Episode 527: Is Austrian Economics Unscientific Because It Doesn't Conduct Experiments?

Guest: Jeff Herbener

WOODS: We're still talking about Caplan's article, but this is a topic we could of course be talking about on its own, and I think for a good chunk of our discussion we'll do that. This is an objection that a lot of people come up against and especially when people are new to the Austrian School, they don't really know how to answer it when people say to them, you Austrians aren't scientific; you're not like other economists who put aside their ideological predispositions and they just sit there and look at the facts or they engage in some kind of econometric analysis that you have for our own ideological or strange methodological reasons ruled out. So you're not scientific; you're just dogmatic. You've got certain views that aren't based on empirical evidence and observations, and this is the very opposite of the scientific method. It's the very sort of analysis that the modern world has been so fortunate as to move away from, and now you want to take us back to heaven knows what age from the past.

Okay, that is really what people do hear, so I want you to talk to us about that a little bit today, about the Austrian method, about praxiology, about the method that Ludwig von Mises considered to be appropriate to economics, and why this is not so-called unscientific. Why don't we start off with that very word — scientific versus unscientific — and try and understand what that really means? When we say something is scientific or not scientific, how should an economist think about these terms?

HERBENER: Right, so we can start with that. Murray Rothbard liked to point out that the root of the word science just means in English knowledge, and so to be scientific just means to discover truth about the world. And as he held and as Austrians who follow him hold, depending on the subject matter that we're investigating as scientists, it might be that if it requires different methods to plumb the depths of understanding in these areas, and so it's always been the Austrian view that to understand what's true and correct about human action requires a method that is different from the hypothesis testing method of the natural sciences. And the reason for this is precisely because the subject matter itself is different. We're dealing with human action in the realm of economics, and in the natural sciences we're dealing with inanimate objects or at least maybe in the realm of biology with living things that don't act as human beings do. So I think that's the basic starting point for discussion of the Austrian view versus this neoclassical approach, which accepts the hypothesis

testing method as appropriate for human action, for discovering truth about the social world of human beings.

WOODS: Let's try and take a specific example I mean, it can be — you can just devise a hypothetical, but show me what a neoclassical economist would do in this regard that an Austrian would find objectionable. What would be an assumption or an approach or an example of a hypothesis that might be tested in this way, where the approach would offend our methodological sensibilities?

HERBENER: Well, I think almost any example of a claim about a law of economics would suffice, so let's say we wanted to look at the Austrian approach to discovering the law of demand versus the neoclassical approach. So the neoclassical approach would be to treat the law of demand as a quantitative relationship that would be found in empirical evidence over time as to the price of a good and the quantity purchased that buyers make at various prices.

WOODS: Okay, so just state the law of demand for people.

HERBENER: Yeah, so the law of demand would say that only at lower prices do people buy more of a good, so price and quantity purchased by the buyers is inversely related. And so they would gather up a big data set and look at the empirical evidence in some particular market — the gasoline market or whatever it is — and see whether or not there was a correlation. When prices go down, do we actually see the quantity purchase go up, and when prices go up in this market over time, do we actually see the quantity purchased go down?

And there are two kinds of objections that Austrians make to this procedure. One we might call theoretical and the other is historical. So the theoretical objection is when the Austrians talk about laws of human action, we're not speaking about empirically testable hypothesis. We're speaking about a different body of knowledge that can be logically inferred from the very nature of human action, the very nature of the fact that people have ends and strive to attain these ends with the application of means, and all of the knowledge that we have about human action that we can ascertain because we are human beings and we can reflect upon the nature of our actions. So if an Austrian wants to develop a law of demand, he would start with just logical propositions about ends and means, noticing that gasoline can be used as a means to attain ends and then having attained the most valuable ends with a given amount of gasoline, only less valuable ends can be attained with additional quantities of gasoline. And for that reason alone, people would be willing to pay less to purchase these additional gallons of gasoline. This is not an empirically testable proposition, but is simply a logical inference from the economizing behavior of human beings.

And then the second problem with testing empirically the hypothesis about the law of demand is that historical events are complex. There are a multitude of different causal factors that enter into the generation of the data of human action, and there isn't any known objective way to disentangle all of these effects. Or as Mises and Rothbard liked to put it, there are no constants in the quantitative data of human

action, in the relationships of these quantitative data sets, so it isn't possible to formulate mathematically a functional expression that can be tested with empirical data.

WOODS: It seems hard to imagine that neoclassical economists haven't at least anticipated or addressed these types of objections somewhere. For instance, let's start with the objection that human beings are not inanimate objects and they have wills and so on. It seems unlikely that they don't see that, but do they think that that's not worth worrying about or they think they've somehow factored that in? What would they say?

HERBENER: Well, as far as I understand it, the only answer to inquiries like this is that they believe that the scientific method, the natural science method is the only way to get at these quantitative relationships, and that if this requires them to construct certain abstract models that aren't consistent with the nature of real human beings, then so be it. The proof of their usefulness would be in the results that we get. But the Austrians' response to this is of course that we do have a method of dealing with the complexity of historical events, and this is the standard method of the historian, is to use human judgment to assess the relevance of each of the causal factors in terms of the quantitative effect that it has on the outcome.

WOODS: But what about the claim that we can't figure out which factor, given that there are so many factors at work in a complex economy — if we increase the minimum wage, there are many factors at work, so we can't say, well, increasing the minimum wage has such and such effect, and I know it because I observed the effects. There are many, many things, some of them that will accentuate the effects of the minimum wage, some of them that will work at cross purposes of it. There's no way to disentangle that. But don't they think they have some way of doing that? And secondly, can you explain to me what a regression is? We hear economists say, well, I ran a regression, and I found such and such. I'd also like to know about that.

HERBENER: Sure, so let's deal with that first, since that is the technique they use to disentangle the causal factors.

WOODS: Right. Yeah, doesn't that work?

HERBENER: Yeah, so here's the problem, or here's how the procedure works. So what's done is that the economist would write out a functional relationship, write out the functional equation that has an independent variable that they're trying to ascertain as causally connected to a dependent variable. And so with the minimum wage, it would be raising the minimum wage, and then there would be other sorts of causal factors that they would identify through various means that they would put into the function. And in the function, then, there of course will be these variables, these independent variables that affect the dependent causally, they claim, and there would be constants. And what the econometric analysis, what the statistical analysis does is to correlate the different changes in each of the independent variables with the changes in the dependent, and then the correlations are used to make quantitative

estimates of these constants. And although Caplan doesn't seem to recognize this, the main objections that Austrians make to this procedure is there are no constants in the quantitative relationships of data that's generated through human action. And therefore, the whole apparatus of establishing a functioning and then saying there are constants of unknown magnitude, and we use the regression to estimate the quantitative magnitude of the constants is inappropriate.

WOODS: So what then is appropriate? What would be your, let's say, five minute explanation of the Austrian approach — the Austrian approach in the tradition of Mises?

HERBENER: Right. Here the approach as Mises laid it out in his book, *Theory and History*, is to use our own human judgment to assess, given the structure of causal factors, what their quantitative magnitude is. So for example, if we had, let's say, the Great Depression, and we wanted an economic history of the Great Depression. Then the Austrian approach would be to identify the causal factors from theory, like monetary inflation and credit expansion of the 1920s, and then other causal factors that would be involved in prolonging the depression, like various wage propping up measures by the Hoover administration and the change of business confidence toward uncertainty and so on and so forth. And then just make an assessment as to how important these factors were in the actual outcome of the Great Depression as it played out.

So for example, the standard Austrian view would be that the financial downturn of '29 to '33 was brought about by the collapse of the previous monetary inflation and credit expansions. We had the asset price inflation of the late '20s and then the corresponding collapse. But the prolonging of the Great Depression into the mid '30s and then later '30s, the main cause here was the uncertainty of investment projects on the part of capitalists, and you can again find data to indicate this, the suppression of actual investment spending and so on. And then unemployment of course would be explained primarily by the falling of the purchasing power of money at the same time that wages were being propped up by various measures of government.

WOODS: All right, let's turn our attention now to Caplan. Now, he doesn't have so much to say in this section that I think is super critical for us to analyze, but just for the sake of completeness we'll look at it. He notes in — and by the way, of course the Caplan article will be at the show notes page, TomWoods.com/527 — he says that it's true — he does share some of the Mises-Rothbard criticisms of neoclassical methodology, so he's not altogether opposed to what they're saying. But he says that, "Austrians reject econometrics on principle because economic theory is true a priori, so statistics or historical study cannot test theory. Fair enough, but...econometrics and other empirical work can play a more modest role: to help determine how big (or trivial) various theoretically relevant factors actually are." Is that a point that can be conceded?

HERBENER: Well, I don't think we can concede it entirely. We might in the best way think of it like this: that of course when someone's interested in the history of some

period, they won't look at empirical evidence, but the question of the magnitude of effects can't be settled by running correlations. Maybe this is clear, by the way, if we make another application of this method of history that Mises makes. Mises points out that this method of specific understanding, as he called it, was also the method used by entrepreneurs to make anticipations of the future, and here it seems pretty clear that if one is to make the most accurate prediction of the future, they would not merely rely upon correlations of data sets in the past, but this of necessity would require that empirical knowledge of the past to be judged by the entrepreneur before the entrepreneur says, okay, well now I think the future will work out with the magnitudes being such and such.

WOODS: Okay, but couldn't we use, just for the sake of argument, empirical study and just rolling up our sleeves and going out and investigating stuff to try and figure out — I mean, I understand that entrepreneurs, there's this misconception that entrepreneurs just have all these just brute facts and they look at these facts and a conclusion just pops out, and then they follow the conclusion. I get that that is the wrong way to think about entrepreneurship, but couldn't we use empirical evidence or empirical study, let's say, of history to try to understand, as you were indicating, the depth of the depression of the 1930s? That, well, maybe fiscal policy or maybe these different programs of the New Deal may have held it back, and that helps to bolster what we understand through theory.

HERBENER: Right, so I think that is right. I don't think there is any — I think Caplan would agree that that's right. I think the point of contention is whether or not the technique of econometrics provides us with essential knowledge that we couldn't acquire more accurately in a different way. I think that's the point of contention. So do we — you know, we absolutely have to do the empirical work to find out what the conditions were and then use the empirical work to judge the magnitude of effects. But whether econometric correlations are necessary in that process, that I think is debatable.

WOODS: Caplan gives 10 different areas that he thinks are interesting and some of the best new ideas to come out of academic economics over the past 65 years or so, or since the publication of *Human Action*. And he concedes — again, he's more skeptical of formal mathematics and econometrics than most economists are, and he lists all these contributions to economics and he says although formal math was the main language used to present these ideas in academic journals it's not clear that math was really instrumental in the discovery of these ideas, and he says econometrics, their "contributions to economics are similarly meager." So he says that "have had fifty years of ever-increasing hegemony in economics. The empirical evidence on their contribution is decidedly negative." So he's sympathetic.

But then he goes on to say, "This does not mean, however, that working economists ought to immediately cease to employ" — and he calls them "M&E" — "in their work. This has been the Austrians' main response" — in other words, that they should cease to employ them — "and it has led to their extreme isolation from the rest of the economics profession. The simple fact is that [mathematics and econometrics] are

the *language* of modern economics, much as Latin was the language of medieval philosophy. These professional languages waste a lot of time and make it difficult for laymen and academics to communicate. But once mastered, even dissident scholars can use these tools to speak their minds." How does that strike you?

HERBENER: Well, yeah, there are a couple things that strike me about that. I don't think it's entirely correct or at least it's not a good analogy to say that mathematics is a language. Mathematics is a system of logic, and whether or not the system of logic can be directly applied to the discovery of knowledge with respect to human action is the open question. I think we would all agree that in order to apply mathematical logic in these models that the neoclassical economists use, they have to make assumptions that are entirely heroic. They assume economic agents; they assume certain abilities of these agents with respect to their utility assessments and functional representations of these things that we talked about before. And so in a way, their beginning point is anti-empirical, and so when they get their result through mathematical logic, they have to translate it back into verbal logic to make their case for applying it to real human action, and that it seems to me is just as problematic as using verbal logic straight out and just working through the normal logical set of arguments and arriving at a conclusion about human action.

WOODS: Let me just give you a flat out objection, just the basic, gut-level objection that somebody might have to the whole Austrian approach, and it's a gut-level objection that I think even some sympathizers have or some young people who are just learning it — they feel funny about it, and it's this: suppose the subject of the minimum wage comes up, and you hear people saying we have these new studies, and they seem to show that modest increases in the minimum wage anyway don't have employment effects really worth worrying about. And the Austrian in the conventional view is basically just sitting there with his fingers in his ears saying, well, I don't care what your stupid studies say, because I already know the answer. How can you make that not seem as bad as it sounds when I just described it?

HERBENER: (laughing) Well, I think the appropriate Austrian response to the studies is not that we can dismiss them, so to speak, we can dismiss the empirical evidence that they contain, but simply that we have to cede theoretically that certain stipulations were in place, certain conditions were in place in the real world that the theory, the sort of straight answer that we get from the theory, that raising the minimum wage would cause unemployment to go up, those conditions in the real world that we did not stipulate in that theoretical expression, were in fact enforced in the real world. So there were certain things that occurred in the real world that, as you said before, either augmented or diminished the ceteris paribus claim that we make about higher minimum wages leading to a lower quantity demand of labor.

WOODS: Okay, what I want to leave with is for people who want to know this better, really want to understand the Austrian position better, you can of course read the beginning sections of *Human Action*, but you may find them difficult. My two favorite treatments of this subject offhand, in addition to conversations with Jeff Herbener, would be Hans' piece — it's an essay, but it's also in a monograph form — "Economics

Science and the Austrian Method." But secondly, interestingly enough, the thing published by the Mises Institute years ago — and it has the word "praxiology" in it and I can't remember the title offhand; I'll put it at TomWoods.com/527 — but it's written by George Selgin. You know the essay I have in mind.

HERBENER: Right. Mm hmm.

WOODS: And I think that's a really good overview of the subject.

HERBENER: Yeah.

WOODS: Is there anything else I should add just for the beginner to get his feet wet on this apart from those two pieces?

HERBENER: No, I think those are — you know, it's a funny thing, that Austrian economists don't actually spend much time writing about method. It's sort of odd if you were —

WOODS: Yet we're accused of being obsessed with it.

HERBENER: Right, but actually we don't write very much about it, so it's sort of hard to find pieces on this. We're actually doing economics. That's what we're interested in.

WOODS: Yeah, we're too busy doing that. Instead of this meta stuff about how somebody should do economics, we're just out doing it.

HERBENER: Right.

WOODS: Yeah, how about that? So we don't really have anything comparable — well, do we have anything comparable to the 1953 Friedman article on method that he and the Chicago people used? Like, what would be our comparable thing? I mean, there are Misesian books on epistemology, but it's not quite the same.

HERBENER: No, it's not the same. You know, Roderick Long has that great critique of Friedman.

WOODS: Yeah, you know what? Let me put that on there too. He's got a Friedman versus Mises kind of article.

HERBENER: Mm hmm.

WOODS: And it's like an Aristotelian — I don't know; I know exactly the piece you mean. It's from the QJ.

HERBENER: Right.

WOODS: Yeah. Yeah, I'll put Roderick's piece on there. Yeah, that really helped me to understand things quite a bit better actually too, and yeah, they were very good criticisms of Friedman from a philosopher, so I enjoyed that. Okay, so those three pieces. If you've read Roderick Long, Hans Hoppe, and — it's funny to be recommending Hans Hoppe and Roderick in the same sentence, and George Selgin. What a funny three people, but honestly they are at their best in this material — you really almost know all you need to know. And then if you want to spend time reading epistemological works of Mises, then you know, you get a gold star and extra credit, but this stuff will really, really give you everything you need. All right, Jeff, thanks for doing this. I mean, this is difficult stuff, and we get this criticism all the time that we're not scientific, but it's an equivocation surrounding the word "scientific," and Aristotle from the beginning made clear that you have a different method based on the nature of the subject matter that you're studying, that that's just a fact, that you don't use the same method in geometry that you use in chemistry or that you use in physics, and you just have to determine what the nature of the field you're studying is, and the appropriate method will follow from that. That's what we're trying to say and maybe not with as much success as we'd like, but doggone it, we're saying it. Jeff, thanks again for your time.

HERBENER: Thank you, Tom.