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CONCLUSIONS OF PART IV

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Theoretical and empirical models differ

Chapter 11 had a dual focus. It concerned both the possibility of validating the models obtained with computer simulations, and the difficulties of assessing their explanatory power. According to what criteria should we evaluate such models'validity and explanatory force? Many commentators are tempted to measure the *explanatory potential* of computer modelling via the models'capacity to represent what happens in reality. Further, they are tempted to conceive of this capacity for representation as the extent to which a statistical model 'fits' the empirical data collected. The *validity* of these models is also frequently assessed according to the same criteria. Researchers following this approach quickly conclude that computer simulations lie halfway between fiction and reality, that they can guarantee no conformity with the observed characteristics of the phenomenon that they represent, and that the relevance of these models is therefore suspect and their explanatory power dubious. We already encountered the same objection when we discussed, in chapter 6 and in the conclusions of Part II, the explanatory power of neuronal models**.**

However "the idea of computer simulation or modelling of theoretical ideas is now commonplace in the physical and biological sciences”, as Burch says, “and would need no special attention were it not for the fact that it is still looked on with suspicion in many social science circles." How can we reconcile the potential attributed to simulation models in the natural sciences with their fictional aspect? Thomas Burch shows us the way : computer modelling, he says, is not intended for producing empirical models. Computer simulation helps to model theoretical ideas. Now, a theoretical model cannot be validated in the same way that one tests an empirical model (in particular, Burch is considering statistical models here.) We should therefore stop measuring the theoretical models generated by computer modelling by the same yardsticks that we use for empirical models. If theoretical, the computer model calls for a different mode of validation. Furthermore, its explanatory power must not be confused with its capacity to represent observed phenomena.

The argument of Burch rests on the assertion that theoretical and empirical models differ. How do the two differ? How can we establish the empirical relevance of a theoretical model, if not in the manner whereby empirical models are evaluated? And what explanatory power can we attribute to a theoretical model that does not represent the empirical data collected? I would like to draw attention to one of the paths explored by Burch in order to answer these three questions, raised by of Eugene J Meehan's 'system paradigm'.

Abandonning the 'covering law' approach

Meehan underlines the need of formal structures in scientific explanation. According to him, scientific explanation of an event does not consist of deduction from a 'general law' conceived as an empirical regularity. In other words, explaining why 'A is B' is not achieved by deducing 'A is B' from 'all As are B'. Meehan therefore dismisses the 'covering law' approach to explanation. Meehan does not dispute the deductive character of scientific explanation. But he does insist that the explanation of specific facts consists of deducing them from a formal system, and not from empirical 'laws' (empirical regularities) and initial conditions. What does he see as the nature of scientific explanation? For Meehan, it consists of creating a formal structure which logically implies or entails the observable events, and which generates expectations within its own boundaries. This structure, which Meehan terms a 'system', does not involve empirical variables unlike the 'laws' of the 'covering law' approach. Its components are conceptual or mathematical. The system must be further specified ("loaded") in order to apply to and explain a concrete event or a class of events. And the formal structure should be rendered "isomorphic" to the real world context in which the event is embedded. This formal structure or system is, as Burch clarifies, the logical form of an explanatory theory, even though Meehan does not employ the term 'theory' himself.

We can immediately see the importance assumed by Meehan’s propositions for those, like Burch, who seek to establish the validity and explanatory power of computer simulation models. Suppose we cease to demand that such models must 'fit' empirical regularities in order to be valid, and we seek instead to create by computer simulation a formal system from which the facts to be explained can be deduced. Such a system, if one can be found, will have both validity and explanatory power. This validity and explanatory power are such as we are accustomed to attributing to theories which pass the tests of the hypothetico-deductive approach: the model, like any theory, will be verified – or corroborated – if the facts that can be deduced from it conform to the facts as observed.

Meehan's proposition presents another major advantage to the researcher : the covering law approach of explanation is severely restricting for social science, since there are, as Burch underlines, "so few 'empirical laws' or nomic empirical generalisations of the sort the paradigm requires, which leads to a pessimistic view of the explanatory capacities of the social sciences. Meehan sees the situation not so much as reflection of 'the weakness of social science' as of 'the limited usefulness of the deductive paradigm'." To hold generalisable empirical regularities necessary for true scientific explanation is to sacrifice any possibility of the social sciences deserving such scientific status. To hold instead that explanation in empirical science consists of deducing facts from a formal system, is to liberate social science from a requirement that cannot be met. And it opens new avenues of research, empirical as much as theoretical.

If the social sciences no longer seek explanations of social facts from empirical regularities, are they abandoning the scientific ideal of the natural sciences? On the contrary, it might be argued that the 'covering law' approach conforms so little to the methodology that has made the natural sciences so successful as to be harmful to the development of the social sciences. In abandoning the 'covering law' approach, we will not be widening but narrowing the gap between the social and the natural sciences.

Many philosophers of science today more or less reject the 'covering law' approach, as this is applied to the physical and biological as well as to the social sciences. This rejection is principally a feature of the 'semantic' line of thought in the philosophy of science. Burch makes reference to it. The semantic approach also conceives of scientific theory as a formal system and not as an empirical 'law', and it was from the study of physics that the main proponents of the semantic approach drew their arguments.

The 'semantic' approach in the philosophy of science

This line of thought is represented principally by Patrick Suppes, Joseph Sneed, Frederick Suppe and Bas van Fraassen, although others propound similar theses, including those behind 'structuralist theory', as put forward by Sneed and by Wolfgang Balzer and C. Ulises Moulines. The semantic approach is less familiar to social science researchers than the 'covering law' and the Popperian falsification approaches, but it occupies a central position in the philosophy of science today. This approach goes some way towards offering a satisfactory epistemological basis for differentiating formal explanatory models from empirical explanatory models, as suggested in the conclusions of Part I, of Part II and of Part III. Let us briefly highlight here some key ideas of the semantic approach. Of course, there are numerous differences between the authors lined up behind the 'semantic' approach, differences that need not detain us here.

We begin with the assertion that there are three 'levels' in the logic of scientific explanation : the level of theory, the level of phenomena and an intermediate level. The 'syntactic' approach of logical empiricism recognises only two levels: those of empirical facts and theoretical statements, whereby the fit of the latter to the former is assured by what is termed 'rules of correspondence'. It is not only from logical empiricism that the semantic approach diverges in positing the intermediate level. The thesis also departs from much of the philosophical tradition - as well as the common sense - that observes dualism of ideas and reality, of theory and experience, of subject and object, and so on. To introduce a third level is a radical rethinking of the whole question of scientific knowledge. How, in this new framework, should we conceive of the levels and their interrelations?

The semantic approach refuses to reduce a theory to a sequence of propositions or statements. Theory is an extra-linguistic entity; it is that to which the theoretical statements refer, and one can therefore give different statements or representations of the same theory. It follows from this that the validity of a theory cannot be established by submitting it to a syntactic test. It is this fundamental difference that has led to some to term this new approach 'semantic', with reference to Albert Tarski's 'formal semantics', and as opposed to the 'syntactic' approach of logical empiricism. Following the semantic approach, a theory is a formal system, empty of any empirical content. What then is to be found in this system, and how does it relate to the two other levels of the logic of explanation?

It is not phenomena in all their complexity which are the object of scientific investigation (F. Suppe, 1989, pp.65-66). The researcher retains only those aspects of phenomena that can be characterised by a small number of parameters. For example, in the study of motion from classical mechanics, he retains only masses, distances, speeds and so on. The theoretical system is the system of abstract parameters that commands the selection of empirical variables whose combination is the object of study. The theoretical system is, in a sense, a filter that retains from complex phenomena only that which must figure in the object of research. The system of empirical variables configured by the theory occupies the intermediate level between theory and phenomena. And it is this system of empirical variables that is the proper object of scientific study. It is the behaviour of the empirical system that is subject to experiment and observation, this behaviour whose explanation is the task of the theoretical system, and this behaviour that the theory must try to predict.

The distinction between the higher theoretical level and the intermediate empirical system, introduced to the logic of explanation by the semantic approach, fleshes out Meehan's intuition that the 'theoretical system' must be 'loaded' in order to apply to and explain a concrete event or class of events. The distinction also conforms to Burch's thesis concerning the need to differentiate theoretical and empirical models, and to evaluate them differently. Finally, the epistemological distinction made by the semantic approach between the theoretical and the empirical system (i.e. between the upper level and the intermediate level) takes us back to the key methodological requirement, emerging from Parts I, II, and III of this book, of modelling the conceptual structure, implied by the system's properties, separately from the modelling of the empirical variables.

The problems arise, of course, in correctly identifying the relations between the theoretical and empirical systems, and between their respective modellings. Difficulties also arise in defining the nature of explanation obtained via the combination of theoretical and empirical systems. And the role played by observed phenomena, at the lower level of the logic of explanation, is unclear; the question of realism in science is therefore left unanswered by the semantic approach.

Induction vs. deduction

The covering law approach likens scientific laws to empirical regularities. But that is not the only thing which characterizes this philosophical conception of science. The covering law approach has helped give new life to the idea that scientific explanation is deductive. We noted that Meehan does not deny the deductive nature of explanation. And few among those who are against the covering law approach do this either - as if the deductive nature of explanation was universally assented to in philosophy. Recently, Mario Bunge (1997, p. 412) strongly attacked the covering law approach as this concerns explanation: "Indeed, stating that a certain fact happens the way it does for being an instance of a generalization is no explanation at all, for it supplies no understanding: it is just identifying the fact in question as a member of the class defined by the given generalization." Does Bunge draw the conclusion that we must abandon the idea that scientific explanation consists in deducing facts from law-like generalizations? No. He proposes to substitute for the “laws” of the covering law approach, certain "law statements that incorporate mechanisms of some sort - causal, stochastic, hybrid or other" (id.p.442). Bunge is right, in my opinion, to underline the importance of mechanisms in explanation. But is he correct in retaining the deductive logic of explanation favored by the covering law approach? He writes (id. p. 443) : "In short, the so-called covering law model of scientific explanation is correct but incomplete, for it only covers the logical structure of the same."

From Bacon to Newton, from Galileo to Descartes, the pioneers of the modern sciences deplored the inability of deduction to reveal anything about reality, and this led them to pursue another line of research. This line was termed ‘induction’ by Francis Bacon, but it is deeper than the conception of induction as generalisation from particular instances[1]. According to this deeper conception, induction consists in discovering a system’s principles from a study of its properties, by way of experiment and observation. This was the approach followed by Galileo, for example, in discovering the law (i.e. the principle) of acceleration. It was from the observation that the accelerations of falling bodies are equal, that Galileo induced the principle: 'speed is a function of time'. (I commented on Galileo's law in the general introduction of the book).The same inductive approach was followed by Newton in discovering the law (i.e. the principle) of gravity (I commented on Newton's law of gravity in the conclusions of Part III).

The new line of research begun in the 17th century is the exact opposite of deduction as this is employed in Euclidean geometry. Deduction in ancient geometry consists in deducing consequences from principles taken as given. Induction consists of the deduction of principles from the study of their consequences. When one defines induction in this way, it clearly is conceived as the inverse of the procedure which is followed in deduction. In my opinion, there is no exaggeration in saying that the modern sciences were born from the abandonment of deduction as a method of explanation, in favor of induction. But we must note that no one, during the classical period, gave up deductive argumentation. And no one contested the role of deduction either in the validation of proposed explanatory principles, or in the generalisation of these principles to apply to new phenomena, or in the integration of explanatory principles within an ordered system. Deduction never stopped being regarded as a crown jewel of scientific development. But one has ceased to credit deduction with the power of explaining phenomena. Explaining phenomena means discovering principles which are implied by the phenomena. It does not mean discovering phenomena which are implied by the principles.

The laws of Galileo or those of Newton are explanatory because they are *implied* by the facts. Implied: that means that without the law (without the principle) the facts observed would be different. And that means also that without the law we would not be able to explain why bodies behave as they do. It is also because these laws are implied by the

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[1] "In establishing axioms, another form of induction must be devised than has hitherto been employed, and it must be used for proving and discovering not first principles (as they are called) only, but also the lesser axioms, and the middle, and indeed all. For the induction which proceeds by simple enumeration is childish; its conclusions are precarious and exposed to peril from a contradictory instance; and it generally decides on too small a number of facts, and on those only which are at hand. But the induction which is to be available for the discovery and demonstration of sciences and arts, must analyze nature by proper rejections and exclusions; and then, after a sufficient number of negatives, come to a conclusion on the affirmative instances - which has not yet been done or even attempted, save only by Plato, who does indeed employ this form of induction to a certain extent for the purpose of discussing definitions and ideas. But in order to furnish this induction or demonstration well and duly for its work, very many things are to be provided which no mortal has yet thought of; insomuch that greater labor will have to be spent in it than has hitherto been spent on the syllogism. And this induction must be used not only to discover axioms, but also in the formation of notions. And it is in this induction that our chief hope lies." (F.Bacon, 1960, pp.98-99)

facts observed, that we can affirm that they are founded in observation or experience. And because they are implied by the facts, they can teach us something about reality. In this way we can sum up the empiricism and the realism of the founders of modern science. The debates which have gone on for thirty years in philosophy of science concerning empiricism and realism do not take account of these forms of empiricism and realism.

Descartes opposes the same empiricism to the metaphysicians of his time. Descartes as an empiricist? Metaphysicians posit principles and deduct their consequences. Descartes (1952, 1701, p. 87-88) recommends an inverse procedure. The first step of the analysis, according to Descartes, is carefully to conduct various experimental tests on that which is to be examined, aiming thus to get to know its properties. That done, the researcher "deduces", from the observed effects, "the mixture of simple natures necessary to produce all the effects observed". What are these 'simple natures'? Descartes refers to them as 'clear and distinct ideas', and gives a range of examples of what he means: geometrical figures, area, movement, that two things are equal if both are equal to a third and so on (op cit: 80-81).Analytic demonstration must move from consequences to principles, and this move according to Descartes is in the direction opposite from demonstration in geometry. In the *Meditations* (1647, 1952, p. 387-388), Descartes asserts that there is no other way to achieve knowledge of principles, whether geometrical principles or others. Where geometricians were affecting to deduce consequences from principles, this was – according to Descartes - "because they were so dependent upon analysis that they wished to keep it to themselves, as if top secret." In the same way, we cannot know the nature of something except through observation of its effects. The nature (*causa* or principle) of things is the mixture of 'clear and distinct ideas' "necessary to produce all the observed effects". Cartesian analysis is *inductive* in the sense that it is from observation of the system's properties that it seeks to discover the principles that are explanatory of these properties.

It will have been noticed that induction, such as this was conceived in the 17th century, is not compatible with the empiricism of Hume or Mill, or with the covering law approach. It is hard to see how a law-like generalization could be *implied* by particular statements! It is the case that empiricist philosophy rejected classical induction, revived the traditional meaning of induction (as generalization) which Bacon had rejected, and restored the deductive conception of explanation which came from Scholastic metaphysicians, which was under attack in the 17th century. Things could not have been otherwise, since classical induction is not conceivable or defensible unless a law is a conceptual principle, or a theory in the sense given to the concept of theory by the semantic approach. Still, we begin to have doubts today about the deductive nature of explanation, or at least to feel less comfortable about that which Wesley C. Salmon (1988) has wryly called (drawing inspiration from J. Alberto Coffa) *explanatory deductive chauvinism*. Are we also about to see a renewal of interest in classical induction in the philosophy of science? I myself have suggested in the conclusions to Parts I, II, and III how social sciences can make profitable use of classical induction. They can construct the functional architecture of a social system beginning from the analysis of its properties (Part I), or even discover the deep conceptual structure implied by these properties (Part III). Once the functional architecture or the conceptual structure is known (by induction)**,** then it can guide (by deduction) the empirical investigation of the social mechanisms which generate the observed properties. The method of reverse engineering and Artificial Neural Networks illustrate in paradigmatic fashion the classical inductive procedure (Part II).

What kinds of implications are involved in induction and deduction?

Gardin is not concerned with the formal structure of the relations between sets of propositions. In his opinion, interpretations of archaeological remains differ not due to errors of logic. If several different explanations are advanced, this may be because the authors focus on different aspects of the object under study, or because they have different information at their disposal. In order to validate one theory or another, it is therefore not a matter of analysing the formal structure of the reasoning that led an author from the data to their explanatory hypothesis or from the hypothesis to the data; rather, what is needed is a comparative evaluation of the selected databases, including the proposed analogies and the author's convictions and beliefs.

Although Gardin refrains from analysing the formal structure of the relations between sets of propositions, he is not silent as to the nature of these relations. He confidently asserts that they can be inductive or deductive, and that they should be considered as implications: "if p then q". Would it be instructive to expand on the kinds of implications that are made when induction and deduction are performed? Let us investigate whether the empirical examination of implications can contribute to making the nature of induction and deduction clearer.

We follow the rules adopted by Gardin: the inferences $p \rightarrow q$ that represent the structure of the argument in the build up of archaeological theories are regarded as mere discursive practices. Let us now put the question: by which discursive practices do archaeological authors guide their readers from p to q? In other words, how do they go about justifying their inductive and deductive inferences? Gardin insists on the omnipresence of analogies in archaeological reasoning. The archaeologist typically affirms that, since q' follows from p', q follows 'naturally' from p. But apart from argument by analogy, there are other discursive practices to which we should draw attention. Here is one example.

"An ambitious scheme of that size required not only imagination and skill, as in the preceding irrigation works; it also implied that the 'local' forms of authority of former times had been replaced or complemented by a higher seat of decision, capable of conceiving and imposing the development of a cross-regional irrigation system (...)"

Gardin asserts that p "implied" q. How does he justify this inference? A higher seat of decision is implied by the data, he writes, because a cross-regional irrigation system "requires" a form of authority capable of conceiving and imposing the development of such an irrigation system. Gardin adds that we are not to date familiar with the form of socio-political organisation by which the super-regional authority in East Bactria was exercised. It is not therefore the socio-political organisation (i.e. the social mechanism) capable of generating the cross-regional irrigation system that Gardin *induces* from the system's properties. What he instead induces from these properties is a general function that - whatever the mechanism - the socio-political organisation must have fulfilled in order to generate the observed irrigation system. In this case, the function is the capacity "of conceiving and imposing the development of a cross-regional irrigation system". Different socio-political organisations could fulfil this same general function, and we cannot therefore affirm the necessity of any one among them. On the contrary the general function without which any such irrigation system would be impossible, is *necessary*. What can we learn from this example? First, the inference p $\rightarrow$ q designated as *inductive* by Gardin is not the generalisation of an observed empirical regularity; instead, it is an implication of the form ‘if p then q’. Second, the implication consists of inducing, from the empirical properties of a system, an abstract and general function *necessary* to the realisation of the system's properties. Third, this type of implication corresponds approximately to one of the two types of explanation that I have distinguished in the conclusive section of Part III, namely *theoretical explanation*, that which consists of discovering the combination of concepts without which the observed properties of a system would be inconceivable or impossible. In the example offered here, the explanatory principle is only invoked; it is not analysed as a combination of socio-political functions without which the irrigation system's observed properties would have been impossible. Yet Gardin’s goal is not to explain the properties of the observed system of irrigation by the nature of the socio-political regime that brought it about, but to establish that Bactria had reached an advanced level of economic and political development long before the Persian conquest of the mid-1st millennium. Still, it might be possible, via a close study of the diverse properties of this irrigation system and of other archaeological remains, to discover the combination of socio-political functions without which this irrigation system, and the other observed facts, would not have been possible. It is on this type of argument that theories of the emergence of the state in human history depend: the still vague concept labelled ‘protoetatism’, for example, seeks to define that without which we could not understand a particular stage of societal evolution.

We turn now to a second discursive practice, which can be illustrated by another quotation from Gardin.

"Thus, it is impossible to account for the sudden abundance of coarse hand-made pottery in ancient Hellenistic sites of the Black sea unless we are able to demonstrate first that those unexpected potsherds belonged to the traditional ware of the Sytho-Sarmatian tribes that roamed through Eurasia in the last centuries of the 1st millennium BC and eventually mixed or traded with Greek settlers in that area "

As in the previous example, p implies q. But this implication is wholly different. The sudden abundance of coarse hand-made pottery implies that this pottery was imported by the Sytho-Sarmatian tribes. Now it is the historical process (the social mechanism) generating the observed phenomenon that is implied by the data, rather than an abstract function of the mechanism as in the previous example. By what discursive practice does the author justify this implication? He writes that it would be impossible “to account for” this sudden abundance of coarse hand-made pottery unless we are able to demonstrate the given hypothesis. The necessity affirmed in this implication results from the absence of other plausible historical processes that could explain the presence of this pottery. However, Gardin considers the functions that the presence of this pottery imply : the functions implied by a sudden abundance of coarse hand-made pottery are *importation,* and *mix* or *trade*. It is on these grounds that Gardin regards his implication as justified.

We should note that, once again, Gardin’s *induction* is not the generalisation of an observed empirical regularity but an implication: if p then q. The *necessity* that accompanies this implication has nothing in common with that which accompanied the implication in the previous example: here, it rests on the fact that no other historical process offers a plausible explanation. Finally, we can see that this second type of implication corresponds to the second type of explanation that we distinguished: *empirical explanation* via a social process or mechanism that generates the observed phenomena.

A third type of discursive practice concerns the implications designated by Gardin as *deductive*. Reconsider the first example. Suppose that we have *induced*, following a detailed study of the diverse properties of the irrigation system and other archaeological remains, the combination of socio-political functions without which the irrigation system's properties and the other observed facts would have been impossible. From this theoretical model we could then *deduce* the socio-political mechanism that enabled the gradual construction of the irrigation system : such a mechanism must have been capable of fulfilling the required theoretical functions. Feudal systems, peer polities, or co-operative management; are they all compatible with the theoretical model advanced? In other words, would these different social regimes have been equally capable of fulfilling the required theoretical functions and bringing about the irrigation system with all its observed properties? It is the theoretical model that guides us in hypothesising as to what was the socio-political regime of the epoch. Notice that we *deduce* from the theoretical model only the *possibility* of one or another regime, and the impossibility of others.

Inductions that are implications rather than empirical generalisations, implications that are inductive rather than deductive, and deductions whose conclusions are possible rather than necessary**:** all this sounds strange from the point of view of formal logic. However, these curiosities are the result of applying Gardin's empirical method to inductive and deductive practices. We also observe that these practices come close to fitting within the structure of explanation presented above.

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