

Hayek and Keynes

A Commonality

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It is the mark of an educated mind to seek in each inquiry the sort of precision the nature of the subject permits.

Aristotle, *Nicomachean Ethics*. Book 1, 10a 4b 25

1. Introduction

It is hardly controversial to observe that Hayek and Keynes take radically different positions on many issues. Moreover the differences between the two span the range of concerns of economists - policy orientations, support for substantive theories, notions of individual morality, and so forth. To get a feel for the strength of opposition it is necessary only to read Hayek's many assessments of Keynes' writings and influence. Even so, the argument I wish to make here, which truly represents but a very simple point, is that despite all of their well-known and often discussed differences, as economists Keynes and Hayek share something very significant in common. Moreover, this common feature not only serves to distinguish their contributions from those of the vast majority of contemporary economists, but ultimately makes their approaches so much the more relevant and potentially fruitful than the whole gamete of activities which currently passes as economics.

The commonality that I have in mind is a disposition or orientation towards the practices of social science, which, for want of a better way of expressing it, I shall refer to as a *realist orientation*. Now by ascribing this stance to Hayek and Keynes I do not wish merely to acknowledge that they are philosophical realists. By philosophical realism I mean a commitment to the existence of some (often disputed) kind of entity (such as black holes, probabilities, class relations, economic equilibria, human minds, etc.). Clearly, on this conception we are all realists of a kind and there are very many conceivable realms. By a realist orientation (in science) I want to convey more than this. Perhaps an *ontological orientation* or a *metaphysical realism* are more, or anyway equally, apt as a description. For the feature which I want to capture is a disposition to inquire into the nature of being, of existence, including the nature of the potential objects of (scientific) study, with the intention of using insights so obtained to select and/or fashion methods, procedures and techniques which are adequate (or identify those which are inadequate) to illuminating the relevant aspects of reality. If, then, I mean by a realist orientation (in science) a forthright concern to elaborate insights into the broad nature of features of natural and social reality, or the implicit conceptions of reality presupposed by procedures widely followed by researchers, I shall tend to restrict usage of the term realism for denoting specific accounts of the nature of reality, thereby obtained.

But what is the value of such a stance or disposition? Why indeed bother with conceptions of realism, ontology and the realist orientation, so understood? Surely contemporary mainstream economists are sensible to avoid wasting time on such matters and correct in accepting the goals, criteria, methods and procedures as laid down in prominent

economic text-books and articles, etc., and just getting on with the job? Even allowing that contemporary academic economics has proven to be notoriously unsuccessful over the last fifty years or so, is not the proper and sensible solution just to try that little bit harder?

I hope that I can be excused here for briefly resurrecting a counterfactual scenario that I have previously discussed elsewhere (Lawson, 1994a). Let me ask the reader to imagine a situation in which an instrument of some sort has been useful for a specific task. Let us suppose that a big stick is used successfully to beat and thereby to clean a dusty old mat. Imagine also that the inference is automatically made that if such an instrument is found to be useful for one job it must be thereby useful for any jobs. Specifically, let us suppose that it is inferred from the success of the big stick in helping to clean the dusty old mat that it must also be of use in cleaning a dusty glass window - and that the window is duly beaten.

No doubt many will find such an inference and act preposterous. But why? Presumably because we know enough of the nature of glass windows and big sticks to infer that 1) beating the window with a stick is unlikely to prove to be a successful way of cleaning it (to say the least); while 2) alternative more promising ways of cleaning the window can easily be devised.

Imagine, furthermore, that the 'stick-cleans-window' theory is put to the test and consequent upon repeated instances of broken glass the inference repeatedly drawn is that it is necessary to 'try that little bit harder' - perhaps on the grounds that it is the wrong windows so far that have been beaten, and/or it is simply imagined that success is (always) just around the corner. No doubt the perpetrator of the theory would be dismissed as a rather unreflective dogmatist.

Now this example in outline, I suggest, is quite analogous in relevant respects to the situation that is found in contemporary orthodox economics. Specifically, as I shall briefly indicate below, contemporary orthodox economists have noted that certain methods of scientific analysis have, like the big stick, been found to be of worth in some important application and they thereby infer that the methods in question must be equally appropriate to any task that appears related. In particular, it is supposed that certain methods of reasoning, aims and criteria that have proven to be efficacious in *particular* natural science contexts, must thereby be equally appropriate to all other scientific contexts including, specifically, the analysis of society and economy. Such an inference is, of course, questionable, and it is the, in fact largely unquestioned, reliance upon this inference, I believe, that is fundamental to understanding the failure of much of contemporary economics over the last fifty years or so. The solution I shall suggest, however, warrants something more than an endeavour to try a little bit harder.

The point that I am wanting to emphasise in all this, of course, is that just as insights into the nature of glass should be brought to bear upon the choice of materials that are used in the attempt to clean the window so insights into the nature of social reality should be brought to bear upon the modes of reasoning and techniques employed in the endeavour to illuminate the economy. And while focusing upon the nature of social material just means giving attention to questions of *ontology*, any theoretical insight into the nature of social material so sustained constitutes a *realist* theory.

It is the disposition so to employ insights into the nature of social reality in fashioning methods of economic analysis, in questioning how best (or how not) to proceed, that is being interpreted here as a *realist orientation* in science. And it is the associated refusal to employ methods for social science without reflecting upon their potential worth - i.e. without questioning their suitability for the analysis of social objects, given their nature - that, I am suggesting, distinguishes the contributions of Hayek and Keynes from those of most contemporary economists. In truth, once stated this claim is hardly contentious. Nevertheless, because the shared commonality identified here is essential to understanding the lasting

relevance of the work of both Hayek and Keynes compared to that of most modern day economists I think it is worthwhile elaborating the various issues involved somewhat further.

2. Contemporary Mainstream Economics

First, what of the claim that contemporary mainstream economics is fashioned by people adopting something other than a realist orientation? After all, I am suggesting that it is in part this situation, if the claim is correct, which justifies giving continued attention to any prominent realist. It is not difficult to establish that the claim is indeed correct. For it is clear enough that mainstream economists 1) never challenge the received view that natural science takes the form of elaborating constant conjunctions of events - regularities of the form whenever 'event X then event Y' - and 2) unthinkingly imagine that the aim of any economics worthy of the label scientific must be to identify similar regularities in the social realm. This situation is well captured by Allais (1992), a recent winner of the Nobel Memorial Prize in Economic Science, in his assessment of "the economic science of today":

The essential condition of any science is the existence of regularities which can be analyzed and forecast. This is the case in celestial mechanics. But it is also true of many economic phenomena. Indeed, their thorough analysis displays the existence of regularities which are just as striking as those found in the physical sciences. This is why economics is a science, and why this science rests on the same general principles and methods as physics (p.25).

It is straightforwardly apparent that this conception of science underpins contemporary econometrics - albeit in a modified probabilistic form. But it is important to see that the structure of science in question also grounds modern economic 'theory'. The latter, as I have argued at length elsewhere (Lawson 1995a), must be recognised as a form of explanation that can be referred to here as *deductivism*. Now to explain something is to provide an account (the explanans) whereby that something (the explanandum) is rendered intelligible. According to deductivism, as I am using the term, to be able to explain an actual event or state of affairs is to deduce a statement of it from a set of initial or boundary conditions plus universal "laws", i.e., plus universal regularities, or constant conjunctions, of the form "whenever event (type) X then event (type) Y"; the explanation of such laws, as well as theories and sciences, likewise proceeds by deductive subsumption.

The fact that deductivism does not usually figure in the characterisations of the essential features of orthodoxy that are provided by its proponents does not mean that its centrality is ever denied. While the deducibility requirement, that the explanandum be deducible from the explanans, is more or less always transparent, the covering-law aspect, i.e. the inclusion of at least one universal law (of the form whenever event (type) X then event (type) Y), is usually met by the axioms. Certainly, axioms take the 'whenever this then that' structure and typically serve to relate hypothetical events or states of affairs, i.e. the sort of phenomena which, if the sort of world implied by such models were ever to hold, would, under such conditions, be objects of actual or possible experience. Many 'theorists', even take axioms to depend upon our actual experiences, i.e. of the social world in which we do live: "It is not that [axioms] ... are divorced from experience or observation but rather that they mark the stage beyond which one does not seek to explain" [Hahn, 1984, p.6]. Elsewhere, and more generally, Hahn writes:

For I do not wish to deny that there are empirical regularities of economic behaviour awaiting discovery. But I claim that these will be, as it were, much deeper down, more elementary and closer to the form in which axioms are postulated than are the complex, institution and history dependent 'facts' of the econometrician. [1984, p. 332]

Axioms are not plucked out of the air and far from distancing the theorist from what somewhat mysteriously is called the "real" world, they constitute claims about this world so widely agreed as to make further argument unnecessary. [1985, p. 5]

In short, if a reliance upon the deductivist mode of explanation is not always explicit in orthodox accounts neither is it denied. Rather its centrality and indeed universality - *to all subjects* and so economics included - are essentially taken for granted, as obvious givers. Those critics who venture to suggest otherwise tend to be summarily dismissed. As Hahn further writes:

Opponents of [economic] theory often argue that it is tautologous because it consists of logical deductions from axioms and assumptions. If one is kind to such critics one interprets them as signalling that they do not care for these axioms and these assumptions. In any case all theory in all subjects proceeds in this manner.

Directly or indirectly, then, the event regularity conception of science, or its structure, can be seen to underpin mainstream economics, and justified *merely* on the basis that it is characteristic of *all* science.

3. The apparent 'anti-naturalism' of Keynes and Hayek

I have suggested that mainstream economists by and large 1) recognise a particular concept of science and explanation as characteristic of the successful natural sciences, and 2) uncritically impose this conception in determining the methods utilized, and practices followed, in their attempts to illuminate social phenomena.

I now want to suggest that Keynes and Hayek take a much more critical and reflective stance with regard to step 2. That is, they are *not* prepared merely to assert that methods or conceptions which have proven to be successful in the illumination of natural phenomena must necessarily be of use in the process of understanding social phenomena. Indeed, on the basis of ontological theorising they end up arguing to the contrary. Let me provide some evidence on this starting with the contributions of Keynes.

Keynes and the nature of science

It is not always clear how far Keynes accepts the event-regularity conception of natural science - although there at least appears to be occasions when he does. In any case, at all times ontological considerations ground his methodological assessments. The first lengthy discussion of science appears in *A Treatise on Probability*. Here Keynes does not so much assent to the contemporary theories and practices of natural science as indicate the sorts of ontological presuppositions that natural scientists must be making given the nature of their (inductive) scientific practices:

The kind of fundamental assumption about the character of material laws, on which scientists appear commonly to act, seems to me to be much less simple than the bare principle of uniformity. They appear to assume something much more like what mathematicians call the principle of the superposition of small effects, or, as I prefer to call it, in this connection, the *atomic* character of natural law. The system of the material universe must consist, if this kind of assumption is warranted, of bodies which we may term (without any implication as to their size being conveyed thereby) *legal atoms*, such that each of them exercises its own separate, independent, and invariable effect, a change of the total state being compounded of a number of separate changes each of which is solely due to a separate portion of the preceding state. We do not have an invariable relation between particular bodies, but nevertheless each has on the others its own separate and invariable effect, which does not change with changing circumstances, although, of course, the total effect may be changed to almost any extent if all the

other accompanying causes are different. Each atom can, according to this theory, be treated as a separate cause and does not enter into different organic combinations in each of which it is regulated by different laws. (1973b, pp. 276, 277)

While drawing attention to this assumption of atomic character of natural law, Keynes is simultaneously raising the logical possibility that not all natural phenomena need be atomic. To the extent that some are not then clearly the methods of natural scientists that presuppose atomicity can not be accepted as universally applicable:

The scientist wishes, in fact, to assume that the occurrence of a phenomena which has appeared as part of a more complex phenomena, may be *some* reason for expecting it to be associated on another occasion with part of the same complex. Yet if different wholes were subject to laws *quod* wholes and not simply on account of and in proportion to the differences of their parts, knowledge of a part could not lead, it would seem, even to presumptive or probable knowledge as to its association with other parts. Given, on the other hand, a number of legally atomic units and the laws connecting them, it would be possible to deduce their effects *pro tanto* without an exhaustive knowledge of all the coexisting circumstances. (1973b, pp. 277, 278)

If Keynes at first - i.e. in the early years of the twentieth century - seems somewhat non-committal regarding the extent to which the material of the natural world can be regarded as atomic, by the mid-nineteen twenties he is definite that social phenomena cannot be. Thus in a 1926 biography of Edgeworth, Keynes writes:

The atomic hypothesis which has worked so splendidly in Physics breaks down in Psychics. We are faced at every turn with the problems of Organic Unity, of Discreteness, of Discontinuity - the whole is not equal to the sum of the parts, comparisons of quantity fail us, small changes produce large effects, the assumption of a uniform and homogeneous continuum are not satisfied (1973b, p. 286)

A decade later, in the context of discussing the methods of econometrics as presented in Tinbergen's work on Business Cycles, Keynes is more definite still. Here he does appear to grant the legitimacy of the event regularity conception of science for disciplines such as physics and chemistry:

In chemistry and physics and other natural sciences the object of experiment is to fill in the actual values of the various quantities and factors appearing in an equation or a formula; and the work when done is once and for all. In economics that is not the case, and to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought. Tinbergen endeavours to work out the variable quantities in a particular case, or perhaps in the average of several particular cases, and he then suggests that the quantitative formula so obtained has general validity. Yet in fact, filling in figures, which one can be quite sure will not apply next time, so far from increasing the value of his instrument, he has destroyed it. (1973c, p. 299)

The point is that the material of economics is recognised as having a nature such that the natural scientific practices and formulae in question are inappropriate to its analysis: "unlike typical natural science, the material to which [economics] is applied is, in too many respects, not homogeneous through time" (1973c, p. 296). This assessment is at the heart of Keynes' view regarding the potential value of econometrics. In an initial response to an invitation from the League of Nations to review Tinbergen's work Keynes writes:

There is first of all the central question of methodology, - the logic of applying the method of multiple correlation to unanalysed economic material, which we know

to be non-homogeneous through time. If we are dealing with the action of numerically measurable, independent forces, adequately analyzed so that we were dealing with independent atomic factors and between the completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action.

In fact we know that every one of these conditions is far from being satisfied by the economic material under investigation ...

To proceed to some more detailed comments. The coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, surely we know that they are not constant. There is no reason at all why they should not be different every year. (1973c, p. 285)

These sorts of comments are repeated throughout the late 1930's and come to a head in Keynes' (1939) eventual review of Tinbergen's book:

Put broadly, the most important condition is that the environment in all relevant respects, other than the fluctuations in those factors of which we take particular account, should be uniform and homogeneous over a period of time. We cannot be sure that such conditions will persist in the future, even if we find them in the past. But if we find them in the past, we have at any rate some basis for an inductive argument.

and he adds

[The] main *prima facie* objection to the application of the method of multiple correlation to complex economic problems lies in the apparent lack of any adequate degree of uniformity in the environment. (1973, p. 316)

Whatever the reader may think about the correctness of these arguments, (and elsewhere I have argued they are indeed valid - Lawson, 1985, 1989, 1995d) it is clear that in making these assessments Keynes draws on insights in social ontology. In short, he is taking what I am calling a realist orientation towards science in determining the adequacy of various methods and procedures for social scientific research.

Hayek on Science

A few years after Keynes' review of Tinbergen's book appeared Hayek commenced his *Scientism and the Study of Society* essay with an even more direct and forceful condemnation of the unreflective imitation of the methods of natural science by economists. Hayek observes that economists had not always displayed an unquestioning naturalism; it came about only in the early nineteenth century following various successes in the field of physics and biology especially. However, Hayek recognises that in the mid twentieth century a felt need to imitate the methods of natural science, a 'tyranny' of natural science over economics, is currently pervasive. And he leaves the reader in no doubt that he regards this 'scientism' as having contributed very little to the understanding of social phenomena. I quote from Hayek on this at length:

In the course of its slow development in the eighteenth and early nineteenth centuries the study of economic and social phenomena was guided in the choice of its methods in the main by the nature of the problems it had to face. It gradually developed a technique appropriate to these problems without much reflection on the character of the methods or on their relation to that of other disciplines of knowledge. Students of political economy could describe it alternatively as a branch of science or of moral or social philosophy without the least qualms whether their subject was scientific or philosophical. The term *science* had not yet assumed the special narrow meaning it has today, nor was there any distinction

made which singled out the physical or natural sciences and attributed to them a special dignity. Those who devoted themselves to those fields indeed readily chose the designation of philosophy when they were concerned with more general aspects of their problems, and occasionally we even find "natural philosophy" contrasted with "moral science".

During the first half of the nineteenth century a new attitude made its appearance. The term *science* came more and more to be confined to the physical and biological disciplines which at the same time began to claim for themselves a special rigorousness and certainty which distinguished them from all others. Their success was such that they soon came to exercise an extraordinary fascination on those working in other fields, who rapidly began to imitate their teaching and vocabulary. Thus the tyranny commenced which the methods and technique of the Sciences in the narrow sense of the term have ever since exercised over the other subjects. These became increasingly concerned to vindicate their equal status by showing that their methods were the same as those of their brilliantly successful sisters rather than by adapting their methods more and more to their own particular problems. And, although in the hundred and twenty years or so, during which this ambition to imitate Science in its methods rather than its spirit has now dominated social studies, it has contributed scarcely anything to our understanding of social phenomena, not only does it continue to confuse and discredit the work of the social disciplines, but demands for further attempts in this direction are still presented to us as the latest revolutionary innovations which, if adopted, will secure rapid undreamed of progress. (pp. 19-21)

In consequence, Hayek sets out to elaborate a non-naturalistic account of the way in which social phenomena are to be understood. I think Hayek does view natural science as necessarily taking the form of focusing on events and states of affairs and elaborating their (presumed) constant conjunctions. Hayek's particular contribution on such matters is to argue that, in the process of elaborating these event regularities, natural science provides a classification of 'external stimuli' different to that immediately provided by the senses. Thus, while, for example, we might first classify all white powders together precisely because they are each white and powdery, natural science comes to reclassify the substances in question according to the sorts of events that ensue when these substances are placed in alternative states or conditions, i.e. according to the event regularities (or natural scientific 'objective facts') with which they are thought to be associated. Hayek writes:

... the persistent effort of modern Science has been to get down to "objective facts" (p. 29)

[Science's] main task became to revise and reconstruct the concepts formed from ordinary experience on the basis of a systematic testing of phenomena, so as to be better able to recognize the particular as an instance of a general rule. (p. 29)

... Science ... begins with the realization that things which appear to us the same do not always behave in the same manner, and that things which appear different to us sometime prove in all other respects to behave in the same way; and it proceeds from this experience to substitute for the classification of events which our senses provide a new one which groups together not what appears alike but what proves to behave in the same manner in similar circumstances. (p. 31)

This process of reclassifying "objects" which our senses have already classified in one way, of substituting for the "secondary" qualities in which our senses arrange external stimuli a new classification based on consciously established relations between classes of events is, perhaps, the most characteristic aspects of the procedure of the natural sciences. (p. 32)

In any case, Hayek is of the view that the methods of natural and social science are necessarily different. Specifically, Hayek elaborates his well known subjectivist account of social science drawing on perceived differences between natural and social phenomena. The significant feature here, Hayek recognises, is that social life depends on human beliefs and conceptions. Thus the starting point of social science must be to grasp these individual perceptions. But the aim is not, as in natural science, to correct these conceptions, but to see how social life develops from them. In short, the aim is to start from individual concepts and views and to build out of these the more complex phenomena of society. Thus social science is seen to be 'compositive' where the natural sciences are regarded as 'analytic' (the natural sciences start from more complex phenomena and work backwards to determine the elements from which the latter are composed). I think Hayek's account of all this will be too well known to require further elaboration at this stage. Let me settle for just one more quotation to back-up the sort of assessment I have just been making:

A few more remarks must be added about the specific theoretical method which corresponds to the systematic subjectivism and individualism of the social sciences. From the fact that it is the concepts and views held by individuals which are directly known to us and which form the elements from which we must build up, as it were, the more complex phenomena, follows another important difference between the method of the social disciplines and the natural sciences. While in the former it is the attitudes of individuals which are the familiar elements and by the combination of which we try to reproduce the complex phenomena, the results of individuals actions, which are much less known - a procedure which often leads to the *discovery* of principles of structural coherence of the complex phenomena which had not been (and perhaps could not be) established by direct observation - the physical sciences necessarily begin with the complex phenomena of nature and work backward to infer the elements from which they are composed. The place where the human individual stands in the order of things brings it about that in one direction what he perceives are the comparatively complex phenomena which he analyzes, while in the other direction what are given to him are elements from which those more complex phenomena are composed that he cannot observe as wholes. While the method of natural sciences is in this sense, analytic, the method of the social sciences is better described as compositive or synthetic. It is so-called wholes, the groups of elements which are structurally connected, which we learn to single out from the totality of observed phenomena only as a result to our systematic fitting together of the elements with familiar properties, and which we build up or reconstruct from the known properties of the elements. (pp. 65-67)

4. Qualificatory Observations

In contradistinction to most contemporary economists, Hayek and Keynes, I think it is clear, adopt the orientation towards scientific practice which I am here designating realist. The simplicity of this claim is such that I worry that there is a real danger that I am interpreted as claiming more than I mean to. In order to minimise this possibility of this let me briefly run through a series of related potential claims which I am definitely *not* wanting to sustain.

Hayek and Keynes are not without error

A first point to emphasise is that I am not suggesting that, in their ontological theorising, Keynes and Hayek are without error. Indeed, I am convinced that the contrary is the case. Most significantly, recent developments in the philosophy of science (for example - Bhaskar, 1978; Harré, 1970; Hesse, 1966) indicate that Hayek and Keynes are certainly mistaken to the extent that they concede natural science to the event regularity conception. It is necessary here

to realise that, outside astronomy at least, most of the event regularities that are considered to be significant in natural science are brought about only in conditions of experimental control. Thus, those wishing to tie science and its laws to event regularities succeed only in 1) fencing off science from most of the goings-on in the world; 2) rendering 'laws of nature' dependent upon human intervention; and 3) leaving the fact that experimental results are successfully applied outside the experimental situation quite unintelligible.

To make sense of this situation it is necessary to acknowledge an ontology of structures, mechanism, powers, and tendencies in addition to actualities such as events and our experiences. Thus, reality is not reducible to such phenomena as falling leaves, movements in iron filings, or puppies turning into dogs, but also consists in such mechanisms as gravitational forces, magnetic fields and genetic codes which underlie actual events and govern them. Given this ontology, the experimental situation can be rendered intelligible not as a site of the operation of laws but as the situation in which the latter are empirically identified. That is, the experimental situation is where counteracting mechanism are held off so that some tendency of interest is revealed. Law statements, then, apply to structured things, their powers and tendencies, not to event regularities *per se* - which represent only the empirical identification of some mechanism. On this conception there is no question of fencing off science from most goings-on; no suggestion that laws of nature depend on human intervention; and no problem rendering the experimental situation intelligible. For, once triggered, a mechanism (which is now recognised as the proper object of a law-statement) can be operative both inside and outside experimental conditions. The gravitational tendency acts on the leaf in the palm of my hand every bit as much as it does the object falling with the constant rate of acceleration in conditions of (a near) vacuum¹.

Recent developments in the philosophy of science, then, reveal that the aim of natural science is the identification of mechanisms and tendencies that act *transfactually*, i.e., whatever the outcome at the level of events. The view that laws, or scientific results, necessitate event regularities, of course, is a legacy of positivistic influence. Hume encouraged it with all his attempted reduction of reality to impression. For if (knowable) reality consists only in events given in experience, then the only possibility for generalised knowledge, including science, depends upon patterns in the succession and coexistence of such events. Thus, regularities of the form 'whenever event X then event Y' are the only form of scientific results conceivable - regularities interpreted, of course, as Humean causal laws. And it is this Humean or positivistic conception that by and large underpins various statements by Hayek and Keynes on the nature of natural science. Of course, the significant point here is that, on finding this conception of science in place, both Hayek and Keynes, being more interested in the analysis of social phenomena, immediately recognised its inappropriateness in the social sphere.

The ceding of science to positivism, however, was not the only error that can be detected on the issues in question. If Keynes and Hayek are mistaken in their interpretations of the goals and practices of natural science, at the outset at least, Hayek, I think, is equally in error in his alternative account of social science. Indeed, although he states explicitly that he is attempting to provide a non-positivistic social science I think Hayek, in his *Scientism* essay, succeeds only in reproducing a subjectivised version of certain of the errors of positivism.

Before elaborating on this claim, however, it can be observed that, in the light of the recent developments in the philosophy of science, the question of naturalism, now correctly interpreted, turns on whether social structures exist to be uncovered in social science in the sense that natural science is concerned to uncover natural ones. And indeed they do - if we accept the defining characteristic of a *social* structure to be its dependency upon human (intentional) agency. Obvious examples include social rules, relations, positions, and the like. Such structures, for example, the rules of language, not only depend upon human agency, but

they pre-exist any individual act and make a difference to social life. If it is the latter capacity to make a difference that (as with non-perceivables such as magnetic fields in the natural sciences) establishes their reality, it is to the former aspect of their pre-existing human agency that establishes their autonomy and constitutes them as potential objects of study.

The social world, then, is open and structured in the same manner as the natural one. The major differences stem precisely from that defining characteristic of social structures that they, unlike natural ones, depend for their existence on human conceptions and actions (even if they are also a condition for such conceptions and actions). In consequence, because such structures are manifest only in human action, where any act could always have been otherwise, they will be manifest usually as tendencies and only rarely as event regularities. Indeed, it follows that if constant event conjunctions can be expected in any social context it is not as spontaneous occurrences somehow taking place behind the backs of individuals, as it were, but as intended productions. That is, any local closure (event regularity) as may occur in the social realm, such as those noted examples of regular annual holidays or the arrival and departure times for school, etc., will usually have been consciously brought about.

The problem for Hayek in his *Scientism* essay is that, despite recognising the inappropriateness of the event regularity conception of science, he fails to elaborate in its place a structured ontology in place of the atomistic one of positivism. Indeed, in the event he allows very little more in his ontology than Hume does. The important difference with Hume is that, in addition to phenomena given directly in experience, human beings, and explicitly social scientists, are able to grasp the contents of other peoples' minds - their attitudes and beliefs. The 'data' of the social science, then, the 'objective facts' of the social realm, are the individual agents' attitudes and opinions:

"... the facts of the social sciences are merely opinions, views held by the people whose actions we study. They differ from the facts of the physical sciences in being beliefs or opinions held by particular people, beliefs which as such are out data, irrespective of whether they are true or false ..."

Now if these 'subjective data', as Hayek calls them, amount to knowable phenomena that are not given in direct experience, their knowability, nevertheless, is closely bound up with experience. For, according to Hayek, it is a *fact of experience* that we all experience the same thing in the same way - even if or when we do not experience things as they really are (i.e. according to the classification provided by natural science). We thus can recognise the opinions, beliefs and attitudes, etc., of others from their actions and doings, and precisely because, or so Hayek suggests, these other individuals experience everything the same way as we do, because we classify all external phenomena in a similar manner:

The ... fact that different men do perceive different things in a similar manner which does not correspond to any known relation between these things in the external world, must be regarded as a significant datum of experience which must be the starting point in any discussion of human behaviour. (p.37).

We know, in other words, that in his conscious decisions man classifies external stimuli in a way which we know solely from our own subjective experience of this kind of classification. We take it for granted that other men treat various things as alike or unlike just as we do, although no objective test, no knowledge of the relations of these things to other parts of the external world justifies this. Our procedure is based on the experience that other people as a rule (though not always - for example, not if they are colour blind or mad) classify their sense impressions as we do.

But we not only know this. It would be impossible to explain or understand human action without making use of this knowledge. (p. 43).

So long as it was naively assumed that all the sense qualities (or their relations) which different men had in common were properties of the external world, it could be argued that our knowledge of other minds is no more than our common knowledge of the external world. But once we have learned that our senses make things appear to us alike or different which prove to be alike or different in none of their relations between themselves, but only in the way in which they affect our senses, this fact that men classify external stimuli in a particular way becomes a significant fact of experience. (p. 48)

Hayek's theory of knowledge as elaborated in the context of social science, then, turns on the ability of social scientists to grasp the opinions, beliefs and attitudes of other individuals. As such, and despite his realist orientation, and specifically his declared aim to transcend positivism, Hayek, in truth, succeeds only in moving, slightly beyond Hume at this stage. The result is that the implicit empiricist theory of ontology presupposed in positivism is taken by Hayek to include, or is now augmented by, such 'mental phenomena' as noted. We can all access the beliefs and attitudes of others, and we know that this is so empirically, i.e. from experience. And the reason we know it to be so, or perhaps another way of saying that we know that this is so, according to Hayek, is the equally empirical fact that we all have minds of a similar structure:

... the facts of the social sciences are merely opinions, views held by the people whose actions we study ... beliefs which as such are our data and which we cannot directly observe in the minds of the people but which we can recognize from what they do and say merely because we have ourselves a mind similar to theirs. (p. 47)

In the sense in which we here use the contrast between the subjectivist approach of the social sciences and the objectivist approach of the natural sciences it says little more than what is commonly expressed by saying that the former deal in the first instance with the phenomena of individual minds, or mental phenomena, and not directly with material phenomena. They deal with phenomena which can be understood only because the object of our study has a mind of a structure similar to our own. That this is so is no less an empirical fact than our knowledge of the external world. (p. 47)

Hayek, then, does move beyond Hume, but not by much. Most significantly, it is important to recognise in all this that Hayek does not posit a structured ontology. Social rules, relations, and so forth, are not (yet) recognised as real causal factors. And a most significant implication of this is that, for Hayek, neither human conceptions nor actions are matters to be *explained* or further analysed in social science, but *merely* items to be grasped or understood. This is why his position is elsewhere characterised as hermeneutic (e.g. Maddison, 1990; Lawson, 1994b, 1995). With a theory of knowledge, and so an implicit theory of ontology, that does not permit structures of determination, conscious action is effectively reduced to the individual's conscious aims or desires. For, just as there can be no unacknowledged (including inadequately understood) social structures or tacit skills, etc., conditioning what takes place, so too all physical conditions, being once more reducible to (*unstructured*) events and states of affairs, can have no bearing upon action excepting in the manner in which they are conceptualised. The last point is significant. For if an insight of the hermeneuticism, which Hayek at this point adheres to, is that social life is concept dependent, it is its characteristic error, which Hayek must also now accept, that social life is concept determined, that it is exhausted by its conceptual aspect. Thus, for social life, for human action and its consequences, and so too for the social scientist attempting to understand, *all* that is of relevance concerning the physical realm is the manner in which physical events, states of affairs and other physical phenomena are conceptualised by the individuals in their action:

In fact, most of the objects of social or human action are not "objective facts" in the special narrow sense in which this term is used by the Sciences and contrasted to "opinions", and they cannot at all be defined in physical terms. So far as human actions are concerned the things *are* what the acting people think they are. (p. 44)
 The point is that [definitions of instruments such as hammers] are abstractions from *all* the physical attributes of the things in question and that their definitions must run *entirely* (my emphasis - T.L.) in terms of mental attitudes of men toward the things. (p. 46)

What is relevant in the study of society is not whether these laws of nature are true in any objective sense, but *solely* (my emphasis again - T.L.) whether they are believed and acted upon by the people. (p. 51)

The upshot then, if to repeat, is that social science, according to Hayek cannot be concerned with explaining social action, but is restricted to understanding. The only conceivable issue of explanatory interest at the level of action is why it is that human agents see any given thing or phenomenon in the way they do, and in particular in the same way as each other, even if they do not conceive of it as it really is. But such matters, Hayek emphasises, are questions for psychology if they can be addressed at all. For the social sciences including economics, the beliefs, desires and so conscious actions of individuals should be interpreted not as matters to be explained but as items to be grasped and understood, constituting data that, once accessed, are then only to be arranged. The sole task which then remains for the social scientist is to use such data to compose out the undesigned results of the sum total of such conscious actions:

It is important to observe that in all this the various types of individual beliefs or attitudes are not themselves the object of our explanation, but merely the elements from which we build up the structure of possible relationships between individuals. In so far as we analyze individual thought in the social sciences the purpose is not to explain the thought but merely to distinguish the possible types of elements with which we shall have to reckon in the construction of different patterns of social relationships. It is a mistake, to which careless expressions by social scientists often give countenance, to believe that their aim is to *explain* conscious action. This, if it can be done at all, is a different task, the task of psychology. For the social sciences the types of conscious action are data and all they have to do with regard to these data is to arrange them in such orderly fashion that they can be effectively used for their task. (p. 68)

Because for Hayek, moreover, conscious actions and individual conceptions are effectively identified with each other, he talks of being able to determine the undesigned effects of human action, of coming to know society, by building it out of the concepts and ideas that people hold:

Not only man's action toward external objects but also all the relations between men and all the social institutions can be understood only by what men think about them. Society as we know it is, as it were, built up from the concepts and ideas held by the people; and social phenomena can be recognized by us and have meaning to us only as they are reflected in the minds of men. (p. 57)

The outcome is that Hayek's achievement in his *Scientism* essay is not so much a transcendence of positivism and its errors as a sideways shift of it all, as it were; a move towards a subjectivised version. Thus, in place of the 'brute facts' of positivism we find in Hayek's limited focus for social science, in effect, the brute opinions, beliefs and attitudes of hermeneutical foundationalism. The self-evidence of the empirical world in positivism is reflected in the self-characterisation of the social world in hermeneuticism; the empiricist foundations of the positivistic natural sciences are translated into the conceptual foundations

of Hayek's hermeneuticised scientism. In short, most of the errors of positivism are not so much overcome by Hayek as reproduced in a subjective or hermeneuticised key.

Now, the hermeneutical tradition, of course, is correct to point out that social science deals with a pre-interpreted reality, a reality already brought under concepts by individual agents. It is also correct to insist, as Hayek does in the *Scientism* essay, that the methodological implications of this be elaborated and acted upon in science. Its error, however, lies in reducing social reality to these concepts, and so social science to little more than their unproblematic grasping. Not only does such a project neglect the physical side, including the material embeddedness, of society and economy, it fails to comprehend that aspects of reality can be *both* social and yet inadequately conceptualised.

The source of all these errors, of course, lies in a continued commitment to an empiricist ontology albeit including, or augmented to include, the concepts and beliefs of others. The solution, then, comes, as I have argued elsewhere, and arguably as Hayek himself eventually realises, with an acceptance of a structured ontology; with acknowledging the reality of structures irreducible to events, including social structures that are dependent upon, but irreducible, to the concepts and actions of human agents. In contrast to hermeneuticism such a position can sustain not only the existence of beliefs, meanings and attitudes independently of their analysis, but can situate the possibility of, and indeed facilitate, their scientific explanation and critique.

Keynes and Hayeks ontological presuppositions are not the same

If I do not want to give the impression that (given the hindsight of recent developments in the philosophy of science) the writings of Hayek and Keynes can be seen to be free of error, equally nor do I wish to imply that, in their realist theorising, Hayek and Keynes are at one. Despite some suggestions by Shackle to the contrary, I think it highly unlikely, for example, that Keynes ever embraced the 1940's radical subjectivism of Hayek. Indeed, although Keynes was never as careful and systematic in setting out his thinking as Hayek, it is easy enough to interpret (at least parts of) his 1936 *General Theory* as resting on the sort of structured ontology argued for in recent philosophy of science.

Consider, for example, chapter 12 entitled the *State of Long Term Expectations*. Here Keynes focuses upon some phenomenon at the level of actual events that is warranting of explanation, namely volatility in the level of physical investment, and sets about attempting to identify the structural factors responsible. Relating this volatility to the pattern of revaluing 'old investments' on the Stock Exchange, Keynes explains the latter in terms of 1) the manner in which modern investment markets are structured in order to facilitate significant 'liquidity', 2) the prevalence of psychological motivations, and 3) the structure of future uncertainty, (Lawson, 1995b). Only much later (if at all) does Hayek appear to allow such an explanatory role for structures including rules and/or psychological motivations explicitly.

Changing Theories

A further impression that I am anxious not to be seen as encouraging is that the theories of Keynes and Hayek on matters of ontology remain constant overtime. To the contrary, Hayek and Keynes came to revise some of even their most fundamental beliefs.

In the case of Keynes, for example, a feature that dominated much of his early work, and in particular his theorising in social ontology, is the rationality of individuals (often in conditions of significant uncertainty). So much is the emphasis upon rationality that Hodgson interprets Keynes' position as a "rationalist conception of action", and concludes that:

Keynes' theory involves the assumption that actions of economic agents are, as far as possible, governed by reason and calculation In his work, reason and

calculation we highlighted; rational agency is over stressed with the neglect of habit and institutions. (Hodgson, 1988, p. 226)

Certainly, in his *A Treatise on Probability* Keynes states his overall aim variously as analyzing: "the actual exercise of reason", the "character and the justification of rational argument", the "actual processes of valid reasoning" and "the methods of reasoning we actually employ" (CW-VIII 3, p. 129, p. 134-5), as if, by and large, these statements amount to one and the same thing.

Yet, later in life, as is clear from his autobiographical essay of 1938, Keynes is quite happy to accept that certain aspects of the conception of human nature which he initially held, carry with them an unrealistic attribution of rationality. He even concludes that it is no bad thing that agents are not always rational, suggesting that good or 'value' may result from apparently irrational outbursts:

As cause and consequence of our general state of mind we completely misunderstood human nature, including our own. The rationality we attributed to it led to a superficiality, not only of judgment, but also of feeling. It was not only that intellectually we were pre-Freudian, but we had lost something which our predecessors had without replacing it. I still suffer incurably from attributing an unreal rationality to other people's feelings and behaviour (and doubtless to my own, too). There is one small but extraordinarily silly manifestation of this absurd idea of what is 'normal', namely the impulse to *protest* - to write a letter to *The Times*, call a meeting in the Guildhall, subscribe to some fund when my presuppositions as to what is 'normal' are not fulfilled. I behave as if there really existed some authority or standard to which I can successfully appeal if I shout loud enough - perhaps it is some hereditary vestige of a belief in the efficiency of prayer.

I have said this pseudo-rational view of human nature led to a thinness, a superficiality, not only of judgment, but also feeling. It seems to me that Moore's chapter on 'The Ideal' left out altogether some whole categories of valuable emotion. The attribution of rationality to human nature, instead of enriching it, now seems to me to have impoverished it. It ignored certain powerful and valuable springs of feeling. Some of the spontaneous, irrational outbursts of human nature can have a sort of value from which our schematism was cut off. (1972b, p. 448)

If Keynes' change of mind here is significant, Hayek's continuously changing course of development is even more so. Having abandoned naive positivism for subjectivism in the *Scientism* essay - itself following on from his 1937 essay 'Economics and Knowledge' in which Hayek raises various problems for orthodox theorising - it is arguable that, in the years which followed, Hayek came to accept the sort of structured ontology outlined earlier. That is, although Hayek's theory of knowledge as elaborated in his *Scientism* essay does not allow him to accept a reality of social structures existing apart from their being conceptualised in action, in his later work things are viewed differently. Thus, by the late 1960's, for example, we find Hayek writing explicitly of "the knowledge that rules exist in the objective world and a disinclination [and so implicitly a real possibility] to deviate from rules commonly followed" (1967, p. 79). It is also clear that action is no longer considered always to be discursively conscious but conditioned by tacit knowledge if amongst other things. Thus we can find Hayek concluding that we learn as children to use language "according to rules that we do not explicitly know" (1967, p. 87); and that in interpreting the world and acting appropriately we draw upon "rules that will guide us though we have never explicitly formulated them". (p. 87). It follows, of course, "that our conscious reason can therefore always take account of only some of the circumstances that determine our action", (1967, p.

87); so that the various conditions of action are issues that need to be identified, that action is something that we can and should attempt to explain.

And, like Keynes, Hayek is explicit in acknowledging his important changes. Thus, in the 1960s, Hayek acknowledges the movement away from positivism and 'technical economics' signaled by his 1937 paper on 'Economics and Knowledge':

... although at one time a very pure and narrow economic theorist, I was led from technical economics into all kinds of questions usually regarded as philosophical. When I look back, it seems to have all begun, nearly thirty years ago, with an essay on "Economics and Knowledge" in which I examined what seemed to me some of the central difficulties of pure economic theory. Its main conclusion was that the task of economic theory was to explain how an overall order of economic activity was achieved which utilized a large amount of knowledge which was not concentrated in any one mind but existed only as the separate knowledge of thousands or millions of different individuals. But it was still a long way from this to an adequate insight into the relations between the abstract overall order which is formed as a result of his responding, within the limits imposed upon him by those abstract rules, to the concrete particular circumstances which he encounters. It was only through a re-examination of the old-age concept of freedom under the law, the basic conception of traditional liberalism, and of the problems of the philosophy of the law which this raises, that I have reached what now seems to me a tolerably clear picture of the nature of the spontaneous order of which liberal economists have so long been talking. (Hayek, 1967c, pp. 91-92)

And in his last book, published a few years before his death, Hayek explicitly acknowledges a change in his thinking on the "question of how our knowledge really does rise". He writes:

Most knowledge - and I confess it took me some time to recognise this - is obtained not from immediate experience or observation, but in the continuous process of sifting a learnt tradition, which requires individual recognition and following of moral traditions that are not justifiable in terms of the canons of traditional theories of rationality (Hayek, 1988, p. 75).

If Different then not Entirely Oppositional

Finally, by stressing the fallible nature, peculiarities, and transient character of the contributions of Hayek and Keynes, I in no way wish to imply their total differentiation. Rather, my perception is that both moved broadly in the direction of the sort of structured ontology that recent philosophy of science has found to be sustainable. In fact, I would suggest that Keynes and Hayek, like Marx, but unlike most contemporary economists, came to view the economy as an irreducible holistic system. Differences, in the end, lay much more in beliefs about possibilities of achieving radical improvements to the prevailing system of capitalism. Keynes seems the more optimistic about changing the system. But even here you have to wonder whether Keynes really ever entertained the hope of bringing about the sort of changes his reasoning led him to advocate. Consider once more his analysis in chapter 12 of the *General Theory*, and in particular his assessment that the problem of investment volatility is due to extensive speculation caused by the existence of organised liquid investment markets:

Speculators may do no harm as bubbles on a steady stream of enterprise. But the position is serious when enterprise becomes the bubble on a whirlpool of speculation. When the capital development of a country becomes a by-product of the activities of a casino, the job is likely to be ill-done. The measure of success attained by Wall Street ... cannot be claimed as one of the outstanding triumphs of

laissez-faire capitalism - which is not surprising, if I am right in thinking that the best brains of Wall Street have been in fact directed towards a different object.

These tendencies are a scarcely avoidable outcome of our having successfully organised "liquid" investment markets. (1973a, p. 159)

In the light of this reasoning, Keynes is moved towards suggesting that investment markets in effect become less liquid: 'It is usually agreed that casinos should, in the public interest, be inaccessible and expensive. And perhaps the same is true of stock exchanges' (*ibid.*: p.159). To achieve this, he reasons, either the transactions cost of transferring investments might be raised sufficiently high so as to make that activity prohibitive, or the ownership of an investment might be made literally indissoluble:

The introduction of a substantial government transfer tax on all transactions might prove the most serviceable reform available, with a view to mitigating the predominance of speculation over enterprise in the United States.

The spectacle of modern investment markets has sometimes moved me towards the conclusion that to make the purchase of an investment permanent and indissoluble, like marriage, except by reason of death or other grave cause, might be a useful remedy for our contemporary evils. For this would force the investor to direct his mind to the long-term prospects and to those only. (GT: 160)

The problem with such a policy proposal is that the 'liquidity' which encourages a swing from enterprise to speculation, may be equally facilitative of the activity of financial investment in the first place, and a condition for new investment in financial assets to be forthcoming:

For the fact that each individual flatters himself that his commitment is "liquid" (although this cannot be true for all investors collectively) calms his nerves and makes him much more willing to run a risk. If individual purchases of investments are rendered illiquid, this might seriously impede new investment, so long as *alternative ways* in which to hold his savings are available to the individual. This is the dilemma. So long as it is open to the individual to employ his wealth in hoarding or lending *money*, the alternative of purchasing actual capital assets cannot be rendered sufficiently attractive (especially to the man who does not manage the capital assets and knows very little about them), except by organising markets wherein these assets can be easily realised for money. (1973a, p. 160)

Not illogically Keynes next entertains the possibility that the 'only radical cure' is to allow the individual no choice except between consuming his or her income or ordering the production of some capital assets (1973a, p. 161); and so on. I wonder how serious these considerations really are - as suggestions for possible policy action.

If Keynes does, however, seem the more certain with regard to the need, and optimistic with regard to possibilities, of reforming the capitalist system (and Marx of course argues that the system of capitalism contains its own seeds of destruction within), Hayek, it warrants emphasis, does not claim that capitalism is the best of all possible worlds. Rather his view is that it is a workable system which has evolved over a very long period of time, and that it would be arrogant as well as foolish for any individual to propose that they knew how to structure, and/or bring about, a more desirable one.

Such examples of systems-thinking have their commonalties - especially in both the questions they pose regarding the appropriate procedures for social scientific analysis and the problems they raise regarding the possibilities of capable and acceptable social transformation. Certainly such considerations serve to differentiate the positions of Hayek and Keynes from the atomistic theorising of contemporary mainstream economists. If it is debatable how much agreement there is in the eventual ontological theories of Hayek and

Keynes, it is an obvious and significant commonality that both, in the end, had little time for that contraction which continues to pass as mainstream economics.

Final Comments and Conclusion

An alternative way of putting the arguments of this paper is that, in their reflections upon proper (and improper) scientific practice, Hayek and Keynes seek to avoid a particular fallacy which is committed by the majority of modern economists. This mistake, termed the epistemic fallacy by Bhaskar (1978), consists in the view that statements about being can be reduced to, or analyzed solely in terms of, statements about knowledge, that matters of ontology can always be translated into epistemological terms. Hume encouraged such a position by using the category of experience to define the world - thus giving an epistemological category an ontological task. In some recent post-modernist writing such a task has been assigned to language or conversation or the text of some such, so that the error might be rephrased as the linguistic fallacy. In mainstream economics the insistence on conceptualising reality according to what can be treated using the formalistic procedures of deductivist reasoning (assumed to predominate in the natural sciences) encourages the label of the formalistic fallacy.

But however we label the mistake in question, the point here is that Hayek and Keynes stand out for having in the main avoided it. In making this argument I have been keen to indicate the peculiarities, fallibility and transient nature of the respective contributions. Indeed, such factors, must always be aspects of accepting a realist orientation (Lawson 1994a). I do though see similarities in the progression of both Hayek and Keynes away from atoms towards a structured ontology, at least with regards to their conception of the social realm.

What then do we learn from Hayek and Keynes? Clearly the position I want to encourage is that social science follows their example and continually modifies its methods and practices in the light of better understanding of social material - always remembering that our current state of knowledge is but a contingently valid, fallible set of insights on the path to a better understanding. Of course, put like this, the conclusion seems almost banal, as indicating an obviously desirable goal that no-one would want to dispute. Yet seriously embraced, it entails that the overwhelming bulk of contemporary academic economics be abandoned. It is precisely because of the nature of the modern situation, the all pervasiveness of its unthinking approach to explanation, that the very simple point of the paper warrants repeating; that the identified commonality of Hayek and Keynes deserves more explicit attention.

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Note

¹ For more extended discussions of this conception, which is due to the work of Bhaskar in particular, see for example, Bhaskar (1978) or Lawson (1989a, 1994a). For recent developments which focus on the work of Hayek see also Fleetwood (1994) and Peacock (1993).

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