

## Social Explanation and Popper

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Lecture given at the Popper Anniversary Conference  
University of Galway, September 2002

I want to consider the nature of one possible answer to an important question. For those of us engaged in social explanatory endeavour it may even be the most important research question we face. Certainly this appears to be the case from the perspective of the project to which I (and numerous others<sup>1</sup>) have been contributing in recent years. As formulated, though, it is a question that Popper seems never quite to have addressed despite his interest in social explanation. However the nature of the particular answer to it on which I shall focus is, I think, very Popperian indeed, both because it is in the broad spirit of Popper's writings and also because Popper provides many (though not quite all) of its central ingredients.

A minor, or supplementary, question I wish to address is whether the current orientation to social explanation in modern economics warrants the label Popperian. Just as Popper often mentioned how his contribution to social explanation drew on economics, the few practising economists who give a nod to the philosophy of social science tend to describe themselves, and or view modern mainstream economics, as being within the Popperian tradition. In concluding this paper I briefly examine the appropriateness of this association<sup>2</sup>.

### **The question**

So what, first of all, is the primary question to be addressed here, the one that I take to be so important? It is the following. *How can social explanatory work proceed in an open system context that lacks the possibility of experimental intervention?* Let me briefly explain my terms and indicate why I believe the question to be such an important one. Actually, I shall suggest that although Popper does not pose the question precisely as stated it may have been high on his agenda during in the last years of his life. For it practically jumps out of his very last contributions. Certainly I think it to be a question the pursuit of which the later Popper would endorse. Eventually, I will, as I say, propose a somewhat 'Popperian' answer to it.

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<sup>1</sup> See especially the contributions in Fleetwood, 1999.

<sup>2</sup> What I cannot do is explore the details of the many ways in which Popper has influenced methodological discussion in economics. But for a sample of the literature the reader might consult Blaug, 1980, 1985, 1990; Boland, 1982; Caldwell, 1984; Hands, 1985, 1991, 1992; Hausman, 1985, 1988; Hutchison, 1956; Latsis, 1972, 1976, 1983; de Marchi, 1988a, 1998b.

## Ontology

The first point to make is that the question just noted arises as a result of ontological investigation. That is, it arises through a study of the nature of social reality, or social being. A concern with ontology is very much the emphasis of the (realist) project with which I have been involved for many years now, one often systematised as *critical realism*<sup>3</sup>. For it is a fundamental tenet of this project that the nature of the material we study bears significantly on how we can study it. All methods presuppose an implicit ontology. That is, all methods are appropriate for some conditions but not others. And all materials are such that they can be usefully investigated by some methods but not others. As Karl Marx once observed "in the analysis of economic forms neither microscopes nor chemical reagents are of assistance" (Capital, vol I, p. 90). But the point is a general one<sup>4</sup>. The particular nature of the material of any sphere of reality makes a difference to how (if at all) we can come to know it.

Now I take the social realm to be that domain of phenomena whose existence depends at least in part on us (a realm which, I take it, includes [but I suspect is not exhausted by] Popper's World 3<sup>5</sup>). And

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<sup>3</sup> I am not the first economists to be comparing critical realism and Popper: see Runde, 1996.

<sup>4</sup> From this perspective social scientists cannot just take natural scientific methods and apply them to the social realm. The usefulness of borrowing from the natural sciences depends on the nature of the methods and the subject material to be investigated. Popper also seems to accept this latter orientation to ontology, at least here and there. Although in modern economics Popper is mostly interpreted as being concerned with methods by which theories are to be rationally appraised, he occasionally explicitly acknowledges that how we can proceed depends on what we are dealing with. Thus for example he writes as early as 1936 that:

"Whether a [social-scientific] student of method upholds anti-naturalistic or pro-naturalistic doctrines, or whether he adopts a theory combining both types of doctrine, will largely depend on his views about the character of the science under consideration, and about the character of its subject matter" (Popper, 1936, p. 290).

<sup>5</sup> Popper writes:

"By 'World 3' I mean the world of the products of the human mind. Although I include works of art in World 3 and also ethical values and social institutions (and thus, one might say, societies), I shall confine myself largely to the world of scientific libraries, to books, to scientific problems, and to theories, including mistaken theories" (Popper, 1982, p. 114)

He continues two pages further on:

"The proposition the truth of which I wish to defend and which seems to me to go a little beyond common sense is that not only are the physical World 1 and the psychological World 2 real but so also is the abstract World 3; *real* in exactly that sense in which the physical World 1 of rocks and trees is real: the objects of World 2 and of World 3 can kick each other, as well as the physical objects of World 1; and they can also be kicked back" (Popper, 1982, p. 116).

according to the conception I defend (see e.g., Lawson, 1997a, 2003) social reality is (found to be) in a fundamental sense *open* (which is why, of course, my question above is formulated in such terms). To understand what I mean by open let me first define a closed system. The latter is one in which regularities of the form 'whenever event (or state of affairs)  $x$  then event (or state of affairs)  $y$ ' (or stochastic near equivalents) occur. Closures are conditions in which correlations hold, in which we find sufficiently strict (deterministic or probabilistic) patterns at the level of *actual* phenomena such as events and states of affairs. The thesis that reality consists only of closures, that all outcomes can, in effect, be accounted for using techniques of correlation analysis, can be referred to as *regularity determinism*<sup>6</sup>. And all forms of explanation which rest on the necessity of positing such closures (typically in conjunction with the stipulating of initial conditions) can be referred to as *deductivist*. Clearly, on this conception deductivism covers most examples of modern micro, macro and econometrics. All are forms of deductivist (closed-systems) modelling.

By interpreting a system as open I mean any that is not closed in the sense described. In an open system, not all events are predictable. It is a system where a knowledge of past events does not of necessity allow any inference as to future events that must come about. And by interpreting the social realm as fundamentally open I mean also to suggest that it is hardly amenable to local closure whether experimentally determined or spontaneous.

However, it is a further feature of the conception I defend that social reality is (found to be) not only open in the manner described but also *structured*. That is, it comprises not only *actualities* such as actual events and states of affairs (some of which we may directly experience) but also deeper structures, powers, mechanisms, and tendencies, etc., which produce, facilitate or otherwise condition these actualities. Tendencies here are a bit like forces. The category expresses the ways things act (irrespective of the *actual* outcome). Gravitational tendencies draw autumn leaves to the ground (or leaves and the ground to each other) even *as* counteracting forces of the wind or aerodynamic tendencies

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<sup>6</sup> Or, as Popper preferred to call it, *scientific determinism*:  
 " '[S]cientific' determinism, that is to say, the doctrine that the structure of the world is such that any event can be rationally predicted, with any desired degree of precision, if we are given a sufficiently precise description of past events, together with all the laws of nature" (Popper, 1982, 1-2).

help send them flying over roof tops and chimneys. So actual events are the result of numerous causal forces or tendencies. That, in part, is why the present and future are open. For the context always affects the outcome and the future context is everywhere not yet determined.

This assessment that reality is so structured (i.e., that it is, in the manner described, irreducible to the actual course of events) immediately guides us in the direction of causal explanatory research or *causal explanation*. For whether or not given phenomena are correlated with others at any one level of social reality, they can be *explained* in terms of (meaning shown to have been produced or facilitated by) their underlying causal structures and conditions. So the ontological conception I defend directs us towards considering how, in economics, we might conduct causal explanatory projects.

Even an analysis of the well-controlled experiment leads us to this conclusion. I say 'even' just because the controlled laboratory situation is the one to which empiricists and others often point when arguing to the contrary that the identification of event regularities is essential to science. For event regularities are regularly associated with experimental work even if they are rare beyond the experimental confines. However this acknowledged restriction of event regularities to controlled experimental conditions warrants explanation. So too does the further observation that experimental findings are often successfully observed outside of experimental conditions where event regularities are rarely to be found.

The explanation is just that the experimental situation is a humanly engineered contrivance in which single (set of) causal mechanisms of interest are insulated from countervailing factors. Any even regularity produced corresponds to the empirical identification of the mechanism; it correlates the triggering event with the mechanism's undisturbed effects. The point here is that even the controlled experiment is not concerned with any event regularity produced *per se*, but with what it serves to identify: an isolated underlying causal mechanism. And of course we can make sense of the successful application of experimental results in non-experimental conditions where event regularities do not occur just by recognising that these results relate to the workings of the mechanism and not the regularity through which it is experimentally revealed. I return to these considerations in due course. For the time being it is sufficient that we recognise that even the experimental situation, when adequately reflected

upon, is found to support the idea that science is ultimately concerned with causal explanation.

Now I believe the conception of social reality I have set out, though not identical to, is very much in line with, that developed by Popper, at least towards the end of his life. I refer in particular (although not only) to the two lectures contained in his *A World of Propensities*, published in 1990, just four years before his death. Before considering this later work, however, I must acknowledge that, prior to it, Popper's emphasis very much reveals *not* a presumption that reality, and in particular social reality, is open, but rather an acceptance that it is reasonably closed, that event regularities do exist. It is Popper's earlier position I want to examine first.

### **Popper and closed systems**

My assessment, as I say, is that early on Popper mostly regards reality as more or less everywhere closed, or at least closable. Thus in the 1940s Popper talks of:

"...a really fundamental similarity between the natural and the social sciences. I have in mind the existence of sociological laws or hypotheses which are analogous to the laws or hypotheses of the natural sciences. Since the existence of such sociological laws or hypotheses ... has often been doubted, I will give a number of examples: 'You cannot introduce tariffs and at the same time reduce the cost of living.' - 'You cannot, in an industrial society, organise consumers' pressure groups as effectively as you can certain producers' pressure groups.' - 'You cannot have a centrally planned society with a price system that fulfils the main function of competitive prices.' - 'You cannot have full employment without inflation' (Popper, 1944, p. 307).

Despite the impression given that these examples took some effort by Popper in constructing, it is easy enough to suggest counterexamples to all of them (tariffs can be introduced as a country joins a trading block which gives massively expanded markets facilitating scale economies and, perhaps simultaneously, even bringing subsidies from centralised resources; advances in [especially communications] technology [including those yet to happen] facilitate all sorts of previously unimagined possibilities including many for (i) organising groups of every kind as well as (ii) other forms of planning; and currently in Cambridge UK [according to some accounts and depending on our categories] price inflation [excluding housing] and zero unemployment effectively coexist). The point, though, is that Popper clearly posited closures in his earlier years and seemed almost to imply he regarded them a common place.

It is relevant, too, that closures appear to be a presupposition of his (later) method of *situational*

*analysis* which Popper devised specifically for the social realm. It was Popper's view in setting out this approach that those sciences which study social phenomena cannot explain or predict 'singular events' only kinds or types of event. The point then, according to Popper, is to construct models of typical situations to see, in effect, the general manner in which social events could have occurred.

Popper's idea of a model is clear when he considers examples for the natural realm, and specifically lunar eclipses. He talks of making perspective drawings, or using a lamp for the sun, a wooden earth, and so forth. He is really looking to understand a *type* of mechanism. Even the initial conditions involved are described only as *typical* initial conditions (1967, p. 358). To see how the earth and moon move in reality, however, we further need to 'animate' the model. Here Newton's laws of motion are called upon. But basically, the model is explanatory of *how* phenomena can come about.

The trick for the social realm, Popper believes, is to construct models of typical social *situations*:

"The fundamental problem of both the historical and the social sciences is to explain and understand events in terms of human actions and social situations. The key term here is 'social situation'.

The description of a concrete historical *social situation* is what corresponds in the social sciences to a statement of *initial conditions* in the natural sciences. And the '*models*' of the theoretical social sciences are essentially descriptions or reconstructions of typical situations" (Popper, 1994, p. 166)

Popper is here setting out the conditions for his method of *situational analysis*. Models constructed according to it are animated by the *rationality principle*, by the assumption that people always act in a manner appropriate their situations:

"As for the social sciences ...we can construct our models by means of *situational analysis*, which provides us with models (rough and ready ones to be sure) of typical social situations. And my thesis is that only in this way can we explain and understand what happens in society: social events.

Now if situational analysis presents us with the model, the question arises: what corresponds here to Newton's universal laws of motion which, as we have said, 'animate' the model of the solar system? In other words, how is the model of a social system 'animated'?

...it is the central point of situational analysis that we need, in order to animate it, no more than the assumption that the various persons or agents involved act *adequately, or appropriately*; that is to say in accordance with the situation. Here we must remember, of course, that the situation, as I use the term, already contains all the relevant aims and all the available relevant knowledge, especially that of possible means for realising these aims.

Thus there is only one animating law involved -- the principle of acting appropriately to the situation..." (Popper, 1967, p. 359).

These models assume, in effect, closed systems, rendering the explanatory schema as a whole a form of deductivism (see e.g. Caldwell, 1991; Runde, 1996; Koertge, 1975, 1979), one systematised by Spiro Latsis as *situational determinism* (Latsis, 1976).

An example of a supposed social event regularity Popper explicitly acknowledges at this stage is "the theory of profit maximisation, [whereby] the businessman maximises his (monetary) profits by a policy of marginal cost pricing" (1994, p. 182). But basically, in situational analysis, the situation of the agent is assumed to be of a sort that there is but one "adequate or appropriate" course of action, and the agent is assumed to take it. In essence the model, including (or along with) the 'rationality principle', provides the closures or (set of) event regularity(ies), whilst a description of a given situation constitutes the initial conditions (always required in deductivist explanation). Putting the two aspects together, predictions are deduced regarding individual behaviour. In this way the models are tested. Popper acknowledges that the process may not always be clear cut. But the method allows deductive testing after a fashion:

"Tests of a model, it has to be admitted, are not easily obtainable and are not usually very clear-cut. But this difficulty arises even in the natural sciences. It is connected, of course, with the fact that models are always and necessarily somewhat rough and schematic over-simplifications. Their roughness entails a relatively low degree of testability. For it will be difficult to decide whether a discrepancy is due to the unavoidable roughness, or to a mistake in the model. Nevertheless we can sometimes decide by testing, which of two competing models is the better" (Popper, 1994, p. 170)

If Popper thought in 1967 and earlier that deductivist testing of this sort was less than clear-cut his ontological assessments of later life likely (and certainly should have) led him to be more cautious still, to the point, I think, of abandoning this approach more or less entirely. Let me now turn to his later writings and in particular his *A World of Propensities*.

### **Propensities and open systems**

Popper's central category here, as the title of his book leads us to expect, is the *propensity*. The category was not entirely new to him at this point in time of course. For in 1956 Popper had published his 'propensity theory of probability'<sup>7</sup>. But in 1990, on recalling this earlier work, Popper writes:

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<sup>7</sup> For a good discussion see Runde, 1996.

"This theory has further grown so that it was only in the last year that I realised its cosmological significance. I mean that we live in a *world of propensities*, and that this fact makes our world both more interesting and more homely than the world as seen by earlier states of the sciences" (Popper, 1990, p. 9).

Popper explains:

"Propensities, it is assumed, are not mere possibilities but are physical realities. They are as real as forces, or fields of forces. And vice versa: forces are propensities. They are propensities for setting bodies in motion. Forces are propensities to accelerate, and the fields of forces are propensities distributed over some region of space and perhaps changing continuously over this region (like distance from some given origin). Fields of forces are fields of propensities. They are real they exist" (Popper, 1990, p. 13)

There is scope for some confusion here. Unlike the categories of my own project, Popper's propensities are aspects not of structures or objects but of situations. They are 'objective probabilities'. Where a cause acts in isolation to produce its direct effect we have the propensity 1. Where countervailing forces act in competition with some other causes Popper seems to suggest that the propensity of its typical effect being actualised is less than 1:

"The propensity 1 is the special case of a classical force in action: a cause when it produces an effect. If a propensity is less than 1, then this can be envisaged as the existence of competing forces pulling in various opposed directions but not yet producing or controlling a real process. And whenever the possibilities are discrete rather than continuous, these forces pull towards distinct possibilities, where no compromise may exist. And zero propensities are, simply, no propensities at all, just as the number zero means 'no number'" (Popper, 1990, p. 12).

I am not entirely convinced by Popper's emphasis on (or use of the category) propensities here. But the important point is that in developing his account Popper reveals an attachment to a conception of reality inclusive of forces and countervailing forces, including those that shift. The result is a perspective in which reality is viewed as open, where the thesis I have stylised *regularity determinism* is rejected as false<sup>8</sup>:

"Now, in our changing real world, the situation and, with it, the possibilities, and thus the propensities, change all the time....Our very understanding of the world changes the conditions of the changing world....All this amounts to the fact that *determinism is simply mistaken*: all its traditional arguments have withered away and indeterminism and free will have become part of the physical and biological science" (p. 17)

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<sup>8</sup> Popper, I think, always did reject determinism. But in the earlier period he seemed optimistic that rough and ready forms of deductivist explanation would be adequate.

A few lines after providing examples of features of reality that fit with his theory, Popper adds:

"In all these cases the propensity theory of probability allows us to work with an *objective* theory of probability. Quite apart from the fact that we do not *know* the future, the future is *objectively not fixed*. The future is *open: Objectively open*. Only the past is fixed; it has been actualised and so it is gone. The present can be described as the continuing process of the actualisation of propensities; or, more metaphorically, of the freezing or the crystallization of propensities. While the propensities actualize or *realize* themselves, they are continuing processes.....

Propensities, like Newtonian attractive forces, are invisible and, like them, they can act: they are *actual*, they are *real*... (Popper, 1990, p. 18 -- emphasis in the original).

Or as Popper writes in the *Introduction* to his collection of essays systematised as *The Myth of the Framework*<sup>9</sup>:

"The future is open. It is not predetermined and thus cannot be predicted -- except by accident. The possibilities that lie in the future are infinite" (1994, p. xiii)

The question that clearly arises is how in such a world we do explanatory work. Popper does not get around to considering this. When, in his *A World of Propensities*, he does turn to questions of method he focuses only on theoretical physics. Here he basically notes that in some cases laboratory experimentation is possible (these are situations he associates with "natural laws of a deterministic character" [p. 22]) and in others it is not. Only in the case of planetary movements do spontaneous closures or naturally occurring laboratory-like situations occur:

"Only the system of our planets is so well isolated from all the extraneous mechanical interference that it is a unique, natural laboratory experiment. Here, only the *internal* disturbances interfere with the precision of Kepler's laws...

In most laboratory experiments we have to exclude many disturbing extraneous influences such as change of temperature or the normal moisture of air. Or we may have to create an artificial environment of extreme temperatures -- say, near to absolute zero....

But what does all this show us. It shows us that in the non-laboratory world, with the exception of our planetary system, no strictly deterministic laws can be found. Admittedly, in certain cases such as the planetary movements, we can interpret events as due to the vectorial sum of forces that our theories have isolated. But in an actual event such as, say, the fall of an apple from a tree, this is not the case. Real apples are emphatically not Newtonian apples. They fall usually when the wind blows. And the

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<sup>9</sup> Or as he writes elsewhere :

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This is of fundamental importance; for it shows that nature, or the universe to which we belong, and which contains as parts the Worlds 1, 2, and 3, is itself open; it contains World 3, and World 3 can be shown to be *intrinsically open* (Popper, 1982, p. 129).

whole process is initiated by a biochemical process that weakens the stem so the often repeated movement due to the wind, together with the Newtonian weight of the apple, lead to a snap of the stem - a process we can analyse but cannot calculate in detail,...(Popper, 1990, p. 24)

Actually, it is possible to *explain* why experimental work is mostly impracticable in the social realm, although Popper does not go so far as doing so. I mentioned earlier that Popper's (implicit) social ontology and that which I defend are rather similar in viewing reality as open and structured. The conception I argue for elsewhere does, however, go somewhat further than that of Popper in portraying social reality as also highly *internally related*, and intrinsically *processual*, amongst other things. By internally related I mean that aspects of reality are what they are can do what they do in virtue of the relations in which they stand to others. Students and teachers (or the positions in which they stand) are internally related to each other, as are (the positions of) employers and employees, landlords and tenants, and so on. The result is a holistic conception that cannot easily be carved up into isolatable atomistic bits. By saying social reality is intrinsically processual I mean that its very nature is that of process. It does not first exist and then experience change; change is intrinsic to its mode of being. Think of language. We do not create it, for it precedes us. But nor is it fixed and determining of what we say. Rather it both conditions our speech acts and also is reproduced or transformed through them. Being continually reproduced or transformed is essential to its mode of being. But on reflection we can see this is true of all aspects of social structure.

So we are left wondering how explanatory endeavour might proceed in a social realm which, *prima facie* at least, is not at all similar to the planetary system, and wherein controlled experimentation seems hardly feasible. Given the fundamental openness of the social system and the recognition that the method of situational logic or situational analysis requires local closures, it is difficult to discern how we can proceed.

We arrive once more, then, at the conclusion noted earlier, that the nature of society is such as to be not amenable to study through either controlled experimentation or Popper's situational analysis. Popper I think had reached a point where he would have accepted this. If not then, as I say, the comments he made, supplemented by the ontological conception sketched above and defended elsewhere, suggests he should have accepted this. One way or another, in any case, we arrive at the

question posed at the outset, and singled out as rather important. How might social explanatory work proceed in an open system context that lacks the possibility of experimental intervention?

### **Critical rationalism**

One answer to this question, I now want to suggest, rests heavily on adopting an orientation similar to Popper's *critical rationalism*. The latter is the name given to Popper's long-standing stipulation that a critical orientation is essential to science, an orientation that requires the scientist to seek for errors and to learn from them. This, of course, is where Popper and critical realism come together most easily. It is not for nothing that both projects identify themselves explicitly with the term *critical*.

Now the critical perspective of which I talk is not at all a recent one for Popper. Although he earlier mostly emphasised versions of it appropriate to closed systems, namely falsificationism and (later) situational analysis, and although these are the only versions most economists (at best) have tended to notice (see especially Boland [e.g. Boland, 1997] on this), his critical rationalism has been present throughout. For example, in 1944 we find Popper arguing as follows:

"According to this piecemeal view, there is no clearly marked division between the pre-scientific and the scientific experimental approaches, even though the more conscious application of scientific, that is to say, of critical methods, is of great importance. Both approaches may be described, fundamentally as utilizing the method of trial and error. We try; that is, we do not merely register an observation, but make active attempts to solve some more or less practical and definite problems. And we make progress if, and only if, we are prepared to *learn from our mistakes*: to recognise error our errors and to utilize them critically instead of persevering in them dogmatically. Though this analysis may sound trivial, it describes, I believe, the method of all empirical sciences. This method assumes a more and more scientific character the more freely and consciously we are prepared to risk a trial, and the more critically we watch for the mistakes we always make. And this formula covers not only the method of experiment, but also the relationship between theory and experiment. All theories are trials; they are tentative hypotheses, tried out to see whether they work; and all experimental corroboration is simply the result of tests undertaken in a critical spirit, in an attempt to find where our theories err" (Popper, 1944, pp. 314, 315 - emphasis in the original).

Clearly at the relatively high level of abstraction these comments are pitched the basic position argued for applies as much to explanatory work in the social realm as any other. Indeed Popper continues:

"For the piecemeal technologist or engineer these views mean that, if he wishes to introduce scientific methods into the study of society and into politics, what is needed is the adoption of a critical attitude, and a realisation that not only trial but also error are necessary. And he must learn not only to expect mistakes but consciously to search for them. We all have an unscientific weakness for being always in the right, and this weakness seems to be particularly common among professional and amateur politicians. But the only way to apply something like scientific method in politics is to proceed on the

assumption that there can be no political move which has no drawbacks, no undesirable consequences. To look out for these mistakes, to find them, to bring them into the open, to analyse them, to learn from them, this is what a scientific politician as well as a political scientist must do. Scientific method in politics means that the great art of convincing ourselves that we have not made any mistakes, of ignoring them, of hiding them, and of blaming others for them, is replaced by the greater art of accepting the responsibility for them, of trying to learn from them, and of applying this knowledge so that we may avoid them in the future" (Popper, 1994, p. 315).

So Popper was always in favour of analyses being subjected to criticism and learning from error. Still, it can be argued, this is hardly enough. Though attractive, it can be said that Popper's attention to criticism and learning from error provide little content about how to proceed. Certainly this appears to be the consensus of those (few) economists who have even noticed his critical rationalist stance (see e.g. Caldwell, pp. 26-7). As Wade Hands observes:

"The real problem for critical rationalism is not that one can say very much against it, but rather that one cannot say very much with it. Critical rationalism is a view which seems palatable by virtue of its blandness, the epistemological analog of the ethical mandate to 'live the good life' " (Hands, 1992)

Certainly if we accept the ontological conception systematised as critical realism, Popper's stipulations appear to be of very little help indeed. For when faced with a social reality as complex as described how can we even begin to undertake explanatory endeavour? How specifically might criticism and error play a role? In a closed systems context Popper was able to be more explicit or definite. Perhaps too much so. Certainly his stipulations on falsificationism and situational analysis are read this way by some. But how do we begin even to proceed with explanatory endeavour in the social realm as understood here? What kind of trials and errors can we make? Certainly, if we are talking about aspects of dynamic or evolving totalities we are not in a position to experiment in the sense of manipulating aspects of our objects. So how can we expect to make explanatory progress at all?

### **Clarifying the question**

I believe we can make explanatory progress, but that in order to do so we first need to be slightly more precise in formulating the nature of our problem. The earlier discussion of the situation of controlled experimentation can help us here. I have identified the task before us as one of causal explanation. Specifically, it is to determine how we might uncover aspects of the social structures, mechanisms and conditions responsible for social phenomena in which we are interested. Now

reflection on the earlier analysis of the controlled experimental situation reveals that there are three interlinked aspects or parts to the problem that arise here, three relative disadvantages facing non-experimental research. For experimentally based causal explanatory endeavour can be usefully viewed under the aspects of (i) identifying an event regularity; (ii) forming causal hypotheses that can account for the regularity; and (iii) discriminating between competing hypotheses consistent with the regularity. It is in relation to these three activities that the problem of social (or, more generally, non-experimentally aided) explanation can be viewed.

To elaborate, there is first of all the difficulty of determining how an explanatory project is to be initiated if, or where, event regularities of the sort engineered in controlled experimental conditions are not in evidence. How do we know where to start?

Second if somehow it proves possible to initiate an explanatory project in a meaningful fashion, there arises the question of how to direct any causal explanatory research. It is easy to demonstrate that experimentally produced event regularities correspond to situations where a single (set of) intrinsically stable mechanism(s) is effectively insulated from countervailing mechanisms. Causal hypotheses are, in this very particular case, directed at the underlying mechanism experimentally insulated. In an open system such as human society, the relative paucity of regularities of the causal sequence sort reflects the fact that events or outcomes are mostly each determined by a multiplicity of causes, with the possibility that at least some of the latter will be highly transient as well as unstable. From the perspective of this understanding, a *prima facie* problem of causal research in the social realm, is with determining how it is possible to pick out one particular cause from the conceivably very many acting on any phenomenon in which we might be interested.

Third, to the extent that an understanding of a single (set of) causal mechanism(s) can be pursued at all, there arises the likely task of discriminating between competing accounts of it, where such arise. In the experimental laboratory background factors can be varied in a controlled and systematic manner. What options are available in the non-experimental situation? Clearly because we are concerned with causal explanation rather than with correlation analysis *per se*, the criterion for selecting amongst any competing hypotheses will not be predictive accuracy but explanatory power. We

can accept the hypothesis which makes sense of the widest range of phenomena within its scope. But in the absence of event regularities what sort of empirical phenomena might we now expect to call upon in assessing the relative explanatory power of competing hypotheses where held?

It is this three-part problem of openness (of knowing how even to *start* the explanatory process in the absence of event regularities of the sort produced in controlled experiments, of determining how to *direct* causal reasoning, and of being able to *select* amongst such competing alternative hypotheses as may be formulated) that remains to be addressed, and on which I propose to focus in much of the remainder of this paper. First I outline one possible solution to the noted three part problem as developed in critical realism. I then turn to indicate why I take this answer to be rather Popperian.

### **A sketch of an answer**

So where might we start in providing an answer to the multi-faceted problem identified? The answer I have focused upon elsewhere can be referred to as *contrast explanation*. According to it, the trick is to seek to explain why two outcomes were not the same as each other when we had good reason to expect that they would be. Put differently it is to ask in respect of one of the outcomes *not* 'why did *x* occur?', but 'why did *x* happen rather than *y*?', where *y* occurred elsewhere in circumstances regarded as similar. The starting point of the exercise, indeed, is a surprise that two outcomes are not the same, or that one is 'x rather than y'. The object then is to explain the difference.

Two conditions are clearly required for such an explanatory approach. The first is just that over some region that I refer to as the *contrast space*, we had good reason to expect outcomes of a certain kind to have a similar causal history and so to be much the same. In other words, the first condition is an informed (if often tacitly held or implicit) judgement about conditions operating over some particular region (which may stretch over geographical space, time, cultures and so forth, where the range of the contrast space will be larger or smaller depending on context). All that is required of any judgement is that it be suitably informed. It is not necessary that the judgement be wholly correct. A correct judgement is specifically a condition of learning by way of identifying a new mechanism coming into play. This scenario, though, is but a special case of contrast explanation.

The second condition is merely that *a posteriori* we are surprised (concerned or otherwise

interested) to find that things do not turn out as expected.

I shall indicate below that such conditions are found to hold quite widely. First let me address the prior question as to how, where the noted conditions hold, such a contrastive approach to explanation might be expected to help. Consider again our three part problem of explanation, turning first to the puzzle of how we might even initiate a causal explanatory project.

### **Initiating the explanatory process and interest relativity**

An entry point can be occasioned by feelings of surprise, doubt, concern or interest, that accompany some contrastive observations. Where we possess knowledge of sorts, and form expectations, we can be surprised by what occurs. Here, then, we have an obvious basis for initiating an analysis. Surprising contrasts serve to draw attention to the possibility that, and to indicate a 'location' where, a hitherto unknown or unidentified causal factor is (or may well be) in play. In an open and highly internally-related system this is rather important. Without such surprising or otherwise *interesting* contrastive observations it is difficult to imagine how investigatory research can proceed in any meaningful or systematic fashion at all.

The notion of *interest* here denotes a relative assessment of course. Further it tends to presuppose a prior (equally relative) assessment of a scenario as *uninteresting*. For a contrast tends to be interesting precisely in situations where its absence would have been regarded as somewhat uninteresting in the sense of expected or taken for granted<sup>10</sup>. Many taken-for-granted things are going on all the time. We often only notice that they have been when something different occurs.

Prior to the 1980's, the sight of cows standing and walking around the field was mostly not of great interest to someone living in the British countryside. Indeed it was an unexceptional common place. It is because of this, however, that the later observation of many cows appearing to lose the ability to stand and walk (with the onset of 'mad cows disease') was interpreted as 'interesting' to the point of disturbing.

As I walk down the street in which I live, people walk past, and birds fly in the air. I usually

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<sup>10</sup> And if there is a sense in which the uninteresting is a condition for the interesting, it is equally the case that the unsurprising can be a condition of the surprising, the expected a condition of the unexpected, the ordinary, a condition of the extra-ordinary, and so on.

take it all for granted. But I would be quite interested if a passing fellow human being suddenly propelled herself or himself into the stratosphere (and even if certain birds of a particularly nervous disposition stayed on the ground and chose to pass me by).

So, when certain phenomena are described as uninteresting this must often be recognised as an achieved view, a relative and knowledgable perspective marking a site where potentially very interesting things may yet arise. The interesting is a realisation of that potential.

In sum, if it is usually a mistake to take anything completely, or even largely, for granted, we can now see that it is often just because we do so that contrast explanation can go to work. Contrasts tend to be considered interesting precisely because, and where, their prior absence was, at that time, regarded as uninteresting in the sense of 'taken for the ordinary'.

So the first component of the three-part problem of openness is met in contrast explanation. The fundamental feature is the element of *surprise*, *doubt*, or, more generally, *interest* in 'surprising contrasts', a feature presupposing a concerned and knowledgeable orientation. It is the human interest that gets the explanatory project going.

### **Directing the explanatory process**

The second problem, the issue of directionality, is resolved as much by the *contrastive* side of interesting or surprising contrastive observations as by the interest or surprise. For just as an event regularity produced in the experimental laboratory *prima facie* marks the site of a single (set of) causal mechanism(s) in play, so a surprising contrast *prima facie* directs us to a single (set of) causal mechanism (s). It directs us to the mechanism(s) explaining the *discrepancy* between outcomes (or between outcomes and expectations), that accounts for the contrast 'x rather than y'.

Consider, once more, the situation of cows and the case of 'mad cows disease'. Consider first someone concerned with explaining any and all aspects of a cow's state or behaviour. Conceivably, any aspect of the cow, its mouth, teeth, legs, tail, parents, all factors that entered into the evolution of cows, and ultimately many factors going back to any big bang, have had a causal impact and so are explanatory of some aspects of the behaviour or general state of cows. Explaining the behaviour or state of cows, in truth, is not a meaningful proposition.

However, consider the situation of someone familiar with cows, who is surprised and concerned to discover that, say, in local herds (this, and perhaps all previous herds, is the contrast space) some, but only some, are showing symptoms of the disease. By attempting to explain *not* the state of cows *per se*, but the observed contrast, i.e., why these cows are ill and those are not, factors which are common to all cows can be standardised for, or factored out, allowing the possibility of identifying the (specific or most direct) cause of the (symptoms of the) disease<sup>11</sup>.

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<sup>11</sup> Now is this enough for our needs? It certainly helps us get at a causal mechanism. But what if we want to learn more about the event that emerges? More specifically, if contrast explanation is directed by an interesting contrast to a specific mechanism which, along with others, co-produces a phenomenon, is there any way of identifying other causal conditions of the phenomenon in question?

If we do indeed want to further our understanding of an open-system (multiply determined) event, i.e., to identify several of the causes bearing on it, one possible strategy is to seek out different interesting contrasts or 'foils' involving it.

Consider an example I explore in depth in Lawson (1997a). The primary outcome (or the 'fact' or actuality of interest) upon which I chose to focus is the UK's productivity record in the early post World War II period. The point is that various aspects of this phenomenon can be determined by setting it against a variety of contrasts or foils, and seeking then to explain the contrast.

If the selected foil is the UK's productivity record before that war, the more recent productivity performance (our primary concern) is found to be superior. Thus we can ask why the recent record is *superior* to, rather than the same as, that before the war. And the likely answer to this contrastive question is the post war expansion of world demand in the period of reconstruction.

However, if the selected foil or contrast is instead the early post war productivity record of certain counties of the continent of Europe, say of the old West Germany, the post war UK productivity performance, our topic of interest, is found to be mostly inferior. In this example, our contrastive phenomenon turns on the discrepancy in cross-country performances. We are concerned to determine why the UK fared so much *worse* than counties like West Germany (rather than as well). The likely answer to this contrastive question is the UK's relatively unique stream of localised (as opposed to centralised) collective bargaining, with its in-built slow responsiveness to change (see Lawson, 1997a, chapter 18).

It is not necessary, here, that the reader accepts the explanations offered of the noted contrastive questions. It is enough that the example demonstrates that where different foils are involved, where different contrastive observations are used to initiate explanatory research, different causal mechanisms bearing on the object of our focus (here UK post war measured productivity performance) are likely to be uncovered. The more contrastive questions we can pose which involve a given phenomenon x, the more, potentially, we learn about its different causes. The feasible result is a range of causal knowledge that might eventually be synthesised to give a more rounded and deeper understanding of the concrete phenomenon of our investigations.

Of course, none of this throws any general insight on the process of retroductive inference, whereby we might move from (an account of) a given phenomenon to a (hypothesis about) an underlying cause. The problem of deciding how to make this move remains a matter of context. But there is no difficulty that arises with retroduction in the context of contrast explanation that does not arise in all other situations as well. The move from phenomenon to cause rests on a logic of analogy and metaphor, luck and ingenuity, here as everywhere else. Any problems of retroductive inference are not specific to non-experimental situations.

### **Discriminating between causal hypotheses**

Finally there is the question of how the third component of the earlier noted three-part problem of openness is (or might be) met. This is the problem of determining, in the absence of event regularities of the sort produced in experimental laboratories, a type of evidence that might usefully be brought to bear in selecting amongst any competing hypotheses. This problem arises most clearly in a situation where we believe a hitherto unaccounted for causal mechanism is responsible for some surprising contrast. And one sort of evidence we might meaningfully seek is precisely sets of contrasts on which our competing hypotheses bear.

Consider the farmer who expected crop yields to be roughly uniform throughout the field but discovers that they are significantly higher at one end. If a river passing by is hypothesised to be the cause, then it may be sensible to check whether, in other fields through which the river passes, crop yields are higher in regions closest to it. If the hypothesis entertained is that shade from trees causes the higher yields it may be possible to examine other fields to assess whether yields are higher where there is shade. And so on. In the case of each hypothesis in contention, inferences are drawn concerning contrasts that we might expect to find. In each case it is inferred that if the hypothesis is correct, yields will mostly be higher in the region of the contrast space closest to the hypothesised mechanism in question. The hypothesis that performs best in terms of accounting for the widest range of relevant contrastive observations can, with reason, be accepted as the better grounded<sup>12</sup>.

### **Facilitating explanatory research in the social domain**

Now the central thesis I want to defend is that the conditions for contrast explanation hold for the social realm in particular. Fundamental here is the general point that a condition for contrast explanation is a *rational judgement* that the contrast space is sufficiently homogeneous (or, more precisely, that events throughout it share a similar causal history). For it is only on the basis of an

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<sup>12</sup> Of course, because the world is open, things will rarely, if ever, be clear cut. Even where a river usually brings positive benefits there may be countervailing factors (such as floods or up-stream spillage of industrial pollution). The rational course of action is to persevere with the hypothesis that has the greater explanatory power, that accommodates the widest range of evidence, and to see if its explanatory failures, where they exist, can be accounted for by countervailing factors, and so on. If they cannot be, the response which is most appropriate will depend on the context. Science everywhere is a messy business. But there is no difficulty here that is insuperable in principle.

informed judgement about the nature of a contrast space that a contrast can be recognised or interpreted as significant.

Contrast spaces are underpinned by expectations of continuity in social life, by expectations that causal processes are such that regularities (strict or particular) of the form "what happens here happens there" are justified. In fact such regularities abound in social life. They underpin all observations of continuity: that prices of stamps, television licences, etc., are (currently mostly) everywhere the same in the UK; that the school-curriculum is identical throughout schools in England; that most English pubs (currently) stop serving at 11:00 pm; that goods everywhere are bought and sold; and so forth. There are definite bounds to all such regularities of continuity, and all are partial. But their nature is often of a sort that an expectation of continuity is knowledgeably formed, that a contrast space is rationally delineated.

The explanatory process gets underway, however, when an expectation of some sort turns out to have been in error. When this happens we can learn in two ways at least. It may be that a new causal mechanism is operating over only part of the contrast space. Or it may turn out that we were wrong from the outset in formulating a contrast space in a particular way. That is, it may often turn out that a surprising *a posteriori* contrast is the result not of a change in circumstances, say the emergence of a new causal factor, but of an error in our previous understanding of the nature of the contrast space. But if so, on examining the cause of the contrast we may well learn that, and how, our original judgement was wrong<sup>15</sup>.

How specifically might this discussion bear on practices of social-explanatory research? Very often, in our day-to-day encounters, observed discrepancies between our best judgements and what happens gives rise to a sense of *surprise* (or even shock) as I have noted. This will be the case, for example, when an acquaintance breaks accepted conventions of polite behaviour, or the UK high street shop does not open on Monday morning as usual, etc. And it will be the case, too, when people travel further afield. For example, a first trip by a British person to Naples brings the 'surprise' that almost no

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<sup>15</sup> Of course, in the special case where the contrast space stretches from the current point in time into the future, and it is expected that things will continue much as they are, the *a posteriori* outcome of getting the contrast space wrong, and that of an unforeseen causal mechanism coming into play may amount to the same thing.

one stops at most red traffic lights.

However, for the social researcher alive to the conditions of contrast explanation, the relevant orientation may well be an informed *curiosity* more often than *a posteriori* surprise or shock. In particular, through recognising both 1) that actual or expected event regularities (of whatever degree of strictness) can, and eventually regularly do, break down, yet nevertheless 2) that existing (fallible) knowledge of certain specific or local conditions (contrast spaces) often suggests uniformity (similarity of causal histories) as our most grounded assessment, the social researcher may search out such scientifically significant contrast spaces just to see if noteworthy contrasts after all occur. In a sense, the social researcher will often be knowledgeably seeking out situations in which either they are surprised, or he or she knows it would have been reasonable (given existing knowledge claims) to have been (and that others probably will be) surprised at the sorts of observations recorded.

For example, by exploring whether changes in given structures (e.g., the introduction of minimum wage legislation, or the legalisation of Sunday trading) impact in a uniform way throughout a given region such as the UK, it may be possible, where discontinuities or differences are observed, to uncover previously insufficiently understood differences in specific social mechanisms (for example the employment process), reflecting, in particular, the nature of their internal relationality to local context, and so obtain a less partial account than hitherto of the mechanisms at work.

In other words, in such situations it is not that a researcher necessarily expects the legislation to impact in the same way in all areas, merely that her or his prior knowledge is such as to have no specific reason to expect of any two sub-regions that the impact will be greater in one than in the other. If after the event a significant difference is observed it is likely that something of note can be determined by pursuing the explanation.

Similarly, by focusing on movements in specific phenomena, say house prices or productivity growth rates, or whatever, it may be found that there are marked differences in outcomes over two (or more) regions, where current understanding would have led the researcher to expect greater homogeneity.

On occasion such a development may lead to the uncovering of a previously unrecognised

causal factor. For example, recent increases in house prices in Cambridge (UK) appear to have been significantly higher in the south of the city. The implicit contrast involved here seems to have been caused by the phenomenon of an increase in the number of house buyers wanting to live in Cambridge, but work in, and so commute to, London. This is an option recently made feasible by the speeding up of the rail link between the two cities, with the railway station situated in the south of Cambridge.

On other occasions, the knowledge acquired may be of factors already recognised but insufficiently understood. For example, differences in productivity growth rates may reflect the fact not of a new causal factor coming into play (such as a faster railway system) but of new developments in technology being assimilated differently according to the different existing systems of industrial relations (or local levels of technical knowledge, or forms of support industries, etc.) throughout the regions of the contrast space.

A further possible basis for contrast explanation arises where a researcher's understanding of the conditions of recent developments, say trend growth rates or whatever, lead her or him to the view that identified trends are likely to continue unabated (or from understandings possessed could with reason have been expected so to continue). A marked downturn (or upturn) would then constitute a contrast with extrapolated outcomes, suggesting a *prima facie* case of a new and identifiable causal factor having come into play.

In short, it is through recognising that generalisations about concrete social circumstances and processes will usually have limits, and through exploring how specific generalisations break down in areas where our current understanding suggests (most reason for supposing) they could nevertheless have held, that we can learn, by way of contrast explanation, of hitherto unknown or insufficiently understood factors that make the difference.

In a world that is open and complex, unforeseen developments are always occurring. But by starting from a (knowledgeable) position where specific changes or developments are not foreseen, those changes such as occur provide points from which it seems feasible to initiate an explanatory investigation, and concerning which, explanatory successes seem likely<sup>14</sup>.

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<sup>14</sup>This, of course, is more to less the opposite emphasis to that of mainstream modellers who attempt to assume away or gloss over discrepancies as 'noise.'

In truth, indeed, we are confronted with noteworthy contrasts of this nature almost everywhere. Is it not significant, for example, that in the modern day UK girls perform significantly better in single sex schools than in mixed ones; that in all schools, girls are beginning to outperform boys academically, when until very recently boys performed significantly better than girls; that teenage pregnancy rates at the start of the twenty first century are reported to be significantly higher than elsewhere in Europe; that men usually get paid more than women for identical work; and so on. In all such cases, the prior expectation need not have been that conditions are everywhere exactly the same, merely rather more similar (throughout the relevant contrast space) than is found to be the case. All that is required for the explanatory process to be initiated is that the contrasts observed are striking enough to suggest that something systematic is going on, given the contrast spaces involved, and that the causes of the contrasts are identifiable. I conclude from all this that contrast explanation holds out the promise for an adequate causalist approach to social science even accepting the social ontology I defend, including an absence of conditions to facilitate experimental enquiry.

The essence of the method set out, clearly, is that we learn by getting things knowledgably wrong. Thus, I am here rejecting positivistic or, more generally, monistic accounts of knowledge, i.e., accounts wherein knowledge is the accumulation of incorrigible facts. And I am reaffirming the familiar realist insight that knowledge, although concerned with an at least partly independent reality or intransitive 'object', is a two way process. Through confronting 'objects' of study we learn not only about them but simultaneously about ourselves, including, in particular, the errors of our current thinking (as well, no doubt, something of our social-cultural situations, values, and so forth). Knowledge is intrinsically a transformational process. And it is a process of transformation in which the continuous absencing of errors of various sorts is fundamental. Although the analytical moment, the elaboration and utilisation of surface patterns, has a role, explanatory research does not reduce to it. Rather the knowledge process is fundamentally dialectical.

### **Back to Popper's critical rationalism**

So what does any of this have to do with Popper's *critical rationalism*? The answer is quite a lot. Indeed, not only is contrast explanation consistent with Popper's emphasis on (self) criticism and

learning from errors, I think Popper provides many of the components of the approach I have defended. The major insight that is missing from Popper is an explicit orientation to contrast explanation and its central categories. But otherwise all the components are provided. To demonstrate this let me concentrate once more on Popper's 1990 book, albeit this time on the second of the two essays entitled *Towards an Evolutionary Theory of Knowledge*.

First recall the two essential preconditions of any contrastive explanatory exercise. The first is a knowledgeable stance regarding the likely causal uniformity over some contrast space. The second is a set of outcomes within this contrast space which surprises or interests us.

So does Popper emphasise the idea of a contrast region? Not exactly. But his starting point is a knowledgeable orientation, and more specifically a recognition that *existing knowledge is a prerequisite for further knowledge*, i.e for *learning*. Indeed, I do not know of any philosopher who emphasises this point more strongly. Moreover in his evolutionary theory of knowledge he generalises this assessment to all other life forms as well. Perhaps the more contentious aspect in Popper's position is the proportion of our knowledge we do not learn or acquire at all but is *a priori* or (as Popper interprets the latter term) inborn (p. 46). Popper writes:

"Most knowledge of detail, of the momentary state of our surroundings, is *a posteriori*....But such a *posteriori* knowledge is impossible without *a priori* knowledge that we somehow must possess before we can acquire observational or *a posteriori* knowledge: without it, *what our senses tell us can make no sense*. We must establish an overall frame of reference, or else there will be no context available to make sense of our sensations" (Popper, 1990, p. 46)

This is clearly a Kantian position, although Popper claims to go far further than Kant in supposing that 99% of all knowledge is inborn<sup>15</sup>. The point here, though, is that Popper explicitly stresses the role of knowledge as a condition of further knowledge. Although Popper emphasises that a condition of possibility of *a posterior* knowledge is *a priori* knowledge, he would not rule out (although he does not always emphasise) the role of already acquired *a posteriori* knowledge in facilitating

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<sup>15</sup> Popper acknowledges his agreement with Kant on the existence of *a priori* knowledge and adds: "But I am going much further than Kant. I think that, say, 99 per cent of the knowledge of all organisms is inborn and incorporated in our biological constitution. And I think that 99 per cent of the knowledge taken by Kant to be *a posteriori* and to be 'data' that are 'given' to us through our senses is, in fact, not *a posteriori*, but *a priori*" (p. 46).

additional such knowledge. I guess I would stress this feature more than Popper does<sup>16</sup>.

How, in Popper's scheme of things, does existing knowledge make advances in learning possible? According to my own account, systematised above as contrast explanation, existing knowledge, beliefs and expectations make surprises or interesting observations possible. Current understanding leads us to have expectations of certain sorts (specifically that outcomes in a contrast space stand in a particular relation to one another). The basis for learning arises when these expectations turn out to be in error. As I have suggested we may not even be conscious of our knowledge and expectations; until things surprise us by being contrary to an expectation sub-consciously held, we may have regarded things in a very taken for granted way. But it is the disappointment of expectations that moves us to explanatory work all the same. Popper takes a similar view:

"Our own unconscious knowledge has often the character of unconscious *expectations*, and sometimes we may become conscious of having had an expectation of this kind when it turns out to be mistaken" (p. 31)

What accounts for the mistake or the sense of surprise? According to Popper:

"when we are surprised by some happening, the surprise is usually due to an unconscious *expectation* that something else would happen" (Popper, 1990, p. 32)

This of course is fairly nearly a formulation of contrast explanation, it is based on an implicit

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<sup>16</sup> Treating features of other life forms as homologous to those of humans, Popper talks of all life forms as possessing a priori or inborn knowledge:

"Philosophers and even scientists often assume that all our knowledge stems from our senses, the 'sense data' which our senses deliver to us. They believe (as did, for example, the famous theorist of knowledge, Rudolf Carnap) that the question 'How do you know?' is in every case equivalent to the question 'What are the *observations* that entitle you to your assertion?' But seen from a biological point of view, this kind of approach is a colossal mistake. For our senses to tell us anything, we must have *prior* knowledge. In order to be able to see a thing, we must know what 'things' are: that they can be located within some space; that some of them can move while others cannot; that some of them are of immediate importance to us, and therefore are noticeable and will be noticed, while others, less important, will never penetrate into our consciousness: they may not even be unconsciously noticed, but they may simply leave no trace whatever upon our biological importance. But in order to do so, it must be able to use adaptation, expectation: prior knowledge of the situation must be available, including its possibly significant elements. This prior knowledge cannot, in turn, be the result of observation; it must, rather, be the result of an evolution by trial and error. Thus the eye itself is not the result of observation, but the result of evolution by trial and error, of adaptation, of non-observational long-term knowledge. And it is the result of such knowledge, derived not from short-term observation, but from adaptation to the environment and to such situations as constitute the *problems to be solved in the task of living*; situations that make our organs, among them our sense organs, significant instruments in the moment-by-moment task of living" (Popper, 1990, p. 37)

questioning of "why this happening rather than that". And Popper well recognises the role of disappointed expectations in science:

"But in all sciences, the experts are sometimes mistaken. Whenever there is a breakthrough, a really important new discovery, this means that the experts have been proved wrong, and that the facts, the objective facts, were different from what the experts expected them to be (Popper, 1990, p. 34).

So has Popper had the basis of an answer to the question formulated at the outset all along? The answer I think is both yes and no. Yes at least some of the various components (a knowledgeable orientation, disappointed expectations) are in Popper (although not the notion of a contrast pace, of a region rationally assessed to be covered by a similar causal history). But without a question being appropriately formulated there is nothing very clear to answer. Whatever else critical realism adds to Popper's contributions on these matters it provides a framework that allows us to determine the important question, to provide an appropriate formulation of it, thereby in turn allowing a suitable combination of the existing components into an answer.

Of course even this latter sort of reasoning is somewhat Popperian. For, as Popper himself stresses, a major part of the science process lies in detecting or formulating the problem to be answered or resolved:

"a new theory is only rarely thought up by more than a few people, even when there are many who agree on the refutation of the old theory. The few are those who *see the new problem*. Seeing a new problem may well be the most difficult step in creating a new theory (Popper, 1990, p. 49 -- emphasis in the original)

And this brings me to a further point of interest. If we agree with Popper that seeing a problem is crucially important, and if a condition of seeing a problem, of getting the explanatory enterprise going, is the experience of disappointed expectations, this argues for a more inclusive academy than we currently find (especially within economics). For the things that surprise us, or rather the expectations which we hold that can be disappointed, necessarily vary very much with our situations. Only those who do *not* expect (or do not accept as 'normal' that) all the best jobs to go to men, most of the wealth of the world to accrue in the hands of the few, peace will regularly be sacrificed in the interests of one group gaining the resources of another, economists will regularly forsake the real world in the interests of

appearing to be skilled in mathematics, will be concerned by, and prepared to question, the situations we find around us. So for wide-ranging explanatory work we need to bring all points of view into the academy. For, as I say, what it is that strikes us as surprising, depends very much on our situated practices and prejudices and so immediate values. It seems that Popper would agree with this too:

"All organisms are problem finders and problem solvers. And all problem solving involves evaluations and, with it, values. Only with life do problems and values enter the world. And I do not believe that computers will ever invent important new problems, or new values" (Popper, 1990, p. 50).

### **Final comments**

Let me, in conclusion, address the remaining question raised at the outset, for it allows me to sum up my assessment of Popper's basic orientation as it bears on social explanation. The second question posed was whether the current orientation to social explanation in modern economics warrants the label Popperian. Now modern economics is a subject which is not in a healthy state. Its methods, mainly those of mathematical formalism, are not very successful. But its results, though questionable, are rarely challenged. Economists choose mostly to live with the situation, rather than to seek to transform it. The few who attempt to do anything different, and specifically to learn from error, are largely ignored. Consider the recent observations of Richard Lipsey (a mainstream, not a heterodox, economist):

"..anomalies, particularly those that cut across the sub-disciplines and that can be studied with various technical levels of sophistication, are tolerated on a scale that would be impossible in most natural sciences -- and would be regarded as a scandal if they were" (Lipsey, 2001, p. 173)

Or consider Leamer's (1978) observations regarding the widespread discrepancy between the theory and practice of econometrics:

"The opinion that econometric theory is largely irrelevant is held by an embarrassingly large share of the economics profession. The wide gap between econometric theory and econometric practice might be expected to cause professional tension. In fact, a calm equilibrium permeates our journals and our meetings. We comfortably divide ourselves into a celibate priesthood of statistical theorists, on the one hand, and a legion of inveterate sinner-data analysts, on the other. The priests are empowered to draw up lists of sins and are revered for the special talents they display. Sinners are not expected to avoid sins; they need only confess their errors openly" (Leamer, 1978, p. vi).

The truth, then, is that Popper the critical rationalist is hardly understood by economists at all. Rather

than pursue existing methods in the name of Popper, it is time, I believe, for a more critical orientation to be taken. Specifically, something more is required than the tired insistence that given methods are fine, or that errors can be lived with. And to the point, a more genuinely Popperian response is needed. Or at least a response is required that fits with Popper's critical rationalism. The aspect of modern economics that warrants criticism is not the making of errors but the widespread reluctance to learn from them, including the pretence that they do not matter. Errors provide a scientific opportunity. This anyway is a position described and defended here and elsewhere (e.g. Lawson, 2003). And I think it entails replacing the Popperianism of modern mainstream economists with the quite different Popperianism of Karl Popper.

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