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The Roots of Health Inequality and the Value of Intrafamily Expertise†

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In the context of Sweden, we show that having a doctor in the family raises preventive health investments throughout the life cycle, improves physical health, and prolongs life. Two quasi-experimental research designs—medical school admission 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.* (*JEL* D15, G22, I12, I13, I14, I18)

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, Lubotsky, and Paxson 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

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[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 conditions in utero or genetic capital may perpetuate economic inequality and Black, Devereux, and Salvanes (2007); Oreopoulos et al. (2008); Almond and Mazumder (2011); Bharadwaj, Eberhard, and Neilson (2018); and Bharadwaj, Lundborg, and Rooth (2018) for more evidence on the causal relationship between early-life health and future economic or health outcomes. See also

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 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 and 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 the perceived value of beneficial health investments to family members, or build trust in the health care system. We focus on outcomes related to the prevalence of lifestyle-related conditions and preventive health investments. These outcomes are nonrival, which makes them highly 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 I, 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 channels of health insurance and access to health care.[4]

In Section II 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. Furthermore, children are substantially

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. See also 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 literature (mostly in US settings) that has investigated the causal effect of health insurance—which lowers the price and increases ease of access to formal health care—on long-run and short-run health outcomes (see, e.g., Sommers, Gawande, and Baicker 2017 and Finkelstein, Mahoney, and Notowidigdo 2018).

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 their families may still yield a biased estimate of the effect of exposure to intrafamily expertise if unobservables 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 the 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 whose members won and lost the admission lottery. Our results from this lottery analysis are consistent with our descriptive findings and show far-reaching health benefits for the admitted applicants' families. 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 or 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 members receive either a medical degree or a law degree.[5] We find striking differences in the health and mortality profiles of these 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. Gaining 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 period of time.

There are two main interpretations of our results, which we discuss in Section IIIA. One is that having a doctor in the family delivers benefits that are intrinsically scarce. Physicians in the family may use their professional clout to get their family members better or more timely care. If so, the health benefits enjoyed by doctors' family members are zero sum, in the sense that they may come at the

[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.

expense of others in society. Under this interpretation, our results may identify a deeply rooted source of inequality. A second interpretation is that having a doctor in the family improves health literacy and delivers health behavior changes and health benefits that are not intrinsically scarce, and thus not zero sum. If doctors get their family members to undertake behavioral changes that are beneficial for health and 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 for their family members.

While the data do not allow us to perfectly adjudicate between the two interpretations, our results clearly establish impacts on outcomes that are not zero sum. For example, doctors appear to increase their relatives' take-up of vaccines and their use of preventive drugs that are cheap and readily available, and to reduce their tobacco exposure in utero and their lifestyle-related chronic diseases. Such benefits 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 family members experiencing nonfatal heart attacks or strokes, finding that spouses and adult children increase their consumption of preventive care (cholesterol-lowering medication) in response.[7] We focus instead on familial transfers of health-related expertise.

Our focus on health-related expertise relates this 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, Oosterbeek, and de Wolf (2013) further find that being a doctor leads to small improvements in self-reported health, but

[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 in Section IV.

[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.

a decline in physical exercise. Frakes, Gruber, and Jena (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 willingness to pay for branded drugs.[8] Another strand of this literature focuses not on physicians themselves, but on how exposure to intrafamily expertise affects the 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 to medical guidelines differently than individuals without access to health expertise. Also closely related to our paper is work by Artmann, Oosterbeek, and van der Klaauw (forthcoming), who use Dutch medical school lotteries to estimate the impact of having a child who is a physician 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 paint a diverging picture, overall. As we discuss in Section IIIA, 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, Oosterbeek, and van der Klaauw (forthcoming) to estimate effects at different points in the distribution of treatment effects, 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, Palme, and Simeonova 2018), and intrafamily 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—about expertise, broadly defined, or 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]

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

[9] See, e.g., Fuchs (1992, 2004); Currie (2011); Cullen, Cummins, and Fuchs (2012); Cesarini et al. (2016); Currie and Schwandt (2016); Dwyer-Lindgren et al. (2017); Almond, Currie, and Duque (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, by contrast, suggest that intrafamily expertise elicits a weakly larger response at the lower end of the income distribution.

I. 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.

A. *Data*

Population and Demographic Information.—The backbone of our data is an extract from the Total Population Register (Statistics Sweden 2013) consisting of all individuals born between 1936 and 2016 residing in Sweden from 2000 through 2016. From Statistics Sweden, we obtained a file that connects each individual in this sample to their spouse or cohabiting partner as well as to their (dead or living) parents, siblings, grandparents, children, and cousins. From the 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 the 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, that is, age 16 or older (Statistics Sweden, n.d.). 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 wages and self-employment income in the prior year and rank individuals within their birth cohorts and genders. To construct a child's income rank at birth, we calculate the sum of both parents' work and business incomes measured one and two years before the child's birth, respectively, and rank them 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 additional educational records for the year 2007 and beyond. First, we add high school GPA from Skolverkets elevregister (Statistics Sweden 2013). 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, or *högskoleprovet* (Universitets- och högskolerådet 2020). Second, we add college application information from Universitets- och högskoleregistret (Statistics Sweden 2013). 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 is 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

[11] We use a CPI inflator (Federal Reserve Bank of St. Louis) before constructing the income ranks.

information from various registers collected by the National Board of Health and Welfare (Socialstyrelsen 2019): 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 (International Classification of Diseases, Tenth Revision; i.e., ICD-10). Drug records contain 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 can potentially respond to access to expertise. In addition, a key constraint, of course, is that the outcomes need to be observable in our data.

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 2 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 exercise 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 the use of chronic medications that are 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 usage of Vitamin D among older women (for whom this vitamin is recommended) 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 alcohol or drug addiction.

[12] Online 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 online Appendix Section B for details.

[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, Driver, and Gaziano 2009).

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 the 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 can observe take-up in our data.

We 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 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 in child health even earlier in life, we use information from the medical birth records to construct an indicator for whether the mother was using tobacco immediately before or during pregnancy, which is known to be associated with substantial risks to the fetus (Centers for Disease Control and Prevention 2020).

B. *Inequality in Health in our Empirical Setting*

Sweden is a particularly attractive empirical context in which to examine health inequality. In addition to the 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 cycle.

[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 underlying mechanism(s) with certainty.

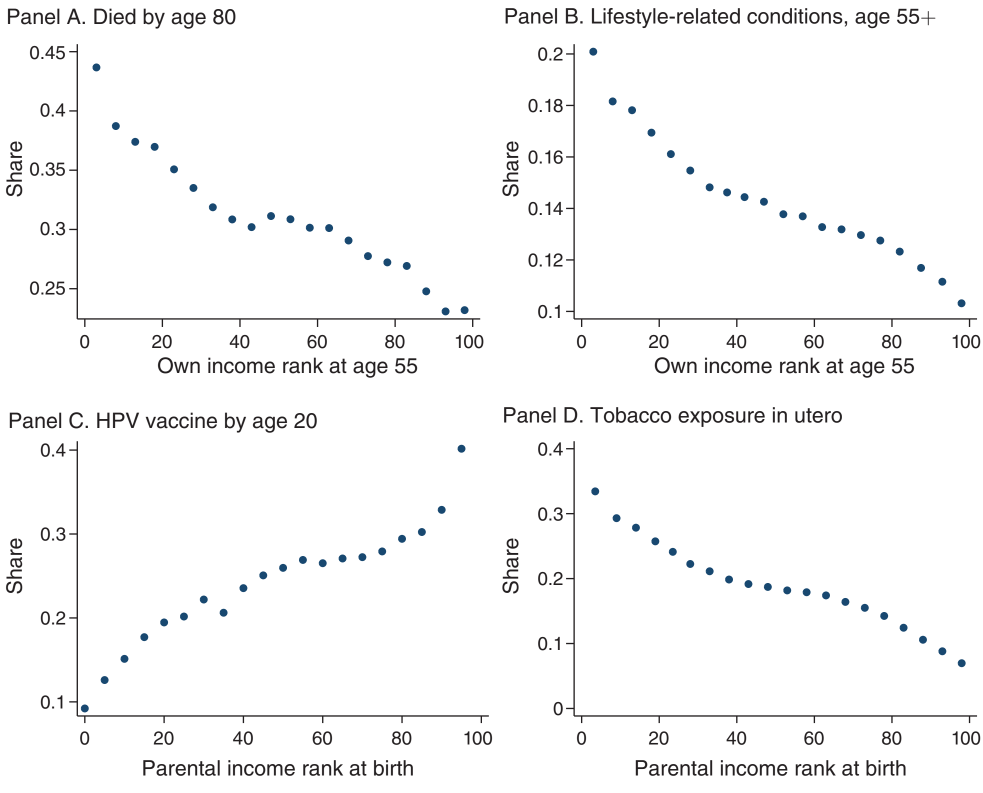

Figure 1. Income Gradients in Mortality and Morbidity over the Life cycle

Notes: 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 2 diabetes. Panel A restricts the sample to individuals born in Sweden between 1936 and 1937; panel B restricts the sample to individuals born in Sweden between 1936 and 1961 and alive at age 55 and year 1997 (first year of inpatient claims). Panel C restricts the sample to females born between 1995 and 1997 and alive at age 20. Tobacco exposure in utero in panel D measures whether the mother used any type of tobacco within three months before or during pregnancy; the sample is restricted to children born between 1995 and 2016.

To study inequality in mortality, we start with all individuals who are alive at age 55 and for whom we can define our longevity measure (alive at age 80).[15] Figure 1, panel A plots the share of individuals who are alive at 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.

[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.

It is instructive to put this in relation to the income-mortality gradient that has recently been documented in the United States. Online 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 compared directly to those reported in Chetty et al. (2016). We observe substantially lower mortality at any point on the relative income distribution in Sweden than in the United States, 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 United States among 75-year-olds but lower in Sweden at younger ages, especially among women.

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

For younger adults, Figure 1, panel C 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 ventile of the income distribution are vaccinated against HPV, while 40 percent of women with parents at the top ventile are.

Even earlier in life, Figure 1, panel D depicts a remarkably sharp gradient in exposure to maternal tobacco 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 online Appendix Figure A2 displays the gradient in the number of inpatient visits in the first five years of life. While we already observe a pronounced gradient at age five, it steepens substantially over the course of the life cycle, as illustrated in panel B of the same figure, which displays the 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 takeaway points. 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.

[16] Many slowly emerging chronic conditions are frequently underdiagnosed. 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.

II. Exposure to Health Expertise and Health Outcomes

A. *Measuring Exposure to Health Expertise*

We are interested in measuring whether exposure to health-related expertise affects individuals' investments in their health and their subsequent health outcomes. Exposure to expertise may affect individuals through multiple mechanisms, many of which the public health literature commonly refers to as health literacy (Kindig, Panzer, and Nielsen-Bohlman 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; and they can help to determine when to seek formal care. Doctors may also be able to use their clout to help family members navigate the health care system—a channel that we view as distinct from health literacy, and to which we will return in our discussion of zero-sum benefits to expertise in Section IIIA.

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 on health is challenging, as 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, possess 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 II, we analyze the aggregate impact of having a health professional in the family on the family's health; we then return to a discussion of interpretations in Section IIIA.

B. *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 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. Figure 2, panel A revisits the mortality gradient from Figure 1, panel A but now plots it separately for individuals with and without a health professional in the narrow or extended family.[17] We drop observations

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

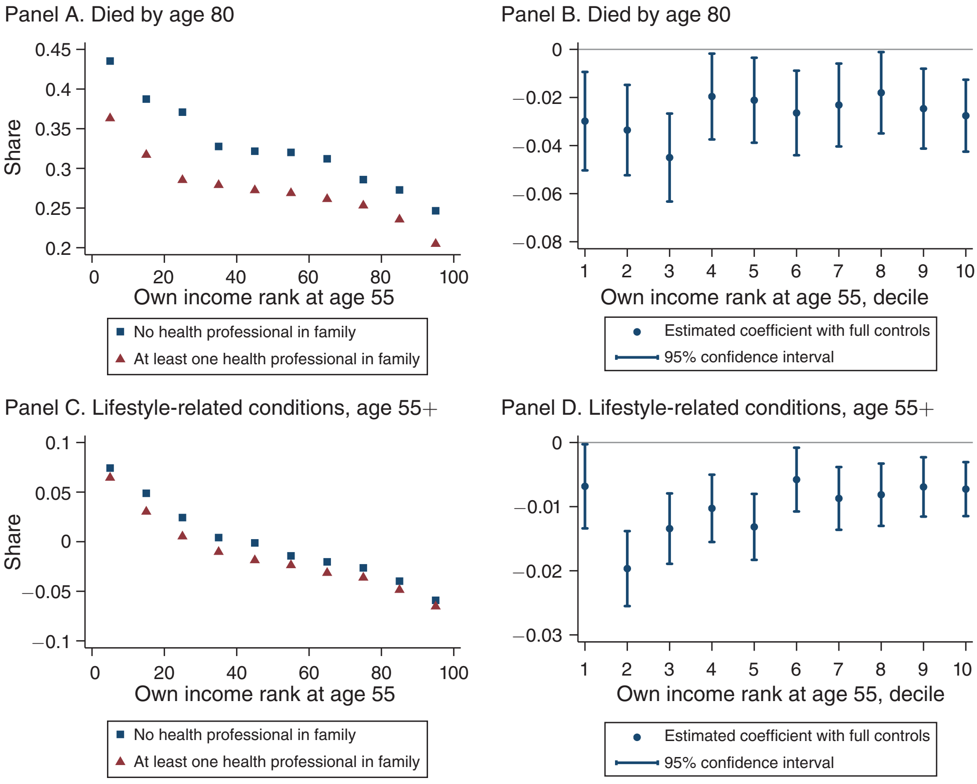

Figure 2. Health Professional in the Family and Health at Older Ages: Descriptive Evidence

Notes: 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 2 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 or not. Individuals are assigned to the subsample "at least one health professional in family" 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 individuals' own income rank percentiles and the income rank percentiles of their highest-earning relatives, 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 percent confidence intervals. Standard errors are clustered at the family level.

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]

Figure 2, panel A 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

calculating income ranks or to replacing individual income rank with household income rank.

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

a 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 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 seventieth to one hundredth percentile in the income rank distribution. Furthermore, 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 online 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:

$$Y_{id} = \delta_d HP_i + \beta_d \mathbf{X}_i + \epsilon_{id}. \tag{1}$$

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 $\mathbf{X}_i$ 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, which measure the average difference in health outcomes between individuals with and without a health professional in the family for each age-55 income decile d, conditional on the demographic controls. We plot the point estimates from this regression in Figure 2, panel B. 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 Figure 2, panel C, 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 (narrow or 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 show a visible separation in the prevalence of these chronic conditions between individuals with and without a health professional in their 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 online 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

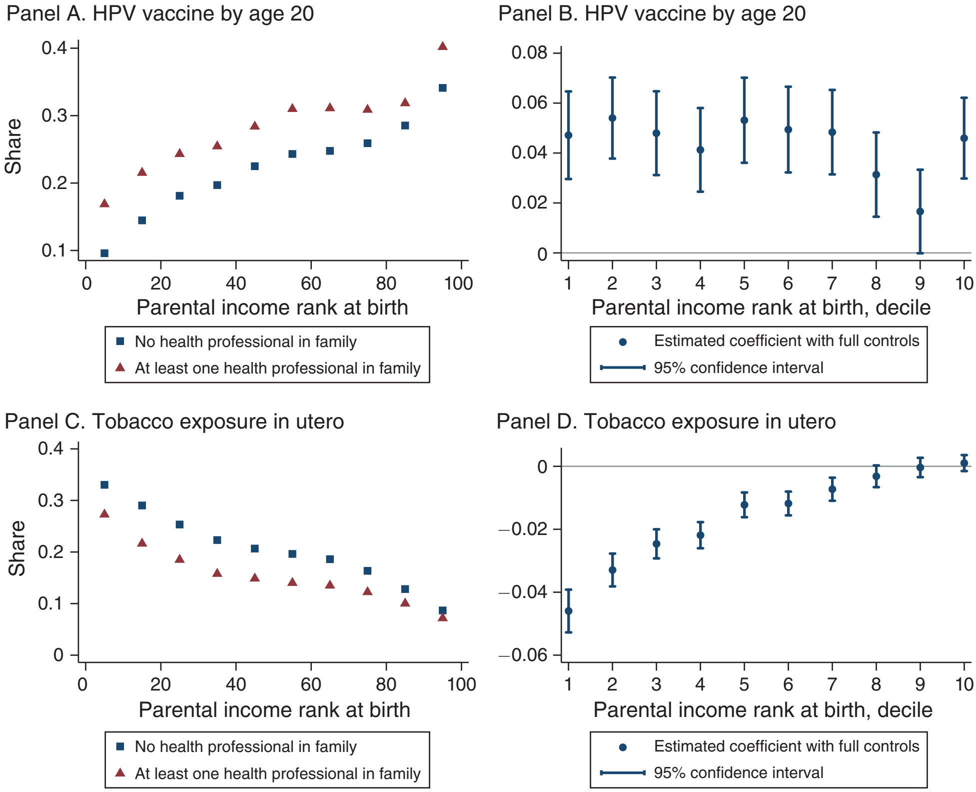

Figure 3. Health Professional in the Family and Health at Younger Ages: Descriptive Evidence

Notes: Panels A and C plot the shares of individuals with the specified health condition by decile of parental income rank at birth. We start with the same samples defined in Figure 1. The samples are split by whether an individual has a health professional in the family or not. Individuals are assigned to the subsample "at least one health professional in the family" 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 also include fixed effects for birth order, mother's education, and maternal age. Vertical lines indicate 95 percent confidence intervals. Standard errors are clustered at the family level.

among older individuals with health professionals in their families. Moreover, the difference remains larger at lower income levels, on average.

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 online Appendix Table A1 as well as panel B of Figure 3.[19]

[19] As 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

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), 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 exposed to tobacco in utero than a child who has no medical professional in the family. As panel D of Figure 3 and panel D in online Appendix Table A1 document, the gap in tobacco exposure rates monotonically declines with income rank and approaches 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. Furthermore, observable differences cannot fully explain the pattern of the gap decreasing monotonically with income rank.

Online Appendix Figures A5 and A6 examine the heterogeneity in our descriptive results along the intensive margin 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 panels in online Appendix Figures A5 and A6 report the estimated differences in health outcomes between individuals without any relative who is a health professional versus those with a broad, but not narrow, relative who is a health professional (dashed lines); and individuals without any relative who is a health professional 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 panels report the same coefficients but split the sample by geographically close (solid line) and far (dashed line) relatives who are health professionals. 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 having 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 further away in the family tree has little effect on the prevalence of lifestyle-related conditions after age 55 (online Appendix Figure A5, panel C), while the effect of having a close relative who is

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 also 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 who 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 up to 20 percentage points lower relative to the general population. Figure A4 in the online Appendix illustrates this striking difference.

[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 people, while the smallest (Gotland) has about 59,000 people.

a health professional is pronounced. The results are more mixed for geographic proximity. Online Appendix Figure A5, panel B 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 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, Gruber, and Jena (2021); and Finkelstein et al. (2021), a remaining concern is that the presence of a health professional in the family may be correlated with unobservables. We therefore use two quasi-experimental strategies to quantify the causal impact on health and longevity of having a health professional in the family.

C. *Leveraging Randomization in Medical School Admissions*

We exploit the fact that admission to medical school in Sweden contained an element of randomization for a subset of years.[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-undergraduate 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.

[22] There are no tuition fees for postsecondary 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).

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 increase steadily 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 admission 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 during 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 admissions were 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 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 reapply in subsequent application cycles.[26] The possibility of reapplication implies that individuals who are not admitted in a particular cycle may still eventually gain admission

[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 nonmandatory 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. 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 admission 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.

[24] Strictly speaking, the admission procedure has two rounds. The first round allocates admission offers by lottery and assigns applicants who are not offered any 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 chances of admission by taking the SAT and attempting admission through the alternative quota or by working.

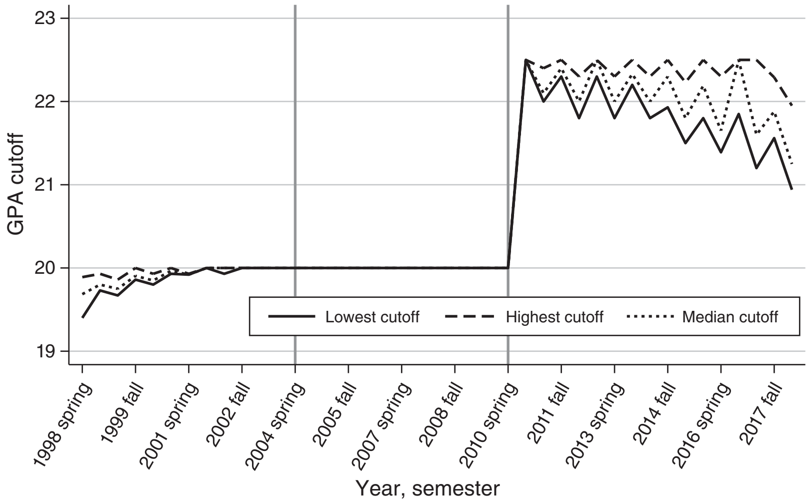

Figure 4. GPA Cutoffs for Admission into Medical Programs

Notes: 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.

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 in 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:

$$Y_{j(i)} = \delta MD_i + \beta_1 \mathbf{x}_{j(i)} + \kappa_1 \mathbf{X}_i + \epsilon_1; \tag{2}$$

$$MD_i = \gamma A_i + \beta_2 \mathbf{x}_{j(i)} + \kappa_2 \mathbf{X}_i + \epsilon_2. \tag{3}$$

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 family members, both narrow and extended), 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 six years, and the first two years after medical school completion. MD_i is an indicator variable that takes a value of one if applicant i matriculated into a medical program. $\mathbf{X}_i$ and $\mathbf{x}_{j(i)}$ are vectors of observable demographics of applicant i and 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.

Furthermore, we control for two variables that mechanically affect the probability of admission (to any program): the 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 receive 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 and who had a GPA of exactly 20.0. Our sample of family members includes all of their grandparents, parents, parents-in-law, spouses, 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 reapply), the corresponding figure is 59 percent, giving us a large and precise first stage of 37 percentage points 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

[27] Sweden had 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 admission 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.

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)	
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 have a high school GPA of 20.0 and applied to at least one medical school for the first time during the Fall 2007 through Spring 2010 application cycles. 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.

group). They had similar ages, although the accepted group is statistically (but not economically) significantly older (19.80 in the accepted group versus 19.57 in the rejected group).[28] Accepted and rejected applicants were equally likely to be born

[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—misclassify the first application cycle and capture individuals who gained work experience before reapplying. The difference in age shrinks substantially when we zoom in on small subsamples, where we can more conservatively

in Sweden and to have parents who were born in Sweden. Both groups had similar parental household incomes and fathers' incomes, 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, mother, or one of their grandparents by the year before the first application to medical school in the admitted and nonadmitted 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 paternal (maternal) grandmothers were deceased prior to the student's application.

In sum, 15 out of 16 observables are balanced across the admitted and nonadmitted 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 students with the highest GPA. 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 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 age 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 IA: lifestyle-related physical health conditions and preventive health investments. We also aggregate the specific outcomes into a single health index. All outcomes are scaled per 1,000 individuals to aid in interpretation.

Columns 1, 2, and 3 report the 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 who 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 who 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 reapply 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 the "control complier mean," following Kling, Liebman, and Katz (2007). This is what we report in column 5.

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.

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)
Panel A. Health index	40	48	121	0	4	3,134
	(16)	(18)	(45)			
Panel 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 2 diabetes	−11	8	15	77	72	1,532
	(14)	(15)	(29)			
Panel 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 from estimating Equation (2) for older (age 50 or above) family members of medical school lottery participants. Outcomes are tracked for eight years after the applicant's matriculation into a medical school after their 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 2 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 their 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 their first medical school application attempt and did not subsequently reapply to medical schools.

Physical Health.—We find that 42 out of 1,000 individuals in the control group had a heart attack during our observation period. Among individuals whose relative won 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 (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 2 diabetes or lung cancer.

Preventive Investments.—Our first group of preventive investments captures the 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 from 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). Furthermore, older family members of lottery winners are 20 percent (ITT) more likely to purchase diabetes medication conditional on having the disease. Older female relatives also take prescription-strength (rather than over-the-counter) vitamin D 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 the 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, Liebman, and Katz (2007). We first orient all outcomes in the same qualitative direction (for example, for statins, more 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

[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 a 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 the composite effect of higher exposure to several cardiovascular medications at a time—which we are also documenting in this section—is unknown.

[30] We condition these regressions on the presence of the following chronic conditions: asthma, type 2 diabetes, heart failure, ischemic heart diseases, stroke, hyperlipidemia, and hypertension. Thus, we can interpret our estimates as the effect on purchasing these drugs conditional on needing them.

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 in 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 under age 25. We measure health outcomes starting in year $t + 1$ after matriculation until year $t + 6$.[32]

Columns 1, 2, and 3 again report the ITT effects without covariates, with covariates, and the LATE, respectively. Among female relatives age 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 between age 10 and 20 do not use hormonal contraception in the control group, 135 more women (21 percent) refrain from this form of contraception among those with a lottery winner (on the first application attempt) in their family (ITT).

Furthermore, we find being exposed to a health professional in the family to have large effects 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 being exposed to a health professional in the family reduces the number of inpatient stays by about two-thirds.[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

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

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

TABLE 3—DOCTOR IN THE FAMILY AND HEALTH AT YOUNGER 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)
Panel A. Health index	31	32	118	−1	37	4,113
	(18)	(18)	(67)			
Panel 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)			
Panel 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 age 25) of medical school lottery participants. Outcomes are tracked for six years after the applicant's matriculation into a medical school after their last medical school application. In row 2 (number of inpatient stays), we drop observations above the ninety-ninth percentile of the distribution in inpatient stays. Row 6 (HPV vaccination) and row 7 (no hormonal contraceptives) restrict the sample to females between age 10 and 25 and females between age 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 their 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 their first medical school application attempt and did not subsequently reapply to medical schools.

summarize our eight measures into a health index, constructed in the same way as for 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]

[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.

D. *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 IIC, 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 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 socioeconomic statuses, with income distributions that are skewed toward the top ventiles. (Online Appendix Figure A7 plots the income distributions.) Moreover, admission 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 begins for families after an individual starts medical training. We use the first medical doctor (or the first lawyer) in a family matriculating 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 a trend in the health of their 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' family members. 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. Online Appendix Figure A8 documents raw differences in the probability of adverse health outcomes between individuals with a child who received a medical degree versus individuals with a child who received 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 spouse who is a health professional.[37] In this sample, we compute the share of individuals who died in each calendar year starting with 1980 (i.e., starting when the individuals are age 40 to

[36] We do not require that students pursue the medical profession after college, although in practice the vast majority do.

[37] For parents of lawyers, we further exclude those who had a child who became a nurse; for parents of physicians, we exclude those who had a child who became a nurse before another child became a doctor.

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 ages. (This is what we would expect absent any dramatic—and unlikely—differences in the probability of having a child who is a lawyer or a doctor 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 die at a slower rate than parents of lawyers. The difference becomes economically and statistically significant over time, reaching 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 a level of less than 1 percent.[38]

In panels C through F, we repeat a similar exercise for the four chronic, lifestyle-related conditions: heart attacks, heart failure, type 2 diabetes, and lung cancer. As our medical claims data start much later than our mortality data, we track individuals starting in the year 1997 (until 2016). To be able to observe individuals prior to older age, when the onset of conditions is likely to have started already, 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 ages 35 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 child who is a lawyer and a child who is a doctor. Eventually, significant differences emerge, 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 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 sample of parents of lawyers), 4 per 1,000 fewer cases of heart failure (10 percent fewer relative to the base of 40 per 1,000 among parents of lawyers), 8 per 1,000 fewer cases of type 2 diabetes (11 percent fewer relative to 76 per 1,000 among parents of lawyers), and 2 per 1,000 fewer cases of lung cancer (18 percent fewer relative to parents of lawyers' 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. Furthermore, we observe nontrivial differences in the incidence of chronic conditions and mortality between these two groups of parents at older ages, despite their largely similar socioeconomic standing (as illustrated in online Appendix Figure A7).

[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.

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

$$(4) \qquad Y_{it} = \alpha_i + \sum_{\tau} \sigma_\tau D_{\tau,it} \times Doc_i + \sum_{\tau} \kappa_\tau D_{\tau,it} + \gamma_t + \beta \times \mathbf{X}_{it} + \epsilon_{it}.$$

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, and 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. Calendar-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. $\mathbf{X}_{it}$ 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 coefficient of interest is σ_τ, which measures the impact of the first health professional arriving in the family on the family members' health relative to the arrival of a lawyer in 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 we allow the data to reveal any changes in 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 estimated relative to the year before the matriculation. 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.

[39] For all individuals for whom we observe the year in which they acquire a medical (law) degree, we count back six (five) years to define the matriculation year, as these are the common lengths of the undergraduate medical (legal) programs. In event studies that examine mortality, we consider cohorts born in 1936 to 1940. In 17 percent of cases for doctors' relatives and 21 percent of cases for lawyers' 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, if the high school graduation year is not observed, the year the individual turned 19. 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.

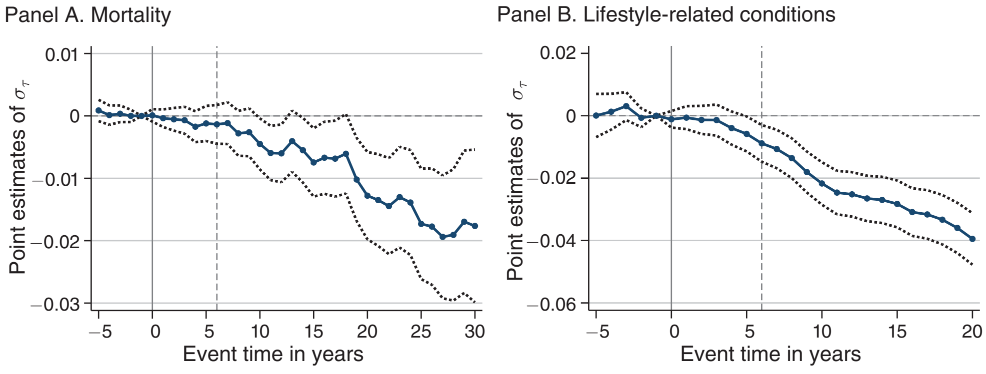

FIGURE 5. DOCTOR IN THE FAMILY AND LONG-RUN HEALTH BONUS: EVENT STUDIES

Notes: The panels plot coefficients σ_τ and 95 percent 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 health professionals or have a spouse who is a health professional. 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 relative who is a lawyer and a relative who is a health professional are dropped from the "lawyer" sample. Panel B excludes individuals who 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 for a medical or legal degree. The dashed vertical line marks the average graduation time for physicians. Standard errors are clustered at the family level.

Panel A illustrates the impact of having a family member trained as a physician on the probability of death.[40] We observe a clear slowdown in the relative mortality rate among relatives of doctors (as compared to the relatives of lawyers), which starts emerging around year eight after the young relative matriculates. The mortality gap then widens steadily for two decades. As Table 4, column 1 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 from the mean among relatives of lawyers, which is 16 percent.[41] Figure 5, panel B 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 2 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 five after matriculation. The divergence widens for two decades after matriculation, as can also be seen in Table 4, panel B, column 1.

We report separate event study results for the four chronic conditions underlying the lifestyle index in online Appendix Table A2, and present them graphically in

[40] The outcome is an indicator taking the value of one if the individual has died by time t and the value of zero otherwise (i.e., if the individual is still alive at 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 10.0 percent effect on the probability of death at event year +25 in the balanced panel and a 10.4 percent effect in our baseline specification with the full sample.

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

		Heterogeneity by					
		Income					
		Below	Above	Family tie		Geographic proximity	
	Pooled	median	median	Close	Distant	Close	Distant
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel 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
Percent effect (at τ=+15/25)	10.4	18.6	17.2	11.3	12.0	31.3	12.5
Observations	1,232,438	1,140,251	1,661,958	463,724	478,190	1,346,729	1,612,541
Panel 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
Percent effect (at τ=+15)	NA	NA	NA	NA	NA	NA	NA
Observations	5,106,719	1,855,563	2,683,800	1,788,661	2,331,308	2,296,651	2,713,836

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 IID. Column 1 reports results 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 fiftieth 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 child who is a doctor is classified as having a close tie, and individuals with a doctor elsewhere in the family are classified as having a distant tie. An individual who has both types of family ties is included in column 4 but not column 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 places of residence are 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 as distant otherwise. An individual who has both types of geographic ties is included in column 6 but not column 7. The lifestyle conditions index in panel B is constructed as the mean of the z-scores of indicators for heart attack, heart failure, type 2 diabetes, and lung cancer; by construction, the index is normalized to zero for the control group (i.e., lawyers' family members). All regressions include the main effects and the interactions between event-year dummies and the dummy for having a doctor in the 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 reported in parentheses.

[a] Among family members of lawyers.

online Appendix Figure A9. We observe that differences in the probability of having a heart attack emerge around year nine after matriculation and for heart failure, around year five. The differences persist and expand over time. We observe a similarly pronounced divergence in the incidence of type 2 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 and in the family tree. For the income-based and geographically-based heterogeneity in mortality, we switch to younger cohorts, from 1946 to 1955, for whom income and geographic information is available. Columns 2 through 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 by exposure to expertise: we find an 8 percent higher relative impact on mortality in the first half of the income distribution. Furthermore, 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 who are closer family members.

III. Health-Related Expertise and Health Inequality

A. *Mechanisms*

There are two broad interpretations of the health rents that we document in Section II. First, these rents may stem from having access to someone inside the health care system, so that family members of health professionals are better able to access scarce resources, e.g., 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 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 do 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 may potentially 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

[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 nonscalable mechanisms empirically in online Appendix Section C.

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, and reminders, nudges, and "nagging" rather than from any costly or 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) to society. Similarly, one patient obtaining the HPV vaccine does not preclude another patient from receiving the same (low-cost) vaccine. Furthermore, we documented a reduction in the number of inpatient stays among young family members of health professionals; under any nonscalable mechanism, we would have expected the reverse.

In sum, many of the outcomes that respond to having a health professional in the family are not inherently 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 professional clout or connections to the health care system are unlikely to matter. Indeed, in our lottery analysis, the treated group was exposed to a young physician—either in medical school or the first two years after completing the degree—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, Oosterbeek, and van der Klaauw (forthcoming) yields another suggestive piece of evidence in support of the "scalable benefits" interpretation of our results. In the Dutch setting, more than 40 percent 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 of the health care system). Consistent with this, we find treatment effects on outcomes capturing scalable benefits in the Swedish context, while Artmann, Oosterbeek, and van der Klaauw find no treatment effects on either mortality or the use of the health care

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

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]

B. *Interpreting Magnitudes*

To put the magnitude of our estimates in context, we conduct a stylized exercise, asking how the mortality-income gradient would change in our empirical context if we—in addition to providing universal health insurance—adopted a hypothetical "universal access to expertise" policy that leads to everyone in society achieving the same level of health literacy as that 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 IV and discuss the literature on related policies in online 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 access to expertise" policy, we start with observed mortality. We then let our hypothetical policy move the percentage of individuals who 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 health literacy benefits so that a

[45] Other differences in institutional settings between the two countries also likely contribute to a different set of results in Artmann, Oosterbeek and van der Klaauw (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 faces 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; furthermore, they document quantitatively important intrafamily 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 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, Oosterbeek, and de Wolf 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.

[46] In online 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 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 multidimensional survey measures of health literacy and formal education in Europe (see, e.g., Sørensen et al. 2015). Using data from the European Social Survey (ESS), we have 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. Online Appendix Table A3 reports these results.

treatment effect of 10 percent is attainable for the whole population and thus presents an upper bound. This calculation, illustrated in panel A of online Appendix Figure A10, suggests that the mortality-income gap would shrink from the observed mortality gap of 0.076 between the top and bottom halves of the income distribution to 0.063 under our hypothetical "universal access to expertise" policy. 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 socioeconomic 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.

IV. Discussion and Conclusion

Growing evidence across various disciplines reveals stark correlations between health capital throughout the life cycle 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 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 socioeconomic 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 contribute 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, studying individuals who have a health professional in their families. Using descriptive evidence and event studies and exploiting admission lotteries for 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 suggests 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 care, 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 stem in part 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 able to mimic what health professionals do for their family members would have the potential to make a substantial dent in population health and to reduce health inequality. Indeed, our analysis suggests that exposure to a health expert at least partially improves health through low-tech (and cheap), nonrivalrous determinants of health such as the 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 intrafamily communication and whether a policy maker may be able to replicate some of them. If intrafamily health professionals simply transfer common knowledge to their family members, then incorporating health literacy as a standard part of school curricula, along with information provision campaigns, may be effective.[47] To the extent that the power of intrafamily communication about health stems from the trust or detailed knowledge about 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 policies 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—in terms of geographic location and administrative hurdles—general practitioner who knows patients, and potentially their whole families, over long periods of time. Further, Alsan, Garrick, and Graziani (2019) suggest that health care professionals who resemble their patients (in their study, same-race providers) may gain more trust. Finally, given the heterogeneous effects across the income distribution, such programs may have the largest potential to reduce health inequality if they were specifically targeted at the poor. Understanding the patterns that underlie the intrafamily 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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