

A Personal Reflection on the Philosophy of Macroeconomics

Xiaoyang Li

May 22, 2025

Contents

1	Preface: This is a Personal Reflection	2
2	What is Economics?	2
3	Why is Macroeconomics Useful?	2
3.1	The Limits of Micro-Analysis	3
3.2	The Supertanker	3
3.3	Macro Models and Philosophy	4
4	How do we know our work isn't nonsense?	5
4.1	The fine line between nonsense and genius	6
5	What is Good Research?	7
5.1	Good research is compelling poetry	7
5.2	What is the goal of a great paper?	8

5.3 Closing thoughts	9
6 References	9

1 Preface: This is a Personal Reflection

This essay reflects my personal journey through economics research: grappling with uncertainty, philosophy, and the role of persuasion in intellectual life. It's not a policy statement or a technical treatise, but a meditation on what makes research meaningful. I share it in the spirit of curiosity, humility, and hope that others in similar shoes may resonate.

2 What is Economics?

Economics is the study of decisions in the presence of scarcity, prompting the necessity of tradeoffs, and the consequent emergent patterns in society.

Microeconomics concerns itself primarily with the first part of that sentence: the study of decisions by one agent, a few agents, and the interactions between them.

Macroeconomics concerns itself with the latter half of that sentence: those emergent patterns in society.

3 Why is Macroeconomics Useful?

One of the central objectives of economic inquiry is to figure out how we can do better with the stuff we currently have, “efficiency”. Since we take as our reference point our current resources, we care less about absolute quantities than about relative quantities.

The central relative quantity in economics is the ratio

$$\frac{\text{output}}{\text{input}}$$

where I mean output and input in an extremely general sense. This quantity may appear in other forms, as $\frac{\text{profits}}{\text{capital}}$ = rate of return, or $\frac{\text{loan}}{\text{collateral}}$.

I am raising this point because often it is tempting for people to question the contribution of macroeconomists to policy. You can ask, why spend time on macroeconomics when we can be encouraging people to be more hard-working, more patient, more frugal? Why not spend our energies on education? Maybe some states are doomed to poverty due to their geography. Does economics make a difference?

You might be astonished at the amount of wealth that nations can gain by simply using their pre-existing resources more effectively. The practical answer to these types of doomsday questions is that there are many extremely smart people who came before us who have produced compelling evidence that nations around the world are not doomed to poverty. Even by holding current resources constant (culture, geography, institutions), a lot of advancements can be made. Or perhaps the reason we become economists comes from the simple tenet that human well-being is worth fighting for, we can probably make large improvements, and we refuse to give up or accept subpar conditions.

3.1 The Limits of Micro-Analysis

The next philosophical question to address is, why don't we run supercomputers to figure out what is best for our economy? What is the point of trying to understand the situation descriptively or write models that seem like a crude approximation of reality? There I refer you to a quote, not by an economist, but by a physicist, that talks about what defines successful scientific inquiry:

“Anyone who wants to analyze the properties of matter in a real problem might want to start by writing down the fundamental equations and then try to solve them mathematically. Although there are people who try to use such an approach, these people are the failures in this field. The real successes come to those who start from a physical point of view, people who have a rough idea where they are going and then begin by making the right kind of approximations.”

- Richard Feynman

3.2 The Supertanker

Suppose we are driving a huge ship - a supertanker. We spot an iceberg tip up ahead and we want to change course. Even if we rotate our helm all the way to the right, do we expect our ship to immediately rotate 90 degrees? No, because our ship is large, it has momentum in that direction, and it's going to take a while for the paddles to provide sufficient force to propel the ship in a different direction.

So it is with economists changing the trajectory of the economy: the economy can be pictured as a giant ship. If we want to change the direction the economy is heading, say towards a recession, we can change interest rates. Does that mean it'll work right away? That people will get their jobs back? No, because the economy is big, it had gained momentum in a negative direction, and it's going to take a while for businesses and households to adjust.

How would a smart helmsman navigate the ship? If we don't turn the wheel until the very last minute, we're probably not going to be able to avoid the danger in time. So we should look far ahead and implement corrections gradually. Similarly, economists try to predict where the economy will be months, if not years from now, before deciding what policies to implement today. That's why in macroeconomics we think about the long run. If we don't think about the long run, it will be too late by the time we actually need to implement our policies: our economy is a supertanker and it will need time to adjust to the current situation. Suppose we are a helmsman departing from New York headed for London. However, the helm is just slightly off course. If it is never corrected, the ship may end up in Spain. It will be too late to correct the helm once we can actually see the borders of Spain, because we'll be thousands of miles away from London.¹

3.3 Macro Models and Philosophy

Macroeconomics still exists largely in the realm of philosophy and poetry. The aesthetic appreciation of these models is definitely an acquired taste. These models contain neither the profundity of philosophy nor the elegance of poetry, nor do they often reflect the state of humankind quite as accurately.

It may be helpful to see models in macroeconomics not as “true” depictions of the world, but rather as *thought exercises*. When macroeconomists write models, they are not claiming that this is exactly how the world works, but rather engaging in a hypothetical about how an artificial world could possibly work. Why is this useful? Refer back to Section 3.1, the quote by Richard Feynman. Feynman was talking about the study of the natural world, but the human world is just as if not more complex. The art of macroeconomics lies in making the right kinds of approximations that serve a useful purpose; that is, in crafting a model can describe some aspect of the world adequately for the purposes at hand.

Identification

What do we mean by identification in macro?

¹This analogy was taken from the publication The Story of the Federal Reserve System, see Section 6

A central difficulty in all of economics is causality. We need to know if X causes Y to determine what policy to implement. However, because the human world is an equilibrium outcome and there are so many variables involved, X and Y are made of components that themselves depend on each other; in other words, X and Y are endogenous. Thus, in economics we search for the part of X that is not dependent on Y (or something else), a piece of X that can be used to infer causality. When we find that piece, we say we “identified” X .

Definition. identify: We “identify” X when we find a valid way to isolate the effect of X on a particular outcome, separate from other economic variables. In other words, we’ve determined a causal relationship, not just a correlation, between X and a particular outcome.

What is an “identified shock”? Suppose we want to understand the effect of a change in monetary policy on output. The problem is that monetary policy shocks are endogenous: monetary policy depends on inflation, and inflation affects output. If you measure the impact of a monetary policy shock on output, you might just end up measuring the effect of inflation on output.

To “identify” the monetary policy shock, we need to figure out the part of the monetary policy change that is a “shock”; that is, a surprise to the markets. For example, we can look at what happens to stocks in the few minutes after the monetary policy announcement and use that movement as an “exogenous shock to markets.” From this, we can infer how the response of markets to a monetary policy announcement impacts output. The key idea is that there is a component of the monetary policy that the markets were not expecting, that was unanticipated and “shocked” the markets. One could say – while investors do not like surprises, economists love surprises!

The notion of identification in macroeconomics is closely related to classic econometrics. In econometrics, identification helps us answer the question: if you had ideal data, how would you estimate your parameters in the data and determine the causal effect of X on Y ? In macro, we use the same idea for causal inference. Why? In both econometrics and macro, we use components or shocks respectively that are uncorrelated with the error term “exogenous”.

Definition. identified shocks: An unexpected/unpredictable event that, under perfect data, can be used to figure out the effect of X on Y . Identified shocks are uncorrelated with the error terms from regressing Y on X ; in other words, exogenous proxies for X . Notice that the macro view and the classic econometrics view are two sides of the same coin.

4 How do we know our work isn’t nonsense?

I used to lament the inability of economics to prove something is “true.”

If only economics could be like physics, I thought – then we wouldn’t have such foundational disagreements, and policy advice would be clearer and more reliable. Our inability to settle those disagreements means that economic advice, especially when exported, can sometimes reflect institutional fashion more than settled science. And when that advice turns out to be wrong, it’s often the most vulnerable who pay the price.

I didn’t realize:

- There is a fine line between nonsense and genius.
- Therefore, physics suffers from the same problem.

4.1 The fine line between nonsense and genius

Research is hard.

A dark matter physicist once confessed:

“My work is bull—. We’ve been looking for dark matter for 40 years. First, we tell the government we need a detector that weighs a ton. We find nothing. So we ask for a detector that weighs 10 tons. Still nothing. Then we say we’ve found something promising and ask for a 100-ton detector. Our next step is to ask for one that weighs 10,000 tons. Of course, if we ever do find dark matter, we get a Nobel Prize.”

Of course, this account is hyperbole: not a literal indictment of scientific research, but an expression of frustration. And yet, it points to a deeper truth: even the most brilliant researchers often grapple with the feeling that they’re chasing shadows. Physicists I’ve spoken to describe their work as groping in the dark – unsure whether they’re on the verge of something profound, or slowly digging a billion-dollar sinkhole of taxpayer money.

Economics, by comparison, is a younger discipline, and often feels even more uncertain. We, too, stumble in the dark. Perhaps the only advantage economists have is that our tools are cheaper. Our laptops don’t weigh 10,000 tons, and at least we can claim to have helped generate more wealth than we’ve spent – fancy conferences included.

And what about classical physics? Sure, we all agree on the math behind gravity. But if you pause to reflect: what *is* gravity? What is energy? Are these real things, or simply mathematical descriptions that happen to predict nature well – for now? Can we truly understand the universe, or is all science just a set of stories that happen to fit the patterns our human minds are capable of seeing?

Does a magical substance called “potential energy” literally transform into kinetic energy when I drop a ball? All we directly observe is me lifting the ball and it falling when released. Yet physicists have persuaded us to accept an abstract entity – “energy” – which supposedly flows from my hand to the ball. Why do we believe this? Because it’s useful.

Philosophers from David Hume and George Berkeley to Karl Popper have long argued that no scientific concept is ever truly real, only practical. Adam Smith, too, described science not as a window into ultimate reality, but as a comforting narrative we construct to bring order to the chaos of experience: a “mere invention of the imagination” to soothe our bewilderment.²

Philosophers have long wrestled with the limits of human reason and perception. Yet their insights are rarely emphasized in STEM education today, even though these questions are foundational to how we build and evaluate models, including in economics.

Discovery sometimes follows a circle, not a line. We can imagine discovery as a line, where we start at pure ignorance, and as we move to the right, we become enlightened. However, we can also imagine discovery as a circle, where it brings us back to that initial feeling of ignorance. It doesn’t mean that we haven’t made progress, it just means that nonsense and genius actually live closer to each other than we thought. It’s as if we’ve walked full circle around the earth and realized the earth is round. Although we’re at the same place, we’re wiser.

Articles that show research is hard:

[Neuroscientists can’t figure out an 80s microprocessor, let alone the brain](#)

[Biologists meme-worthy model of the radio](#)

5 What is Good Research?

5.1 Good research is compelling poetry

Once upon a time, I worked up the courage to press my advisor: *What exactly is good research?* After a few minutes, he finally said:

“It’s like that Supreme Court case. I know it when I see it.”

²From *The Infidel and the Professor*, p. 41.

At first, I found this answer maddening. But over time, I've come to interpret it as something profound: great macroeconomic research is an art form – one that requires both skill and taste. We are, in a way, poets and philosophers. Just as poets can be taught literary devices like metaphor and alliteration, economists can be taught models and methods. But no one can be *taught* how to write a great poem – or a great paper.

Why not? First, greatness in any art form is inherently subjective. Second, it emerges from a delicate mix of choices, framing, and nuance: none of which can be fully reduced to rules or checklists.

I'm not sure whether macroeconomic research is more subjective than microeconomics or econometrics. But I do believe research in every field contains an element of art. Even in the so-called “hard” sciences, people debate which assumptions are elegant and which proofs are beautiful. That poetic quality may be especially pronounced in macroeconomics, where many questions are, in principle, fundamentally unidentifiable.

At its core, success in research is about persuasion. Much of what becomes accepted as truth depends not only on its empirical validity, but also on whether it persuades those with the authority to elevate it. This doesn't mean truth is arbitrary, but it does mean persuasion plays a role in what gets heard.

And persuasion is not just about the number of strengths in our work – it's about whether our audience is willing to accept its flaws. We can serve a magnificent feast, but if there's a bug in one dish, that's all anyone will talk about.

So what counts as “quality research”? It's our ability to answer people's questions in a way they find convincing. What people find convincing depends not only on their research priorities and expertise, but also on their lived experiences, personal biases, and idiosyncratic tastes.

Back when I was a confused and idealistic graduate student, and my advisor told me that good research was like the Supreme Court case – “*I know it when I see it*” – I realized I'd heard that line before. In fact, my first thought was that it's also used to describe great poetry. And suddenly, it clicked. Why do we think something is good poetry? Because it reflects our worldview, panders to our biases, and resonates with our sense of reality.

5.2 What is the goal of a great paper?

As a goal-oriented person, I used to find comfort in imagining an objective yardstick, a holy grail of research quality to strive toward. But now, as a more seasoned economist, I can finally offer my own answer to the question: *What is the goal of research?*

- **My personal goal:** to learn something and say what I truly think.
- **My professional goal:** to persuade those with power with a masterfully constructed story. Not for manipulation's sake, but because meaningful change requires navigating real-world gatekeepers. In academia, that means professors and journal editors. In industry, it might mean a judge. Accomplishing these goals require an understanding of what my audience finds convincing, even if it means stepping outside my own beliefs about best practices. However, the necessity of stepping outside our comfort zones of beliefs and habits is true for all scientific inquiry, not just economics. Insight alone doesn't speak; rather, someone has to make it speak persuasively.

5.3 Closing thoughts

What is “truth”? Physicists invented the concepts of energy and gravity – not in the sense of fabrication, but in the sense that these are stories, or frameworks, that conform to observed data. Thus, if those with power disagree with our stories and put roadblocks in our paths, then we must seek out new audiences who are open to seeing their value.

What makes a story “good”? It is the creativity and precision with which it mirrors reality. When a framework consistently predicts the world in a clear and useful way, we start to call it beautiful. That is the strange alchemy of good research and of any meaningful intellectual pursuit.

6 References

Alex Alright's Blog, [How I Learned to Stop Worrying and Love Economics](#)

The Story of the Federal Reserve System. Published by the Federal Reserve Bank of New York.

[Identifying structural shocks in Macro Models](#)

Dennis C. Rasmussen's [The Infidel and the Professor](#)