

Comment on McGovern „Comparing the Relationship Between Stature and Later Life Health in Six Low and Middle Income Countries“

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Epidemiologists, economists, and researchers of many other stripes have devoted much effort to understanding the links between early-life health and outcomes later in the lifecycle. Some of this interest can be traced to the *fetal origins hypothesis* (Barker 1995)—which posits that *in utero* conditions affect adult health—but much of it extends beyond gestation, into the early stages of childhood. Research on this topic has relied on two approaches (Currie and Vogl 2013). The first focuses on the effects of specific insults to early-life health, such as disease or nutritional deprivation; the second relies on proxies for early-life health, most commonly birth weight or height. While the first typically bears a more direct link with policies aimed at affecting early-life health, the second can lead to a richer depiction of the later-life sequela of various early-life health shocks, jointly considered. Birth weight is of course set before birth, but height too is largely determined in the early years of life (Tanner 1978). As such, these anthropometric measures serve as useful retrospective proxies for health during this formative period. However, their associations with adult outcomes are not always easy to interpret, posing an impediment to the proxy approach. Comparisons of these associations across different study settings or time periods can aid tremendously in the interpretation of their magnitudes.

In this sense, McGovern’s paper in this issue does a great service to the literature by providing estimates of height-health associations in adults from several continents. Using survey data from China, Ghana, Mexico, Russia, and South Africa, McGovern studies how height correlates with self-reported health and disability, grip strength, and lung function. He estimates both linear regressions and concentration indices, allowing for a fuller description of the joint distributions under study. These analyses within each country are careful and thorough, but the study contributes the most—and poses its greatest puzzle—in enabling us to make comparisons across countries. The puzzle: the rank order of height-health associations across countries differs substantially by the measure of adult health. Among men, for example, height-health associations in Ghana and Mexico are relatively weak for self-reported health status and disability, relatively strong for lung function, and diverge for grip strength, with a weak association in Mexico and a stronger one in Ghana. While these ambiguous results may to some extent reflect sampling variability, the paper goads us to ask why the true associations might differ across populations.¹

So how to conceptualize cross-country differences in these cross-sectional associations? And what to make of the variation across self-reported health, self-reported disability, grip strength, and lung function? Following Currie and Vogl (2013), one can gain insight into these issues by writing linear production functions capturing the health effects of a composite measure of early-life health, z , and a vector of other childhood characteristics, X (e.g., ethnicity or parental education). Two production

¹McGovern does not provide test statistics for differences in coefficients, but one can use the appendix tables to construct t -statistics for differences between any two coefficients. For many outcomes, the difference between the weakest and strongest coefficients has a t -statistic greater than 3.

functions are necessary, one for an adult health outcome (y) and one for adult height (h):

$$\begin{aligned} y &= \alpha_y + \beta_y z + X' \Gamma_y + \varepsilon_y \\ h &= \alpha_h + \beta_h z + X' \Gamma_h + \varepsilon_h \end{aligned}$$

The random disturbances ε_y and ε_h are assumed to be uncorrelated with each other, with z , and with X . β_y and β_h represent the health effects of early-life health. However, because z is typically unobservable, researchers like McGovern regress y on h :

$$y = a + bh + X'C + u$$

The researcher may or may not control for X in this regression, depending on his opinion of whether characteristics like ethnicity or parental education affect adult height and health through their effects on early-life health or through some other mechanism. If these effects work primarily through early-life health, then the inclusion of X as a covariate can decrease statistical power and, under some measurement error assumptions, lead to over controlling. McGovern does, so the probability limit of the estimated slope coefficient is:

$$\text{plim } \hat{b} = \frac{\beta_y}{\beta_h + \frac{\sigma_h^2}{\beta_h \sigma_{z|X}^2}}$$

where σ_h^2 is the variance of the disturbance ε_h , and $\sigma_{z|X}^2$ is the variance of the childhood conditions index z after linearly conditioning on X . Under the assumption that β_y and β_h are positive, $\text{plim } \hat{b}$ is positive as well.

This expression highlights two potential reasons for slope coefficients to differ across populations and outcomes. First, as the variance of early-life health ($\sigma_{z|X}^2$) increases relative to the variance of the other determinants of height (σ_h^2), the slope coefficient increases. In other words, the slope coefficient is large when the signal-to-noise ratio of height as a measure of early-life health is large. This source of variation in slope coefficients is general to the population and does not depend on the choice of adult health outcome. Second, as the effect of early-life health on adult health increases (β_y), so too does the slope coefficient. Unlike the first, this source of variation in slope coefficients is specific to the adult health outcome (within a given population).

Given this reasoning, cross-country comparisons would be most interpretable if the coefficients for different health outcomes had the same rank order across countries. Then one could interpret them through the lens of population-level differences in the signal-to-noise ratio $\frac{\beta_h \sigma_{z|X}^2}{\sigma_h^2}$, which would presumably reflect variability in food accessibility and the disease environment. In fact, if the effect of early-life health on adult health were the same in all populations, then variation in the signal-to-noise ratio would imply that the proportional differences in coefficients across countries would be the same for all health outcomes.

However, the data show neither constant proportional differences nor even similar rankings of

countries across adult health outcomes. Within the framework presented here, one must conclude that part of the variation in slope coefficients comes from variation in β_y , the effect of early-life health on adult health outcomes. As useful as his estimates are, McGovern does not provide us with a framework for thinking about why a country might show a strong height-health association for one health outcome but a weak association for another. Perhaps the answer lies in differences in the types of physical labor performed, or in health behaviors, or in health care or social support systems, or in one of many other possibilities. Perhaps as important as the paper's statistical results is its implicit challenge to formulate a broad framework that can explain these cross-country variations.

References

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