


Diamond makes a lot of other arguments surrounding his central one, ranging from why all domesticable mammals (such as horses) were killed off in America (answer: humans came suddenly, not giving the animals time to evolve as wary; so the "hunters" looking for an easy meal, or maybe merely that nice task, just walked up to the mastodons and horses and corralled them on the beach, Indo-style) or why the Austronesian (e.g. Hawaiian) explosion from (of all places) Taiwan did not overwhelm the New Guinean highlanders (answer: the highlanders had developed food production very successfully, and so could outnumber the invaders).

Interesting. But my point here is that in making his arguments Diamond does science. He doesn't do what economists, without acquaintance with any alleged science but their own, persist in imagining is science. Diamond is not big on phony, existencetheorem math or phony, significance-testing statistics. (The temptation to be so must be considerable, since the neighboring field of population biology, like economics, is in love with the cargo-cult techniques perfected after World War II, axiomatic and significance-test gaming.) He is big on quantitative arguments based on factual matter, argument that have simple

For example, in arguing the case for New Guinea as a test of how important food production is in causing societies to flourish he uses new linguistic evidence on the origins of Micronesian and Polynesian languages, such as their crop vocabularies. He uses the diversity of American Indian languages (shown nonetheless by Joseph Greenberg and his school to be at root one) as evidence of how hard it was for cultures in the Americas carrying biological innovations (corn, for example) to spread quickly—in contrast for instance to the rapid expansion of the Indo-Europeans on horseback.

The philosopher of history R. G. Collingwood, himself a historian of Roman Britain, once defined "scientific" history (by contrast with "scissors-and-paste" history) as studying problems, not periods, asking questions about the world, and seeing one's way to answering them. He notes that a scientist is neither a theorist-philosopher speculating about whether an endogenous-growth model has equilibrium solutions under assumptions x, y, or z nor a scissors-and-paste economometrician rummaging in bad data for significant coefficients; she is on the contrary a maker of testable arguments about real worlds, like a detective.

Diamond does science, I say. He's a detective. At a session of the Economic History Association last year in Los Angeles I heard him talk about his book, after I had gobbled it up (as you will, right?). After Diamond spoke, our own Jeff Sachs gave a similar presentation of his new ideas about geography and underdevelopment—especially the immense yet unaddressed challenges of public health in tropical climates. Consider Africa. Consider AIDS. Or "just" malaria and sleeping sickness. Jeff's scope was the past two centuries, and the century to come, though even this looks a little narrow beside Diamond's 13,000 years. Still, they both were making the same point—biological location matters a lot for economic growth—and making it the same way. Sachs, like Diamond, is a detective, a scientist. So we can all be, if we'll stop spending our valuable time on the non-scientific talk about things "existing." (The economics students in France, by the way, are in open rebellion against Cartesian-Samuelsonian-Arrowian economics; they have taken to calling it "autistic" economics. Aax barricades?)
Observe that Diamond is "just" a botanist and Ferguson is "just" a non-quantitative historian and Tompkins is "just" an English professor. Not physicists. Not mathematicians. Not a significance test in hundreds and hundreds of well-written pages. Not a theorem in sight. Yet all three are really serious about knowing things about the work. So the issue is not "science vs. the humanities" or some other simplistic philosophy of knowledge. What we seek is science in the usual non-English sense, "inquiry."

Get with it, oh my beloved fellow economists. Read, and get that queasy feeling in the pit of your stomach. Read the linguistic Merritt Ruhlen's *The Origin of Language.* Read the literary critic Stanley Fish's *Surprised by Sin.* Read, and compare what these scientists do in discussing language families and Milton's epic poems with the pseudo-science that makes nonsense of even the best articles in our splendid field. And if your stomach really comes to bother you, get Howard Spiro's great book, *Clinical Gastroenterology,* beautifully written (about bowels), steadily quantitative (about ulcers), a detective's guide to gut science.

REFERENCES


BOOK REVIEWS


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This short book on empirical modeling consists of three chapters. Two are the 1998 Marshall Lectures that Granger gave at Cambridge University. These provide undergraduate-level discussions and focus on how empirical economists construct and evaluate models. Chapter 1 discusses problems with model construction, and Chapter 2 discusses problems with model evaluation. Chapter 3 adds to the Marshall Lectures by providing a more technical treatment of how to evaluate economic models.

In Chapter 1, Granger uses the example of deforestation of the Amazon rainforest to discuss model building. The message is that empirical researchers should think more about what they are doing. Variables are difficult to define and measure, and different people or different data-collection mechanisms can obtain inconsistent measurements. We are warned about using data-mined results for model assessment and evaluation. We are introduced to the methodological minefield of economic theory, hypothesis testing, and data exploration that all applied economists must navigate. In short, Granger presents these aspects of empirical model-building that we spent too little time on in graduate school. Chapter 1 helps us see the forest through the trees.

All faculty and graduate students would benefit from reading Chapter 1, particularly teachers of econometrics. Today, econometrics courses are largely technique driven. Data and measurement issues are seldom confronted. Students are typically handed data sets and given model specifications. The "teaching" then begins as the students are asked to check for anomalies and prescribe remedies. Students are not taught to ask whether the patient's vitals are correct, whether the patient is worth saving, or how to know when the patient is cured.

In Chapter 2, Granger discusses the evaluation of empirical models. How does one know if an empirical model is any good? Granger argues that we should evaluate the output of the model (out-of-sample predictions) rather than sample statistics such as t-ratios and the value of $R^2$. Problems with evaluating or testing a specific theory are demonstrated by a series of examples. The first example is Robert Hall's theory of consumption. Granger points out many difficulties in evaluating this simple theory. The next example is the evaluation of forecasts in finance. Here Granger points out that forecast evaluation is well-developed compared to other empirical research, but is by no means complete. The point is made that forecast errors should be evaluated in terms of the costs of being wrong, which need not be symmetric. Because consumers of forecast information may have different cost functions, producers of forecasts should provide the entire conditional distribution of predictions. This would allow consumers with different cost functions to make better use of the forecast information. The concept of sto-