

Thoughts on AJR's Nobel Prize
Jón Steinsson
October 2024 (and September 2025)

The announcement that Acemoglu, Johnson, and Robinson will be awarded the 2024 Nobel Prize in Economics has given rise to very strong reactions both positive and negative. This is unsurprising. AJR's Colonial Origins paper has been among the most influential, but also most controversial, papers in Economics ever since it was published.

AJR's detractors make many arguments. I will focus on only one of these arguments. This is the argument that AJR don't deserve their Prize because there was already a large literature prior to their work that developed the idea that institutions are crucial for economic development; that all AJR did was to recast ideas previously developed among economic historians, political scientists, historians, and other scholars in the causal inference language of modern economics. Worse still, some will argue, the use of modern econometric methods is exactly where things went wrong: their data is bad, their instrument is invalid, their standard errors are wrong, etc., etc.

I strongly disagree with this line of thought. Let me try to explain.

The literature that preceded AJR's work largely did not use formal econometric methods. Let's take Douglas North's work as an example. North is a good comparison for several reasons. His work is widely celebrated. (He was awarded the Nobel Prize in 1993.) He worked on the same set of issues as AJR. And some of AJR's detractors point specifically to North's work and say: what do we learn from AJR that we didn't already know from North's work?

North's work contains both a wealth of information and sophisticated arguments. But it is largely informal. For concreteness, consider some of North's most influential pieces of work: his books *Institutions, Institutional Change, and Economics Performance* and *Structure and Change in Economic History* and his article "Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England" (joint with Barry Weingast). This work uses a rich array of facts to support the arguments that are made. But it uses very little formal data. The books do not contain a single empirical figure or table. There are some numbers here and there. But very few. North and Weingast (1989) does use some data, but the amount of data used is modest and the paper certainly does not formally test any empirical hypotheses.

AJR in contrast formally evaluate their theory using data and a formal econometric procedure.

Now, before discussing the advantages of AJR's more formal approach, I would like to stress that I am not trying to say that the informal methodology used by many scholars in the economic history literature (as well as in history, political science, and other fields) is worthless. Very much to the contrary, I spend a great deal of my time reading such work and feel that I have learned an enormous amount from it. Some of my absolute favorite pieces of scholarship are of this type. Work such as Robert Allen's *Enclosure and the Yeoman*, Joel Mokyr's *The Lever of Riches*, Robert Brenner's "Agrarian Class Structure and Economic Development in Pre-Industrial Europe," Angela Redish's *Bimetallism*, Carlo Cipolla's *Money, Prices, and Civilization in the Mediterranean World*, Barry Eichengreen's *Golden Fetters*, and Milton Friedman and Anna J. Schwartz's *A Monetary History of the United States*, just to name a few. These are wonderful books and articles that shape the way I think about the subjects they cover.

But as good as this work is, it suffers from an important drawback: because the work is informal it is very hard to evaluate its validity. Suppose one is skeptical. How can one tell whether the collection of facts that are emphasized is unbiased? Perhaps the authors are cherry picking facts and omitting other facts that are less supportive of their narrative.

The standard response to this concern is that one needs to know the relevant history. But consider what that means. It means that to critique the empirical argument in (say) Douglas North's work, one needs to know as much history as Douglas North. I am exaggerating a bit. But even so, this is clearly an enormous entry barrier. Only a tiny number of people can ever hope to get past the sledgehammer response: You don't really know the relevant history.

But do those wielding the sledgehammer really know the relevant history? The concern above may be viewed partly as a concern about how representative the sample is; but it is also a concern about which variables are being used; and it is also about what weights are applied to different pieces of evidence. If one tries to port the informal approach into formal language, it is usually clear that the underlying dataset that is implicitly being used has enormous numbers of missing datapoints.

Usually, the facts mentioned are not a systematic account of some variable over time or space. Rather, a particular variable may be mentioned for a few places and a few time periods. Implicitly, that variable is left missing for other time periods and places and one is meant to trust that the researcher in question has chosen to discuss the most important instances of the variable in an unbiased manner. Often it is clear that data on the variable does not exist for other times and places. Sometimes more data does exist, but the narrative analysis does not lend itself to a systematic analysis of the whole available dataset.

The formal approach adopted by AJR forces them to be explicit about the sample period, the sample of countries studied, and the specific variables used. Furthermore, their choice of estimator (e.g., OLS or IV) determines the weights that they place on different datapoints. This provides an enormous amount of discipline for the analysis. In addition, the empirical specification forces the authors to make explicit the identifying assumptions the analysis relies on. And the formal analysis also introduces the notion of statistical significance. Together, these elements constitute a much higher standard or rigor for the analysis.

They also make the analysis much more transparent and therefore easier to critique. By adopting a more transparent methodology, AJR are exposing themselves to critique. Had their analysis been more opaque, it would have been harder to critique. They could have hidden behind the opaqueness of the analysis and claimed that their wise reading of the nebulous set of facts at their disposal was correct.

The informal style of analysis is more opaque and harder to critique. But is this a strength? That doesn't seem correct to me.

Now, AJR's analysis is not easy to critique only because it is more transparent. It is also the case that something real is lost when the analysis moves from the informal style to the formal style. The informal style lends itself to a richer analysis in certain ways. An author doing informal analysis can draw on a larger array of facts because they are not obliged to perform systematic analysis of each type of fact they make reference to, and they don't need to worry about how to deal with missing observations. For this reason, the analysis can have much more texture than formal analysis. Historians typically decry the loss of this richness and texture when analysis moves in a formal direction, and they have a point.

So, all in all, there is a real tradeoff. A richer story can be told with greater ease using informal methods. But it is harder to evaluate its validity. AJR's formal methods provide more discipline and make it easier to see the strong assumptions the analysis relies on. But it is hard to incorporate into the analysis various sources of known information that are hard to quantify or for which there are lots of missing observations.

In my opinion, there is clearly a place for both methodologies when analyzing the origins and fundamental causes of economic growth and prosperity. Before AJR, these questions were predominantly analyzed using informal methods. AJR pioneered the use of formal causal methods in this area. This was a major contribution. It deserves to be highly celebrated. The rich, fascinating, and influential formal causal literature in economic history that has followed is a convincing testament to this.