Methodology and History of Economics

Essays in Honour of D. Wade Hands

Reflections with and without Rules

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Piecemeal Empirical Models

The period since the Second World War can be well described as the age of models in science – not only in the natural sciences, but also in economics and, perhaps to a lesser degree, in other social sciences. While philosophers of science and philosophers of economics are alert to the importance of models, they have little noticed how much quantitative empirical research dominates economics. Influential philosophical discussions of economic models have focused on making sense of how theoretical models that are "caricatures" or "unrealistic" can nonetheless be informative about the real world (Gibbard and Varian 1978, pp. 665–666, Sugden 2000, p. 3). They explicitly eschew consideration of quantitative empirical models or address them only at the level of "casual empiricism" (Gibbard and Varian 1978, p. 665, Sugden 2000, p. 6).[1]

In the wider philosophy of science, there is a vision of how models should relate to the world that Paul Teller (2001) refers to as the Perfect Model Model. The perfect model would tell us everything about everything. An example of perfect-model thinking is Pierre Laplace's famous claim that given a complete set of initial conditions and the laws of physics, one could, in principle, write the history of the world in detail in advance. A similar vision is implicit in the economics of his compatriot, Leon Walras (1874[1954]), whose general equilibrium model aims at a comprehensive account of the interdependence among all economic agents. Walras's theory is, even more than Laplace's, a schema, but the idea in both cases is to fill in the missing pieces completely, and the measure of success is how far current models deviate from the ideal perfect model.

An alternative vision – and the one that I shall argue for – measures our scientific achievement not against the inaccessible perfect model at the end of the scientific journey, but against the history of that journey. Success is counted in additions to the stock of facts and explanations. Herbert Simon and Nicholas Rescher (1966) present an alternative to the Laplace/Walras vision of a comprehensive perfect model embodying universal laws. The function of the scientific model in their vision is to articulate the causal

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structure of the world. Simon and Rescher would readily concede that, in principle, everything is connected to everything else. Yet the actual acquisition of causal knowledge cannot proceed on that basis. Rather, it must set aside many potential causal connections in order to effectively uncover the most important ones. A causal understanding of the world can be constructed only on the basis of the Empty World Postulate, which posits that causal connections are not so dense that the investigator cannot isolate subsystems, and the Postulate of Prepotence, which states that subsystems can be isolated in which causal relationships internal to the subsystem are strong relative to those between the subsystem and the world external to the subsystem (see also Simon 1996, ch. 8). These two postulates amount to a methodological vision that knowledge can be effectively acquired in a piecemeal fashion.[2] In a world of piecemeal acquisition of knowledge, models are esteemed when they prove to be powerful instruments both of fact gathering and explanation.

As instruments or tools, models are a puzzle to philosophers. Models are generally thought to represent the world. Yet the world is too complex to be represented completely, so that models, at best, incompletely achieve their task. Worse still, in practice some features of models seem to *misrepresent* the world. Much of the discussion of models as idealizations or fictions addresses the implied puzzles: how can models that are false be part of good science? How can mispresentation be useful? The statistician George Box frequently wrote variants on a favorite theme: "All models are wrong, but some are useful" (Box and Draper 1987, p. 424). My thesis is that Box is wrong: *All useful models are* essentially *true.*[3]

Truth and Knowledge

The puzzles about how models that appear to misrepresent or to trade in fictions arise from a peculiarly philosophical treatment of truth and knowledge. (Note that I say a *philosophical* treatment and not a *philosopher's* treatment; for it is a mode of thinking that is also found among economists and other practitioners when they reflect on their own activities.) Philosophical treatments often link knowledge not only to truth, but to perfect truth: "the truth, the whole truth, and nothing but the truth." The standard is impossibly high and very far from the kind of truth that we demand of our children, our friends, and our fellow citizens. Truth in ordinary language really is thought of as something like Tarski's (1944) famous deflationary formulation "'snow it white' is true if, and only if, snow is white." The expectation for truth telling that we typically demand of others credits them with telling the truth if the propositions that they assert reliably correspond to the relevant feature of the world and conform to a rhetorical rule – "do not express such propositions in a manner that is calculated to mislead." Even in an American court, where one swears to tell "the truth, the whole truth, and nothing but the truth, so help you God," judges and lawyers limit the scope of one's testimony: the whole truth is not, legally speaking, truth without any omission and even

statements that do not fully conform to the Tarskian standard are problematic only if they intend to mislead.

I take knowledge to be the stock of truths. A venerable philosophical account of *knowledge* defines it as *justified, true belief.* The definition has, of course, been a source of paradox and the subject of criticism. The focus of both the paradoxes and the criticism has typically been on justification: is a proposition an element of knowledge when its truth is adventitious, because its justification is defective? I would criticize it from a different angle: if knowledge is justified, true belief and knowledge must be of truth that is final and indefeasible, then the conception of truth is so pure that nothing could ever warrant our belief. We might actually possess such a final truth and, indeed, we might have justification for our belief, but that justification will always fall short of indefeasibility; we cannot know with certainty that we know what is true on such an extreme conception of truth and of the requirements of knowledge. It is this problem that ultimately lies behind such anti-scientific-realist views such as Laudan's (1981) "pessimistic induction" and instrumentalism more generally. Knowledge on such a view becomes an empty category.

Again, philosophical thinking carries us far from ordinary people's understanding. When I was a child, we owned an encyclopedia called *The Book of Knowledge*. It was old, even when I began using it, and, no doubt, many facts and explanations recorded in it were not seen as correct by advanced opinion even in my childhood, much less today. Yet, ordinary usage would not, for that reason, reject the encyclopedia's title as pernicious. The common phrase "to the best of our knowledge" conveys both that the target of knowledge is truth and that what is regarded today as knowledge may prove to be defeasible and supplanted tomorrow. We commonly assert that we know something and yet admit that being wrong is not beyond *all* possibility. To say that we "know" is to vouch strongly for our belief, our justification, or both. We can also say, "we thought that we knew, but it turned out that we were wrong." If run through the mill of a severe logic, the ordinary usage of "truth" and "knowledge" is rife with tensions and contradictions. Such tensions and contradictions signal that knowledge in ordinary language – and also in science itself – while it aims at truth and aims at strong justification, nonetheless cannot live with the notion that knowledge consists only of indefeasible truth.

We need to understand the relationship among past, present, and future knowledge, accounting for the growth of knowledge without taking each revision in our beliefs as wiping the slate clean. We need to account for the piecemeal manner in which knowledge is acquired. Modeling practices make sense only with respect to that kind of account.

Models as Instruments for Telling the Truth

It is not uncommon to say that a model is wrong or not true; but strictly speaking, models are not true or false, because propositions are the bearers of truth and models are not propositions. Models are objects and may be used or referred to in propositions to represent other objects, processes, events, and so forth. Models are possibly physical objects – e.g., wind-tunnel models or A.W. H. Phillips's famous hydraulic model of the economy (1950). Many scientific models are abstract – for example, they are embodied in interpreted systems of equations. (Even abstract models are frequently physically embodied in marks on paper or physical states and processes on a computer.) The properties of models are not intrinsic. An object becomes a model only when it is used to represent, and representation involves constraints imposed both by its author and by its receiver or interpreter.

How does a model get its representational grip on the world? The relationship between model and world is not typically anything so strict as isomorphism, as some older, formalist accounts of scientific theories suggest. Ronald Giere (1999, ch. 7, Section 3) likens models to complex predicates. A *classical particle system* is a model of behavior that obeys the Newton's laws and the law of gravity for interacting point masses. We can take "is a classical particle system" to be a predicate, and we can predicate it of our solar system on Giere's account, just as the predicate "is red" can be predicated of a rose. "The rose is red" is true, if, and only if, the rose is red; "the solar system is a classical particle system" is true, if, and only if, the solar system is a classical particle system. To say that our solar system is a classical particle system is to make a claim that this model can be predicated of the solar system or that the solar system is a member of the same type as the model (Giere 1979, ch. 5, also Giere 1999, pp. 98–100, 122 and 2006, p. 65, cf. Hausman 1992, p. 74). Some economic examples of theoretical models that might fit the models-as-predicates view might include the prisoners' dilemma or Akerlof's market for lemons.

Epistemically, the Tarskian formulation is an empty shell. If we actually want to check whether the rose is red, we must implicitly or explicitly appeal to a standard. We might hold up a red color sample against the rose and ask whether they look alike. Even, so simple a model as a color sample points to some of the complexity of models in general: we still have to abstract from the materials and shape and other properties of the color sample and concentrate only on its color if the model is to do its work. Weisberg (2015), among others, argues that the relationship of model to target is one of similarity. In the case of models more complex than color samples, the relationship might be better described as one of analogy.

We can think of analogies as structured similarities. The American pragmatist philosopher, mathematician, and scientist Charles S. Peirce (1.69) defined *analogy* as "the inference that a not very large collection of objects which agree in various respects may very likely agree in another respect."[4] The emphasis on "respects" points to the fact that we do not expect analogies to be complete. We should not expect them to stand in a one-to-one relationship on dimensions of similarity nor on degree of match on any particular dimension. Analogies are *apt* when they broker a satisfactory match of dimensions and a satisfactory degree of match on those dimensions for our particular purposes.

In an account of the role of analogies in science, Mary Hesse (1966, pp. 8–9) distinguished among positive, negative, and neutral analogies (see, also Morgan 1997, pp. S304–S305). Many philosophers have explicated models through the example of maps (e.g., Giere 2006, ch. 4, Hoover 2012). Roman Frigg (2010, pp. 276–280) is particularly illuminating on the required rules for reading maps in relationship to both analogical features and conventions embodied in scales, legends, keys, and so forth. Maps set up both positive and negative analogies. A map that is to scale for measuring distances between points may fail to draw the width of the lines representing roads on the same scale (or, indeed, on any fixed scale) relative to the actual roads, and the colors of different roads on the map may correspond to a fact about the actual road (e.g., main road or secondary road) but probably not to the color of the road surface.

Models are similar. Many claims that some feature of a model is a fiction or a misrepresentation result either from misunderstood or underspecified rules or from a dispute over the implicit rules of reading the model. A map employing a Mercator projection of the Earth is sometimes treated as a false representation of scale. Properly, it is false only if it is supposed to represent scale in a particular linear way rather than to be used exclusively for representing compass headings. In fact, many Mercator maps *do* represent scale by including a continuously graduated nonlinear key to the scale that varies by latitude. A map or a model trades neither in fiction nor in misrepresentation when the rules for reading imply a negative analogy.

The same objects, events, or phenomena are describable from multiple perspectives using distinct models drawing on different analogies (Hoover 2012). Through a judicious choice of negative analogies, such differing perspectives and models need not conflict. Teller (2008, p. 245) points out that water is usefully analogized to a continuous, incompressible medium when analyzing its fluid properties but must be analogized to a collection of discrete particles when analyzing the diffusion of a drop of ink.

Even perspicacious friends of perspectival and piecemeal knowledge often cannot resist the rhetoric of fiction and misrepresentation, despite their awareness that models, like maps, are read according to rules that treat some aspects as positive analogies and others as negative analogies that are meant to be ignored. It is "unfortunate," Giere (2006, p. 78) writes that "Mercator's map … presents a quite distorted picture of the geography of the Earth." But of course it does not present a distorted picture, except when we insist on reading it differently than it was intended.

Similarly, Teller (2008, p. 239) argues that "generally an idealization involves some radical misdescription." More specifically, he takes his own example of treating water as a continuous medium as "a mistake": water "is not a continuous medium"; yet, he points out scientists "continue to use Euler's equations [that seem to treat water as a continuous medium] in a wide range of applications" (p. 245). Therein lies one of the major philosophical puzzles surrounding scientific models. How can modeling moves that are radical misdescriptions, mistakes, or distortions be so useful? I suggest that this is a puzzle of the philosophers' own making that result from misapprehending the nature of the model and its relationship to its target. No analogy is complete or exact. If "my love is like a red, red rose that's newly sprung in June," I still don't think that planting her in a bed fertilized with fish meal would be the key to her flourishing. The analogical power of models – whether of Euler's equations of fluid dynamics or of a simple paint sample on cardboard – rests on distinguishing between dimensions of similarity that are to be used and dimensions of dissimiliarity that are to be ignored.

The temptation to fall into the trap of treating models as if they are deficient when not every feature is similar or similar to the highest degree points toward a more subtle account of the functioning of analogies between models and their targets. Models are often constructed to meet certain positive desiderata, and we may know some of the negative analogies from the first formulation of the model (e.g., idealizations are often made at the outset). Some negative analogies may simply be forced upon us, owing to the fact that models are objects with lives of their own (Morgan and Morrison 1999). Scale effects provide physical examples. It is impossible to maintain proportional linear outlines of countries and their areas and true compass headings when a roughly spherical Earth is projected onto the flat plane of a map (Boumans 2005, ch. 6). Either one has to give up proportionality for some of the features or one has to provide a complicated nonlinear key to guide the reading. Similarly, it is impossible in the case of a wind-tunnel model to maintain the proportionality of linear, area, and volume measurements at the same time. In each case, either some features must be regarded as negative analogies or compromises must be reached among conflicting desiderata or more subtle rules must be developed for reading the models. Even abstract models may be similarly constrained. Models using differential calculus may be forced to treat variables as continuous, which may be a positive or negative analogy.

Some features of a model may be classified in advance into positive (determining whether the analogy is apt) or negative (staking out the features that are to be set aside). Others may not be known in advance. These are the *neutral analogies* – that is, those aspects of the analogical object (e.g., the model) for which we do not know at the point of formulating the analogy whether to classify as positive or negative. The "may very likely" in Peirce's definition of analogy ("agree in some respects *may very likely* agree in another respect") highlights the possibility of a neutral analogy and explains why he regards analogy as a form of inference.[5]

Neutral analogies can be illustrated through the textbook supply-and-demand model. The model consists of two curves and conventions for operating with them: a price change represents a movement along a curve; a change in any other factor is a shift right or left of the curve. The analogy is drawn when the modeler identifies actors to which the curves apply and classifies various

real world factors into those shifting supply and those shifting demand. These are positive analogies. Depending on what version of the supply-and-demand model we choose, there may also be negative analogies, such as only qualitative and not quantitative relationships should be considered. We cannot read any of the desired results from the model unless we decide at what point on the diagram we should read prices and quantities. It may seem obvious that the point at which the curves cross is the point to read.[6] Yet, even if we read it at that point, there is a question about the nature of the analogy. Yes, we take it to be a positive analogy that price and quantity in the model correspond to price and quantity in the world; but if we have analogized the supply and demand curves to the conjectural responses of market participants in the world, to what do we analogize the crossing point?[7]

Consider three interpretations: first, the crossing point may not be analogized to anything at all. It may simply be a requirement of making the diagram yield a definite result that we pick the crossing point. We would then regard it as a material condition of making the model work – similar to the supports that hold up a wind-tunnel model or to the pump that drives the colored liquid through the perspex tubes of the Phillips machine, each of which is a negative analogy, in that nothing in the world corresponds to that particular feature of the model and each of which is required to make the model function in a way that makes the positive analogies possible. Arbitrage models in finance, which take as their starting points that prices are aligned such that no profit opportunities are available, while not containing any element that corresponds to the mechanism that keeps them aligned, make precisely the same kind of interpretation.

Second, we could equally propose the interpretation that the crossing point corresponds to the behavior of an auctioneer. If the market that we are modeling actually has an auctioneer, the analogy is positive, and we have precisified the connection of the model to the world. Third, on the other hand, if the market that we are modeling does not have an auctioneer and, yet, the model still seems to capture the behavior of prices and quantities as if it did, then another response would be to ask, what is it in the world that serves the function of an auctioneer? It is not, then, that the model has a missing piece, so much as it provides a characterization of what any piece needs to look like to fill the auctioneer-shaped hole.

Models are a powerful resource to science in part because they frame our ignorance; but in part because they frame it in such a way that they supply guidance about how to resolve it, not unlike the way that the geometry of a hole in a jigsaw puzzle and the visual patterns adjacent to the hole provide guidance for sorting the remaining unplaced pieces and identifying the missing piece.[8] One way to understand the role of story-telling in model building or the exploration of fictional or credible worlds is either as a way of characterizing specific missing pieces or, more generically, as a building up of sets of potential positive analogies that could be called upon where appropriate (e.g., Frigg 2010, Morgan 2001, Sugden 2000, Teller 2001).[9]

Again, it is important to recall that propositions, and not models, are the bearers of truth values. The proposition that some aspect of the world is captured in a model is the assertion that there is an apt analogy with the appropriate similarity relationships. When the asserted relationships hold positively, then the proposition is true – even when there are, as always, negative analogies as well. No fiction or falsehood is involved when a model contains a negative analogy, even when that feature is needed for the positive functioning of the model; for the point of stipulating that an analogy is negative or, in the case of empirical exploration of neutral analogies, the point of resolving a neutral analogy into a negative analogy is to define a rule of reading for model that forbids us to assess the truth of a proposition involving the model by any correspondence to a real-world counterpart to that feature. The observation that some parts of the model do not correspond to parts of the world really amounts to no more than the noting that a negative analogy is not a positive analogy. Since no correspondence is asserted, nothing is said about the world that is untrue. Of course, a model in which negative analogies outweigh positive ones will not be especially informative about the world.

To illustrate, consider my quartz watch as a model of the *time-keeping* of my mechanical clock. The only positive analogy that I wish to assert is between the times indicated on their dials. When I assert that the watch is a model, I perfectly correctly claim that I can track the movement of the hands of the clock by the movement of the hands of the watch, notwithstanding the negative analogy of vastly different internal mechanisms, each of which is necessary to its time-keeping function. There is nothing the least fictional or untrue in my claim – no radical misdescription, no mistake, no distortion. Of course, if I asserted that I could understand *how* my clock tracked the time using my watch as a model, I would be asserting a very different proposition and one that would in fact be untrue, which, if followed through, would thwart any would-be clockmaker who sought to use my watch as a guide to repairing the clock.

Quantitative, empirical models require more than the coarse classification into positive, negative, and neutral analogies. It is useful to draw a clear distinction between accuracy and precision. By *accuracy* I want to indicate whether a particular positive analogy, a particular correspondence between the model and the world holds. By *precision* I want to indicate a measure of fineness of the standard by which that correspondence is judged. Precision may have a qualitative aspect defined, in part, by the nature and number of the negative analogies a model involves and, in part, by the scope of the model – the ambition to apply it to a narrower or wider domain. Nonetheless, I will here confine attention to the quantitative aspects of precision. A model can be used to assert something true about the world when it contains features that are accurately positively analogous to a feature of the world at a particular level of precision.

One of the propositions for which Nicholas Kaldor (1961) first coined the term *stylized fact* states that labor's share in GDP is constant. Here Kaldor implicitly employed a wide standard of precision. It is accurate to assert that labor's share in GDP in the post-World War II United States stood at a constant 0.7 with a precision of ± 0.1. The statement is true, not nearly true or approximately true, but simply true. Yet, it would not be accurate to say that it stood at a constant 0.7 with a precision of ± 0.01. The point of calling a fact "stylized" is either to assert that it is accurate at a coarse level of precision but would not be accurate at a finer level of precision that is readily available to us or to assert that it is not accurate at our usual level of precision for such matters but would be accurate at a coarser level of precision that is good enough for present purposes. The possibility of stylization (of facts or of features of models) or of degrees of precision finer than those that we have chosen to employ do not automatically render stylizations inaccurate or false or fictional.

What is more, stylizations do not trade in approximation. Approximation is a distinct notion from stylization or degree of precision. Properly speaking – though loose speaking on this point abounds – *approximations* should be asserted only when there is a clearly characterizable standard against which to measure the approximation. Thus, if there is a complex function, it might be approximated through a much simpler function, and how good the approximation is can be assessed either through explicit or implicit measures of the deviation or through setting bounds on the limits of the deviation. This is the sense in which a higher order polynomial can be approximated by a linear function at a point.

Approximation has important scientific uses; but too often the term is used even in cases for which we do not have a clear characterization of the target of approximation. Frequently, it is asserted that a model is a good one if it approximates the ultimate truth about the world. That ultimate truth, however, is not available to us as a standard against which to judge the approximation. The use of the term, then, is not so much wrong as empty and useless.

One temptation is to change the target from the ultimate truth about the world to the observed data. If data are regarded as facts, transparently true of the world, then a model might be regarded as good if it approximates the data. It would still be false, but it would be "true enough" as a description of the data (Elgin 2004). A theoretical model might, then, explain those facts (e.g., provide reasons why the facts should come out in the way that they do). Such an understanding of the role of approximation promotes the idea that we might have models that become more and more precise until they eventually form the perfect match for their target. This is the lure of the perfect model. Catherine Elgin (2004, p. 116) gives an example of this way of thinking in her discussion of curve smoothing as a kind of fictionalization of the observed data. The problem with Elgin's view is that the target of theoretical models is not, in the first instance, to explain the observed data, but rather to explain the smoothed curves. Curve smoothing is an example of a model of data (see Suppes 1962). The goal of the scientist is more accurately described not as explaining the smooth curve but as reconciling a theoretical model with a model of the data.

Suppes' distinction between theoretical and data models aims to characterize the complexity of empirical modeling in a manner related to Bogen and Woodward's (1988) distinction between *data* and *phenomena* (cf. Duarte and Hoover 2012, pp. 227, 244–246). Early statistical techniques (for example, Gauss's development of ordinary least squares estimators) aimed at isolating phenomena distinct from the data. The point was decidedly not (*pace* Elgin) to fit each observed point, but rather, in Gauss's case, to characterize the shape of an orbit with an empirical relationship that was truer than the ostensibly raw observations. The goal was not only to describe the target more accurately but also more precisely as well.

Bogen and Woodward's distinction between phenomena and data is a relative one, since recorded data are representations, and all representations say less than everything that can be said of their referents. What is more, most data should be seen as the product of a model, even if it is only as simple a model as a classification scheme or the taking of average values (such as price indices as models of the general price level). Some data are much more deeply theoretical than smoothing or averages or classification. For example, the U.S. Congressional Budget Office publishes "data" on potential output and the natural rate of unemployment that are created using a theoretical model – a particular expectations-augmented Phillips curve. These data are taken as a primary input into other empirical economic models and are treated in that context as "raw observations" (cf. Hoover and Juselius 2015, Section 4).

It might seem that the difference between Elgin's "true enough" and my insisting on "true" with an explicit or implicit standard of precision as the appropriate virtues for models is a merely semantic distinction. And, of course, I should acknowledge that Elgin, like Giere, Teller, and others, adopts views that are in large measure compatible with mine in that they oppose the Perfect Model Model or favor the piecemeal acquisition of knowledge. Yet, the distinction is not entirely semantic, as the semantics reveal an apologetic attitude, aimed more at parrying the perceived shortcomings of models than of explaining why they possess their positive virtues.

Some may read the notion of giving up the perfectly precise, perfectly accurate model as a target of scientific investigation in favor of the model that is perfectly accurate for a level of precision that is adequate to a particular purpose as a kind of instrumentalism (Hands 2001, pp. 235–238). I have argued elsewhere that it is a kind of Peircean pragmatism that warrants a perspectival realism (Hoover 2012; see also Giere 2006). The problem with instrumentalism of a kind that requires only, as per van Fraassen (1980), that models save the phenomena or that holds that good models are not true, but they work, is that it makes understanding why they work (or why they sometimes do not work) into a mystery. The kind of realism that I wish to endorse says that models are good and they work when they accurately capture relevant features of the world (e.g., when they accurately articulate causal mechanisms) up to the levels of precision and from the points of view that are relevant for the particular problems that we wish to address.

This view of models suggests a natural answer to the pessimistic induction. When a model that is to some significant degree successful at achieving its purposes is supplanted on the same explanatory domain by apparently radically different models, it poses a scientific problem. The successor model may look radically different because it has adopted a different point of view and takes, in part, different aspects of reality as its target. To the degree that it aims to explain the same aspects of reality, it incurs an obligation to explain the success of the supplanted model, insofar as it was successful. The obligation arises because, if the supplanted model was in fact reliable on such matters, the only non-mysterious account of its reliability was that it actually did characterize some aspects of reality accurately from its point of view.[10]

The Analytical Method[11]

The vision of science as a system of a relatively few, relatively simple universal laws governing large domains of events is hardly adequate to the actual practice of most sciences – certainly not to economics – or to the nature of knowledge that is acquired in bits and pieces. We seem to know a lot, but what we know is clearly massively incomplete. Viewing science in terms of its disparate models rather than in terms of its comprehensive system of laws captures much better the actual practices of economics and of many other sciences. Greater attention needs to be paid to how models function in scientific inference. In line with my declared pragmatism, let me go back to the first pragmatist – Peirce (Hands 2001, p. 218).

Peirce famously rejected the division of inference into deductive and inductive, dividing it instead into deductive and ampliative and subdividing ampliative inference into induction and abduction. What is less well known is that Peirce argued that *analogy* constituted an important fourth form of inference. Analogy, he maintained, was a hybrid of abduction and induction.

The purest form of induction in Peirce's view is simply a kind of counting. Suppose that one wants to know the proportion of black and white beans in a bag. Naturally, one can count them all. Even such a count requires a *frame* (not Peirce's term) that determines how beans are to be classified as black or white, given variations in the colors of individuals. One might also learn the proportion – especially if the numbers are large or indefinite – using more elaborate frames. For example, one might thoroughly mix the beans (a process of *randomization*) and then draw a sample of say a cup of beans. Then classifying and counting the beans in the cup allows one to measure the proportions. The step of asserting that the proportions determined by the count are the proportions of the beans in the whole bag relies vitally on the frame. In this case, the frame consists in the classification standard and the assumption that the mixing in fact achieves randomization. In order to quantify the precision of the induction, additional elements are included in the frame, such as the assumption that the inferential setup is analogous to a particular probability model. It is on this basis that formal statistical procedures permit us to assign standard errors to inductive estimates.

Peirce was impressed that quantitative induction disciplined by a probabilistic frame was self-correcting in the sense that the more samples one drew (with replacement, as we are not considering an exhaustive count), the higher the precision of the measurement of the proportions. In that sense, induction was a self-correcting procedure that was bound to lead to the truth if pursued long enough. The self-correcting nature of induction is obviously conditional on the accuracy of the frame in which the induction is conducted. While Peirce would not reject Hume's objection that induction has no logical warrant if induction is taken to be unconditional, he argues that conditional on the frame, it is a deductive truth that induction has the self-correcting quality. Even some elements of the frame can be examined within the context of the frame itself. Are the samples actually random? Probability theory allows us to test randomness – not absolutely, but up to a level of precision that depends on sample size and other conditions. But again, such tests are conditional on other aspects of the frame itself. Nothing can rule out a radical mistake in the frame: if on the fifteenth sample, we discover a yellow bean, the black/white framing assumption is simply wrong and will have to be replaced.

The idea that there are frames that, while not rendering the induction itself into deductive truth, render its self-correcting properties into conditional truths is the fundamental idea behind diagnostic testing in econometrics and statistics. The coefficient estimates and standard errors of an ordinary-least-squares regression have very nice properties conditional on, for example, regressors being exogenous and errors being identically, independent, random errors. These properties are, themselves, testable within a conditional frame.

Where do frames come from? For Peirce, abduction is the supplier of frames. The abductions take the form:

> The surprising fact, C, is observed;
> But if A were true, C would be a matter of course,
> Hence, there is reason to suspect that A is true.
>
> (Peirce 5.188)

Put this way, abduction has been treated as a species of inference to the best explanation or a Popperian conjecture (Hands 2001, pp. 118–119). But a better interpretation is that an abductive inference amounts to the process of asserting a model, and modeling fits into a larger account of scientific method.

What has come to be thought of as modeling is a good example of what Peirce (1.64) referred to as the "Analytic Method to which modern physics owes all of its triumphs." Peirce also notes that it has "been applied with great success in the psychical sciences. (Thus, the classical political economist,

especially Ricardo, pursued this method" – as, I would argue, they still today. The analytical method is

> to substitute for those problems others much simpler, much more abstrac of which there is a good prospect of finding probable solutions. The the reasonably certain solutions of these last problems will throw a lig more or less clear upon more concrete problems which are in certa respects more interesting.
>
> (Peirce 1.6

In the analytical method, abduction supplies the frame in which inductic does its work.

Abduction operates through analogical relations. Although Peirce does n use the term model, he does note that "analogy" is a translation of Aristotl *paradeigma*, from which the word *paradigm* is derived. Peirce uses the ter in its pre-Kuhnian sense, based on its Greek roots, *para*, meaning "parall or going beyond," according to the *Oxford English Dictionary*, and *deigm* meaning "a sample or pattern." The word itself aptly conveys the strateg of analogical reasoning that Peirce attributed both to physics and to class cal political economy and maps very nicely onto modern practices in whic stripped down or idealized root models are elaborated successively to com closer to empirical observations while maintaining their underlying bas character and tractability.

Peirce clarifies analogy as hybrid of abduction and induction with a extended analysis of Kepler's discovery of his laws of planetary motic (Peirce 1.72–74, especially 1.74). Kepler began with Copernicus's hypothes of the planets in circular orbits around the sun and Tycho Brahe's and h own observations. The analogy was between Kepler's mathematical mod (to use the modern term), with its precise orbits, and the actual observation The analogy was not, at first a good one: the Copernican model fit the da rather badly. Out of keeping with Popper's later methodological pronounce ments, Kepler did not simply scrap Copernicus's model. His procedure w not haphazard, but systematic and conservative, in the sense that, at each ne abductive step, he tried to preserve his quantitative success hitherto – that i to stay within the bounds of error already achieved – and to use the specifi ways in which the hypothesis fell short to suggest the next abductive ste Kepler's own abductive contribution was to consider the dynamical impl cations of the sun, which he knew to be vastly larger than any of the plane and which he conjectured and exercised some vaguely-defined causal pow over them. Alternating abductions to introduce modifications and inductior to characterize the nature and degree of the deviations between conjectur and data, Kepler refined the model:

> never modifying his theory capriciously, but always with a sound an rational motive for just the modification – of most striking simplicit

> and rationality – which exactly satisfies the observations, it stands upon a totally different logical footing from what it would if it had been struck out at random, or the reader knows not how, and had been found to satisfy the observation.
>
> (Peirce 1.74)

The analytical method for Peirce is largely the method of refining and precisifying analogies. The Copernican model relied not on detailed observation but on stylized facts. Kepler modified the analogy but also particularized and added precision to the facts. Economic laws, for Peirce, are truer on average than they are for any individual. Economics is thus the natural landscape for statistical refinement, for inductions that raise the precision of the models (Peirce quoted in Eisele 1979, p. 251).

Analogical reasoning represents a *constructive* interplay of abduction and induction. Any induction presupposes a prior abduction. A surprise or an inconsistency between the induction and the prior abduction provides the impetus for a new abduction. Such an abduction could be wholly novel or it could be, which is vastly more likely to be fruitful, a carefully selected variation on the original abduction.

In starting with a simple hypothesis and successively modifying it within a class of possible hypotheses that are obvious from the start, we are, Peirce believes, engaged in induction, not abduction: "induction adds nothing. At the very most it corrects the value of a ratio or slightly modifies a hypothesis in a way which had already been contemplated as possible" (Peirce 7.217). Peirce's idea seems to be that even a fairly specific hypothesis, such as that the data are quadratic, can be treated not as a precise claim but in a manner close to original meaning of "paradigm," as an instance or exemplar that can serve as an index for wider family of precise hypotheses. Thus, the quadratic hypothesis could be taken to be an index of the whole polynomial family; and, just as enumerative induction of a statistical kind results in a narrowing of the bounds on the value that a true ratio could take, inductions of a nonstatistical kind can be taken as a narrowing of the subset of family members compatible with the truth. The true abduction – the inference that might provide "unexpected additions to our knowledge" – is the replacement of one family of models by another.

The promise of both statistical and nonstatistical induction for Peirce is their property that, if we keep at it, they are bound to reveal the falsity of our hypothesis – even the falseness only beyond some limit of precision – in the fullness of time. There is no certainty of their success even in a negative sense: the falsifying observation may always lie just round the corner. And there is absolutely no certainty in the positive sense: an observation may tell us that we are entertaining entirely the wrong family of hypotheses; it requires luck or a natural affinity for the truth, an affinity which can be grounded only in hope and not in knowledge, to pick the right family (Peirce 1.121, 7.219).

Peirce holds up Ricardo's theory of rent as a prime example of analytical reasoning, not only for economics but also for all science. The manner in which he moves seamlessly in his exposition of the theory from the specific details of rent to the question of tax incidence, which involves demand analysis – quite new since Ricardo – suggests that he regards the theory as a starting point, an initial template, an index of a family of hypotheses whose members differ in their scope and complexity, which is ripe for inductive precisification and which, in the manner of Kepler's introduction of the causal powers of the sun, can be joined to novel abductions to build an empirically grounded economic model of real-world phenomena (Peirce 4.115).[12]

Macroeconometric Models

To take stock: I have argued that the function of models in economics and other sciences is to serve as instruments for telling the truth – that is, to portray the world accurately for a purpose and from a point of view up to an implicit or explicit degree of precision. Models operate through analogy, and analogy is based not on any notion of identity but on a complex of cases, divided into similar aspects (positive analogies), on which the working features of the model are based, dissimilar aspects (negative analogies), which must be bracketed off and ignored, and yet-to-be-classified aspects (neutral analogies), which represent the ground for scientific exploration. The practice of empirical scientific modeling can be seen as an application of Peirce's analytical inference – a progressive interplay of abduction and induction. Abduction supplies paradigmatic models that serve as indices to classes of models and provides the frames in which induction precisifies the models, both systematically selecting particular members out of a contemplated class of models and quantifying parameters of the model at increasing levels of precision.

There are many illustrations of using models in conformity to the analytical method in the history of economics. I will end by examining a little piece of the history of macroeconometric models (see Morgan 1990, ch. 4, Bodkin, Klein, and Marwah 1991). The earliest modern macroeconometric models were first developed in the 1930s by Jan Tinbergen for the Dutch and U.S. economies and were significantly advanced through the work of Lawrence Klein in the late 1940s. Subsequently, increasingly complex macroeconometric models were developed for many countries for use in economic research and for practical economic analysis by governments, central banks, and businesses. One might be tempted to view the increasing complexity of macroeconometric models between the 1930s and the 1970s as the relentless pursuit of the perfect model, but that would, I believe, misunderstand the actual practice of the macroeconometric modelers. Rather than attempt a methodological history of eight decades of macroeconometric modeling, I will focus on one early contribution – the family of three models examined in Klein's monograph *Economic Fluctuations in the United States, 1921–1941* (1950).

A singular feature of Klein's monograph is that, standing near the beginning of the macroeconometric program, it is both a methodological investigation of how to build and use macroeconometric models and an investigation into the nature of the American economy. Some of the issues of interest to the philosopher of science are, as a result, more explicitly brought to the fore than with later macroeconometric models.

Klein presents three models of the U.S. economy: Model II is the least complex, a three-equation model (one behavioral equation and two identities); Model I is a six-equation model (three behavioral relationships and three identities) that supports the estimation of a system of three equations; Model III is a "large," sixteen-equation model (eleven behavioral equations and five identities).[13] None of these models is partial in the sense of being confined to a subpart of the American economy; rather each models the whole economy with different degrees of articulation of important aspects. The situation is not unlike that of a realistic cityscape in which human figures appear in the distance, yet when examined at very closely the figures resolve into a few simple blotches of paint.

To cast this into the framework of Peirce's analytical inference, the fundamental abduction is that the economy can be described in a Keynesian framework of aggregate supply and aggregate demand. Aggregate demand is determined by the decisions of types of agents (consumers, firms, etc.), who must make decisions over the flows of their expenditure (consumption, investment, etc.), as well as decisions over their holdings of stocks of assets (money, bond, shares, real capital, etc.). Aggregate supply of goods and services is determined by production technology and the decisions to supply factors of production (labor and capital, etc.). Prices of goods and financial assets coordinate the whole system, though in a manner that permits some available factors of production to be less than fully employed. This is but a partial accounting of the basics Keynesian framework, but even when more fully specified, it characterizes, from a quantitative, empirical view, an extremely broad class of models that might be instantiated in a variety of ways. To call a model "Keynesian" is rather closer to calling it heliocentric than to calling it Copernican, where that term is taken to imply circular orbits.

Klein does not attempt a comprehensive search over every member of the class of Keynesian models. It is not even clear how such a search could be constructed. Rather he approaches the task much like Kepler in Peirce's characterization by starting with a very simple member of the class. Model II (Appendix 1) can be taken to be Klein's Copernican model – an extremely simply index of the class. It is essentially the textbook "Keynesian cross" model, used since the 1940s to teach undergraduates about the fundamental concepts behind Keynesian economics. As a textbook model, it could easily be thought of as a caricature model along the lines of Gibbard and Varian (1978). Yet, Klein solves out the identities to obtain a single equation, associates each of its theoretical variables with data collected by U.S. government statistics bureaux, and estimates the resulting equation (a "reduced form").

What is to be gained from the estimation – an induction conditional on a frame that, Klein himself regards as too coarse, to serve his ultimate purpose of a policy relevant model?

On the one hand, Klein is applying Simon and Rescher's Empty-World and Prepotence Postulates in assuming that many possible connections either do not exist or are sufficiently weak that, at a loose enough standard of precision, the model may nonetheless accurately characterize the economy. On the other hand, Klein is employing an idea similar to Peirce's that a model too simple to be accurate at a desired level of precision will reveal through the detailed ways in which it deviates from accuracy the direction in which the model would need to be modified (Peirce 7.221). One question that he poses to Model II is whether the analogy with the probability model of independent random errors is a good one. In particular, he looks at test statistics that are meant to be sensitive to the presence of serial correlation in the residuals, which that model presumes to be absent. He deems the model to be inadequate, though the level of precision is not, in fact, very high. For example, he calculates the government expenditure multiplier implied by his estimates and finds it to be implausibly high; but he notes that the confidence interval for the multiplier is very wide. Thus, while the model is not necessarily inaccurate in the sense in which I have defined there term, it also would not be precise enough to support policy analysis, which is Klein's ultimate purpose.

Klein also estimates another model within the class defined by his reduced-form equation. The model is suggested by the large standard error on the estimated parameter α_3 governing the strength of the connection between money and GDP. The imprecise estimate may either be that the effect is weak (lack of prepotence) or that it is nonexistent (empty world). While modern economists have turned significance tests into a nearly mechanical judgment, it is clear from the way in which Klein discusses this parameter estimate that the idea that an analogy is being drawn between the patterns of his data and a specific model of probability implied by the stochastic specification of his model is exactly what lies behind such tests. Given this signal, Klein specifies another model with money omitted ($\alpha_3 = 0$), and he finds that the simpler model is accurate to essentially the same degree of precision as the model with money. Since money appears in the theoretical model only in the consumption function (Equation (3.2.1)), Klein takes the lesson from Model II that more complex models, whatever other role they assign to money, can likely omit it from the consumption function – as, in fact, Models I and III do.

Once money is omitted from the consumption function, Model II is more or less nested in Models I (Appendix 2).[14] Like Model II, Model I is an exploration of a model intentionally simpler than any model that Klein would regard as adequate for his purposes. Its target, however, is still the whole economy. This is evident in the expenditure identities (Equations (3.1.19) and (3.1.20)) that cover GNP just as completely (now from an income, as well as an expenditure point of view), albeit with a finer breakdown into its components than the expenditure identity of Model II (Equation (3.2.2)).

To investigate the adequacy of the statistical model, Klein estimates it using both a full-information maximum likelihood (FIML) estimator and a limited-information maximum likelihood estimator (LIML). These draw somewhat different analogies to the probability model invoked in the statistical design. The advantage of LIML was that it permitted estimates of coefficient standard errors, while, with the statistical technology of 1950 (no longer a constraint today), FIML did not. In effect, while FIML provided quantitatively more precise estimates of the means of the probability distributions, LIML provided a qualitatively more precise estimate, in that it permitted an estimate of two moments of the probability distribution, but at the cost of some quantitative precision. Given the levels of precision, both methods provide accurate estimates of the theoretical model, conditional on both the underlying theoretical model and the probability model invoked by the statistics. In each case, Klein checks the adequacy of the statistical model using the same approach as with Model II.

Klein uses the LIML estimates as the key measure of adequacy of the theoretical model. Again, as with Peirce's Kepler, Klein takes the discrepancies on various dimensions not as rejections, but as invitations to explore the space of the subclass of models in the immediate vicinity of the initial version of Model I.

Models I and II can be seen principally as scouting expeditions. Neither is adequate to Klein's ultimate purpose, which is to construct models in which it would be possible to conduct policy-relevant counterfactual experiments. In part, Klein was simply using these models to learn how to integrate recent developments in econometric theory, guiding the statistical frame with theoretical models. In employing models that were members of the same broad class and were aimed at the same real world target, however, Klein engaged not only in a technical exercise of auto-pedagogic value, he also sought to learn from the inductive step of estimating the models statistically useful directions to develop the model into one that could represent accurately at a useful level of precision the causal relationship of policy instruments (e.g., government expenditure or taxes) to the targets of policy (e.g., GDP growth or labor demand). Model III is his first attempt at such a model. The investigation and development of this more complex model follows a similar strategy to the investigation of the simpler Models I and II. It offers us no particularly new insights into the process.

In the event, Klein did not regard Model III as either sufficiently accurate or sufficiently developed on the necessary dimensions to support the needs of policy. It too was a step in the iterative process of analytical inference (model building) that Peirce locates at the heart of quantitative sciences. The next step was an even larger macroeconometric model, the *Klein-Goldberger Model*, which proved to be the first macroeconometric model of the United States actually used for practical policy analysis (Klein and Goldberger 1955). The Klein-Goldberger model was the ancestor of a several generations of macroeconometric models – some still in use in the U.S. government and the

Federal Reserve system and the inspiration for a number of other models in the United States and many other countries (see Bodkin et al. 1991).

All Useful Models are Essentially True

Much of the attention to models in economics has focused on the activities of economic theorists, as opposed to empirical researchers. Theorists spend a good deal of time elaborating models and specializing and adapting them to particular targets. The goal of the theorist is often to highlight a surprising or overlooked phenomenon or to work out a mechanism that could operate in principle. Such perfectly creditable goals, I believe, account for the popularity of the notion that models in economics are caricatures or devices for exploring credible worlds or fictions of varying degrees of realisticness. Quantitative modeling receives far less attention. In explaining why their focus is on theoretical models as caricatures, Gibbard and Varian (1978, p. 665) "liken econometric models … to photographs." They express the view that the representation of an economic phenomenon might "gradually evolve [from a caricature model] into an econometrically estimable model" – that is, from "a literal caricature" to a "photograph" of the economy (pp. 665, 666). The nature of empirical model building and of the process of evolution is, however, not their topic – it is mentioned merely to be set aside. The invocation of the photographic realism, however, suggests something like the Perfect Model Model.

That actual empirical models fall short of photographic realism probably explains the appeal of ideas such as instrumentalism ("all models are wrong, but some are useful"), and that models are approximations or "true enough." These ideas fail to explain how models work or why they are effective. The best explanation is that models somehow capture truths about the world. This point is implicitly understood by the philosophers and methodologists of purely theoretical models: caricatures contain truths that are exaggerated for emphasis; a credible world is a world that, while not the actual world, contains mechanisms of the actual world; realistic fiction, including realistic fictional models, is realistic because important aspects of their working corresponds to mechanisms in the actual world.

A model that aims to capture empirical truth about the economy at any reasonable level of quantitative precision needs to go further than the theorists. If they are not photographically realistic – and few would assert that they are – if they do not approach the perfect model, how do they succeed? Models work analogically, and analogies can be loose or tight, they can possess different degrees of articulation. Empirical models work when they are based in apt analogies with a degree of articulation adequate to the purposes and required precision of the modeler. When they succeed in this way, they are instruments for telling the truth.

The key point about Peirce's account of analytical inference is that accurate and quantitatively precise analogies must be shaped or molded. When we think of economics as a model-building science, it is precisely this shaping and molding with its interplay of abduction and induction that constitutes the ordinary life of the empirical economist. Importantly, abduction is not a haphazard activity; rather it is a directed activity in which new hypotheses build on the fate of old ones revealed and evaluated in the light of the inductions that the old abductions had framed.

Shaping or molding models seems to contravene methodological strictures to which economists pay frequent lip service. We are often taught that science is a sequence of hypothesize, test, accept or reject, and that it is bad science to tailor hypotheses to the data. Partly, this come from the fact that Popper's methodology of conjectures and refutations remains popular among practicing economists if not among philosophers and methodologists of economics, and Popper was strident about "immunizing strategies." Partly, it comes from an analogy with hypothesis testing in statistics, where it can be shown that certain kinds of specification search results in test statistics whose actual size (probability of type-I error) are much higher than their nominal size. One response is a general prejudice against anything that smacks of "data mining."

Elsewhere, I have answered the objection to data mining through statistical specification search.[15] That answer takes a similar form to the analysis in the current paper. Statistical estimation relies on an analogy between the data packaged in a certain way and a probability model. The first question for any analogy is, is it apt? Are the supposed analogical features actually similar? Estimation is the inductive step in Peirce's analytical inference at the level of the data model. Precision in such an induction is useful only *conditionally* on the analogy in fact being accurate. Specification search (aka data mining) amounts to the effort to mold an accurate analogy. If done right (and that is a big if), specification search adapts to constraints in the world, which produces a model that can be accurately predicated of the data and quantified through induction.

The focus of the current paper was not on data models, but on theoretical models that are quantified through their analogical relations to data models. Klein's macroeconometric models illustrate that the process of molding models sequentially is typical of the way economists actually work. If this contravenes methodological strictures, the account of model building that I am advocating locates the problem, not with the practices of Klein and other economists, but with the methodological strictures themselves.

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Notes

1. There are exceptions: for a penetrating analysis of empirical models, see Boumans (2005).
2. The language of piecemeal knowledge has been adopted, especially by philosophers of biology (e.g., Wimsatt 2007).
3. While this paper was not originally motivated by Reiss's (2012) "explanation paradox," it can be seen as an argument we should dismiss the paradox altogether. Hands (2013, p. 235) summarizes the paradox as "the inconsistency of accepting the three statements: (i) economic models are false, (ii) economic models explain, and (iii) only truth explains." My argument accepts (ii) and (iii) and rejects (i), so that no paradox is in play.
4. Following the custom of Peirce scholarship, references to Peirce's *Collected Papers* (1933–1958) are by volume number and paragraph: e.g., 1.69 = volume 1, paragraph 69. For a brief account of Peirce's pragmatism, see Hands (2001, pp. 218–224) or Hoover (1994).
5. Hands (2001, p. 224) suggests that "abduction is a relatively loose notion of inference," which explains why it has been neglected in the Received View of the philosophy of science, which emphasized rigor and, therefore, deductive and "formulaic" inductive inference. Section IV below gives grounds to question Hands' notion that abduction is a loose form of inference.
6. It is, in fact, not obvious. There are supply-and-demand models in which prices and quantities are not read at the crossing point – e.g., cobweb models of price dynamics.
7. Arrow (1959) poses exactly this question with respect to the supply-and-demand model.
8. Reacting to such guidance results in a process that Boumans (2005, chs. 1 and 3) refers to as "mathematical molding"; see also Hoover (2013).
9. The "socialist calculation debate" of the 1930s hinged on the credibility of two fictional worlds. Both sides referred to a Walrasian general equilibrium model, which like the supply-and-demand model provides no mechanism for actually setting prices, though it shows where they must be set to clear the market. The socialist side suggested that the hole could be credibly filled with a computer, which had not yet been invented. Even now no computer would be adequate to the task. The Austrian, free-market side argued that the market itself was an effective computer.
10. The notion that distinct perspectives permit accurate representations that may used to make true claims and that science should find a way both to incorporate them into common perspectives when they conflict and to account for their successes, even when superseded, raises some of the issues of pluralism versus unificationism in science that are addressed in Hands (2021).
11. This section draws on draft material from a book in progress, joint with James Wible, on Peirce's engagement with economics.
12. Peirce's treatment of Ricardo is deeply consistent with Morgan's (2012, ch. 2) treatment of him, not only as a generic "modeler" but also as one who used the generic model as a template for quantitative refinement.
13. Here the models are discussed in their order of complexity. For reasons related principally to his investigation of estimation methods, Klein considers the simpler Model II after the more complex Model I.
14. "More or less" because variables in Model II, unlike Model I, are expressed in per-capita terms. Both Model I and Model III, which also does not use per-capita variables, aim to capture some of the same features through time trends.
15. See Hoover (1995, 2013) and Hoover and Perez (1999, 2000, 2004).

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Appendix 1: Klein's Model II

Structural Model

$$\frac{C}{pN} = \alpha_0 + \alpha_1 \frac{Y}{pN} + \alpha_2 \left(\frac{Y}{pN}\right)_{-1} + \alpha_3 \left(\frac{M}{pN}\right)_{-1} + u \qquad (3.2.1)^1$$

$$GNP = C + I' + G \qquad (3.2.2)$$

$$GNP = Y + T \qquad (3.2.3)$$

Reduced Form

$$\frac{Y}{pN} = \frac{\alpha_0}{1-\alpha_1} + \frac{\alpha_2}{1-\alpha_1}\left(\frac{Y}{pN}\right)_{-1} + \frac{\alpha_3}{1-\alpha_1}\left(\frac{M}{pN}\right)_{-1} + \frac{1}{1-\alpha_1}\left(\frac{I'+G-T}{pN}\right) + \frac{1}{1-\alpha_1}u \qquad (3.2.5)$$

Reduced-Form Estimates

Estimates based on annual data for the United States 1921–1941.

$$\frac{Y}{pN} = 186.53 + \underset{(0.13)}{0.30}\left(\frac{Y}{pN}\right)_{-1} + \underset{(0.10)}{0.13}\left(\frac{M}{pN}\right)_{-1} + \underset{(0.35)}{2.36}\left(\frac{I'+G-T}{pN}\right) + u' \qquad (3.2.1)$$

(Standard errors in parentheses below coefficient estimates.)

Variables

Current Dollar Variables:

C = consumption;

Y = disposable income;

M = money supply;

I' = gross investment;

G = government expenditure;

GNP = gross national product;

T = government receipts.

Other Variables:

p = cost-of-living index;

N = population;

u = structural random disturbance

u' = reduced-form disturbance.

Statistics:

$\bar{S}$ = mean squared error of regression;

$\left(\frac{\delta^2}{S^2}\right)$ = measure of serial correlation for jth equation; 5 percent acceptance region for null of no serial correlation = 1.25–3.00.

Appendix 2 Klein's Model I

Structural Model

$$C = \alpha_0 + \alpha_1(W_1 + W_2) + \alpha_2\Pi + u_1 \qquad (3.1.16)$$

$$I = \beta_0 + \beta_1\Pi + \beta_2\Pi_{-1} + \beta_3 K_{-1} + u_2 \qquad (3.1.17)$$

$$W_1 = \gamma_0 + \gamma_1(Y + T - W_2) + \gamma_2(Y + T - W_2)_{-1} + \gamma_3 t + u_3 \qquad (3.1.18)$$

$$Y + T - W_2 \qquad (3.1.19)$$

$$Y = W_1 + W_1 + \Pi \qquad (3.1.20)$$

$$\Delta K = I \qquad (3.1.21)$$

Structural Estimates (LIML)

Estimates based on annual data for the United States 1921–1941.

$$C = 17.71 + \underset{(0.04)}{0.87}(W_1 + W_2) + \underset{(0.07)}{0.02}\Pi + u_1'' \qquad (3.1.31)$$

$$I = 22.59 + \underset{(0.23)}{0.08}\Pi + \underset{(0.21)}{0.68}\Pi_{-1} - \underset{(0.04)}{0.17}K_{-1} + u_2'' \quad (3.1.32)$$

$$W_1 = 1.53 + \underset{(0.03)}{0.43}(Y + T - W_2) + \underset{(0.03)}{0.15}(Y + T - W_2)_{-1} + \underset{(0.02)}{0.13}(t - 1931) + u_3'' \quad (3.1.33)$$

(Standard errors in parentheses below coefficient estimates.)

$$\bar{S}_1 = 1.30 \quad \bar{S}_2 = 1.43 \quad \bar{S}_3 = 0.77 \quad (3.1.34)$$

$$\left(\frac{\delta^2}{S^2}\right)_1 = 0.98 \left(\frac{\delta^2}{S^2}\right)_2 = 2.18 \left(\frac{\delta^2}{S^2}\right)_3 = 2.10 \quad (3.1.35)$$

Variables

Constant Dollar Variables:

C = consumption;

G = government expenditure;

I = gross investment;

K = capital stock;

W_1 = private wage bill;

W_2 = government wage bill;

Y = gross national product income;

Π = profits.

Other Variables:

t = time trend;

u_j'' = random disturbance (j = 1, 2, 3).

Statistics:

$\bar{S}_j$ = mean squared error of regression for jth equation;

$\left(\frac{\delta^2}{S^2}\right)_j$ = measure of serial correlation for jth equation; 5 percent acceptance region for null of no serial correlation = 1.25–3.00.

Note

1. Equation numbers refer to the numbers in Klein's original text.

8 Institutional Economics and John Dewey's Instrumentalism

Malcolm Rutherford

Introduction

As noted by Wade Hands "it is almost a cliché that any discussion of American institutionalism must include a reference to the 'impact' of pragmatic philosophy," but, with the exception of the very clear and obvious relationship between John Dewey and Clarence Ayres, Hands seems to regard most of the proposed connections between institutionalism and pragmatism to be "rather controversial" (Hands 2001, p. 231). The argument to be made here is that connections between institutionalism and pragmatism, and especially of John Dewey's pragmatic instrumentalism, during the period when both were at their peak, are not hard to see. This is less a matter of explicit references to Charles Peirce or Dewey, or to any writings in philosophy, but more a matter of the practice of institutional economists. Institutional economics in the interwar period can be seen as an attempt to carry out a research program along instrumentalist lines.

It must be said that Veblen is something of an exception to this characterization. Veblen's work does share a number of characteristics with Peirce and Dewey, but his basic view of science was not instrumental, so several aspects of the instrumental approach common to other institutionalists are not evident in Veblen's writings. This is a key factor in Veblen's relative lack of interest in practical reform efforts. Veblen did not subscribe to the enthusiasm for "social control" that was shared by Dewey and virtually all other institutionalists, as his view was that significant institutional change could only come from evolutionary processes that altered the "discipline" of life. A different issue concerns Clarence Ayres. Ayres clearly had a close relationship with Dewey and was strongly influenced by his ideas, but Ayres' training was in philosophy not economics, and his major works focused on questions of valuation and the definition of "progress" (Ayres 1944). What I want to focus on here is the *economic* research done by institutionalists, and Ayres did very little economics of the type done by other institutionalists.

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