

Part II

POSSIBILITIES FOR ECONOMICS

There are various ways in which the sort of ontological conception outlined in Part I can impact on social theorising. It is indicative, for example, of the range of conditions for which the social researcher might with reason be prepared. One question revealed to be of immediate interest and significance is how it is possible for explanatory endeavour to proceed in the face of a social system that is found to be fundamentally open. How in particular might an explanatory project get under way without the possibility of experimental intervention to fall back on? Is it in fact necessary to oversimplify in the sense of knowingly to fictionalise in order to get anywhere? Many economists appear to believe so. Perhaps most do. My concern in Chapter 4 is with establishing that any such belief is ill founded. I demonstrate that despite the fundamental openness of the social domain, successful social-explanatory endeavour which does not knowingly fictionalise is entirely feasible.

Economists need not always fashion their methods from the 'ground up'. Amongst other things they are at liberty to borrow from other disciplines. Indeed, economists have always done so. As I write these lines more and more economists appear to be interested in borrowing from evolutionary biology, and in applying the Darwinian 'natural selection' model in particular. In Chapter 5 I look at the implications of the understanding of social reality achieved in Part I for the way, if at all, the natural selection model might carry over to the social realm. I argue that ontological considerations are fundamental to processes of abducting from other realms.

To the extent that ontology is used to identify the nature of specific spheres of reality, its results also provide a non-arbitrary basis for distinguishing and identifying the different sciences. That is, ontological results can provide a basis for delineating the various sciences according to the different types of material and principles that are studied. It is in such terms, indeed, that sciences like physics, chemistry and biology have always been distinguished. It is only modern mainstream economists who have attempted to define a discipline according to its (supposed) methods. Once we abandon this orientation in favour of a realist one based on the properties of materials to be studied, it is possible to raise anew questions bearing on the nature and scope of the discipline. Specifically, how might we

distinguish economics from other branches of social theorising? Further, is there a sense in which economics is or could be a science? If so, is it, or could it be, legitimately distinguished as a separate science, i.e. one with its own material of study? These are the sorts of issues addressed in Chapter 6, an essay reprinted from Economie Appliquée.

EXPLANATORY METHOD FOR SOCIAL SCIENCE

How does an ontological conception such as I defend in this book bear upon the project of social explanation? One thing it does not do is determine how we *must* proceed, certainly not in any very specific way.¹ It does, though, serve numerous purposes. One such, upon which I focus in the current chapter, is to indicate something of the sorts of conditions or scenarios for which it is prudent to prepare ourselves. In this it imparts directionality to social scientific endeavour.

Conditions of social explanatory endeavour

According to the conception I defend, social reality is open in a significant way. Patterns in events do occur. But where the phenomena being related are highly concrete (such as movements in actual prices, quantities of materials or outputs, and most of the other typical concerns of modern economic modellers), such patterns as are found tend to take the form of *demi-regularities* or *demi-regs*, that is, of regularities that are not only highly restricted but also somewhat partial and unstable (Lawson 1997a: 204–21). Moreover, patterns relating events standing in causal sequence (i.e. patterns where some of the associated events causally condition the others) seem to be rare indeed in the social realm. At least this is so once we look beyond situations involving rule-governed, reasonably routinised forms of behaviour (cars stopping at red traffic lights, and the like).

However, social reality is found to be not only open in the manner described but also structured. That is, it comprises not only 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 events and states of affairs (see Chapter 2). This assessment 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.²

Causal explanation and retroduction

So the ontological conception defended in Chapter 2 directs us towards considering how, in economics, we might conduct causal explanatory projects. This emphasis, in turn, points to a need to develop modes of inference over and above (the usual forms of) deductive and inductive logic. A reliance on these latter forms of reasoning, as usually interpreted, restricts the researcher to considering only the level of reality at which the phenomenon to be explained is found. However, for causal explanation it is usually necessary to go deeper. Deduction, of course, moves from the general statement to the particular. For example, if we accept that 'all metals expand when heated' we can deduce that 'this metal before us will expand when heated'. Induction takes us from the particular statement to the general. If our research practices reveal that 'each examined (bit of) metal expands when heated' we might be tempted to speculate inductively that 'all metals expand when heated'. In each case we move from a statement about the behaviour of metals to a second statement at the level of the behaviour of metals.

To pursue causal explanation as interpreted here, we require a mode of inference that takes us behind the surface phenomenon to its causes, or more generally from phenomena lying at one level to causes often lying at a different deeper one. This is *retroduction*. It takes us from a recognition that 'this metal before us expands when heated' to a conception of the metal's intrinsic structure (or whatever) in virtue of which the metal has the power to expand when heated.

Little can be said outside a specific explanatory context about how in practice the retroductive process might proceed, other than it will often follow a logic of analogy and/or metaphor, and rest usually upon ingenuity as well as luck (see *Economics and Reality*, especially chapter 15). For example, following the discovery, in the late 1980s, that cows in the UK showed symptoms of the illness we now call 'mad cow disease', it was retroduced, by way of analogy with other illnesses, that a virus was causing the problem. This retroduced hypothesis, however, proved not to be correct. Only with a lot of skill and luck was the prion located as the most likely (explanatorily powerful) causal hypothesis (see for example Lawson 1997a: 293–4).

It is clearly significant, here, that causal explanation under the guidance of retroductive inference does not necessitate that only atomistic explanatory accounts be contemplated. Retroductive inference *per se* places no restriction on the sort of explanatory conception that may be uncovered. This is important here because the ontological conception I defend suggests that social reality, as well as being open and structured, is also highly internally related, intrinsically dynamic, and so forth. It may be that the conception which best explains some identified phenomenon of

interest is a holistic entity, or an atomistic one, an evolutionary process, or of a momentary impulse. Each and all types of phenomena can be accommodated through retroductive reasoning. Thus in focusing on causal explanation and retroduction, we are staying well within the framework of the ontological conception I defend.

The central problem of social explanation

If the openness of social reality is a spur to pursuing causal explanation, is it not simultaneously a fundamental obstacle to success in doing so? Although the frequent productions of experimental event regularities in certain natural scientific contexts can themselves be adduced as evidence that these natural realms too are open (it is only if a domain of reality is open that an experimental closure [of part of it] can be humanly engineered), there is no doubting that the well controlled experiment aids causal analysis precisely because it allows a causal mechanism to be insulated from other factors and thereby empirically identified. A central challenge to much social explanatory endeavour, then, and in my view the most fundamental one, is to determine how explanatory work might proceed in an open system context that lacks the possibility of experimental intervention.³ At least this is a fundamental challenge where social processes do not reduce to the rule-governed routinised sort.

There are three interlinked aspects or parts to the problem that arise here, three relative disadvantages facing non-experimental research. In the experimental context, causal explanatory endeavour is usefully viewed under the aspects of

- (i) identifying an event regularity
- (ii) forming causal hypotheses that can account for the regularity
- (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. The analysis of Chapter 1 suggests 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 begin 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 chapter.

The usual recourse of mainstream economists faced with the noted situation, is to maintain, in effect, that universal and strict (or well behaved probabilistic) event regularities of the sort engineered in experimental laboratories are ubiquitous in the social realm, after all, if as yet remaining undiscovered. Mostly this course of action is driven not by any reflection on the ontological conditions of social reality, but by the (largely unexamined) triplet of a desire to be scientific in the sense of natural science, an unquestioning confidence that the latter is possible, along with an (erroneous) belief that science necessitates a reliance on mathematical methods. Certainly, the overriding goal, typically, is a tractable mathematical model. And if each modelling endeavour presupposes a closure of a certain sort, the insistence on formalistic methodology for all occasions requires precisely that social closures of the relevant sort are ubiquitous. The openness of social reality is assumed away.

As I say, this mainstream response is mostly driven by an unthinking *a priori* presumption that formalistic modelling must have relevance, and it

is only a question of time before we get it right. There are others, though, who ultimately take the same deductivist path whilst adopting a different attitude (or 'justification'). Specifically, there are those who, though enamoured of neither the deductivist approach to explanation *per se*, nor by its desire-to-be-mathematical motivation, adopt the mainstream deductivist approach anyway. This group appears resigned to the idea that, because social reality is rather complex, there is little alternative but to proceed *merely as if* universal and strict (or well behaved probabilistic) closures of the causal sequence sort are ubiquitous in the social realm after all, even though they see good reason to reject any presumption that they are. Fundamental, here, is a common, if erroneous, apprehension that because, or where, social reality is complicated we must knowingly fictionalise so as to simplify enough to say anything. This is where isolationist procedures are often brought in (if occasionally misdescribed as methods of abstraction). For those accepting this outlook (which is easily shown to be misguided – see Lawson 1997a: ch. 9),⁴ it is mostly an act of faith that deductivism through fictionalising can help (in so far ill defined ways). This act of faith follows on from a supposition that there is no alternative but to persevere with deductivism in the face of repeated failure so far. My aim here is to show that the latter supposition is as unfounded as the former resort to unreasoned hope is unnecessary.

I might note, parenthetically, that members of both these groups, in seeking formulations resting on correlations of the causal sequence sort, display a (seemingly increasing) tendency to refer to the results of stretching such rough and ready patterns as are found as stylised facts. The project undertaken, then, is a sort of stylised-facts-based deductivism. Now, Kaldor and others who earlier advanced the terminology of stylised facts in economics used it to indicate the rough and ready nature of such patterns as are found (Kaldor 1985: 9). Of course this is precisely the opposite of what stylised means in common parlance. And this has led some contributors to employ the category to cover formulations that are recognisably extreme idealisations, even plain fictions, whilst simultaneously apparently seeking to claim for their constructions the more realistic pedigree of the likes of Kaldor. At best the terminology serves to confuse. I thus continue the practice of referring to partial patterns in actual events and state of affairs not as stylised facts but as demi-regularities or demi-regs. This label better conveys the insight that the patterns implied are usually indeed partial and unstable at best, not something to be stretched into the sorts of strict conjunctions that are the provenance of closed systems.

So my task, to state it now more fully, is to demonstrate that, and how, in social research that is concerned with wider issues than routinised behaviour, it is possible both to initiate and also to direct causal-explanatory endeavour, as well as to discriminate amongst any

contending causal hypotheses, in an open-system context in which well controlled experimentation appears infeasible, making use only of such event patterns, and in particular demi-regularities, as are found to be available, i.e. without pretending the situation is entirely different to the way we continually find it.

It will be appreciated that, so conceived, my overall goal remains highly limited. This, as I noted at the outset, is necessarily the case. The most I can hope to achieve is a demonstration that successful explanatory work in such conditions is indeed possible. I can at most seek to identify a method or approach that carries the potential (and regularly proves) to be fruitful in such situations. The actual relevance of any specific method or approach elaborated will always depend on context. However, even this minimal objective seems to be regarded as unattainable by many, as I have just noted. Instead there is a widespread conviction that social explanatory research ultimately has to rely on methods whose usage require that we treat the relevant domain of reality as closed, even if, and where, we believe that it is not. So my aim here, though limited, appears not to be insignificant.

I might emphasise too that the limited approach I defend here cannot be formulated in any very great detail. My goal with this chapter is certainly to be as definite as possible in terms of how we might undertake the task of explaining social phenomena generated in an open system. Indeed, I want to set out and defend a way of proceeding that can be interpreted as a definite explanatory approach. But the aim is not, and in my assessment could not be, to provide a list of highly specific or concrete methodological rules that need only be registered and followed. Indeed, if the reader understands methodology as a narrow concern with specifying specific watertight rules for conducting research, then the project to which I have been contributing is not one in methodology (in this sense) at all.⁵ It is, though, methodological in the broader sense of underlabouring for social science.

A point of departure

So where might we start? It is a matter of some significance here that most of us are quite successful every day in negotiating the open, dynamic and complex social world in which we find ourselves. And such successes do not reduce just to our following (rule-governed) routinised practices. Certainly the latter are essential for everyday interactions with others. But these interactions typically figure as but a component of broader (short-term and longer-term) projects. We may aim to get from A to B successfully; draw money from the bank, perhaps of an amount sufficient to cover activities for a fixed period of time (say a week); organise a meal for friends, neighbours or in-laws; deal with minor accidents or emergencies; choose a school (or holiday play scheme) for a child; organise a

sabbatical period abroad; join a public demonstration (critical, say, of the way certain groups are oppressed by others or of how the world's resources are being used); prepare for, or attend, a wedding, an anniversary event, or a funeral; raise some finance for a project; show a continuous sensitivity to the projects and expectations of others, whether strangers, family or friends; and so on. Each such activity involves our (simultaneous) participation in a large number of complex social processes. And it is difficult to believe that our regular successes in these activities do not require that we are highly knowledgeable of the social structures and processes, including systems, in which we so regularly partake. Clearly, we all make mistakes. But even the recognition of these mistakes reflects the degree of success most of us achieve in our everyday projects and actions. The question, then, is not if, but how, a knowledge of the range of causal structures of an open social reality is obtained. And one promising place to start in investigating this matter is with the sorts of successful explanatory strategies we all, on a day-to-day basis, undertake.

My specific orienting claim, here, is that we all very often advance our knowledge of aspects of the world in which we live, including of the social system, by way of first questioning why something is not quite as we expected it to be. And this observation, which I think is rather clearly a generalised fact of experience, proves to carry quite fundamental import for social scientific research.

Certainly, examples of the sort of questioning described come easily to mind. Why are my keys not in my pocket as usual? Why does my bicycle not work as well today as it did yesterday? Why did the students do so much worse/better in the exams this year compared with other years? Why, in so many parts of the world, are enrolments in economics currently declining when those in seemingly comparable subjects are increasing? Why has productivity growth in the UK over the last hundred years or so often fallen below that of other industrialised countries? Why have house prices in the UK recently been rising faster in the south-east than in the north? Why, in the 1990's, was the NATO reaction to the situation in Kosovo different to its earlier reaction to the events in Bosnia? Why do I feel so ill/tired today compared with normal? Why was the milk not delivered this morning? Why, in the UK, did less people than usual go away for the start of the New Year coinciding with the new millennium?

I think it will be recognised that surprised expectations or quizzical reflections of this sort are everyday occurrences. But, significantly, so too are their resolutions. We normally can and do figure out why our keys are not as usual in our pockets (perhaps there is a new hole); why our bicycle is not running smoothly (a stick caught in the mudguard); why the recent group of students fared relatively poorly/well in their exams (an exam setter who misunderstood the coverage of the course); etc.

Other questions may require more effort, but still seem open to explanation (I think, for example, the central thesis of this book, appropriately interpreted, effectively identifies significant reasons for the continuing decline in enrolments to study economics).

If it is accepted that the posing and answering of questions of the sort identified is rather widespread, it may yet seem that such practices are of only marginal significance at best to the task of determining explanatory procedures for social-scientific research. My contention, as I say, is that, to the contrary, they are fundamental to this issue. Indeed, through understanding certain central features of what is going on in these examples, it is possible to determine a somewhat (as it turns out) general basis for developing research methods appropriate to causal explanation in open and complexly structured systems lacking the experimental option.

So what do examples such as those briefly listed reveal? What accounts for successes, when achieved, in answering the sorts of questions formulated? We can address such matters, first by examining the structure or nature of the sorts of questions and answers noted in order to understand precisely what is going on, and second by determining their (ontological) preconditions, i.e. the conditions which must hold given that such successful practices occur. From an understanding of the latter conditions we can infer wider consequences for social explanatory work.

Contrasts and interest

I consider first, then, the structure or nature of questions of the sort posed and responses obtained. Here it is significant to note that in each case the question posed is not of the form 'why x?', but of the form 'why x rather than y?'. In each case there is a contrast made, and surprise, concern or interest revealed that the outcome is x rather than y as expected.

Correspondingly, in each case the answer is a causal factor that does not account for x *per se* but explains the contrast 'x rather than y'. It explains the observer's surprise or topic of concern. Here in a nutshell, I am going to suggest, we have one significant solution to the three-part problem of openness. This contrastive orientation allows the initiation of an explanatory endeavour and facilitates the process of identifying a particular mechanism. And competing hypotheses can be selected amongst simply by bringing a range of contrastive phenomena to bear.

Once this solution to our three-part problem of openness is accepted, we need only determine the conditions of possibility of proceeding in this way to appreciate the relevance of this approach. Before turning to this matter, however, the brief account of the structure of the relevant form of question and explanation just set out will likely benefit from some elaboration by way of an illustration. For I doubt that, from the

discussion so far provided, it will be at all obvious that the contrastive structure singled out is either essential to such successes as are regularly achieved in social life, or adequate to facilitate explanatory scientific research in the context of open systems where opportunities for experimental closures are lacking. Moreover, there is rather more to the explanatory practices in question than is immediately apparent.

An illustrative example

To demonstrate the relevance of the contrastive form of the question posed, as well as to uncover the relevant conditions of such explanatory successes as are achieved, it is useful to look more closely at a particular example of a successful explanatory project. I need to focus on an explanatory exercise where it is clear both that, and how, success has been achieved. Here, though, I encounter a first significant problem. For although it is possible to turn to published social theoretical studies to see *how* results and conclusions have been derived, there is always going to be a difficulty in convincing the critical reader or opponent that such analyses are indeed successful. That is, before using any examples to illustrate an argument about how successful research may proceed, there has to be some agreement that the examples chosen do constitute, or in some way typify, explanatory successes. But of course, a major problem in the charged atmosphere of modern social science is that few if any sets of systematically laid-out practices or results are so regarded in an unambiguous fashion.

For strategic reasons only, then, I turn to the practices of certain natural sciences, or anyway to those of other 'accepted' forms of science (as I have previously and for related considerations – see Chapter 2). I do so, as I say, merely to identify examples of practices which illustrate the approach I wish to convey but are already widely regarded as successful. It will eventually be seen that the insights of the examples considered do indeed bear on the social realm too. I return to an explicit consideration of explanatory endeavour in the social domain in due course.

Scientific experiments once more

In fact, I turn to an example of controlled experiments, which most commentators seem to agree are (frequently) successful. But I do not want to focus on those most controlled of 'controlled experiments' in the natural sciences, those laboratory experiments wherein all factors, bar the one whose properties are being investigated, are held either constant or at bay. Such a scenario is hardly feasible in the social realm, and would not easily suit my purposes here. However, this precise scenario is largely infeasible in various other domains as well, including some

where successful controlled experiments do nevertheless occur. Not all controlled experiments proceed on the same basis, and some occur in conditions where it is not feasible to hold off, or hold constant, all background factors. It is experiments of the latter sort I want to consider here, experiments which, it will be seen in due course, do bear implications that have a relevance for social science (even though such experiments may not actually be executable there). Experiments in plant breeding can serve as an illustrative example.

Plant breeding

In plant breeding experiments, researchers are often concerned with determining whether a substance, say a chemical compound, has the potential to act as a fertiliser, to increase the yield of certain crops. The context of such experiments, however, is that they are frequently carried out in open fields in which the sorts of factors that can affect crop yield are often large in number, variable, transient and/or immeasurable, frequently unknown, and seemingly capable of combining in organic (non-mechanistic) ways. Certainly, it is typically not possible to isolate the crops from the effects of all but one factor, or to hold the effects of other causal factors constant.

In this context, agricultural researchers have nevertheless been successful in their work. The successful strategy has usually been to divide the land into a set of plots, some of which receive (varying amounts of) the compound and some which receive none. To the extent that there is a systematically higher average yield where the compound is applied, researchers have with reason attributed this to the compound whose fertilizing capacity is being investigated.⁶

How does this discussion have any bearing on our social explanatory concerns, and, in particular, relate to the sorts of questions listed above? It has an immediate bearing in that the focus is not on a specific outcome *per se*, i.e. the level of yield of the crop, but on a comparison or contrast: whether or not the yield is significantly higher where the compound has been added.

Of course, the plant breeding example motivating the discussion is not directly analogous to the scenarios we expect to find in the social realm simply because there is an element of experimental design involved. I will consider what follows from this disanalogy for social explanatory research in due course. For the time being I think we can agree that agricultural experiments of the sort examined are often successful, and I want to examine their conditions of possibility. For we shall see that these conditions, only slightly modified, along with the possibilities for explanatory success they facilitate, carry over to social situations as well.

Conditions of possibility of successes

What then are the necessary conditions for successful plant breeding experiments of the sort discussed? First there must be an observation domain of some sort, one I shall refer to as the *contrast space*, over which the experiments are performed. A contrast space is merely a domain over which it is meaningful, given our current understandings (possibly reflecting experimental design, although not necessarily), to draw comparisons. It is a space over which any observed systematic contrasts can be regarded as *prima facie* significant or of interest. In plant breeding the contrast space stretches over geographical space (in time). It comprises the plots of land utilised throughout the period of the experiment.

A second essential condition for the experiment to be successful, or at least to be successful on its own terms (the relevance of this qualification will become clear in due course) is that all relevant parts or aspects of the contrast space are correctly assessed to be subject to roughly the same set of causal influences bar one, i.e. except for the compound which is deliberately applied to some plots only.

In this particular example of plant breeding, I repeat, there is no requirement that the causal factors operating throughout the contrast space remain constant over time. All may change and in unknowable ways. It may even be that some of the relevant factors in play change from day to day (or even second to second), either in having an effect, or in the nature of their effects. The relevant requirement, or condition of possibility of experimental success, is only that at any point in time, the relevant causal factors (excepting the compound being experimentally investigated) are roughly the same throughout the contrast space, i.e. over the plots of land utilised in the experiment.⁷ I further repeat that there is no presumption that any causal factor, including the compound under investigation, interacts with other causal factors mechanistically/atomistically. The success of such experiments is rendered intelligible simply if, over the plots utilised in the experiment at any point in time, the set of causal factors in play are sufficiently similar, excepting for the compound whose causal properties are the concern of the investigation.

Moving towards the social domain

Now consider a situation which is very similar to the one of plant breeding described above, excepting that there is no agricultural experimenter allocating the compound systematically to plots, but instead a farmer who discovers *a posteriori* to her or his surprise that the crop yield is twice as high on average at one end of the field. It is not too fanciful to suppose that on examining the field the farmer is successful in identifying a factor (the compound in question or shade from trees or a local river or whatever)

located only at the end of the field where the yield is high, which is the cause of the observed contrast. The scenario of this example does seem analogous in relevant ways to the examples with which we started concerning the social realm, and (as we shall see) in essential respects it is equivalent to the plant breeding experimental situation as well.

Of course, a clear difference between this scenario of the surprised farmer and that of the plant breeding experimenter is that of orientation. The experimenter is forward-looking, concerned with designing the experiment, triggering a mechanism, and observing the result. The surprised farmer is backward-looking. He or she starts from a situation where the *a posteriori* result occasions a sense of interest or surprise. The experimenter engineers a scientifically significant situation (albeit only where conditions allow). For non-experimentalists such as the surprised farmer, it is necessary to 'wait' for scientifically significant situations to emerge. But these are differences not in the conditions in which an increase in knowledge is possible, only in how they come about. As long as surprising, or otherwise significant, contrasts of the sort in question abound in the social world, and I indicate below that they do, there is no reason to suppose that, in itself, backward-orientated research (like the farmer seeking the explanation of the *a posteriori* situation of high yields at one end of the field) cannot be as successful as research that is experimentally designed.

However, there remains a second and rather significant difference between the two scenarios, or at least between the experimental situation and a conception along the lines of the example of the surprised farmer (the example that most corresponds to the situation in the social realm). This relates to the accuracy of the judgement formed over the contrast space. If an experiment is to be successful on its own terms, it is essential that the conditions of the experiment are more or less as the experimenter supposes. If, for example, a second factor influencing yield just happens to be located in the field in places where the compound is added (perhaps something has infected all, or a sample, of the compound before it was applied) then the experiment may fail in that erroneous conclusions are drawn.

Now equally, of course, if the surprised farmer is to track down a previously unknown cause (of higher yield) at the end of the field, it is essential that his or her prior assessment about conditions in the field is broadly correct, excepting for the unknown causal factor at one end. At least this is so if progress in knowledge is to take the form of an awareness of a causal factor that has somehow newly entered the contrast space.

However, it is immediately apparent for the non-experimental situation at least, that we can relax this latter requirement or condition (actually we shall see below that the same holds for experimental situations as well). And this possibility turns out to be rather important. A surprising contrast may ultimately reveal not that a previously unknown

causal factor has somehow emerged in the contrast space, but that the judgement made concerning the degree of continuity, homogeneity or sameness occurring over the contrast space, though based on the observer's best evidence to hand, was incorrect. In such situations there is (or can be) progress in knowledge all the same. It merely takes the form of a transformation in understandings previously taken for granted, rather than an insight into something newly in play.

In the case of our farmer, for example, it may be that the supplier of seed for the crop always mixed a variety of types. There never was homogeneity. Some types of seed produce more yield, some less. It just so happened that on this occasion seeds of the high-yield-producing variety became somehow grouped together in a container that was applied at one particular end of the field.

I am probably overdoing the plant breeding illustration by this point. However, a case along the sorts of line just illustrated remains a logical possibility, and its nature, as we shall see, is one that has some substantial relevance for learning in the social domain. The significance of the example is that there is more than one way to learn in a situation of surprised contrasts. The basis of our surprise may indeed be a new causal factor coming into play, but equally it may merely be that our prior assessment of the nature of the contrast space was significantly in error all along. From such considerations we can see that the possibility of a progressive transformation in our knowledge does not require that we have judged the contrast space correctly, merely that we had rational grounds for the judgement formed.

In the end, of course, even the controlled experiment can go wrong. A (fallible) judgement about the adequacy of the experimental conditions is always involved. If anomalies are produced, this may lead the experimenter to discover factors not sufficiently controlled for. But when this occurs, and it does seem to be a common occurrence, then the experimenter is simply in the situation of our surprised farmer, in both making a discovery of a factor hitherto unknown or previously unrecognised as an influence, and doing so after the event.

I emphasise, though, that in the non-experimental scenario (just as much as the experimental one) a situation of *informed* prior judgement concerning the contrast space remains essential if progress in explanatory understanding is to be at all feasible, including getting an explanatory endeavour off the ground. Although the observer may get things wrong somewhere, he or she must get things wrong in a nevertheless knowledgeable and informed way. An explanatory endeavour is not going to be triggered by any old contrast. It has to occasion surprise, concern, feelings of doubt, or interest.

To construct an (extremely simple) example specifically for the social realm, if X notices that her or his residence and that of person Y are

painted in different colours on the outside, such a contrast normally (i.e. for most examples of X and Y, and in most contexts) is unlikely to occasion surprise, and thereby trigger an explanatory exercise into the cause of the contrast. If, however, the two residences are two halves of the same house, X owns them both, and a hired painter had been instructed to paint them in identical fashion, any observed *a posteriori* contrast in their colour may indeed occasion surprise and the seeking of an explanation.

Further, if an explanatory exercise is initiated, any new insight gained may, as I say, come in a variety of ways. The cause of the contrast may well be a new factor in play. Perhaps X's spouse and joint owner of the house contravened X's original instructions without telling X. However, it may also be that there is no new factor in play. Perhaps X is a (normally) absentee landlady/lord, and is in fact viewing the wrong property mistakenly. Even here there is something to be learned, albeit only for our landlady/lord to discover he or she is in the wrong street.

The general insight these sorts of considerations bring out is the transformative nature of the process by which progress in knowledge occurs. The complex structures of the world are not revealed just by our sensing them directly. Nor is knowledge created out of nothing. Rather we start out, at any point in time, with a stock of knowledge, hunches, data, anomalies, suspicions, guesses, interests, etc., and though interacting with the world we come to transform our understandings. Knowledge, then, is found to be a produced means of production of further knowledge.

The specific insight achieved concerns the nature of the explanatory process in an open system. Through illustrating how human knowledge and expectations are a vital input to the explanatory process, the above considerations lead to a model of explanatory discovery. It is a model of which the social examples originally listed, like those of plant breeding, are special cases.

Contrast explanation

The various illustrations and considerations reflected upon above in fact lead us to a model of scientific endeavour that can be referred to as *contrastive explanation* or, as I now prefer to call it, *contrast explanation*.⁸ According to it, successful explanatory endeavour requires just the two basic conditions or ingredients identified.

The first essential ingredient is an informed (if often tacitly formed or implicit) judgement about conditions operating over some contrast space (which may stretch over geographical regions, 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. As we

have seen, a correct judgement (about past conditions at least) is specifically a condition of learning by way of identifying a new mechanism coming into play. This scenario, then, is but a special case of contrast explanation.

The second essential ingredient of contrast explanation is that a relation between outcomes within the contrast space is eventually recorded that is regarded by the researcher (or whoever) as surprising or in some way of concern or interest.

The satisfaction of such conditions, I now want to suggest, gives us all we need to embark systematically, and with some reasonable expectation of success, on explanatory endeavour, and in particular on such endeavour as can proceed in the face of the three-part problem of openness identified earlier in this chapter. Let me recall what the three noted aspects to the problem are.

Causal explanation, in an experimentally closed system, is able to move from the detection of regularities, to the formulation of causal hypotheses, to a weighing of any competing hypotheses in terms of predictive power with respect to such regularities. The three-part problem that arises in situations lacking the possibility of producing the sorts of regularity often achieved in controlled experimental contexts, lies in determining how even to *start* the analysis, how to *direct* it to get at a specific (aspect of a) causal mechanism, and how to *discriminate* between competing hypotheses without event regularities to facilitate event prediction. Let me indicate how we are now in a position to deal with each aspect of this problem.

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. Because we always 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.⁹ Many taken-for-granted things are going on all the time. We often only notice that they have been doing so when something different occurs.

Prior to the 1980s, the sight of cows standing and walking around the field was mostly not of great interest to a UK country person. Indeed, it was an unexceptional commonplace. 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 cow disease') was of 'interest' 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 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.

I might note, at this point, that in previous writings (e.g. Lawson 1997e: 79) I have often referred to some sorts of relatively strict event regularities as somewhat scientifically uninteresting. By this I have indeed meant something like 'unremarkable', 'well understood' or 'taken for granted'. Usually these event regularities have been of a merely concomitant kind, where the events are correlated not because one subset causally conditions the other but because they share a similar causal history. We can now see, indeed, that regularities of this 'uninteresting' sort are polar or limiting conceptions (i.e. strict versions) of the sorts of patterns that are projected for contrast spaces. When correlations of this sort are expected to be strict their failure or breakdown can be interesting in the sense relevant here. These event regularities of the merely concomitant sort, then, are limiting forms of the patterns that lay the basis for contrast explanation to be initiated.

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 knowledgable 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 again the farmer concerned with the yield of her or his crops. The total yield will depend on numerous, possibly complexly interacting factors, and typically be too difficult to explain. However, the yield differential, the contrast between the average yield at one end of the field and the average yield elsewhere, may, when we have reason to expect a high degree of uniformity throughout, give *prima facie* reason to suppose a single explanation.

Or consider once more the situation of cows and the case of 'mad cow 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 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.

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 was 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 various contrasts.

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 postwar expansion of world demand in the period of reconstruction.

However, if the selected foil or contrast is instead the early postwar productivity record of certain countries of the continent of Europe, say of the old West Germany, the postwar 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 system of localised (as opposed to centralised) collective bargaining, with its in-built slow responsiveness to change (see Lawson 1997a: ch. 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 the UK's postwar 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 causal-explanatory 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.

Actually, it may be the case that the surprise or shock of an unexpected contrast encourages us to examine our existing understandings and realise that we already have an explanation of it. We (and our situations) are usually characterised by tensions and inconsistencies of sorts, and a development which shocks or surprises or otherwise interests us may

(whatever the precise cause) lead first and foremost to introspection or self-reflection, and thus to a resolution by way of understandings already (if only tacitly) possessed. These are stages in the knowledge process that Clive Lawson (2000) describes as 'epistemically significant moments'. Lawson suggests that 'The main feature of these moments is some form of transition, crisis or rupture in the structural conditions of practice, that prompt a "reclaiming" of knowledge that the agent already has, but is not discursively aware of' (189).¹⁰ Such processes of 'recovery' clearly deserve attention in social theoretical research.¹¹ The point here, though, is that whatever the mechanism whereby specific knowledge claims come to be formulated or 'reclaimed', contrasts of surprise or interest provide seemingly appropriate conditions, and so opportunities, for causal hypotheses to be meaningfully formulated.

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 again the example of the farmer surprised by higher crop yields at one end of the field. 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 empirical adequacy in this sense over the widest range of relevant conditions can, with reason, be accepted as the better grounded.

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 upstream 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.

Facilitating explanatory research in the social domain

Now the central thesis I want to defend, of course, is that the conditions for contrast explanation hold for the social realm in particular. Fundamental here is the general point, noted throughout, that a condition for contrast explanation is a *rational judgement*, or a knowledgeable assessment, 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 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. We saw in Chapter 2 that 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 most are partial within their bounds. But their nature is often of a sort that an expectation of continuity is knowledgeably formed, that a contrast space is rationally delineated.

As I have emphasised, 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.¹²

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 give 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 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, first, that actual or expected event regularities (of whatever degree of strictness) can, and eventually regularly do, break down, yet nevertheless, second, 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 they know 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 their 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, England, 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, leads them 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.

An example of the latter is, in effect, provided in a study by Goldin and Katz (forthcoming). They observe that just after 1970 in the United States there was a dramatic increase both in the fraction of women college graduates entering professional programmes, and in the age of all college graduate women at first marriage. For example, the percentage of first-year law students who were women rose from 10 per cent in 1970 to 36 per cent in 1980. And whilst almost 50 per cent of a cohort of women college graduates born in 1950 were married before they were twenty-three years of age, of a cohort born seven years later, fewer than 30 per cent were married by this age. These authors also provide other related data suggesting that something was affecting the life styles of (certain groups of) US women at about 1970.

The contrastive phenomenon here, then, is the trend in developments before and after the early 1970s. The explanation provided (whose correctness need not detain us here, but which anyway is carefully and convincingly defended by Goldin and Katz) is that these changes resulted in some significant part from both the introduction of 'the pill' in the 1960s along with the change in the legal environment that enabled young unmarried women to obtain it.¹³

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.¹⁴

And so it is with the examples of everyday (contrast) questions listed at the outset, questions to which responses were felt to be feasible: Why are my keys not in my pocket as usual? Why does my bicycle not work as well as it did yesterday? Why did the students do so much worse/better in the exams this year compared with other years? And so forth. It is not necessarily the case that my keys have not been misplaced before, or that the cycle has never before worked badly, etc. It is merely that I had reason for supposing that matters would be other than they are currently found to be.

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, as something of our social-cultural situations, values, and so forth). Knowledge, as I have already stressed, 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 to play in explanatory research, it does not exhaust the latter. Rather the knowledge process is fundamentally dialectical.

A seemingly general explanatory model

The various explanatory scenarios referred to throughout have all been found to be open to investigation via a reasonably general (if abstractly formulated) approach that I have referred to as contrast explanation.¹⁵ This is an approach that certainly uses such partial patterns in events as can be determined. But unlike deductivism, it seeks neither to stylise such demi-regs as are found nor to utilise any such regularities solely for purposes of deducing consequences. Rather, event patterns are but a moment in the causal process which goes beyond them.

Now, not only does contrast explanation not reduce to the sort of deductivist reasoning aspired to in modern economics, but we can at this point see that deductivist 'explanation' is actually a special case of the former. In fact, I want briefly to indicate that a variety of explanatory approaches appear to be (or perhaps more accurately can be interpreted as) special cases of contrast explanation too.

I start, though, with the sort of controlled experimental set-up associated with much of natural science, in which all but one (set of) factor(s) is held constant or at bay. This is the set-up presupposed by deductivists, and in particular economic modellers, including econometricians, even though it represents the configuration that seems least likely (and *a posteriori* least often) to obtain in the social realm.

From the perspective set out it is now possible to recognise this scenario as a very special case of contrast explanation in which two conditions hold in particular. The first is that the contrast space is regarded as enduring (it is at least expected that the experimental conditions can usually be re-established). The second condition is that causal factors operating throughout the contrast space are more than uniform in their action over the space: they are actually constant or reduced to zero. If, in an experimental set-up of this sort, a mechanism is triggered and its effect recorded, the contrast is implicitly between that which takes place prior to the mechanism being triggered and that which occurs after the mechanism is triggered. However, because in the experimental set-up of this sort, there will often be a way of viewing matters to suggest that 'nothing' is going on before the mechanism is triggered, there will be a sense in which it seems that the effect of the mechanism is being read straight off. In truth, though, the effect is determined as the change in outcomes in situations before and after the triggering of the mechanism. This is the contrastive observation. And the change can, with reason, be attributed to the mechanism so triggered. It is this special case, with all background factors held constant or at bay (or, in a statistical set-up, rendered orthogonal to the explicitly included factors), that is erroneously generalised in the economic modelling practices of the modern day.

So contrast explanation ultimately lies behind causal explanation of the sort facilitated even in the most controlled of controlled experimental situations. The latter is but a very special case of the former. Indeed, we can now see that closures of the causal sequence sort presuppose closures (or regions) of continuity (i.e. the intended objects of contrast spaces). However, whilst causal analysis in general requires only that the outcomes of contrast spaces stand in a given relation to each other, the controlled experiment, and exercises in economic modelling, require that they are also held constant.

Actually, there is a more generalised contrastive phenomenon in play here. Behind this discussion of the controlled experiment it is recognised that event regularities of the causal sequence sort can often be produced under experimental conditions but rarely under others. Now an 'interested' philosophical orientation that asks 'Why do they often occur under experimental conditions but not in general?' stimulates an ontological (explanatory) investigation and arrives at the specific ontology of critical realism. That is, by addressing this latter contrast, in explaining it, we uncover something about the basic nature of reality: that it is open, structured and differentiated. For it is only the latter (critical realist theory of) ontology that can render intelligible the result that, by way of experimental manipulation, event regularities of the sort in question are sometimes achieved that would not have occurred otherwise. It is just because relatively strict event regularities of the sought-after sort occur mostly (if not only) under the restrictive conditions noted that they are significant in science. Each experimentally produced event regularity is a measure of the difference between the experimental and non-experimental situations. In the former situation, but not the latter, a (stable) mechanism is both triggered and isolated from countervailing forces so that its effects are correlated with the triggering conditions. Critical realism identifies the ontological conception wherein such a scenario is possible.

So we can now see that contrast explanation has a field of application that is wider in scope than the sorts of examples used to motivate the discussion. For if a specific contrastive phenomenon, restricted to a specific contrast space, allows the identification of a specific mechanism, the systematic contrast between (typically experimental) conditions where strict and stable event regularities of the causal sequence sort are produced (and often reproduced) and others where they are not, allows the inference to the ontology of critical realism (that reality is generally open, but also structured in a way that localised closures are possible, and correspond to, or allow, the empirical identification of underlying mechanisms). Specific contrasts to scientific hypotheses of specific mechanisms; generalised contrasts to philosophical ontologies. Each is an example of contrast explanation.

A further example of contrast explanation is provided, I think, by the situation wherein, despite the context being open, some event in and of

itself is so surprising or interesting that we are moved to ask how or why it happened. In such situations there is also an implicit contrast being addressed, this being between the event happening, and its not happening. In this case the straight negation of the event is the foil. For an outcome x , the contrastive question is 'why x rather than not x '. Now at first sight this might seem to render all outcomes open to contrast explanation, and thereby perhaps to trivialise the explanatory approach being elaborated. But this is not so. For an essential condition of contrast explanation is surprise, interest or doubt, etc., in the contrast. We need a situation where our existing knowledge is such that we expected 'not x ' (or might have expected 'not x ' if we had thought about it) but x did occur. To learn that the outside of person P 's house is not white is hardly going to occasion surprise if we have no reason to expect it to be white. But if we learn that a sports team acknowledged as outstanding has failed to beat a team regarded as very poor, we may well be surprised. The explanation may be complacency, bribes, tiredness or whatever. Although the foil or contrast is the opposite of that which came about, our background knowledge is such that we still have reason, under such conditions, to suppose it likely that an identifiable mechanism is at work. In the final chapter of this book I ask the question why the mainstream formalistic modelling project remains so dominant in modern economics, despite its limited (absolute or relative) successes in illuminating the world in which we live. Clearly I am 'surprised' at this outcome, given there are other potentially more fruitful projects in place. Or at least I find this outcome sufficiently interesting and curious and even regrettable, as to question why it, rather than a more varied or pluralistic academic situation, has not emerged in modern economics.¹⁶

As a final consideration, we might note that a Kuhnian paradigm shift also in effect invokes the idea of contrast explanation. According to the Kuhnian view, any attempt at understanding presupposes a broad framework or paradigm, which is largely taken as given. This is certainly so in much of natural science. However, results are often obtained that cannot be explained within the accepted framework or paradigm, and indeed seem rather problematic in the light of it. When this happens these results are often put aside as anomalies. These anomalies are in effect surprising contrasts, divergencies between the (sorts of) outcomes the paradigm leads us to expect, and what actually is found. Eventually the set of anomalies builds up. At some point the set of anomalies becomes of central focus. In a sense the contrast question is asked: 'why this set of anomalies rather than outcomes consistent with the basic framework?'. A paradigm shift occurs when a new framework is derived and accepted which is at least as explanatorily powerful as the previous one but can avoid, and perhaps make sense of, the recognised anomalous outcomes of the earlier one.¹⁷

Demi-regularities

In the light of the foregoing discussion it is possible, and I suspect useful, to clarify the notion and role of demi-regularities or demi-regs. Some commentators on *Economics and Reality* have (erroneously) interpreted my account of demi-regularities there as expressing one type of situation only. This is the case where a relationship (of causal sequence) holds between measurable economic variables, and does so with sufficient strictness as to facilitate the successful application of standard techniques of econometrics, albeit only within a limited span of time and space. The scenario imagined is one in which correlations between economic variables do occur behind our backs, but only over limited intervals.

For example, an econometric model of aggregate consumption (with consumption expressed as a fixed function of measured disposable income) estimated on UK data over the last twenty years (say) and found to 'forecast' sufficiently accurately within this twenty-year period but not outside it might be said to be underpinned by a demi-regularity so interpreted.

Not surprisingly, once my account of demi-regularities has been reformulated in this fashion, and this fashion only, it has been 'found' that the approach I defend is not essentially different to that of the deductivist mainstream after all.

Now any such uncovered event regularity that is both of the sort presupposed by the use of standard procedures of econometrics and partial merely in the sense of being (sufficiently) strict (for such methods to be applied) but only within limits, is actually a restricted closed system. Even experimental closures are of this form (being restricted to the experimental conditions). In the example just noted it is effectively being claimed that 'within a specific twenty-year period then whenever x then y ' (or a stochastic near equivalent is being expressed).

But still, whether or not such a pattern constitutes a restricted closure, is not the latter (assuming it were to occur) an example of a demi-reg as I have conceived it? The answer is that 'it all depends', and that if or when the pattern in question constitutes a demi-reg, it is but a degenerate special case.

On what does it 'all depend'? Central here is the conception of the 'system' being addressed. The notion of a system in the given context carries no independent meaning. It merely signifies the stretch of conditions, or the space, in which the event pattern in question is taken to hold (or not). In the just noted econometric example an event regularity is taken to hold over a twenty-year period. This period and context is a restricted closed system. Now let us extend the 'system' by ten years. In the later ten-year period it is found that, unlike in the first twenty years, when the antecedent x is instantiated the expected consequent y does not (always) occur. This is definitional of an open system. Should we describe the

system as closed at first and then open, or is it open throughout the period? Nothing hangs on the answer. It depends on how the system is defined. If the first twenty years are considered separately as a system the latter is closed. If the system comprises the thirty-year period it is open.

It is from the perspective of the thirty-year system (and from others containing, but not reducing to, the twenty-year one) that the patterning of events in question, being partial, can be said to take the form of a demi-reg. Strictly speaking, of course, the content of any restricted closure is then a demi-reg when viewed from the perspective of any wider or encompassing system which both contains the restricted closure and also includes at least one occasion when the antecedent event occurs but is not followed by the usual consequent. But a scenario such as the latter is clearly, at best, a special or degenerate case of a demi-reg, one wherein over a continuous sub-period the regularity holds quite strictly. It is a special case for which a closure is a sub-system.

Now in *Economics and Reality* I certainly did not envisage the category of demi-regularity reducing to the case just discussed. Indeed, the latter represents a configuration of a sort of which I questioned the *a posteriori* relevance. My assessment there was that most observed regularities are not uniform or without exception even within limited stretches of space-time.

Moreover, in *Economics and Reality* I indicated my view that the types of demi-reg that both abound and are of some relevance to social scientific advance are very often those which express surprising or significant contrastive patterns (observed over certain contrast spaces as conceptualised above).¹⁸ In short, I maintained (and maintain) that most such interesting and partial patterns have taken the form of contrastive demi-regs.

Enduring or widespread social processes

With this discussion in mind, it is clearly of interest to social research to seek out those scenarios where

- (i) the contrast space is relatively wide (i.e. to seek conditions in which *a priori* it might be expected that a wide stretch of outcomes [over time or space] share a similar causal history), and
- (ii) a resulting (unexpected or otherwise interesting or significant) contrast turns out to be, if less than regular, nevertheless frequently apparent throughout significant stretches of this space.

In other words, an interesting case to contemplate or seek out is where the contrast of interest (if partial) makes an appearance over a wide stretch of time and/or space.

An example of this sort, to which I gave a good deal of attention in *Economics and Reality* (and which I also touched upon briefly above), relates to contrasting rates of measured productivity growth. I noted that, for a given period of time in recent history, measured productivity growth is observed to have been significantly less in the UK than in certain otherwise comparable industrial countries on the continent of Europe. Of course, such a contrast is only surprising or significant if our theories or current understandings lead us to conclude that the contrasted phenomena stand *a posteriori* in a different relation than might reasonably have been expected. In the discussion above (when comparing different contrasts involving productivity performance) I focused only on the immediate postwar period. But actually, a surprising or noteworthy feature of the contrastive phenomenon discussed here, is that up until about 1980 it held, on and off, throughout much of the preceding hundred years or more (when many economic theories would lead us to expect roughly the same performance throughout). And it is the surprising longevity that gives hope of uncovering a mechanism with a degree of space-time endurance.¹⁹ Contrastive demi-regs, then, may be surprising, and so become the object of study, not just because a contrast is involved, but because the contrast in some form makes a somewhat frequent appearance, because it is also a demi-reg with a significant presence over time.

A further example of an enduring contrastive demi-reg of this sort is the relative movement in the prices of primary products relative to those of produced goods over the last hundred years (see Pinkstone, 2002, on this).

Other candidates include the significantly high proportion of senior posts in the academy and industry, or indeed anywhere in most modern societies, which are occupied by men, or the differential in pay for men and women doing comparable work, and so on.

Ultimately, even the sorts of contrasts which involve well controlled laboratory experimentation are forms of widely experienced (often repeated) demi-regs. Experiments, including repetitions of previously successful ones, do often fail, due to bad luck, or through being inexpertly or otherwise poorly performed. And if the results of individual experiments are demi-regs, so too is the contrast between experimental and non-experimental results that ultimately helps ground the ontology defended in this book.

The feasibility of social explanation

To sum up, it is a widely observed phenomenon that in our daily lives we are often somewhat taken aback, or at least find that our interest is aroused, when two (or more) sets of outcomes are discovered to stand in a relation to each other which is significantly contrary to expectation, or at least contrary to the situation which, on reflection, we might have expected in the light of

the theories we currently hold. A precondition for this to happen, clearly, is that we have felt able to form expectations concerning the relation between (or, more precisely, to take a reasoned view about the commonality of the causal history of) the sets of outcomes over a domain that I have called the contrast space (and that these expectations have been disappointed). Further, in such situations we are often able to detect the reasons for our errors. It may turn out that a new causal factor has come into play or, and somewhat significantly, that our understanding of the nature of the contrast space, specifically of the relation between causes operating within it, has been faulty all along.

A recognition and understanding of this daily explanatory activity can inform research methodology for open systems quite widely. For if background knowledge of the sort identified can facilitate day-to-day knowledgeable human interactions, it can (and does) facilitate scientific (and philosophical) advance as well.

The explanatory process so facilitated is necessarily backward looking. The essentially open nature of reality, both natural and social, necessitates that we very often start our explanatory endeavour from situations that have turned out to surprise, occasion a feeling of doubt, or otherwise interest us in some way (the failed experiment, the unusual reaction from mixing chemical compounds, the differing *a posteriori* experiences of apparently similar economies, the accumulation of anomalies, etc.). But so long as surprising contrasts are to be had, this is not a handicap for explanatory endeavour.

It does follow that successful social science will be highly context-(contrast space) dependent. And it follows, too, that science can benefit from drawing on contributors with a range of experiences, capable of being differentially surprised, interested or concerned. So scientific positions and resources should be accessible to people from all backgrounds on methodological grounds, in addition to those stemming from considerations of fairness or democracy.

But whatever else follows it is not the impossibility of a successful explanatory social science (that does not depend on closing or otherwise [knowingly] distorting an open social reality). To the contrary, if we simply transfer the successful social-theoretical practices of our everyday lives into our social-theoretical research activities, albeit, perhaps, with the aim of pursuing these practices in more systematic, explicitly formulated, critically examined and self-reflexive ways, there is every reason to anticipate a more successful performance of our discipline.

Needless to say, there are further issues bearing on all this that I cannot consider here, although I have dealt with many of them elsewhere (e.g. Lawson 1997a: ch. 15; also see Chapter 6 below; and see as well C. Lawson 2000; Runde 1998). In the current chapter I have sought to draw out and systematise those matters I think will often be central to

the process of explanatory endeavour in open systems, but overlooked in the modern economics academy. The concrete details will doubtless be significantly dependent on the context of explanatory study. But progress in knowledge is, in the face of an open social system, certainly possible. For it to be actualised, it should by now be clear, we need to reorient our explanatory approach. I have suggested that contrast explanation, as I have elaborated it, appears capable of being especially useful. But this is merely an illustration of the more general insight already noted that, given the open, processual and highly internally related nature of social reality, we need to be not only analytical in our reasoning, but also, and I suspect primarily, dialectical.