

My research philosophy is an amalgamation of insights from a diverse array of economists, scientists, writers and philosophers. I would ascribe to it the following three tenets.

Topic

What should one research? Good research answers questions thoroughly and transparently, but important research asks important questions. James Tobin once said the most important decision a scholar makes is what problems to work on. I believe it is important to address “big” ideas, ideas that concern society and its wellbeing as a whole, at the local or international level. For me, this means studying features of our current financial capitalist economic structure and understanding the impacts they may have in future periods. It means trying to gain insight into what structural or institutional changes may assist all of society to achieve a safe, stable, and healthy life. It also means focussing on economics as a mechanism for transmitting and accumulating power in our society.

Examples of ongoing research questions include:

- Is the rise of inequality in Anglo-Saxon countries since 1970 driven by the “triumph of the english language”?
- Do individual political preferences shift with one’s income level?
- Constructing a continuous Minsky financial instability index: relative weights of hedge, speculative, and Ponzi finance units.
- How beneficial is inequality? Examining innovation, work effort, and investment.
- Collective intention and language as a basis for institution formation.

Methodology

Trygve Haavelmo believed micro-founded modeling was “actually beginning at the wrong end.” Many times it makes no sense to reduce complex human and social interactions into mechanized, deterministic rules. Instead a dynamic, unknowable society and its economic phenomena should be modeled in its entire form, statistically and probabilistically with rigorous specification testing. Haavelmo’s research philosophy is a starting point, but only when the question and data are appropriate. This *general-to-specific* framework allows flexibility with ex-post hypothesis testing and stronger predictive powers. It even predates, and embraces, contemporary appeals of “big data”

At the individual or organization level, natural experiments allowing for “as good as random” variation are invaluable and necessary tools for unpacking economic mechanisms. Clever research design can circumvent the need for complicated statistics, which often shroud any significant results.

While I am partial to applied econometric methods, I also value a detailed, structural, theoretical model. As George Box once said, “all models are wrong but some are useful.” Economic modeling creates useful philosophical frameworks, each an attempt to understand our society and, if done correctly, offering up testable hypotheses for applied researchers to examine and evaluate.

Interdisciplinary

It is imperative that researchers are exposed to other, often foreign, controversial, and uncomfortable, ideas, disciplines, philosophies, methods, and general ways of thinking. Research ideas and methods are often shaped and advanced when confronted by scholars of contrasting backgrounds,

or simply contemplating tangent notions. For example, I worked with a group of computer science and computational linguistics students on a project concerning the 2008 financial crisis. This led me to a parallel literature in network modeling, and eventually the work of Matthew Jackson, whose research was a foundation for the theoretical model developed in my job market paper. In another instance, simply reading a book on freedom on neurobiology by John R. Searle prompted me to reflect on questions of institutional formation and group dynamics, leading to a new research question to pursue.

Relationships, and potential collaborations, with a broad spectrum of researchers seem most fecund with younger scholars. My experience as a Graduate Fellow in the Graduate Center's Advanced Research Collaborative highlighted the fact that older faculty are much more hesitant towards interdisciplinary work, their methods and perspectives ossified by decades of research. They seem unwilling to assertively advise. I believe fruitful research must cross disciplinary boundaries if it is to address important questions with the broadest reach.