The Roots of Health Inequality and The Value of Intra-Family Expertise*

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Abstract

In the context of Sweden, we show that having a doctor in the family raises preventive health investments throughout the lifecycle, improves physical health, and prolongs life. Two quasi-experimental research designs – medical school admissions lotteries and variation in the timing of medical degrees – support a causal interpretation of these effects. A hypothetical policy that would bring the same health behavior changes and benefits to all Swedes would close 18 percent of the mortality-income gradient. Our results suggest that socioeconomic differences in exposure to health-related expertise may meaningfully contribute to health inequality.

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1 Introduction

Poorer people have worse health at birth, are sicker in adulthood, and die younger than richer people (see, e.g., Marmot et al. 1991, Case et al. 2002, Deaton 2002, Currie 2009, and Lleras-Muney 2018). The causal links driving these associations are the subject of significant academic and policy interest. Prominent explanations include socioeconomic differences in health at birth and in access to health care, as well as incomplete insurance of income losses in response to health shocks.[1] In addition, socioeconomic differences in health literacy – "the degree to which individuals have the capacity to obtain, process and understand basic health information and services needed to make appropriate health decisions" (Parker and Ratzan, 2000) – have been hypothesized to be one critical yet a less-examined mechanism driving health inequality (Nutbeam and Kickbusch, 2000; Saha, 2006; Sentell and Halpin, 2006; Volandes and Paasche-Orlow, 2007; Tang et al., 2019). If health literacy improves health investments, or the use of the health care system, then differential health literacy across the socioeconomic spectrum contributes to health inequality.[2]

In this paper, we estimate the causal impact of having a health professional in the family on health behaviors and health outcomes. Exposure to a health expert in the family is a natural measure of variation in health literacy. The health expert can provide information about appropriate treatment, raise family members' perceived value of beneficial health investments, build trust in the health care system, or use clout to get a person better or more timely care. We focus on outcomes related to the prevalence of lifestyle-related conditions and preventive health investments. Unlike the benefits of exposure to expertise that rely on the expert's professional clout, these outcomes are non-rival, making them more relevant from a policy perspective. Quantifying the impact of exposure to a health expert allows us to speculate about the potential role of differential health literacy, more broadly, in sustaining health inequality.

We use Swedish administrative population-wide health records, tax records, and family tree linkages for our analysis. These data, described in Section 2, allow us to identify health professionals, link them to their family members, and track these family members' health as well as socioeconomic status (SES). Beyond the availability of data, Sweden is a particularly attractive empirical context because its universal health insurance system allows us to shut down one often-hypothesized driver of health inequality: inequality in formal access to health care. Given this, we start by briefly examining whether there is any health-SES gradient left in the setting that we study. Despite Sweden's universal health insurance and extensive social safety net, we document substantial health inequality, across the life cycle. In fact, at the end of life, health inequality is as pronounced in Sweden as it is in the United States.[3] This underscores the importance of studying drivers of health inequality that go beyond the supply-side health insurance and health care access channels.[4]

[1]See, e.g., Currie (2011), Aizer and Currie (2014), and Persson and Rossin-Slater (2018) for evidence on how early-life health disparities driven by differential *in utero* conditions or genetic capital may perpetuate economic inequality, and Black et al. (2007), Oreopoulos et al. (2008), Almond and Mazumder (2011), and Bharadwaj et al. (2018a,b) for more evidence on the causal relationship between early-life health and future economic or health outcomes. Also see Adler et al. (1994) for a review of early evidence of a socioeconomic gradient in mortality across different countries and a discussion of possible drivers.

[2]It is well established that many routine health behaviors such as smoking, exercise, eating habits, and vaccinations display sharp gradients. See, e.g., Rehm et al. (2016), Hiscock et al. (2012), and Ogden et al. (2010).

[3]Data from the United States used in this comparison is reported by The Health Inequality Project. Also see Sjögren and Hartman (2018) for an analysis of how mortality inequality has evolved over time in Sweden.

[4]The idea of considering factors other than formal access to health care is consistent with the largely mixed findings of a voluminous

In Section 3, we examine whether having a doctor or nurse in the family is associated with improved health and health behaviors across the life cycle. We begin by comparing individuals with and without a doctor or nurse in the family in the raw data. Conditional on individual income rank at age 55, individuals with a doctor or nurse in the family are more likely to survive until age 80 and less likely to suffer from chronic lifestyle-related conditions. Further, children are substantially more likely to have undertaken a preventive investment that we observe – HPV vaccination – and less likely to have been exposed to tobacco *in utero*. These patterns remain economically and statistically significant when we flexibly control for a wide range of observable demographics.

Comparing demographically equivalent individuals with and without a health professional in the extended family may still yield a biased estimate of the effect of exposure to intra-family expertise if *un*observables are correlated with this exposure. To assuage this concern, we pursue two quasi-experimental approaches. First, we leverage the fact that some admissions to medical school in Sweden were adjudicated by lottery. We use data on health outcomes and health behaviors of 7,247 family members of 743 first-time medical school applicants from 2007 through 2010 to compare health outcomes and health behaviors between families who won and lost the admissions lottery. Our results from this lottery analysis are consistent with our descriptive findings and show far-reaching health benefits for the admitted applicants' extended family. Among older relatives, having a doctor in the family reduces the occurrence of lifestyle-related diseases and improves preventive care. For example, eight years after the applicant's matriculation, older relatives are 4 and 5 percentage points less likely to have had a heart attack and heart failure, respectively, and are 27 percent more likely to take medication that can prevent heart attacks conditional on needing such drugs. Among younger relatives, having a doctor in the family also raises preventive investments—for example, increasing the probability of HPV vaccination by 22 percentage points.

While the medical school lottery resembles an ideal experiment, this design only permits a relatively short follow-up period, as the lotteries were recent. This precludes studying outcomes such as mortality and the gradual onset of some lifestyle-related chronic conditions, as the parents of medical school applicants are relatively young (while grandparents are frequently already deceased). We therefore complement this analysis with a second quasi-experimental approach: event studies that compare individuals' health before and after their (often younger) family member receives either a medical degree or a law degree.[5] We find striking differences in the health and mortality profiles of the two groups in the raw data, and our results are confirmed in a rich regression specification that reveals no differential trends in health outcomes predating the arrival of a health professional or lawyer in the family. "Getting" a doctor in the family yields a 10 percent reduction in mortality 25 years after the doctor's matriculation, along with substantially lower rates of heart attacks, heart failure, diabetes, and lung cancer. These effects emerge gradually, which points to improved health investments over a long time period.

There are two main interpretations of our results, which we discuss in Section 4.1. One is that having a doctor in the family delivers benefits that are intrinsically scarce. A more health literate individual may have a

literature (mostly in US settings) that has investigated the causal effect of health insurance – which lowers the price and ease of access to formal health care – on long- and short-run health outcomes (see, e.g., Sommers et al. 2017, and Finkelstein et al. 2018).

[5] The comparison of morbidity and mortality profiles at older ages between parents of doctors and lawyers is motivated by the fact that doctors and lawyers are both high-social status professions with similar income distributions; we verify that the parents of lawyers and doctors also have similar income distributions in our data.

better understanding of how to use the health care system, which may translate into getting faster care or seeing more knowledgeable specialists, for example. If so, the health benefits enjoyed by doctors' family members may come at the expense of others in society; thus, under this interpretation, our results may identify a deeply rooted source of inequality. A second interpretation is that having a doctor in the family delivers health behavior changes and health benefits that are *not* intrinsically scarce. If doctors' get their family members to undertake health behavior changes that are beneficial for health but "cheap" for society, then these benefits could, in principle, be delivered to everyone. Under this interpretation, our results suggest that there may be an important opportunity to improve population health and reduce health inequality through public policies that mimic what doctors do to their family members.

While the data does not allow us to perfectly adjudicate between the two interpretations, our results clearly establish impacts on outcomes that are *not* intrinsically scarce. For example, doctors accomplish an increase in their relatives' take-up of vaccines and use of preventive drugs that are cheap and readily available, a reduction in tobacco exposure *in utero*, and a reduction in lifestyle-related chronic diseases. Such benefits to health literacy do not come at the expense of others in society and are, at least conceptually, readily scalable.

In the final part of the paper, we use our estimates of the effect of having a doctor in the family to construct a counterfactual that informs a more speculative discussion of the potential role of differential health literacy, more broadly, in sustaining health inequality. We show that a hypothetical policy[6] that could bring the health benefits associated with access to an expert to everyone in society could close 18 percent of the mortality-income gradient. Our results thus suggest that socioeconomic differences in exposure to health-related expertise may meaningfully contribute to health inequality.

Our work builds on and contributes to several strands of the literature. While a broad literature studies the importance of the family as a source of insurance (see, e.g., Lee and Persson 2016, Autor et al. 2019, and Persson 2020) or shocks (e.g., Persson and Rossin-Slater 2018), a smaller body of work examines the importance of the family as a nexus for transmission of expertise, information, salience, and norms. For example, Fadlon and Nielsen (2019) study how individuals respond to a family member experiencing non-fatal heart attacks or strokes, finding that spouses and adult children increase their consumption of preventive care (cholesterol-lowering medication) in response.[7] We instead focus on familial transfers of health-related expertise.

Our focus on health-related expertise relates the paper to the emerging literature on the impact of information and expertise on health behaviors. One strand of this literature compares the behaviors of physician-patients to other patients. Johnson and Rehavi (2016) show that female physicians are less likely to receive a C-section when they themselves give birth. Using randomization in medical school admissions in the Netherlands, Leuven et al. (2013) further find that being a doctor leads to small improvements in self-reported health, but a decline in physical exercise. Frakes et al. (2021) find no meaningful differences in the use of low-value care between regular patients and physicians as patients, while Janssen (2020) finds that individuals with a medical degree have lower

[6] While one such hypothetical policy could be to expand the number of health professionals, this is likely not implementable in practice. We turn to a discussion of how potentially implementable "universal access to expertise"-policies might look like in Section 5.

[7] Outside of the health context, Bell et al. (2018) analyze parent-child transmission of know-how and norms relevant to innovation, and Hvide and Oyer (2018) study parent-child communication of industry-specific knowledge.

willingness to pay for branded drugs.[8] Another strand of this literature focuses not on physicians themselves, but on how exposure to intra-family expertise affects health and health behaviors of physicians' family members. Here, in the same Swedish setting, Finkelstein et al. (2021) find that physicians and their family members adhere differently to medical guidelines than individuals without access to health expertise. Closely related to our paper is also work by Artmann et al. (forthcoming), who use the Dutch medical school lotteries to estimate the impact of having a physician child on parents' mortality and utilization of health care. They find no improvements in mortality and little difference in the use of health care among Dutch parents of physicians. While we do not examine these outcomes in our lottery analysis, our results overall paint a diverging picture. As we discuss more in Section 4.1, the differences in the medical school admission systems (as well as other differences in health care institutions between Sweden and the Netherlands) likely lead us and Artmann et al. (forthcoming) to estimate effects at different points in the treatment effect distribution, contributing to the contrasting findings.

More generally, our findings contribute to the literature documenting a positive association between educational attainment and own health and health behaviors (see, e.g., Cutler and Lleras-Muney 2008, Smith 2007, Cutler and Lleras-Muney 2010, and Meghir et al. 2018), and intra-family spillovers of education on health (see, e.g., Currie and Moretti 2003, McCrary and Royer 2011, Lundborg and Majlesi 2018). We build on this literature by considering a precise type of education, a medical degree, and by analyzing spillovers across large family trees.

Finally, contrary to the papers cited above, on expertise broadly defined or on educational attainment in particular, we quantitatively explore the implications of our findings for the broader question of the roots of health inequality, relating our work to a plethora of research on health inequality.[9] Here, we make two distinct contributions. First, we deliver estimates of the income gradient in morbidity and show that it steepens over time using comprehensive, administrative data on both health outcomes and precise measures of income. Second, we provide quasi-experimental evidence on one particular causal mechanism underlying the health-income gradient, and show that it may play a quantitatively important role in sustaining health inequality.[10]

2 Institutional setting, data, and facts

Sweden has universal health insurance. Patients pay at most a small co-pay for medical treatments or prescription drugs. Thus, individuals at any point in the income distribution have similar formal access to health care.

2.1 Data

Population and demographic information The backbone of our data is a population register consisting of all individuals born between 1936 and 2016 residing in Sweden from 2000 through 2016. From Statistics Sweden we obtain a file that connects each individual in this sample to their spouse or cohabiting partner as well as to

[8]Similarly, Bronnenberg et al. (2015) provide evidence that pharmacists' have lower brand premiums for pharmaceuticals.

[9]See, e.g., Fuchs (1992, 2004), Currie (2011), Cullen et al. (2012), Cesarini et al. (2016), Currie and Schwandt (2016), Dwyer-Lindgren et al. (2017), Almond et al. (2018), and Thakrar et al. (2018).

[10]Our exercise relates to that of Aizer and Stroud (2010), who show that the arrival of novel information – in particular, the Surgeon General's recommendation that women should refrain from smoking during pregnancy – induced more educated women to respond but little response among the less educated, thus increasing inequality at birth. Our findings, in contrast, suggest that intra-family expertise elicits a weakly larger response at the lower end of the income distribution.

their (dead or alive) parents, siblings, grandparents, children, and cousins. From these data, we also infer links to aunts, uncles, nieces, and nephews. For each individual, the register includes information about the year of death (if applicable).

We merge these data to Statistics Sweden's longitudinal database of individuals (LISA) from 1991 through 2016, which contains information drawn from various administrative records for the adult population (16 or older) (Statistics Sweden). For each individual, the register includes information about birth year, gender, and region of birth. From the income records, we construct an adult's income rank in a given year by calculating the sum of wage and self-employment income in the prior year, and rank individuals within birth cohort and gender. To construct a child's income rank at birth, we calculate the sum of both parents' work and business income measured one and two years before the child's birth, respectively, and rank within the child's birth cohort.[11]

In the LISA database, we also observe each individual's highest completed degree in each calendar year, which contains information about the degree subject (e.g., medicine or law).

For our lottery analysis, we merge in additional educational records from Statistics Sweden for year 2007 and onwards. First, we add high school GPA. This allows us to identify medical school applicants with top GPAs, who would be competitive for randomized admission spots. We also add information about whether an individual has taken the Swedish Scholastic Aptitude Test (SAT, in Swedish *högskoleprovet*). Second, we add college application information. As college admissions in Sweden are centralized, we can observe the full set of programs to which each individual applies in each application cycle. Third, we add college admission outcomes, allowing us to track who gets admitted into (undergraduate) medical programs.

Health care records To construct measures of health outcomes, health investments, and health care utilization throughout individuals' lives, we merge in information from various registers collected by the National Board of Health and Welfare: inpatient records (covering the years 1997 through 2016), specialist outpatient records (2001 through 2016), prescription drug records (2005 through 2017), and medical birth records (1995 through 2016). We do not observe primary care except during pregnancy, which is recorded in the medical birth records. For each inpatient and outpatient specialist visit, we observe the date of the visit and the diagnosis codes (ICD-10). Drug records contain the universe of an individual's prescription drug purchases made in outpatient pharmacies. For each purchase, we observe the drug's Anatomical Therapeutic Chemical (ATC) classification code, which allows us to link drugs to diseases.

Outcomes capturing health and health investments in (older) adulthood We use these health care records to construct variables that capture health outcomes and health investments at various points in the life cycle.[12] We want to capture outcomes that individuals have (some) agency over, so that they potentially can respond to access to expertise. In addition, a key constraint, of course, is that the outcomes need to be observable in our data.

[11] We use a CPI inflator (OECD) before constructing the income ranks.

[12] Appendix Section A reports the ICD-10 and ATC codes we use for defining diseases and health investment outcomes. We use different cohorts to study different outcomes. This is a natural consequence of the fact that different outcomes are observed (and relevant) at different points in the life cycle and our years of data vary slightly across different outcomes. See Appendix Section B for details.

For our analysis of health and health behaviors in adulthood, we define two broad sets of such outcomes. The first is *physical health* outcomes that individuals may be able to influence through their own decisions or behavior. We define indicator variables that capture any occurrence of four common and malleable[13] chronic conditions that we can measure precisely in our data: heart attack, heart failure, lung cancer, and type II diabetes. We refer to these as "lifestyle-related conditions."

The second set of outcomes captures *preventive health investments*, that is, behaviors that individuals have some control over and that are believed to be beneficial for health. While many of the most obvious candidates, such as diet or exercising habits, are not observable in the data, our prescription drug and patient records allow us to precisely capture several other important proxies for preventive investments in (older) adulthood. Using our prescription drug records, we construct indicators for use of chronic medications that are well-known to reduce the risk of initial or recurring cardiovascular episodes (statins, blood thinners, and beta blockers), or prevent complications from diabetes or asthma. We define usage as purchasing the drug conditional on having the relevant diagnosis. We also define useage of Vitamin D among older women (for whom this vitamin is recommended in older adulthood) as a preventive investment. Using our patient records, we also define two additional preventive health investments in old age that we can measure: the number of preventable hospitalizations and an absence of diagnoses for addiction to alcohol or drug substances.

In addition to these outcomes capturing health and health behaviors in adulthood, we capture longevity with an indicator for whether an individual is alive by age 80 conditional on being alive at 55.

Outcomes capturing health and health investments early in life Younger individuals do not have a high prevalence of the same chronic conditions as older adults. More generally, severe physical health conditions are less common, and a larger share of the interactions with the health care system concern preventive health.

In light of this, our key outcomes for younger individuals capture *preventive health investments*. Our first such measure is take-up of the HPV vaccine among women. While many vaccines are provided through the primary care system in Sweden as a part of a standard immunization protocol – and therefore unobservable in our prescription drug claims data – we observe this particular health investment because our prescription drug records span a time period when the HPV vaccine was not (yet) incorporated into the standard immunization protocol. This is a key preventive health outcome in our analysis of young individuals, as it satisfies desirable criteria: the vaccine is known to be beneficial, and we observe take-up in our data.

We also define three additional preventive health investments that we can observe: not experiencing an injury or poisoning; an absence of clinical substance addiction diagnosis; and refraining from the use of hormonal contraceptives.[14]

In addition, we define indicators for three *physical health* issues that are common in childhood and early adulthood and observable in our data – experiencing a respiratory infection, an intestinal infection, or chronic

[13] The malleability and lifestyle attribution of these common chronic conditions have been well documented in the medical literature (see, e.g., Wannamethee et al. 1998, Knowler et al. 2002, and Djoussé et al. 2009).

[14] We consider *not* using hormonal contraceptives as a positive health investment, since we find overwhelming evidence of physicians themselves substituting away from hormonal birth control. Concerns about the side effects of these medicines that have been documented in the clinical literature may drive this observation, although we cannot pin down the exact underlying mechanism with certainty.

tonsil diseases; we also use the total number of inpatient stays as an (admittedly coarse) summary measure of physical health.

Finally, to capture preventive investments into child health even earlier in life, we use information from the medical birth records to construct an indicator for whether the mother is using tobacco right before or during pregnancy, which is known to be associated with substantial risks to the fetus (Centers for Disease Control and Prevention, 2017).

2.2 Inequality in health in our empirical setting

Sweden is a particularly attractive empirical context to examine health inequality. In addition to excellent availability of data on both income and morbidity, the Swedish universal health insurance system allows us to examine the health-income gradient in the absence of large differences in formal access to health care. We begin by briefly characterizing health inequality in this setting, at various points in the life cylce.

To study inequality in mortality, we start with all individuals that are alive at age 55 and for whom we can define our longevity measure (alive by age 80).[15] Panel A of Figure 1 plots the share of individuals who are alive by age 80, by income rank. It illustrates that, despite Sweden's generous social safety net and equalized formal access to health care, there is a strong mortality gradient. At the very bottom of the income distribution, more than 40 percent of people die by age 80; at the very top, the corresponding number is below 25 percent.

It is instructive to briefly put this in relation to the income-mortality gradient that has recently been documented in the U.S.. Appendix Figure A1 plots one-year log-mortality against own income rank in both countries, for three combinations of age at death and age of income measurement for which we were able to construct estimates that can be directly compared to those reported in Chetty et al. (2016). We observe substantially lower mortality, at *any* point in the relative income distribution, in Sweden than in the U.S., consistent with the notion that universal health insurance and a broad safety net may raise a society's overall level of well-being. The differences in mortality *inequality*, however, are more nuanced: inequality is equally pronounced in Sweden and the U.S. among 75-year olds, but lower in Sweden at younger ages, especially among women.

Turning to morbidity gradients in adulthood, Panel B of Figure 1 displays the share of individuals aged 55 or older that had at least one of the four lifestyle-related diagnoses after age 55. The panel displays a steep gradient: individuals at the bottom ventile of the income distribution are about twice as likely to have at least one of these conditions (20 percent) than individuals at the top ventile (10 percent).[16]

For younger adults, Panel C of Figure 1 displays the gradient in take-up of the HPV vaccine by age 20 among women. The figure shows a sharp (reverse) gradient in this preventive health measure: only about 10 percent of women born into households at the bottom of the income distribution get vaccinated against HPV, while 40 percent of women with parents at the top of the income distribution do.

Even earlier in life, Panel D of Figure 1 depicts a remarkably sharp gradient in exposure to maternal tobacco

[15] At age 55, individuals are still several years away from retirement, allowing us to measure their income rank with high accuracy. We restrict the sample to individuals with positive work income.

[16] Many slowly emerging chronic conditions are frequently under-diagnosed. If the rate of diagnosis conditional on disease is lower at the bottom of the income distribution, which appears probable, then we are likely underestimating the steepness of the gradients for the prevalence of lifestyle-related conditions.

use in utero. While more than 30 percent of mothers in the bottom income ventile report using tobacco around the time of pregnancy, the corresponding number at the top of the income distribution is only slightly above 5 percent - a substantial difference in this important aspect of the prenatal environment. Finally, to track the evolution of the health gradient over the life cycle, we use a health measure that is relevant at all ages: the number of inpatient visits. Panel A of Appendix Figure A2 displays the gradient in the number of inpatient visits in the first five years of life. While we observe a pronounced gradient already at age 5, it steepens substantially over the course of the life cycle, as illustrated in Panel B of the same figure, which displays the same outcome between ages 45 and 50.

In sum, our empirical setting is characterized by substantial health inequality: Despite Sweden's broad social safety net, the health-SES gradient emerges early in life and becomes steeper in adulthood.

These facts suggest two take-aways. First, factors other than social insurance and differences in the formal access to health care ("supply-side" factors) must be important drivers of health inequality. Second, Sweden is a highly suitable setting for trying to understand the "demand-side" drivers of health inequality, as its institutional environment shuts down the supply-side mechanisms. In this paper we examine the idea that differences in health-related expertise could be a quantitatively relevant demand-side channel.

3 Exposure to health expertise and health outcomes

3.1 Measuring exposure to health expertise

We are interested in measuring whether exposure to health-related expertise affects individuals' investments into their health and their subsequent health outcomes. Exposure to expertise may affect individuals through multiple mechanisms, all of which the public health literature commonly refers to as "health literacy" (Kindig et al. 2004). Experts can transmit new knowledge about the costs and benefits of healthy behaviors and health investments; they can remind, nudge or corroborate existing knowledge, making it more salient and trustworthy; they can help determine when to seek formal care and use their "clout" to help family members navigate the health care system.

While it is intuitive that exposure to any or all of these underlying mechanisms may lead to better health outcomes, investigating the causal impact of exposure to health expertise, and the associated improvements in health literacy, on health is challenging, since these objects are hard to capture empirically. Here, we zoom in on a narrow environment where we can precisely measure individuals' exposure to expertise: the presence of a health professional in the family. The idea is simple. It is reasonable to think that health professionals are experts in the field of health who on average posses the highest degree of health literacy in a society. Family members of a health professional enjoy increased exposure to such expertise in daily informal interactions, which in turn should increase their own health literacy and thereby may improve health outcomes.

In the remainder of Section 3, we analyze the aggregate impact of having a health professional in the family on the extended family's health; we then return to a discussion of interpretations in Section 4.1.

3.2 Descriptive evidence

We use the records of higher education to identify individuals with health professional degrees – physicians and nurses – among the cohorts of working age adults in our analytic sample. We define two groups of individuals who may benefit from (differential degrees of) access to expertise: the health professionals' *narrow* and *extended* families, respectively. The narrow family is defined as the health professional's spouse, parents, parents-in-law, children, and children-in-law. The extended family further includes the health professional's siblings, aunts and uncles, grandparents, and cousins.

We start by documenting differences in health between individuals in families with and without a health professional. Panel A of Figure 2 revisits the mortality gradient from Panel A of Figure 1, but now plots it separately for individuals with and without a health professional in the extended family.[17] We drop observations for individuals who are educated as health professionals themselves, so that we are measuring the effect of exposure to a health professional instead of being a health professional.[18]

Panel A of Figure 2 reveals two clear patterns in the raw data. First, there is a visually detectable difference in the probability of being alive at age 80, which persists throughout the income distribution: Conditional on income rank, individuals with an extended relative who is educated as a physician or a nurse are more likely to survive to age 80. Second, this mortality difference is larger at the bottom of the income distribution. We estimate that, on average, individuals who have at least one extended family member who is a doctor or a nurse are 5.9 percentage points less likely to have died by age 80 conditional on being alive at age 55. This is a large difference relative to the average probability of having died by age 80 in the full sample, which is 31 percent, as it implies a 19 percent reduction in the probability of death. This is equivalent in magnitude to moving from the 70th to 100th percentile in the income rank distribution. Further, the difference varies by income rank, ranging from 7 percentage points on average in the lower half of the distribution to 4 percentage points in the upper half of the distribution (Panel A of Appendix Table A1 reports these estimates for each income decile).

Next, we examine whether these differences remain when controlling for a wide range of observable demographics. For that, we estimate the following OLS specification, separately for each income decile:

$$Y_{id} = \delta_d HP_i + \beta_d X_i + \epsilon_{id} \tag{1}$$

Here, Y_{id} is the mortality (or health) outcome of interest for individual i in income decile d, HP_i is an indicator variable that takes the value of one if individual i has at least one medical professional in the family, and X_i is a set of demographic controls that includes fixed effects for: own income rank percentile, highest-earning relative's income percentile, year of birth, gender, individual's (discretized) educational attainment, and county of residence at age 55. The coefficients of interest are δ_d that measure the average difference in the health outcome between individuals with and without a health professional in the extended family, for each age-55 income decile d, conditional on the demographic controls. We plot the point estimates from this regression in Panel B of Figure

[17] Recall that the x-axis is the rank based on individual age-55 work income, which includes wage income and self-employment income. Our results are not sensitive to also including government transfers and capital income when calculating income ranks, or to replacing individual income rank with household income rank.

[18] Appendix Figure A3 reports the share of individuals with a doctor or nurse in the family by income rank.

2. The pattern remains qualitatively the same across all income deciles: Individuals with a health professional in the family are less likely to have died by age 80, and the difference is on average larger at the lower end of the income distribution.

We now revisit the prevalence of chronic conditions that are commonly considered to be linked to lifestyle decisions throughout the life-cycle. In Panel C of Figure 2, we report differences in the probability of having one of the four lifestyle-related conditions, by whether or not an individual has a health professional in the extended family. The conditions are aggregated into a z-score index by first standardizing each outcome by subtracting the control group (i.e., $HP_i = 0$) mean and dividing by the control group standard deviation and then taking the average of the standardized outcomes. The raw data again shows a visible separation in the prevalence of these chronic conditions between individuals with and without a health professional in their extended families. The differences in the raw data are larger at the bottom of the income distribution. This pattern is still preserved when we condition on a rich set of observables. As Panel B in Appendix Table A1 shows, less than 50 percent of the difference can be explained by our rich set of covariates, leaving us with a clear pattern of significantly lower prevalence of lifestyle-related conditions among older individuals with health professionals in their families. Moreover, the difference remains on average larger at lower income levels.

Figure 3 reports similar analyses for younger ages. In Panels A and B, we examine the probability of young women receiving the HPV vaccine by age 20. We observe large differences in the probability of this health investment between young adults with and without a health professional in the family, across all points in the income distribution. About two thirds of the difference persists when we control for observable characteristics, as can be seen in Panel C in Appendix Table A1 as well as Panel B of Figure 3.[19]

Finally, Panels C and D of Figure 3 report the same analysis for the probability of being exposed to tobacco use *in utero*. We observe large differences in tobacco exposure rates for an unborn child in families with and without a health professional (including the mother of the child herself), especially at the lower deciles of the income distribution.[20] A child with parents in the first two deciles of the income distribution who has a health professional in the family is up to 8 percentage points less likely to have been prenatally exposed to tobacco than a child who has no medical professional in the family. As Panel D of Figure 3 and Panel D in Appendix Table A1 document, the gap in tobacco exposure rates monotonically declines with income rank and gets close to a precise zero at the top of the income distribution. While a substantial portion of the differences can be attributed to differences in observable demographics, observables do not account for the full gap, leaving a significant discrepancy of up to nearly 5 percentage points (or 14 percent) at the lower end of the income distribution. Further, observable differences cannot fully explain the pattern of the gap decreasing monotonically in income rank.

Appendix Figures A5 and A6 examine the heterogeneity in our descriptive results along the intensive margin

[19] Since Figure 3 focuses on younger ages, we include a different set of covariates as compared to Figure 2. For HPV vaccination, the covariates include fixed effects for parental income percentile at birth, highest-earning relative's income percentile, year of birth, gender, and mother's county of residence in the year before the child was born; for tobacco use *in utero*, we further include maternal age and fixed effects for maternal birth order and mother's education.

[20] Notably, we find even larger differences when we consider only (children of) expecting mothers that are health care professionals themselves. There is almost *no gradient* in the probability of tobacco exposure *in utero* among children of these mothers, with a level difference of up to minus 20 percentage points relative to the general population. Figure A4 in the Appendix illustrates this striking difference.

of exposure to a health professional in the family. We examine two dimensions of heterogeneity: geographic proximity and proximity in the family tree. The left-hand side panels in Appendix Figures A5 and A6 report the estimated differences in health outcomes between: (i) individuals without any health professional relative versus those with a broad – but not a narrow – health professional relative (dashed lines); as well as (ii) individuals without any health professional relative versus individuals with a health professional in their narrow family (solid line). The reported differences in health at each point in the income rank distribution come from the OLS regressions with the full set of controls, as in Panels B and D of Figure 2, as well as the respective Panels in Figure 3. The right-hand side panels report the same coefficients, but now splitting the sample by geographically close (solid line) and far (dashed line) health professional relatives. We define two family members as being geographically close if they have lived in the same county for more than 50 percent of the time during which they are observed in the sample.[21]

For both older and younger relatives, we consistently find that the effects of a health professional in the family are more pronounced if the health professional is a close relative. The differences are especially clear when zooming in on the lower part of the income distribution. For example, at the lower rungs of the income distribution, having a health professional relative far away in the family tree has little effect on the prevalence of lifestyle-related conditions after age 55 (Panel C of Appendix Figure A5), while the effect of having a close relative who is a health professional is pronounced. The results are more mixed for geographic proximity. Panel B of Appendix Figure A5 shows that the mortality effect is mainly driven by geographically close relatives at the lower rungs of the income distribution, while the geographic location of the health professional relative matters less at the higher end of the income distribution. For young children, family proximity and geographic proximity are often hard to separate, as children are likely to live in the same household as a close relative. In both cases, however, we find that HPV vaccination is more pronounced among young adults exposed to a health professional who is either a close family member or lives close by. For tobacco exposure, there is little difference on either of the intensive margins at the top of the income distribution, while the intensity of exposure appears more important at the bottom of the income distribution. These results suggest that the intensity of access to expertise is crucial for health production.

While we control flexibly for a wide range of individual characteristics in this analysis, akin to the approaches in Bronnenberg et al. (2015), Johnson and Rehavi (2016), Frakes et al. (2021), and Finkelstein et al. (2021), a remaining concern is that the presence of a health professional in the family may be correlated with *un*observables. We therefore use two quasi-experimental strategies to quantify the causal impact on health and longevity of having a health professional in the extended family.

[21] County ("län") is the top level geographic division in Sweden, with 21 counties as of 2019. The largest county (Stockholm) has 2.3 million individuals, while the smallest (Gotland) has about 59 thousand people.

3.3 Leveraging randomization in medical school admissions

We exploit the fact that admission to medical school in Sweden, for a subset of years, contained an element of randomization.[22] "Medical school" in the Swedish context refers to an undergraduate major in medicine, as medical training starts directly in college and not in a post-graduate professional school. Students choose their undergraduate majors before starting higher education, apply to specific departments, and follow a curriculum recommended by the department.

University applications in Sweden are centralized and handled by a governmental agency, *Universitets- och högskolerådet* (henceforth UHR). All prospective students interested in studying for all degrees and at all universities apply through the same system. There are two university application cycles per year, for programs starting in the fall and spring semesters, respectively. In each application cycle, a prospective student submits a rank ordered list of programs to the UHR. The applicant is not required to apply only to programs in the same discipline. For example, an applicant may rank the medical school program at the Karolinska Institute in Stockholm as her first alternative, the medical school program at Gothenburg University as her second alternative, a program in business at Lund University as her third alternative, and so on.

One mechanism by which the centralized agency allocates applicants to programs is by ranking them by their high-school GPA.[23] The applicant with the highest GPA gets her preferred choice, the second highest ranked applicant gets the highest available choice for which she qualifies, and so on. For competitive programs, in which demand exceeds supply, this process generates GPA admission cutoffs for each program (Universitets- och högskolerådet 2008, 2018), around which admission is effectively randomized.

The high school GPA ranges from 0.0 to 20.0. Since the inception of this grading system in 1997, grade inflation has been substantial (see, e.g., Diamond and Persson 2016). The share of students graduating from high school with a GPA of 20.0 increased from less than 0.1 percent in 1997 to 0.8 percent in 2008 (Vlachos 2010), an increase of more than 800 percent. As a consequence, many university programs saw their GPA admission cutoffs steadily increase over time. For medical school programs, which generally have the highest cutoffs of any programs in Sweden, this process eventually led to the cutoff hitting the 20.0 mark at all medical schools.

Figure 4 displays the maximum, minimum, and median GPA cutoffs for admissions to Sweden's six medical schools from 1998 to 2017. Prior to the fall 2002 application cycle, the admission cutoffs were gradually increasing over time, with slightly higher cutoffs in the fall than in the spring (reflecting the fact that more students apply right after graduating high school in the preceding summer). Starting in the fall of 2002 and during the subsequent fifteen application cycles (until the spring of 2010), both the highest and the lowest cutoffs were 20.0. Thus, admission to any medical school in the country necessitated the highest possible GPA of 20.0, and admission was

[22] There are no tuition fees for post-secondary education in Sweden. To cover living expenses, most students are eligible for financial support (part loan/part grant) from the Swedish Board of Student Finance (CSN).

[23] The GPA quota is one of several quotas allocating applicants. Another quota allocates applicants to slots based on their scores on the Swedish SAT, a non-mandatory test administered by the Swedish Council for Higher Education. In addition, small quotas are reserved for students with five years of work experience and, in some universities, for students admitted based on interviews, respectively. A student automatically competes in all quotas for which she is eligible. We observe whether a student took the Swedish SAT, and thus competed in the second admissions category, and control for it in all regressions. While we do not directly observe whether a student has five years of work experience, we can restrict the sample to applicants who graduated less than five years before applying to medical school. See footnote 28 for further discussion of this.

randomized by the UHR within this group.[24] Our primary identification strategy leverages this randomization, by comparing applicants to medical school with a GPA of 20.0 who were admitted and not admitted to medical school.[25]

While the randomization of applicants with 20.0 into admission resembles a perfect randomised control trial (RCT), one aspect of the institutional context complicates our analysis: Applicants who are not admitted on their first attempt have the option to re-apply in subsequent application cycles.[26] The possibility of re-application implies that individuals who are not admitted in a particular cycle may still eventually gain admission and become physicians; thus, even conditional on a GPA of 20.0, being lotteried in or out is not a "sharp" allocation of students to medical schools. At the same time, not all students who are declined admission in their first application round choose to reapply. Thus, being admitted on the applicant's first application cycle (which is effectively random) affects the probability of eventually matriculating into a medical program.

Given this, we exploit admission on the student's *first* application attempt as an instrument for whether an individual becomes a medical student and ultimately graduates with a medical degree. We proceed by estimating the following two stage least squares (2SLS) relationship (and the associated intent to treat (ITT) relationship):

$$Y_{j(i)} = \delta MD_i + \beta_1 x_{j(i)} + \kappa_1 X_i + \epsilon_1 \tag{2}$$

$$MD_i = \gamma A_i + \beta_2 x_{j(i)} + \kappa_2 X_i + \epsilon_2 \tag{3}$$

In Equation (2), $Y_{j(i)}$ is the incidence of a health outcome of interest for applicant i's family member j (we consider all members of the extended family), measured over a period of six or eight years, to allow for a sufficient number of observations as well as a relatively long period of tracking. For individuals who matriculate into a medical program, this time horizon captures the period of medical education, which typically lasts for 6 years, and the first two years after medical school completion. MD_i is an indicator variable that takes the value of one if applicant i matriculated into a medical program. X_i and $x_{j(i)}$ are vectors of observable demographics of applicant i and his or her family member $j(i)$.

The demographic covariates for the applicant and family member are not necessary for identification but improve precision. They include the applicant's birth year fixed effects, sex fixed effects, whether the applicant was born in Sweden, as well as covariates for family members' characteristics that include birth year fixed effects, sex and educational attainment fixed effects, fixed effects for the type of relative that $j(i)$ is to applicant i (grandparent, parent, child, aunt or uncle, sibling, sibling's child, or cousin), and whether the family member was born in Sweden.

Further, we control for two variables that mechanically affect the probability of admission (to *a* program): the

[24] Strictly speaking, the admission procedure has two rounds. The first round allocates admissions offers by lottery and assigns applicants who are not offered admission to a waitlist. In the second round, any declined offers go to the waitlisted applicants. In practice, this distinction is immaterial in medical school admissions as there is near-universal take-up of admission offers.

[25] Randomization is not common, but is present in multiple higher education settings across different countries. See, e.g., Ketel et al. (2016) on the economic return to medical school admission lotteries in the Netherlands, as well as Stasz and von Stolk (2007) on the overview of lottery use in multiple countries.

[26] While waiting for the next application cycle, applicants may try to increase their admission chances, either by taking the scholastic aptitude test and attempting admission through the alternative quota, or by working.

number of medical schools the individual i applies to in the first application round, and whether the applicant took the Swedish SAT.[27] The identifying assumption is that, conditional on the number of medical school applications and whether the student took the Swedish SAT, admission is randomly assigned.

The coefficient of interest in Equation (2) is δ, which measures the effect on health outcomes of having a family member get medical training. This coefficient may be biased if individuals whose relatives are in worse (or better) health systematically select into medical training. To address this concern, we instrument for MD_i with A_i as specified in Equation (3). A_i takes the value of one if student i was admitted to medical school in the first application cycle. The resulting estimate of δ measures the effect of having a family member initiate medical school for the group of compliers. Here, the compliers are family members of applicants who went to medical school because they won admission on their first application attempt, but who would not have received medical education had they lost this first lottery. The standard errors are clustered at the family level.

Our baseline sample of applicants includes all applicants to medical school in Sweden for whom we can track family members' health outcomes for at least six or eight years after their last medical school application *who had a GPA of exactly 20.0.* Our sample of family members includes all their grandparents, parents, parents-in-law, spouses, own children, aunts and uncles, siblings, siblings' children, and cousins.

Table 1 displays the mean of observable baseline demographics as well the probability of matriculating into medical studies for two groups of applicants in our baseline sample: those who were admitted (188 applicants) and those who were not admitted (555 applicants) in their first application cycle. First, we see a large difference in matriculation into medical school. Among applicants admitted in the first application cycle, 96 percent matriculated into a medical program. Among those who were not admitted in their first cycle (but could re-apply), the corresponding figure is 59 percent, giving us a large and precise first stage of 37 percentage point difference in the matriculation probability.

The accepted and rejected students were equally likely to be women (56 percent in the accepted group) and had an equal number of siblings (1.80 in the accepted group). They had similar ages, although the accepted group is statistically (but not economically) significantly older (19.80 in the accepted group vs. 19.57 in the rejected group).[28] Accepted and rejected applicants were equally likely to be born in Sweden, and to have parents that were born in Sweden. Both groups had similar parental household income and father's income, both measured before the applicant's high school graduation and before the first medical school application cycle. A similar share of applicants had lost their father, or mother, or one of the grandparents by the year before the first application to medical school in the admitted and non-admitted groups (in the admitted group, 1 percent of fathers, 1 percent of mothers, 57 (48) percent of paternal (maternal) grandfathers, and 32 (30) percent of

[27] Sweden has six medical schools during the time period for which we observe admissions data, with a seventh added in 2010 (in Örebro), and an individual competes for admission at all schools to which she applies. In a similar vein, taking the SAT allows the applicant to compete for admission in a second admissions category. As the exact randomization algorithm used by the UHR is not known, this control variable also accounts for the possibility that the test influences how ties are broken in the GPA category.

[28] The difference in age stems from the institutional nuances of the admission system: Applicants can strengthen their applications by gaining five years of work experience; so, if there is a big time gap between the first and subsequent applications for some applicants who chose this route for their applications, we may in a small number of cases mis-classify the first application cycle and capture individuals who gained work experience before re-applying. The differences in age shrinks substantially when we zoom in on small subsamples where we can more conservatively define the first application cycle and focus on high school graduates from the same year. The sample of these individuals, however, is too small to perform our analysis. Hence, we keep the sample in Table 1, and control for birth year fixed effects in our regressions. Results are similar when replacing birth year fixed effects with age as a control.

paternal (maternal) grandmothers were deceased prior to the student's application).

In sum, 15 out of 16 observables are balanced across the admitted and non-admitted groups, and the t-test comparisons are far from any conventional significance levels with the lowest p-value of 0.38 (for whether the applicant was born in Sweden) for all observables except age. We conclude that the evidence in Table 1 is consistent with an essentially random breaking of ties in medical school admission decisions for this group of highest GPA students. We thus proceed to use the first application cycle admission decision as an instrument for whether an individual matriculates into medical school. We report results (ITT and LATE estimates) separately for older and younger relatives of the medical school applicants, as the sets of relevant health outcomes and health investments differ.

Health of older relatives Table 2 reports our estimates of the effect of an individual gaining a medical education on the health outcomes of relatives aged 50 and above. For each relative, we track health outcomes starting in year $t+1$ after the applicant's last medical school application and until $t+8$.

We study two sets of outcomes capturing health in adulthood, as described in Section 2.1: lifestyle-related physical health conditions and preventive health investments. We also aggregate all the specific outcomes into a single health index. All outcomes are scaled in per 1,000 individuals terms to aid interpretation.

Columns (1), (2), and (3) report the intent to treat (ITT) effects (with and without controls), and the local average treatment effect (LATE), respectively. In columns (4) and (5) we report two different benchmarks that are useful for interpreting the ITT and LATE coefficients. Column (4) reports a simple mean among family members of applicants who lose the lottery on their first application attempt. Within our control group, we observe individuals that decide not to attempt another medical school application when they lose the lottery. Conceptually, this group of individuals includes (untreated) compliers and never takers. From Table 1, we see that only 4 percent of individuals that win the lottery do not matriculate into a medical school, so the share of never-takers in our data is extremely low. In that case, family members of individuals who lose the lottery and do not re-apply are predominantly compliers. Computing the mean of outcomes among the family members of these individuals allows us to directly estimate the mean of potential outcomes under no treatment for compliers, which we refer to as control complier mean following Kling et al. (2007). This is what we report in Column (5).

Physical health. We find that 42 out of 1,000 individuals in the control group have a heart attack during our observation period. Among individuals whose relative wins the medical school lottery, the rate declines by 19 out of 1,000 people - a remarkable decrease of 45 percent (ITT). Exposure to a health professional among compliers leaves this group with only 23 per 1,000 individuals with a heart attack. Importantly, as we are measuring the effects over a relatively short time frame, we cannot ascertain whether this decline is permanent or represents a short-run delay of this acute cardiovascular event.[29] We find similarly large effects on the probability of being diagnosed with heart failure: a decline of 27 per 1,000 individuals (ITT), or 54 per 1,000 among compliers

[29] In general, our estimates are not inconsistent with the results of related clinical trials; however, the direct comparison is hard to achieve for two key reasons: first, our timeline is actually long in the world of clinical trials, hence only few trials have been run over a comparable time period; second, nearly all large-scale clinical trials test the effect of one medication at a time, so that the composite effect of higher exposure to several cardiovascular medications at a time – which we are documenting further down in this Section – is unknown.

(LATE), off of the mean of 74 per 1,000 cases in the control group and 83 among control compliers. We do not find any evidence of a reduction of type II diabetes or lung cancer.

Preventive investments. Our first group of preventive investments captures utilization of medication recommended for preventing or ameliorating common chronic conditions. Individuals in the treatment group, i.e., relatives of individuals who win the medical school lottery in their first application attempt, are substantially more likely to purchase such medication.[30] The effects are economically large for all three cardiovascular drugs, and statistically precise for statins and blood thinners. For example, on average 247 out of 1,000 individuals in the control group purchase blood thinners, while in the treatment group it is 29 more people per 1,000 – a 12 percent increase (ITT). The effect of having a relative matriculate into medical school (on the compliers) is in turn larger, with 73 more people taking the blood thinning medication, which represents a 27 percent increase off of the control complier mean (LATE). The relative increases are qualitatively similar for the other two cardiovascular drugs: 32 percent and 7 percent increases in use of statins and beta blockers, respectively (LATE). Further, older family members of lottery winners are 20 percent (ITT) more likely to purchase diabetes medication conditional on having the disease. Female older relatives also take prescription strength (rather than over-the-counter) vitamin D (vitamin D deficiency is common with aging, especially among women) at higher rates: 27 out of 1,000 untreated compliers take the vitamin, while 49 per 1,000 more do so among treatment compliers (LATE). We find positive but noisy differences in use of asthma medication. We find no systematic evidence of a decline in what we measure as preventable hospitalizations for older adults, but find some evidence of a decreased probability of addiction to alcohol or drug substances.

Health index. To address the issue of inference with multiple outcomes and to improve statistical power, we aggregate all eleven measures into a "health index," following the approach in Kling et al. (2007). We first orient all outcomes in the same qualitative direction (for example, for statins more is "good," while for addiction less is "good"). We then construct a z-score for each outcome (subtracting the control group mean and dividing by the control group standard deviation) and take an unweighted average across all outcomes.[31] The result suggests that, among older adults, exposure to a family member who matriculates into medical school yields a large and statistically precise improvement in health.

Overall, the point estimates in our analysis are consistent with the idea that older relatives of a physician are in better health and that they undertake more - cheap and simple - investments into their health, than similar individuals in families without a physician in training or early in her career.

Health of younger relatives Table 3 reports estimated effects of exposure to health expertise on the health outcomes of relatives aged under 25. We measure health outcomes starting in year $t+1$ after matriculation and until year $t+6$.[32]

[30] We condition these regressions on the presence of the following chronic conditions—asthma, type II diabetes, heart failure, ischemic heart diseases, stroke, hyperlipidemia, or hypertension; thus, we can interpret our estimates as the effect of purchasing these drugs conditional on needing them.

[31] The index is on average equal to zero in the control group, by construction, since we are normalizing the z-score to the control group mean.

[32] We take a shorter time window for younger relatives. There are fewer younger than older relatives in the sample, while they experience adverse health events much less frequently. A shorter follow-up window allows us to increase the sample size, and unlike for the chronic conditions of the older relatives, for the conditions of the younger relatives, we would expect the effects to appear

Columns (1), (2), and (3) again report the ITT effects without and with covariates, and the LATE, respectively. Among female relatives aged 10 to 25, we estimate a large and positive effect on take-up of the HPV vaccine.[33] While 119 out of 1,000 individuals are vaccinated in the control group (174 among control compliers), our estimates imply increases of 56 per 1,000 (ITT) and 218 per 1,000 (LATE) – an increase of more than 100 percent among compliers. We estimate similarly large effects for the avoidance of hormonal contraception. While 644 out of 1,000 young women aged between 10 to 20 do not use hormonal contraception in the control group, 135 women (21 percent) more refrain from this form of contraception among those with a lottery winner (in the first application attempt) in the extended family (ITT).

We further find large effects of being exposed to a health professional in the family on the probability of having a substance addiction that warrants a visit to a hospital or specialist care among young individuals. The rate in the control group is 19 per 1,000; the corresponding number in the treatment group is significantly lower at 8 per 1,000 – a decline of 58 percent (ITT).

Our estimates for inpatient stays paint a similar picture. We find that younger relatives of those who win the lottery only have a third of the number of inpatient stays of lottery losers' younger relatives.[34] We do not capture any differences in the rates of severe injuries or poisoning (which are experienced by about a quarter of individuals in the sample), and we also do not find that being exposed to a health professional lowers the rates of respiratory infections, intestinal infections, or chronic tonsillitis.

Overall, we conclude that for younger generations, having a new doctor-in-training in the family has positive effects on health: We see a larger probability of preventive investments, a lower prevalence of addiction, and fewer inpatient stays. Finally, we summarize our eight measures into a health index, constructed in the same way as for the older family members. The health index estimate suggests that, among younger family members, informal exposure to a health professional yields economically and statistically significant improvements in health.[35]

3.4 The event of a family member becoming a health professional

While the Swedish medical school lotteries resemble an RCT and thus represent a near-ideal setting to examine causal effects, the relatively short follow-up period precludes us from studying the long-run impact of exposure to expertise on older adults. We therefore complement this research design with event studies that exploit the timing of the arrival of a health professional in the family. Consistent with the analysis in Section 3.3, we define the event of a family member matriculating into medical school as the start of exposure to expertise.[36] Which families experience this event is not random. However, we can get some indication of whether having a doctor in the family appears to impact the health of family members by observing how the trends in health evolve over

faster.

[33] Since we require a shorter follow-up time in this analysis as compared to the descriptive exercise above, we are able to extend the age range for the HPV vaccination outcome to ages 10-25, closer to the full range of ages for which an HPV vaccine is recommended.

[34] For the count of inpatient stays, we drop 27 observations (99th percentile) with very high counts of inpatient stays that are clear outliers in the distribution. Our point estimates are larger and slightly more precise when we include these observations, but excluding outliers gives us a more conservative estimate of the marginal effects given the high sampling variance.

[35] Power analysis suggests that except for one outcome, all outcomes in the lottery analysis have a power below 0.8 and most outcomes have a power below 0.5. This suggests that we are likely under-rejecting the null hypothesis of zero treatment effects, i.e., the health effect of having a physician in the family is likely more significant than what we estimate.

[36] We do not require that the medical profession is pursued after college, though in practice the vast majority pursue medicine.

time for families that experience this event relative to the trends in health in (similar) families that do not.

In particular, we compare the families of individuals trained as medical doctors to the families of individuals trained as lawyers. Both groups of families have similar socio-economic status, with income distributions that are skewed towards the top ventiles (Appendix Figure A7 plots the income distributions). Moreover, admittance into law school – a similarly prestigious education – also requires a high GPA.

We need two key identifying assumptions. First, we require that access to health expertise arrives to families after an individual starts medical training. We use the first medical doctor (and the first lawyer) arriving into a family as the time of arrival of medical (or legal) expertise. Second, for our results to be consistent with a causal interpretation, we need to assume that individuals do not decide to undertake medical training based on the *trend* in the health of their extended family members. In other words, we need the counterfactual trend in morbidity and mortality of health professionals' family members to be parallel to that of lawyers. These assumptions appear plausible given the long timeline that typically accompanies the decision to pursue a medical degree and the slow process of chronic disease development.

Before turning to a formal regression specification, we investigate whether the assumptions we require for identification, as well as the hypothesized effects, are supported by the raw data. Appendix Figure A8 documents raw differences in the probability of adverse health outcomes between individuals with a child who received a medical degree versus a law degree. In Panel A, we plot mortality. In particular, we take the five cohorts of individuals born in Sweden between 1936 and 1940, and select individuals who have at least one child with a medical or law degree. We exclude individuals who are health professionals themselves (either a doctor or a nurse) or who have a health professional spouse.[37] In this sample, we compute the share of individuals that died by each calendar year starting with year 1980 (i.e., starting when the individuals are aged 40 to 45). We keep individuals in the sample even if they die, so the figure records cumulative mortality. Panel B confirms that individuals in our two samples have identical average age. (This is what we would expect absent any dramatic–and unlikely–differences in the probability of having a lawyer or a doctor child across the 1936 to 1940 cohorts.) In earlier years, mortality rates are visually identical between the two groups. Around 1995 (ages 55 to 60), however, a diversion emerges between the mortality trends of lawyer-parents (hollow circles) and doctor-parents (filled triangles). Throughout the years 1997 to 2017, parents of doctors are dying at a slower rate than parents of lawyers. The difference becomes economically and statistically significant over time, converging to a difference of 269 per 1,000 individuals having died among lawyers' parents by 2017, as compared to 238 per 1,000 individuals among doctors' parents. The difference of 31 per 1,000 lives (or 12 percent) is statistically significant at less than 1 percent level.[38]

In Panels C to F, we repeat a similar exercise for the four chronic, lifestyle-related conditions: heart attacks, heart failure, type II diabetes, and lung cancer. As our medical claims data starts much later than our mortality data, we track individuals starting in year 1997 (up until 2016). To be able to observe individuals prior to older

[37]For lawyer-parents we further exclude those who had a child that became a nurse, while for the physician-parents, we exclude those who had a child that became a nurse before another child became a doctor.

[38]Note that in Sweden, it is extremely rare for individuals to reside with their children, even at a very old age; the social norm is that parents live alone and, if this is no longer possible, move into a long-term care facility (which is part of the municipal social insurance system). Thus, our results do not reflect an in-house caregiver effect.

age when the onset of conditions is already likely to have had started (and to increase precision), we increase the sample size by pooling the cohorts from 1936 to 1961. This cohort choice implies that we track individuals' chronic diagnoses from the age of 36 to 81. We observe remarkably similar patterns across all of these conditions. As with mortality, in the early years of our data, the prevalence of chronic conditions is indistinguishable among individuals with a lawyer and a doctor child. Eventually, significant differences emerge, however, with parents of doctors having a persistently lower prevalence of all four chronic conditions. By the end of our sample period, in 2016, parents of doctors will have had 3 per 1,000 fewer heart attacks (7 percent fewer compared to the base of 42 heart attacks per 1,000 individuals in the lawyer-child sample); 4 per 1,000 fewer cases of heart failure (10 percent fewer relative to the base of 40 per 1,000 among lawyer-parents); 8 per 1,000 fewer cases of type II diabetes (11 percent fewer relative to 76 per 1,000 among lawyer-parents), and finally 2 per 1,000 fewer cases of lung cancer (18 percent fewer relative to the lawyer-parents baseline of 11 per 1,000 cases). All of these differences are again economically large and highly statistically precise.

These sharp patterns in the raw data support our event study approach, as they suggest that the deviation in the trend of chronic condition incidence and mortality happens long after individuals' children are likely to decide to start their (undergraduate) degrees in law or medicine. Further, we observe non-trivial differences in the incidence of chronic conditions and mortality between these two groups of parents at older ages, despite their largely similar socio-economic standing (as illustrated in Appendix Figure A7).

We now turn to a more formal analysis of these patterns and estimate the following event study-style specification:

$$Y_{it} = \alpha_i + \sum_{\tau} \sigma_\tau D_{\tau,it} * Doc_i + \sum_{\tau} \kappa_\tau D_{\tau,it} + \gamma_t + \beta * X_{it} + \epsilon_{it} \tag{4}$$

In this specification, Y_{it} is the health outcome of interest for individual i at time t. While for simplicity we considered only parent-child links when graphing the raw data above, we now expand our analysis to the full set of relatives and consider health outcomes of parents, parents-in-law, as well as aunts and uncles of a medical doctor or a lawyer. Doc_i is an indicator that takes the value of one if individual i has a child who becomes a medical doctor rather than a lawyer. $D_{\tau,it}$ is a set of event year dummies. The individual fixed effects α_i measure time-invariant unobserved determinants of individual i's health. Year fixed effects γ_t capture general time trends in population health and allow us to account for secular trends in health care delivery and medical innovation. X_{it} is a set of time-varying demographic controls, in which we include the entire vector of age fixed effects to account for the fact that age is one of the most important determinants of health.

κ_τ:s are the coefficients for event year dummies and separately capture the evolution of health in event time. The coefficients of interest are σ_τ, which measure the impact of the first health professional arriving into the family on the family members' health *relative* to the arrival of a lawyer into the family. τ measures the number of years since the arrival of the health professional relative to time t. The range of τ's varies by outcome, depending on the availability of data. We do not impose a time break and allow the data to flexibly reveal any changes in the health patterns around the time when a family member starts training as a physician (or a lawyer). We normalize σ_{-1} to zero, so that all other σ_τ's are interpreted as changes in health relative to one year before the young family member matriculates into a medical or a law program. For a subset of families with a health professional (or lawyer) in the family, we do not observe the time at which they acquire their medical (or legal) degrees. Rather than excluding these individuals from the sample, we impute the timing of their degrees using high school completion year or year of birth.[39]

Figure 5 illustrates our results. We consider two main long-run outcomes: mortality and chronic conditions at older ages. For each health outcome, we plot the estimated σ_τ:s against τ. Coefficient estimates for negative τ:s allow us to assess whether the data are consistent with the assumption that individuals are not sorting into the medical profession based on trends in familial health. Our estimates strongly support this assumption, which is also consistent with our observations in the raw data on parental mortality and morbidity.

Panel A illustrates the impact of having a family member trained as a physician on the probability of death.[40] We observe a clear slow-down in the relative mortality rate among relatives of doctors (as compared to the relatives of lawyers) that starts emerging around year 8 after the young relative matriculates into college. The mortality gap then steadily widens for two decades. As Column (1) in Table 4 reports, the point estimates suggest a 1.7 percentage point decrease in the probability of death by event year 25, which corresponds to a 10 percent decline off the mean among relatives of lawyers, which is 16 percent.[41] Panel B of Figure 5 captures the impact on the aggregated incidence of lifestyle-related chronic conditions. We plot the same "lifestyle" index that we examined in the descriptive analysis, which is a z-score incorporating the following conditions: heart attack, heart failure, type II diabetes, and lung cancer. Consistent with the observations in the raw data and our lottery-based results, we observe a significant divergence in health between relatives of doctors and lawyers that emerges around year 5 post college matriculation. The divergence widens for two decades post college matriculation, as can also be seen in Column 1, Panel B of Table 4.

We report separate event study results for the four chronic conditions underlying the lifestyle index in Appendix Table A2, and graphically in Appendix Figure A9. We observe that differences emerge at around year 9 post matriculation in the probability of having a heart attack and at around year 5 for heart failure. The differences persist and expand over time. We observe a similarly pronounced divergence in the incidence of type II diabetes and lung cancer. Fifteen years after matriculation, relatives of doctors are 1 percentage point (23 percent) less likely to have a diabetes diagnosis and 20 percent less likely to have lung cancer. These long-run patterns are consistent with the idea that responses to exposure to expertise include the formation of healthier behaviors and the adoption of new preventive measures, and that these long-run processes yield cumulative health benefits that grow important over time.

We further examine the heterogeneity of our event study effects by income and proximity, both geographically

[39] For all individuals for whom we observe the year in which they acquire a medical (or law) degree, we count back 6 (or 5) years to define the matriculation year, as these are the common lengths of the undergraduate medical and legal programs. In event studies that examine mortality, we consider cohorts born in 1936 to 1940. In 17 percent of cases for doctor-relatives and 21 percent of cases for lawyer-relatives, we do not observe the exact year in which the relative acquired a medical or a legal degree. For these observations, we impute the age of matriculation as the year of high school graduation or the year the individual turned 19 if high school graduation year is not observed. In the analysis of chronic conditions, we observe the exact graduation date in 97 percent and 96 percent of cases, and impute the rest using high school graduation year or year of birth if high school graduation year is not observed.

[40] The outcome is an indicator taking the value of one if the individual has died by time t, and the value zero otherwise (i.e., if the individual is still alive in time t). The outcome thus captures the timing of death.

[41] Some individuals in our sample are observed only for a subset of event years. When we restrict the sample to a balanced panel (59 percent of the sample), we get nearly identical results – a 10 percent effect on the probability of death at event year +25 in the balanced panel and 10.2 percent effect in our baseline specification with the full sample.

and in the family tree. For the income- and geographic-based heterogeneity in mortality, we switch to younger cohorts, 1946 to 1955, for whom income and geographic information was available. Columns (2) to (7) in Table 4 report the results. As in the descriptive analysis, we find that individuals with income in the lower half of the distribution are more affected (we find an 8 percent higher relative impact on mortality in the first half of the income distribution) by exposure to expertise. Further, family members who live closer to the health professional geographically benefit more. We do not find substantial heterogeneity in the impact on mortality with respect to proximity along the family tree, though the impact on chronic conditions is stronger among individuals with health professionals that are closer family members.

4 Health-related expertise and health inequality

4.1 Mechanisms

There are two (broad) interpretations of the health rents that we document in Section 3. First, these rents may stem from having access to someone "inside" the health care system, so that family members of health professionals are more able to access scarce resources, e.g., getting faster or higher-quality care. If so, these benefits are intrinsically zero-sum and not scalable. A second interpretation is that having a family member trained in medicine delivers benefits that are *not* zero-sum, and that hence may be scalable. For example, by providing information about the benefits of vaccines, doctors may be able to increase their family members' vaccine take-up. This would not reduce other individuals' ability to get vaccinated (so long as there is no shortage of vaccines).[42]

In reality, both mechanisms may be at play. While our data does not allow us to do an exact decomposition, our results allow us to convincingly rule in the second interpretation; moreover, we are able to speculate about the quantitative importance of the first.[43]

Among older adults, we studied a number of chronic cardiovascular and metabolic conditions that are commonly attributed to a combination of lifestyle (e.g., diet, exercise, smoking) and the use of medications preventing these conditions (rather than to any expensive clinical interventions, which potentially may be in shortage in the Swedish health care system). Improvements in these physical health outcomes are *not* zero-sum: one patient avoiding a heart attack through lifestyle changes, for example, does not raise another patient's heart attack risk. In a similar vein, the preventive investments that we analyzed are inherently not zero-sum – the drugs that we examined are cheap and easily obtainable (off patent and included on the national prescription drug formulary).

Increased consumption of these drugs (conditional on needing them) among family members of health professionals likely stems from a combination of better information about the benefits of these drugs; more trust in this information; as well as reminders, nudges, and "nagging" – rather than from any costly and scarce interventions.

Among younger individuals, our results also allow us to rule in benefits to health expert exposure that are *not* inherently scarce. For example, one child avoiding tobacco exposure *in utero* is costless (if not cost-saving)

[42]Yet another interpretation is that our results could be driven by an income effect. We are able to test this interpretation directly using the lottery sample and do no find evidence in support of income effects in the Swedish context.

[43]We examine some examples of non-salable mechanisms empirically in Appendix Section C.

to society. Similarly, one patient obtaining the HPV vaccine does not preclude another patient from getting the same (low-cost) vaccine. Further, we documented a *reduction* in the number of inpatient stays among young family members of health professionals; under any non-scalable mechanism, we would have expected the reverse.

In sum, many of the outcomes that respond to having a health professional in the family are inherently *not* zero-sum. Instead, they reflect decisions that individuals make in their everyday lives, and that everyone in society could make without imposing a meaningful social cost – lifestyle choices concerning diet and exercise, tobacco use (or cessation) during pregnancy, vaccine take-up, and the use of cheap and readily available drugs. These are outcomes for which the professional "clout" or connections to the health care system are unlikely to matter. Indeed, in our lottery analysis, the "treated" group is exposed to a young physician – either in medical school, or in the first two years after finishing medical training – who likely has few connections and can mostly influence the health of family members by transmitting new or better explained information.

Another way to shed light on the importance of the scalable benefits channel would be to vary the extent of medical information held by a family member, *without* varying whether the family member is an "insider" in the health care system. In this spirit, the comparison of our results to the investigation in Artmann et al. (forthcoming) yields another suggestive piece of evidence in support of the "scalable benefits" interpretation of our results. In the Dutch setting, more than 40% of lottery losers go on to pursue professions that likely lead to high health literacy.[44] This stands in contrast to the Swedish context, where individuals not admitted to medical school through the GPA-based admission mechanism most frequently decide to pursue professions related to business, economics, and engineering. This suggests that there is a greater difference in basic health-related expertise between lottery winners and losers in Sweden than there is in the Netherlands (while winners in both settings end up "inside" the health care system). Consistent with this, we find treatment effects on outcomes capturing scalable benefits in the Swedish context, while Artmann et al. (forthcoming) find no treatment effects either on mortality or the use of the health care system. When comparing individuals with a much larger distance in medical expertise – physicians and lawyers – the same large positive treatment effect of expertise emerges in the Dutch setting as in the Swedish setting.[45]

4.2 Interpreting magnitudes

To put the magnitude of our estimates in context, we conduct a stylized exercise, asking: How would the mortality-income gradient change in our empirical context if we – in addition to providing universal health insurance – adopted a hypothetical policy that exposes *everyone* in society to the same health-related benefits

[44] Specifically, 18.2% pursue professions in biomedical science, movement science, therapeutics, and rehabilitation; 10.1% pursue psychology; 7.7% pharmacy and 6.8% other health occupations.

[45] Other differences in the institutional settings between the two countries also likely contribute to a different set of results in Artmann et al. (forthcoming). For example, while Sweden's public health insurance system entails no choice – everyone living in the same region has the same public plan, and thus face the same prices – the Dutch context features a managed competition system with a possibility of choosing different insurance plans. Handel et al. (2020) show that individuals with different occupations make systematically different health insurance plan choices in the Netherlands; further, they document quantitatively important intra-family spillovers in plan choice. This means that in the Dutch setting, doctors' family members may face different prices for publicly provided health care, which could e.g. attenuate the relationship between exposure to expertise and the utilization of health care. Another difference is that in the Netherlands, medical school lottery winners earn higher incomes than lottery losers (Leuven et al., 2013), yet we find no such income differences in the Swedish context. As Handel et al. (2020) show that individuals with higher incomes on average choose public health insurance plans with larger deductibles, these income effects may constitute another channel through which winning the lottery affects prices for health care in the Dutch setting.

that are enjoyed by those who have a health professional in the family? (While one such hypothetical policy could be to expand the number of health professionals, this is likely not implementable in practice. We return to a discussion of how potentially implementable "universal access to expertise"-policies might look in Section 5, and discuss the literature on related policies in Appendix Section D.)

For the purpose of this stylized exercise, we use the share of individuals with a college degree as a proxy measure of baseline (pre-hypothetical-policy) health literacy in the population.[46] For the cohorts born in 1936 and 1937 – for whom we can observe mortality by age 80 conditional on being alive at age 55 – 7 percent of individuals had a college degree in the lower half of the income distribution, while 31 percent had a college degree in the top half of the income distribution.

To compute how mortality would change at the bottom (top) half of the income distribution under our hypothetical "universal expertise"-policy, we start with observed mortality. We then let our hypothetical policy move the percentage of individuals that have access to expertise by 93 (69) percentage points (to 100 percent), and let these "treated" individuals experience a 10 percent reduction in mortality (i.e., the estimated effect from our event studies). This assumes that our mortality results are fully driven by scalable benefits, so that a treatment effect of 10 percent is attainable for the whole population, and thus presents an upper bound. This calculation, illustrated in Panel A of Appendix Figure A10, suggests that the mortality-income gap would shrink under our hypothetical "universal access to expertise"-policy: from the observed mortality gap of 0.076 between the top and bottom halves of the income distribution, to 0.063. This corresponds to a 18 percent reduction in the mortality-income gradient.

This thought experiment suggests that asymmetry in the quality and ease of access to health-related experts and expertise across the socio-economic distribution can, in theory, generate and sustain a quantitatively substantial share of the health-SES gradient – *even* in the presence of equalized formal access to health care and a generous social safety net.

5 Discussion and Conclusion

Growing evidence across various disciplines reveals stark correlations between health capital throughout the course of life and a range of measures of socioeconomic status. Yet, the mechanisms underlying these associations are poorly understood. A common explanation (and a ubiquitous focus of policy discussions) for the existence and persistence of the health-SES gradients is the difference in formal access to health care. Our evidence suggests that this explanation can only be one piece of the puzzle when it comes to understanding the origins of SES *gradients* (as crucially distinct from levels) in health. We document that strong socioeconomic gradients in mortality and morbidity across a range of ages and conditions persist in Sweden, a country that boasts universal formal access

[46] In Appendix Section E we discuss a version of this exercise that takes the literal measure of exposure to expertise that we use in this paper—the number of physicians in the family. Since having a physician in one's family is naturally not the only way to attain health-related expertise, we use educational attainment as a broader proxy for differences in health literacy. Indeed, several studies in public health have documented a tight linkage between multi-dimensional survey measures of health literacy and formal education in Europe (see, e.g., Sørensen et al. 2015). Using data of the European Social Survey (ESS) we have ourselves verified the strong association between education and measures of health literacy that were available for Sweden in the 2004 and 2014 waves. As a sanity check on the ESS data, we also verify that individuals without a college degree report being in worse health. Appendix Table A3 reports these results.

to health care and a well-developed social safety net. This fact motivates us to examine a mechanism other than access to health insurance and formal health care that may perpetuate socio-economic gradients in health. Specifically, we investigate whether exposure to health-related expertise over the course of an individual's life can build health literacy that improves health, and whether differences in such exposure across the SES spectrum contributes to health inequality.

To create a quantifiable metric of health literacy, we zoom in on an environment where we can precisely measure individuals' exposure to a health expert, by studying individuals who have a health professional in the family. Using descriptive evidence, event studies, and exploiting "admissions lotteries" into medical school, we find that having a health professional in the family improves physical health and boosts preventive health investments among younger as well as older generations. Our back of the envelope calculation based on these estimates suggest that, more broadly, differences in this exposure across the SES spectrum can account for as much as 18 percent of the mortality gradient.

One view of our results is that they emphasize the limits of government intervention. Sweden equalizes the supply-side of health, providing cheap and universal access to inpatient and specialized care, prenatal care, primary care, prescription drugs, and vaccines; yet substantial inequality remains. Our results indicate that this remaining inequality could in part stem from demand-side factors: decisions that individuals make outside of the health care system, such as whether to undertake beneficial lifestyle investments, whether to take prescribed (and cheap) drugs, whether to take up vaccines, and whether to cease tobacco use during pregnancy.

A more positive interpretation of our findings is that they suggest that a policy that were able to "mimic" what health professionals do for their family members would have the potential to make a substantial dent in population health and reduce health inequality. Indeed, our analysis suggests that exposure to a health expert at least partially improves health through "low-tech" (and hence, cheap) and nonrivalrous determinants of health, such as take-up of vaccines, the use of preventive medication, and cessation of tobacco use. At least conceptually, these benefits are scalable at the population level.

It is thus worthwhile to consider the specific features of intra-family communication, and whether a policy maker may be able to replicate (some of) those. If intra-family health professionals simply transfer (common) knowledge to their family members, then incorporating health literacy as a standard part of school curriculum along with information provision campaigns may be effective.[47] To the extent that the power of intra-family communication about health stems from trust or detailed knowledge about the health history and habits that come with a relationship that spans a long period of time, however, effective policies may need to strive to mimic the depth of these relationships. Elements of such a policy could include nurse outreach programs with a strong emphasis on the continuity of care (yielding *long-term* relationships), coupled with a strengthening of the role of a trusted and easily accessible – both in terms of geographic location and the administrative hurdles – general practitioner who knows patients, and potentially their whole family, over a long period of time. Further, Alsan

[47] Notably, health literacy instruction in sexuality and the family planning domain has become an standard part of secondary school curriculum in many countries. A multitude of studies has documented that the existence of this instruction and what is being taught in the classroom has significant effects on behavior. We are not aware of settings where broader health literacy instruction that could transmit information ranging from which food to eat to how to make an appointment with a doctor is systematically included in the national secondary school curriculum. A number of pilot interventions, however, have been implemented across different countries.

et al. (2019) suggests that health care professionals that resemble the patient (in their case, same-race providers) may gain more trust. Finally, given the heterogeneous effects across the income distribution, such programs specifically targeted at the poor may have the largest potential to reduce health inequality. Understanding the patterns that underlie the intra-family transmission of expertise in health (and other) domains, and the potential replicability of this transmission by public policies, remains an important area for future work.

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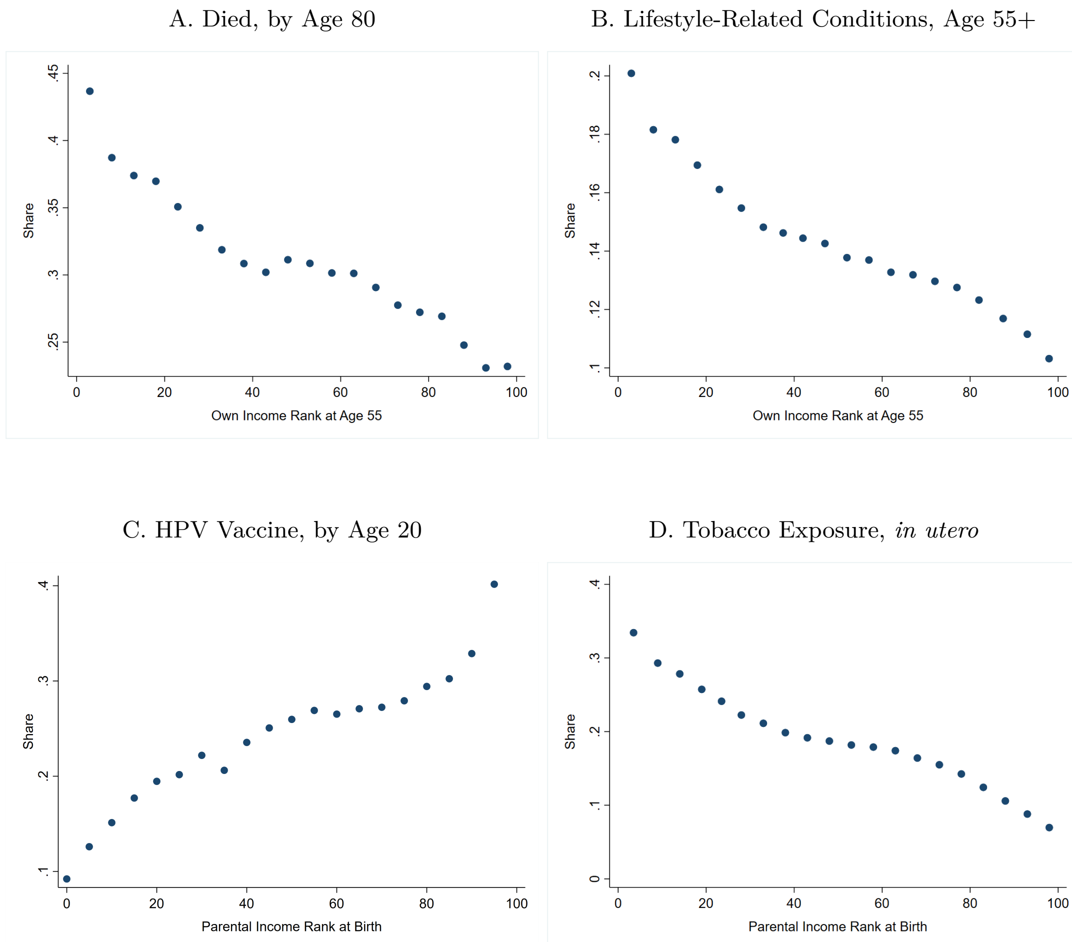
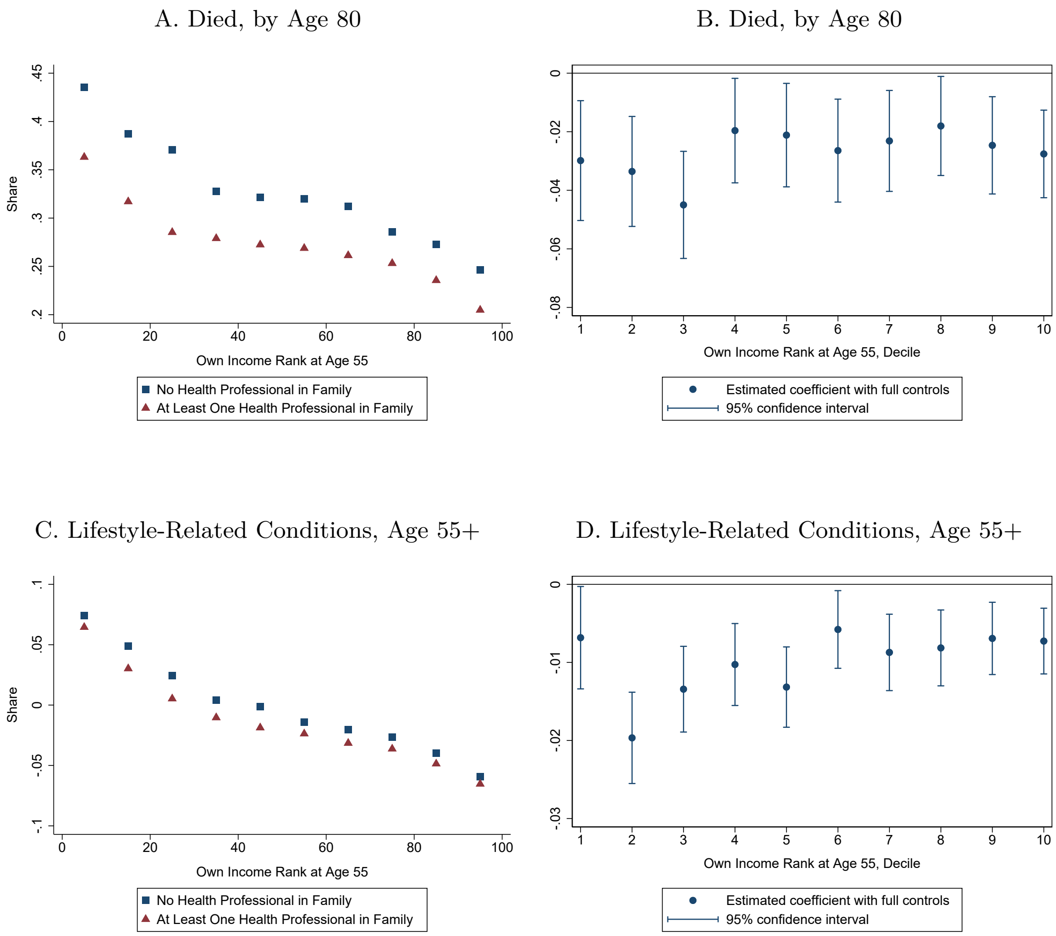
Figure 1: Income Gradients in Mortality and Morbidity over the Lifecycle



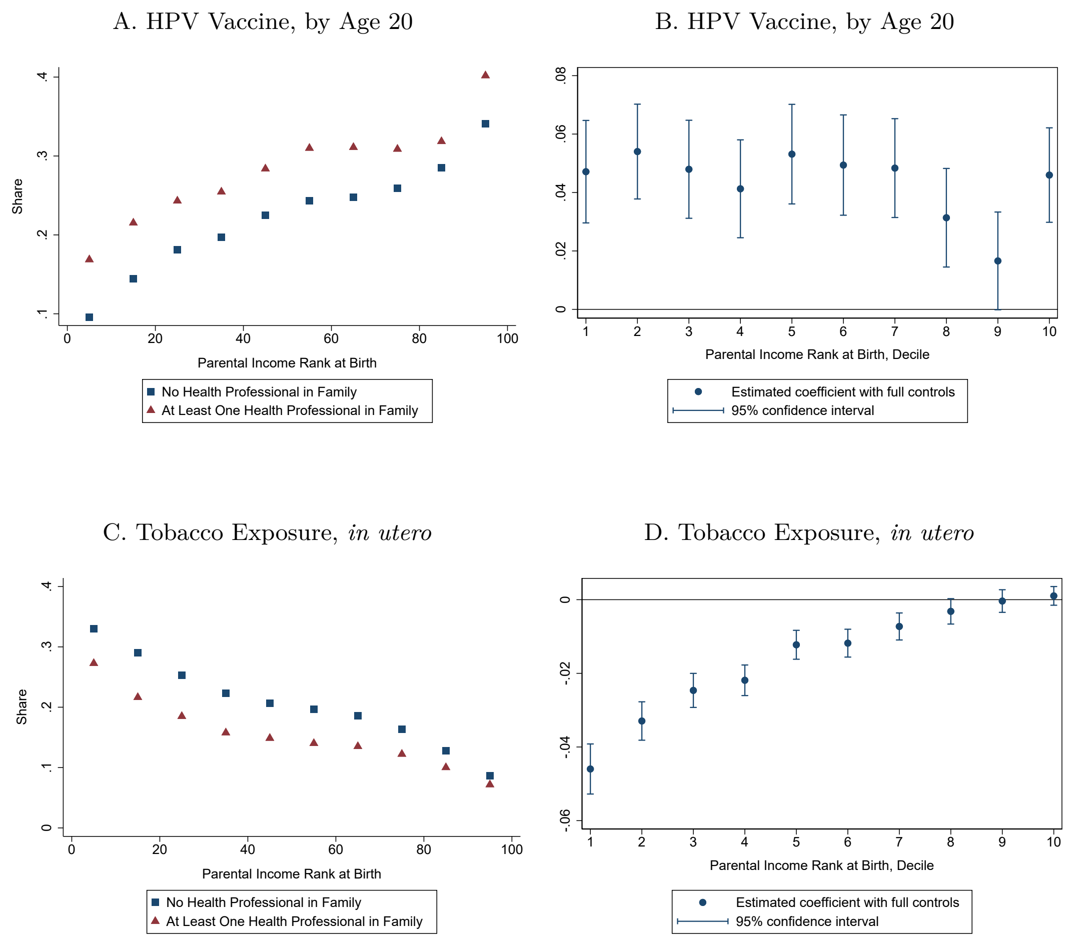
These Panels show the share of individuals with the specified health condition (vertical axis) by ventile of own income rank at age 55 or parental income rank at birth (horizontal axis). Individuals with zero or negative (parental) work-related income are excluded. Own income rank is assigned based on each individual's own income at age 55 relative to other people in the same gender-birth cohort. Parental income ranks at birth are assigned based on the average of parents' incomes in the two years before the child was born relative to other parents with children in the same birth cohort. Panel B is defined as having diagnosis codes for any of the following conditions after age 55: heart attack, heart failure, lung cancer, or type II diabetes. Panel A restricts the sample to individuals born in Sweden between 1936-1937; panel B restricts the sample to individuals born in Sweden between 1936-1961 and alive at age 55 and year 1997 (first year of inpatient claims). Panel C restricts the sample to females born between 1995-1997 and alive at age 20. Tobacco exposure *in utero* in panel D measures whether the mother used any type of tobacco within 3 months before or during pregnancy; the sample is restricted to children born in 1995-2016.

Figure 2: Health Professional in the Family and Health at Older Ages: Descriptive Evidence



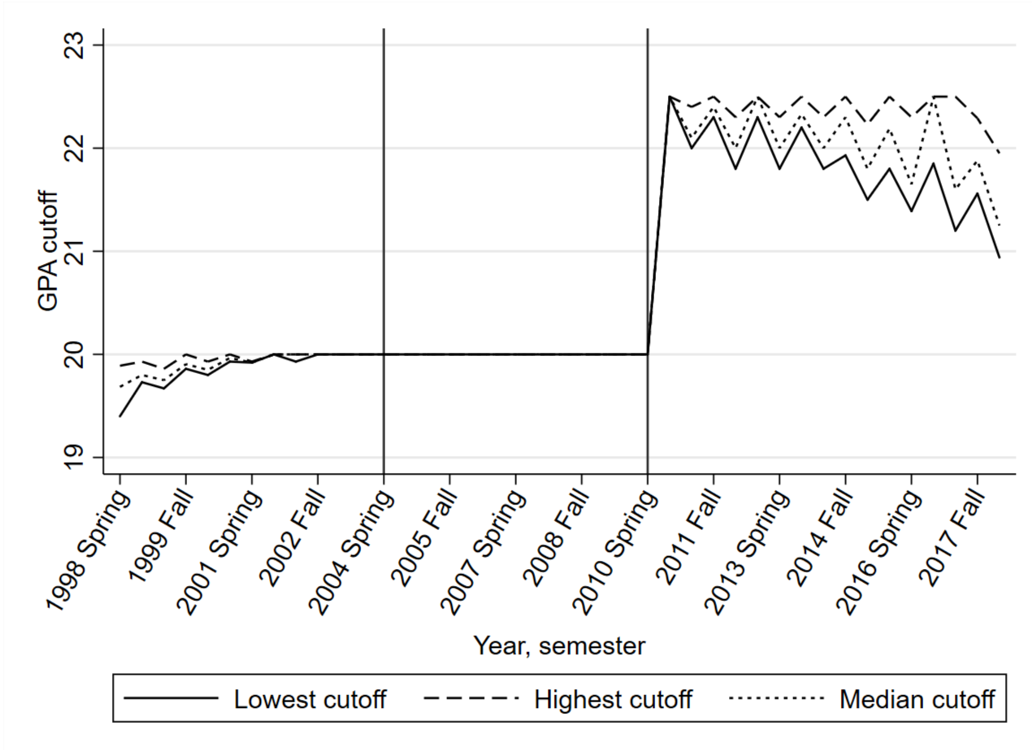
Panels A and C plot the share of individuals with the specified health condition by decile of own income rank at age 55. The outcome in Panel C is a z-score index of four underlying conditions: heart attack, heart failure, type II diabetes, and lung cancer; the index is constructed as specified in the text. We start with the same samples as defined in Figure 1. The samples are split by whether an individual has a health professional in the family. Individuals are assigned to the sub-sample "with a health professional" if at least one member of their broad family (spouse, sibling, cousin, child, child-in-law, niece/nephew, grandchild) has a university degree in medicine or nursing. We exclude individuals who hold a degree in medicine or nursing themselves. Panels B and D report coefficients from OLS regressions of each outcome on the dummy indicating whether the person has a health professional in the family. The covariates include fixed effects for individual's own income rank percentile and income rank percentile of the highest-earning relative, year of birth fixed effects, a gender dummy, fixed effects for discretized education levels, and fixed effects for the county of residence at age 55. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the family level.

Figure 3: Health Professional in the Family and Health at Younger Ages: Descriptive Evidence



Panels A and C plot the share of individuals with the specified health condition by decile of parental income rank at birth. We start with the same samples as defined in Figure 1. The samples are split by whether an individual has a health professional in the family. Individuals are assigned to the sub-sample "with a health professional" if at least one member of their broad family (sibling, cousin, parent, aunt/uncle, grandparent) has a university degree in medicine or nursing. Panels B and D report coefficients from OLS regressions of each outcome on the dummy indicating whether the person has a health professional in the family. The covariates include fixed effects for parental income rank percentile and income rank percentile of the highest-earning relative, year of birth fixed effects, a gender dummy, and fixed effects for mother's county of residence before birth; the covariates in Panel D further include fixed effects for birth order, mother's education, and maternal age. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the family level.

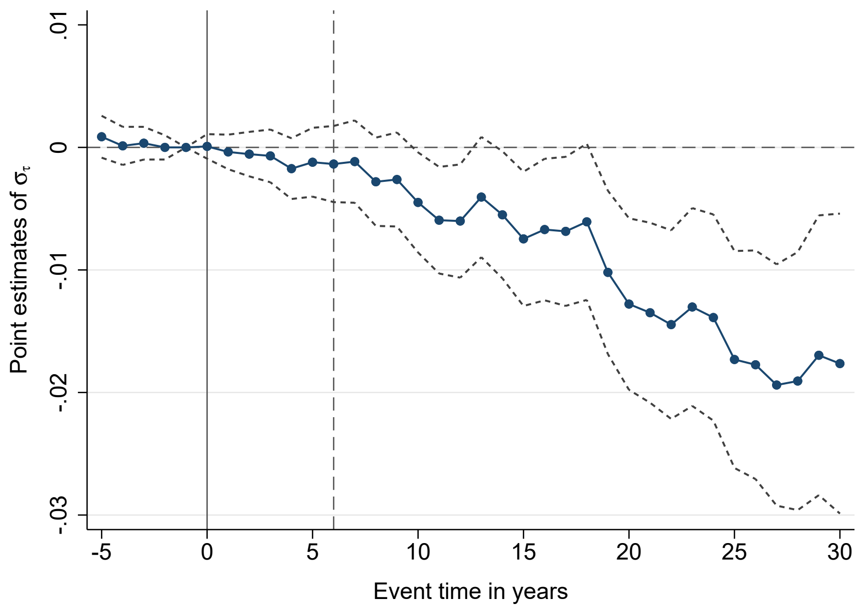
Figure 4: GPA Cutoffs for Admission into Medical Programs



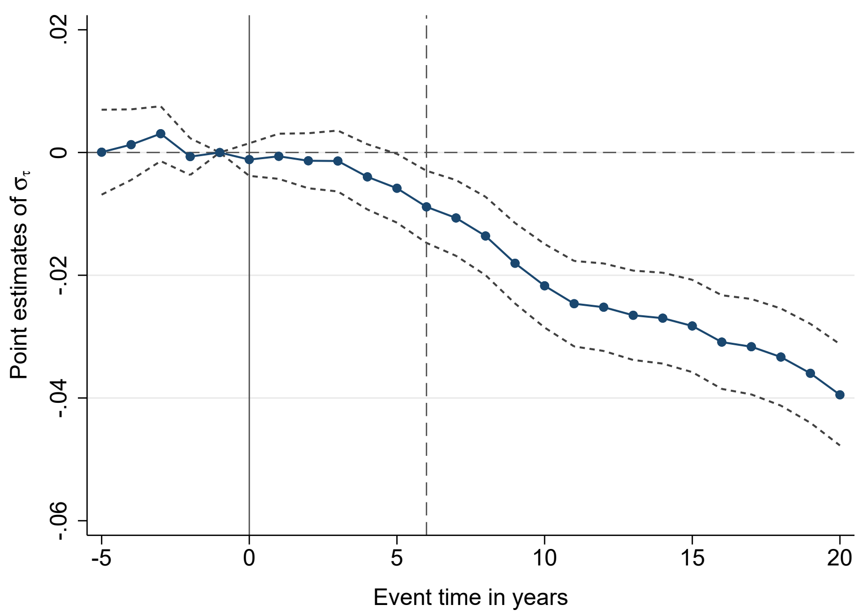
This figure plots the lowest, median, and highest GPA cutoffs for admission into undergraduate medical programs in Sweden, from 1998 through 2017. Each observation is a school and (semi-annual) application cycle.

Figure 5: Doctor in the Family and Long-Run Health Bonus: Event Studies

A. Mortality



B. Lifestyle-Related Conditions



The panels plot coefficients σ_τ and 95% confidence intervals from the event study specification in Equation (4). The analysis sample is restricted to family members of doctors and lawyers. Panel A includes family members born in Sweden between 1936 and 1940. Panel B includes family members born in Sweden between 1936 and 1961. In both Panels, we exclude family members who are themselves a health professional, or have a health professional spouse. In addition, family members with a relative who became a nurse before another relative became a doctor are dropped from the "doctor" sample, and family members with both a lawyer and a health professional relative are dropped from the "lawyer" sample. Panel B excludes individuals that have died before the first year of clinical records—1997. The regressions are centered at event year -1, i.e., one year before the child's year of matriculation in a medical or legal degree. The dashed vertical line marks the average graduation time for physicians. Standard errors are clustered at the family level.

Table 1: Medical School Lotteries: Balance of Baseline Observables

	Admitted (1)	Not Admitted (2)	p-value (3)
Medical School Matriculation	0.96	0.59	0.00
	(0.19)	(0.49)	
Demographics			
Female	0.56	0.60	0.41
	(0.50)	(0.49)	
Age	19.80	19.57	0.04
	(1.50)	(1.27)	
Number of siblings	1.80	1.79	0.92
	(1.05)	(1.05)	
Born in Sweden	0.97	0.95	0.38
	(0.18)	(0.21)	
Father born in Sweden	0.87	0.85	0.43
	(0.34)	(0.36)	
Mother born in Sweden	0.87	0.85	0.57
	(0.34)	(0.36)	
Parental income (10k krona, inflation-adjusted)			
Year before high school graduation	96.82	92.18	0.39
	(61.07)	(63.17)	
Year before first application	96.34	92.67	0.50
	(62.37)	(63.76)	
Father's income (10k krona, inflation-adjusted)			
Year before high school graduation	57.01	55.29	0.72
	(52.43)	(56.02)	
Year before first application	56.24	55.32	0.85
	(52.67)	(56.48)	
Relative deceased by year of first application			
Father	0.01	0.01	0.63
	(0.07)	(0.09)	
Mother	0.01	0.01	0.62
	(0.07)	(0.09)	
Paternal Grandfather	0.57	0.55	0.76
	(0.50)	(0.50)	
Paternal Grandmother	0.32	0.35	0.51
	(0.47)	(0.48)	
Maternal Grandfather	0.48	0.51	0.54
	(0.50)	(0.50)	
Maternal Grandmother	0.30	0.28	0.69
	(0.46)	(0.45)	
Number of Observations	188	555	

Notes: The table reports the probability of medical school matriculation and the sample mean (standard deviation in parentheses) of observable demographics for students who (i) have a high school GPA of 20.0 and (ii) applied to at least one medical school for the first time during application cycles Fall 2007 through Spring 2010. The sample in Column 1 includes applicants who were admitted to a medical school on their first application attempt. Column 2 reports the same outcomes for applicants who lost their first application lottery. Column 3 reports the p-value of a two-sided t-test for the equivalence in means between Columns (1) and (2).

Table 2: Doctor in the Family and Health at Older Ages: Medical School Lottery Evidence

	Intent to Treat					
Outcomes per 1,000 Individuals	No Covariates (1)	With Covariates (2)	Local Average Treatment Effect (3)	Control Mean (4)	Control Complier Mean (5)	Observations (6)
A. Health Index	40**	48***	121***	0	4	3,134
	(16)	(18)	(45)			
B. Physical Health						
Heart Attack	-12	-19*	-40*	42	48	1,532
	(10)	(11)	(21)			
Heart Failure	-21	-27*	-54*	74	83	1,532
	(13)	(14)	(29)			
Lung Cancer	3	3	6	6	6	1,532
	(5)	(5)	(11)			
Type II Diabetes	-11	8	15	77	72	1,532
	(14)	(15)	(29)			
C. Preventive Health						
Statins	23	37**	93**	281	293	3,134
	(17)	(18)	(45)			
Blood Thinners	31**	29*	73*	247	273	3,134
	(15)	(15)	(38)			
Diabetes Drugs	8	15	36	74	76	3,134
	(8)	(9)	(22)			
Beta Blockers	10	9	22	302	309	3,134
	(16)	(16)	(40)			
Asthma Drugs	9	9	22	179	187	3,134
	(15)	(16)	(39)			
Vitamin D	11	20*	49*	32	27	1,642
	(10)	(11)	(27)			
Preventable Hospitalizations	-14	1	3	197	235	1,532
	(38)	(42)	(84)			
Addiction	-8	-12	-25	26	30	1,532
	(8)	(10)	(19)			

Notes: The table reports the results of estimating Equation (2) for older (age 50 and above) family members of medical school "lottery" participants. Outcomes are tracked for 8 years after the applicant's matriculation into a medical school of the last medical school application. The sample size varies across outcomes due to differences in pharmaceutical and clinical data availability. The aggregate health index is an unweighted mean of z-scores of all individual outcomes. Columns 2 and 3 report ITT and LATE estimates with a full set of covariates, including the family member's birth year fixed effects, gender, educational attainment, family tie fixed effects (e.g., sibling, parent, etc.), whether the family member was born in Sweden, fixed effects for the applicant's birth year and gender, whether the applicant was born in Sweden, whether the applicant took the Swedish SAT, and the number of medical schools that the applicant applied to in the first application cycle. In regressions using statins, blood thinners, diabetes drugs, beta blockers, and asthma drugs as the outcome, we also control for whether the family member has asthma, type II diabetes, heart failure, ischemic heart diseases, stroke, hyperlipidemia, or hypertension. Standard errors clustered by the applicant are reported in parentheses. Column 4 reports mean outcomes in the control group, i.e., among family members of applicants who lost the lottery on the first medical school application attempt. Column 5 reports mean outcomes among the "control compliers," i.e., family members of applicants who lost the lottery on the first medical school application attempt and *did not* subsequently re-apply to medical schools. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table 3: Doctor in the Family and Health at Younger Ages: Medical School Lottery Evidence

Outcomes per 1,000 Individuals	Intent to Treat: No Covariates (1)	Intent to Treat: With Covariates (2)	Local Average Treatment Effect (3)	Control Mean (4)	Control Complier Mean (5)	Observations (6)
A. Health Index	31*	32*	118*	-1	37	4,113
	(18)	(18)	(67)			
B. Physical Health						
Number Inpatient stays	-33*	-38*	-140*	215	200	4,086
	(20)	(21)	(80)			
Respiratory Infection	-5	-4	-13	38	30	4,113
	(7)	(7)	(26)			
Intestinal Infection	-3	-2	-9	18	14	4,113
	(4)	(4)	(16)			
Chronic Tonsil Diseases	4	5	17	21	13	4,113
	(6)	(6)	(22)			
C. Preventive Health						
HPV Vaccination	42	56**	218**	119	174	1,192
	(26)	(26)	(108)			
No Hormonal Contraceptives	55	135***	562**	644	594	514
	(48)	(50)	(227)			
Addiction	-12***	-11***	-42***	19	15	4,113
	(4)	(4)	(16)			
Injury/Poisoning	2	-2	-7	265	251	4,113
	(16)	(17)	(61)			

Notes: The table reports the results of estimating Equation (2) for younger family members (younger than 25) of medical school "lottery" participants. Outcomes are tracked for 6 years after the applicant's matriculation into a medical school of the last medical school application. In Row 2 (number of inpatient stays), we drop observations above the 99th percentile of the distribution in inpatient stays. Row 6 (HPV vaccination) and Row 7 (no hormonal contraceptives) restrict the sample to females aged between 10 and 25 and females aged between 10 and 20, respectively. The aggregate health index is an unweighted mean of z-scores of all individual outcomes. Columns 2 and 3 report ITT and LATE estimates with a full set of covariates, including the family member's birth year fixed effects, gender, educational attainment, family tie fixed effects (e.g., sibling, parent, etc.), whether the family member was born in Sweden, fixed effects for the applicant's birth year and gender, whether the applicant was born in Sweden, whether the applicant took the Swedish SAT, and the number of medical schools that the applicant applied to in the first application cycle. Standard errors clustered by the applicant are reported in parentheses. Column 4 reports mean outcomes in the control group, i.e., among family members of applicants who lost the lottery on the first medical school application attempt. Column 5 reports mean outcomes among the "control compliers," i.e., family members of applicants who lost the lottery on the first medical school application attempt and *did not* subsequently re-apply to medical schools. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table 4: Doctor in the Family and Health: Event Study Evidence

		Heterogeneity by					
		Income		Family Tie		Geographic Proximity	
	Pooled	Below Median	Above Median	Close	Distant	Close	Distant
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Mortality							
τ=-5 (τ: event year)	0.001	0.001	0.000	0.001	-0.001	0.000	0.000***
	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.000)	(0.000)
τ=+15	-0.007**	-0.008***	-0.005**	-0.006	-0.007	-0.010***	-0.004**
	(0.003)	(0.002)	(0.001)	(0.005)	(0.005)	(0.002)	(0.002)
τ=+25	-0.017***			-0.020**	-0.020*		
	(0.005)			(0.007)	(0.008)		
Mean of Dep. Var. (at τ=+15/25)[a]	0.163	0.043	0.029	0.177	0.166	0.032	0.032
% Effect (at τ=+15/25)	10.4	18.6	17.2	11.3	12.0	31.3	12.5
Number of Observations	1,232,438	1,140,251	1,661,958	463,724	478,190	1,346,729	1,612,541
B. Lifestyle Conditions Index							
τ=-5	-0.000	0.007	-0.007	0.006	0.002	-0.000	0.003
	(0.004)	(0.006)	(0.005)	(0.006)	(0.005)	(0.006)	(0.005)
τ=+10	-0.022***	-0.023***	-0.019***	-0.022***	-0.016**	-0.028***	-0.023***
	(0.003)	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
τ=+15	-0.028***	-0.026***	-0.026***	-0.034***	-0.022***	-0.035***	-0.027***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
Mean of Dep. Var. (at τ=+15)[a]	0.000	0.000	0.000	0.000	0.000	0.000	0.000
% Effect (at τ=+15)	NA	NA	NA	NA	NA	NA	NA
Number of Observations	5,106,719	1,855,563	2,683,800	1,788,661	2,331,308	2,296,651	2,713,836

[a] Among family members of lawyers

Notes: The table reports coefficients σ_τ from the event study specification in Equation (4). The event time, sample restrictions, and set of family members included in the analysis are described in Section 3.4. Column (1) reports results for for mortality (Panel A) and lifestyle conditions (Panel B). Columns (2) and (3) split the sample by whether the individual's income rank is below or above the 50th percentile, dropping individuals with zero or negative income. Columns (4) and (5) split the sample by whether the doctor is a "close" or "distant" family member. An individual with a doctor child is classified as having a "close" tie, and individuals with a doctor elsewhere in the family is classified as having a "distant" tie. An individual who has both types of family ties is included in Column (4) but not (5). Columns (6) and (7) split the sample by whether the doctor is geographically "close" or "distant." Family members are classified as living "close" to each other if their place of residence is recorded to be in the same county for more than 50 percent of the years between matriculation (into law or medicine) and the last year of data (2016), and "distant" otherwise. An individual who has both types of geographic ties is included in Column (6) but not (7). The lifestyle conditions index in Panel B is constructed as the mean of the z-scores of indicators for a heart attack, heart failure, type II diabetes, and lung cancer; by construction the index is normalized to zero for the "control" group of lawyer family members. All regressions include the main effects and the interactions between event year dummies and the dummy for having a doctor in the (broad) family. The regressions further include the following covariates: age fixed effects, calendar year fixed effects, and individual fixed effects. Standard errors clustered by family are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

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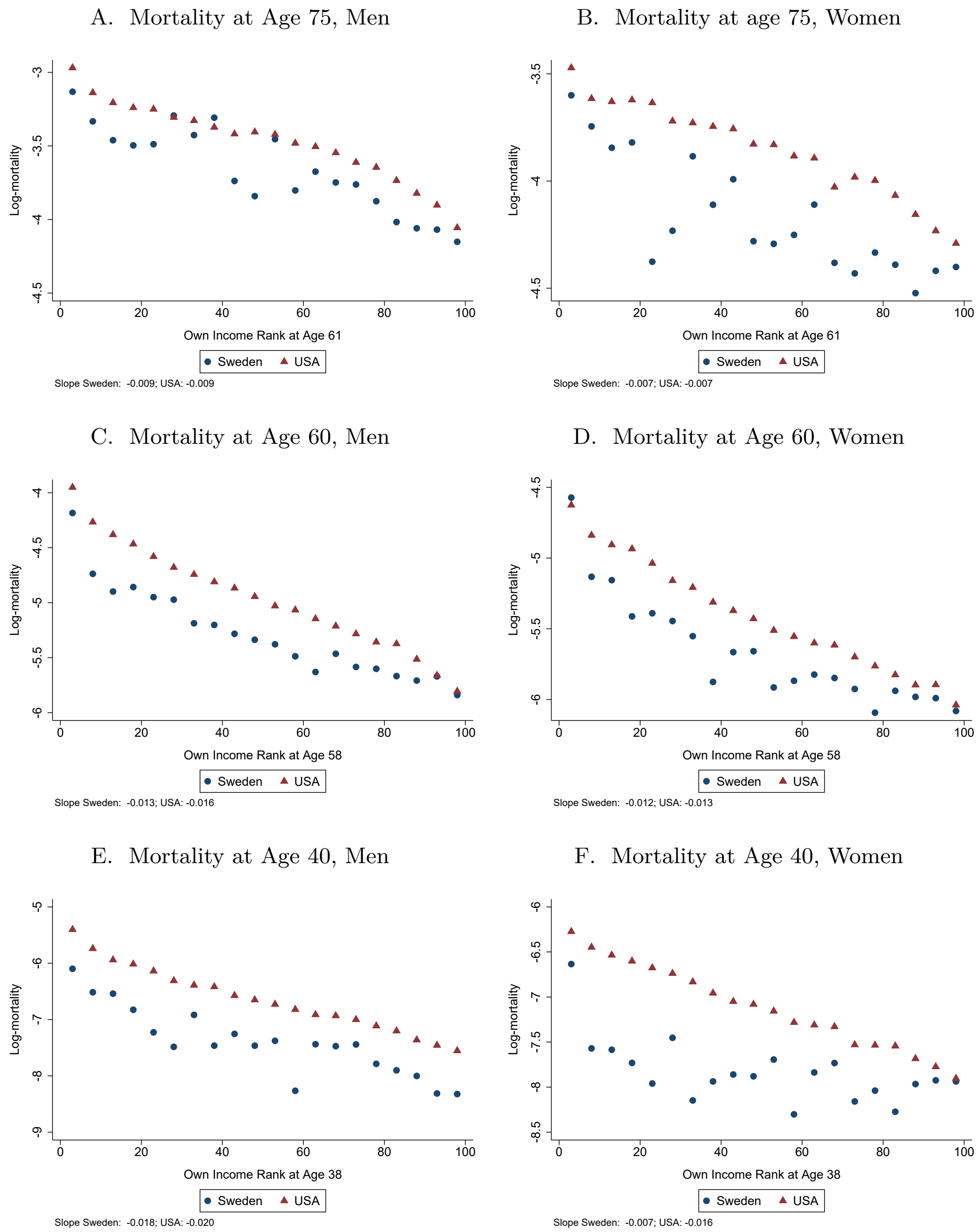
Appendix figures and tables

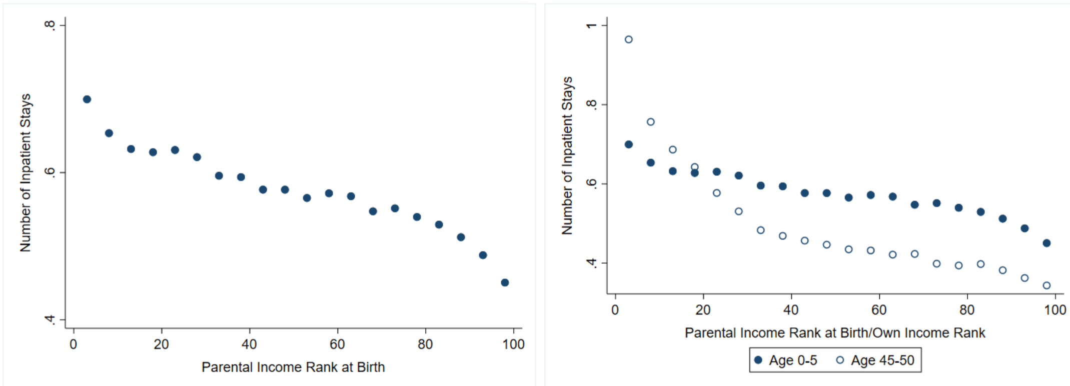
Figure A1: Income Gradients in Mortality in the US and Sweden



Figures compare mortality rates by income rank ventile between Sweden and the US. We plot log-mortality at age 40, 60 and 75 conditional on ventiles of income rank at age 38, 58, and 60 (Sweden) or 61 (US), respectively. The latter measures incomes a year before the earliest retirement ages in the respective countries. US mortality data is derived from the data reported by The Health Inequality Project. The sample for Sweden is restricted to the same birth cohorts as in the US data. Income is measured as the equivalent of the US adjustable gross income (includes work-related income, self-employment income, and capital income). We exclude individuals with zero or negative income from the analysis. The note in each panel reports the estimated slope of a linear regression of log-mortality rate on income rank percentile, separately by country and gender. We cannot reject the statistical equivalency of the slopes measuring the mortality gradient for men at age 40 and at age 75 (p-values 0.46 and 0.87, respectively), as well as for women at age 75 (p-value 0.96). We reject the equivalency for the mortality gradient for men at age 60 and for women at age 40 (p-value 0.00), and for women at age 60 at the 10% significance level (p-value 0.05).

Figure A2: Early Emergence of the Health-SES Gradient

A. Number of Inpatient Stays, by Age 5 B. Number of Inpatient Stays over Life Cycle



Figures plot the share of individuals with relevant outcomes for each ventile of income rank. Parental income rank at birth is assigned based on the average of parental incomes in the two years before the child was born relative to other parents with children in the same birth cohort. Income rank for adults aged 45-50 in Panel B are assigned based on each individual's own income at age 40 relative to other individuals in the same gender-birth cohort. Inpatient stays due to pregnancy, childbirth and the puerperium are excluded from the count of inpatient stays in Panels A and B.

Figure A3: Share of population with a doctor or a nurse in the family

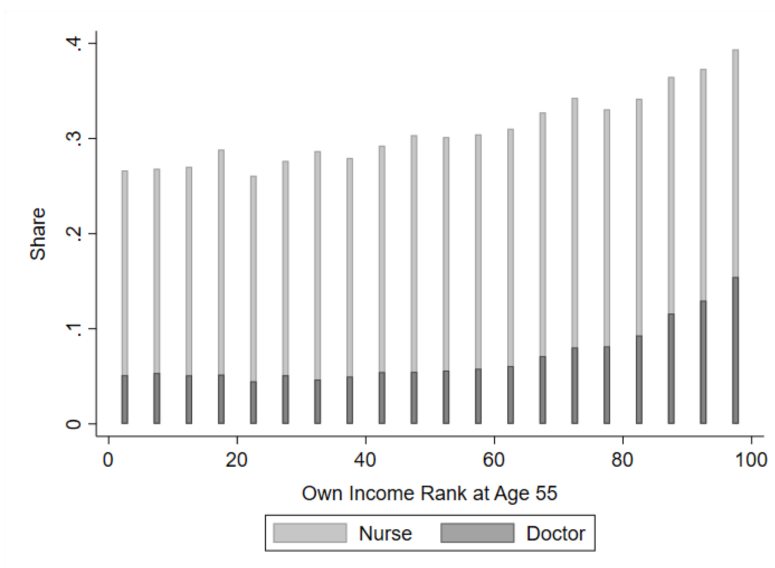


Figure plots the share of individuals with a nurse or a doctor in the family by income ranks. The sample of individuals is defined as in Panel A of Figure 2.

Figure A4: Tobacco Exposure, *in utero*

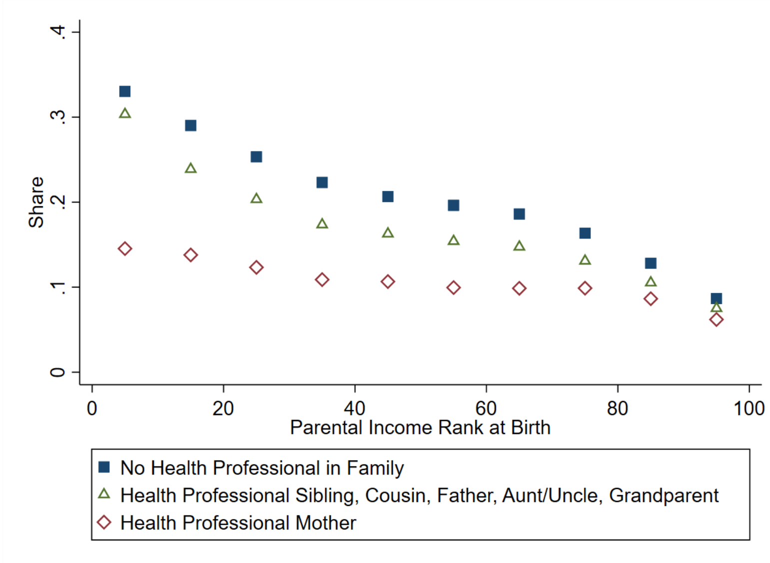
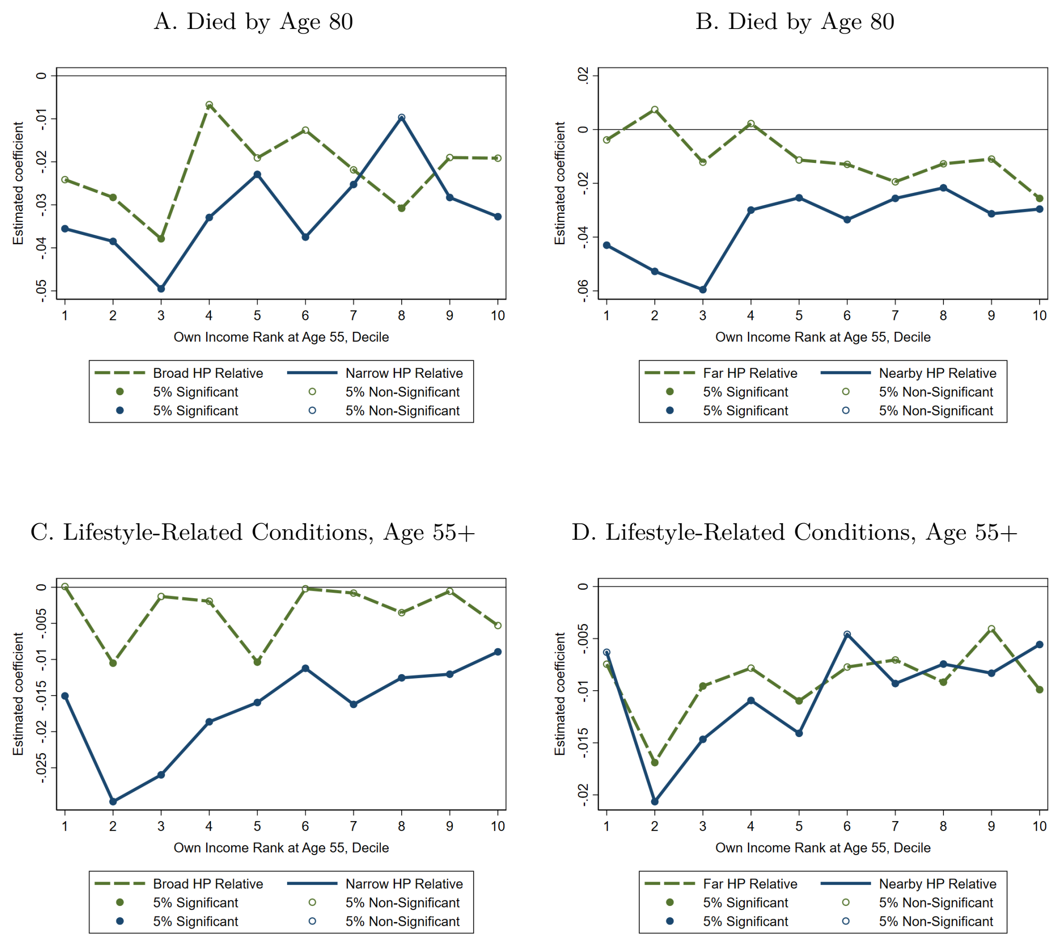


Figure plots the share of children exposed to tobacco *in utero* by parental income rank at birth. Parental income rank at birth is assigned based on the average of parental incomes in the two years before the child was born relative to other parents with children in the same birth cohort. We start with the same data sample as defined in Panel C of Figure 3. The sample is split by whether an individual has a health professional relative or a health professional mother. Individuals are assigned to the sample "with a health professional" if at least one member of their broad family (sibling, cousin, father, aunt/uncle, grandparent) has a university degree in medicine or nursing. Individuals are assigned to the sample "with a health professional mother" if the mother has a university degree in medicine or nursing.

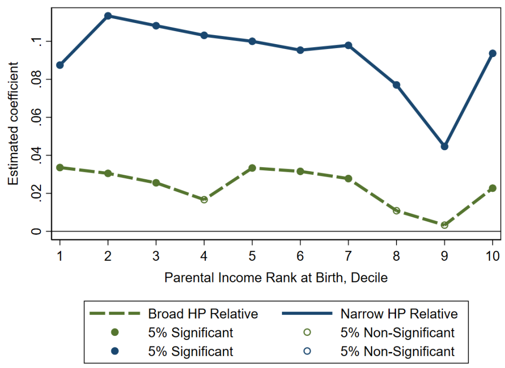
Figure A5: Health Professional in the Family and Health at Older Ages: Heterogeneity



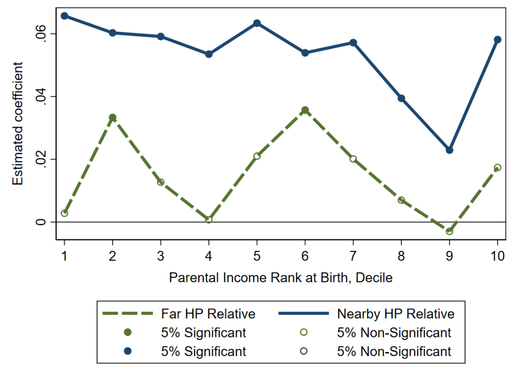
Figures replicate the analyses in Panels B and D (specifications with the full set of covariates) of Figure 2 for sub-samples of the data. Panels A and C re-estimate conditional differences in mortality and the prevalence of lifestyle-related conditions separately for individuals that have a broad, but no narrow, health professional in the family (dashed line), and for individuals that have a narrow health professional (solid line), relative to individuals that have no health professionals. A broad family tie is defined as having a health professional, who is a sibling, cousin, niece/nephew, or a grandchild. A narrow family tie is defined as having a health professional, who is a child, child-in-law, or a spouse. Panels B and D split the same based on geographic proximity. An individual is defined to have a nearby health professional relative (solid line) if both reside in the same county in the same year for more than 50 percent of the years that are observed between 1991 and 2016. The individual is defined to have a far health professional relative otherwise (dashed line). In all regressions, the comparison group is the set of individuals without any health professional relative. Coefficients are reported as filled circles if they are estimated with at least 5% statistical significance level, and as hollow circles otherwise.

Figure A6: Health Professional in the Family and Health at Younger Ages: Heterogeneity

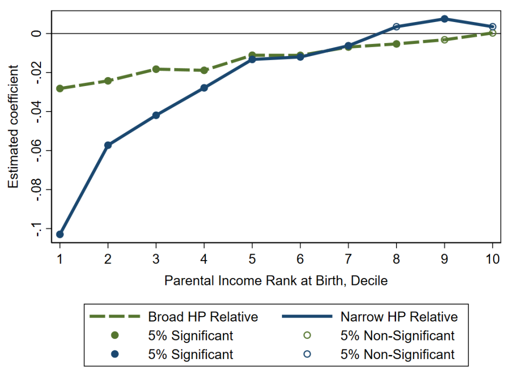
A. HPV Vaccine, by Age 20



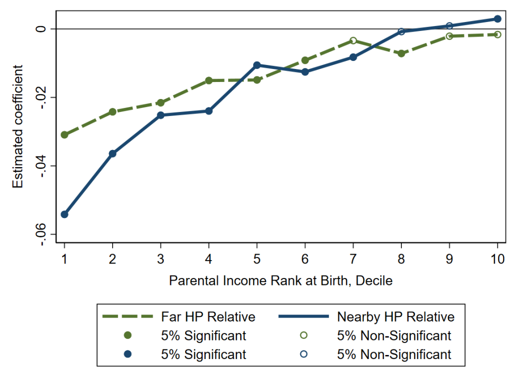
B. HPV Vaccine, by Age 20



C. Tobacco Exposure, *in utero*



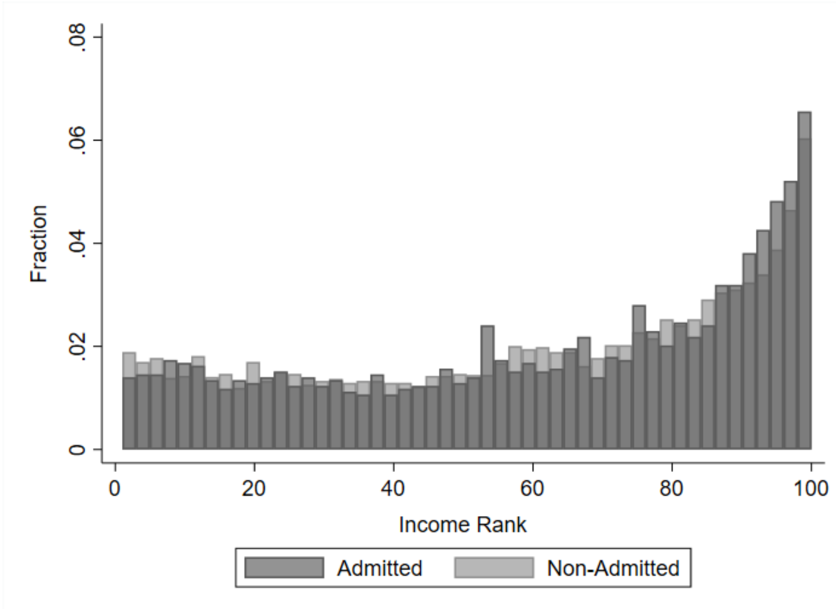
D. Tobacco Exposure, *in utero*



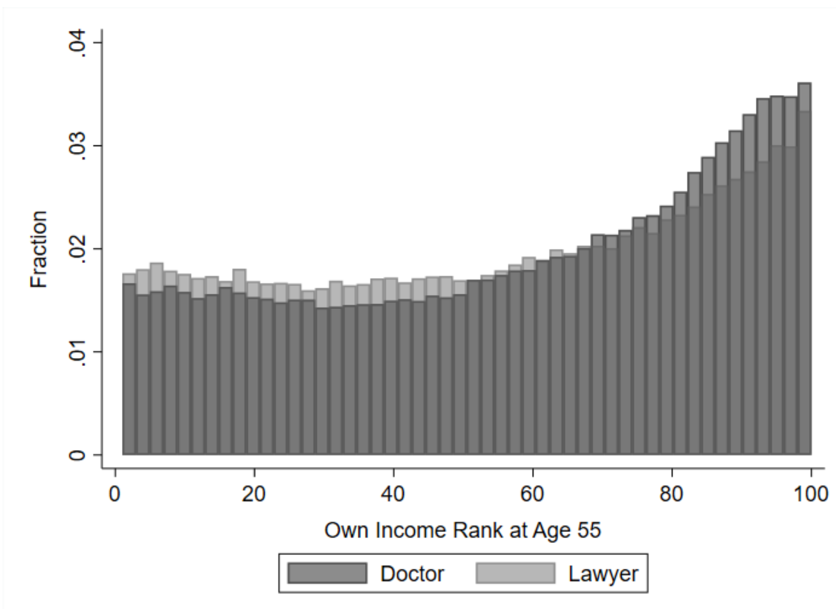
Figures replicate the analyses in Panels B and D (specifications with the full set of covariates) of Figure 3 for sub-samples of the data. Panels A and C re-estimate conditional differences in HPV vaccination and tobacco exposure *in utero* separately for individuals that have a broad, but no narrow, health professional in the family (dashed line), and for individuals that have a narrow health professional (solid line), relative to individuals that have no health professionals. A broad family tie is defined as having a health professional, who is a sibling, cousin, aunt/uncle, or a grandparent. A narrow family tie is defined as having a health professional mother or father. Panels B and D split the same based on geographic proximity. An individual in Panel B is defined to have a nearby health professional relative (solid line) if the individual's parents reside in the same county in the same year for more than 50 percent of the years that are observed between 1991 and 2016. A child in Panel D is defined to have a nearby health professional relative (solid line) if in the year of birth, a health professional relative lived in the same county as the mother. The individuals are defined to have a far health professional relative otherwise (dashed line). In all regressions, the comparison group is the set of individuals without any health professional relative. Coefficients are reported as filled circles if they are estimated with at least 5% statistical significance level, and as hollow circles otherwise.

Figure A7: Income Distribution of the Medical School Lottery and the Event Study Analysis Sample

A. Medical School Lottery Sample

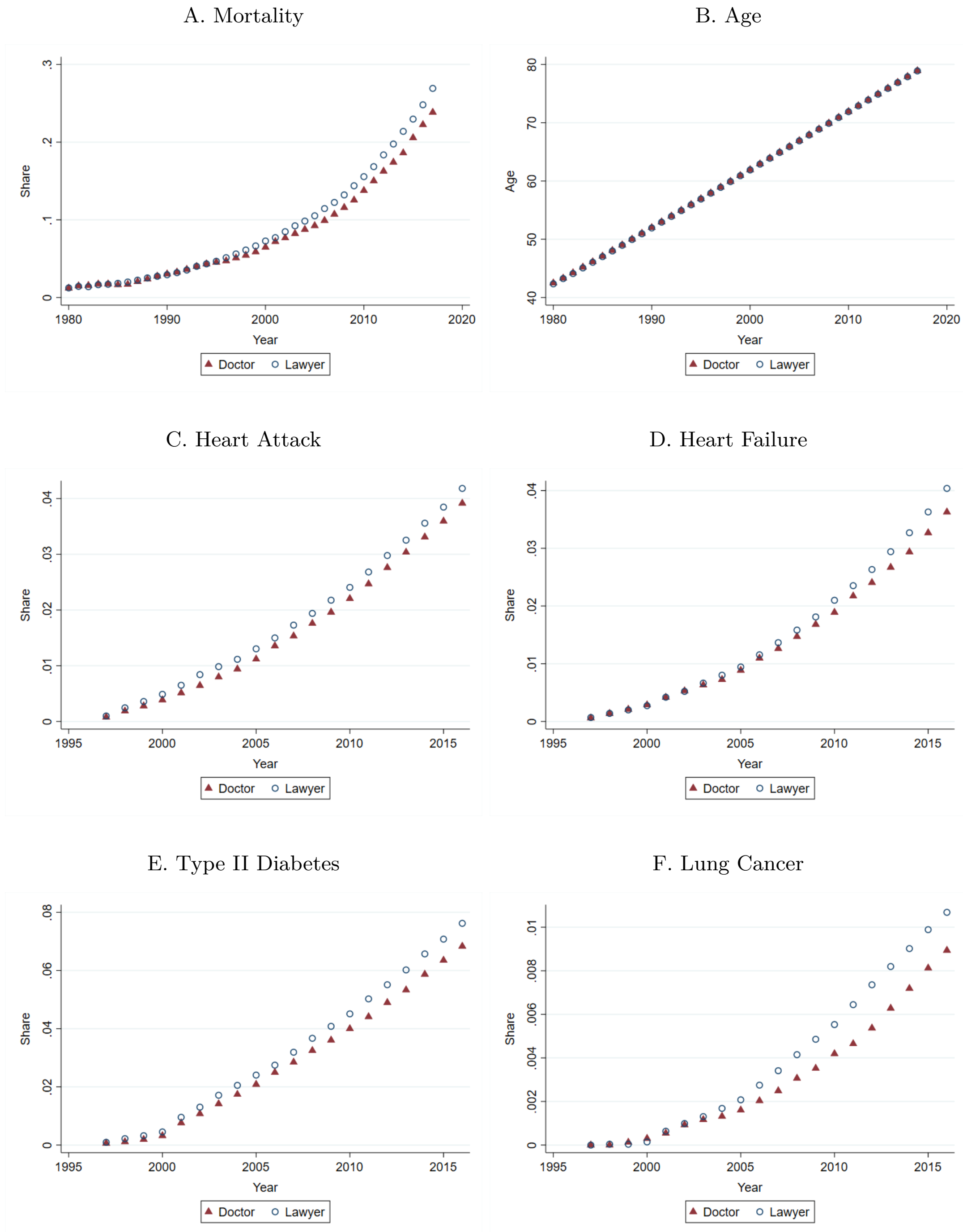


B. Event Study Sample



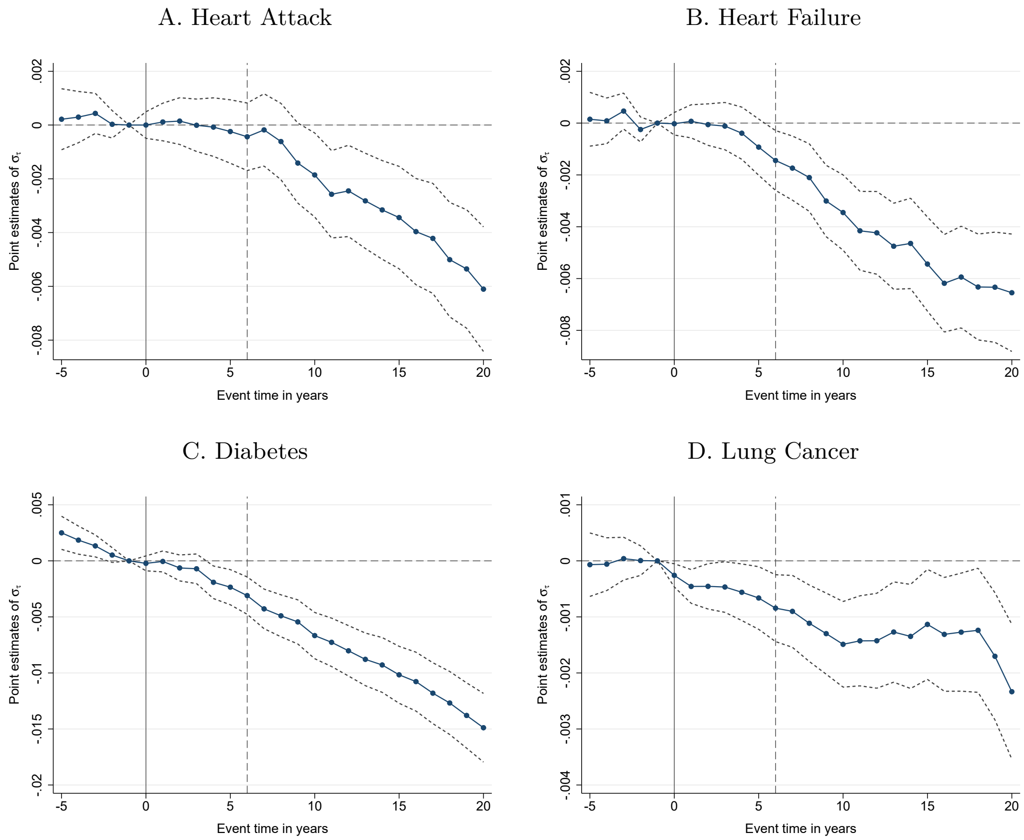
Panel A plots own income rank at age 30, 40, or 55 for the sample used in the medical school lottery analyses, individuals that are too young or too old to be observed income are not included in Panel A. Panel B plots own income rank at age 55 for the sample used in the lifestyle-related conditions index event study analyses (i.e., Panel B of Figure 5).

Figure A8: Doctor in the Family and Long-Run Health Bonus: Descriptive Evidence



Panel A records the cumulative share (y-axis) of individuals born in Sweden in 1936-1940, who have died by a given calendar year (x-axis). Panel B records the average age of the same individuals by calendar year, keeping deceased individuals in the sample. Panels C to F record the share of individuals born in Sweden between 1936 and 1961, who have acquired the specified chronic condition by a given calendar year (x-axis). Deceased individuals are kept in the balanced sample. In all panels the sample is restricted to individuals who at some point in their lifetime had a child matriculated in the study of either law or medicine. The outcomes are shown separately for the group of individuals whose child matriculated into medical (filled triangles) or legal (hollow circles) studies. We exclude observations if at least one parent is a health professional (a physician or a nurse) herself or himself. Parents with a child who became a nurse before another child became a doctor are not included in the "doctor" sample; parents that have a child trained as a lawyer and another child trained as a doctor (or nurse) are excluded from the "lawyer" sample.

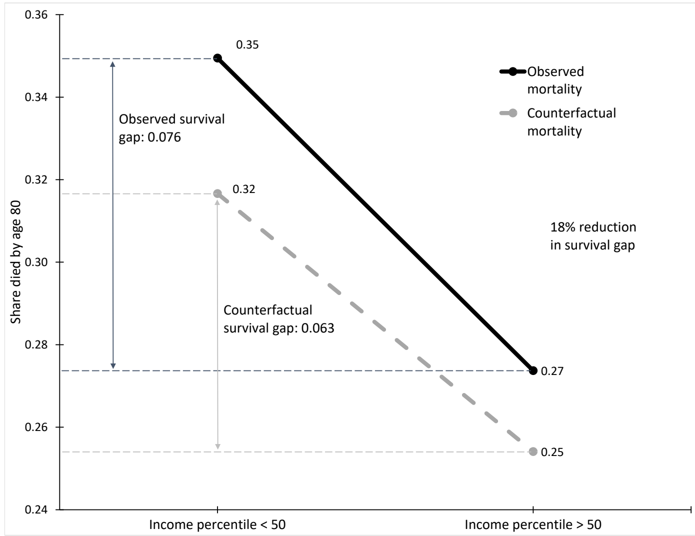
Figure A9: Doctor in the Family and Long-Run Health Bonus: Event Studies



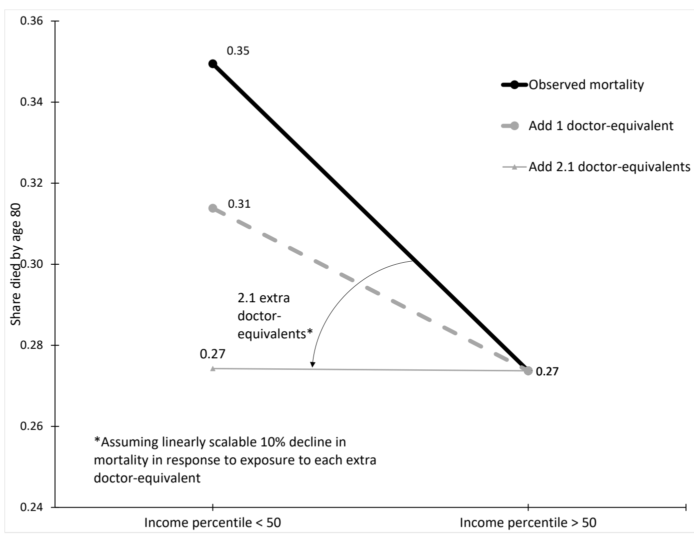
Figures plot coefficients σ_τ and 95% confidence intervals from the event study specification in Equation (4). The sample is restricted to family members of doctors and lawyers born in Sweden between 1936 and 1961. In both Panels, we exclude family members who are themselves a health professional, or have a health professional spouse. Family members with a relative became a nurse before another relative became a doctor are dropped from the "doctor" sample; family members with both a lawyer and a health professional relative are dropped from the "lawyer" sample. All panels exclude individuals that have died before the first year of clinical records (i.e., 1997). The regressions are centered at event year -1, i.e., one year before the year of matriculation in a medical or legal degree. The dashed vertical line marks the average graduation time for physicians. Standard errors are clustered at the family level.

Figure A10: Exposure to Expertise and Income-Mortality Gradient

A. Universal access to expertise



B. Health-income gradient in "doctor-equivalents"



Both panels plot observed and counterfactual gradients in mortality. The sample is defined as in Panel A of Figure 1. In both panels, the solid line ("observed mortality") plots observed average probability of individuals surviving until age 80 conditional on being alive at age 55, across the first half and the second half of the income rank distribution at age 55 (among 1936-1937 cohorts). In Panel A, the dashed line ("counterfactual mortality") is computed as follows. We multiply observed mortality within each income group by the treatment effect (1-T) and then re-scale the result by (1-T*s) to account for the differences in the underlying prevalence of access to expertise. T is the treatment effect of access to expertise (estimated at 10 percent for mortality, following the descriptive results in Appendix Table A1 and the event study estimates in Panel A of Figure 5); **s** is the share of individuals with health literacy at the baseline, proxied by the college completion rate (among 1936-1937 cohorts) of 7 percent and 31 percent percent at the bottom half and top half of the income distribution, respectively. The resulting formula for counterfactual mortality is: observed mortality within the income group times the treatment effect (1-0.1), divided by (1-0.1*0.07) for the first half of the income distribution, and divided by (1-0.1*0.31) for the second half. The denominator term re-scales the numerator to account for the differences in the baseline levels of access to expertise and hence the share of individuals that gets treated. In Panel B, the points on the dashed and light grey line are computed by applying 10 percent estimate of mortality reduction for exposure to each additional doctor at the bottom half of the income distribution.

Table A1: Health Professional in the Family and Health Outcomes

Panel A: Died, by Age 80

	(1) Income Decile 1	(2) Income Decile 2	(3) Income Decile 3	(4) Income Decile 4	(5) Income Decile 5	(6) Income Decile 6	(7) Income Decile 7	(8) Income Decile 8	(9) Income Decile 9	(10) Income Decile 10
Health Professional in Family	-0.059***	-0.058***	-0.073***	-0.039***	-0.043***	-0.045***	-0.045***	-0.026***	-0.035***	-0.036***
(no covariate)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)
Health Professional in Family	-0.030***	-0.034***	-0.044***	-0.019**	-0.021**	-0.027***	-0.023***	-0.018**	-0.025***	-0.028***
(full covariates)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Mean of Dep. Var.	0.41	0.36	0.34	0.31	0.30	0.30	0.29	0.27	0.26	0.23
Std. Dev. of Dep. Var.	0.49	0.48	0.47	0.46	0.46	0.46	0.46	0.44	0.44	0.42
Number of Observations	11,454	12,850	12,777	12,928	12,882	12,823	12,786	12,681	12,252	13,396

Panel B: Index of Lifestyle-Related Conditions, Age 55+

	(1) Income Decile 1	(2) Income Decile 2	(3) Income Decile 3	(4) Income Decile 4	(5) Income Decile 5	(6) Income Decile 6	(7) Income Decile 7	(8) Income Decile 8	(9) Income Decile 9	(10) Income Decile 10
Health Professional in Family	-0.008**	-0.017***	-0.016***	-0.013***	-0.016***	-0.007***	-0.009***	-0.009***	-0.008***	-0.005**
(no covariate)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Health Professional in Family	-0.007**	-0.020***	-0.013***	-0.010***	-0.013***	-0.006**	-0.009***	-0.008***	-0.007***	-0.007***
(full covariates)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Mean of Dep. Var.	0.07	0.04	0.02	-0.00	-0.01	-0.02	-0.02	-0.03	-0.04	-0.06
Std. Dev. of Dep. Var.	0.66	0.63	0.60	0.57	0.57	0.55	0.54	0.53	0.51	0.48
Number of Observations	191,979	215,131	215,099	215,522	214,393	213,476	211,081	205,246	203,877	213,377

Panel C: HPV Vaccine, by age 20

	(1) Income Decile 1	(2) Income Decile 2	(3) Income Decile 3	(4) Income Decile 4	(5) Income Decile 5	(6) Income Decile 6	(7) Income Decile 7	(8) Income Decile 8	(9) Income Decile 9	(10) Income Decile 10
Health Professional in Family	0.073***	0.070***	0.062***	0.058***	0.059***	0.067***	0.063***	0.049***	0.033***	0.060***
(no covariate)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Health Professional in Family	0.047***	0.054***	0.048***	0.041***	0.053***	0.049***	0.048***	0.031***	0.017*	0.046***
(full covariates)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
Mean of Dep. Var.	0.11	0.16	0.20	0.21	0.24	0.26	0.27	0.28	0.30	0.37
Std. Dev. of Dep. Var.	0.31	0.37	0.40	0.41	0.43	0.44	0.44	0.45	0.46	0.48
Number of Observations	11,327	12,568	12,516	12,469	12,539	12,427	12,511	12,500	12,471	13,683

Panel D: Tobacco Exposure, *in utero*

	(1) Income Decile 1	(2) Income Decile 2	(3) Income Decile 3	(4) Income Decile 4	(5) Income Decile 5	(6) Income Decile 6	(7) Income Decile 7	(8) Income Decile 8	(9) Income Decile 9	(10) Income Decile 10
Health Professional in Family	-0.066***	-0.076***	-0.069***	-0.066***	-0.058***	-0.056***	-0.051***	-0.041***	-0.028***	-0.015***
(no covariate)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Health Professional in Family	-0.046***	-0.033***	-0.025***	-0.022***	-0.012***	-0.012***	-0.007***	-0.003*	-0.000	0.001
(full covariates)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Mean of Dep. Var.	0.33	0.28	0.24	0.21	0.19	0.18	0.17	0.15	0.12	0.08
Std. Dev. of Dep. Var.	0.47	0.45	0.43	0.40	0.39	0.38	0.38	0.36	0.32	0.27
Number of Observations	162,203	188,606	191,519	192,978	193,265	193,549	193,522	192,928	191,628	205,000

Notes: Tables report the results of OLS regressions of the outcome of interest on an indicator for having a health professional in the family, estimated separately for each decile (reported in columns (1) to (10)) of the individual's (or parental) income rank. In each panel, the first regression includes no covariates and the second regression includes the full set of covariates. Health professional in the family is an indicator variable that equals one if the individual has at least one relative with a completed medical or nursing degree. In panels A and B, the set of relatives includes spouse, sibling, cousin, child, child-in-law, niece/nephew, grandchild. In panels C and D, the set of relatives includes sibling, cousin, parent, aunt/uncle, and grandparent. Panel A restricts the sample to individuals born in Sweden in 1936 and 1937. Panel B restricts the sample to individuals born in Sweden between 1936-1961 and alive at age 55 and year 1997 (first year of inpatient claims). Panel C restricts the sample to females born in Sweden between 1995-1997 and alive at age 20. Panel D restricts the sample to children born in Sweden between 1995 and 2016. Covariates in Panels A and B include fixed effects for individual's own income rank percentile and income rank percentile of the highest-earning relative, year of birth fixed effects, gender dummy, fixed effects for discretized education levels, fixed effects for the county of residence at age 55. Covariates in panel C include fixed effects for parental income percentile at birth, highest-earning relative's income percentile, year of birth, gender, mother's county of residence in the year before child birth. Covariates in panel D include fixed effects for parental income percentile at birth, highest-earning relative's income percentile, year of birth, gender, mother's county of residence in the year before child birth, maternal birth order, mother's education, maternal age. Standard errors are clustered at the family level. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table A2: Doctor in the Family and Health: Event Study Evidence

		Heterogeneity by					
		Income		Family Tie		Geographic Proximity	
	Pooled	Below Median	Above Median	Close	Far	Close	Far
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Heart Attack							
τ=-5 (τ: event year)	0.000	0.002	-0.001	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
τ=+10	-0.002*	-0.003*	-0.001	-0.002	-0.002	-0.002	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
τ=+15	-0.003***	-0.005**	-0.002	-0.004*	-0.003*	-0.003	-0.005***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Mean of Dep. Var. (at τ=+15)[a]	0.025	0.029	0.020	0.022	0.027	0.024	0.025
% Effect (at τ=+15)	12.0	17.2	10.0	18.2	11.1	12.5	20.0
Number of Observations	5,106,787	1,855,723	2,683,824	1,788,661	2,331,329	2,296,701	2,713,869
B. Heart Failure							
τ=-5	0.000	-0.000	0.000	-0.000	-0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
τ=+10	-0.003***	-0.004**	-0.002**	-0.003*	-0.003**	-0.005***	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
τ=+15	-0.005***	-0.005**	-0.003**	-0.005***	-0.005***	-0.006***	-0.005***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean of Dep. Var. (at τ=+15)[a]	0.022	0.025	0.015	0.019	0.025	0.022	0.021
% Effect (at τ=+15)	22.7	20.0	20.0	26.3	20.0	27.3	23.8
Number of Observations	5,106,787	1,855,723	2,683,824	1,788,661	2,331,329	2,296,701	2,713,869
C. Type II Diabetes							
τ=-5	0.003***	0.002	0.001	0.003*	0.003*	0.002*	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
τ=+10	-0.007***	-0.005**	-0.006***	-0.007***	-0.005**	-0.008***	-0.006***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
τ=+15	-0.010***	-0.006**	-0.010***	-0.011***	-0.008***	-0.013***	-0.008***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Mean of Dep. Var. (at τ=+15)[a]	0.044	0.049	0.034	0.039	0.048	0.046	0.043
% Effect (at τ=+15)	22.7	12.2	29.4	28.2	16.7	28.3	18.6
Number of Observations	5,106,787	1,855,723	2,683,824	1,788,661	2,331,329	2,296,701	2,713,869

Table A2: Doctor in the Family and Health: Event Study Evidence (Continued)

		Heterogeneity by					
		Income		Family Tie		Geographic Proximity	
	Pooled	Below Median	Above Median	Close	Far	Close	Far
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D. Lung Cancer							
τ=-5	-0.000	0.000	-0.000	-0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
τ=+10	-0.001***	-0.002***	-0.001	-0.002***	-0.001	-0.002***	-0.001**
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
τ=+15	-0.001*	-0.002	-0.001	-0.003**	-0.000	-0.002**	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean of Dep. Var. (at τ=+15)[a]	0.005	0.006	0.004	0.005	0.006	0.006	0.004
% Effect (at τ=+15)	20.0	33.3	25.0	60.0	0.0	33.3	0.0
Number of Observations	5,106,787	1,855,723	2,683,824	1,788,661	2,331,329	2,296,701	2,713,869

[a] Among family members of lawyers

Table reports coefficients σ_τ from the event study specification in Equation (4). The event time, sample restriction, and the set of family members included in the analysis are described in Section 3.4. Column (1) reports pooled results for 1936-1961 cohorts. Columns 2 and 3 split the sample by whether the individual's income rank within his/her gender-birth cohort is below or above the 50th percentile, with income measured at age 55. Individuals with a zero or negative income at age 55 are dropped from analyses. Columns 4 and 5 split the full sample by the type of family tie: parents-children in "close" family tie and aunts/uncles vs. nieces/nephews in "far." Individuals with both ties are excluded from analyses in Column 5. Columns 6 and 7 split the sample by geographic distance. Family members are classified as living "close" if their place of residence is recorded to be in the same county for more than 50 percent of the years between matriculation (into law or medicine) and the last year of data (2016). Coefficients are reported for event years -5, 10, and 15 (i.e. 5 years before, and 10, and 15 years after matriculation into the study of medicine or law). All regressions include the main effects and the interactions between event year dummies and the dummy for having a doctor in the (broad) family. The regressions further include the following covariates: age fixed effects, calendar year fixed effects, and individual fixed effects. Standard errors clustered by family are in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table A3: Survey Evidence on Health Literacy and Education

	Prefer Seeing Same Doctor (1)	Believe Doctor Always Tells Truth (2)	Regular Vegetables (3)	Regular Fruit (4)	Regular Sport (5)	Not Smoking (6)	Good Health (7)
No College Degree	0.06*	-0.07**	-0.19***	-0.16***	-0.06	-0.14***	-0.06**
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Weights Used	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Year	2004	2004	2014	2014	2014	2014	2004
Mean of Dep. Var.	0.70	0.28	0.77	0.55	0.56	0.84	0.76
Std. Dev. of Dep. Var.	0.46	0.45	0.42	0.50	0.50	0.36	0.43
Number of Observations	927	927	738	738	738	738	927

Table reports OLS relationship between the level of education and health-related behaviors. The analysis is based on 2004 and 2014 waves of the European Social Survey for Sweden. The sample is restricted to working age individuals between age 30 and 60. We regress the outcome of interest on an indicator for having no college education (defined as not having a "completed tertiary education"). The OLS regression uses post-stratification survey weights and controls for age fixed effects. Binary outcome variables were constructed from underlying categorical survey responses to the following 7 questions or statements: 1) "Prefer same doctor for all everyday health problems"; 2) "Doctors keep whole truth from patients"; 3) "How often eat vegetables or salad, excluding potatoes"; 4) "How often eat fruit, excluding drinking juice"; 5) "Do sports or other physical activity, how many of last 7 days"; 6) "Cigarettes smoking behavior"; 7) "Subjective general health". For example, "Prefer seeing the same doctor" takes the value of one if individuals answered "Same for all health problems" in response to the question of whether they "Prefer same doctor for all everyday health problems." *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

A Identification codes for diseases and drug use

Diseases We identify diseases using the following ICD-10 diagnosis codes:

Conditions	ICD-10 Codes
Heart Attack	I21, I22, I23
Heart Failure	I11, I13, I50
Type II Diabetes	E11, E13, E14
Lung Cancer	C34
Addiction	F10-F19
Injury/Poisoning	S0-S9, T0-T9
Respiratory Infection	J00-J06, J20-J22
Intestinal Infection	A00-A09
Chronic Tonsil Diseases	J35
Asthma	J45
Hypertension	I10
Hyperlipidemia	E78, I70
Ischemic Heart Diseases	I20-I25
Stroke	I60, I61, I63, I66, G45, G46
Pregnancy, Childbirth and the Puerperium	O00-O99

Drug use Drug use is identified based on the following Anatomical Therapeutic Chemical (ATC) codes:

Drugs	ATC Codes
Statins	C10AA
Blood Thinners	B01AC
Diabetes Drugs	A10B
Beta Blockers	C07
Asthma Drugs	R03
Vitamin D	A11CB, A11CC
Hormonal Contraceptives	G03A excluding G03AD
HPV Vaccine	J07BM

B Study cohorts and data years

We use these health care records to construct variables that capture health outcomes and health investments at various points in the life cycle. We use different cohorts to study different outcomes. This is a natural consequence of four facts: First, different outcomes are observed (and relevant) at different points in the life cycle. For example, we study mortality by age 80; this requires studying individuals who are old enough for us to know whether they were alive at that age. Second, our different health records span slightly different years. We have in- and outpatient claims and birth records through 2016, but drug claims and death records through 2017. Third, the cohorts we use depend on the observation window in the outcome. For example, since the inpatient data range is 1995 through 2016, to observe the number of inpatient stays through age five, we need to restrict the sample to individuals born between 1995 and 2011. Fourth, the cohorts that we use are also driven by the years for which our key sources of variation are available.

	Study cohorts	Data years
Panel A. Descriptive analysis		
Mortality by age 80	Birth cohorts 1936-1937	1961-2017
Lifestyle-related conditions	Birth cohorts 1936-1961	1997-2016
HPV vaccination	Birth cohorts 1995-1997, females	2005-2017
Tobacco exposure	Birth cohorts 1995-2016	1995-2016
Asthma, by age 5	Birth cohorts 2001-2011	2001-2016
High-risk birth	Birth cohorts 1995-2016	1995-2016
Number of inpatient stays, by age 5	Birth cohorts 1995-2011	1995-2016
Number of inpatient stays, age 45-50	Birth cohorts 1950-1966	1995-2016
Panel B. Lottery analysis		
Older family members		
Diseases/Preventable hospitalizations	Family members aged 50 or above of application cohorts 2007 fall-2008 fall[a]	2008-2016
Drug use other than vitamin D	Family members aged 50 or above of application cohorts 2007 fall-2009 fall[a]	2008-2017
Vitamin D	Female family members aged 50 or above of application cohorts 2007 fall-2009 fall[a]	2008-2017
Younger family members		
Outcomes except HPV vaccine/contraceptives	Family members aged below 25 and born before the application of application cohorts 2007 fall-2010 spring[a]	2008-2016
HPV vaccination	Female family members aged 10-25 of application cohorts 2007 fall-2010 spring[b]	2008-2017
Hormonal contraceptives	Female family members aged 10-20 of application cohorts 2007 fall-2010 spring[b]	2008-2017
Panel C. Event study		
Mortality	Birth cohorts 1936-1940	1961-2017
Lifestyle-related conditions	Birth cohorts 1936-1961	1997-2016

[a] Age refers to age at the year of the applicant's medical school application.
[b] Age refers to age at the end of the tracking period, i.e., six years after the applicant's medical school application.

C Examining zero-sum, and thus conceptually non-scalable, benefits of exposure to experts

To get a sense of how the "social capital" channel could operate and how likely it is to be quantitatively significant in our context, we discuss several examples of this channel on outcomes that we can capture in the data that reflect expensive or (in our setting) potentially rationed health care services.

First, we investigate whether family members of health professionals obtain more expensive heart attack treatments. The underlying idea is as follows. There are two common invasive therapies, one of which is substantially more expensive than the other, and one non-invasive (drug) therapy, which is the cheapest; under the "social capital" hypothesis, connected patients may be more likely to get a relatively expensive treatment option (holding severity of the condition constant).[48] We find no evidence of differences in the probability of getting an invasive (versus non-invasive) heart attack treatment across patients with and without a health professional in the extended family. Further, we find no difference in the intensity of invasive treatment conditional on getting an invasive (i.e. surgery rather than drugs) treatment.

Second, we investigate whether family members of health professionals have systematically longer stays in the hospital after childbirth (conditioning on a wide range of characteristics capturing postpartum maternal health and the child's health at birth). Despite the fact that the duration of postpartum care is generally rationed in the Swedish health care system – mothers are discharged as early as six hours after childbirth, while mothers in the U.S. are legally entitled to stay for up to 48 hours, depending on the state – we do not find any differences in the length of stay across patients with and without a health professional in the family.

Third, we examine the importance of the access to care channel for cancer treatment, as existing literature has documented that connections appear relevant for the choice and speed of cancer treatments Fiva et al. (2014). Here, we do find a smaller time window between the first diagnosis of breast cancer and breast cancer surgery among family members of health professionals. Interestingly, however, there is no pronounced income gradient in the prevalence of cancers, nor in mortality attributable to cancer.[49] This suggests that, at the population level, the access to care channel does not generate substantial differences in cancer-related outcomes across the income distribution.

In sum, the broader picture that emerges is that, while some non-scalable benefits may be at play in our empirical setting, our results allow us to argue that there *exists* an effect of exposure to expertise that does not run through social capital, that may be scalable, and that is currently being left on the table.[50]

[48] The two invasive therapies are coronary artery bypass grafting (CABG) and percutaneous coronary intervention (PCI), with the former being a more expensive open heart surgery.

[49] In fact, if anything, we observe an *inverse* SES-gradient in cancers, which likely is driven by competing risks with cardiovascular diseases as well as more screening at the upper end of the income distribution. The exception is lung cancer; however, it accounts for a very small share of all cancers.

[50] Our results are consistent with the idea that historically, the "extensive" margin of disease prevention through changes in social and individual investments into health - that we hypothesize can be affected by access to health expertise - has had a substantially more pronounced effect on population health than the "intensive" margin of moving from a lower to a higher quality of care provider, or getting care faster within a given system (Cutler and Lleras-Muney 2008).

D Further related literature

D.1 Early childhood interventions

A growing literature has documented that early life interventions have a positive effect on infant mortality and can promote health in the long-run, suggesting that conditions in infancy are a relevant source of health and socioeconomic disparities in later life. A Nurse–Family Partnership program in the US that provides regular home visits by certified nurses to low-income mothers from early pregnancy until the child reaches the age of two, has been found to have positive effects on birth outcomes and health in childhood (Agency for Healthcare Research and Quality 2014). For a universal home visiting program implemented in Denmark, Wüst (2012) show that the intervention had a positive and significant effect on the infant first-year survival rate in Danish towns and was most effective in the majority of small and medium-sized municipalities. The authors suggests that the main driver of the program's impact was the promotion of breastfeeding and appropriate infant nutrition by visiting nurses. In a related study, Hjort et al. (2017) examine the long-term impact of the Danish home visiting program. They find that treated individuals that were visited by nurses in infancy experience better health mid-life: they have lower mortality rates, spend fewer nights at hospital, and are less likely to be diagnosed with cardiovascular diseases. Similarly, Bütikofer et al. (2019) investigate the long-term impact of mother and child health care centers in Norway and find that the increasing access to well-child visits had a positive effect on health, education and earning of treated infants when they reach age 30 to 40. Moreover, the authors find a stronger impact for children from lower socioeconomic background. Similarly, Sweden saw the introduction of a nurse home visiting program in the early 1930's and Bhalotra et al. (2017) find that, in the long-run, the infant care provided by nurse home visits reduced the probability of dying by age 75 by seven percent.

D.2 Community health workers and access to primary care

Community health workers (CHWs) have been employed in many countries to provide health-related services to their fellow community members. Although there has not been many rigorous evaluations, most existing evidence suggests that CHWs increase takeup rates of a wide variety of healthy behaviors and improve disease management in the community, notably for health behaviors such as cancer screening and immunization, and management of diseases such as asthma, hypertension, and diabetes (see e.g., Norris et al. 2006, Haines et al. 2007, and Najafizada et al. 2015). In addition, by assisting individuals in navigating the health care system, CHWs have been shown to improve access to medical services, especially for marginalized populations (Felix et al. 2011, Najafizada et al. 2015).

Access to primary care in the community setting has also been found to be an effective way to improve patients' health. Bailey and Goodman-Bacon (2015) use the rollout of community health centers (CHCs) in the U.S. from 1965 to 1974 to study the long-term health benefits of increased access to primary health care for the poor. The paper finds that, in one decade after CHCs were established, CHCs reduced age-adjusted all-cause mortality rates by 7 to 13 percent among the poor aged 50 and older, with the reduction primarily driven by the decline in cardiovascular-related deaths. The authors argue that having access to a regular source of care, lower medication cost, and improved compliance with prescription drugs were the main mechanisms for the effects of CHCs on mortality.

Moreover, a growing body of evidence suggests that the ease of access to nurses improves the health of patients with chronic conditions. Fergenbaum et al. (2015) present a systematic review of six randomized control trials that study the effects of home visits with nurse-led guidance in disease self-care management. Home visiting programs result in fewer hospitalizations, fewer emergency department visits, and better patient quality of life. Studies on nurse-led clinics that provide disease knowledge and support for disease self-care management report similar health effects: these clinics significantly reduce patient emergency department visits, hospital readmissions, and mortality rates, and improve patient medication adherence (Agvall et al. 2013, Gandhi et al. 2017, and Liljeroos and Strömberg 2019).

D.3 Patient education

An extensive body of work has evaluated the effectiveness of patient education interventions, finding such programs to be generally effective in promoting population health. For chronic diseases, Stenberg et al. (2018) review existing studies - 56 face-to-face intervention among patients living with chronic illess - on the impact of education programs that target chronic obstructive pulmonary disease (COPD), asthma, chronic pain, heart disease, and diabetes patients. The authors find that, regardless of study design and time horizon, interventions that promote patient education are beneficial in terms of decreased hospital admissions, fewer visits to emergency departments or general practitioners, and in terms of increased quality-adjusted life-years

Similarly, Wang et al. (2017) review randomized control trials that investigate effects of self-management education among patients with COPD. The paper highlights that such education programs improve patient disease-specific knowledge and quality of life, and reduce respiratory-related hospital admissions and emergency department visits. Anderson et al. (2017) focus on the educational component of cardiac rehabilitation for patients with coronary heart diseases. The study reviews 22 randomized control trials that assigned patients to different educational interventions that ranged from face-to-face counseling to residential stays with follow-up sessions. Patients in control groups received usual medical care in cardiac rehab that comprises exercise counseling and training and psychological support. The paper finds that, although there is limited evidence that education-based interventions reduce total mortality, the risk of a heart attack, or the number of hospitalizations, these interventions result in lower risks of cardiovascular events and better quality of life. Similarly, Menichetti et al. (2018) review randomized control trials that promote patient engagement among older adults with osteoporosis, diabetes or cardiovascular-related health problems. The authors find that such interventions often demonstrate positive effects on patient compliance with treatment regimens.

In the context of health behaviors, Aveyard et al. (2012) and Stead et al. (2013) show that medical advice and provision of behavioral or pharmaceutical assistance on smoking cessation increase the frequency and success of smoking cessation attempts. Kaner et al. (2018) review the literature on alcohol interventions provided by health professionals and conclude positive effects of these interventions on reducing excessive alcohol consumption. For weight control, Aveyard et al. (2016) show that a randomized trial that provides interventions delivered by trained general practitioners improves body weight control among obese patients.

Another strand of literature examines the effects of public health education campaigns promoted by social media. A comprehensive summary of this literature can be found in Giustini et al. (2018). Many topics have been included in these social-media education campaigns, including health behaviors such as smoking cessation, healthy diet and physical activity (see, e.g., Chang et al. 2013, Williams et al. 2014, Swanton et al. 2015, and Chakraborty et al. 2018), and prevention and management of diseases such as diabetes and cancer (Gabarron et al. 2018, Han et al. 2018). Existing studies generally suggest positive effects of these campaigns on population health.

E Interpreting magnitudes in "doctor-equivalents"

We consider a stylized setting that divides individuals in our sample into two groups – those in the top and bottom halves of the income distribution, respectively. In the data, the average probability of dying by age 80, conditional on having survived until age 55, is 35 percent in the first half of the income distribution and 27 percent in the second half of the income distribution. These two data moments are plotted as a solid line in Panel A of Appendix Figure A10. From our estimates of treatment effects in Appendix Table A1 and Table 4, we have that exposure to a physician in the family leads to a 10 percent decline in mortality (on average). If the health benefits from exposure to expertise scaled linearly with each doctor in the family, it would take a difference of 2.1 "doctor-equivalents" on average between families at the top and the bottom of the income distribution to account for the full difference in mortality.[51]

Naturally, the *levels* of intra-family exposure that we observe in the data are much lower – in practice, exposure to doctors happens not only through family members, but also through friends, neighbors, and colleagues. What's more, health literacy does not only stem from relatives who are health professionals, but from a range of sources, all of which are likely to be more readily available at the top of the income distribution (Kindig et al. 2004). The gradient in exposure between the first and second half of the income distribution that we observe in the data, however, is striking: While individuals at the top half of the income distribution have on average 0.26 physicians in the family, those at the bottom have on average 0.05, or five times fewer, doctors in the family. To get 2 more "doctor-equivalents" at the top of the income distribution, keeping the gradient the same, we would need to have at least 2.5 "doctor-equivalents" at the top of the income distribution on average and 0.5 at the bottom. Another way to summarize these magnitudes, staying for simplicity with exposure to physicians per se, is as follows. If individuals in the top half of the income distribution on average know one doctor, while only every fifth individual knows a doctor at the bottom of the income distribution, then this difference alone could account for a third of the health-income gradient.

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