

Macro Political Economy

James Alt, Harvard

I will speak about comparative macro political economy then and now (leaving developing countries to Bob and international political economy to Jeff). But I first want to turn to a topic where my comparative advantage is larger: a personal note on “working with Ken” for the last 37 years. We've been friends and colleagues for a long time. I first met him one very early morning in the spring of 1978 in the lobby of the old Braniff Place Hotel in New Orleans. I was leaning on Norman Schofield at the time; Norman walked up to Ken and said, “Ken, I want you to meet Jim Alt. You should hire him for that position you have in political economy.” After a short conversation, Ken asked me if there was anyone in the states who could write me a letter. Out of a hat I picked the name Mo Fiorina, whom I had met just three days before, but with whom I had discovered compatible interests. The rest, as they say, is history. Ken became not just a friend but a mentor who patiently guided me through the intricacies of everything professional in American political science. For that I owe Ken a debt beyond my words to describe and means to repay.

Anyway, I joined Ken at Wash. U. (we lived a few houses apart and walked to and from work together) and initially at least under his guidance embarked on a decade-long program to invent (or better put, since much was owed to the early centers of excellence at Rochester and CalTech, reformulate), populate, stabilize, publicize, and ultimately institutionalize a program in political science and economics. We called it positive (to distinguish it from historical or normative) political economy. It was to be (his phrase) “**devoted to the dual analysis of the role of economic (i.e., strategic agents surrounded by institutional frameworks) behavior in political processes and of political behavior and constraints in economic exchange**”. Ken and I played a lot of institutional inside baseball at Wash U. and later at Harvard, organizing seminars and getting support for a program, leveraging outside offers for more resources, getting grants (Ken's vita lists the Mellon and Ford money that got us going at Harvard), and designing a curriculum, later including a series of undergraduate lectures and dinners by visitors like Aldrich, Baron, Calvert, Cox, Hardin, Laitin, Laver, Lupia, Tsebelis, and Weingast who talked about, among other things, cognition and learning, communication processes and social norms, bicameralism, electoral coalitions, political parties, and integration through federalism.

By then we had edited *Perspectives* to get the fundamentals out into the wider world, using a mix of contemporaries and senior scholars who wrote on individual decision, exchange transactions, rent-seeking and indivisibilities. You can see that Ken is transitioning away from his more Public Choice origins, and it showed: Charles Rowley reviewed the book for the *JEL* and hated it, singling out for abuse the chapters by Milgrom and Roberts on bargaining, influence, and interest groups and the classic Corporate Culture of Dave Kreps.

Let me use that moment to turn to macro political economy, then and now. Ken will not appear so often because he never liked macro a lot, as he actually said in his one article that appears in an NBER *Macroeconomics Annual*. Fifty years ago Goodhart and Bhansali were four, Kramer five, Nordhaus nine, Tufte ten, and Hibbs eleven years away from pioneering publications in the economics of voting and politics of the business cycle. In Britain (where I was) economists hid behind the benevolent dictator to avoid all discussion of politics while political scientists had a romantic notion of decisions unconstrained by decentralized exchanges, despite the collapse of both incomes and monetary policy in the same year. In 1975 the nine-volume *Handbook of Political Science* had no chapter on political economy (though it did have one each on collective choice and on formal theory).

The next two decades were the boom years, and twenty years ago I could write (in the *New Handbook*, which had one whole part of nine on political economy) that out of a wave of systematic, positive, empirical work had come an increased understanding of the interrelationships between delegation, jurisdiction, and agenda power on the one hand, and of the nature of politico-economic cycles on the other.

Other areas on the verge of a takeoff included trade policy and whether cross-country differences in investment and economic growth were influenced by democratic institutions, political instability, income inequality, and/or social polarization. Two decades of progress on theory followed by two decades of progress on the empirics mean that political economy now has its own *Handbook*, running over a thousand pages. Two significant achievements along the way, both taking the scale of empirical work and its integration with theory to new levels, were Alesina-Rosenthal (*The American Political Economy*) and Persson-Tabellini (*Economic Effects of Constitutions*).

As an example, consider specifically the relationship between inequality and redistribution (or the scale of the welfare state), often framed as why the recent rise in inequality has not resulted in more effort to redistribute incomes. Fifty years ago Duncan Foley set out a key result on the gap between median and mean incomes and majority choice of a tax schedule; via Romer and Roberts we get to the now-canonical Meltzer-Richard paper, which in interaction with institutional developments like the setter model and Ken and Barry's analysis of the *ex post* veto sparked 35 years of theoretical and empirical (sometimes linked) research on the question. Good developments abound: How do the tensions resulting from rising inequality, wage stagnation and loss of jobs from technological change and globalization interact with migration movements in the rise of "populist" parties and strands within established parties? Are populism and increasing inequalities inherently connected? How are the effects of inequality shocks on populism mediated by institutional differences in, among other things, training schemes and electoral systems? Under what conditions do immigration and ethnic divisions reduce voters' willingness to fund public goods? Is redistribution a substitute for law enforcement? Does centralization weaken electoral support for the redistribution by alienating voters or producing tax revolts among the influential and affluent?

Just today my email has brought two referee requests, one paper from *JLEO* on "Testing for Political Influence on Public Corruption Prosecutions" and the other from *Comparative Political Studies* for "Partisan-Electoral Cycles in Public Employment: Evidence from Developed Democracies". Both these papers are in a long tradition of asking how electoral motives influence policies and economic outcomes. Recent meta-analyses point to debt as a control variable and transparency as a key condition, which opens an avenue to study strategic misrepresentation in national accounts, a link to the study of political corruption. Summarizing forty years in a sentence: the view of motive and strategy has broadened; the concern to show feasibility (often by establishing policymaker jurisdiction) has increased; context, generality, and voters' knowledge, learning, and information sources are all points of active research. Ken's fingerprints are evident in many of those developments, even if his pen is not. The keys to this growth are what they should be: better data, more careful estimation, and awareness of context conditionality. Identification remains a nightmare when randomization is unavailable. And all that is in one small part of subfield! Political economy is healthy, even crowded: the frontiers have shifted out, even if many fundamental questions remain.

The future? I lack the vision to forecast, but we could think about the world-wide breakdown (shifting) of borders and movement of people within and between (changing) political jurisdictions. The growing concern with migration, ethnic heterogeneity, division, populist politics, and secession should stimulate the study of the micro-politics of civil war. What will be important? The expansion of our ability to collect and analyze vast amounts of data will continue. Advances in natural language processing and computer science soon will allow us to automatically (machine) code, in real time, massive amounts of English language and non-English language text. The coded text will yield data on multiple dimensions of events (source-action-target-location-time strings) and hence allow us to chart the outbreak, spread, and consequences of civil war for spatially and temporally disaggregated units of analysis like municipalities and departments. In turn, theories of civil war will be meaningfully tested and extended. And the reasons for and effects of the migration of people in anticipation, during, and after civil war will be better understood. If you think about the rise in (im)migration of people between regions of the world including the influx of refugees and asylum seekers in Europe and the United States, it has had enormous impacts on institutions, human security, labor markets and human capital stocks, and understandings of community.