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Interviewer (Takami) :

Basically, there is no lecture in economic methodology in Japan, and so this talk was part of economics. So, ordinary readers of this magazine (The Keizai Seminar) wouldn't know economic methodology. So if they ask you what this subject is, how are you going to answer?

D. Wade Hands:

Well, if students know something about the history of economic thought, you could address economic methodologies through the history of economic thought. For instance, if you think about someone like John Stuart Mill; Mill had an economic theory. You might learn about this economic theory in your History of Economics class, but John Stuart Mill also wrote about economics as a science and how economics was a science and in what way economics could be a science and that's economic methodology. Economic methodology is a study of how it is that economics is a science or whether it is a science or not, and it doesn't have to be economics broadly. It could be a study of a particular economic theory or a particular model or something. So, methodology raises questions about the scientific status of economics in various economic theories, and every major figure in the history of economic thought implicitly had a methodology. They all had a view of how, what it is they were doing with science as opposed to religion or philosophy or sociology or something else.

Interviewer (Nohara):

So, and as you know, in the U.S., faculties of economics in most universities use a program focusing on mathematical approaches, and so they exclude philosophical and historical subjects, so in that sense, economics is very unique among other, among social sciences because it uses mathematical methods. So, in that sense, are you focusing on your audience...? Is your audience maybe economists or social scientists, engineers who do not understand the mathematical method, or your audience mainly discovers on ____ methodologies may orient economists who research mathematical resource?

D. Wade Hands:

So, is your question about audience or question about the book "Reflection without Rules" or generally what I write?



Interviewer (Nohara) :

Generally what you write.

D. Wade Hands:

Well, the things I write about economic methodology are for a number of different audiences. In the last few years, I've had papers on the history of economic thought journals, but I've also had papers on the philosophy of science journals. So it really depends on what the topic is. With respect to mathematics, I don't see mathematics as either a particularly positive or a particularly negative issue. I would say that most of the mathematical economists historically from Walras to Pareto had an argument about how mathematics contributed to the scientific character of their research and now a lot of other people would say that it distracted from it, so there is, certainly a methodological debate about mathematics in economics. Over my lifetime, I have written a lot about the methodology of mathematical economics, but that's not the only [methodological issue](#) that I have written about.

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Interviewer (Nohara):

So, in your research on economic methodology, what do you think about the present mainstream economists? In your own view, it is very nice or it is very bad... evaluation of the present mainstream economic method?

D. Wade Hands:

Well, I am a historian as well, and as a historian, answering a question like that is very difficult because these things are very complex. They are not simple – I would never say contemporary economics is good or contemporary economics is bad. Some of contemporary economics is quite good and some of that is quite bad, and it depends on what part you're talking about.

Interviewer (Nohara):

Yes. Do you think present mainstream economists, such kind of New Keynesians, can explain the economic phenomenon that all economists cannot explain economic phenomenon in reality, what do you think about...



D. Wade Hands:

Well, the New Keynesian economics is a bit of a problem for me and that's because they accept too much of new classical theory. They accept the new classical framework; they simply add some imperfect competition or some wage rigidities or something. And, to me, if you want to be a Keynesian, you should be a Keynesian; you shouldn't be a Keynesian by tacking on something to a new classical model. So I am not particularly sympathetic to that research program, but that's a very specific thing.

There are a lot of things that are going on in economics right now; more broadly, like behavioral economics that I think are extremely important. But again, thinking as a historian, whenever you've a substantive change in science, there are going to be both costs and benefits. I think behavioral economics is a lot more empirical; it's a lot more testable; it's a lot more in touch with the real world and real behavior. But, on the other hand, behavioral economic theories are very idiosyncratic. 40 or 50 years ago, economics was a unified discipline and it had a coherence, and now it has lost that because of all recent changes. So there have been both costs and benefits. Economics in the last couple of decades has had a revolution in terms of the role of evidence and evidence-based research. And there is now more empirical data that goes in economics and that's a good thing; but, as a result of that, we have also kind of lost our coherence and the common body of theory that every economist learned and used, and so, as I say, there are costs and benefits. Overall, I think the benefits outweigh the cost, and there is a positive net benefit, but that doesn't mean that the empirical turn has been free; it doesn't mean that there is no costs involved with these changes.

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Interviewer (Nohara):

So, as you know, many economists now seem to lose their interest in general theories; instead, they focus on some very peculiar topics, very peculiar aspects. So, in general, behavioral economics, neuroscience, neuroeconomics and so many approaches exist, but they focused on some aspect of economic behavior, economic phenomenon. So, many economists focus on the peculiar, so you can trust this and lose your interest in general **story (theory?)**. So is that tendency influencing your research or is it scholars and economic methodology? I mean is that loss of interest in economic methodology as a loss of interest in general theory something related to your research or are you...



D. Wade Hands:

Well, one part of this movement away from grand theory, whether it's Marxist theory, neoclassical theory, or Keynesian theory, is just a general movement in contemporary intellectual life. For example, if you look at a discipline like sociology, they used to be debating about whether they were structuralist or functionalist, but now, they are just doing empirical studies on various societies; they have also abandoned the grand theories. If you look at the natural sciences today you see a lot of this; even physics and some mathematics is unclear about what the grand theory is. They would still like one, but they don't necessarily have one, and so, part of this change has nothing to do specifically with economics. It's just a general trend away from grand theorizing. In some sense, 19th century social science was very much oriented towards displacing religious thinking and that displacement required grand scientific theories.

For instance, John Stuart Mill was trying to displace a religious tradition in the west with a secular scientific tradition, and so the kind of theorizing that was done was quite grand because it had to replace everything. It had to replace ethics, it had to replace esthetics, so that whole tendency has sort of run its course in science; in part I think because of where those grand theories ended up in the 20th century with communism and various totalitarian governments. And so, there has been a retreat from grand theorizing in all scientific disciplines, and I think economics reflects that. I don't think it's just economics.

On the one hand, I think this change has helped economic methodology because in a world where debates were about Keynesianism versus monetarism, or neoclassicism versus Marxism, economic methodologists really only had a few simple things to say. They could either say neoclassicism is good and Marxism bad, or that neoclassicism is bad and Marxism good, but now you can concentrate on all these specific research programs. There are all these various research programs and models that you can go into as a methodologist; you can look at something like behavioral economics. And you can say, look, there are some parts of behavioral economics that are methodologically good and some that are bad.

In my own case, the libertarian paternalist program to me is deeply problematic. On the other hand, there are parts of behavioral economics like social and/or constructed preferences that I think are extremely important. So you can actually start to do useful epistemic work by focusing on these various small programs, whereas in the old days, all you could do was to say all good or all bad; that's all you could do. There wasn't any real



productive labor going on by economic methodologists; where now methodologists can actually help economists make decisions about whether they should go this way, or that way, within a particular research program. But again, some things are lost. I personally kind of liked grand theories and I miss them.

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Interviewer (Takami):

So what is the difference between practicing economists' views about economics and economic methodologies, but they are also called the economist view, all the economics that are how, how do you acquire a perspective as a philosopher of economics or economic methodologies?

D. Wade Hands:

How do you...?

Interviewer (Takami):

How can you train yourself to become a professional economic methodologist or here also called the economist?

D. Wade Hands:

Yes, it's very difficult because on one hand, I think the attitude today is so different as I was talking about yesterday. I mean it is not like Mark Blaug who wanted to just say "good, bad" "good, bad" "good, bad". Today it is presumed that every particular field of economics has some scientific value to it, and it's the job of the economic methodologist or the philosopher of economics to figure out what that good is. Now, probably, each also has some bad, and you need to sort it out, and to do that, you really can't pay much attention to what the economists think that they are doing; you need to pay attention to what they are, in fact, doing and what practicing economists say about knowledge, testing, data and all that; it is good to listen to it, but you can't take it as the whole story. You really need to look at what they do, how do they model, how do they use evidence, how do they do these things, and try to judge each part – it's much like being an anthropologist.

If you are an anthropologist and you go to some premodern society, you've to understand their values and you have to understand what's meaningful to them, but you can't lose



yourself in what's meaningful to them and start to believe that what's meaningful to them is meaningful to you and other professionals in your field. You have to keep a distance where you can understand, but you can also criticize, reflect, guide, and so forth. It's not an easy task to decide how you do it for each particular model or each particular community. The job of an economist is quite different.

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Interviewer (Takami):

But how did you yourself, in your idea, how did you yourself become acquainted with this opportunity?

D. Wade Hands:

Well, this is very personal, and this is not a guide to doing economic methodology, but my own intellectual development had a bad side and a good side. The bad side was that when I was a young scholar, I kept bouncing from one extreme view to the other. There was a time when I was a libertarian; there was a time when I was a Marxist; there was a time when I was an intuitionist. I don't think that's a good way to be a scholar, but it did allow me to see those different points of view. I also wrote a PhD thesis on mathematical general equilibrium theory, and so, even though I don't do that kind of research now, I once experienced that intellectual community, I was briefly inside that community. And so I think one of the things that has given me a unique perspective on economics is to have spent a brief time in a number of different communities and to see the world the way they see it; that's not the way I see it now, but it was good to have an understanding of what was meaningful to them. Just a couple of years ago, I had a paper in a philosophy of science journal on general equilibrium theory, so I am still trying to understand that field even though I haven't actually participated in it for over 30 years.

So, in my own case, that was part of the process, but not everybody can do that, and I am not suggesting that one should bounce around the way I did; but it is part of what I did and it influenced the way that I think about history of economic thought and economic methodology today. And I can say the same thing about philosophy of science; I spent some time with positivism; I spent some time as a Popperian. I also spent almost a decade thinking in pragmatic terms, and so I can see scientific knowledge through those different spectacles which gives me a unique handle on them. But to some extent it also means that in my case, there has never been that single great book. I have never managed to bring it



all together into one coherent, all encompassing, Wade Hands story of economics. I do a paper here and a paper there.

Interviewer (Takami):

"Reflection without Rules" is...

D. Wade Hands:

I think "Reflection without Rules" is a significant book, but it's a survey. I am trying to cover a lot of ground, a lot of different ideas, so my background works very well in that context. But to write a book like Dan Hausman's "Inexact Science." where he says, okay, this is the way economics is, and this is the way economics should be, full stop. I have never done it, and to this point, I have never felt comfortable doing it.

Interviewer (Takami):

(? 19:12)If you are asked to design a course to train lots of our economists or economic methodology ___ economic methodology, so giving a lot of various perspective to students, that's what basically useful.

D. Wade Hands:

Yes, it's difficult, you know, the Erasmus program is a dedicated program in economic methodology and philosophy of economics. And when "Reflection without Rules" was still as a manuscript, it wasn't in a book yet, I did teach their introductory graduate course for a semester, and it's a very difficult thing because you're trying to **do so many things at once**. Many of the students had an economics background, but they either had a very mainstream economics background and don't know anything about other ways of thinking about economics, or they were committed to some heterodox program and didn't really understand the mainstream. And their philosophical background was either weak in general or very specialized. And so I really gave them a little bit of this, and gave them a little bit of that, and then tried to let them find something that they were interested in and help them to bring the pieces together to accomplish something with the task that they have set out. But there is no magic formula to training economic methodologists.

I teach an undergraduate course in economics and philosophy, and it's the same way and these are undergraduate students who are not going to be professionals in the field. They are going to be doctors and lawyers and do other things, but it's the same problem. I have



to teach some economics; I have to teach some mathematics; I have to teach some philosophy; I have to teach some sociology. And the best I can do is give them little pieces of things and hope that they can see how it all comes together, but it's not going to all come together unless they have a goal. And then if they give me a particular research target, I can usually help them find the resources that will help with that, with that goal that they have, but they are going to have to decide what their interest is.

Interviewer (Takami):

So, there is something close to your, you know, general theory or general perspective of philosophy of economics, so you don't have that, but, if you can stress it, say something about it.

D. Wade Hands:

Yes, I end up being a kind of a knee-jerk pragmatist in general. I am an ontological realist, but I am a scientific realist in the weak, Popperian, sense. I think prediction matters, but I am not an instrumentalist, and the kind of instrumentalism that appeals to me is not positivist-inspired instrumentalism, but more of a Deweyan instrumentalism where practice is broadly defines, not just empirical prediction, but practice more broadly matters.

But that said, I also think that while the details of Popperian philosophy are problematic, the general critical attitude of Popperian philosophy is a worthwhile thing. So, if you think about Popperian philosophy, it's a general idea that we should be critical, and we should try to falsify and all of that. And I think that's exactly right, but then when you get to the details of Popperian philosophy about sophisticated falsificationism versus other versions of falsificationism and such, I don't think that's very helpful, So, I am not an anti-Popperian in spirit. I think in spirit, I am very close to Mark Blaug, but where I am different is that I wouldn't then take a falsificationist approach and actually use it to chop up various economic theories.

Interviewer (Takami):

So we can safely say that you say that the "Reflection without Rules" is still representing your objective point of view?

D. Wade Hands:

I think it is, yes, I think that's right.



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Interviewer (Nohara):

So, finally there are two very general questions. We, as students of economics, ask you what is economic methodology and why do I...

D. Wade Hands:

That one is answered.

Interviewer (Nohara):

Yes, yes. So another question is what's your intention to publish or write your book and contribute to ___? What is your intention?

D. Wade Hands:

Why did I write "Reflection without Rules"?

Interviewer (Nohara):

Yes.

D. Wade Hands:

Well, my motivation for writing "Reflection without Rules" involved two things. One is I wanted to survey the literature on economic methodology, and I didn't think that the existing surveys were very good. In particular, I didn't think Mark Blaug's survey was very good because he had a specific point of view and every time he would discuss somebody or some idea, he would then either criticize them or praise them on the basis of whether they agreed with him or not. I wanted a more objective balanced treatment of these various different points of view. So, one goal was to discuss these various points of view and do that in a clear and understandable way, but to not pick winners or losers, not to take sides on it. So that was one goal.

The second goal was to say that with respect to philosophy, I don't think that the philosophy of science is the only thing that's relevant to economics. In economics methodology broadly defined, I think, that you will have to look at other areas of philosophy as well, so that you want to have a fairly broad church approach to these things and that would even include some elements of science studies, not just philosophy or philosophy of



science, but also science studies; the sociology of science, fields like that.

So, the second goal was to make sure that economic methodology wasn't a just philosophy of science applied to economics, so the reason I used the term "science theory" was that I didn't want to make it just philosophy of science. I wanted it to apply all kinds of theorizing about science, even sociological theorizing about science, to economics. So, I wanted to have a very wide range of science-theoretic tools. When I talked about economic examples, I tried to bring in economic examples from all over; there is a place where I talk about Veblen's Institutionalism, and there were places I talked about Keynesian ideas, and equilibrium ideas, so I didn't want to focus on just one kind of economics as the target. So, I wanted to use a wide range of science theory and apply it to a wide range of economic theories and then let people sort out what they found was interesting or useful.

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Interviewer (Takami):

Can I ask something?

D. Wade Hands:

Sure, sure.

Interviewer:

In Reflection you discuss the relationship between the example of the Schultz & Hotelling work on empirically testing demand theory and its relationship to the economics and under-determination.(27:44)

D. Wade Hands:

Right, right, right.

Interviewer (Takami):

Can you expand on this a little bit? Can you pick another example in this kind of result...?

D. Wade Hands:

Of what? I mean another example of...?

Interviewer (Takami):



...of empirically testing the core axioms of economic theory?

D. Wade Hands:

I think if you want to test the core axioms of economic theory, you want to look at what they were doing in the 1930s and compare it to what what they are doing today in behavioral economics. Trying to talk about rationality and rational decision-making and the anomalies of rational decision-making and all that, is a very different approach than what Schultz and Hotelling were doing. The approach in the 1930s was hypothetical deductivist testing. You had a theory of demand based on constrained utility maximization, and that theory of demand had some, but very few, empirical implications for demand functions. The substitution effect had to be negative and all that, and that's what they tested. So they had a theory; the theory implied certain observable or potentially observable things, and they tried to test whether the implications of the theory were true or not for the available data.

You could take a Popperian view of that and say that since basically all the tests failed that they should have thrown out utility theory, but that's not what they did. What they did was what scientists generally do in such situations, to argue that the data wasn't right or the test wasn't clear. So, it's a very good example of under-determination problems where you have a theory and you have the implications, and you are testing implications, and something is wrong. You don't know whether your data was bad, or whether your statistical methodology was bad, or whether one little part of the theory is bad, or whether the core of the theory is bad. In Popperian terms, you don't know where to aim the arrow of modus tollens and that problem is a problem that exists throughout science.

I just think that testing demand theory in the 1930s was a particularly clear example of it, but you have these problems every time you try to test anything. It's a good example because everyone agreed on basically the four empirical implications of demand theory. So there wasn't any disagreement about what the theory was supposed to predict for anyone involved in the debate. And when you get to real economic practice, often times, there is a lot of disagreement about even what the implications are, as well as what the theory says, what the data should be, and all that. And yet all that agreement didn't help. Science is hard.

Interviewer (Takami):

For economists, the empirical science that's the kind of, you know, crucial test that they



needed to make or they have needed to make, but as far as I know, this kind of research is quite pure in at least in the history since 1930s and that's ____...(? 31:10)

D. Wade Hands:

Well, look, every time you do any kind of an econometric test of anything, you're using data, and when you put people in a laboratory, you're generating empirical data. And when you get an anomaly, there are all kinds of ways that the relevant economic theory gets tested and challenged by the evidence. It's just that usually it doesn't fit as neat and tidy into the hypothetical deductive framework that you see in philosophical science, and so, it's messier but from macro to everything everything else, there is always data being involved. It's just a question of whether it's the same kind of test that a positivist philosopher of science would talk about, and I think that's one of the nice things about that example from the 1930s.

It is a test of demand theory in the same way that a positivist philosopher of science would design a test. I mean if Karl Popper or Carl Hempel said how do you test this theory of utility maximization? That was how you should test it. And the profession didn't pay attention to what these philosophers of science would have said, but that doesn't mean that that's the only way that theories come into contact with evidence. You have stylized facts and whether the theory tracks stylized facts; you have time series data; you have experiments; you have randomized controlled trials. You have all of these different ways that you are challenging various economic concepts with empirical evidence.

Interviewer (Takami):

I think my question should have been how – whether do you think this effort of economists to test our economic theory was serious enough in the history of 20th century economics ____?(? 33:20) Do you think they were scientifically – did they have the scientific integrity in terms of empirical aspect of economic security, at least in the mid-20th century, philosophical science with positivism? And economists have to be able to defend their discipline in terms of positivist ideas of science, but still I don't think, you know, there was enough testing of economic theory even in that period and so what...

D. Wade Hands:

But it's very very difficult. You have a theory that starts with subjective preferences, so you have a theory that starts with non-observables, and then how people conceive the



constraints face is unclear, what they perceive as prices and what they perceive as cost is also unclear. I mean it's all very mentalistic; standard choice theories are very mentalistic and that makes it very very difficult to test, and the whole history of modern economics has been various ways to try to deal with that by moving more in a behavioral direction and trying to get away from these mental categories or trying to control **them** by using an experimental context. We have more control by approaching it in the Vernon Smith way, where we are not really trying to predict individual behavior, but rather about market behavior; we care about what happens in markets, not what happens to individuals.

So there's this constant ebb and flow, this constant struggle between the theory and evidence, and none of that would fit into a simple positivist or falsificationist framework, and so, the practice of economics has been inconsistent within either of those two key philosophical frameworks, but the history of physics is inconsistent with either one of those two frameworks, as is the history of biology. I want to understand the details of what happened in economics, not try to chastise the whole discipline for not being empirical enough because I don't think that's right. I think it comes and goes.

Interviewer:

Thank you so much.