers and controls.



Supplementary Figure A: The age of each cohort from birth to year 2010. Each line represents a cohort: The cohort born 1962 is the top line, …, the cohort born 1971 is the bottom line. The gray horizontal lines (ages 6 and 15) mark the lower and upper bounds for onset ages. The gray vertical lines (years 1977 and 1986) mark the bounds for year of diagnosis. The black vertical line (year 1990) marks the first year with earnings data