Introduction

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David Colander has been writing about economic methodology for over 30 years, but he goes out of his way to emphasize that he does not see himself as a methodologist. In one of our private email exchanges on a well-known economic methodologist's work, he told me that it looks interesting but is a bit too deep for his interests. As far as I know him, this is just a polite way of saying that he won't spend much time on that methodologist's work as it is irrelevant to what he sees himself as an economist/educator doing. Indeed, he once complained openly in a review on a book written by two philosophers of science that:

> Like economic theory, methodological arguments have a habit of taking off on their own into a philosophical quagmire through which most economists cannot travel, and from which most economists who try, do not return.
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> Just as economics has become remote from the real-world economy, so too has economic methodology become remote from what economists do. Methodology has become an end in itself, quite separate from a description of what is done in economics, or a prescription for what should be done. (Colander, 1997, p. 141)

Colander's interests in what is done and what should be done in economics led him to write about methodology from the perspective of an economist and economist watcher, not from the perspective of a philosopher or methodologist of economics. For him, useful methodological discussion for most economists has a strong practical purpose; the profound philosophical questions that tend to attract methodologists have never been the focus of his methodological writing, and he argues that they are quite irrelevant for most economists.

Colander does not deny the importance of fundamental philosophical questions to methodology. But he argues that what economics needs much more is some rough and ready hands-on guidance on the workaday economic problems that economists experience as they do their research. Methodologists who do not deal with such workaday problems have essentially removed most of their usefulness to the profession.

Despite his limited engagement with formal methodologists, it would be a mistake to think that Colander's writings on methodology are not of significance to them, because if he is right, his arguments present serious challenges to the standard economic methodology literature at a foundation level. There are at least two challenges raised by Colander's methodological work which methodologists should not neglect. The first challenge concerns the subject-matter of the methodology of economics and the role of economic methodologists; the other concerns the nature of economics. They are related and the second challenge is particularly crucial, because if Colander is right about the nature of most economists' work, then their current practice is based on a shaky ground. If it is indeed true, as Colander claims, that the nature of the problems which most economists are dealing with is not science, then it also implies that the majority of methodologists who treat methodology of economics solely as methodology of science have failed to get the grand question concerning the nature of their discipline right.

In Colander's view, the relevant methodology is embedded in the workaday activities of economists: one cannot understand economists' methodological practices through abstract methodological discussion; one can only understand economists' work by understanding their craft, the institutional incentives they face, and the specific problems they are trying to solve. Colander argues that, generally, these problems are not pure theoretical or formal empirical problems; they are practical policy problems that require a topic-specific methodology that generally differs from the methodology deemed as 'scientific' by philosophers of science or methodologists. To constrain economists' methodology to fit a precisely determined scientific methodology does not make sense. Doing so, Colander argues, has caused a problem of cognitive dissonance to economists. He argues that currently the standard practice of academic economists involves serious methodological mistakes which result in various problems, such as the disconnection between models and reality and the neglect of needed value judgments. His explanation for why economics is making such mistakes is that economists are attempting to conform to two conflicting goals – to follow a scientific methodology and to do solid policy analysis. Since the two methodologies are different, if one has to meet both criteria, the only answer is the cognitive dissonance that economists demonstrate in justifying their approach. It is such questions that Colander wants methodologists to engage. He believes that methodologists should be telling economists they are making such methodological mistakes, and offering ways to avoid them. But that doesn't happen in most literature on economic methodology. The difference in his views regarding what methodologists should do, and what they actually do, is so huge that he decided to leave behind almost the entire economic methodology literature of the twentieth century and follow his own route.

A continuation of the Mill–Keynes tradition

Colander's methodological approach may appear eccentric in the landscape of the modern methodology literature, but it is not so unconventional if seen from a longer-term perspective in the history of economics. As he emphasizes, the methodological approach he advocates is not unique to him, but is, in fact, part of long tradition that is to be found in Classical economic writing. Specifically, he sees his methodological approach as a continuation of the Mill–Keynes approach, updated for developments in computational and analytic techniques. According to Colander, the Classical methodological tradition he has been following fully understood the limitation of scientific methods in providing answers to practical policy questions and hence had a much better balance between scientific methods and other methods when approaching this type of question. His claim is that this Classical approach was lost, unfortunately, in the neoclassical period as economists tried to be more scientific than was possible when dealing with policy issues. That desire to meet scientific standards led them to spend too much of their time analysing issues abstractly and developing models that deviated far from the real-world problems faced by policy makers. Methodologists followed suit. They focused only on whether economists were following scientific methodology, not whether they were following a reasonable methodology.

In Colander's view, most of what economists do is applied economics, which is, following John Neville Keynes, neither normative nor positive economics; instead it belongs in a third category – the art of economics. This tripartite division of economics guides Colander's thinking about methodology and can be seen as one of the foundation stones on which he builds his arguments of methodology. Only when one understands the way Colander divides economics can one make sense of his claims for economic methodology.

In this tripartite division of economics, positive economics is part of science. So economists doing positive economics should take guidance from scientific methodologists on the appropriate methodology for positive economics. Normative economics is a part of moral philosophy. So economists doing normative economics should take guidance from moral philosophers on the appropriate methodology for normative economics. Applied economics is a blend of both of these, and it is here where most economists need pragmatic methodological guidance. Such pragmatic methodological guidance is largely missing in the literature.

Methodology based in engineering, not science

For Colander, applied economics is best thought of as engineering, not as science.[1] Following Billy Vaughn Koen, Colander makes a clear distinction between engineering and science and subsequently between the engineering methodology and scientific methodology. In engineering, the primary goal is to solve real-world problems; in science, the primary goal is to find the truth. It is the difference in goals that leads to the difference in methodology.

By engineering methodology, Colander means 'the strategy for causing the best change in a poorly understood or uncertain situation within the available resources' or 'use the best available engineering heuristics to solve problems' (Chapters 4, 9 and 17, this volume). It uses any information available. In contrast, the scientific methodology focus is on knowledge that meets generally accepted scientific criteria. The requirement rules out historical knowledge, intuition, and guesstimates. That scientific methodology doesn't work best for engineers who have to come up with an answer to a problem.

When given a problem to solve, engineers need to arrive at an answer even if some facts are unknown to them or certain observed phenomena cannot be explained by existing scientific theories. The way they fill the gap between scientific knowledge and solutions is to make judgments based on all the information and tools they have; judgments that do not and cannot meet strict scientific standards. Thus, for example, if only a small amount of data is available, they will use that data to provide a rough estimate, at the best level of confidence they can, increasing the fudge factor to account for the limited accuracy. In making these judgments engineers will introduce whatever evidence they can, and then, through experimentation, attempt to gain better understanding of the underlying properties through experience with building the actual product, or with tailored experiments designed to better understand the relationship.

Engineers often use backward induction thought experiments, hypothesizing from the desired end product to the underlying relationships needed to get them there. Then they try a small experiment to see if it works in practice. If it does there must be some scientific foundation for it, even if it has not yet been discovered. Engineering is very much a creative exercise that precedes science as much as it is built on science. This entire process of working directly with a specific goal, separate from finding the truth, is what Colander calls the engineering methodology, and it is that methodology that Colander argues that applied economists should follow and methodologists should focus on.[2]

Complexity vision and economics

A final introductory comment that should be made about Colander's methodology concerns its relationship to complexity economics, which Colander argues is the future of economics (Colander, 2005). The fact that abundant yet diverse work in the literature has been associated

with complexity economics leaves it unclear what exactly constitutes complexity economics. In Colander's use of the term, it involves a complexity vision of the economy, a scientific complexity theory of the economy and a complexity approach to policy analysis.

With a complexity vision, one sees the economy as a complex evolving system undergoing continual evolutionary change; that is, the economy is seen as an interconnected collection of co-evolving, adapting agents interacting at a variety of time/space scales, evolving in complicated ways in which path dependencies, novelty, and emergent structures play a major role. This is a vision shared by many economists who see themselves as doing complexity economics.

The element in Colander's version of complexity economics that is less commonly noticed is its importance for applied policy. According to Colander, the approach adopted by policy-focused complexity economics moves away from thinking that general abstract models are going to lead to direct policy results. Instead, in policy-focused complexity economics, the researcher has a vision of the economy, which he or she uses as a general guide, and the researcher refines that vision with multiple models designed to answer one of the multitude of questions relevant to the policy at hand. This complexity approach, which has, at its core, a loose vision, rather than a narrow well-defined theory, he argues, is much more conducive to his pragmatic methodology than is the current Walrasian general equilibrium vision, which most economists consider the core of economics.

Another difference between complexity economics and much modern standard economics structured around a Walrasian general equilibrium vision is that Walrasian modern economics attempts to analyse economies with a set of analytic and computational tools that were state-of-the-art analysis decades ago, but which no longer are. Complexity economics embraces new analytic and computational technologies. In Colander's view, the adoption of complexity economics, and its new analytic and computational technologies, will lead to the adoption of the methodological position he advocates, because recognizing the complexity of the system undermines the ability of a researcher to rely on a single precisely-specified general theory to anywhere near the degree to which modern economics relies on Walrasian general equilibrium theory.

This view of changing analytical and computational technology allows Colander to see the art and craft applied policy methodology he advocates not only as the methodology of past Classical economics, but also as the methodology of the future. Colander argues that complexity economics will become seen as a continuation of Classical economics updated for new analytic and computational technology, with neoclassical economics being seen as a diversion.

Note that despite being highly enthusiastic about complexity economics as a scientific program, Colander does not consider it the major task for most economists to study it. To study it rigorously requires high-level mathematics that is often not suited to policy questions and to the training and ability of most economists. Colander argues that the policy research program in complexity economics will be a pragmatic combination of current practices modified to incorporate new analytic and computational technology, interpreted within a complexity evolutionary framework.

According to Colander, it will likely take decades for the complexity scientific research program to replace the current Walrasian scientific research program both because researchers are vested in the current program, and because the complexity research program is still in its

infancy. Thus, were one to argue that applied economics was only applied science, then the complexity vision would have no effect on policy. But Colander argues that that is not the case. He argues that we do not need to wait for a paradigm shift to occur in the scientific branch of economics in order to adopt a new methodological approach for applied economics. A policy research program shift often precedes a scientific research program shift. According to Colander, the complexity policy research program, based on data science, agent-based modeling techniques, and increased use of non-linear dynamics is changing the way applied economic research is done, and will eventually change the science of economics.

Structure of the book and articles selected

Colander has written 80 or 90 articles related to methodology. In this book, I have selected 17 of those articles that give the reader a sense of his approach. The articles are organized into four parts. Part I puts together the articles that provide a framework underlying Colander's methodology and introduces Colander's methodology for economic policy within that framework. Part II introduces Colander's view on the methodology for microeconomics. Part III looks at Colander's methodology for macroeconomics. Part IV looks at some broader issues.

Part I: Methodological framework and methodology for economic policy as art

I begin with a fun piece, 'Why Aren't Economists as Important as Garbagemen?' that was written in 1987 when Colander started forcefully arguing that what academic economists were studying had gradually drifted away from the real economic world. While he had written other methodological articles before then, this article can be seen as the starting point of Colander's series of writing on reflecting the methodology of economics and the economics profession.

Intriguingly, for Colander, the main reason why much of economists' research has lost contact with the real world is not because economists have attempted too little. On the contrary, it is because they have been trying to do something unachievable – to avoid all value judgments in their analysis. In his own words, 'economics tries to do too much – to be too objective, to be too fair. The only way economics can do that is to make it irrelevant, and that's what it has done' (Colander, 1991, p.7). The discussion in Chapter 1 thus served as Colander's first broadside attack on welfare economics. After 30 years, the core of Colander's argument remains the same and it is applicable to current applied economics in general. The relevance of the article for today is largely due to his insights into the nature of economists' role in society. How economists perceive their role will determine what they do as economists.

In Chapter 1, Colander argues that economists today do not play a more direct role because they have a fundamentally incorrect view of the nature of their role. Specifically, he argues against what he calls the 'outside perspective', that is, a perspective whereby economists view themselves as outside the economy when judging how well the economy is functioning. The outside perspective has made the natural/unnatural dichotomy pervasive in economic terminology and policy analysis. In Colander's view, this natural/unnatural division is not a helpful way of looking at society. In fact, it often causes confusion as if the division itself can serve as an appropriate basis for evaluating policies. It is not incidental that the neo-liberal thought can easily latch onto the mainstream economics to become the dominant economic thinking over the past decades; with the outside perspective, the laissez faire policy conclusion is preordained. Ironically, the main reason for economists to adopt the outside perspective is

to avoid value judgments in their analysis. Their attempt to be objective has resulted in a paradoxical situation. On the one hand, economists have made their research irrelevant. On the other hand, their seeming neutral position has led to an outcome opposite to their intention – the natural/unnatural narrative following from the outside perspective has been used uncritically to support certain policy standpoints. If, as Colander argues, the criterion of objectivity required by science has been unduly imposed by economists onto their policy analysis, it implies that in order to make economics relevant again, economists have to liberate themselves from the straightjacket of scientific methodology when doing applied policy analysis.

After the first publication of 'Why Aren't Economists as Important as Garbagemen?', Colander continued analysing methodology in a series of papers trying to provide a theory of why what happens in economics happens. He used what he called an 'economic approach to methodology', that is, an approach that examines how economists' research behavior has been influenced by the institutional incentives under the assumption that economists are primarily self-interested people, rather than assuming that economists always aim to search for the truth regardless of the institutional incentives, as most approaches to scientific methodology implicitly do. It is close to a sociological analysis of economists' behavior. Chapter 2, 'Vision, Judgment, and Disagreement among Economists', is one of those articles adopting such an economic approach of reasoning and provides insights into the essential role of vision and judgment in not only choosing among policies but also in selecting among assumptions and models, which was traditionally believed to be value-free.

Chapter 2 starts with a challenge to a common view that there is too much disagreement among economists. For Colander, significant disagreement is both to be expected and desirable given the nature of the questions economists ask. The real problem for him is that economists often disagree about the things they should agree about, and agree about things they should disagree about. In the chapter Colander argues that disagreements among economists are often due to the differences in vision of how an economy operates and the differences in judgments about how the political system would implement a policy or on what effect the policy will have on existing institutions. Yet economists often shy away from this type of difference, because those differences are deemed subjective or non-scientific and cannot be captured in formal models. Colander calls this fear of discussing difference in judgment and vision 'artiphobia'. Partly as the consequence of the artiphobia, economists spend enormous amounts of time disagreeing about what Colander considers relatively minor modeling issues. This chapter explains the formulation of this artiphobia among economists by analysing institutional incentives that guide economists' actions. It argues that this type of differences cannot be resolved by formal empirical testing: only when economists accept an open treatment of the disagreements can we better handle the debates about policy.

To have a good sense of the term artiphobia introduced in Chapter 2, it needs to be read in the context of Colander's writing. Chapter 3, 'Retrospectives: The Lost Art of Economics', is the first article in which Colander directly argued that applied economics should be treated as art, rather than science. It presents Colander's methodological argument within a historical context, showing how his interest with methodology is blended with his interest in history of economic thought. In it Colander returns to Friedman's famous article on positive economics, in which he cites J.N. Keynes as his forerunner. What Colander points out is that Keynes's view was fundamentally different than Friedman's and Friedman and economic methodologists

have totally lost sight of a central part of Classical and Keynes's methodology – the view that there is a tripartite division of economics, not a bipartite division. Following Keynes, Colander argues that the study of economic policy belongs in the art of economics, not in the positive science of economics nor in the normative branch of economics. This division of economics sets the stage for Colander's later writing on the methodology of economics. He doesn't claim to be making new arguments; he is simply restating Classical economists' arguments.

With all the methodological debate about Friedman's essay, how could methodologists have missed that simple division and sleight of hand by Friedman? Chapter 4, 'The Systemic Failure of Economic Methodologists' provides Colander's answer to that question. Academic methodologists were lost in their own little debates and weren't worried about applying their arguments to economics, but were more interested in making esoteric points and winning academic debates. To do that, they found it useful to take implicit neutral positions on economists' work. They didn't say – this approach does not make sense if one's goal is to answer policy questions; they instead tried to fit economists' work into a scientific methodology.

Colander challenges that neutral standpoint held by many methodologists, and argues that, by trying to maintain a neutral stance, and not making judgments, economic methodologists have shirked their moral responsibility to the economics profession and to society. In this chapter one can see clearly Colander's standpoint regarding the responsibility of economic methodologists to make judgments and make the basis for those judgments clear. In this chapter Colander also elaborates on what he means by an engineering methodology, and why he believes that engineering methodology is the appropriate methodology for the study of economic policy.

Colander is often known as a critic of modern mainstream economics. Unlike many other critics calling mainstream economics neoclassical economics, Colander is wary of using the label neoclassical economics to refer to the economics he is criticizing. In his view, economics has changed significantly over the past one hundred years since the term 'neoclassical economics' was initially coined by Thorstein Veblen in 1900. He argues that the moniker cannot capture the ever-changing dynamics of economics nor can it capture the diverse content of what economists are doing today. More importantly, the inconsistent, sometime even schizophrenic, use of the term neoclassical economics is detrimental to constructing effective methodological discussion. For Colander, calling current economics neoclassical economics does not add anything to, and often obscures, our understanding of the current failings of economics. From the methodological point of view, adopting a new classifier that can provide a more accurate picture of economics today is a crucial step forward in order to better understand the methodological problems of current economics and to enhance the effectiveness of conversation in the methodological literature.

Chapter 5, 'The Death of Neoclassical Economics', is another fun piece to read. It provides Colander's explanation of why the term neoclassical economics should die. The chapter was initially his presidential address to the History of Economics Society in June 1999, but its theme goes beyond the realm of history of economics. It argues that what defines modern economics is not content, but method, that is, the modeling approach to problems. 'Modern economics', according to Colander, 'is economics of the model'. Note that this is a descriptive definition, not a prescriptive one. It does not suggest that all economists should do is modeling; it simply describes the phenomenon that modeling is the central element of modern economics.

That said, Colander indeed conveys a prescriptive message in this article regarding how to use models. After examining how the use of models has changed over time in both micro and macro, Colander praises the modern movement to applied modeling, but he also raises the alarm about the undue weight economists gave to formal empirical testing of models. In his view, it is not possible to test many of the applied models economists use in a formal manner without being ad hoc, given that the assumptions of those models are themselves ad hoc. But he doesn't see this as a real problem to modern applied economics because in his view applied economics should be treated as engineering, not science, and it follows that applied models can still be meaningfully tested in an informal empirical way that works for the needed engineering knowledge. To Colander, the real serious problem of modern applied economics is the way economists try to avoid the semblance of ad hoc pontificating by structuring their models in scientific clothing.

Part II: Methodology for microeconomics

In Colander's view, abstract modern theory and models are extremely useful for a subset of policy problems in micro that involve constrained optimization. Examples include allocation problems and algorithmic shadow price models. The problem is that there are many other problems where economist's standard models aren't that useful, and economists haven't distinguished these from those where it is useful. The problem has two dimensions. First, the dominant optimal control framework is not suitable for studying numerous problems and questions that belong in a micro framework. For example, what policy should one follow if tastes are partially endogenous, if people are highly uncertain about what their goals are, if a number of people find markets morally unacceptable, or if results of a policy could be changed by changing the institutional structure? Or what policy should we follow when people's views of the goals of policy differ widely? The optimal control framework is not especially helpful with such questions. Second, even in those areas where the control framework of analysis seems appropriate, the application of theory or model to the policy often goes consistently wrong. These are normal occurrences and Colander argues economists need methodological guidance on how to use their models in real world situations.

In Chapter 6, 'Applied Policy, Welfare Economics and Mill's Half-Truths', Colander argues that that methodological guidance for applied economics has already been provided by J.S. Mill. He argues that the economics profession needs to return to Mill's earlier methodological approach, which held a strict separation between policy and theory and saw models as aids to judgment, not as definitive guides to policy. This chapter first examines how this traditional methodological view got lost in the development of economics in the twentieth century and then suggests how the understanding of this difference in methodology between the classical and modern approaches may affect economics policy training.

Chapter 7, 'A Failure to Communicate: the Fact–Value Divide and the Putnam–Dasgupta Debate', co-authored by Colander and myself, can be seen as an extension of the argument in Chapter 3 and an application of the Mill–Keynes approach to understanding some of the contemporary economic policy work. It is often believed by philosophers that the fact–value dichotomy has impoverished the ability of welfare economics to evaluate economic well-being and that it is impossible for economics to be free from ethical values. Hilary Putnam, for one, is a strong advocate for such a view. His view led to a long-lasting debate between him and Partha Dasgupta. By exploring and disentangling the debate between these two leading

scholars, this chapter demonstrates that the so-called fact–value divide is a pseudo-problem in the Putnam–Dasgupta debate, and that there is much more agreement between good economists' methodology and philosophers' desired methodology than generally recognized.

Chapter 7 reiterates the argument in Chapter 6, and uses the works of Mill and J.N. Keynes to clarify the mixing up of the art–science distinction and the logical positivist fact–value dichotomy. In the Mill–Keynes tradition, the separation of applied economic policy analysis from the scientific branch of economics is meant to enhance the quality of the latter by improving the understanding of economic phenomena through adopting appropriate methods for the question at hand.

Chapter 8, 'Framing the Economic Policy Debate', analyses how the currently dominant frame of economic policy came to exist. It does so by examining the history of economics from the perspective of analytic technology. In doing so, Colander argues that the rise of the 'market failure' policy frame in the 1930s is highly related to the introduction of multivariate calculus into economic theory at the time.

The reason why modern microeconomics has been focused on analysing efficient allocation problems rather than other aspects of economic policy cannot be separated from the fact that the allocation problem can be easily portrayed as a LaGrangian constrained optimization problem. Looking in this light, Colander argues that the market success policy frame variants based either on Buchanan and Tullock's government failure policy frame or on Stigler and Coase's promarket policy frame are merely adjustments, not alternative frames to the market failure policy frame developed by Samuelson and Lerner. Moreover, given the analytic technology, the market failure frame was more teachable and better fit the evolving pedagogical needs of the economics profession at the time. After analysing how the rise and dominance of the market failure policy frame is tied to analytic technology, Colander suggests how the changing analytic technology can open up the possibility for movement away from the market failure policy frame.

As the economics profession evolved, Colander's thinking of methodology also has evolved. While the core ideas in Colander's methodology have remained mostly the same over the decades, new elements have been added in response to the recent development in economics. Chapter 9, 'Complexity Economics and Workaday Economic Policy', brings Colander's reasoning to his latest framework which emphasizes the importance of explicitly recognizing the economy's complexity within economics and within economic methodology. Colander argues that a complexity evolutionary framework will supplement the Walrasian general equilibrium framework for thinking about policy. That change is not dependent on a paradigm shift, because the policy framework is not dependent on the existing scientific paradigm in economics.

In Colander's view the tendency of methodologists and economists to think of economic policy as a direct application of economic science has made them miss the importance of the complexity revolution, which currently is a much greater revolution for policy than it currently is for science. The standard argument is that since complexity is not ready to become the new paradigm in economics, it has few implications for applied policy. For many economists, the story about complexity economics ends here. Colander challenges this common view shared by economists. He argues that once one recognizes that applied policy is engineering, rather than direct application of science, the importance of the complexity revolution is clear. Thus, the complexity framework still can free applied policy analysis from the Walrasian general

equilibrium paradigm because in that complexity framework abstract theory has little direct relevance to workaday economics. This article also shows how the acceptance of complexity framework can supplement the standard allocation policy with formation policy, a set of policies designed to influence the ecostructure within which individuals operate. It uses distribution policy as an example to demonstrate the difference between complexity policy and the standard policy.

Part III: Methodology for macroeconomics

For Colander, the complexity vision is applicable to both microeconomics and macroeconomics. However, accepting this vision means that the appropriate methodology for each is different. Given the dominance of the current general equilibrium paradigm, the complexity vision has particular importance for macroeconomic policy. Unlike in micro, where the general equilibrium models are applicable to certain problems, in macro the dynamic complexity of the macro economy makes it next to impossible for economists to capture the macro economy with precise formal models. It follows that the attempt to develop specific general equilibrium theories to predict how macro economy works is doomed to fail. Nevertheless, this does not mean that economists cannot develop applied macro theories. It only means that their applied theories will be highly limited and ad hoc. Thus, when discussing macro theory, economists should make it clear that they are talking about heuristic theories and not presenting their models as pure scientific theories.

Chapter 10, 'The Macrofoundations of Micro', discusses a methodological perspective that Colander calls a 'macrofoundations-of-micro' perspective, a then newly emerging perspective when the article was published in 1993 for the first time. By perspective or vision, Colander means 'a way of putting reality together'. This perspective can be seen as a reverse of the microfoundations-of-macro perspective, the dominant approach in macroeconomics since 1970s. The basic premise of the old perspective was that if an aggregate model were to assume any individual behavior, that behavior had to follow from a microeconomic choice theoretic framework. In contrast, the new perspective maintains that before there is any hope of undertaking meaningful micro analysis, one must first determine the macro context within which that micro decision is made, as well as the micro context for macro results.

More than twenty years after the article was written, the arguments in this chapter remain relevant to today's economics, especially with the rise of complexity economics. Between these two perspectives, it is the macrofoundations-of-micro perspective that fits better with the complexity vision of economy. In the past, economists used to perceive the economy as a mechanical system consisting of homogeneous atomic individuals. In such a system, the aggregate properties at the macro level was simply the adding-up of the properties of individuals at the micro level; there is no qualitative transformation from micro to macro. It follows that as long as we understand micro, macro can be understood with the same logic. But in recent years, more and more economists started to accept that emergent phenomena at the macro level generated by the interaction of individuals cannot be approached by the traditional microfoundations-of-macro perspective. This has significant implications for the way in which rationality is thought about; all rationality becomes context dependent. Although placed in Part III on the methodology for macro, this chapter also has implications to the methodology of micro. It argues that the macrofoundations-of-micro perspective should complement the microfoundations-of-macro perspective. In so doing, not only the way macro

is done will change, but also the way micro is thought about, since much of what economists currently teach as micro is outside of its macro context, and hence missing important complexity that creates a non-removable systemic uncertainty.

Colander has called his complexity interpretation of macroeconomics *Post Walrasian macroeconomics*, where by Post-Walrasian he means work that comes after Walrasian economics and that takes the complexity of the economy seriously, rather than assuming it away. He has written a number of articles and edited two books on Post Walrasian macroeconomics. The first edited volume (1996) focuses on outlining the problems with the Walrasian agenda; the second one (2006) goes further to specify a positive program that constitutes a Post Walrasian macro research agenda.

Chapter 11, 'Post Walrasian Macro Policy and the Economics of Muddling Through', is one of Colander's early writings on how an acceptance of Post Walrasian economics would significantly change the focus of macro policy discussions. In this chapter, Colander makes a contrast of what he calls 'muddling through' approach to policy in Post Walrasian economics with the traditional 'economics of control' approach to policy based in Walrasian economics. The two differ in how they relate theory to policy. The muddling through approach is a search for rules of thumb that work temporarily in a specific institutional environment since no universally agreed-upon model of the economy is available in Post Walrasian economics. It is essentially a pragmatic exploration of better, not optimal, policy in the environment in which policy makers do not know the underlying outcome-generating mechanism of the economy. In contrast, Walrasian economics assumes infinitely bright policy makers who face a full information environment. Hence, it is possible for them to take the economics of control approach, that is, a precise calculation of optimal policy by maximizing a social welfare function subject to constraints. Colander argues that, by moving from the latter to the former, economists will change the way they apply models to real problems and will be able to broaden the goals of policy from efficiency to distribution.

The financial crisis of 2008 led to significant criticism of contemporary economics and of the economics profession. Many people were asking and trying to answer why economics has failed to warn us. Paul Krugman in a well-known *New York Times Magazine* article 'How Did Economists Get It So Wrong?', suggested that Classical economists were blinded by the beauty of mathematics, and that Keynesian economics is the path of the future. Chapter 12, 'How Economists Got It Wrong: A Nuanced Account', is Colander's response to Krugman's argument.

Colander argues that Krugman's story is too black and white. He argues that the evolution of economic thinking is much more nuanced than Krugman portrays it. According to Colander, the systematic failure of the economics profession is closely related to their belief that the complexity of real-world economy can be captured by a unified model. In his view, both New Classical economics and Neo Keynesian economics suffer from that same problem of blending policy and theory, that is, using the conclusions of theoretical models as direct guides for policy precepts. It follows that what the economics profession needs to do is not to switch from New Classical economics to Keynesian economics as Krugman argues, but to return to the Classical tradition in the sense that it recognizes that the economy is a complex system and it is far too complex to be captured by any unified model. In this tradition, economists should not draw policy implications directly from models, including the most complex models. Models can be, at best, only rough guides to policy; they cannot determine policy.

Chapter 13, 'Economists, Incentives, Judgment, and the European CVAR Approach to Macroeconometrics', covers a wide range of issues discussed in this volume, including the institutional incentives for economists and the real forces behind the evolution of economic theories. By placing this article in Part III, we attempt to draw the attention of the reader to the contrast between the methodology of DSGE and the methodology that follows from a complexity view of macro – an empirically based co-integrated vector autoregression (CVAR) approach. Colander's argument that the data-first approach of CVAR is more reasonable than the theory-first approach of DSGE is forceful and needs to be taken seriously by anyone who believes that economic theories should not be detached from the reality as they currently are. In this chapter, Colander argues that doing good empirical work requires judgment that cannot be captured in the information normally reported in a published article and that if the CVAR approach doesn't get accepted as widely as DSGE, the reason is not that it isn't a better way to undertake macro policy analysis, but rather because it doesn't fit the current institutional structure of academic economics. The current structure tends to encourage publishing for career advancement, rather than understanding advancement, and the CVAR approach is less likely to lead to a 'publishable' article because it builds in the need for researcher judgment. For Colander, institutions guide methodological choices and can distort them from what an outside observer would say are reasonable ones. One can only understand a field's methodology if one understands the incentives built into its institutional structure.

Chapter 14, 'Beyond DSGE Models: Toward an Empirically Based Macroeconomics', is a joint article by Colander and four other well-known macroeconomists who share Colander's vision of how macro should be done. This short article was written as a type of Post Walrasian manifesto and presented at the American Economic Association meetings. It outlines the problems with current DSGE macro models and explains why having a complexity vision of the economy leads to a quite different way to do macro. It concludes by arguing that this new way of doing macro would be much more empirically based, following a data-first approach. It would use theory to interpret data rather than to guide and limit the search for data.

Part IV: Pragmatic methods for doing economics as a profession

The first three parts of the book deal with the methodology of doing economics relevant for both micro and macro. Part IV focuses on methodology in a broader sense. It sees methodology as embedded in the institutions of economics and argues that the only way to change economics is to change the institutions.

The development of economic theories and the economics profession has never been as simple as the story that the best always wins out. Some economists may genuinely be unaware of the problems of the methods they are using. But for most of them, sticking to what they are doing generally is the best choice for their professional career. They have no incentive to make changes to their practice as economists. After all, being an economist is not just about pursuing the truth or the best knowledge about the economic world. All individuals are driven by a blend of broad social goals and private individualistic goals. Institutions determine how that comes out. Changes will only come about through policies based on a deep understanding of those institutional structures which led to the current situation of the economics profession.

The occurrence of the financial crisis in 2008 gave many the hope that economics would change. Colander has never been one of them. He was pessimistic about the possibility of change because of his above institutional and sociological view regarding economics and the

economics profession. After the financial crisis, Colander gave testimony at two different Congressional hearings for the House Science and Technology Committee. Chapter 15, 'Written Testimony of David Colander', is the second testimony, in which he tries to persuade those who have the power over economic research funding to make structural changes to economics professions. This chapter first explains how economists failed society in the financial crisis in 2008. Colander focuses on two causes: first, the problems of the DSGE model; and second, the way in which the DSGE model has been used in guiding policy: Each of these two points is discussed more extensively by chapters in Part III. Then the chapter turns to the main theme of Part IV – the role of institutional incentives in shaping the development of modern economics and how those institutional incentives can be changed. It explains how the success of the DSGE model came as the consequence of the existing institutional incentives. The chapter concludes with two proposals for the National Science Foundation to change the way funds are allocated to economic research.

Like all other institutions and systems, there are strong forces preventing the economics profession from making the enormous effort needed to overhaul itself. Before the external institutional environment changes, it would be unrealistic to expect that most economists would have incentives to adopt new methodology. Does that mean there is nothing individual economists can do? Colander believes that keeping up a constructive and effective conversation with economists in the inner circle is important. He considers the approach currently used by critics or economists who are outside the mainstream as an approach that will not resonate with the mainstream circle. In several articles, Colander suggests some pragmatic approaches for those who see themselves as heterodox economists in order to establish better lines of communication with the mainstream economists.

Chapter 16, 'Moving beyond the Rhetoric of Pluralism: Suggestions for an "Inside-the-Mainstream" Heterodoxy', is one of the articles with the goal stated above. In this chapter, Colander first points out how the approach used by many self-described heterodox economists stops dialog with mainstream economists. In its place he advocates that heterodox economists consider an alternative strategy, which he calls an inside-the-mainstream heterodoxy. According to Colander, the best of the mainstream group are open-minded to new ideas and he encourages heterodox economists to direct their arguments to this open-minded group. His position is grounded on a rule that 'the only ones who are allowed to break to the rules are those who have demonstrated a full command of them'. Colander makes it clear that his suggestions are not directed at all heterodox economists, but he hopes his approach will be considered by some of the younger heterodox economists as a pragmatic alternative to current heterodoxy.

The financial crisis in 2008 triggered the public's concern regarding not only the competence of economists in doing their job but also their integrity. From the movie, *Inside Job*, one gets the impression that economists are ethically challenged because they take payments for writing papers that say what the funders of their research want them to say. In the last chapter of this volume, 'Creating Humble Economists: A Code of Ethics for Economists', Colander takes issue with that view, and suggests that the more serious ethical problem of economics has to do with lack of humility. By this, Colander means that economists have a tendency to convey more scientific certainty in their policy positions than the theory and evidence objectively would support. Colander's solution to this is to see economists as engineers, rather than as applied scientists. His hypothesis is: if it is true that economics is essentially

engineering, then a code of ethics for engineers should nicely translate into a code of ethics for economists. As a preliminary attempt, Colander arrives at a code of ethics for the economics profession by adopting the code of ethics for the National Society of Professional Engineers with a global change of the word 'engineer' to 'economist'. Colander argues that adopting a variation of an engineering code of ethics, which is an individual action oriented code, could create a professional ethic that is stronger and more inclusive than a code that deals with moral judgments such as 'opposing oppression' and 'giving voice to the needs and aspirations of the dispossessed'. Ultimately, Colander hopes that such a code will help to create humble economists.

Annotated bibliography and list of book reviews

The articles included in Parts I–IV should be seen as an introduction to, and a brief summary of, Colander's policy methodology. The ideas presented in them are further developed in other work. At the end of the book, I provide an Annotated Bibliography that can be used as a guide to further reading. In addition, there are other useful materials if the reader wishes to learn more about Colander's methodology. In some of his review essays, Colander made important arguments concerning methodological issues. Thus, I also include a list of book reviews by Colander which are relevant to the methodological issues discussed in this volume. Moreover, Colander has made a number of methodological points in his textbooks, especially in his principles text. His ongoing regular column 'Colander's Economics with Attitude' in the *Eastern Economic Journal* since 2013 is also a valuable source for those who are interested in keeping up with Colander's latest thinking about methodology. Each column is freely available on the website of the journal.

The richness in Colander's writing on economic methodology has inspired me to work on a book like this so that anyone who is interested in the methodology of economics would not miss his work and could approach it in a more systematic manner. Enthusiastic as I might be, I want to emphasize that the articles presented in this volume should be seen as a stepping stone, not a sorcerer's stone. Their role is to stimulate thought, not to provide final answers. Colander is comfortable with that. 'Pragmatic' and 'humble' are key concepts that characterize Colander's methodological prescriptions and also characterize his methodology. For him, ultimately, it is best to consider all methodology a heuristic that is to be judged only on its usefulness for the question at hand. That gives Colander's methodology its pragmatic sense. It is also humble – it doesn't try to answer deep questions nor does it pretend it could. Instead, it attempts to answer workaday questions that all economists face as they go about their work. More importantly, Colander sees that his approach must also be turned on itself; if everything is a heuristic, then so too is his methodology. It is not a deep truth, but simply a heuristic to be used when useful, and dumped when not.

Notes

1. In Colander's original writing, he contrasted engineering with applied science and argued that applied economics is engineering, not applied science. By applied science, he meant the application of scientific theories in the narrowest sense of application, that is only the theories derived from those methods which meet so-called scientific criteria can be applied to solving practical questions. In other words, methods like using judgments based on intuition or common sense are not legitimate methods. It is in this sense that Colander differentiated engineering from applied science. As this meaning of applied science is different from the daily loose use of the term by the public, and some engineers do see engineering as applied science but not science, I replace Colander's

own term 'applied science' with 'science' in this introductory chapter to avoid confusion. The reader should bear this in mind when they come across the term 'applied science' later in Colander's articles.

2. In a broader sense, Colander, following Koen, argues that science also follows an engineering methodology, but that it is an engineering methodology with a specific sub-goal – to discover the truth. He describes scientific methodology as a set of heuristics that have developed to guide researchers searching for the 'truth' and engineering methodology as a set of heuristics that have developed to guide researchers searching for answer a problem. The specific heuristics will differ as the problems addressed differ.

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