An Interview with Kevin Hoover
Young Back Choi
St. John’s University

Kevin D. Hoover is known for his acclaimed book, *The New Classical Macroeconomics: A Sceptical Inquiry* (1990). While he specializes in Macroeconomics, he has also written on economic methodology. He received his B.A. in Philosophy at the College of William and Mary in 1977, a second B.A. (1979) and D. Phil. (1985) in economics from Oxford University. He has been a visiting economist at the Federal Reserve Bank of San Francisco. He is currently associate professor of economics at University of California, Davis and serves on the board of editors of *American Economic Review*. We talked in Washington, DC, in December 1990.

I have read some of your papers and you seem to have interesting views about the progress of economics as a science. Would you explain how your view is different from, say the typical.

It seems to me that there is a general problem in methodology to which the Lakatosian view is particularly subject. Methodologist and economists—presumably this is true in sciences as well—are to a large degree out of touch with practices that inform people who are at the sharp end of the discipline, namely, econometricians or macroeconomists who are simply publishing work on macroeconomics and not on methodology. When you think about the discipline methodologically, there is a tendency to try and force the practices, as observed from the outside, into tight strait-jackets. The Lakatosian view suggests that there are these competing camps which have some difficulty in understanding and communicating with each other, and that there are some over arching principles guiding the progress and development of these camps. So you have the Lakatosian hard core and positive heuristics and things like that. It seems to me that none of that is actually very persuasive.

It is quite true that there are competitive camps, there are schools of thought, and there are people who have fundamentally different outlooks. But they can understand each other, they talk to each other. The things that motivate them are in general less programmatic concerns than just the particular works of their colleagues and opponents. They look at a particular paper, or model; they either like it or dislike it. They see ways of extending, or communicating it. Most of what they do is fairly tightly geared to concrete products of the discipline.

That seems to me to be in some sense more of a Kuhnian view. Again there is a lot of lip service to scientific revolutions, but the particular Kuhnian idea that I want to pick up on is the one which very few people in economics picked up on: a paradigm is a concrete exemplar of some theory or practice or scientific achievement. It is the thing to which other practitioners will look. Well, if that’s the view you are taking, that’s very sympathetic to the point that I’m making about the practice of a macro-economist and an econometrician. It is to get down and see what a particular colleague or opponent has done. They are working from their paradigms, but none of those paradigms are particularly privileged. Somebody comes up with one model and another person uses that as a basis for his model. The first model may continue to be the basis for many other models, or it may be largely forgotten because the other model is considered to be superior in some other way. And I think that if we really want to understand what people are doing we have to understand the way in which particular achievements radiate and how they connect. That doesn’t rule out the fact that we have some idea of scientific programs or scientific schools. What it rules out is the kind of straight jacketing that some of these methodological discussions try to impose on the disciplines.

In your book, *New Classical Macroeconomics*, do you take *New Classical Macroeconomics* as a concrete exemplar or collection of exemplars?

Yes, exactly. It is a collection of exemplars. So that the New Classical economics to me does not represent a program which can plausibly be described in a Lakatosian mode. Or that’s not exactly true: it can be plausibly described in a Lakatosian mode. It
is just that there is about 1800 different descriptions which are equally plausible.

In dealing with concrete exemplars, could you assess the New Classical exemplars, in terms of how they function, and whether they are desirable or whether there can be alternative exemplars, and if you do, how they may look like, etc.?

Well, I think that the way it functions is relatively straightforward. Take Lucas & Rapping in 1969, who are worried about the microfoundations of macroeconomics. So they write a paper, in which everything is done consistently and derived from optimization. They don't have rational expectations; the idea has not really occurred to them, yet. Later other people pick out what they're doing, saying, "This is the way, Lucas & Rapping did it!"; this is a good way to approach the problem. Lucas & Rapping themselves later say, well, you know we really didn't like the way we did the expectations", and they start fishing around for how to do this better, and they come across Muth's paper. Muth's paper is quite an essential concrete exemplar. Muth shows everybody how to do it. Nobody paid any attention to Muth before Lucas & Rapping. But once they start paying attention, then, all of a sudden this paper has resonance, and everybody cites it and sees the way it could be done.

Now we have rational expectations, we have a general equilibrium framework and the rest of new classical economics just grows based on these papers as examples. And this is exactly the way it is taught to graduate students; they are taught to copy the work of others until they are able to make some progress on work of their own. What they are taught to copy are not grand principles but the particular papers or particular work of their teachers or of the people their teachers point them to. In this case, it is a good thing.

Well I'm not deeply sympathetic with new classical macroeconomics, though I think this is a point more of substance than methodology. New Classical Macroeconomics misses really important things about the world when it tries to force the world into the mold of market clearing: All markets are clearing essentially all the time up to the limits of information and other kinds of friction. So if you want to find out why something looks like it is no: market clearing, you want to find out why it's actually an equilibrium outcome if you really understood what the problem was. It just seems to me that there are too many phenomena in the world which are more conveniently, more plausibly, understood as non-market clearing than as market clearing, e.g., unemployment. Lucas will attack the notion of involuntary unemployment by saying that people are making optimizing choices. They may be faced with constraints that make those choices extremely unpleasant, but in fact they are doing the best they can for themselves under the circumstances. Now it seems to me that that's actually implausible because we know in certain situations we are in the depth of a recession. A factory has laid people off. At some point maybe they want to hire a few of them back. They put an announcement that says that are going to hire people. They get thousands of people applying for very few jobs. These are people who are willing to work at the wages which are being offered. Some of them maybe unqualified but probably most of them are perfectly qualified to do the job and so the ones who are turned away fit precisely into Keynes's definition of what involuntary unemployment is. And the puzzle is really, why do you have those kinds of situations and at the same time not have a tendency of wages to fall in such a way that will clear markets? While you can always try to fabricate explanations for why this is some equilibrium outcome, it seems to me that you are doing triple back flips to do that. It's much more straightforward to see that in fact the Keynesian notion of involuntary unemployment makes sense and that there really are disequilibria and that sorting them out might take some time. That's something that new classical macroeconomics rules out. But I don't think that any of that is deeply methodological. This is the substance of macroeconomics that we are arguing about not how to do it.

But if so many insist that this is the theory and are not willing to listen to any kind of counter argument, how do you deal with the theory? I guess this is relevant to methodology.

Well I think there are two methodological issues here. One is, we have to be really careful of our language and careful about sorting out the logical criteria for different ideas, I think that Lucas, for instance, has misinterpreted Keynes's definition of involuntary unemployment. It is certainly worthwhile to try and get the semantics straight because that's going to govern a lot of what we find persuasive, how we do our empirical research and things like that. I think it is worth correcting those kinds of misinterpretations, you can consider that a methodological issue.

Another methodological issue that comes up here is, what is it about empirical evidence that can persuade you to abandon a theoretical commitment? And that's a question in which I'm very interested. I am deeply committed to an empiricist point of view in the sense that we have to have inference from empirical evidence back to theory. There have to be times when we modify our theories because of the evidence. The nature of that process is not altogether
clear, neither in sciences, nor in economics. It is something that methodologists can profitably spend a lot more time on. But it is certainly an area on which I think the controversies are very great. For example, there is an in-house fight within new classical school. We have new classical economists like Sargent and his students, colleagues and crony's who think that the right way to go about testing your models or dealing with and quantifying them is the following. You take the theory, derive a model from it, go out to the data, use some appropriate econometric technique, estimate the model and then use appropriate statistical test to tell whether that model is justified on the data. It’s more or less the standard view most of us have been brought up with. On the other side you have someone like Prescott who says estimation of macroeconomic models is absolutely the wrong thing to do. One should not estimate them, because when one does, they are almost always rejected. What one should be trying to do is essentially taking correct theory and filling in from a wide variety of sources parameter values that the theory doesn’t itself specify; and then, conditional on your theory being correct, doing whatever policy experiments that you want to do with it. But there is nothing in Prescott that suggests that there is any route by which the evidence would make you reconsider your deeper theoretical commitments. So in that sense it is almost like the Lakatosian hard core.

*Like a revealed religion?*

Yes, these are commitments that are so deep that he has no intention of ever giving them up. The reason why I don’t subscribe to the notion of Lakatosian hard core in general is that I see very little evidence that the profession as a whole, or even in any particular school of thought, has commitments so deep that one way or another they can’t be persuaded out of them. What interests me in this case is that as an empiricist I find something very disturbing about this inability to reconsider commitments in the face of evidence.

*Could you say as an empiricist that if you cannot change Prescott’s mind, it is just that the evidence is not strong enough for him?*

No, I don’t think that’s what it is. It is a question of kind; they don’t believe that evidence could exist against certain of their beliefs. Prescott has a paper, “Theory Ahead of Measurement.” The point of that paper is that a lot of the evidence that is being brought up against his approach wouldn’t be evidence against his approach at all if the data were in fact constructed consistently with his theory. He thinks for theoretical reasons they should be. That’s not a totally obviously reprehensible view. You can’t construct any data without having some kind of theoretical commitments. Let’s say you want to set up national income accounting data. Unless you have some notion of how the economy is constructed and how income flows work, you would have never chosen the categories that we all now know as National Income Accounts. It’s not obvious that that’s the only way we can categorize the data; and, whatever categorizations we choose, we would like them to map on to some sort of theoretical understanding. But I still find it troublesome if the theoretical understanding is so all embracing that any time the evidence didn’t suit the theory, you would just say, well, we hadn’t constructed it correctly. Then, there is no opportunity for repudiation.

*So what do you perceive is the likely course in economics if one group of people that are so deeply committed to their views and other groups are not, and how do you suppose these different groups should communicate, if at all?*

Well I don’t think that there really is a deep problem of communication, I think I know perfectly well what the new classicals are doing. I’ve put myself in their shoes enough times that I could see what they’re doing. I think that I could play their game. I would have to be a little bit more technically geared up than I am for some things, but I know what techniques I would have to learn. I don’t think I really have any problem understanding them, and they don’t have any problem understanding their opponents. The impossibility of communication to me seems to be a totally sham issue. How will we ever develop from one way of thinking to another way of thinking? Well, largely that happens because people get old, tired and die. There are people who are flexible enough to shift between different ways of thinking. As a historian of economic thought, I’m sure you could think of examples. But most of us are fairly bloody minded and believe what we believe, and it’s real hard to sway us. But there are new people coming into the profession all the time, and those people are going to choose the path that seem to be the most profitable to them. They’re going to take the things which seem intellectually interesting, not totally worked out. I think what happens with a lot of these things is that they do get worked out, until, there just seems to be no further developments that you can make with this sort of model. You’ll go onto other things, whether these different things will have a totally different point of view will just depend on a lot of sociological factors. It will depend on the way people who are not committed perceive from the outside the intellectual success of the program. There are... lot of people who are fence sitters, neither new classical nor new Keynesian. There are people who try to see all the sides. They’re probably not very important people in the profession as any one
individual, because you have to have certain commitments to be a leader. But they’re the mass of humanity in the profession, and the way that they tend to shift is in some sense going to shift the whole profession. People will develop different allegiances in that way.

Do you think that new classical economists have exhausted their models?

No, I don’t think that’s true. It’s easy to see how it continues to have its own dynamic. There are certain parts of it that are totally exhausted. When the rational expectations hypothesis was first imported into macroeconomics, the big interest was in policy ineffectiveness, and in unanticipated money being the cause of business cycles. That literature is essentially dead. It’s been played out as much as it can be. It’s partly dead for empirical reasons because, though there were no decisive experiments, the weight of evidence ultimately shifted against this view. Out of the ashes of that we have real business cycles models which are such close cousins to the monetary models that I would think anybody would be quite mistaken to think they belonged to different schools of thought. The recent developments in new growth theory, e.g., growth with increasing returns, has largely taken place within the context of new classical ways of thinking. So I’d say there’s still quite a bit of life left in the project, it doesn’t make me necessarily sympathetic to a lot of it. But I don’t think it’s dead by any means, although some parts of it are.

You mentioned real business cycles theory. I am somewhat puzzled by the assumption of representative man as the economic agent. Do you have any problem in building a macroeconomic model based on the assumption of representative man?

I have a tremendous problem with it. That’s sort of my bugbear. All models are abstractions and to simply point out that a model is an abstraction and criticize it on that basis is silly. Again this is a question not so much about the methodology but about the substance of economics. We know that the economy has millions of agents and we also know that theoretically the conditions under which one can aggregate those agents precisely are very, very strenuous, and almost certainly not fulfilled. And so the representative agent assumption I find troubling at the point at which one maps from the model onto the real quantities of the economy. It’s OK to use that assumption to illustrate a point. If you are simply trying to show how in principle something works, for a lot of purposes, that is probably perfectly all right. But Sargent, for example, sets up a representative agent model and derives certain cross equation from that model, and then takes aggregate data and estimates the model, and then tests whether those cross equation restrictions hold. It doesn’t seem to me that that proves anything. If they hold, it’s almost pure chance because we know that the conditions for which that representative agent model could represent aggregate data are not fulfilled. You might say, well, they’re fulfilled close enough. I’m not sure how you would measure that. I think more work needs to be done to demonstrate that this is not illegitimate. It comes up on the other side too. The calibrators, the real business cycle types, don’t estimate their models. Nevertheless, they have representative agent models. One rationale that they give for calibration is that they want to get down to the real deep parameters. Since you are working with aggregate data, estimating the model is probably going to give you all the wrong parameters. But what you can do is to go and look at microstudies, for example, and find the elasticities of substitution. Then bring those up from the microstudies, stick them into your representative agent model, and that way you are going to get a better model. It’s like trying to derive the gas laws in physics from the behavior of a single molecule, by scaling up that molecule to room size. But that’s not what it is about; the gas laws are about the behavior of millions of molecules, not the behavior of one molecule. A representative agent model is not in any way an obviously sensible model to quantify, yet that’s what they’re all doing. They’re not alone either. I mean new-Keynesian economists do the same thing to a large degree, so it is again one of these things that crosses the borders of different schools. It’s done for tractability and to suit the rhetoric of microeconomics.

Everyone feels that we have to have micro foundations. So you take a representative agent, give him a utility function and that looks like a microeconomic problem. But it isn’t really because whose utility function is that? It’s not any individuals. It is not society’s. It’s nobody’s and there’s no reason to think that the rationality that applies to actual agents, people among millions of people, is the same that applies to this representative agent that treats the GNP of the economy as his own income.

So, do you see any reason to sustain the effort to provide microeconomic foundations?

Well, I think that the effort is somewhat misguided. We need to pay attention to individuals. Clearly the economy is made up of individuals. How individuals act is important, we get insights into what happens at the macroeconomic level by knowing how people will behave as individuals. When we teach macroeconomics, we think about how individuals are likely to behave. The question really has to do with how tight the mapping is from that type of understanding of individual agents to the behavior
of economic aggregates. When we know conditions of aggregation are not fulfilled, how should we deal with the fact that we have a mass of individual agents? And some work is being done along these lines, e.g., work on coordination failures, partial rational expectations models in which different classes of agents react in different ways and the interactions among different classes of agents are important. This is a step in the right direction, but still suffers from the same problem to a large degree.

Another bit of work that I’ve seen recently are people who are simply concerned about what kind of stochastic characterization you could give to all the agents and the economy so that you can treat thousands of agents. You obviously can’t treat them as individuals, but we can bring them under some kind of stochastic characterization. That seems to me to be a very fruitful idea although it is very undeveloped at this point. In a sense that’s reclaiming macroeconomics for macroeconomics. What’s really important about macroeconomics is that individuals are operating socially or in big groups and that we have to do something principally about these interactions. That undermines what I think is the substantive case for micro-foundations, which really wants to go down and model each individual agent—which is something we’ll never do.

So you think that macroeconomics is a somewhat separate province?

Yes, I think it has a kind of autonomy and we might actually learn things about micro from macro, which very few people would believe today. My colleagues in micro say modern macro is micro. Lucas says that his goal is to obliterate the distinction between micro and macro, so that it is just “economics.” I don’t think that is desirable or likely to succeed because we have two different kinds of problems. We have a problem of what do two individuals do when, more or less, you can take the behavior of the other individual as given because they are essentially autonomous. And another problem is how masses of people behave when they are interacting. I think we can fruitfully isolate those things. In fact the only useful macroeconomics has to isolate that. You think of useful macroeconomics as what can help guide a policy maker, Congress, the Fed, or the President, or somebody like that. It’s not at all clear to me that dealing with the individual agents that are at the center is going to be very helpful.

Let’s suppose that macroeconomics is a separate province and that economists are useful in policy making as you say. Then economists’ structural model should be able to tell what action will cause what effects. In estimating the structure, how can you tell whether this is a lasting structure or whether there will be a shift in the structure before the action?

It’s not obvious to me that one should expect invariance in estimated econometric relations. The world is a complicated place. What makes the economy different from the physical world is that we have intentional agents, people make decisions, and they keep changing the rules. I think rational expectations and new classical school are absolutely right about this, that if you change the constraints that people face, then people will adjust to those constraints. Relationships may breakdown. Nevertheless, we are more alike than different. I suspect our behavior is more habitual than not. There probably are, at least in broad brush, some stable relationships. The consumption function probably is a more or less stable relationship. People have to eat. They basically behave more or less in the same way from day to day, year to year. You may like a different kind of food than I, but, in general, given our body weights we’re going to eat proportionally. I think they’re probably more or less constant relationships. Do they have the kind of degree, precision and constancy of physical laws? Probably not. Should we expect to see the econometrics breakdown from time to time? Depending on what they are, probably yes. Can we learn anything from that? Yes, I think so, because one thing we want to know is which way controllability runs. Can the Fed influence the economy? Can Congress influence the economy in various ways? Having a changing regimes is one way in which one can elicit causal information about which things influence other things, in which directions influence runs. A lot of my interest rests on causal modeling. That doesn’t make me too sympathetic to big giant macro models, e.g., the DRI model or Wharton Model. I don’t think they can be expected to fit well. But I think we can learn at a cruder and more piecemeal level quite a bit about the economy econometrically. You can know approximately what’s going to happen conditional on various things. You can probably make reasonable forecasts for lots of things. The question is what’s reasonable. Can you make a lot of money off of it? Probably not. So I guess I’m optimistic about econometrics and empirical economics but I don’t really expect to see stability. I think that as institutions change our relationships will change, and we can learn something from those changes but we wouldn’t expect to see stability, all the time.

So your are optimistic about empirical economics?

I’ve seen people who seem to do sensible econometrics, who come up with relationships that seem to have some kind of endurance. That are
theoretically interpretable, make sense to us, and give us some purchase on understanding what’s going on in the world with quantitative measures. Econometrically, I’m a Hendrophile. The kind of work that many colleagues have done on consumption or money demand is very useful, and suggests that, for long periods of time, there are relatively stable relationships in the economy which help us to quantify what’s going on in the economy. On the other hand, the most useful economics doesn’t come from making detailed forecasts from macroeconomic models. The most useful economics comes from understanding a few, fairly basic principles, doing back-of-the-envelope calculations, and playing things by ear. That’s what the world of the policy makers is like. The reason is not because they’re ignorant of highbrow methods. But, in fact, it is because that is the level where economics can say something that is worth knowing.

You just mentioned that certain econometric models make theoretical sense. Now what is the role of theory in making sense, or not making sense of empirical models?

You couldn’t look at any model without some theoretical understanding.

Aside from the question of working and not working?

Yes, you see some odd facts in the world. You do some statistical calculation, and a correlation comes out in a way that you didn’t expect to see, and you’ll only be happy if you can give an explanation for that. The role of theory is to provide explanations which are satisfactory in the sense that they link the empirical result to some principle that we understand. I have no problem with theory in that sense. I think theoreticians are doing a very important job by giving us important tools for understanding what we see, and I hope that empirical economists are doing an important job by using those theoretical tools to dig up more facts that make theoreticians alter and adjust and remodel things to give us a better explanation of what’s going on. There just seems to be this interplay between theoretical understanding, which is the linkage to basic principles, and facts in the world which you get by starting with one theoretical understanding but being open to reconsideration.

Let me shift the ground and ask you this question. Some people like Arjo Klamer seem to argue that what is written in articles or books is not sufficient to give sense of real insights or attitude of economists who write books and articles. They advocate interviews and talking in person to find out more in depth. What do you think about this?

Well, Arjo Klamer’s book of interviews with various economists about new classical macro. is an extremely enlightening book and an extremely enlightening project, because people tend to write down very refined versions of their own thinking and leave the tentativeness and the history and the story of the genesis of their ideas out. It is very often interesting to know those things, and may actually be useful to know those things, to help find different paths and different ways forward. I don’t have any problem with that. I think the more we know about these things the better. On the other hand, the reason people don’t write down some of the things they say in interviews is because they really don’t want to be committed to them. These are not the finished products of their thought, and they really only want to be committed, quite properly, to the things that they have worked out in some detail. After doing this interview, I might regret some of the things that I’ve said. People should take that into account. This is a framework in which you are thinking on your feet, and you haven’t thought about a particular issue very carefully, or very deeply, or for a long time. Does it get to the real truth in economics, more than reading the articles? Well I’m not sure about that. But it’s certainly of great interest. I have other problems with the emphasis on rhetoric, but not with interviews.

What kind of reservations do you have on rhetoric, or the emphasis on rhetoric?

Well, I have a suspicion but its not completely well informed. I mean I’ve read McCloskey, but not for awhile. I’ve read various other people but not necessarily carefully. But I have a suspicion that the project on rhetoric is a project to undermine careful attention to the standards of what should be persuasive. When they talk about rhetoric, they talk about what people in fact find persuasive. And that’s all well and good: it is a sociological fact about the profession. We know that when we write articles we intentionally do that, we try to bring people around to it in all different kinds of ways. But there are genuine methodological problems faced by practitioners of economics — rather than methodologists, who are outsiders — the genuine problems of deciding what should count. The rhetoric project, it seems to me, doesn’t really address that issue at all. It is not at all helpful, in the issues about calibration versus estimation — the case in which I am currently concerned. I think there is a real serious issue here, and that issue is not solved by saying that many people are persuaded by the kinds of simulations that Prescott does. The issue is: should they be persuaded? Is there any kind of reasonable case that could be made for that kind of procedure? I think, in fact, that there might be a reasonable case for it. But we want to stop shouting at each other. If we want to actually reason together and solve disputes
reasonably, we have to be able to come up with principles of resolution. I think that one thing that is giving methodology a bad name is that people want to stand up and create very grand and overarching principles. Those are usually either too big to be useful or too strait-jacketing to be believed. And so this sort of big methodology doesn’t seem to be a very useful enterprise. But when you get to the sharp end of particular econometric problems or particular modeling problems, you get into these kinds of disputes, like the calibration/estimation debate. But once we come up with some kind of reasonable resolution for it, there’s probably a generalizable rule. I’ve only met McCloskey once, and I asked him about this point. I said, “grand methodology is nothing to me, but it seems to me that we do have to be willing to generalize from the lessons that we learn in particular cases.” The answer that I got from him at the time, was no he didn’t think so, everything was a special case. So if everything is a special case, then I don’t think we are going to make any progress at all. That’s I guess the heart of my exception to the rhetoric approach.

As an empirical researcher do you find that certain kinds of arguments should be persuasive, and others unpersuasive?

I’ll tell you the simplest example. People make elementary, logical mistakes. It happens all the time in economics articles. As a referee you find them, but also you find them after they are in print. There are people who don’t understand the difference between necessary and sufficient conditions. An argument which abuses those elementary and logical distinctions is not an argument which should be found persuasive. Although sometimes they are sugared up in such a way that they really appeal to people’s instincts on various grounds. That’s the most basic example on which I suspect nobody disagrees with me.

But when it comes down to an econometric argument, then there are issues still outstanding about how one should do econometrics, how one should interpret statistical tests, and whether data mining in one form or another is a legitimate or illegitimate procedure, etc. And the way you come down on those issues is going to determine both what you do in your own practice and also how you judge the practice of others.

So is it largely the question of ethics or some established rules?

I think its largely a question of logic taken in a very broad sense here. Logic being canons for sound reasoning. My favorite philosopher, Charles Peirce, said that logic was a branch of ethics. So, I guess it is a branch of ethics. But in many cases where we have methodological disagreements, we would not be willing to inpute bad motives to the people we disagree with, so in that sense I’m not in some violation of morality or something like that. It is just ethics in the sense of ethics as the discipline of telling you what you should do, rather than what is; in that sense, in this broad sense, logic is an ethical concern. That’s what I think the dispute comes down to.

I myself have a little trouble with this rhetoric approach. Once I used McCloskey’s intermediate micro textbook, and there he says it doesn’t matter whether US runs a trade deficit. As long as foreigners are willing to take IOU’s, so much better for the Americans. He concludes that journalists’ and politicians’ concern about the trade deficit is based on money illusion. I wrote to him suggesting that IOU’s represent purchasing power, it’s not as if Americans have comparative advantage in printing IOU’s and Japanese, in producing cars and they derive utility from IOU’s. Japanese can buy oil, farms, factories, etc. McCloskey replied saying that he and I had two different metaphors. In his metaphor, Americans act as world bank and as long as Checks, IOU’s are accepted, it’s good. In my metaphor Americans are seen as a family, using up heirloom and inheritance. He asserted his metaphors are more appropriate. I was quite amazed by his argument.

It disturbs me a little bit when arguments are being carried on at that level because some people think that it is a tremendous revelation to be told that large parts of thinking are metaphorical and large parts of metaphorical thinking are not equivalent to deductive logic. That doesn’t seem to me to be a great revelation at all. The issues come down to this: he has a metaphor and you have a metaphor of how the economy works; and you do have to decide what is appropriate for whatever it is that you are concerned with. Now his ends may be different from your ends, in which case there may be no dispute. There may be nothing to dispute. On the other hand, there may be something precisely to dispute because you both have the same goal and the question of what will actually achieve that goal is an issue. Establishing that each of us has a metaphor, and saying that this is the one I prefer, doesn’t help. You have to be able to persuade people that there are good reasons why they should prefer your metaphor. In many cases, reasons like the aesthetic beauty have nothing to do with the ends to which we may well be agreed we are aiming. So for example, if you are aiming for price stability, there may be many metaphors of the way that the monetary economy works. Some of them may point you in contradictory directions. But when it comes down to it, the guy at the trading desk of the New York Fed who makes an open market purchase or
sale is going to take a concrete action, and that concrete action may or may not have an effect on the price level, and we precisely want to know whether it will. Dressing up that whole discussion as the infinite ramifications or variations of different metaphors is not very useful. In the end we are going to have to take away some bit of common information from them and be able to say something useful to the person who has to act, rather than merely speculate.

Presumably because these arguments are the basis of action?

Exactly. Action I think resolves the arguments. What you really believe, in the end, is expressed in the actions that you are willing to take and it doesn’t matter finally, how you got to that belief, what kind of representation your thoughts take, i.e., different metaphors and different models. What ultimately matters is what actions you take and what happens as a result. We can always argue about that or how to interpret it or what really did happen. I’m not saying that any of that is really cut and dried, but it comes down to very concrete things. Of course, that’s the reason why we can talk to each other, too. When anybody argues that I’m on a metaphorical plane and you are on another metaphorical plane and we can’t communicate because we have incommensurable metaphors, he is largely mistaken, because, to the degree that those metaphors have any purchase on reality, they result in actions which are quite concrete and which we will both see and both agree on. An open market purchase is an open market purchase; either the bonds are sold or money is transacted and it doesn’t matter how you think about it. If I could understand the actions you end up taking, I have some way of understanding your way of thinking.

What about McCloskey’s argument that established economists, connoisseurs of art, set the standard that multitudes must follow and they are the market outcome?

I don’t know. I guess what it finally comes down to is this: When I think about myself as a macroeconomist, not as a methodologist, and when I think about the problems which enter from the actual concrete world, I see absolutely nothing in what I have read of the rhetoric literature, that affects my practice in any way. So to me the rhetoric project is a very visible, but entirely peripheral, phenomenon, irrelevant to the progress of economics.

So economics progresses through the practitioner’s efforts?

Absolutely, and talking about it is not a useless exercise. People write thought papers to try and tie threads together and that’s a very useful thing to do. It helps people see where they might go on. So I don’t have any objection to meta-level discussions. It’s just that the meta-level discussions that I’ve seen on the part of the rhetoric project have not been the ones that I’ve found the least bit informative to practice.

You say that economic progress by economists who do economics, that sounds like Jacob Viner’s definition of economics. What is exactly doing economics? It could be different for different people.

Yes, it could. There are different definitions of what economics is. I’m kind of old fashioned in this. I think economics is about production, distribution, and wealth: getting and spending. That’s more of a classical view than economics being about choice under constraints, which perhaps is a neo-classical view, and which makes economics a subject that has claims on lots of other disciplines. I think economics is about what the layman understands to be the economy. Money, production of goods, earnings, employment, those kinds of things. But I understand the other point of view, and I think we can all live very happily with these competing definitions and with competing interests. Letting a thousand flowers bloom, seems to be a perfectly sensible approach.

In doing substantial economics involving production, distribution and so on, you seem to say that most of the issues can be decided one way or another?

I don’t know. There are issues which are contentious, and we need to think about how to decide them. Now whether they ultimately can be decided is another question. I’m generally an optimist. I generally think that reasonable people can reason together and work things out. But maybe there are things which are undecidable. Maybe there are things which ultimately cannot be decided one way or the other, in a persuasive way. There is some empirical evidence for that, since these disputes keep cropping up over and over again, and they don’t necessarily go away. I guess all I really claim is the disputes that are worth thinking about, the disputes that are at the level of the practice of economics, are not at the level principally of meta-economics.

Perhaps you can tell us what specific projects of yours are related to methodology.

I happened to work on the things which are empirical but are in highly contentious methodological areas. My central interest has been in causality and issues like “does money cause prices or do prices cause money?” or “do taxes cause spending or does spending cause taxes”? The methodological issue is obvious because we need a proper analysis of causality. And that is a very contentious issue. But there’s also the completely practical issue here, what are the numbers; what do the numbers tell us, what kind of advice could you
give to the Federal Reserve on the basis of what numbers tell you? That to me is the real issues in economics. The methodological issue arises because you can’t address that economic issue without thinking about the conceptual problems of causality.

I have seen a number of people using the Granger test to decide causality. What do you thing about it?

Granger causality is, I think, a misnamed notion, although Granger claimed he has the right to use the word “causality” any way he sees fit. My use of the term causality is related to the ability of someone to control something. When you go and hit the light switch, the light comes on. You know its controllability that’s the issue there. I think that’s really very close to the typical layman’s definition. When I talk to people in the economics profession most of the people I talk to will agree that that’s what they’re thinking about. But it is clear that causality in the sense I’m interested in is different from causality in the sense in which Granger is interested. His test is in fact appropriate to his definition, it is not appropriate to my definition, and he doesn’t pretend otherwise. He’s made the statement in more than one place, that controllability is a deeper issue than causality as Granger defines it.

What I think is disturbing about the Granger causality test is in fact how easy it is to run econometrically and as a result, how many people use Granger Causality test without having addressed the conceptual issues. I’ve seen lots of cases where people have tried to infer controllability from the Granger test. But it’s not something that you can infer from the Granger test.

It’s done for spurious correlation?

Yes, and it’s done all the time. Talk about rhetoric. Here careful attention to semantics would be very helpful to the profession, because it would stop being confusing. I think Granger misled the profession when he called his test a test of causality. Obviously he doesn’t see it that way, and would very much say the same thing about me. I think the semantic issues are very important because of the way in which they can lead people, in practice, to do the wrong thing.

So in your definition of causality as controllability, how would one go about testing causality. I’m not asking you to describe in detail, but in general terms, how would you describe your approach to causality?

It seems to me that a causal (controllable) relation is one which is invariant to the act of controlling it. If you say, “A causes B”, but in fact A hasn’t been activated, i.e., not A and A causes B. Then somebody comes along and they say, “OK we can activate A, we’re going to hit the light switch.” But then you say, “when A is activated then A doesn’t cause B anymore.” In that case A never caused B in the first place. The causal relationship is one that is invariant to the act of controlling it, of using it as a means of control. So it has to be one in which you have some kind of linkage so that I can either make it A or not A, and by doing so I either get B or not B. Now it is more complicated than that, because there are all kinds of issues about multiple causation, and changing regimes and things like that. So causal direction could conceivably change, but basically controllability is very tightly related to invariance.

In practice, what my own empirical approach to causality has been is to notice something about econometrics which has been noticed before about regression. Say you have a particular relationship which you can define as causal because it has this flip-the-light-switch-and-then-light-comes-on property. Say that’s the truth about the world. You never really know what the truth about the world is: we can’t see the truth about the world. If you think of this world as one of linear equations, then you can think about regime changes as simply being cases in which someone is able to intervene to change the parameters of the equation. But if you are in a world in which there are no regime changes, you don’t know what the truth is: it doesn’t matter which way you run regressions, you are not going to be able to find some distinguishing feature about the regression that tells you what’s the causal direction. So you can run a regression of A on B or B on A, there is going to be no distinguishing feature between them. On the other hand if you have good reasons for believing that there has been some intervention or some change of parameters and you run the regression in the direction which essentially corresponds to the true causal direction, then that’s going to be invariant.

There are all kinds of caveats and exceptions which you have to think about, but in the simple case it will be invariant. If you run it in the other direction it won’t be invariant and that’s really the basis for my empirical works.

For instance, in the tax and spending paper, we fit models of the revenue process and the government spending process. We look at the historical record. We find periods in which, for institutional reasons, we don’t think there have been very many or any important interventions. We fit the models in those periods and do specification searches to get food fitting models that have nice properties like predicting out of sample for a period in which we don’t think that there have been any other interventions. Then we continually re-estimate them out of this tranquil non-interventionist period into the periods in which we think there might be interventions. We look for
statistical breakdowns for non-invariance of different regressions conditional on different things or marginal of everything. We interpret which regressions break down at which points, while comparing them to the institutional evidence, as evidence for the pattern of causality the underlying causal relation. If the underlying causal relation runs one way, you get one pattern of breakdowns. And if it runs the other way, you get another pattern of breakdowns, in principle. In practice it is messy; there are always different interpretations; there are always difficulties; it is open to dispute. But in the cases that I’ve worked with, it seems to me that the directions that I’ve finally decided upon are those to which the evidence seems to point; the ones for which the best case could be made. Somebody might come along and argue that I should reinterpret the data, but I think I could make a pretty strong argument why different causal directions don’t look as plausible. Somebody then could extend the data to other regimes or other countries or might suggest different results. But I think it is to the point where we have something for which we can really debate about the substance and not about the method.

Let me ask a slightly different question. In one of your papers, you mentioned the role of the history of economic thought in economic studies. There you referred to history of economics as a rudder. Can you elaborate on this?

Well, we are always rediscovering the wheel in economics. As a profession on the whole, we forget our history. People like Prescott and Kydland and real business types, are beginning to rediscover the disputes between Burns and Mitchell and Koopmans. They are beginning to rediscover the initial ideal of the econometrics society which was to join mathematics with statistics. People would have said those things are like oil and water; they don’t really mix; and after a certain period they begin to separate. The ideal is to bring them together. Prescott, and people like him, are trying to reclaim that tradition. But I don’t think that they were actually, until relatively recently, very well informed about that tradition. I think by knowing the history of economic thought you see that there are opportunities that we didn’t take. You can see the genesis of particular ideas; and if the idea is fizzling out you may be able to take a few steps back to find ways in which it could have been developed differently. Go back to critical junctures and that’s the sense in which I think it’s like a rudder. We’re keeping a log in the history of thought of all the different routes we’ve taken. which also therefore is a guide to possibilities which have not yet been realized but are not necessarily dead. People say economics is essentially using neo-classical,