Complexity, the evolution of macroeconomic thought, and micro foundations[1]

David Colander [Middlebury College, Middlebury, Vermont, USA]

Economics has always had an underlying tension between two visions of economics. One is an equilibrium vision that conceptualizes the economy as relatively stable and focuses on the forces that push the economy toward a long-run equilibrium. The other is a complexity vision that conceptualizes the economy as in constant flux, evolving in ways that we cannot predict. Both visions focus on competition, but the equilibrium vision focuses on competition as a state or market structure, while the complexity vision focuses on competition as an unending process. The two visions are not mutually exclusive, and an economist can see both as useful reference points when trying to understand the economy. Which is more useful depends on the question being asked.

While the two visions can be simultaneously held, generally, in setting a research agenda, one or the other dominates, and in recent years the equilibrium vision has dominated. This domination has influenced economic methodology and the way economists approach policy questions. Nonetheless, the complexity vision is still held and respected within the mainstream profession as demonstrated in the Nobel Prizes given to economists whose work reflects a complexity vision, such as Herbert Simon, Frederick Hayek, Douglas North, Eleanor Ostrom, and Ronald Coase. Their work is considered mainstream, but is seen as part of a separate tradition in economics that is not so much an alternative to standard mainstream economics, but rather another, less explored, parallel track. One of the goals of this paper is to encourage exploration of this alternative track.

Differences in theoretical methodology: equilibrium vs. complexity vision

The two visions draw lessons from theory differently, and are associated with quite different research programs, especially as they relate to policy. The equilibrium vision sees formal theory as providing a necessary blueprint for policy. Franklin Fisher (2011) nicely captures this view. He writes,

> "It is not an overstatement to say that they (the general equilibrium welfare theorems) are the underpinnings of Western capitalism… So elegant and powerful are these results (G.E.'s exploration and proofs of existence, uniqueness, and optimality) that most economists base their conclusions upon them and work in an equilibrium framework."

In the equilibrium vision, without formal theory, policy has no scientific foundation. It takes the position: Better to have an inadequate formal theory than no formal theory at all.

[1] This paper was the keynote address at the "Microfoundations for Macroeconomics: retrospect and prospect" workshop at the the University Nice Cote d'azur, GREDEG CNRS.

The complexity vision sees developing a useful tractable formal general theory as currently far beyond our capabilities and instead focuses on gaining partial insights into the complex dynamics of the economy in whatever way it can – agent based models, simulations and general exploration of non-linear dynamic models. Since formal dynamics is analytically so difficult, the complexity vision is content with informal theory especially when talking about the aggregate economy. It takes the position: When guiding policy it is better to recognize that we have no directly useful formal theory than to confine policy analysis to an inadequate formal theory. Within the complexity vision ultimately, because the formal specification of the economy is so beyond our current analytic capabilities, even the best economic policy is based on heuristics, not scientific theory, and thus, in a formal scientific sense, is ungrounded. Policy advice should not be presented to policy makers as otherwise.

The complexity approach to policy holds that, because of the complexity of economic theory's relationship to the real world, policy discussions are best separated from scientific discussions. Policy discussions should be based not directly on formal scientific theory, but, instead, on educated common sense – a wide ranging knowledge of economic scholarship that includes a good understanding of where researchers are in advancing formal theory, a good understanding of the history and institutions of the economy, a detailed familiarity with empirical data about the economy, and a philosophical understanding of the role that ethical views play in arriving at policy advice. Good policy is based on far more than just economic science.

The complexity approach divides economic analysis into two separable fields: science, whose goal is to discover the truth, and applied policy, whose goal is to solve real-world problems. The two fields are separated by a firewall to reduce the possibility of policy views influencing scientific judgments.[2] The goal is to allow specialization and gains from trade. The same economist could do both science and policy, but the two activities would use different methodologies, and would require different skill sets.

While the complexity methodology downplays the importance of formal theory in directly guiding policy, it is not against formal deductive theory, abstract mathematics, or sophisticated empirical research. But the goal of that theoretical research is a scientific goal – to better understand the economy; the goal is not to guide policy (although some policy guidance might follow as an unintended consequence). Thus, the complexity vision's scientific research agenda is consistent with a vigorous and highly abstract theoretical and empirical research agenda that, if anything, because its focus on complex dynamics, is even more mathematically and statistically complex than the current research agenda associated with the equilibrium vision. In that sense the complexity methodology is quite different from the critical realist methodology espoused by heterodox critics of economics such as Tony Lawson.

Critical realists criticize equilibrium methodology for its emphasis on abstract mathematics; complexity theory embraces mathematics. Complexity economists criticize the equilibrium methodology for the way it uses theory in thinking about policy, not for its use of mathematics. Whereas the equilibrium methodology treats formal theoretical results as central to its applied policy research, the complexity methodology uses formal theory more as a fable or heuristic, which may or may not be relevant for policy. Within the complexity vision, formal theory is best thought of as a thought experiment that can be useful both for thinking creatively about

[2] I expand on this distinction in Colander and Freedman (2019).

policy problems and for preventing logical mistakes in reasoning. But, because the theory is only tenuously related to the real world economy the theory is meant to capture, the results of formal theory are not to be thought of as a blueprint for policy.

The distinction among the equilibrium, complexity and critical realist/heterodox views of the equilibrium methodology can be seen in reference to the well-known "searching for the keys under the lamppost joke." The standard interpretation of the joke embodies the critical realist view. It is that economic theorists are out there in La-la-land, doing highly abstract economic research unrelated to the real world.

> "Isn't it stupid – searching where you haven't lost the keys just because that's where the light is?"
>
> "Isn't it stupid – working on models that you know are so far from reality that they can't possibly describe reality: representative agent super rational choice models, when it's obvious that the action is in interactive effects; Isn't it stupid to work with strict rationality models, when it's obvious that people are at best boundedly rational?"

From a complexity standpoint, a research strategy of "searching where the light is" is far from stupid. Where else but where the light is can one do formal theory? Where the complexity vision has a problem with the current equilibrium methodology is with its attempt to apply the abstract theory, developed where the light is, directly to policy. That's the equivalent to searching for the keys where you did not lose them, and deserves the critical realists' scorn. The complexity vision sees the goal of theorists searching in the light to be discovering potential patterns that help them understand the economy. While the goal is not to guide policy the discovered patterns might be helpful to applied policy researchers exploring in the dark. Theorists are developing an abstract knowledge of economic topographies, exploring abstract topographies where there are the equivalent of rocky cliffs, where there are smooth deltas, rolling hills, and where sudden storms changed the topography quickly, as a small creek becomes a raging river. This leads to a second role for theorists—to develop creative abstract policy solutions to deal with different topographies. These abstract solutions may or may not be transferrable to the real world. But the exploration can suggest other solutions that might work. The goal of this part of policy theorizing is creative design of policy.

An engineering methodology

The exploration of creative policy solutions uses what I call creative design engineering methodology. Engineering methodology differs from scientific methodology; it is a heuristic methodology used by a craftsperson. Billy Vaughn Koen (2003) defines it as "the strategy for causing the best change in a poorly understood or uncertain situation within the available resources" (p. 7). Koen argues that this definition is operationally equivalent to a second definition – "use the best available engineering heuristics to solve problems".

Because complexity engineering is designed to deal with policy, it does not attempt to be value free; instead it attempts to be value transparent. Whereas scientific methodology eschews philosophical methodology, engineering methodology incorporates it as the best way to integrate values into the analysis. Compared with scientific methodology, engineering methodology is much less constrained, and loose. It is an educated common sense

methodology in which "anything goes" as long as that "anything" is useful in arriving at a possible solution to a problem. It is a creative methodology that is not afraid to deviate from current scientific conventions. Context, not fixed methodological rules, determines method, and the guiding heuristics are determined not by specialized philosopher of science methodologists, but rather by the researchers themselves. Here is what has seemed to work in a similar case; maybe it will work in this case.

In place of a formal theory to guide policy, the complexity policy methodology uses an informal general theory that focuses on change and process, not on equilibrium, as its general framework for thinking about applied policy. For complexity economics that informal general theory is best described as a multi-level evolutionary theory that has much in common with the multi-level evolutionary theories used in evolutionary biology. One may talk about institutionally constrained equilibria, but such equilibria will be seen as part of an evolutionary system and not as a final resting place of the economy. By equilibrium theory standards, the complexity vision theory is more a conceptual theory than a formal theory.

The theoretical debate within complexity economics is not about whether the evolutionary theory is correct; the debate is about the nature of that evolution. Most complexity economists assume that the economy's evolution is multi-level, which means that, while the economy is assumed to have developed from the decisions of rational agents, the nature of rationality has evolved to fit the institutions that coordinate agent's actions to promote the group's interest as well as its own. Where rational agents have found it useful, they have cooperated and developed behavioral norms, and have built institutions based on those behavioral norms. These institutions and norms have solved coordination problems; it is their existence that prevents chaos so, to reasonably discuss policy, one must have a model that includes them. Developing a precise model of this evolutionary system is impossible since these norms and institutions have become nested in other norms and institutions in complex ways. Over time, the nature of the bounded rationality changes as institutions and norms evolve. Appropriate policy evolves as the economy evolves.

The behavioral constraints nested in these institutions significantly complicate what is meant by agent rationality; within some specification of evolutionary theory, just about any agent action can be considered rational. This possibility undermines the usefulness of any simple individual rational choice model that doesn't incorporate real world institutions and norms. The current real-world rationality must be discovered empirically. Thus, the complexity vision is consistent with behavioral economics, whereas the equilibrium vision is tied to traditional individual rationality approach to behavior.

Rational individuals solve problems by coordinating their actions, creating institutions that solve some problems but add others. These institutions compete and collaborate, creating an ever increasing array of new coordinating institutions as technology changes and as new discoveries are made. Thus, the complexity vison sees the economy as an evolving complex system that exhibits all the characteristics that evolving complex systems exhibit – multiple basins of attraction rather than a unique long run equilibrium, natural selection, mutations, adaptations, sensitive dependence on initial conditions, path dependence, and potential phase transitions that cannot be deduced from the study of individual agents separate from their interaction. It thinks about policy informally within an evolutionary framework. The complexity scientific research program is designed to abstractly explore that multi-level evolution.

Macroeconomics and complexity

So far, I've talked about general methodological differences between the complexity vision and the equilibrium vision. Let me now turn specifically to macro theory and consider how the complexity approach to macro and the equilibrium approach to macro differ. Probably, the biggest way in which they differ is in what they see as the central question that macro theorists are trying to answer.

Since the equilibrium vision starts with the assumption that, in the absence of constraints, the economy will gravitate toward a predetermined desirable Pareto-optimal equilibrium, which is assumed not to include large fluctuations in output, the questions it tries to answer are: Why are there significant fluctuations in the macro economy? Why doesn't the economy settle down to an equilibrium reflecting agent's desires? Why are there business cycles and fluctuations? And, if there are undesirable fluctuations how can we stop them? It sees fluctuations as being caused by exogenous shocks imposed by government or by technology. Its explanation for why these fluctuations are not eliminated is that institutions prevent the competitive market from solving the problem. Institutions and norms that lead individuals to deviate from self-focused individual rationality are the problem in the equilibrium vision. These institutions and norms impose price rigidities, and constraints on behavior, which prevent the market for achieving a global Pareto-optimal equilibrium.

The complexity vision is trying to answer a quite different question: It has no trouble explaining undesired fluctuations because it does not start with the assumption that Pareto optimal equilibrium would be achieved by the market in the absence of outside shocks and institutions. In the absence of the imperfect institutions that have evolved, the complexity vision would expect chaos and enormous fluctuations in a system. Institutions are a key part of the way an economy coordinates agent's actions. Thus, the macro question the complexity vision is trying to explain is not: Why does output fluctuate? Instead, it is trying to explain why the economy is as stable as it is. Its base assumption is that in the absence of some additional stabilizing forces, the economy would be chaotic and highly unstable. Within the complexity vision, markets do not exist in a void, and thus cannot solve coordination problems unless the underlying institutional structures, such as property rights and norms of behavior, upon which markets are built, have been developed. Markets are institutions; they are not the default reality.

The complexity vision explanation of why fluctuations are as small as they are is the institutions that have developed. Where fluctuations have posed problems in the past, agents in the system have self-organized and created institutions and norms that reduce fluctuations. One of those institutions is the market. In the complexity vision markets are seen as endogenously developed coordination devices. Thus, whereas in the equilibrium vision institutions are a cause of fluctuations, in the complexity vision institutions are what prevent chaotic fluctuations.

These institutions include not only markets, but also government. So whereas the equilibrium vision sees government as exogenous to the system, the complexity vision sees both government and markets as having endogenously evolved. A theory that does not include endogenous government and markets sheds little light on real-world problems. Since institutions provide the stability to the system, they cannot be assumed away in any useful analysis of real-world problems. This makes abstract theorizing about how a market economy would operate without the current institutions, such as is done by researchers holding an

equilibrium vision of the economy, for other than general thinking about abstract issues, of little use.

How the complexity vision was lost in macro

These complexity/equilibrium differences do not fit into the traditional Classical/Keynesian distinction. In fact, the complexity/equilibrium distinction has essentially no correspondence to the Classical/Keynesian distinction.[3] There are Classical economists who emphasized a complexity vision and there are Classical economists who emphasized an equilibrium vision. Similarly with Keynesian economists. There are complexity Keynesians and equilibrium Keynesians. But somehow, the complexity interpretations of both Classical and Keynesians have been lost, and the Keynesian/Classical debate has been between a Keynesian equilibrium vision and a Classical equilibrium vision.

It didn't have to be that way. Within both Classical and Keynesian economists, there were both complexity and equilibrium advocates. In fact, up until the 1930s within Classical macroeconomics, the complexity approach was dominant. But starting in the 1930s internal incentives within the profession were moving the profession toward the equilibrium vision and away from the complexity vision. This occurred in both microeconomics and the emerging macroeconomics. One aspect of this is the movement from Marshallian methodology, which followed a Classical methodological approach and which downplayed the importance of equilibrium theory to policy, to a Walrasian methodology, which made general equilibrium theory central to policy.

You can see Marshall's complexity vision methodology in his view about the role of pure theory in economic reasoning. He writes:

> "It seems strange to me to be asked my views as to the study of pure economic theory; as tho' that were a subject on which I were fit to speak. For indeed I was never a partisan of it; and for more than a quarter of a century I have set my face away from it. As early as 1873 (I think it was the year) Walras pressed me to write something about it; & I declined with emphasis. The fact is I am the dull mean man, who holds Economics to be an organic whole, & has as little respect for pure theory (otherwise than as a branch of mathematics or the science of numbers)…" (Letter from Alfred Marshall to W.A.S. Hewins, October 12, 1899, in Coase, 1994, pp. 172– 173).

This dismissive view of general equilibrium theory was held by the majority of economists up until the 1930s and 1940s. It held that the pure general equilibrium theory of economics wasn't worth developing not because it wasn't important, but because economists didn't have the analytical tools to deal with it. Using the tools they had, the results were trivial, obvious, or irrelevant. That left macroeconomics to be verbally debated, not to be debated in terms of formal models or equations.

[3] That's why in my work on the evolution of modern macroeconomic theory, (Colander, 1996; 2006) I emphasized used different classifiers – Walrasian and Post Walrasian, rather than Classical and Keynesian – with the Walrasian economists maintaining a commitment to equilibrium methodology, and Post Walrasians maintaining a commitment to complexity methodology. There can be either Keynesian or Classical Post Walrasians.

In the 1930s that started to change, as economists abandoned the earlier Classical liberal/Marshallian methodology which had a strict firewall between scientific theory and policy. Instead, they began using a Walrasian methodology that drew policy conclusions directly from scientific theory. The development of macro occurred at this time, and its evolution was significantly influenced by those methodological developments, which shifted the profession from a Marshallian to the Walrasian methodology.

Classical economists didn't formalize their micro analysis into a formal macroeconomic theory because they didn't believe that their micro reasoning about individuals and firms translated to aggregate results in useful ways. They fully accepted what would later become known as the fallacy of composition argument. The aggregate economy was far too complicated for formal theoretical exposition based on an analytically tractable micro foundation.

Rather than a formal theory, Classical economists advanced some general insights about the workings of the macro economy: Say's Law, the Quantity Theory of Money, and the dichotomy between the real and nominal sector. These three insights were developed not as a formal theory, but simply as some insights to correct simplistic, logically incorrect, arguments that had been made by lay people (and some economists) about the workings of the aggregate economy.

For example, lay people often argued that if people saved, it would mean that there would not be enough aggregate demand to buy the aggregate supply. Say's Law countered that simplistic argument, and pointed out that, in the aggregate, supply was intricately related to demand through financial market interconnections between saving and investment. Classical economists fully agreed that that interrelationship between aggregate supply and demand was noisy and unstable. All Say's Law implied for policy was that the interconnection was definitely something to keep in mind when thinking about macro policy, and that the simplistic arguments, which held that saving would necessarily imply a shortage of aggregate demand, were not correct. Similarly, with the lay arguments that confused price level with relative price, or held that an increase in money supply would necessarily make society richer. Such arguments missed the insight that the wealth of nations resided in real output and that one needed to account for price level changes in determining relative prices over time, and in determining whether a change in the aggregate wealth of a society over time has occurred.

Classical economists recognized that there were all kinds of ways in which that equality between aggregate supply and demand could be broken, and that "Say's law" was fully consistent with widespread temporary unemployment, business cycles, and recessions.[4] The same was true for the Classical propositions about the neutrality of money and the quantity theory. In short, these Classical insights are best understood not as formal theories, but rather as general insights about the aggregate economy that were meant to be understood in the context of the debate in which they were used, not to be used as part of a precise equilibrium theory about how the real-world economy would operate. If you are always moving from one

[4] Petur Jonsson (1997) makes this point clearly. He notes that Say wrote "In the first place my attention is fixed by the inquiry, so important to the present interests of society: What is the cause of the general glut of all the markets in the world, to which merchandise is incessantly carried to be sold at a loss? What is the reason that in the interior of every state, notwithstanding a desire of action adapted to all the developments of industry, there exists universally a difficulty of finding lucrative employments? And when the cause of this chronic disease is found, by what means is it to be remedied? On these questions depend the tranquility and happiness of nations." This is hardly a statement that a person who believed that Say's Law implied that there could be no unemployment would make.

equilibrium to another, and you never arrive at any equilibrium, being precise about final equilibrium that the economy is aiming for is not all that important.

Consistent with the view that Classical economists did not have a formal macro theory, Classical practical guidance on short run macro policy did not follow directly from these theories and laws. Early Classical policy discussions, such as the bullionist/anti-bulllionist debate, reflected an institutionally rich understanding of policy by individuals knowledgeable in both the abstract theory *and* the current institutions. Walter Bagehot's (1873) discussion of monetary policy, which blended institutional insights and theoretical insights into insightful pragmatic policy guides, is an example of what I mean by the Classical applied policy methodology. It reflects a complexity vision – it is a practical, educated common sense approach to policy. These Classical applied policy works blended theoretical Classical macroeconomic insights with deep institutional knowledge and arrived at useful guidance on the conduct of monetary policy. No formal general theory is required or is even seen as useful.[5]

Cutting edge Classical economists knew that their three propositions were not an acceptable theory for short run aggregate fluctuations. They were simply insights about general tendencies and what the logic of the model implied. Good Classical economists knew that they had no formal equilibrium theory of the aggregate economy. But they did have a set of policy precepts that were based on past empirical evidence and insights, not on theory. Based on that evidence, in the 1920s and 30s, they assumed that a fall in aggregate output would be of short duration since that was their experience with past fluctuations. Their policy suggestion of government not stepping in was based on experience, (and concern about whether government would or could effectively do something to reduce the depression) not on any formal theory.

With the Great Depression of the 1920s in Europe and 1930s in the US, Classical economists' policy precepts were rightly being questioned; the empirical pattern had changed. As the depression continued, their assumption that in long run the fall in output fluctuations would resolve themselves on their own, which for the most part was unexamined, was requiring a "long run" that was much longer than policy makers would accept.

In response to these developments theoretical Classical economists started exploring the interconnection between micro decisions on supply and their relationship to aggregate demand in more detail. For example, economists such as Ragnar Frisch (1933) began formally exploring macro dynamic sequence models, starting from micro foundations, in which interconnected industries transmitted a negative demand or supply shock in one industry into other industries, setting up a potential feedback reinforcement loop that could lead to depression-like conditions. Today, we would see that work as part of a study of complex non-linear dynamics of a system with multiple basins of attraction. But at that time, their formal work was generally ignored since most economists didn't have the mathematical background to understand this advanced work, and it didn't lead to any specific policy recommendations.
Keynes was a cutting edge Classical economist, who followed a Classical liberal methodology. But unlike other Classical economists working on macro dynamics, he was not interested in developing a carefully spelled out formal theory of dynamics connected to micro foundations. He was more interested in capturing the big picture and conveying it to other

[5] Since data was limited and statistical techniques were not yet developed, formal empirical work also played little role in the policy debates. The policy debates were "armchair" debates done without formal theory.

people in a way that would help him win his policy arguments. Thus, while following a Classical liberal methodology in some ways, he was willing to violate the classical firewall between theory and policy. This violation made it impossible to separate out the theoretical and policy differences between Classicals and Keynesians, so that they could both have the same scientific theory, but different policy views. Losing the firewall made it almost impossible to have a non-ideological debate about the theory. Both sides had policy agendas built into their "scientific" theories which made neutral objective discussions of them impossible.

Keynes was a skilled advocate and marketer of ideas, and in his *General Theory* he developed a highly simplistic informal aggregate theory that could be represented in a simple graphical aggregate demand/supply expenditure model (the Keynesian Cross) that emphasized aggregate adjustment via output fluctuations rather than adjustment by price level fluctuations. Keynes' alternative model had multiple equilibria dependent on exogenous shocks to demand. Rather than supply creating its own demand, in Keynes's alternative model, demand created its own supply. So rather than both sides agreeing that aggregate supply and demand were interconnected, which could create serious dynamic adjustment problems, we had two opposing theories each connected to specific policies.

To contrast his new theory with existing views, Keynes created a Classical straw man equilibrium theory of the aggregate economy. As opposed to saying that Classical economists had no theory of dynamic adjustment, and that he was adding one possible adjustment mechanism, he attributed an equilibrium theory of the aggregate economy to Classicals. Essentially, the argument he made was the following: Say that all three Classical propositions hold. Then, as long as there is no deviation between aggregate supply and demand, the economy remains in equilibrium. But if, for some reason, aggregate demand slightly differed from aggregate supply, and both AS and AD were perfectly inelastic, classical theory in its most formal specification, had no dynamic adjustment mechanism to bring them into equilibrium. (Price level influences on aggregate output in the absence of an international sector, were ruled out by Classical assumptions that aggregate supply and demand were perfectly inelastic, and were interconnected by Say's Law.)

This Classical equilibrium theory didn't capture the thinking of cutting edge Classical economists, but rather set them up to be refuted by his alternative theory. Keynes suggested that Classical economics explained unemployment and the depression, not as being caused by dynamic adjustment problems that they could not analytically model, but rather as being explained by a partial equilibrium model in which too high wages were the culprit for unemployment. This belief could be easily shown to be an unsupportable theory. He suggested that their macro model consisted of three propositions that kept the aggregate economy in constant equilibrium. This required attributing a formal equilibrium model to them. Keynes created a straw-man Classical equilibrium theory based on rational agents, and showed how the three Classical insights that made up this straw man characterization came to the conclusion that the macro economy would always be in aggregate equilibrium at full employment, and that had the policy implication that government demand management policy could not affect the aggregate output of the economy. [6]

[6] Classical economists responded to this argument by arguing that technically their model was not totally illogical; price level adjustment could technically bring about equilibrium via the Pigou effect. They also agreed that as a practical matter the Pigou effect was too small to achieve the desired equilibrium, and that "on the checkerboard of real life" it was irrelevant.

Where Keynes went beyond other complexity-vision Classical economists was in proposing an alternative dynamic adjustment mechanism. He argued that, faced with excess demand, rational suppliers would cut real output, which would reduce income, which would further reduce output. The economy would find itself in a downward output spiral. In principle that downward spiral could continue forever. But, Keynes, following Kahn, argued the spiral would stop because of psychological laws governing micro behavior as captured by relatively stable marginal propensities to save and consume. Because people saved a relatively fixed proportion of their income, as aggregate output fell, aggregate demand would fall by less. In each round of the process the disequilibrium would decrease, and the economy would asymptotically approach an equilibrium. In this Keynesian model individual rationality did not bring about aggregate equilibrium; agent irrationality – the habit captured in the constant marginal propensity to consume – did.

Had this multiplier storyline been seen as Keynes' key contribution to macroeconomics (which it was initially by some economists) macroeconomic theorizing would have followed a quite different path than it did. But the mathematics involved in formally working on dynamics and interrelating them into an equilibrium model were treacherous. It required going into issues involving complex dynamics that were technically far beyond the capabilities of most economist of the time. Richard Goodwin, in his work on matrix multipliers, was an exception. He captured the problems, writing "Combining the difficulties of difference equations with those of non-linear theory, we get an animal of a ferocious character and it is wise not to place too much confidence in our conclusions as to behavior" (Goodwin, 1950).

To have a full theory Classicals needed to spell out that dynamic adjustment mechanism in which price level adjustment could not bring about equilibrium. So in the complexity version of macro history, Keynes's contribution was to point out that Classical economics had no aggregate dynamic adjustment mechanism. This was consistent with models that cutting edge Classical theorists were working on directly from supply side considerations. Had it been presented this way Keynes would not have been seen as offering an alternative to Classical theory, but rather as offering an extension of Classical theory, which incorporated dynamics. His multiplier model offered one possible dynamic story, but there were many more alternative ones. Had macro economists followed their complexity vision, researchers' focus would have been on developing alternative dynamic stories, and then testing them empirically. Macroeconomic science would have become much like weather science where there is only one science based on general laws, but many models that reflect different dynamics. There would be no Classical/Keynesian theoretical debate about equilibrium models; there would be debates about alternative dynamic adjustment theories.

As should be clear in the tone of my writing, as much as I admire Keynes, I also blame him for this movement from a complexity focus to an equilibrium focus. By not making it clear that his insights about the problems of Classical theory were understood by other cutting edge Classical economists, and that neither his, nor their, theory led to any firm policy conclusions, he led the profession into fruitless debates about formal equilibrium theories. Had he maintained the Classical firewall between science and policy, the policy debate would have been separated from the theoretical debate. The policy debate would be seen as a debate about subtle issues involving politics and sensibilities, not about macro theory. People could reasonably differ about sensibilities, and that debate would not, and could not, be settled by scientific methods.

Perhaps fittingly, in creating a straw man to attack, Keynes set Keynesian economics up for failure. By that I mean that the equilibrium characterization of Classical economics also led to Keynes' theory being interpreted in a similar equilibrium setting. This undermined any complexity vision interpretation of Keynes' ideas, which was the revolutionary part of Keynes' thinking. Instead of being the entre into dynamics, the multiplier was integrated into static equilibrium and the debate became about equilibrium models, not dynamic models. This forced Keynesians to answer the equilibrium macro question – why the aggregate economy would not move to a Pareto-optimal equilibrium, rather than to answer the complexity macro question of why dynamic forces could cause problems that the current institutions did not resolve.

The equilibrium characterization was quickly built into the standard Keynesian model, which shifted from the dynamic multiplier model, to a multi-market general equilibrium model, the essence of which was captured in what was called the four quadrant diagram, which showed goods/market/money, market/bond market dynamic adjustment to equilibrium. That four quadrant diagram, which demonstrated (with a lot of hand waving) equilibrium as being asymptotically reached, soon gave way to the IS/LM version of the model that totally hid the assumed dynamic adjustment underpinnings of the argument, and presented both the Classical and Keynesian models in equilibrium space. The LM curve captured money market equilibrium; the IS curve captured goods market equilibrium. The model was in "general equilibrium" when the two curves intersected. As a geometric exposition of how to solve comparative static equations, all this was very nice, but the model obscured all the dynamics that would have been the focus of debate in the complexity interpretation of both Keynesian and Classical economics. In the IS/LM model, in which the multiplier worked instantaneously, the multiplier dynamics were hidden in the shape of IS curve. Multi market equilibrium was characterized as being as easily achieved as a single market equilibrium.

The IS/LM model became the totem for what came to be called NeoKeynesian macro. This model totally obscured issues of dynamics. NeoKeynesians and NeoClassicals were differentiated on their beliefs about the shape of the LM and IS curves, not on their beliefs about dynamic adjustment processes, which is where the complexity vision put the differentiation. The entire complexity debate about dynamics, based on judgements about how the dynamic adjustment process worked, was lost.

Most of what went under the name macroeconomic theory in the 1950s and 60s explored equilibrium issues. This presented a serious problem for Keynesian economics.[7] For long run full employment equilibrium not to be the outcome, one had to posit some price adjustment inflexibility in the system – fixed wages, fixed prices, or below zero equilibrium interest rates are examples. But if one's model does not include the need for institutions which impose those constraints, those inflexibilities created by institutions seem ad hoc. In a Walrasian general equilibrium model, Keynesian economics loses to a Classical model, which is precisely what happened with the New Classical revolution.

[7] Some Keynesians pointed these problems out. But those who worked on these dynamic interpretations, such as and G.L.S. Shackle (1949) and Paul Davidson (1970), soon found themselves banished to the realm of heterodoxy. They had few followers; their explanations of Keynes's ideas did not fit the sweet spot of theory within the institutional structure of the economics profession of the time, which required theory to be specified in tractable equations that economists of the time could follow and work with – not to complex, not too simple. IS/LM found that sweet spot.

Much of modern macroeconomics still conceives of the macro problem in that equilibrium framework. In it the difference between Keynesian and Classical economics became differences of assumptions about an equilibrium model: Keynesians assume inflexibilities in the system that prevent equilibrium from being reached; Classical don't. This essentially made Keynesian economics an addendum to the Classical straw man that Keynes had created to have something to critique. NeoKeynesian economics became a straw man attack of a straw man creation. The belief that Keynesian economics actually involved revolutionary thinking – that what we should be studying are dynamics, not equilibrium – disappeared.

Micro foundations and the fallacy of composition

Let me now turn to where micro foundations fits in the history I am recounting. Micro foundations, in some form, has always been part of macro and always will be part of it. While the ideas in Keynes' *General Theory* acquired the name macroeconomics, (it was first called macro dynamics) from the beginning, much of the discussion in the *General Theory,* and debate about it, involved discussion of micro issues, and it is often said that the General Theory is 70% micro.[8] What differentiates that micro foundations discussion from what has become known as micro foundations is how the micro discussions are connected to macro results.

In the complexity vision, there is no reason for macro results to follow directly from micro decisions. In fact, such a connection would not be expected. Any differences can be resolved by appeal to the fallacy of composition – what is true for the parts is not necessarily true for the whole. So in the complexity vision micro decisions and macro results are related, but not in any simple way. The fallacy of composition black box allowed micro decisions to be connected to macro results in many different ways.

In the *General Theory*, Keynes invoked the reasoning behind the fallacy of composition often. Thus, we can see him talking about animal spirits guiding the economy, beauty contest coordination problems, and the distinction between uncertainty, which cannot be hedged, and risk, which can be hedged, throughout the book. Those discussions were based in the complexity vision, not an equilibrium vision of macro. The scientific complexity macroeconomic research program is to unpack that fallacy of composition black box to better understand how they are connected. It is that that modern complexity economists are doing with their studies of non-linear systems, agent based models, and network models.

Complexity and current macro policy

The policy complexity macroeconomic research program has two components—one concerned with the practical problem of guiding current policy, and the second concerned with exploring ways in which the fallacy of composition black box can be changed so that micro decisions lead to desirable macro results.

[8] For the most part, that micro foundations discussion involved modifications and adjustments to the point that Classical economists had no acceptable theory of aggregate dynamics, and that if Keynes was talking about a long run unique equilibrium model, Keynes had not fully specified his alternative theory.

I don't have much to say about the first of these, but I will provide a brief summary of my thinking. Much of the current macro policy debate concerns which model to use to guide monetary and fiscal policy decisions. There are two general positions: one is that one should use some variant of a VAR model. The other is that you should use a formal model – either DSGE or IS/LM – and empirically estimate the relevant structural equations. The complexity approach comes out strongly on the side of using a variant of the VAR model, because it is data, not theory, centered. The reason is that, from a complexity vision, that entire structural macro modeling project is problematic because it doesn't take account of the institutional complexities that play a central role in the dynamics of the system. Those institutional complexities are too complicated to formally analyze from first principles, so a more macro analysis is needed. Put simply, from a complexity perspective, the macro economy is too complex to formally model from first principles, taking into account the complex dynamics that the complexity vision believes need to be taken into account. This hold for both structural IS/LM type models and DSGE type models.

The modified VAR modeling approach that is associated with the complexity approach can be seen in the work of David Hendry and Katarina Juselius, which elsewhere I have called the European approach to macro econometrics. (Colander, 2009) Unfortunately, their work is not seen as central to standard econometrics by many econometricians, especially those in the U.S., where their work is often little known. I have hope that this approach will become more popular in the future because it is closely related to what is being called a data science empirical approach, which is gaining wide acceptance outside of economics. I see data science as the complexity approach to empirical work, and I contrast it with the current more structural econometric approach.

The two approaches differ by the role they see for the interaction of theory and data. Both see empirical work as central, but the US standard econometric approach sees formal theory as a necessary guide for our understanding of data. Econometrics has its methodological foundations in logical positivism – it puts theory first and is designed to test theories and shed light on causality. It interprets data through the lens of formal theory. The complexity approach follows a data science approach that puts data first. It agrees that data has to be interpreted through some lens, but the appropriate lens in economics is not an inadequate general equilibrium theory. It is, instead, an educated common sense lens. Data science methodology is meant to find patterns in the data without first subjecting the data to any predetermined theoretical lens. Theory is still important, and a loose sense of theory cannot be avoided in the initial collection of data and in interpreting empirical data. But to the degree possible the goal of data science is to let the data speak.[9]

For most economists, trained in econometric methodology, this "data first" approach to empirical work seems questionable, and unscientific. It has a long history within the broader statistics community from which econometrics developed. Econometrics simply took a different path, and, as econometric practices within the economics profession developed, it emphasized certain asymptotic aspects of statistics – those aspects that fit with testing and relying heavily on theories – and downplayed other potential methodologies that exist in the statistical research inventory, such as non-parametric empirical analysis, bootstrapping and a variety of other methods that fall within the data first approach.

[9] It is, however, recognized that different people with different theories will interpret the same empirical data differently. These differences are natural, and can only be resolved by philosophical methods – engaged discussion and debate amongst researchers – not by scientific methods.

The distinction I am drawing between data science and econometrics is, of course, far too stark. Both econometrics and data science blend theory and data, and both have their roots in formal statistics. Moreover applied econometrics is changing, as more and more econometricians have started using data science methods such as bootstrapping, and non-parametric econometrics and formally dealing with cases where they accept that the assumptions needed for asymptotic econometrics do not exist. This is not surprising; the two empirical approaches come from the same statistical bag of tool. The difference is simply a matter of separate evolutions; the state of the arts in statistics and econometrics have evolved differently, and "statisticians" (researchers trained in a formal statistics department) developed different traditions and emphasis that did econometricians, (researchers trained in economics departments) as institutional incentives facing researchers have pulled them in different directions and led them to look at different type problems. Advances in computational technology are now pulling the two closer together.

Actually, even in practice, I am not sure that the differences between the structural IS/LM, DSGE and VAR models matter all that much, because of the ad hoc way in which I suspect structural models are brought to the data. Here is my suspicion: In applied policy models such as those used at central banks, to make the abstract DSGE and IS/LM models fit that data, macro modelers make adjustments to the pure theoretical models. With sufficient adjustments it is not clear how much the core model is guiding the results, and how much the intuition of the modeler is guiding the results. In both cases the intuition of the modeler plays a central role in determining the model's results. I have not kept up with macro econometrics and my assessment is based on the assumption that current practices have not changed from past practices.

Let me explain where my suspicion comes from. I came of age in the macro econometric modeling in the 1960s when large structural IS/LM macro models were central to macroeconomics. Each of the major models of the 1960s had 100s of equations that captured the various sectors of the economy, but which were, by today's standards, rudimentary. The model would be divided into sectors and subgroups of researchers would estimate the equations that specified the sector. The head modeler would put the equations together, and run the model. Inevitably the initial results were so far from believable that they had to be modified and adjusted. So researchers would go back to the drawing board and adjust or tweak the underlying equations and run the aggregate model again. And again, and again… Adjustments would continue until the model came out with a reasonable forecast.

I am not suggesting that this was a bad way to do macro empirical policy work; I certainly had no better way. But, the process of adjustment in the model's conclusions suggested to me that the head modeler's intuition, not the structural model, was not driving the results. I lost faith in macroeconometrics of the time. My sense was that in the end, there were so many modifications and ad hoc adjustments made to the structural model that no one had any idea of precisely what structural model was being tested. One was simply fitting the model to the data.

The actual results were generally not that bad. But that was not because of the model. It was because of the modeler. Since working with data and the model provide the modeler with an intuitive sense of how the economy works, over time, modelers gained an understanding of the economy, which leads to better predictions than predictions made by individuals who did not immerse themselves in the data.

Differences in policy methodology: complexity micro foundations and creative theorizing

While I see less of a role for macro theory in guiding actual monetary and fiscal policy analysis, I do see theory as having an important role in a complexity vision of applied policy research that it does not currently play. The role does not involve guiding monetary and fiscal policy. The role is, instead, a creative design role – exploring how theoretically institutional changes might affect the compositional black box through which micro decisions of agents are connected to aggregate results, in a way that leads to preferable outcomes. That role is what I call creative theorizing. If we find that the institutions are not working, then the policy role for complexity economists is to theoretically and empirically explore how those institutions might be changed to better coordinate the system. For macroeconomics, this means that the key policy question is: Can we develop institutions that would better coordinate aggregate results?

Monetary and fiscal policy can be seen as policies that might better coordinate agent's decisions so that they lead to more desirable aggregate results. They change the compositional black box, with the government trying to counteract agent decisions that lead of undesirable results. Fiscal policy involves the government varying its spending in a countercyclical way to smooth out fluctuations. Monetary policy involves the central bank modifying the financial environment affecting agent decisions to smooth out fluctuations. From a complexity vision standpoint, they are proxy policies that modify existing institutions to attempt to better coordinate aggregate decisions on spending. But, assuming no transactions costs, (as is the case with most economic theories of markets) there are, theoretically, much better ways to bring about the desired coordination. Creative theorizing explores those alternatives in the search for alternative methods.

The theoretically neatest way to do this would be to create markets in the output dimension one wants to control. To solve a coordination problem with an existing market, you simply create an additional market by creating property rights in the outcome that you want to coordinate, and allowing trading those outcome rights, and presto, the new market solves the coordination problem. The policy role I see for complexity economists involves exploring those alternative market institutions theoretically, and then seeing if any of these alternative institutions can be actually used in practice.

Complexity policy macro economists would explore many possible institutional structures, seeing how they work in abstract models, and determining whether they have analogs that might be possible to implement. So, from a complexity standpoint, a major role for macroeconomic theorists that they are not playing is the role of design engineer. In that role they explore ways of adjusting institutions, or creating new institutions, that make it so that micro decisions lead to preferable aggregate outcomes.

Here is the reasoning: If one wants different aggregate results, one need to explore policy changes that will lead agents to make different choices than they currently make. The policy research agenda is to explore alternative institutional structures that will better coordinate individual decisions. Instead of asking, what will the result of individual actions for the aggregate economy, one asks, how can we solve coordination problems? One way to do that is through backward induction and mechanism design, in which one specifies the aggregate results one wants, and explores alternative institutional structure would lead to that outcome

in a model. This is what I call the Coase method. One posits zero transactions cost and creates a property right structure guiding agent behavior to the desired result.

Let me give an example. Say you desire a system that has zero inflation. Such a system would require that all price changes be relative price changes, not price level changes. That could be achieved by an institutional structure in which whenever someone raised their price, someone else would be required to lower their price by an offsetting amount.

It was precisely such an approach that I developed with Abba Lerner and Bill Vickrey in the 1970s when inflation was seen as a major problem. The plan was called MAP, which stands for market anti-inflation plan. It consisted of assigning property rights in appropriately defined value added prices, so that any agent wanting to change their value added price had to pay another agent to change their value added price by a countervailing amount so that the index of prices would not change.[10] Any agent wanting to raise (or lower) their price would have to offset the effect of that by buying the right to do that from someone who lowers their appropriately weighted price by an offsetting amount. With a MAP system in place, all price changes had to be relative price changes, not absolute price changes. Theoretically, the market solved the inflation problem. (The proposal works equally well for stopping deflation. If there are deflationary pressures, an individual lowering his or her price would have to pay someone else to raise theirs by an offsetting amount.)

If there was inflationary pressure, the price of raising prices would be positive, and that price would offset any inflationary pressure. So with MAP there could be no inflation, no expectations of inflation, and no acceleration of inflation. Instead of a tradeoff between inflation and unemployment, there would be a tradeoff between the price of raising price and unemployment, so if unemployment was being used to hold down inflation, that unemployment could be eliminated since the MAP program was holding down inflation. In this model, monetary and fiscal policy had a role to play in fighting inflation, but it was an indirect, not a direct, role. Monetary and fiscal policy might affect the price of raising price, and thereby change the steady state equilibrium unemployment rate of the system.

My interest in the plan was primarily theoretical – to try to better understand the inflationary process. Bill Vickrey and Abba Lerner both thought that MAP was implementable, and strongly advocated it. The profession did not agree, and it lost interest in the plan. I argued that even if the plan was not worth implementing, it was nonetheless important in terms of our theoretical understanding of macroeconomics. It removed the issues of price controls and incomes policy from the theoretical debates, and put it in the practical implementation debate involving transactions costs. Markets have costs, as do all methods of coordination. All coordination problems can be thought of as problems of missing markets, but they may be missing because they have too high transactions cost compared to the benefits they provide. Within the missing market policy frame, an incomes policy was simply a replacement for a missing market, and is as consistent with macro theory as is any other policy. The debate about incomes policies and price controls should be about alternative goals of policy and about transactions costs of different policies, not about macro theory.

The backward induction approach to policy is not limited to inflation. To show the usefulness of the backward induction approach to micro foundations, let me discuss another “macro

[10] Obviously there are a number of technical issues involving price indices that I won’t go into here. They are discussed in a number of articles and a book I did with Abba Lerner, (Lerner and Colander, 1980). The price index stabilized was value added per unit input prices, not output prices.

policy solution" that follows from it, and what that solution means for the debate about activist monetary and fiscal policy. Within the complexity approach aggregate output fluctuations occur because there is no explicit coordination mechanism in the economy to determine aggregate spending. If there are unwanted fluctuations, then the fluctuation is being caused by a faulty institutional structure that is not coordinating individuals' demands in a way that is socially desirable. The backward induction solution to the problem would be to create property rights in spending, so that anyone who wanted to increase their spending would have to buy the right to do so from someone else who decreased their spending by an offsetting amount (and vice versa). A system with such a property right system in place could eliminate aggregate income fluctuations.

If spending were too low, the price of spending rights would be negative, spending would be subsidized, leading agents to spend more. If spending were too high, the price of spending rights would be positive, leading agents to spend less. The greater the deviation of desired spending with actual spending the higher the price of spending rights would be. With property rights in spending, price level changes would not be needed to stabilize the economy. The appropriately designed spending rights market would adjust the price of spending to an aggregate level consistent with the desired aggregate level of spending.

Let me be clear; I'm not advocating that such a market to be created. But I am arguing that thinking about the aggregate output fluctuation problem in this way suggests the uselessness of the debate about whether an activist monetary and fiscal policy is consistent with macro theory. In the complexity research program there is no theoretical foundation to macro policy needed; we know how to theoretically solve the problem with the market – create property rights in the action needing coordination. Whether that is a good policy is a practical institutional question, not a theoretical question. Policy research would explore the costs of various coordinating mechanisms compared to the cost of fluctuations.

There are many variations of this spending market plan that could be developed. Fiscal policy is a partial solution that involves one agent – government – doing all the adjustment. Theoretically, it would be preferable to have all agents doing the adjustment based on their cost of adjusting spending. In theory, the market in spending rights achieves this end. Thinking about such abstract alternatives and whether those abstract markets suggest any practical alternatives is what is meant by creative theorizing.

Let me emphasize once again that I am not advocating implementing these markets as an actual policy.[11] Rather they are presented as examples of the type of creative policy thinking that I believe macro economists should be doing as part of complexity policy analysis. By thinking about abstract policies that help "solve" the coordination problem, creative theorizing directs debate away from theoretical debates about what causes the coordination failure within an hypothesized economy that has never and can never exist, and toward the question: what can we do to reduce coordination problems and achieve a more desirable result.

[11] One macro market that I believe is worth exploring is a countertrade market in which as part of a broad trading agreement countries use these markets to keep their international trade balance within agreed upon limits. It would operate in a similar fashion to Keynes's Bancor plan and require surplus countries to share in the adjustment process. See Colander, 2017.

Conclusion

This has been a fast and broad-brush overview of macroeconomics, complexity and microfoundations. It differs significantly from the standard history of macro, in that it sees the relevant theoretical debates as debates about dynamic adjustment and policy debates about pragmatic methods of coordination. It leads to a suggestion for an increase in mathematical complexity of theoretical macroeconomics, but no direct application of the models to policy, which are seen as institutional based decisions that theoretical macro models shed little direct light on. Macro models are used as reference tools, not direct guides to policy. Economists are a long way from such a complexity approach, but I remain optimistic that in the long run they will adopt it, perhaps even before we are all dead.

Bibliography

Bagehot, Walter (1873) *Lombard Street: A Description of the Money Market.* London: Henry King and Company.

Coase, Ronald (1994) *Essays on Economics and Economists, Chicago.* University of Chicago Press.

Colander David (2009) "Economists, Incentives, Judgment, and the European CVAR Approach to Macroeconometrics" *Economics E-Journal* 3(9), pp. 1-21, April.

Colander, David and Craig Freedman (2019) *Where Economics Went Wrong*. Princeton: Princeton University Press.

Colander, David (1996) *Beyond Micro Foundations.* Cambridge: Cambridge University Press.

Colander, David (2006) *Post Walrasian Macroeconomics, Beyond the Dynamic Stochastic General Equilibrium Model*. Cambridge: Cambridge University Press.

Colander, David (2017) "How to solve the trade problem: the generalized buffett countertrade plan" *Eastern Economic Journal*, Vol. 44, Issue 2.

Davidson, Paul (1978) *Money and the Real World, London.* London: Palgrave Macmillan

Fisher, Franklin (2011) "The stability of general equilibrium – what do we know and why is it important?" In *General Equilibrium Analysis: A Century after Walras*, P. Bridel (Ed.). London: Routledge.

Frisch, Ragnar (1933) "Propagation problems and impulse problems in dynamic economics." In *Economic Essays in Honour of Gustav Cassel,* Gustav Cassel (Ed.). London: Allen & Unwin, pp. 171–205.

Goodwin R. M. (1949) "The multiplier as a matrix." *Economic Journal*, vol. 59, pp. 537-55.

Jonsson, Petur O. (1997) "On gluts, effective demand, and the true meaning of Say's Law." *Eastern Economic Journal,* Vol. 23, No. 2 (Spring), pp. 203-218.

Koen, Billy Vaughn (2003) *Discussion of the Method: Conducting the Engineer's Approach to Problem Solving*. New York: Oxford University Press

Lerner, Abba and David Colander (1980) *MAP A Market Anti-inflation Plan.* New York: Harcourt Brace.

Shackle G.L.S. (1949) *Expectation in Economics.* Cambridge: Cambridge University Press.

Author contact: colander@middlebury.edu

SUGGESTED CITATION:
Colander, David (2020) "Complexity, the evolution of macroeconomic thought, and micro foundations." *real-world economics review*, issue no. 91, 16 March, pp. 2-19, http://www.paecon.net/PAEReview/issue91/Colander91.pdf