

and real activity that follows these crises. His explanation is that after large losses, banks are less able to tolerate further losses, which lowers their ability to intermediate, and so their future profits. Equity holders can then be stuck in a coordination failure, where no one wants to inject new equity unless others do so as well, banks are stuck in a low profit equilibrium, and the recovery must come through the slow process of retaining earnings by banks.

Matteo Maggiori (2012, Berkeley, NYU)

Maggiori postulates that countries with more developed financial markets are able to better deal with lack of funding in a financial crisis. They use this ability to sell insurance to less developed countries, so that in normal times they receive an insurance premium in the form of capital gains on foreign investments that sustain persistent trade deficits. During a crisis though, the advanced countries should suffer the heaviest of capital losses and a larger fall in consumption, a prediction consistent with what happened in the United States, but less so with what happened in Germany during the Euro crisis.

Joe Vavra (2012, Yale, Chicago)

Vavra used data on individual prices to find that changes in prices tend to be more dispersed and more frequent in recessions. He explains this by firms adjusting their prices more often in recessions, in spite of the costs of doing so, because the volatility of their firm-specific productivity is higher. But, with this more frequent price adjustment, monetary policy shocks will be less effective at boosting real activity in recessions.

In my reading, this is all exciting work, connected to relevant applied questions, and that takes data and models seriously. In contrast, in the caricatures of the state of macroeconomics, there are only models with representative agents, perfect foresight, no role or care for inequality, and a cavalier disregard for financial markets, mortgage contracts, housing, or banks. Supposedly, macroeconomic research ignores identification and does not take advantage of plentiful microeconomic data to test its models, which anyway are too divorced from reality to be useful for any real world question. Compare this caricature with the research that I just described: the contrast is striking. Not a single one of these bright young minds that are the future of macroeconomics writes the papers that the critics claim are what all of macroeconomic research is like today. Instead, what they actually do is to mix theory and evidence, time-series aggregate data and micro data, methodological innovations and applied policy questions, with no clear patterns of ideology driven by geography.

Blanchard (2016), Korinek (2015), and Wren-Lewis (2017) worry that the current standards and editorial criteria in macroeconomics undermine promising ideas, deter needed diversity in the topics covered, and impose mindless work on DSGEs that brings little useful knowledge to policy discussions. Smith (2016) emphasizes that we have far less data than we would need to adequately test our models, and Romer (2016) that identification is the perennial challenge for social sciences. Smith (2014) and Coyle and Haldane (2014) characterize the state of economics, not as the perennial glass half full