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

Swedish Childhood Diabetes Study Group

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1 Abstract

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

16

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

18

19 1. Introduction

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

34

35 Type 1-diabetes (hereafter referred to as diabetes) is a disease with a sudden onset and well-
36 documented consequences on everyday life and future health. Parents describe onset as a time of
37 crisis, as learning the child's daily management routines and accommodating family routines to the
38 requirements imposed by the disease generally poses a great challenge to the entire family (Wennick
39 & Hallström, 2006). Managing the diabetes is complex and time-consuming, involving several daily
40 insulin injections and blood glucose check-ups, exercise, and dietary restrictions. The imposed focus
41 on a healthier lifestyle with daily routines could have positive effects, fostering children to become
42 more responsible adults. Moreover, caring for a child with diabetes imposes a host of long-term
43 stressors (e.g., fear of hypoglycemia and increased insecurity about future health) (Sparud-Lundin et

44 al., 2013). Therefore, the time and effort needed to manage the disease and the stress and
45 insecurities that follows are likely to affect the entire family, even though some children with
46 diabetes are relatively healthy during long periods.

47

48 Several studies find that childhood onset of diabetes has adverse educational and labor market
49 consequences for the affected individual (see, e.g., Dahlquist et al., 2007; Fletcher & Richards, 2012;
50 Lundborg et al., 2014; Minor, 2011, 2013; Persson et al., 2013; Steen Carlsson et al., 2010).
51 Moreover, child health shocks have been shown to strain family resources (e.g., by reducing parents'
52 working hours (Kvist et al., 2013)), change intra-household resource allocation (e.g., parents
53 compensate for or reinforce differences in child endowments (Almond & Mazumder, 2013; Currie &
54 Almond, 2011)), and affect the quality of interaction among family members (Heckman & Mosso,
55 2014).

56

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

69

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

79

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

92

93 Using detailed Swedish longitudinal registers, covering the years 1990-2010, I examine the
94 progression of annual earnings for individuals with a sibling that was diagnosed with diabetes in age
95 6-15. Following the earnings trajectories of individuals aged 19-48 during this time period (hence
96 born in 1962-1971), I take an exploratory approach to assess the potential influence of both
97 unobservable individual-specific factors and mediator variables related to education and family
98 formation.

99

100 Psychological research in diabetes and child health suggests that siblings of chronically ill children
101 have contradictory feelings towards their sick brother or sister: a strong sense of responsibility (e.g.,
102 acting as protector and caregiver); resentment (e.g., being jealous of the sick sibling receiving extra
103 attention); exaggerated sibling rivalry (e.g., fighting for parents' attention); and social and emotional
104 isolation (e.g., being afraid to increase their parents' worries and evoke their anger by showing
105 negative feelings for, or fail to protect, their sick sibling) (Wennick and Huus, 2012; O'Brien et al.,
106 2009).

107

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

115

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

124 2. Data

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

137

138 The SCDR comprises 2,551 non-diabetic siblings of children who were born in 1962-1971 and
139 diagnosed with diabetes between 1977-1986. I excluded 527 siblings born before 1962 and 846

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

153

154 Because siblings are often born with only a few years apart and both the children with diabetes and
155 their siblings were relatively old (between ages 6-15) at the time of diagnosis, the sample is unlikely
156 to be skewed by the possibility of the onset of diabetes affecting parents' fertility choices. The
157 siblings were on average 11.9 (standard deviation 2.4) and the mothers were 38.6 years old
158 (standard deviation 5.7) at the time of diagnosis. In the late 1960s, Swedish women were on average
159 26.4 years old at childbirth (all births) and having children after age 40 was uncommon (less than
160 four out of 1000 births) (Statistics Sweden, 2014). Moreover, the number of siblings of the diabetic
161 children is similar to that of the general population: the average number of children per woman was
162 2.13 in 1962 and 1.96 in 1971 (Statistics Sweden, 2014), while the families with a diabetic child in
163 SCDR have on average 1.96 children when including families without siblings and 2.4 children in the
164 studied sample. 47.0% of the siblings of diabetic children are same-sex siblings and 56.8% are older

165 than the child with diabetes. 45.8% of the sibling pairs are born within two years, and 94.9% are born
166 within five years of each other.

167

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

171

172 Following earnings in the years 1990-2010, the dataset is a panel with 128,235 observations and an
173 average of 20.5 observations per individual. The timeline of the sample and how each cohort
174 contributes to the studied panel are shown in Supplementary Figure A [INSERT LINK TO ONLINE FILE
175 A]. In order to exclude individuals with only short-term (holiday) jobs, I restrict the sample to the
176 years that each individual is part of the labor force, defined as having annual earnings exceeding one
177 price base amount (PBA), as did Lundborg et al. (2015). Fifteen siblings (18.1% of the sibling-year
178 observations) and 127 controls (20.0%) fall below this threshold. The PBA is a measure based on
179 changes in the general price level and is set by the Swedish government. It increased from SEK
180 29,700 (\approx EUR 2,970) to SEK 42,400 (\approx EUR 4,240) over the study period.

181

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

187

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

199 3. Method

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

209

210 3.1 Entropy balancing

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

220

221 By including interaction terms of the background variables, covariates are balanced across subsample
222 groups, such as individuals with two university educated parents. The standardized differences in
223 means of the variables are a quality measure for the matching process. Figure 1 shows that these
224 differences ranged from -0.42 to 0.15 before EB and are reduced to zero afterwards, confirming that
225 EB has improved balance for *all* conditioning variables (see also Table A.1 showing the mean of the
226 conditioning variables before and after EB). I use the Ebalance package for Stata (Hainmueller & Xu,
227 2013).

228 3.2 Regression analysis

229 The effects of child health on adult earnings are likely to vary over time. Whereas Heckman et al.
230 (2006) suggest that early health shocks accumulate over time so that even small health shocks could
231 lead to adverse adult outcomes, Grossman (1972a,b) instead predicts that the effects of health
232 shocks diminish over time. It is possible that spillover effects on siblings exhibit a similar behavior. To
233 account for this possibility, I assess age-specific differences in earnings between siblings and
234 weighted controls. I use the following specification for individual i in year t :

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

235

236 The dependent variable, y_{it} , is the natural logarithm of annual labor earnings for individual i during
 237 year t , conditional on earnings > 1PBA. This variable comprises all (gross) earnings from employment
 238 and self-employment reported to the Swedish Tax Agency. D_i is a dummy variable which takes on the
 239 value one for siblings. AGE_{it} is a vector of dummy variables representing the age categories 26-30,
 240 31-35, 36-40, 41-45, and 46-48 years. The reference category is 19-25. The interaction terms,
 241 $D_i * AGE_{it}$, capture age-specific differences in average annual earnings (in percentage points)
 242 between the siblings and the weighted controls. The coefficient of D_i shows how much the average
 243 annual earnings differ between the siblings group and the weighted controls at age 19-25, and the
 244 coefficient on, for example, the second interaction term, $D_i * AGE_{i2}$, displays the additional difference
 245 between the groups at age 26-30. λ_t is a vector of year dummies (1990-2010), which control for
 246 aggregate changes in the economy over time and ε_{it} is an idiosyncratic error term.

247

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

248

249 The second specification adds X_{it} , which is a vector of observable potential mediators, including
 250 education (compulsory, upper secondary, or university) and indicator variables for having children
 251 and marital status (married, divorced, and widow(er)). These variables are added to test whether

252 they are channeling the studied relationship by absorbing some of the effect of the sibling spillovers.
 253 If so, the coefficients of the mediator variables may not capture their causal effect and, more
 254 importantly, including these variables could bias the sibling estimates. Therefore, the results from
 255 equation (2) should not be interpreted as causal effects.

$$y_{it} = \alpha + \beta D_i + \sum_{Age} \gamma_{age} AGE_{it} + \sum_{Age} \delta_{age} D_i * AGE_{it} + \lambda_t + \mu_i + \varepsilon_{it} \quad 256 \quad (3)$$

257

258

259

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

263

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

274

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

280 4. Results

281

282 4.1 Earnings differences

283 Table 2 displays estimates of age-specific sibling spillovers. Appendix B shows results for mediators.
284 The results do not indicate any significant spillover effects for sisters, while brothers' earnings appear
285 affected when using the FE estimator (column 8). The FE estimates for brothers are significantly
286 lower for all age categories compared to those of the weighted controls (except for ages 46-48,
287 probably due to the small number of observations in this category). These differences increase with
288 age, from 4.8 percentage points lower earnings growth than the weighted controls at age 26-30 to
289 7.6 percentage points at age 41-45.

290

291 The OLS estimates show no statistically significant difference in earnings between siblings and
292 weighted controls. The positive (but insignificant) estimate for brothers in the reference ages could
293 suggest higher initial earnings, but the positive coefficient turns negative at older ages. This is likely
294 to be the case if brothers, as a group, are less educated and therefore enter the labor market earlier
295 than weighted controls. However, educational level does not appear to be a channel through which
296 sibling spillovers affect earnings. Nor is marital status or having one or more children, as the
297 estimates appear robust across specifications, with only small deviations in size when adding
298 observable potential mediators. Appendix C, assessing potential mediators as outcome variables,

309 shows no significant differences between siblings and weighted controls regarding their probabilities
300 of having a university education, having children, and being married at age 30. These findings give us
301 no reason to believe that the positive (but insignificant) estimate for brothers in the reference ages
302 relates to level differences in earnings capacity.

303

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

310

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

322

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

330

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

342

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

347

348 4.2 Sensitivity analysis

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

360

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

367

368 Testing the results sensitivity to the 1PBA threshold, Table E.1 presents unconditional estimates and
369 estimates conditional on having annual earnings above SEK 100,000. This higher threshold has
370 previously been shown to yield results similar to those of hourly wage when studying the returns to

371 education in Sweden (Antelius & Björklund, 2000). These results confirm the FE results, but the size
372 of the estimates is sensitive to the chosen threshold, suggesting that spillovers influence earnings via
373 both wages and labor supply. Therefore, we need to be aware that the estimates are conditional on
374 earnings > 1PBA when interpreting the results. Finally, Appendix F presents results for brothers and
375 controls that is matched using propensity score weighting rather than entropy balancing. The results
376 are similar to those obtained by the entropy balancing method.

377 5. Discussion

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

383

384 The negative net-effect of sibling spillovers on brothers' earnings increases over time, from 4.8
385 percentage points lower earnings growth than peers at ages 26-30 to 7.6 percentage points at ages
386 41-45. This finding is likely an upper bound for the effect, as this study focuses on the siblings that
387 are the closest in age, and thereby likely to be the most exposed to their sibling's disease. The results
388 are insensitive to controlling for educational level and family formation, but selection into different
389 occupations or fields of education might still be an important explanation driving the results. I leave
390 this to future research to determine.

391

392 There are two major strengths of this study. First, diabetes is an interesting case to study as spillovers
393 between siblings could potentially arise from and affect many aspects of life, as diabetes has diverse

394 and well-documented impacts on the entire family (Wennick & Hallström, 2006). Second, this study
395 tests whether individual-specific factors appear to influence if sibling spillovers affect earnings. The
396 results indicate that such factors are influential and suggest further research to disentangle the
397 mechanisms behind their importance. It is possible that individual-specific factors moderate the
398 relationship, so that individuals with different inherited abilities respond differently to having a
399 sibling with diabetes.

400

401 The reason behind the different findings for sisters and brothers remains to be explained. Potential
402 explanations could relate to differences in childhood adjustment strategies. Boys have been shown
403 to adopt externalizing behaviors, which have in turn been linked to adverse educational and labor
404 market outcomes (Gregg & Machin, 2000; McLeod & Kaiser, 2004; Miech et al., 1999). Conversely,
405 girls more often show internalizing symptoms, which have been reported as less important for future
406 outcomes (McLeod & Kaiser, 2004; Miech et al., 1999). Another potential explanation could relate to
407 differences in peer-effects and parent-child interactions between boys and girl. Previous results
408 underline the importance of family interactions for early-life medical interventions (Breining et al.
409 2015) and developmental disabilities (Fletcher et al. (2012), suggesting that this type of potential
410 differences may be an important channel of the gender differences in spillover effects. However,
411 converse to the results of this study, Fletcher et al. (2012) show that girls are more negatively
412 affected than boys by sibling spillovers from developmental disabilities. They explain their finding by
413 women experiencing greater intimacy in the sibling relationship. This explanation may be an
414 important explanation of the robustness of women's earnings to exposure to a diabetic sibling, as
415 greater intimacy could counteract an exaggerated sibling rivalry and feelings of social and emotional
416 isolation that are often reported by the psychological research in diabetes and child health (Wennick
417 and Huus, 2012; O'Brien et al., 2009).

418

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

433

434 This study underlines the importance of considering all family members when studying the
435 consequences of childhood onset of chronic illness. These findings for siblings of individuals with
436 diabetes suggest the importance of actions acknowledging a broader family impact when initiating
437 further research and support children's diabetes management programs that target also siblings.
438 However, the shown difference in spillovers for brothers and sisters indicates that spillovers from
439 one family member to another might differ within a family. More research is needed to further
440 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 mean	Women controls mean	t-test p-value	Brothers mean	Men controls mean	t-test 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
<i>Mothers</i>						
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
<i>Fathers</i>						
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	

^aSEK 2010 prices (SEK 10 ≈ EUR 1)

^bChi-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 (1)	OLS (2)	OLS (3)	FE (4)	OLS (5)	OLS (6)	OLS (7)	FE (8)
<i>Sibling</i>	-0.0130 (0.0238)	-0.0113 (0.0245)	-0.0125 (0.0240)		0.0283 (0.0232)	0.0314 (0.0236)	0.0296 (0.0237)	
<i>Sibling*26-30</i>	-0.00164 (0.0283)	0.00123 (0.0281)	0.00861 (0.0275)	-0.00622 (0.0291)	-0.0403 (0.0250)	-0.0431 (0.0246)	-0.0422 (0.0246)	-0.0480* (0.0257)
<i>Sibling*31-35</i>	0.0275 (0.0331)	0.0332 (0.0330)	0.0324 (0.0324)	0.0180 (0.0331)	-0.0312 (0.0302)	-0.0360 (0.0295)	-0.0351 (0.0292)	-0.0515* (0.0310)
<i>Sibling*36-40</i>	0.0232 (0.0351)	0.0305 (0.0349)	0.0285 (0.0345)	-0.00173 (0.0344)	-0.0525 (0.0337)	-0.0566* (0.0327)	-0.0514 (0.0320)	-0.0638* (0.0333)
<i>Sibling*41-45</i>	0.0246 (0.0397)	0.0308 (0.0395)	0.0265 (0.0394)	-0.00517 (0.0366)	-0.0313 (0.0410)	-0.0426 (0.0397)	-0.0372 (0.0384)	-0.0760** (0.0365)
<i>Sibling*46-48</i>	0.0358 (0.0763)	0.0262 (0.0773)	0.0166 (0.0796)	-0.00377 (0.0681)	0.0575 (0.105)	0.0486 (0.0966)	0.0576 (0.0939)	-0.106 (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

^asdiff 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** (0.0388)	-0.106*** (0.0369)	-0.0617** (0.0244)	-0.0580** (0.0247)
University	0.183*** (0.0191)	0.167*** (0.0186)	0.184*** (0.0204)	0.177*** (0.0199)
Married		-0.000423 (0.0179)		0.106*** (0.0181)
Divorced		0.0366 (0.0310)		0.0433 (0.0376)
Widow(er)		-0.330*** (0.0499)		-0.221*** (0.0476)
Child(ren)		-0.164*** (0.0164)		0.0695*** (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) Log (Earnings)	(2) University	(3) Child(ren)	(4) Married	(5) Log (Earnings)	(6) University	(7) Child(ren)	(8) Married
<i>Sibling</i>	0.0311 (0.0332)	-0.0365 (0.0317)	0.0459 (0.0318)	-0.0221 (0.0311)	-0.0519* (0.0296)	-0.00269 (0.0275)	-0.0102 (0.0256)	0.0275 (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)
<i>Sibling</i>	0.00771 (0.0199)		0.00921 (0.0205)	
<i>Sibling*26-30</i>	0.0173 (0.0209)	0.00864 (0.0208)	0.0134 (0.0213)	0.0189 (0.0208)
<i>Sibling*31-35</i>	-0.0208 (0.0229)	-0.0280 (0.0228)	0.00765 (0.0230)	0.0140 (0.0226)
<i>Sibling*36-40</i>	-0.0473* (0.0242)	-0.0586** (0.0239)	0.0245 (0.0231)	0.0323 (0.0222)
<i>Sibling*41-45</i>	-0.0142 (0.0287)	-0.0354 (0.0264)	-0.00485 (0.0280)	0.0102 (0.0250)
<i>Sibling*46-48</i>	-0.0151 (0.0596)	-0.0462 (0.0633)	-0.0180 (0.0700)	-0.0183 (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'
<i>Sibling*26-30</i>	-0.0522 (0.107)	-0.00250 (0.0190)	0.0984 (0.101)	-0.0264 (0.0162)
<i>Sibling*31-35</i>	-0.176 (0.127)	0.000167 (0.0215)	0.0271 (0.117)	-0.0373* (0.0197)
<i>Sibling*36-40</i>	-0.295** (0.134)	-0.00388 (0.0232)	0.136 (0.116)	-0.0299 (0.0218)
<i>Sibling*41-45</i>	-0.234 (0.158)	-0.00252 (0.0267)	-0.0119 (0.149)	-0.0565** (0.0256)
<i>Sibling*46-48</i>	-0.302 (0.425)	-0.0493 (0.0506)	-0.0572 (0.293)	-0.0579 (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
<i>Sibling</i>	0.0177 (0.0266)	
<i>Sibling*26-30</i>	-0.0254 (0.0302)	-0.0260 (0.0317)
<i>Sibling*31-35</i>	-0.0253 (0.0338)	-0.0371 (0.0357)
<i>Sibling*36-40</i>	-0.0791** (0.0318)	-0.0843*** (0.0313)
<i>Sibling*41-45</i>	-0.0409 (0.0396)	-0.0843** (0.0356)
<i>Sibling*46-48</i>	0.0791 (0.0884)	-0.0960 (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

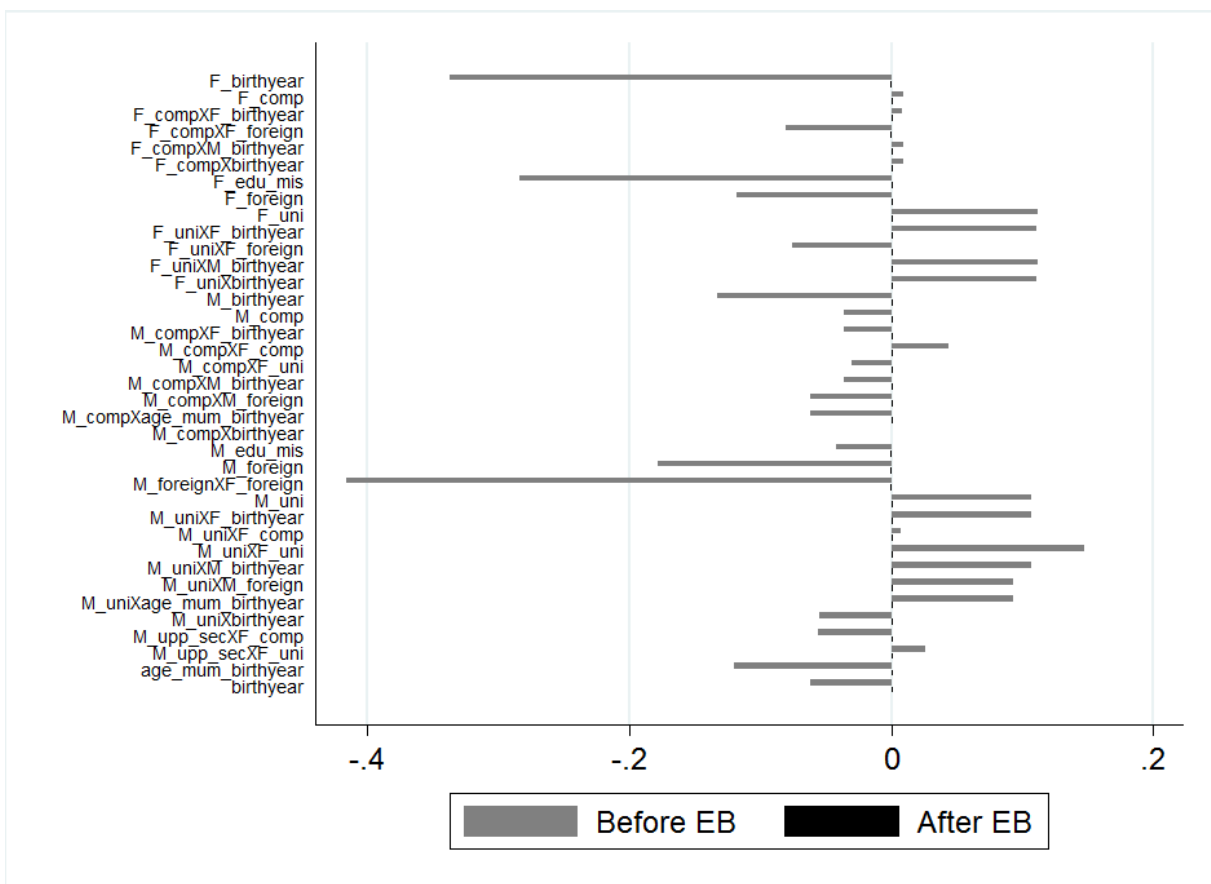
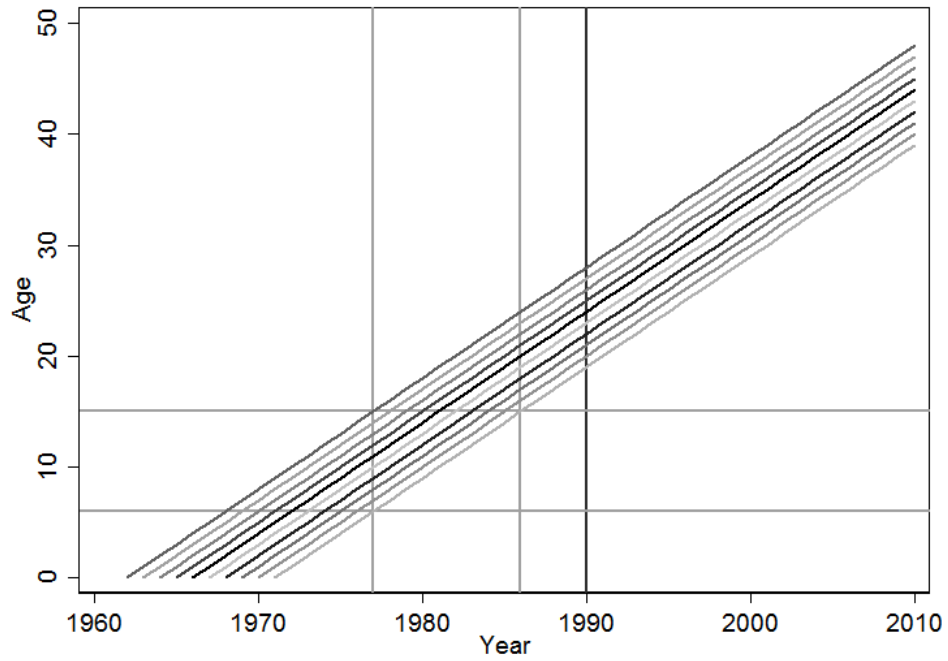


Figure 1: Covariate balance (standardized differences in means) for all moment conditions before (gray bars) and after (black bars) entropy balance (EB) weighting for brothers and controls.



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