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The systemic failure of economic methodologists

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This paper argues that economic methodologists failed to point out to the profession or to policy makers that the method macroeconomists (and applied economists generally) were using was problematic, and therefore bear a portion of the blame for macroeconomics’ failure to prepare for the recent financial crisis. The reason they did not was systemic; they did not see doing so as their job. The paper argues that the systemic failure of the economics profession in the financial crisis and the systemic failure of economic methodologists reflect the same cause. Both groups see their primary role as detached scholars or as scientists providing abstract understanding, not as engineers whose primary role is to provide insight and analysis for individuals attempting to achieve better real-world outcomes. Rather, they see themselves as detached scholars. This paper argues that the roles should be reversed – the economics profession’s primary goal should be achieving better real-world outcomes, and its secondary goal should be better understanding of the economy for the sake of understanding.

Keywords: methodology; engineering; systemic failure; macroeconomics; value judgments; intuition

In Colander (2009, 2011), I argued that one of the causes of the recent financial crisis was a systemic failure in the economics profession. In this paper, I argue that one of the causes of the economics profession’s systemic failure was a parallel systemic failure of economic methodologists. Specifically, I argue that economic methodologists failed the economics profession by not actively pointing out to the economics profession or to the general public that, if an economist’s primary goal was to provide policy advice to society, then the standard methodology being used by applied macroeconomists had serious problems.1

I see methodologist’s failure as a systemic failure because the reason they did not point out to the profession or to policy makers that the method macroeconomists (and applied economists generally) were using was problematic was systemic; they did not see doing so as their job. They did not see their job as trying to affect economists’ methodology or even to make judgments about whether it was good or bad. Instead, they saw their job as trying to understand that methodology. Thus, when Solow (2007, pp. 235–236; 2008) bluntly stated that the modern macroeconomics was best seen as a ‘rhetorical swindle’ that ‘seems to lack all credibility,’ we saw no organized group of economic methodologists either supporting or attacking Solow. His strong statements about fellow economists were seen as a breach of academic decorum, acceptable because of his wit and fame, but not an issue that methodologists should actively weigh in on.

The systemic failure of the economics profession in the financial crisis and the systemic failure of economic methodologists reflect the same cause. Both groups see their primary role as detached scholars, or as scientists providing abstract understanding, not as
engineers whose primary role is to provide insight and analysis for individuals attempting
to achieve better real-world outcomes. Criticism of the economics profession’s failure in
the crisis, and my criticism of economic methodologists in this paper, is based on the
belief, which I believe is generally held by members of society, that the roles should be
reversed – the economics profession’s primary goal should be achieving better real-world
outcomes, and its secondary goal should be better understanding of the economy for the
sake of understanding.

The issue is one of primacy of engineering or scientific goals, not whether engineering
and science are related. Obviously, they are related; both indirectly contribute to the
other’s goal; better scientific understanding improves real-world outcomes, and solving
real-world problems leads to better abstract understanding. But primacy is important
because the primary goal determines methodology, and an engineering methodology
tailored to solve problems is different from a scientific methodology designed to discover
truth. If engineering is not applied science, and applied economics is primarily
engineering, applied economists should be judged by an engineering methodology, not by
a scientific methodology. Economic methodologists have not done that.

Economic methodologists’ interpretation of their primary role as detached scholar
searching for the truth is reflected in whom they see as their primary audience. Just as
applied macroeconomists see other macroeconomists as their primary audience, economic
methodologists see other methodologists, not the public or policy makers using economists’
advice, as their primary audience. This tendency to write for one’s peers leads scholars to see
themselves as detached scholars whose job is to discover the truth, not as active participants
in policy determination whose job is to see that society gets better real-world outcomes.
Writing primarily for one’s peers separates the actual real-world outcome and the scholar’s
work. Thus, following a scientific methodology, macroeconomists did not see themselves as
bearing any significant responsibility for the financial crisis. While Solow’s views that
macroeconomics were studying the wrong models may be shared by many in the economics
profession, one would not discover such shared views from reading most mainstream
economists’, or economic methodologists’, work because, from a scientific methodological
standpoint, it is not clear whether Solow is right. However, from an engineering
methodological standpoint, it is hard to come to any conclusion other than Solow’s.

Because macroeconomists see themselves as detached scholars – macroeconomic
scientists – they do not take responsibility for what happens in the macroeconomy.
Similarly, economic methodologists do not take responsibility for what macroeconomists
do. Methodologists do not see their primary job as seeing to it that applied policy
economists are using the best methodology they can to solve real-world problems. Since it
is not their job to see that economists use the best methodology, methodologists do not see
themselves as bearing any responsibility if the method actually used by macroeconomists
proves inappropriate, just as macroeconomists do not see themselves as bearing
responsibility for the economic crisis.

I am quite willing to accept that macroeconomists’ and economic methodologists’
detached approach may reflect the state-of-the-art scientific method. If an economist’s
goal is understanding for the sake of understanding, then I am willing to accept that there is
no systemic failure. But, in my view, scientific understanding is not the primary goal that
society wants from macroeconomics or from economic methodologists. Society primarily
wants help from economists in solving the problems it faces. What I am arguing is that the
scientific methodology is the wrong methodology to use, or to judge macroeconomists’
work in reference to, if the primary goal is solving the macroeconomic problems society
faces. The appropriate methodology for that goal would be an engineering methodology.
In the next part of the paper, I will outline a general engineering methodology and contrast it with an applied scientific methodology, discussing how economists’ and methodologists’ research and role would change were they to see themselves as primarily engineers rather than as primarily scientists or philosophers of science. Then, I will discuss institutional modifications that could change the situation to achieve what I believe would be better outcomes.

**The engineer’s general method**

The general methodology of science has been extensively explored by scholars. The general methodology of engineering has received little discussion. The reality is that engineers do not spend a lot of time writing about abstract methodological issues. Instead, there seems to be general acceptance of a general engineering methodology as spelled out by Koen (2003). Koen defines the engineering method as ‘the strategy for causing the best change in a poorly understood or uncertain situation within the available resources’. Koen argues that this definition is operationally equivalent to a second definition – *use the best available engineering heuristics to solve problems*.

Since Koen sees no part of knowledge as infallible, heuristics includes all theories and models, and any other aid, such as intuition, experience and expert knowledge that may usefully lead to a solution. In engineering, nothing is off the table. By explicitly calling the models and aids that an engineer uses to arrive at a conclusion, heuristics, he calls attention to any model’s problems and encourages a methodological openness in inquiry that is open to all evidence and arguments.

Koen does not discuss what the appropriate heuristics are for particular problems or even for a particular sub-branch of engineering. Such specifics are to be decided by the engineering community working on the problem, not by a specialized methodologist. Koen argues that engineering fields will come to decisions about the appropriate heuristics and will characterize a certain set of heuristics as the ‘state-of-the-art’ heuristics. He argues that these heuristics will be changing, and discussion of them will be part of what every engineer does. Thus, while abstract methodology is not much discussed by engineers, practical methodology is constantly discussed. It is integrated into what engineers do, so all engineers are simultaneously engineers and methodologists. Methodology is an important part of engineering, but it is a narrow applied micro methodology of best practices for particular areas, with a very loose general methodology that can probably be described as an educated common sense methodology. Koen calls it a ‘universal method.’

This engineering methodology is quite different from an applied science methodology that applied economists use, and that economic methodologists judge them in reference to. A scientific methodology is focused on understanding for the sake of understanding and the ‘truth’; an applied scientific methodology is focused on applying the truths one has found in science to real-world problem. Applied science methodology uses engineering knowledge as a stepping stone to scientific knowledge, which is close to truth as can be reasonably asked for.

An engineer is not specifically interested in truth; he or she is interested in solving problems; the approach an engineer uses may be based on science, but it need not be. An engineer makes no claim for the heuristic he or she is using being the truth or even being a correct representation of the problem. Truth is not an issue of importance to an engineer except as it relates to the solution of the problem at hand. Engineering is all about heuristics. Heuristic knowledge may include scientific knowledge – scientific knowledge
is knowledge that has been developed using a scientific heuristic and that meets generally accepted scientific criteria – but heuristic knowledge is not limited to applied scientific knowledge. It also includes a variety of sources of understanding that experience has shown to be useful.

The particular branch of engineering, and the particular problem the engineers are trying to solve, will determine how important the scientific heuristic is and how important other heuristics are. There is no one overriding engineering heuristic. The difference between thinking about applied economics as applied science and as engineering can be seen by considering what is at the core of the model used. Applied science sees scientific knowledge at its core. It excludes all non-scientific knowledge. The engineering core includes all applied science but can include non-scientific knowledge in the core as well as established scientific knowledge. Historical knowledge, intuitive knowledge and guestimates are all allowed as foundations for engineering models. This larger core opens up the analysis to a wider range of acceptable models than does applied science.

Let me consider the difference in terms of macroeconomic theory. From an engineering standpoint, to try to understand what policies to use to guide the macro economy, it seems reasonable that interactions of heterogeneous agents are likely to be important; in a model with interacting heterogeneous agents, there will likely be herding, fads and bubbles. We have some intuition and observations of how such interactions might affect the macro economy, and a variety of ad hoc models, but we have no scientific knowledge of how they do. The heterogeneous agent models are too rudimentary at this point to warrant being called scientific models. Thus, the reigning academic methodological approach to macroeconomics holds that any macro model must use a scientifically acceptable Dynamic Stochastic General Equilibrium (DSGE) model (see, for example, Chari and Kehoe [2006, 2008] who argue that any macro model that does not use a DSGE framework should not be seriously considered). It was general acceptance of this argument by the profession that led to the use of the DSGE model as the foundation of modern macroeconomics.

I am not arguing that modern macroeconomists do not recognize the importance of heterogeneous agents; they are developing such models on the ‘to do’ list (Kocherlakota, 2010). But I am arguing that their applied scientific methodology holds that until formal models of interacting heterogeneous agents are developed within an acceptable formal DSGE framework, the informal models of how heterogeneous agents might affect policy results are outside of the science of economics, and this is not what applied macroeconomists should study or use. Thus, in the science of macro, informal models based on loose insights about interactions of heterogeneous agents have no place. They would be ad hoc and not scientific. If modern macroeconomists used an engineering methodology, such informal models not only would be allowed but also could and should be a core focus of macroeconomists’ research. Whether the models were ad hoc or scientific would be irrelevant.

The relation between engineering problems and applied economics problems

If one were to take the standard definition of economics given in most principles textbooks – the study of the allocation of scarce resources among alternative ends – and develop from that a related definition of applied economics, the textbook definition of applied economics one would come up with would be something like the following – the strategy for causing the best change within the available ends, where ‘best change’ means achieving a goal with the least amount or resources. Such a textbook definition of applied
economics is very close to Koen’s definition of engineering, with two fundamental
differences. The first is that Koen’s definition emphasizes ‘poorly understood or uncertain’
situations, and the applied economics definition does not. The second is that Koen
specifically includes the engineer’s time as a scarce resource, whereas in an economist’s
standard definition, limitations in the economist’s time to study the problem are generally
not taken into account. Both distinctions are important and are derived from Koen’s
thinking of engineering not as applied science, but more as ‘applied objective analysis.’

Koen carefully points out that, while there is a connection between engineering and
science, engineering is not applied science; rather, science should be seen as applied
engineering. By that he means that science is the application of the engineering
methodology ‘Use the best available engineering heuristics to solve problems’ to a
particular problem – the problem of understanding for the sake of understanding or for
‘finding the truth.’ For an engineer, abstract truth is not that important, except as it
contributes to an understanding of what works and will continue to work for the real-world
problems he or she is trying to solve. Thus, a central difference between a scientist and an
engineer involves different goals. Engineering primarily concerns how to best solve a
problem or achieve a specified goal. Finding the truth or understanding for the sake of
understanding is a side benefit. The primary goal of science is to find the truth; any applied
usefulness is a side benefit. The methodology of each is designed to achieve the primary
goal. Thus, the methodology of science is concerned with rules that help a scientist find
that truth. The goal of an engineer is to find a solution to a specified problem – to achieve a
goal in the best way possible, and the methodology of engineering is concerned with the
heuristics that help an engineer do so.

This difference in goals accounts for the two differences in the definitions of applied
economics and engineering. The engineering definition explicitly takes into account the
time allotted to finding a solution to a problem, because it is concerned with finding a
solution to that problem. Koen emphasizes that solving problems takes the engineer’s
time, and embodies that in his methodological prescription. Engineering problems have
deadlines – and at some point an engineer must come up with a workable solution to a
problem; thus, one can only understand engineering methodology by understanding how
one manages one’s scarce time, and you can only judge an engineering solution in
reference to the amount of time and other resources the engineer had to arrive at that
solution. The pressures of time means that there is no blanket condemnation of ad hoc
models or of rough and ready guesses within models.

In almost perfectly understood situations, the engineering and scientific methods may
asymptotically approach each other, and the better the situation is understood, the closer
the two methods are likely to be. But Koen emphasizes that engineers deal with ‘poorly
understood and uncertain’ situations, and the less understood, and more uncertain, the
situation, the more likely the methods will differ. That is why engineers do not care a lot
about the fine points of scientific methodology; the marginal contribution to a better
understanding of a fine point of scientific methodology is unlikely to have a high payoff in
the situations that engineers are dealing with.

These different goals lead to fundamentally different ways of going about scientific
research and engineering research. An engineer uses science when he or she deems it
appropriate, but when science does not have an answer to a part of the question that is
needed to come to a policy recommendation, or to achieve a goal, the engineer finds the
best objective answer he or she can, and uses that answer until a better answer is found.
The engineer does not claim that his/her is the right answer, or the truth. The question the
engineer focuses on is: does the answer he or she has found work for this particular
problem. It is to emphasize the known fallibility of engineer’s approach that Koen calls the engineering method a heuristic, and his proposed method a heuristic as well. In doing so, he makes no claims to it being an absolute standard, but simply a working standard that describes what good engineers do.

His emphasis on uncertainty reflects a descriptive, not a prescriptive reality. Engineers are almost inevitably concerned with uncertain situations, and achieving some goal. If the goal and the method of solving the problem were certain, it would be a manufacturing problem, or a mathematical problem, not an engineering problem.

How will the adoption of this engineering methodology change applied economics?

Now that I have distinguished an engineering methodology from a scientific methodology, let me discuss why I believe economists would serve society much better if they followed an engineering methodology rather than a scientific methodology in guiding their applied research.

Once one considers the researcher’s time as a scarce resource, the scientific heuristic, which is designed to uncover truth, often is the inappropriate heuristic. It simply requires too much time and effort to come to a workable solution for uncertain and poorly understood problems. Engineering heuristics do not carry out any part of the analysis to a higher degree of precision than the weakest link of the chain of reasoning. This is often called the significant digit heuristic. Since most problems faced by engineers and by applied economists have highly uncertain elements, scientific precision is usually not appropriate for the relevant heuristic. Instead, engineers use rules of thumb with large fudge factors, which become precise only for those problems where precision is easily achievable and needed. This is not what applied economists do when they follow an applied scientific methodology, as should be clear in the macroeconomic DSGE example. The scientific methodology puts science first. It directs economic thinking toward first solving abstract problems, and then relating that abstract solution to real-world problems; the engineering methodology puts applied work first. It directs economists toward first solving imprecise real-world problems, and only secondarily relating that solution to abstract problems.

The acknowledged switch from an applied science methodology to an engineering methodology may seem minor, but such a switch would lead to major changes to the way in which applied economics is practiced. Specifically, it will lead to five changes in how applied economics is done.

More careful specification of the real-world goal of the research

An engineering methodology would require applied economists to much more carefully specify the real-world policy goal of their applied research. Only if the goal of the research is better scientific understanding would an economist follow a scientific methodology. If the goal of the research is to solve some other problem, the looser engineering methodology would be employed, and the precise nature of the problem to be solved would determine the methodology used.

Incorporate value judgments

An engineering methodology would use a much broader and looser methodology that would blend economic and non-economic considerations. All aspects of the problem necessary to arrive at an actual answer would be included in an applied economist’s research. Thus, for example, if judgments about tradeoffs of individual’s welfare were
necessary, the economic engineer would develop as objective a method of making those judgments as possible. Earlier economists, who took a more engineering approach, were quite willing to develop models involving interpersonal welfare comparisons (Colander, 2007b). For example, Fisher (1927) and Frisch (1932) developed a statistical method of making interpersonal comparisons of wants; they justified their models by pragmatism. Fisher posed the rhetorical question about whether the necessary assumptions can be used, and answered: ‘To all these questions I would answer “yes” — approximately at least. But the only, or only important, reason I can give for this answer is that, in actual practical human life, we do proceed on just such assumptions.’ He continues: ‘Philosophical doubt is right and proper, but the problems of life cannot, and do not, wait’ (Fisher, 1927, pp. 179–180). Maximizing a non-operational social welfare function is not a policy goal of engineering research.

An engineering methodology would lead to a quite different way of doing applied economics, one that embraces, rather than avoids, value judgments because value judgments must be made to arrive at policy recommendations. Science rightly avoids such value judgments, and with applied economics based on the science of economics, applied economics has a tendency to avoid them as well. The scientific methodology guiding applied economics work explains why the economics profession has gone to great length to avoid value judgments. An engineer would not avoid value judgments but instead would attempt to make them as transparent as possible, so that other individuals using the economist’s suggested solution can see whether the value judgments used match their value judgments. The reason is that in order to talk about economic policy, we need to know the goals of individuals and society. What policy is presented as best depends heavily on the value judgments we make. Using an engineering methodology, economists’ recommendations would be presented as being explicitly contingent on the underlying value judgments.

Discussions of value judgments involved in particular solutions would be a central element of discussion of applied economists. This would mean that an applied economist would not provide a definitive answer, but rather a particular answer from his point of view. Koen writes:

A fundamental characteristic of an engineering solution is that it is the best available from the point of view of a specific engineer. Theoretically, then, best for an engineer is the result of manipulating a model of society’s perceived reality, including additional subjective considerations known only to the engineer constructing the model. In essence, the engineer creates what he thinks an informed society should want based on his knowledge of what an uninformed society thinks it wants.

An applied economist using an engineering methodology would do the same. Whereas science avoids all issues involving value judgments, engineering embraces value judgments because they cannot be avoided and still arrive as a solution.

More informal empirical work

An engineering methodology would integrate empirical work into economic analysis differently than is currently done. Modern economists’ strong reliance on scientific standards of significance in its applied work would decrease. Many economists today have a tendency to think of applied economic research as applied econometrics, and they approach most applied problems by bringing their econometric skills to bear on it. They choose problems and research topics accordingly. This follows from their applied science methodology. That strategy would change if they saw themselves as engineers. For an
engineer, arriving at an answer to a question might involve back-of-the-envelope calculations, input from other specialties, guestimates and individual judgment – whatever is needed to provide a policy solution to the problem at hand. In deciding how to allocate scarce research time for thinking about the problem, the state-of-the-art engineering heuristic follows the weakest link principle – it allocates marginal research resources to that part of the problem that seems to be the weakest link, regardless of whether that aspect of the problem is scientific or not.

Using an engineering methodology, applied economics would not be thought of as econometric analysis; econometrics would simply be one of the many empirical tools that comprise applied economics. Case studies, exploratory data analysis, interviews with specialists and any other technique that might shed light on the problem would be part of applied economics. Using Deirdre McCloskey’s terminology, engineering economics would rely on oomph, not ‘it’ statistics, and would vary the degree of statistical precision needed with the time available to arrive at a solution, the degree to which policy goals can be precisely specified, the importance of the empirical measure to solving the problem and the nature of the problem. Each branch of applied policy economics would develop its own empirical heuristic, rather than there being a single empirical heuristic centered on statistical significance measures.

More reliance on intuition
An engineering methodology would involve a greater focus on intuition and back-of-the-envelope calculations, and on a practitioner’s understanding of the problem, not an outside observer’s. Policy work would involve much greater focus on design and actual implementation, not on abstract solutions. The science branch and the engineering branch would be blended, and Bernanke’s (2010) statement that even as the economy crashed the underlying macroeconomics could not be criticized would be seen as inappropriate. If the economy crashed, applied economics cannot plead that the science of economics was not at fault; if the economy crashed, the economics profession failed society.

Less abstract, more practical, goals of research
An engineering methodology would have different output measures than applied economists currently use. The primary output measure would be the implementation of the actual program, not a research paper. One could still have scientific economists exploring models with little relevant to policy, but they would make it very clear that their models had no direct policy relevance. Measuring output in terms of research papers is a scientific convention, not an engineering convention. Output of applied economists would more likely be measured in terms of projects worked on, and contributions to problem, just as engineers do now.

Elsewhere (Colander, 2010), I have described that the change in research economists seeing themselves as engineers rather than scientists would do as a change from hands-off to hands-on research. Hands-off research involves writing abstractly about approaches to problems; it involves doing econometric studies relevant to problems and criticizing other economists’ approaches. Hands-on research would give a much greater focus to actually trying to solve problems as opposed to advice from afar. Economists would spend less time criticizing other economists and more time coming up with alternative solutions. As Koen writes “The argument about which heuristic is preferable is not done primarily by discussing their relative merits, but rather by demonstration.”
The acceptance of an engineering methodology would mean that workable mechanism designs would become much more a focus of what applied economists do. There might be general regulatory designs, market-based designs and tax-based designs, and those would be analyzed in terms of how they work in practice, not how they work in the abstract. The debates within applied economics would switch to practical applied economics – more institutional in nature and less abstract theoretical.

Other changes to the profession
Besides the above changes, there would also be changes in pedagogy and the teaching of economics. Economists’ training would be much more closely tied to what applied economists do, and far less concerned with both models and methods. Economic education would spend much more time preparing students to solve real-world problems and less time on how to write academic papers, or how to develop and solve abstract models (see Colander [2010] for a further discussion).

J.M. Keynes once said that he hoped one day economists would be thought of in the same way as dentists. You have a problem with your teeth – you go to the dentist to solve it; you have a problem with the economy, you go to an economist to solve it. The suggestions in this paper are very much in line with that view. Using an engineering methodology, economists’ suggestions about policy will be presented much more humbly than they currently are. The subjective and *ad hoc* nature of the engineering method would be recognized and accepted, and any model used would not be portrayed as representing the correct model, but simply as a useful model for one set of particular problems.

There will, of course, still be economists who are primarily economic scientists, not economic engineers. For them, economists’ current methodological approach would likely remain, since their primary goal will be to find the truth, not to solve a particular problem. They would not take a primary interest in the implications of their research for policy (Gerard Debreu is a good example of such a theoretical economic scientist). But such scientific economists would likely be only a small part of the profession. In a profession guided by an engineering methodology, most economists would be applied economists, and as such methodological generalists, not methodological specialists. Whereas currently economists require formal models because they see themselves as scientists who are adhering to a scientific method, engineering economists would be happy with *ad hoc* informal models that seemed to work. An economic engineer would see himself as more of a generalist and would not be constrained by a scientific method.

The economic engineer would use many different methods that would likely vary with the particular problem he or she is working on, the time he or she has to find a solution and the nature of the problem. At times their focus might be philosophical questions having to do with goals; at other times their focus may be on judgment questions, having to do with how best to incorporate value judgments into their policy solution. At other times it might focus on institutional questions having to do with implementation, or historical questions having to do with how similar problems occurred in the past.

A throwback to classical methodology
Were applied economics to adopt this engineering methodology, it would not represent an entirely new approach. In many ways, it would be a movement back from the scientific methodology adopted in the 1950s that centered around the Walrasian general equilibrium model and welfare economics, moving directly from models to policy, to a classical methodological approach, where applied policy economics was strictly separated from the
science of economics. Within classical methodology, applied economics had a different methodology than did positive economics. Keynes, in his summary of the classical methodological approach, wrote

[F]ew practical problems admit of complete solution on economic grounds alone ... [W]hen we pass, for instance, to problems of taxation, or to problems that concern the relations of the State with trade and industry, or to the general discussion of communistic and socialistic schemes – it is far from being the case that economic considerations hold the field exclusively. Account must also be taken of ethical, social, and political considerations that lie outside the sphere of political economy regarded as a science [p. 34] ... We are, accordingly, led to the conclusion ... that a definitive art of political economy, which attempts to lay down absolute rules for the regulation of human conduct, will have vaguely defined limits, and be largely non-economic in character. (1891, p. 83)

The role of the methodologist and methodology in the economic engineering

The above discussion has focused on how the role of an applied economist would change if the profession adopted an engineering methodology. Let me now turn to how I see the role of an economic methodologist changing. First, it changes who is an economic methodologist. In the engineering model, methodology is embedded in the heuristics of applied researchers, so every applied economist would be concerned with what might be called micro methodology – the consideration of methods that worked for solving the type of problems they are trying to solve. So applied economists will discuss methodology much more – this is a heuristic that works for this particular type of problem, this one does not – than they currently do.

As methodology becomes embedded in applied economics, the role of the methodological specialist will change. They too will likely be applied economists as well as economic methodologists, and their research will focus more on discussing whether specific heuristics used in this particular sub-area might usefully transfer to the heuristics used in other areas. Their research would be far less likely to focus on abstract issues. Methodologists would simply be a group of economic engineers whose goal is to assist other engineers in finding the best heuristic. They would work with practicing economists, not write about practicing economists.

How to change the profession

A key difference between an engineering methodology and a scientific methodology is that a researcher using an engineering methodology is working on solving a particular problem. That holds for economic methodologists as well, and the problem I am attempting to solve is how to get economists to see themselves as using an engineering methodology rather than a scientific methodology. That will not be done by writing papers such as this one for other economic methodologists. In my view, economists fully recognize the problem and often joke about it. Walter Heller’s definition of an economist as ‘a man who, when he finds something works in practice, wonders if it works in theory’ captures that understanding. But under current institutions, they have no incentives to change.

Contrary to what some heterodox economists claim, the economics profession is not in crisis – far from it, it is highly successful in recruiting students and in being respected by the public. The experience with the financial crisis hardly dented it at all. My message for economic methodologists who consider themselves applied engineering methodologists trying to make the profession better, as opposed to philosophical methodologists who are
trying to understand what economists are doing, is that the only way the system will change is if the incentives change. The ones with the power to change incentives, and the ones whom I believe want change in the economics profession, are policy makers and the funders of economic research. Therefore, the engineering methodologist’s intended audience should not be other economic methodologists, but rather policy makers and funders of economic research.

In Colander (2009), I have outlined the design of a system that would significantly change in what applied academic economists do and lead them to use an engineering rather than a scientific methodology. The proposal for change is directed not at academics but at funders of academia – various levels of government, foundations and individuals. The change essentially involves making full outside funding of research a requirement for a much larger percentage of the academic profession than it currently is and separating out the implicit funding for research built into current academic institutions. As opposed to applied academic researchers being given a goal of solving some abstract problem – providing knowledge and letting the applied academic researcher determine what issue he or she will study – as the current system does, academics need to be given the goal of assisting society in solving particular problems.

This change can be accomplished in a number of ways. One method I developed is for governments and foundations to fund some part of university research indirectly by providing vouchers to groups who want applied research done, rather than directly. I proposed that funders divide funding into ‘hands-on’ (applied) research funding and ‘hands-off’ (scientific) research funding, as they choose (Colander, 2009). The ‘hands-off’ research vouchers would go to scientific funding agencies such as National Science Foundation (NSF). The ‘hand-on’ research vouchers would go to non-profits and other organizations that needed problems solved.

Say that 5% of a state’s funding of a university was given in the form of ‘hands-on’ research vouchers to the non-profits or government agencies who could use assistance of academics, and that 5% of the professor’s pay was paid only when he or she acquired a voucher. (The percentage of an academic’s pay could be expanded over time to include the full amount of the time the academic is expected to spend on hands-on research.) The non-profits could then use those vouchers to hire academics when they help them solve problems. The academic would then turn over the voucher to the university who would get the funding if the university agreed that it would use that voucher as a measure of the researcher’s hands-on research.

Depending on to which groups the funding agency gives the research vouchers, the change in funding would involve a major change in research incentives. If the funding agencies give the vouchers to scientific foundations, such as the NSF, then it would involve little change. If, however, they gave it to non-profit agencies, such as a development agency, or non-profit agencies, it would involve significant change. Rather than trying to solve abstract problems, the academic doing research for a development agency would involve in solving real-world problems. The higher the percentage of money going to hands-on vouchers, the greater the amount of hands-on research. (This plan is spelled out in more detail in Colander [2009]). This change would replace academic publication as a measure of research with research vouchers, putting more emphasis on service and solving problems. I proposed the creation of an online consulting market in these vouchers, with academics checking to see what jobs needed doing and how many vouchers they would receive for it. They could use students to help them in their research, possible paying them some percentage of these vouchers, so that these vouchers could help reduce student tuition.
There are other ways to achieve similar ends and well, and in other work (Colander, 2010) I have explored some of these. They involve changes in research funding mechanism that would give funders much more control over the nature and type of research that academic economists do, and allow for more funding of hands-on research.

**Conclusion**

Seeing oneself as an engineer and not as a scientist would have a useful humbling effect on economists, but it would also free them up to apply economics in ways that currently they cannot. Recognizing that you are working on heuristics, and not offering the truth, frees one to be more imaginative and expansive in one’s thinking. It would reduce the tendency for economists to herd since there is far less tendency to search for a single truth. For example, because of my particular historical circumstances, I have been lucky to have had the luxury to let an engineering methodology guide my work, and few would regard me as herdable.

My work is seen as idiosyncratic by most economic methodologists – as a strange admixture of wild ideas, conjecture and reporter level research. It is engineering research much more than scientific or philosophical research. Such research is not seen as appropriately scientific for a methodologist, or for an economist. It is the type of research that a funding group would be more interested in than an academic group. This is to be expected. The natural consumer of an economic engineering methodologist’s research would not be other methodologists; rather, it would be groups that hire economists, groups that fund economists and the groups that give research grants to economists to achieve some end. But methodologists do not write for such groups. We should. These funding groups are the ones who can best decide whether economists are fulfilling their goals or not. Economists cannot objectively decide. Thus, it would make sense for each of these groups to have their own economic methodological consultants who could advise them on how to structure incentives for economists to bring about their desired ends. The engineering methodologist’s goal would be to design incentive systems facing economists so that their research reflects the desires of the funding agency.

I have been lucky because with few people following my approach, my work can stand out. If more economists were doing what I am doing, I have no doubt that they would do it much better than I do it. But they do not do it because they have few incentives to do it. Were a young economic methodologist to do it, he or she would likely not remain an academic economic methodologist. That is why I argue that methodologists failed society, and why that failure is a systemic failure, not an individual failure.

In summary, economists are not playing as positive a role in society as they should. Economic methodologists should see as one of their important roles as improving the efficiency of economists – designing changes that will lead economists to play a more positive role. But they are subject to the same forces as are economists, and the incentives that lead economists away from most efficiently solving problems also lead methodologists to fail to point out the problems.

**Notes**

1. The systemic failures result from a deeper systemic failure in academia, which has separated it from the society that supports it, but I will not address that broader systemic failure here other than to note that the failures discussed here are paralleled in other areas of academia.
2. The argument carries over to much of what ‘scientists’ of all sorts do; much of what scientists do is engineering. The distinction is important not because it matters whether one is called an engineer or a scientist. It matters only because different methodologies are appropriate for each.
If applied economists accepted that they should use engineering methodology, I would have no problems with calling them scientists.

3. One can see this in the discussions of top economists. For example, Eric Maskin states, ‘I don’t accept the criticism that economic theory failed to provide a framework for understanding this crisis . . . ’ (as cited in Taylor, 2010). Similarly, Ben Bernanke argues that the mistakes that were made were primarily engineering or management mistakes, not mistakes in the fundamental science of macroeconomics, which he argues is sound. He writes, ‘The recent financial crisis was more a failure of economic engineering and economic management than of what I have called economic science’ (Bernanke, 2010).

4. Whereas an engineer is not a scientist, a scientist can be seen as an engineer, and scientific methodology can be thought of as a sub-area of engineering methodology, in which the goal is to find the truth.

5. In my interviews with graduate students (Colander, 2007a), students pointed out that data availability determined the research they focused on, not the importance of problems. One stated that the difference between an economist and a sociologist was that an economist studied unimportant problems about which he could say something precisely, whereas a sociologist studied important problems about which he could not say anything precisely. Were an engineering methodology to be followed, an economist would study important problems as precisely as he could be but not more so.

References


