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ECONOMETRICS AND REALITY

Kevin D. Hoover

Working Paper #97-28

Department
of Economics

Econometrics and Reality

Kevin D. Hoover

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This paper was prepared for the conference "Fact or Fiction? Perspectives on Realism and Economics" in honor of Uskali Maki's inaugural at the Erasmus Institute for Philosophy and Economics, 14-15 November 1997. It owes its existence to the conjunction of several events. It is largely a reaction to the work of Tony Lawson and Nancy Cartwright, which I have followed with great interest for many years. The immediate stimulus was the publication of Lawson's *Economics and Reality* and the two lectures, "Where Do Laws of Nature Come From?" and "The Rule of Physics/The Rule of Economics: Empire or Alliance?," delivered by Cartwright on 10 April 1997 in the University of California, Davis. I had for some time believed that Lawson was wrong to see econometrics and critical realism were incompatible. When I made this point to Jochen Runde at the Vancouver meetings of the History of Economics Society in 1996, he urged me to write my thoughts down on the grounds that "Tony likes a good argument." I thank him for his encouragement. And I thank Uskali Maki for having early on stimulated my thinking about realism and, more practically, for providing the venue and the deadline that ensured that this paper would be written.

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Abstract

Starting with a realist ontology the economic methodologist, Tony Lawson, argues that econometrics is a failed project. Apparently more sympathetic to econometrics, the philosopher of science, Nancy Cartwright, again from a realist perspective, nonetheless argues for conditions of applicability that are so stringent that she must seriously doubt the usefulness of econometrics. In this paper, I reconsider Lawson's and Cartwright's analyses and argue that realism supports rather than undermines econometrics properly interpreted and executed.

Econometrics and Reality

Is econometrics possible? The question reminds me of an argument between two divines at an ecumenical conference. BAPTIST MINISTER: "I don't believe in infant baptism." ROMAN CATHOLIC ARCHBISHOP: "Not only do I believe in infant baptism, I have seen it done." Every applied economist has seen it done; yet Tony Lawson in his recent book, *Economics and Reality* (1997), assures us that, splash the econometric holy water as you may, economic heaven will not be a whit closer. The metaphor is perfectly apt; for Lawson's reason for dismissing econometrics is metaphysical: transcendental realism provides an accurate ontology of the economic world; econometrics is necessarily incompatible with transcendental realism. Nancy Cartwright, who is also a realist, appears more favorably disposed to econometrics. After all, in her ***Nature's Capacities and Their Measurement*** (1989), she stokes the self-esteem of economists by suggesting that quantum physicists might learn a thing or two about handling probabilities from econometricians. Beneath a cheery exterior, her views are in fact stunningly pessimistic: the conditions in which econometric methods can succeed are strict; they may be met in the land of quantum physics, but never in their country of origin. Against both Lawson and Cartwright, I maintain that econometrics and realism are compatible and, indeed, that realism helps us better to understand the role and successes of econometrics.

I. Realism

According to Uskali Mäki (1996) ***ontologrcal realism*** is the doctrine that entities beyond the realm of the commonsense exist externally (i.e., independently of any individual human mind) and objectively (i.e., unconstituted by our representations of them). Scientific realism is opposed to Humean empiricism (or as Lawson, following

Bhaskar, terms it "empirical realism"). Empiricism maintains that reality consists of "objects of experience constituting atomistic events" (Lawson 1997, p. 19). For Hume, one billiard ball strikes another, and the other moves. There is no more to the story than what our own minds supply. Lawson (1997, part I) adopts the transcendental realism of Roy Bhaskar (1975). Bhaskar (1975, ch. 1) proposes a stratified ontology of mechanisms, events, and experiences. Experiences are people's direct (subjective) perceptions of events. Events are the realizations of mechanisms. And mechanisms are the underlying, not directly accessible structures whose complex interactions determine which events are actually realized. It is the reality of such mechanisms, of their powers, dispositions, capacities, efficacy, anti necessary connections, that Humean empiricism denies (or, at least, denies us the the knowledge of).

Bhaskar offers a transcendental argument in favor of the reality of the stratum of mechanism and, hence, terms his metaphysics ***transcendental realism.*** Cartwright (1994, p. 279), like Bhaskar, develops a similar argument based on Kant's classic "puzzling" form. Let X be features of the world we are loth to deny; let Φ be an "abstruse philosophical position." The argument then runs: without Φ, X would be inconceivable; hence X. Both Bhaskar and Cartwright use Kant's transcendental argument to move from the accepted success of experimental sciences to the reality of a realm of powers, capacities, dispositions, or meclhanisms. Cartwright, in fact, goes somewhat further, including in her X "the possibility of planning, prediction, manipulation, control and policy setting."

Transcendental realism gets started with the observation that regularities of the sort contemplated by Hume are: relatively hard to find and must be created

experimentally, and experiments are manipulations that presuppose that the things manipulated act in reliable ways. For Bhaskar, experiments are about making things happen in regular relationships that they do not normally show in the world. This requires intervention and manipulation. To show that a feather and a stone fall from the same height in the same time, to exemplify Newton's law of gravity, requires experimental control – removing the air from the space through which they fall. For such an experimental procedure to make sense the feather and the stone must have the disposition to fall according to a similar rule, but quite different dispositions to react to air resistance; the scientist must be able to reliably remove the air from the chamber in which they fall (i.e., the machine must be (disposedto act as a vacuum pump and to respond to his initiation); and so forth. A world of dispositions or powers is presupposed, a world in which such dispositions and powers mix in varying ways, an ***open*** world in which a scientist may intervene to create *closure* to isolate and separate enduring capacities or dispositions that are not normally distinct Humean empirical regularities. Some closures may be spontaneous (the solar system, for example, which runs according to nearly Humean regularity without controls). Most closures are the product of experimental intervention.

The necessity of closure for the appearance of regularity is nearly tautological in Bhaskar's account: closed systems are defined as "systems in which constant conjunctions occur" (Bhaskar 1975. p. 33; cf. Lawson 1997, p. 19).

Although Bhaskar in *The **Possibility of Naturalism*** (1979) goes out of his way to defend the view that essentially the same analysis that he applies to physical sciences can also be applied to social sciences, Lawson denies that an analogous transcendental

argument can be applied directly to "mainstream" economics. The argument cannot get started, Lawson (1997, p. 56) believes, because the social sciences do not have the demonstrated success needed as a premise of the transcendental argument. With respect to "mainstream" economics, Lawson regards the lack of success simply to be a fact. But that fact is partly explained by the nature of social sciences generally., It is not usually possible to create experimental ,closuresof social situations. There are practical difficulties: society is too complex and heterogeneous; there are legal and ethical barriers. And there is human agency: planets, atcms, and rocks do not make choices; people do. Lawson's strategy is not to start with the practice of economics, as Bhaskar started with the practice of natural sciences, but to use the insights of Bhaskar's ontology as the basis for a methodological reform of economics. Transcendental realism is transcribed into a new mode: ***critical realism***. Whereas for physical sciences, their success was an explanatory premise, for social sciences – particularly for economics – its failures are the explanandum

II. What's Wrong with Econometrics

Lawson: ***The Search for Covering Laws and the Failure of Closure***

Lawson characterizes "mainstream" economics as engaged in a search for covering laws – universal regularities that connect observable events "[E]conometricians concern themselves with attempting to determine constant event conjunctions . . . of a probabilistic sort" (Lawson 1997, p. 69). Econometrics is an example of ***regularity stochasticism:***

> for every (measurable) economic event or state of affairs y there exists a set of conditions or events, etc., $x_1, x_2, \ldots x_n$ say, such that y and $x_1, x_2, \ldots x_n$ are regularly conjoined under some (set of) "well-behaved probabilistic formulation(s). In other words, ***stochastic closures*** are everywhere assumed to

> hold; for any (measurable) economic event y a stable and recoverable relationship between a set of conditions $x_1, x_2, \ldots x_n$ and the average or expected value of y (conditional upon $x_1, x_2, \ldots x_n$) or some such, is postulated. [Lawson 1997, p. 76]

The axioms and assumptions of economic theory are similarly regarded as claims about constant conjunctions. Theoretical explanation in economic theory is based on deductions from covering laws, "constant conjunctions of the form 'whenever event *x* then event y','' and initial or boundary conditions (Lawson 1997, p. 91). The theorist subscribes to a "degenerate" form of regularity stochasticism – *regularity determinism* (Lawson 1997, p. 98).

Lawson's argument against econometrics starts with claim that there are no laws in economics (Lawson 1997, p. 70). The Lucas critique provides an explanation of this fact from within economics itself.[1] Lucas (1976) argued that large-scale econometric models, and more generally, empirically estimated aggregate relationships are likely to be unstable because they reflect the decisions made by economic agents in particular policy environments and those policy environments change. For example, the expectations-augmented Phillips curve, which relates the unemployment rate to rate inflation would not remain stable if there were a change in monetary policy. Agents would integrate their knowledge of how policy was conducted into their expectations of future policy outcomes (e.g., knowledge of the central bank's money supply rule into knowledge of the future path of the money supply), changing the phenomenal relationship of unemployment to inflation with each shift in policy regime. Lawson (1997, pp. 71-75) regards the Lucas critique as sound as far as it goes. But he stigmatizes responses such as

[1] The acclaim of the Lucas critique among economists overshadows the fact that there is relatively little empirical support for its importance, which undercuts Lawson's thesis; see Ericsson and Irons (1995).

Sargent's program of providing microfoundationally based econometrics as unable to succeed because the Lucas critique is merely a symptom of a larger failure of macroeconomic systems to achieve intrinsic or extrinsic closure.

In physical sciences, regularity stochasticism fails when there is insufficient shielding to isolate a system From conflating causes. In macroeconomics, regularity stochasticism fails for at least two additional reasons. First, a microfoundational approach such as Sargent's fails because the technical conditions needed for aggregation of individual behavior to well-ordered relations among aggregates are not met (Lawson 1997, pp. 80-91). Second, an econometric model can fulfill the conditions of regularity stochasticism only if it can limit the range of agent's behavior is definite conditions to "only one outcome or 'exit'" (Lawson 1997, p. 79). But this is a misrepresentation of the intrinsic openness of economic systems in which agents can genuinely choose and in which, therefore, their behavior cannot be governed by a predictive law.

Lawson's argument against econometrics stands or falls with his characterization of the discipline as principally concerned with establishing covering laws. To establish this Lawson (1997) quotes on behalf of economic theory From Frank Hahn (e.g., p. 18) and on behalf of econometricians from David Hendry (p. 301). Setting aside the (fairly serious) issue of whether these economists – eminent as they are – are actually representative of their subdisciplines, we nevertheless should heed D.H. Lawrence's dictum to trust the tale, not the teller. When we examine the practice of economists, Lawson's characterization of the discipline: as searching for covering laws seems misconceived.

The concerns that animated Lucas (1976) are old. He acknowledges some precursors, such as Tinbergen, and overlooks others, such as Haavelmo, but makes no claims for originality. Reading the: early contributions to econometrics collected in Hendry and Morgan (1995) one cannot but be struck with the clearness with which early econometricians understood the difficulties of isolating particular causal relationships from the dense webs of economic influences and of accounting for the ever changing institutions and background conditions. It is inconceivable that one could believe these econometricians to be, or that they could have believed themselves to be, in search of covering laws, if by that one means in search of invariant event regularities that remain constant over time and against all changing background conditions. Yes, they sought robust regularities, but they expected neither the precision nor the freedom from context and contingency that is implied in Lawson's covering-law characterization.

Nor has this changed recently. Sargent (e.g., Hansen and Sargent 1980) seeks to resolve the Lucas critique by actually modeling the processes of expectation formation (in Lawson's language to achieve closure). The intent (whether it is successful is another question) is to rock-bottom tastes and technology, the so-called "deep parameters," utility functions (representing people's preferences), budget constraints, and production functions (presenting engineering relations). I have argued elsewhere (Hoover 1988, 1992) that this program is not likely to succeed. But the point now is just that these components do not look like covering laws. A utility function is not an empirical relationship at the level of events. It is more like a power or capacity; it is a description of the disposition of an agent to act, provided that a situation is constructed around him (that is the role of the budget constraint) that permits him to realize his disposition.

Lucas (1987) and Kydland and Prescott (1991, 1996) are sceptical Sargent's program. They prefer to construct theoretical models that are "calibrated" to yield empirical content, yet are not estimated econometrically (see Hoover 1995 and Hartley, Hoover and Salyer, 1997, 1998). But this is an in-house dispute with Sargent over realisticness (to use Mäki's term) or idealization or deducibility. The conception of models as being built up out of components that are not covering laws but more like powers or capacities is shared by all the disputants. Seen this way, Lucas and company are much closer to transcendental realism than to positivism.[2]

Nancy Cartwright: The Difficulty of Constructing Closures

Cartwright is more sympathetic to "mainstream" economics, though her ontology is similar to Lawson's and Bhaskar's. Cartwright (1983) maintained that the "laws of physics lie" in the sense that they are never instantiated without substantial human interference in the form of experimental controls and substantial interpretive license. More recently, Cartwright (1994, 1995, *MBL*[3]) has refined her objection to laws: it is not so much that they lie, but that they are properties of highly particular organizations of things and are only known to be true within the limited domains in which we have instantiated them. She stigmatizes the claims that laws are universal as "fundamentalist" and describes her own views as "local realism" (Cartwright 1994). This distinguishes her

[2] Lawson's central evidence for the claim that economic theory subscribes to the covering-law model is found in two quotations from Hahn (Lawson 1997, p. 92). These are a weak reed. One contrasts the "complex, institution and history dependent 'facts' of the econometrician" with deeper regularities. The other, claims that the axioms of theory are not arbitrary but widely agreed characterizations with empirical content. The first quotation could easily be read as consistent with transcendental realism, and the second falls far short of endorsing a covering-law account. Rosenberg (1992, pp. 24-25) argues that economics has been less affected by positivism than any other social science. Hausman (1992) offers a non-positivist interpretation of microeconomic theory.

[3] "*MBL*" stands for "Models: The Blueprints for Laws," undated typescript.

somewhat from Lawson (1997, p. 23) who believes that the actions of mechanisms are not subject to *ceterisparibus* conditions, but are universal, even though rarely observed acting in isolation.

Like Bhaskar and Lawson, Cartwright believes that to observe a law directly is possible only in contrived circumstances – e.g., in experiments shielded from outside interferences. In such "set-ups," what empirical regularities result depend upon the interactions of the capacities of the components. A law of physics such as F = ma really describes such a capacity. It operates for all forces (F), masses (m), and accelerations (a) so long as those terms can be meaningfully applied. It interacts with other such capacities and is not guaranteed to hold outside its local domain.

Cartwright (MBL) calls the set-ups, the "highly structured arrangements" that generate reliable empirical regularities, *notnological machines.* The solar system (an example also favored by Bhaskar and Lawson) is an unusual naturally occurring nomological machine. A more typical example is a particular harmonic oscillator: say, a spring and a weight. A model, in Cartwright's account, is a blueprint for a nomological machine. It tells us how to construct them out of parts with the necessary capacities. To provide a model is at once to establish or to widen the domain for the underlying capacity and to provide a recipe for producing an empirical regularity. The capacities of a harmonic oscillator are described by a formula: $A\frac{d^2z}{dt^2} + Bz = 0$. Models connect this formula to physical set-ups. Both the spring and weight and a coil and capacitor can be configured into analogous oscillators of different physical forms but conforming to the formula. In the one case, A, B, and z are interpreted as mass, distance, and force constant;

and, in the other, as inductance.,(inverse) capacitance, and charge.[4] "[O]nce you call something a harmonic oscillator, then mechanics [or electronics] can get a grip on it . . ." (Cartwright MBL).

The central message of *Nature's* ***Capacities and Their Measurement*** *(1989)* was that structural econometrics in the Cowles Commission tradition, the estimation of identified systems of causally interpretable equations, was possible only with strong (singular) causal assumptions. Though she did not use this terminology then, the central claim can be put in other words: to measure causal probabilities in an econometric system, the nomological machine must be completely articulated. Cartwright's interest in econometrics in *Nature's* ***Capacities*** was instrumental. She looked to it for a logical or methodological lesson for quantum physics. For that purpose it did not matter whether econometrics was actually successful in its; own domain.

Cartwright (*MBL*) illustrates the kind of specific causal knowledge needed "to get causes from probabilities" with a schematic design (a "blueprint") for a machine that delivers a particular set of stochastic regularities, ones in which a true cause lowers the conditional probability of the effect. The (details of the machine, originally designed by Towfic Shomar, are not important, except to note that they are quite specific and as straightforwardly instantiated as the recipe for *Pfeffernüsse* in ***The Joy*** of *Cooking*. Instead of pepper, molasses, flour and butter, Shamor uses chambers, a source of alpha particles, a proton, and magnetic fields. To understand or to explain the probabilistic behavior of the proton in Shamor's machine is, according to Cartwright, to be able to model the nomological machine and to measure the relevant probabilities. This synthetic

[4] **See, for example.** Halliday **and Resnick** (1962, **pp. 855-856) which spells out the formal analogy in detail**

understanding is not, however, all that there is to understanding The probabilistic behavior of Shamor's machine is driven by a radioactive source with well-behaved probabilities of emission of alpha particles My understanding is that, while quantum mechanics provides some further synthetic explanation analogous to that provided by Shamor's machine, we need not go too far down before we reach Hume's rock-bottom in which we cannot say why, but merely that, a regularity occurs This too is knowledge.

The story of Shamor's machine reinforces the theme begun in ***Nature's Capacities***: regular probabilistic behavior is a property that is assured only in tightly controlled set-ups Only if one were to repeat Shamor's set-up with considerable precision could one expect Shamor's probabilistitic relations to recur Haavelmo's (1943) well known example of the regular relationship between throttle setting and the speed of a car is similar The relationship can be reproduced only with the same make of car in very similar circumstances (air temperature, humidity, road surface, and so forth) Cartwright (BML) provides an example of a nomological machine in economics in the guise of Pissarides's (1992) model of the persistence of unemployment We shall consider Pissarides's model presently In the meantime the important point is that Cartwright shows that, to pin down probabilities for observables in the model, things "must be engineered just so" She proceeds (BML, Table 3) to list sixteen particular assumptions needed to make the model deliver its results

Despite offering the example of a nomological machine in economics, Cartwright's pessimism about econometrics is implicit in the example The assumptions of the Pissarides's model are too particular and too implausible to be fulfilled, so that, while econometrics seems possible in principle, it is hard to imagine how it could

possibly succeed practically. Once the image of nomological machines is firmly rooted, it is hard to imagine them in the economy, and yet harder to imagine that two economies might possess nomological machines of the same make and model. In a public lecture, Cartwright (*WDL*[5]) was able to illicit gales; of laughter from the audience (largely of philosophers) simply by describing (with hardly a raised eyebrow and certainly no smirk) economists trying to determine the effect of educational expenditure on living standards in Sri Lanka by treating Sri Lanka and other countries as draws from a stable regular association common to them all.[6] If there are nomological machines in economics, ***that*** surely is not one.

Anand and Kanbur (1995, p. 321) exposit a number of related studies with

> the following model [in which] some measure of living standard, H_{it} for country i and time t:
>
> $$H_{it} = \alpha_t + \beta Y_{it} + \delta E_{it} + \lambda_i + u_{it},$$
>
> where Y_{it} is per capita income; E_{it} is social welfare expenditure; α_t is a time-specific but country-invariant effect assumed to reflect technological advances (e.g. disease eradication techniques); λ_i is a country-specific and time-invariant 'fixed effect'; δ is the marginal impact of social expenditure on living standards; and u_{it} is a random error term.

The object of these studies is to measure δ or, at least, to better understand the relationship between E and H. I am too ignorant of this literature to pass any judgement upon it in economic or econometric terms. The important point here is that it is characteristic of large areas of empirical economics; so, if Cartwright can dismiss these

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[5] "*WDL*" stands for "Where Do Laws of Nature Come From?." a lecture by Nancy Cartwright in the Philosophy Department of the University of California, Davis. References are all to my personal notes of the lecture.

[6] My notes leave me in considerable doubt whether I have identified the particular study that Cartwright had in mind. My best guess is Anand and Kanbur (1995), which surveys and criticizes cross-country studies of the relationship of social welfare expenditure to standards of living and offers an alternative analysis of Sri Lankan time-series data.

studies on the basis of a prior understanding of the requirements of local realism, that is a pessimistic conclusion indeed.

III. The Possibility of Econometrics

The Second Transcendental Argument

Cartwright's pessimistic conclusion is not, I think, an implication of her realism; it is hardly compatible with it. Cartwright argues for the realism of mechanisms and their component capacities from the essential role they play in making the practice of experimentation possible. While Cartwright correctly stresses the stringent conditions often necessary to establish stable regularities in an experimental context, perhaps too much of her focus is on the mechanisms that those regularities instantiate. Bhaskar's and Lawson's (1997, p. 25) argument for a world of structured, intransitive objects emphasizes the same point. Yet Cartwright draws a second conclusion from the success of experimentation. Experiments must be structured and shielded. To engineer and construct them, we must be able to control and manipulate things and environments. In so doing, we rely on prior knowledge, some of which is no doubt theoretically warranted and precise, but much of which comprises imprecise facts of whose domain we have only limited knowledge. The transcendental argument is straightforward: "If I do not know these things, what do I know and how can I come to know anything?" (Cartwright 1994, p. 280).

Bhaskar and Lawson conclude that the world must be "open" if experiments are to make sense. The second transcendental argument establishes that there also must be a useful number of reliable and manipulable pre-theoretical regularities if scientists are to be able to create closures in an open world. Lawson (1997, p. 27) writes: "outside of

astronomy at least, most of the constant-event conjunctions which are held to be significant in science, in fact occur *only* under the restricted conditions of experimental control." That could only be true if "constant conjunction" means exceptionless conjunction on a universal domain. True, the path of a falling leaf (Lawson's example) or of a $1000 bill blowing through St. Stephen's Square (Neurath's example cited by Cartwright (1994, p. 283)) is hard to predict. Still, though we know exceptions, the general rule "if I drop it, it falls" is reliable and essential to our making our way through the world. A coin dropped from an upper story window exemplifies that regularity, while the coin that returns to one's hand after being cast over the cliff at Blowing Rock, North Carolina is the exception that proves;the rule. We adapt our expectations for the coin to the altered circumstances fairly easily. Nor is it just a matter of some regularities being overwhelmed with exceptions, while others have fewer. Some regularities might be perfectly exceptionless but imprecise. An iron ball dropped from Carfax tower will land in Oxford city center; a balsa wood glider in Oxfordshire; a helium ballon on Earth.

Openness is relative. Uncontrolled.,non-experimental situations, not just in astronomy, may be closed enough to deliver regularities of varying degrees of precision and reliability. Conversely, no experiment is perfectly immune to outside influences. Closure too is relative. And, in large measure, closure is secured using regularities readily to hand in the world as instruments.

The Success of Economics

There are social regularities as we'll. These are not all the result of laws or institutional restrictions or conventions, although people are seldom found outside such contexts. That I can predict the route my daughter will take home from school, even

though there are many possible routes, accurately enough to meet her mid-way, even though we had no prior coordination, is based on a social (or at least human) regularity. It does not call her agency into question; she genuinely chooses her own route home. Making our way in the world requires us to exploit many social regularities: traffic is heavy from 4 to 6 PM; a store manager will tolerate verbal aggression up to a point; a punk on the streets will tolerate much less verbal aggression; money will purchase goods; people will understand me when I talk. These examples are "micro" (except perhaps the traffic example). Yet, reliable social regularities may also be "macro" or at least aggregate: electricity usage in California is higher in January and August than in April or October; the crime rate is higher when the proportion of men 16-24 years old to the whole population is higher. Again, the reliability of such regularities is no threat to agency. I must decide when to turn my air conditioner on; I have a choice. Pacific Gas and Electric Company would have a difficult time modeling my individual choice in a precise way, yet it may be able to predict its aggregate load rather accurately.

Lawson overstates the failures of econometrics – or at least of empirical economics.[7] As in physical sciences, what are in practice referred to as "laws;' are a hodge-podge of summary statements of differing epistemological status. Some are axioms, some analytical truths.,some heuristic rules; but some are also empirical generalizations. Although one can posit models in which they are deductive

[7] It is unclear how Lawson would distinguish the two. He says (1997, p. 69) that he does not question the use of means, growth rates, or other summary statistics which are legitimate where feasible. A substantial part of my argument below is that much of econometrics is in fact more sophisticated versions of these "legitimate" activities and investigation into the conditions of their "feasibility." One strategy open to Lawson would be to define econometrics as the search for constant conjunctions so that it necessarily fails if there are no such constant conjunctions, but this would do little justice to the reality of econometrics as it is practiced.

consequences, the law of demand, Engel's law, Okun's law, Gresham's law are, first and foremost, robust empirical generalizations. They are like the regularity of the balsa wood glider thrown from Carfax Tovver landing in Oxfordshire: robust because imprecise. Lawson (1997, p. 70 and fn. 3, p. 301) cites the well-documented inaccuracies of economic forecasts. But econometrically-based forecasts, however, inaccurate are better than non-econometric forecasts. And, what is more, there is a difference between a relationship holding in different times, the sense of robustness that is analogous to constant conjunction, and a relationship being useful to connect the present to the future, as in an unconditional forecast. The theory of efficient markets predicts (in the sense of asserting the robust relationship to hold in different times and places) that the price of publicly traded shares is unpredictable (in the sense of tomorrow's value being foreseeable today). Robust, but imprecise:, relationships are routinely made more locally precise. This is what Pacific, Gas and Electric does when it estimates electricity demand on the basis of temperature, time of' day, price, and other variables. The relationships are well known qualitatively, but its business decisions require more quantitatively precise information. They do not regard it as a threat to those decisions if the precise relationship they estimate for California in 1997 is not the same as for California in 1957 or for Holland in 1997. Academic economists too easily forget that business and government empioys large numbers of their peers in part because of the practical and monetary value that they correctly assign to their quantitative conclusions (also see Hoover 1995).

The Role of Experiments

If Lawson exaggerates the failures of empirical economics, Cartwright

exaggerates the centrality of experirnents. Cartwright takes an engineer's view: the problem is how to construct nomological machines or experiments. Her focus is on a certain sort of knowledge. Other sorts of knowledge should not be neglected. There are observational sciences as well as experimental or analytical sciences. The balance between observation and analysis corresponds roughly to the ease of experimentation. There is a growing field of experimental economics; nevertheless, economics is more like meteorology, vulcanology, or epidemiology than it is like physics or chemistry. The problem in these sciences is less how to ***construct*** nomological machines than how to infer what nomological machines in fact operate (i.e., what the principles of their operation (their capacities) are) on the basis of feasible observations.

The difference is illustrated by an exercise set to some college electronics classes. One learns the natures or powers of electronic components by learning Ohm's law, Kirchoff's law, and so forth, and by learning the ways in which particular resistors, capacitors, transistors, and other components exemplify these natures or powers (that is, one learns their capacities). This knowledge permits the student to construct radios, oscillators, and other nomological machines. In the exercise, the situation is flipped around. A particular circuit is embedded in epoxy, so that only its various terminii are exposed. Using measuring instruments, the student must infer the structure of the internal circuit. The student is aided by the fact that he has a pretty good idea of the menu of capacities: he knows Ohm's law and how particular resistors might embody it. It would be a harder case, if he had to both infer the structure and characterize the capacities simultaneously.

Economics presents us with this harder case. To be sure, John Stuart Mill and Austrian economists generally have argued that economics is a compositive discipline in which components with well-understood capacities are combined (see Hausman 1992). One might see the elements of neoclassical heuristics, such as utility functions and budget constraints, as the analogues of resistors and capacitors. Unfortunately, their capacities are generic, unlike the capacity of, say, U^{235} to emit alpha particles, suitable only for rough sketches of nomological machines – not for blueprints.

IV. An Illustrative Case

Pissarides's Machine

Econometrics is possible and compatible with realism because the argument for realism implies the existence of robust regularities. Econometrics aims to characterize those regularities. It seems to be an impossible discipline only when we either think of it as aiming to quantify covering laws or to characterize directly the performance of nomological machines. The issues can be illustrated with Cartwright's (MBL) example of a nomological machine in economics: P'issarides's (1992) model of the persistence of unemployment.

Cartwright (BML) makes the same point about Pissarides's model as she does about Shamor's proton machine: "it takes hyperfine-tuning . . . to get a probability." In the case of Shamor's machine, this hyperfine-tuning took the form of choosing a radioactive source with just the right rate of emission of alpha particles, an electric field of just the right strength, and so fortlh. In the case of Pissarides's model, the hyper-fine tuning takes the form of assuming that people are identical, live exactly two periods, are equal in number in each generation, engage in Nash wage bargains with their employers,

have matching probabilities that can be described according to rather precise formulae, and so forth. It is striking that, while Shamor's machine could be built according to his blueprint, I would not know where to begin to build a real-world version of Pissarides's machine. Shamor's design is a recipe for *Pfeffernüsse*; Pissarides's is a recipe using salt with spherical crystals, unsweet sugar, and the spice of the fairy bush; I can build Pissarides's machine, but only as a computer simulation, not in an actual economy. The probabilities of Shamor's machine are rooted in the fact that the radioactive source really does emit alpha particles randornly vvith known probability; the probabilities of Pissarides's machine are deductions from axiomatic assumptions about the probabilities of job match. If the economy were the way that Pissarides describes it, then these deduced probabilities would be observed. But it is not really that way.

The point is not that Pissarides's model is wrong or useless, but merely that it stands in a different relationship to the world than Shamor's model. It is, perhaps, a toy, bearing the same relationship to the economy as a model airplane does to a Boeing 747. Toys have their uses – even scientifically. In the movie of ***Planet of the Apes,*** the intelligent apes deride the astronauts' claim to have flown to their planet, on the grounds that flight is impossible. A paper airplane, quickly folded and flown the length of the room, provides an eloquent refutation. At best, Pissarides's model is an idealization. As such it raises subtle questions about the relationship of idealizations to real-world data that interest not only methodologists and philosophers, but which form the heart of serious practical disputes among economists, as the case of the contrasting reactions to the Lucas critique of Sargent, on the one hand, and Lucas, Kydland and Prescott, on the other (Section II above; cf. Hoover 1994, 1995).

It is instructive to examine Pissarides's (1992, Section V) own discussion of the empirical implications of his model. He considers an empirical model of two equations:[8]

(31) $$v = F(\phi, w, s, d)$$

and

(32) $$q = G(v, s, c, \sigma),$$

where v is the measure of vacant jobs, 4 and w "are the usual variables that influence the demand for labor in frictionless models" (p. 1387), s is the number of job seekers, d is the duration structure of unemployment, c, is the intensity of search, and σ is a measure of mismatch or sectoral shifts.

How do these equations map onto Pissarides's theoretical model? Equation (31) is supposed to correspond to equation (7) in the theoretical model:

(7) $$J_t = Lk[1 + y + (1 - y)q_{t-1}]q_t,$$

where **J** is the number of jobs, L is the nurnber of workers in each generations, k is the inverse of the cost of opening a job for one period, y is a productivity parameter, q is the probability of a match between a job seeker and a job opening, and subscripts indicate the relevant time. Roughly v in equation (31) corresponds to **J** in equation (7). ϕ and w are parameters is the theoretical model($\phi = 2$ and w = 1) and so do not show up explicitly. d is a summary measure reflecting the time dependence induced by the interaction of the two matching probabilities at different dates, q_{t-1} and q_t.

Equation (32) is supposed to correspond to equation (8) (p. 1377):

(8) $$q_t = \min\{x(J_t/2L, 1), 1\},$$

[8] **Equations numbers refer are reproduced from the original**

where $x(\cdot, \cdot)$ is a twice-differentiable function with positive first-order and negative second-order derviatives, homogeneous of degree 1 and satisfying

(2) $$x(0, 2L) = x(J_t, 0) = 0$$

and

(3) $$x(J_t, 2L) \leq \max\{J_t, 2L\}.$$

Again, v in equation *(32)* corresponds to J in equation *(8)* and s to L. c "is implicit in"

(19) $$S_t = q_t[1 + y + q_{t-1}^2(1 - y)]L.$$

"[S]ectoral shifts [σ] were ignored in the theoretical discussion."

The point of recounting, these details of Pissarides's model is to show just how rough the correspondence is between his theoretical model, with its painstaking details needed to achieve closure and to deliver probabilities, and the schematic empirical model of equations *(31)* and *(32)*. The variables are vaguely defined and not perfectly analogous. What data do they represent? The functional forms are left general. To estimate equations (31) and (32), these forms would have to be made concrete, most likely linear or log-linear, though, in any case, they could not be direct transcriptions of equations (7) and (8), since they include variables omitted in the theoretical model. At best, Pissarides's model is suggestive of the qualitative relationships one might look for in the data. Unlike Shamor's proton machine, the hyper-fine details of Pissarides's model do virtually no work in helping us to create a chance set-up. They are not the elements of the blueprints for a nornological machine.

If we admit that Pissarides's model is not a blueprint of a nomological machine, does that mean that the idea of a nomological machine can do no work in econometrics? Not at all. Another feature omitted, from equations (31) and *(32)* are the error terms

always tacked onto econometrically estimated equations to reflect factors of which we are ignorant or have ignored. Let's add them:

(31') $$v = F(\phi, w, s, d) + \varepsilon$$

(32') $$q = G(v, s, c, \sigma) + \omega,$$

It is a necessary, though not sufficient, condition for the econometric model connecting two variables to have accurately recapitulated the probabilistic relationship generated by the underlying mechanism that the error terms, ε and ω, be random and uncorrelated with each other. If, say, we choose particular functional forms for equations (31') and (32') and the errors are not random, then we know that the estimated equations do not belong to the class of possible recapitulations. Econometricians worry about specification, appealing to entirely statistical criteria, precisely because they worry about a mismatch between what they estimate and what the unknown ***mechanism*** must have generated. The nomological machine is a regulatory ideal. We do not necessarily need a blueprint, though we do need to understand the implication of a machine being there in reality.

Pissarides (1992, p. 1390) particularly concerns himself with the possibility – suggested not by his theoretical model, but by general considerations – that d in equation (31') might be a function of q in equation (32'). The two equations would then be simultaneous, the error terms correlated; and the estimate of the marginal effect of d on v, which is his primary interest, would be biased. He considers the problem of finding instruments that would permit him to obtain unbiased estimates. The criterion on which he judges most instruments to be unsuitable is statistical – the fact that they are not

correlated with d and/or are correlated with v. Such instruments are the genuine equivalents of shielding in experiments. Their utility is found not with respect to the probability structure that Pissarides's assumptions guarantee for his theoretical model, but with respect to the probability structure of the error terms, which are not mentioned in his model at all. The important probabilities are not the ones that find their source in the analogue to Shamor's source of alpha radiation, but ones that reflect the fact the estimated system is carved out of a more complex system. The idea of the nomological machine has a heuristic role even when we lack a recipe.

The theoretical model is not a blueprint; it is interpretive and in economics there is usually a gap in precision between the interpretation (often only qualitative) and the estimated regularity. This is not a necessity. It is not that a nomological machine could not exist, though Pissarides's model is not a blueprint for one. Fortunately, the possibility and usefulness of econometrics does not depend on it being one.

A Bit of Old-fashioned and Primitive Econometrics

Pissarides *(1992,* p. 1390) mentions that an implication of his analysis is that a plot of the unemployment and the vacancy rate should make counterclockwise loops over the business cycle, the overall relationship (the Beveridge Curve) between them being inverse. Furthermore, he states that such loops are observed. I did not know that. Is it true? A little investigation into this question will provide a concrete example that may illustrate some of the points about econometrics and realism that I have made more abstractly already.

Vacancy data is better for the U.K. than the U.S., but having easiest access to U.S. data I plotted a measure of help-wanted advertisements in newspapers against the

unemployment rate for the U.S., quarterly from 1951 (earliest available data) through 1986, retaining ten years of observations for checking stability.[9] Figure 1 presents the scatterplot with a regression line. The data seem to indicate the relationship is not inverse as expected but direct. These data certainly do not look like data from a well-defined chance set-up. In Figure 2, I connect the data points in chronological,sequence. They are not random. They show two patterns: loops, indicative of serial correlation; and a drift up and to the right, indicative of nonstationarity. Figure 3 plots the two time series against the business cycle. Vacancies reach a high and unemployment a low near the peak of each business cycle. The extreme points drift higher with each cycle.[10] Figure 4 plots data transformed by subtracting the value at the previous cyclical peak from each series eliminating the drift. Figure 5 is the scatterplot of this data with a fitted regression line, which is now clearly inverse. The data is clearly still serially correlated, though the loops are now difficult to see since they are all centered on the regression line. Is it stable? Figure 7 plots the data from 1951 to 1996. A formal test of stability rejects the constancy of the regression coefficients. Nevertheless, comparison of Figures 5 and 6 suggest that, as a coarse, economic regularity, the relationship is robust. The regression slopes are not very different and there is no dramatic change in the scatter of the points. Elimination of the trends from the two series and their positive long-run associations clearly reveals an inverse relationship, but does not eliminate the serial correlation – the loops remain. Figure 7 plots a representative loop, from the 1983 to 1990 business cycle

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[9] **Data for vacancies are "Index of** Help-wanted advertising **in newspapers** (1967=100)," **series** LHEL; **for unemployment "unemployment rate, total civilian labor force," series** LHUR. **Both series are from the** *DRI Basic Economics: Macroeconomic Database.* McGraw-Hill **publisher, September 1996 edition.**
[10] **Formally, neither series can reject the hypothesis of a unit root on standard tests.**

(peak to peak) in which the counterclockwise pattern is evident. The relationship appears to be stable.

This exercise is econometrics of a very primitive sort. It actually exemplifies pretty well the sort of econometrics that was done in the period before electronic computers (see Hendry and Morgan 1995, Morgan 1989, Klein 1996). It differs in detail, not in spirit, from the econometrics discussed in Pissarides's article and much of the econometrics currently practiced. It illustrates a number of points with respect to this paper.

We have uncovered three robust facts: 1. unemployment and vacancies trend together in the long run; 2. they are inversely related for any business cycle; 3. their relationship is nonlinear (the loops). These facts are robust, but they are imprecise. It is clear from comparing Figure 1 and Figure 6 that Lawson and Cartwright are perfectly right that what we observe are complex products of deeper interactions. The superficial conjunctions of data, if they show any pattern at all, may be profoundly misleading. To discover what the enduring relationships are requires interventions or, at least, accounting for conflating factors, as we dici in controlling for the trend in going from Figure 2 to Figure 5. The control here was of a rather unspecific kind, unlike the hyper-fine assumptions of the nomological machine. We set aside the trend movements without shedding any light on what factors govern the trend. We were governed by an economic intuition that economic relations are more likely to be stable within a business cycle than from one cycle to another. There were no guarantees. It worked; but it might not have.

We have not found a covering law or directly exhibited the capacity of a nomological machine. On general economic grounds, it is more likely that the

relationship between unemploymenlt and vacancies is the result of a common cause than that one causes the other directly."[11] Yet, it may nevertheless be useful to know this non-causal correlation. It is probably not a bad guide to newspaper managers of the demand for advertisements conditional on forecasts of the unemployment rate.

Despite the ambitions and rhetoric of the Cowles Commission, econometrics is rarely about the measurement of completely articulated causal systems. It is about observation (cf. Hoover 1994). As such there is no conflict with realism. What is observed is the consequence of the underlying (Lawson's intransitive) reality. Observations invite explanation. Even if a fact, such as the relationship of unemployment to vacancies, were to vanish (say, for the reasons highlighted in the Lucas critique), its having been so now stands in need of explanation.

V. To End Optimistically

I am more optimistic about the prospects for econometrics than either Lawson or Cartwright. I cannot agree with Lawson that realism implies the impossibility of econometrics. Econometrics is not about measuring covering laws. It is about observing unobvious regularities. The existence of such regularities, at least locally, is a requirement of realism.

Nor can I agree with the message implicit in Cartwright's work that the conditions

[11] Unlike the equations that usually represent them, regressions are directed. In a causal context. one would treat the independent variables as causes of the dependent variable. If I am right, that unemployment and vacancies are correlated because of a common causes. then there is no reason to prefer the regression of vacancies on unemployment. which is the regression line in the diagrams to one of unemployment on vacancies. The former minimizes the variance of the error measured as deviations between the regression line and observed vacancies, the latter as deviations between the regression line and observed unemployment. The fitted curves have different slopes, although they are qualitatively similar.

under which econometrics could succeed are too demanding to be met. The goal of econonometrics is not to serve as a nomological machine nor as its blueprint, but to discover facts that are generated by unobservable nornological machines, facts that theoretical models explain by providing rough drawings, if not blueprints. The situation is like the British code-breakers during World War II. There were intercepted messages (data); patterns were discovered in the messages (econometrics); a design for a machine that could generate such patterns, starting first with a general conceptualizations (an idealized theoretical model) and ending with a working model (a goal which for many practical and, perhaps, metaphysical reasons may be beyond economics).

The robustness of econometric facts is an argument for the existence of nornological machines, but the tools for discovering those facts do not presuppose (fully articulated) knowledge of the construction of those machines. The existence of robust facts is always contingent. Consider the attempts described in Anand and Kanbur (1995) to determine the effect of social expenditure on economic welfare in Sri Lanka. There may be good economic reasons to doubt that this can be measured accurately by situating Sri Lanka in a cross-country study that presupposes that each country in the study is the outcome of a common process. Anand and Kanbur implicitly reason that, if there is enough commonality of structure between the countries in the study (if the data are examples of the successful operation of a nomological machine), then the data will have certain econometrically observable features. When that proves not to be the case, they conclude that the cross-sectional investigation is fruitless and move on to a time-series study of Sri Lanka alone. Realistic metaphysics could not have told us a priori that they were right to do so.

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Figure 1. Vacancies and Unemployment 1951-86

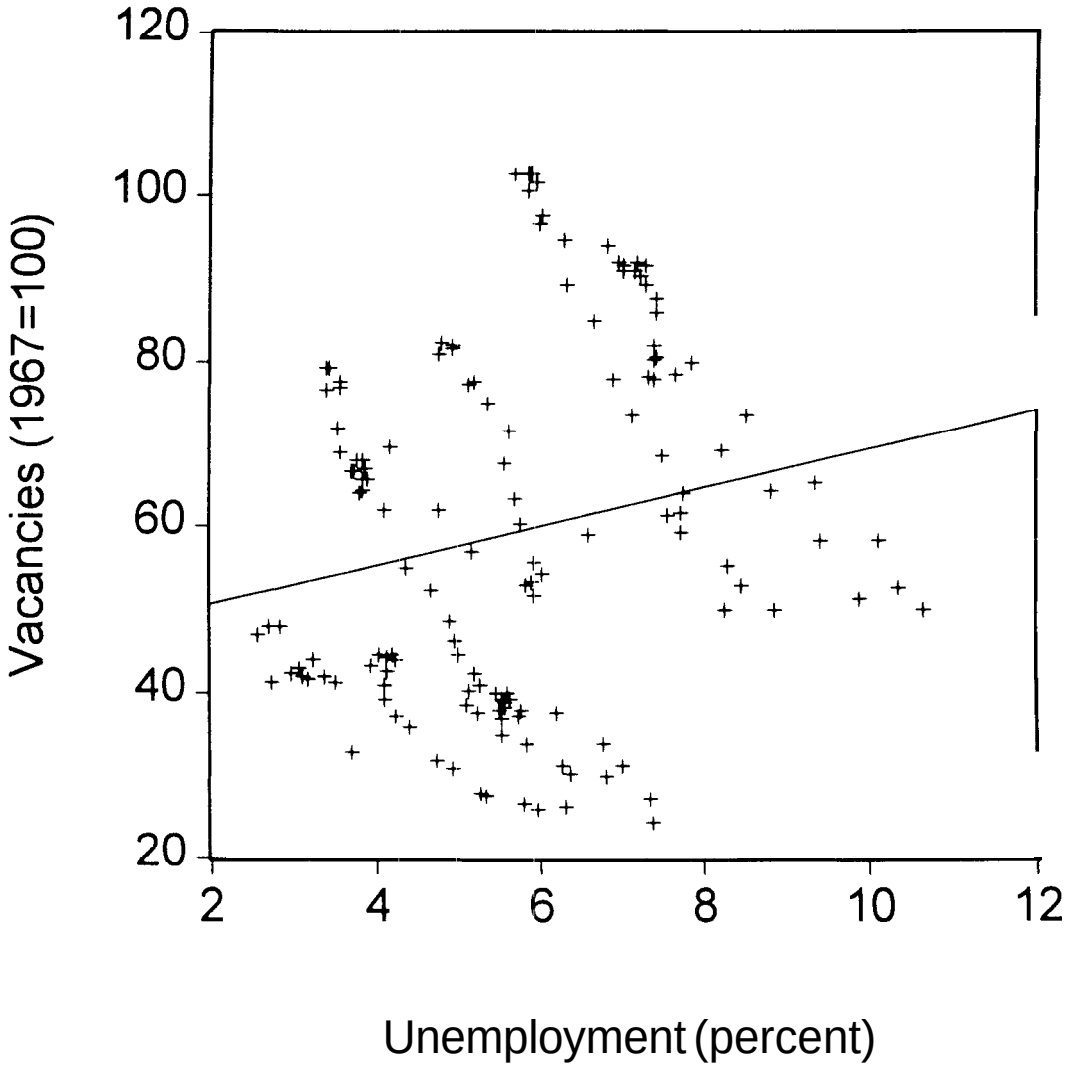

Figure 3. Vacancies and Unemployment 1951-86

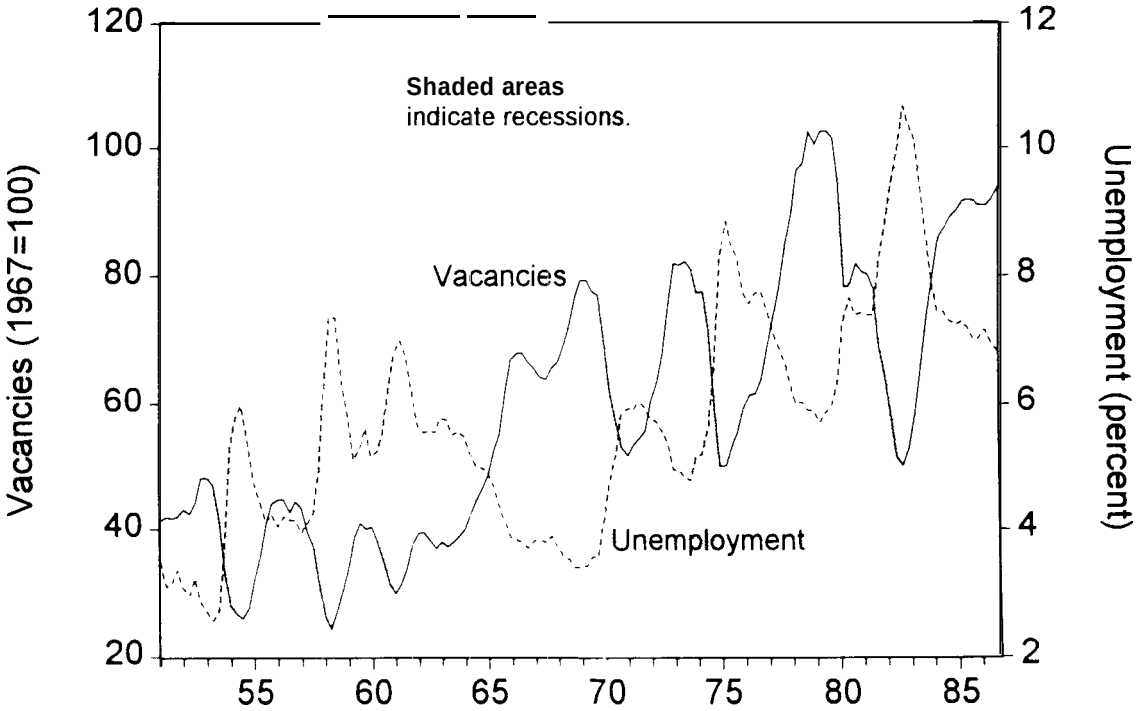

Figure 4. Vacancies and Unemployment 1951-86

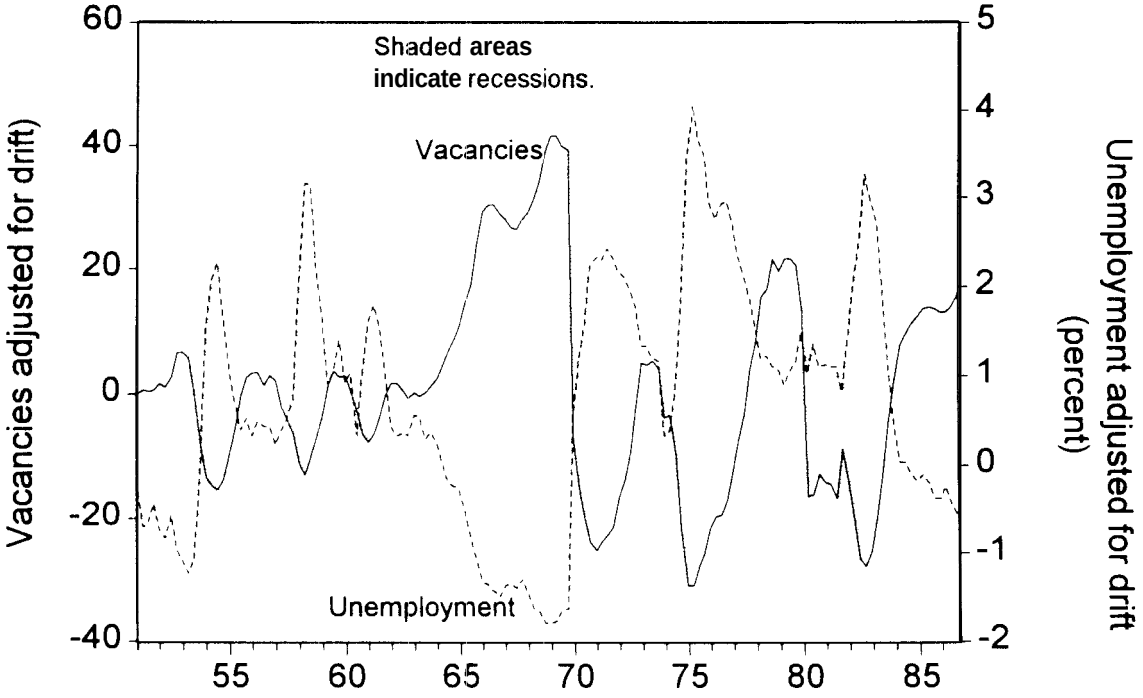

Figure 5. Vacancies and Unemployment 1951-86

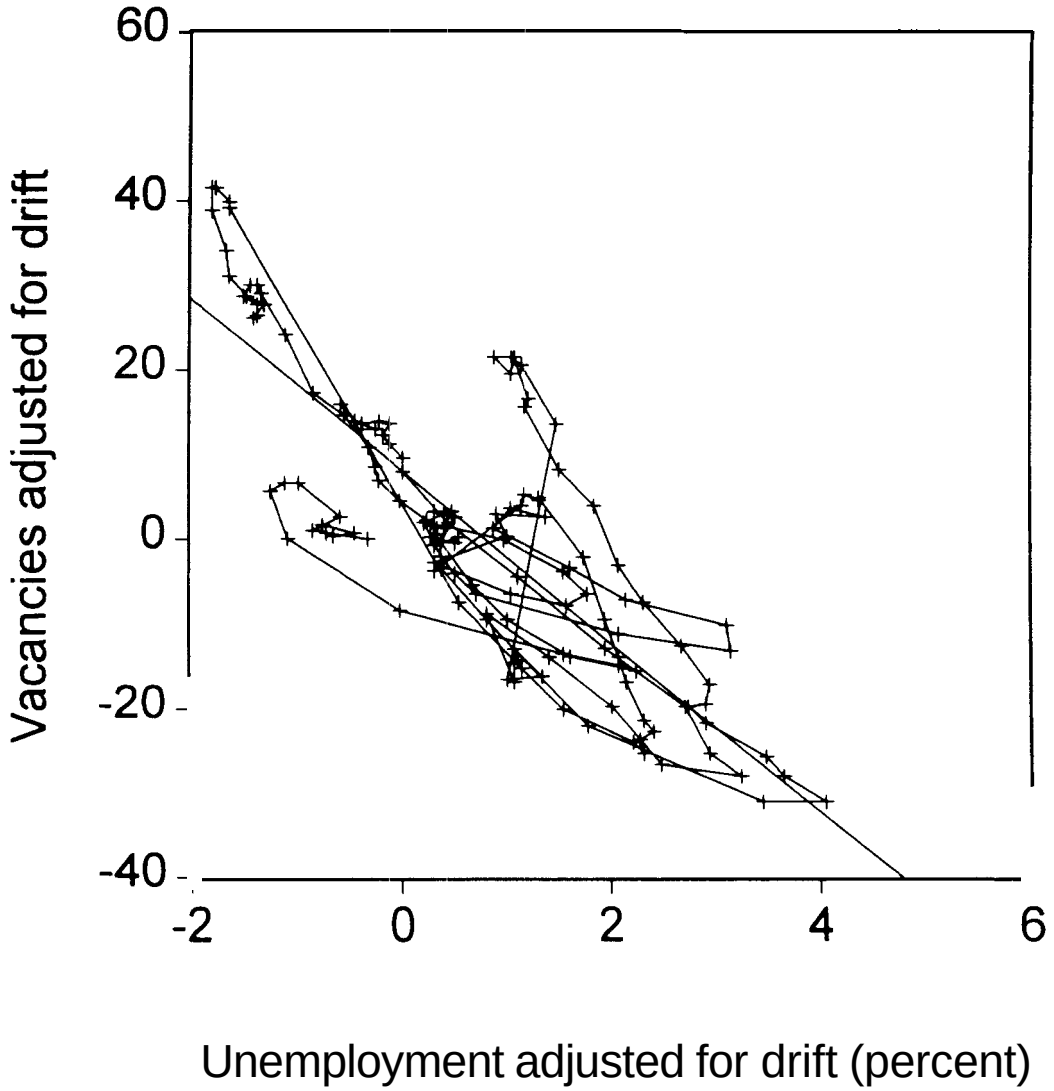

Figure 6. Vacancies and Unemployment 1951-96

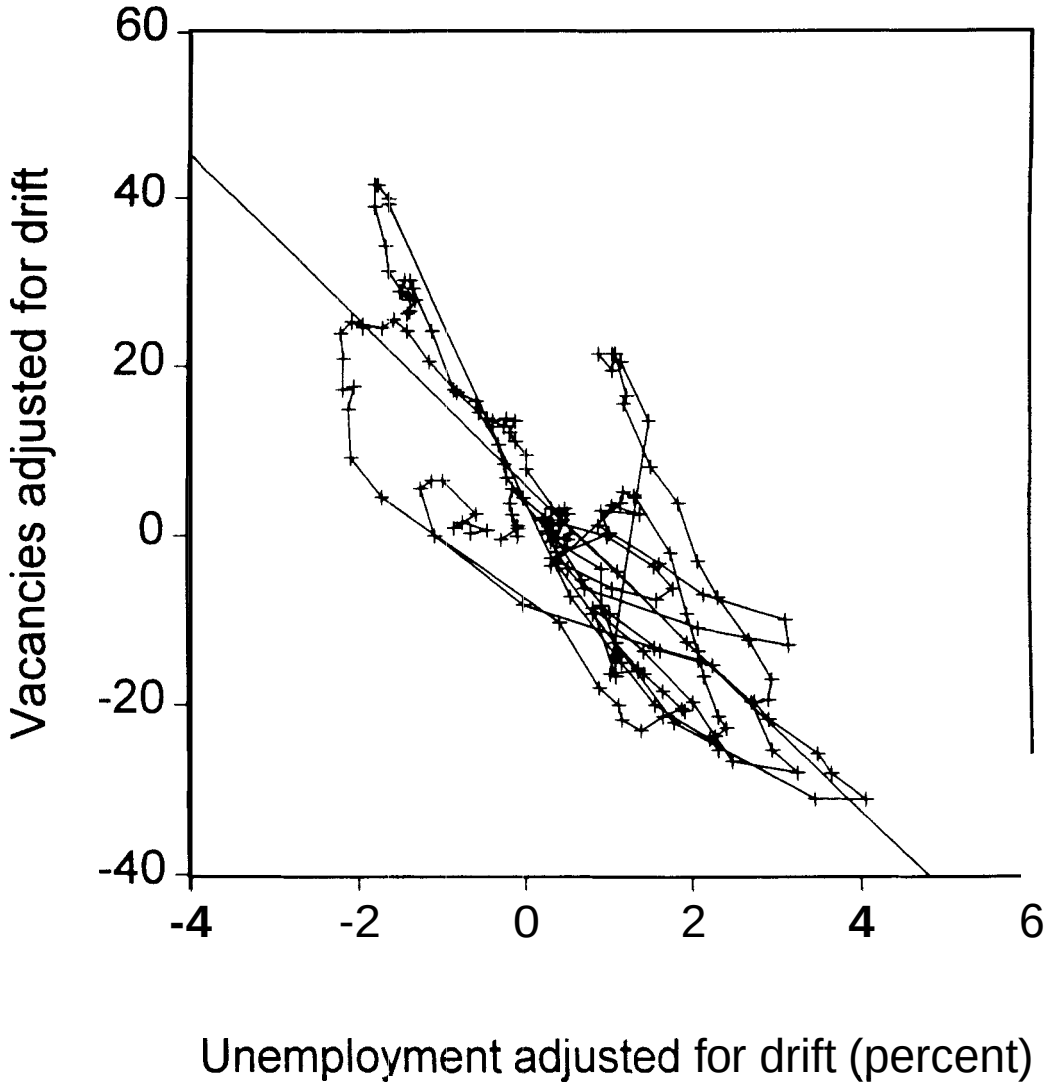
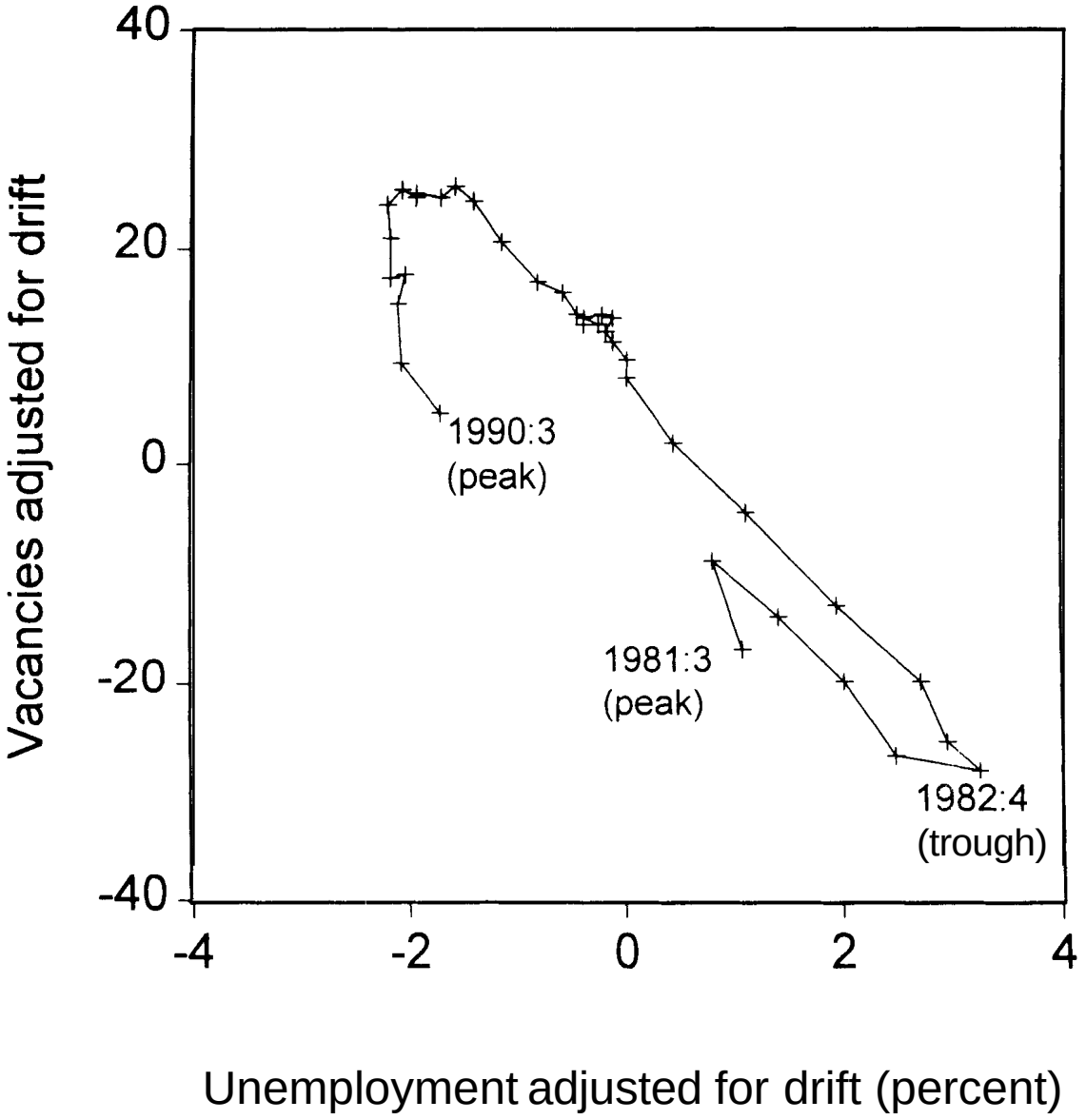
Figure 7. Vacancies and Unemployment over a Business Cycle (peak to peak)