
Do Old Fallacies Ever Die?

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Communication

Do Old Fallacies Ever Die?

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In 1933, Harold Hotelling reviewed a book by Horace Secrist, entitled *The Triumph of Mediocrity in Business*, in which Secrist presented evidence purporting to show that business enterprises were tending to converge in size. Hotelling pointed out that Secrist's evidence did not justify his conclusions. For a number of different variables, Secrist had plotted

averages of groups, arrayed according to the value of the variable in the first year of the series. If the concerns were arrayed according to the values taken by the variable in the last year of the series, the lines would diverge. . . . The seeming convergence is a statistical fallacy, resulting from the method of grouping. . . .

The real test of a tendency to convergence would be in showing a consistent diminution of variance, not among means of groups, but among individual enterprises. (Hotelling 1933, p. 464)

In 1991, Jeffrey G. Williamson reviewed a book by William J. Baumol, Sue Anne Batey Blackman, and Edward N. Wolff, *Productivity and American Leadership*. One thesis of the book is that the rates of growth of various countries have tended to converge. As it happens, their thesis appears to be correct, whereas, according to Hotelling, Secrist's was not. But both the reviewer and the book cite evidence for convergence that is tainted by the statistical fallacy that Hotelling called attention to almost sixty years earlier.

To quote from Williamson's review, "Figure 4 [reproduced here as Figure 1] confirms the convergence thesis: The bigger the productivity gap in 1950, the faster the growth 1950–79"

(1991, pp. 57–58). The figure does no such thing. Suppose we do as Hotelling suggests and use the terminal year rather than the initial year in plotting the rates of growth. The result is Figure 2. Japan and the U.S. clearly show convergence even on this biased basis. But the other countries show essentially no correlation: the calculated regression coefficient is positive, but not statistically significant. Figure 2 does support the convergence thesis, but only because it shows so much less close a positive correlation than it would if the regression fallacy stressed by Hotelling were alone operative.

The book does, while the review does not, present in Figure 5.2 (Baumol, Blackman, and Wolff 1989), reproduced here as Figure 3, what Hotelling called "The real test of a tendency to convergence." These plots of the coefficient of variation of GDP per work-hour and GDP per capita do show, in Hotelling's words, "a consistent diminution of variance . . . among individual" countries. Figures 5.1 and 5.4 in the book are also relevant evidence, untainted by the regression fallacy.¹ But that is not true of three other figures (5.3, 5.5, and 5.6), all of which are tainted.

I find it surprising that the reviewer and the authors, all of whom are distinguished econo-

¹ Moses Abramovitz has called my attention to a significant qualification of Hotelling's "real test": it implicitly assumes that measurement error (or transitory variance) is the same in the years compared. That qualification may be minor for the data in Figures 2 and 3 for the post-World War II period. However, improvements in the accuracy of the data may well account for a significant part, even if not all, of the decline in dispersion in Figure 3 from 1860–1940.

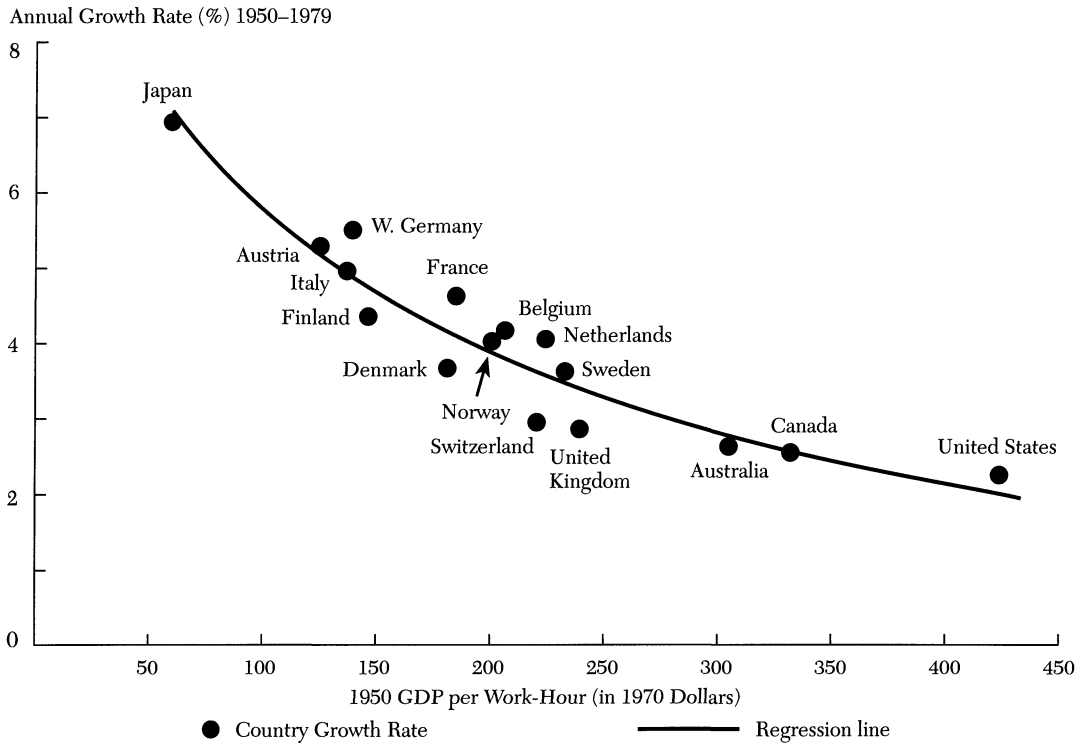


Figure 1. Postwar Productivity (GDP per work hour) Growth Rate, 16 Industrialized Countries

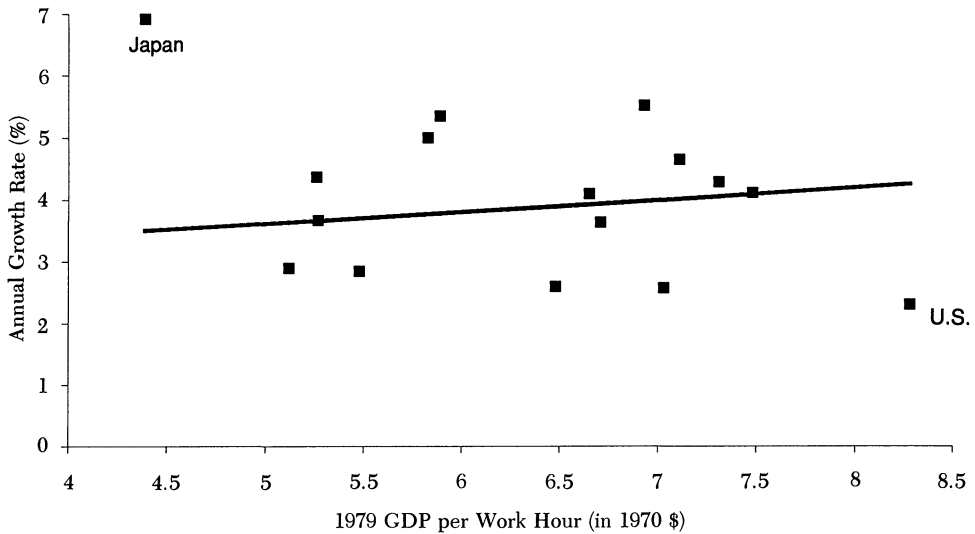


Figure 2. Growth Rate of Productivity vs Terminal Level, 1950-1979

(NOTE: Regression Excludes Japan and U.S.)

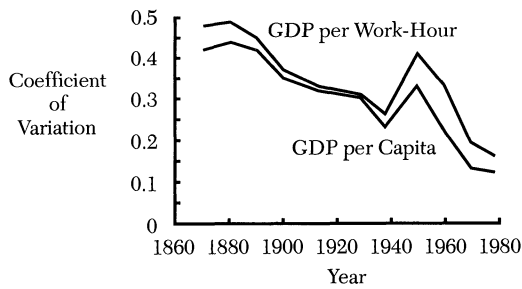


Figure 3. Coefficients of Variation, GDP per Work-Hour and GDP per Capita, 16 Countries, 1870–1979

mists, thoroughly conversant with modern statistical methods, should have failed to recognize that they were guilty of the regression fallacy. After all, the phenomenon in question is what gave regression analysis its name.² However, surprise may not be justified in light of the ubiquity of the fallacy both in popular discussion and in academic studies.

For example, “everyone knows” that job creation comes mainly from small firms. That proposition may be true but the evidence offered for it that I have seen classifies firms by size in an initial year and traces subsequent levels of employment—precisely what Secrist did. I have yet to see what the data show if firms are classified by their terminal size, or by their average size over a period.

Similarly, in academic studies, the common practice is to regress a variable Y on a vector of variables X and then accept the regression coefficients as supposedly unbiased estimates of structural parameters, without recognizing that all variables are only proxies for the variables of real interest, if only because of measurement error, though generally also because of transitory factors that are peripheral to the subject under consideration. I suspect that the regression fallacy is the most common fallacy in the statistical analysis of economic data, allevi-

² Galton examined the heights of fathers and sons, and found that the sons of tall fathers tended to be shorter than their fathers, i.e., regressed toward the mean; similarly, the fathers of tall sons tended to be shorter than their sons, i.e., regressed toward the mean. This is simply the well-known phenomenon that in a linear regression of x and y , the regression of y on x is flatter relative to the x axis than the regression of x on y .

ated only occasionally by consideration of the bias introduced when “all variables are subject to error.”

As a student of Hotelling not long after his review had been published, I early became aware of the regression fallacy—or, perhaps better, trap. That knowledge came in handy when Simon Kuznets and I studied the relation between incomes of the same individuals in different years. To avoid the trap, I introduced the concept of permanent and transitory components of income, in the process referring to Hotelling’s review (Friedman and Kuznets 1945, p. 331).

Later, in my *Theory of the Consumption Function* (1957), the regression fallacy came to my aid in resolving the apparent conflict between budget studies and Kuznets’ time series data. It was generally believed that budget studies had demonstrated that the marginal propensity to consume was greater than the average propensity, i.e., that the higher the income, the lower the fraction that would be spent on consumption. Yet Kuznets’ time series data showed no such tendency.

The explanation was that budget studies almost invariably classified consumer units by income and then calculated averaged consumption for various income classes. Few if any consumer units were in the lowest income class despite having an unusually high income; many were there because of an unusually low income; their permanent income exceeded their measured income, and conversely at the other end of the scale. Consequently, it was not surprising that units with low measured incomes would spend on consumption a higher fraction of their measured income than units with high measured income. The only budget study for which I could get average income for units classified by consumption spending showed the expected reverse relation. Indeed, the regression fallacy was the seed out of which my permanent income hypothesis grew (Friedman 1957, ch. 3, and pp. 201–02).³

Williamson’s review and the book he

³ Still later, in our book *Monetary Trends*, Anna Schwartz and I allowed for the regression effect by systematically reporting upper and lower limits of computed parameters derived by reversing independent and dependent variables in regressions. See Friedman and Schwartz (1982, esp. ch. 5, fns. 28 and 29, pp. 173–74; ch. 6, p. 227).

reviewed are excellent vehicles for demonstrating the difficulty of rooting out appealing fallacies, precisely because both are of unusually high quality, and are marred in only a minor way by having fallen into the regression trap.

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