§26. RADU J. BOGDAN

Two Turns in Induction

My discussion of Adler's paper concentrates on two major turns in our search for a good inductive logic: the methodological and the conceptual turn. The methodological turn brings together a variety of recent concerns for statistical decision and testing, acceptance, epistemic utilities, local justification, etc. I take Adier's defence of recent developments in inductive logic and his constructive suggestions to reflect the methodological turn. But I point to some tensions in his discussion of local induction, subjectivist theories, and Carnap's programme.

In the second part of my reply I show that the latter can be reinterpreted as a form of conceptual modelling that produces a global probability model. This leads us to consider the conceptual turn in inductive logic. Unlike Adler, I think that Carnap's inductive logic fails to capture the logic of induction in science, and give some reasons for thinking so. I also try to show that this should not deter us from giving due attention to the stage of conceptual modelling in scientific theorizing. Our inductive strategies depend on, and therefore should reflect, that stage.

Jonathan Adler's decision to take Carnap's programme as a frame of reference, review Lakatos's critique of it, and see how and why some post-Carnapian developments can avoid this critique, sets a good framework for our discussion. While discussing some of Adler's claims I want to exploit this framework from a different perspective. Carnap's programme identifies two basic dimensions of an inductive logic, one conceptual and another methodological. Historically, they often appear as conflicting turns in the development of inductive logic. They need not be so if we are prepared to let induction cover more than just prediction or choice or acceptance or justification and thus incorporate both methodology and conceptual constructions. Beyond the details of my discussion of Adler on Lakatos on Carnap this is the view I want to advocate.

1. The Methodological Tum

Most criticisms of, and alternatives to, Carnap's programme indicate a definite turn toward (what Carnap called) the methodology of induction. Carnap's own list of methodological problems [1] anticipates 'most of these reactions as well as many recent concerns for statistical decisions and testing, acceptance and epistemic utilities, etc.Contrary to what Carnap believed, the methodology of induction turns out to be much more than just applying an inductive formalism. To many people it is induction or, rather, it characterizes a variety of ways of doing induction in which prior input of various sorts, contexts, theories, and conceptual commitments, different and often conflicting epistemic objectives (such as content, simplicity, etc.) and theoretical tasks (such as explanation, prediction, etc.) play an equally important role. For brevity, I will call this variety *methodological induction* and the ideology behind it the *methodological* turn.[2] These terms characterize no particular approach to induction and in fact lump (indiscriminately) many approaches together. My reason for introducing them is primarily dialectical: by sharpening the Carnapian contrast between formalism, conceptual framework, language, on the one hand, and use, application, reallife constraints, on the other hand, I want to probe some of its underlying assumptions and suggest a new interpretation. As far as this task goes, what is common to different methodological approaches is more important than what is not.

The methodological turn indicates a tendency to 'spread' inductive justification or support over a more extended, more complex and ramified sequence of steps of which evidential and theoretical support, informational content, parameters of epistemic caution and of conjectured regularities in the domain, etc. are only a part. The conceptual effort to capture these ingredients in one single (numerical or qualitative) measure should not obscure their initial diversity. It is the recognition of the latter that sets methodological induction apart from other approaches.

The methodological turn is also a sign of epistemological modesty. It tells us that in the process of reconstructing scientific knowledge a good inductive logic should apply later and to less by presupposing more -where the comparison is made with premethodological, typically globalist beliefs about what an induction-from-scratch can and should do. I take such modesty to reflect in many ways the locality inherent in methodological induction. To this extent, then, the methodological criteria for a good inductive logic should reflect the various ongoing concerns for localization and, as Adler put it, be less absolute and unidimensional and relativized to the means, tasks, values, and stages of an enquiry.

I take this view of methodological induction to be in some agreement with the final message of Adler's paper. Since, at this point, I do not find much interest in adding details to Adler's able and informative survey, let me mention some areas where my reading of the methodological turn seems to differ from Adier's.

As a matter of philosophical strategy, Adler starts by granting the globalist, induction-from-scratch view too much. Generous as this strategy may be, it entails the risk of taking epistemology as being almost coextensive with a global and presuppositionless account of inductive knowledge. This should be resisted on both methodological and (as we shall see later) conceptual grounds. Indeed, I see this concession as conflicting with the spirit of the methodological turn.

One instance of this is when Adler worries that localization as either acceptance of unproblematic input or contextual relativization may undermine our epistemological ability to account for the objectivity of the growth of knowledge. This worry may be tactical since later in the paper Adler observes that any input and context can be criticized and revised. Still the worry itself is premethodological. Knowledge by induction simply is contextual and input-dependent and to this extent local, and so is its justification. It is the ways people interpret this locality that should concern us. For some it is linguistic (or conceptual) localization, for others the localization is initially subjective, for still others it is (intersubjectively) pragmatic or experiential or built into some background knowledge. We need more epistemological insight and research to sort these out but the f act of locality is as brute as any and to doubt or question it is reactionary.

Another instance concerns Adler's claim that the subjectivist theory of probability and induction explains and justifies the objectivity of the growth of knowledge. This is no longer a tactical claim but a conviction based on the familiar argument that the theory assumes very little, invokes only accumulation of data, and obtains objectivity as intersubjective agreement. There are well-known objections to this being an adequate model of inductive knowledge and I will not repeat them here. Instead, I will only emphasize the implicit equation of the growth of knowledge with a nonmethodological induction governed solely by some priors, coherence, and conditionalization. The subjectivist's point is (or had better be) not that we induce this way, for there is no convincing psychological or social evidence that we do. Whether we should induce this way really depends on what we take, and want, our knowledge to be; but this requires another discussion. The real point behind Adler's claim is rather that we can retrospectively reduce our inductive feats to the model, and thus provide an adequate justification. It is a logical virtue of the subjectivist theory that this can often be accomplished. But the trouble with this theory is that it either takes induction to assume too little, which is quite unrealistic, or it is willing and able to incorporate many non-evidential factors and parameters but, as in a conditional proof, the latter get eventually dismissed as auxiliary premises, which goes against the methodological way of looking at inductive justification.

Finally, whereas I see methodological induction to be a radical departure from Carnap's inductive logic, Adler seems to contemplate a certain continuity. So, unlike Lakatos, he thinks that Carnap's program is not a degenerating one. If, as it seems, he has in mind Carnap's (pure) inductive logic designed to capture induction in science (the aim of the 1950-2 system), then I again disagree. I think that there are enough criticisms around, some well documented in Lakatos' essay, showing why Carnap's inductive logic fails to be a logic of scientific induction. I share the view that Carnap's later shift to a normative decision-theoretic position can be taken as a tacit admission of this failure.

If this is so, then what is the role of an inductive logic like Carnap's? And what exactly is inductive in such a logic? Let us consider these questions under a new angle, one which might also contribute to a better understanding of what makes induction possible.

2. The Conceptual Turn

The angle I am going to discuss now marks the *conceptual* turn in the design of a good inductive logic. The conceptual turn is intended to refer here to a crucial stage of scientific theorizing, a stage of rational ideality which enables us to deal conceptually and formally with an empirical reality. I will call it the stage of *conceptual* modelling when an abstract, idealized model of an empirical domain is posited. It attributes a neat, well structured 'ontology' (of ideal gases, or mass points, etc.) to an otherwise untidy, open-ended part of the empirical world. Such models are not the result of inductive experience, although the latter may test their adequacy and serviceability. Nor should they be confused with specific, empirical claims intended to account for what happens in the model-posited world. Nor, finally, are they to be regarded as outcomes of lucky guesses. In other words, they are independent of induction, discovery, and methodology, at least as currently viewed.

Naturally the models scientists cherish most are formal. In many sciences these are probability models. They define a space of possible events, and incorporate. or are associated with, certain probability distributions and certain general patterns of (in)dependence, (non)randomness, (a)symmetry, and the like, governing those distributions. Quite often statistical analysis starts from such models.

With this sketchy background consider now the following slow motion reconstruction of Carnap's enterprise. It starts by being an exercise in mathematical theorizing. Up to a point, to use Freudenthal's term, only the 'infrastructure' of first-order logic and its semantic representation distinguish it from what a pure probability theorist does. Nothing inductive so far. Beyond this point, however. we can take Carnap as constructing an implicit universal or *global probability* model whose posited ontology (regarded purely semantically) may be characterized as an 'urn ontology', i.e., a most abstract representation of a chance (or stochastic) set-up. There is (strictly speaking) nothing inductive about this stage either. Only some general assumptions about the ontology are made.

These two stages are clearly acknowledged by Carnap [3] in his discussion of the reasons for accepting the axioms of his inductive logic, in particular the 'general axioms'. He then considers, in a third stage, some 'special axioms' which are designed to capture some inductive constraints, basically learning from experience. But let us read the end result in a different way. Suppose we want to capture C*-induction but take the axioms as so many constraints our model imposes on the world where this induction takes place. In other words, suppose that we look for a world that only Carnap c*-model fits. Then ask yourself, somewhat transcendentally: What kind of world would make this supposition true? An answer that comes to mind is a certain statistical-mechanical universe. Another, suggested by Ian Hacking, is that of a metaphysical universe as envisaged by Leibniz. Be this as it may, the point is that in one form or another such an universe comes with associated assumptions concerning the probability distributions of its basic configurations and concerning patterns of (in)dependence, (non)randomness, etc., governing the former. It is this group of associated assumptions that enables the model to deliver the required logic of confirmation. But let us see if the model actually delivers the logic. Consider several important possibilities.

In the spirit of our earlier transcendental exercise, consider a world that instantiates Carnap's c*-based model. Then one has either to accept the strong assumptions associated with such a world, [4] which is a very stiff price to pay for an inductivist, particularly because there is very much prior input to rely on; or one has to face the serious objection that in such a world there may be no need for induction to begin with, and that probability deductions from the model may suffice. Then there is the conflict discussed by Salmon: If the degree of confirmation is designed to capture the basic Humean dimension of induction, namely the logical independence of the past/observed from the future/unobserved, and do so via partial entailment, then learning from experience is impossible. If, on the other hand, the degree of confirmation captures the latter, it fails to account for independence. [5]

In the statistical-mechanical version of the world that we are still contemplating there is a familiar illustration of these two sets of objections. Consider the latter. Carnap's ct corresponds to the Maxwell-Boltzmann statistics while his C* to the Bose-Einstein one. The former treats individual particles as being (practically) independent whereas the latter does not. So far no particle has been found to obey the Maxwell-Boltzmann statistics. This may be an empirical accident, although there are people who think that this is no accident because the assumptions of independence and noninteraction underlying those statistics are simply wrong.' Be this as it may, we are left with the important and philosophically plausible suggestion that we should contemplate a natural connection between ontological dependence and interaction and the possibility of learning from experience. After all, this is why causation plays such a central role in induction (as Hume himself was so much aware) and why the acquisition of information is possible only when finding or positing structures in the domain under investigation. Consider now the former set of objections. A Bose-Einstein universe, for example, comes with so many (theoretical, empirical, stochastic) assumptions that either induction is no longer needed or its working in such ideal conditions is irrelevant to the rough world of the methodologist. In a sense, this explains Carnap's nonchalance toward, say, scientific laws or inductive acceptance. Indeed, in such an universe one does not look for laws; most lawful features are already contained in the assumptions. Nor does one need to accept anything, in any plausible sense of acceptance; one just makes th required computations, and this is what Carnap meant by probability assignments. In other words, it is the strength of the assumptions that enables Carnap to concentrate on singular predictions only and to disregard acceptance.

But many people will disagree with this transcendental construal, so let us relax its requirements. Up to a point, I think, one can still make the same claim. Thus suppose that, locally, Carnap's model applies to a given empirical situation where a particular statistical probability (be it relative frequency or propensity) is known and reflected by a Carnapian inductive probability. Then, by Carnap's own admission, [7] the latter may as well be dispensable. If, on the other hand, that statistical probability (or a parameter) is unknown, then an estimate is required. Although the problem of estimation in Carnap's work is a tricky one, here are some possible objections. First, there is the objection that the estimate itself is a tentatively accepted conjecture,' which is a very un-Carnapian thought. Second, if this is not so, then estimation is again a purely formal computation and one falls back on the previous objections as to why this is possible in the first place. Finally, I see a potential and unilluminating regress in Carnap's notion of the reliability of an estimate.

Thus, no matter how looked at, Carnap's probability model fails to deliver a consistent logic of induction. This is where my perception of Carnap differs from Adler's. My distinction between model and logic, and the resulting reconstruction of Carnap's programme are not only heuristic. Mature scientific theorizing consists very much in empirically interpreting given conceptual models-when it is independently established or assumed that type empirical domain obeys the constraints of the model. This then guarantees the applicability of formal methods and calculi. Carnap's mistake was to believe that his global probability model can deliver the logic of evidential support, or confirmation, in science. The objections presented so far (as well as many others in the literature) have the feature of either breaking the connection between Carnap's logical probability (based on his model) and induction, or building so many assumptions into this connection that induction becomes a mere exercise in computation relative to a universe about which we already know a lot.

In addition to all this, there is more to induction than evidential support, and there is more to evidential support than a probability-model it may rely on. Then there are many contexts where no probability models are available or where different models are used, in which case there might be no conceptual grounds for support to be probabilistic. Even when support is probabilistic, and relies on an adequate model, the assumptions that a scientific theory associates with the model are going to make a lot of difference. This is what is going to distinguish the powerful models of mechanical statistics from those, say, of population statistics. To a large extent, the postcarnapian developments that Adler successfully defends against Lakatos's critique of Carnap reflect an awareness of these various circumstances. But they do so, I believe, by radically departing from Carnap's initial programme.

The Carnapian failure, however, should not obscure the crucial role conceptual models play in our understanding and design of a good inductive logic. Otherwise we would not only misrepresent or totally ignore a vital segment of scientific theorizing that has a bearing on our inductive strategies but, philosophically, concede too much both to the excessive methodolokist and the radical subjectivist. Consider again our transcendental exercise. What it assumes, they would say, is that there is a structurally true story of the universe which fits a certain model. But in real life, they would go on, we do not know whether this is so or not. This is where empirical knowledge and induction come in, and where the methodological (or subjectivist) turn, or rather retreat, starts from. This may be correct in the long run but (remember Keynes's phrase?) at each stage, before so retreating, we had better make sure that we have an idea (model, projection) of what the domain of enquiry is structurally like, i.e. what configurations of entities, properties, and relations should we expect to find. This is precisely what models help us to do.

At issue here is also the problem of in-formed realism. It does no good to say, as many philosophers do, that scientific knowledge approaches a mysterious, formless truth. We had better have an anticipation of what this truth might be like - or else we might miss it altogether. First metaphysically, and then through modelling, this is how science operates.

Excessive methodologism misses this point. Here I would side with Adler against Lakatos. Thus, although aware that Carnap's inductive logic works when applied to 'closed games' or 'closed statistical problems', [9] Lakatos maintains that science **is** an open game' and that 'urn games are poor models of science' on the (Popperian) evidence that the possible variety of the universe is not exhausted by urns and balls and that in fact 'you may equally well pull out a rabbit, or your hand may be caught in the urn, or the urn may explode...... [10] Possible as this may be, it still betrays a misunderstanding of modelling in science. Although, as we saw, Carnap's conflation of a probability model with the logic of confirmation may have contributed to this confusion, the closed (urn or statistical) games are certainly models in, and not of, science. It is precisely by positing a structurally idealized ontology associated with certain regularity patterns that such models have the serviceable virtue of not allowing urns to explode or contain rabbits or not letting us bother if they occasionally do. When this occurs more than occasionally we may be well advised to play a different game with different models.

The catastrophic view I am criticizing here squares not only with the uninformed and unanticipating view of truth discussed earlier but also with the (still widely shared and respected) Humean notion that ontological anarchy follows from the logical independence of individuals or events. To show that both the catastrophic view and the Humean notion are mistaken one has to examine the role theories play in induction and the many ways in which models, laws, and strategies of generating data conspire in detecting and/or imposing higher (such as invariance, conservation, etc.) and lower (i.e., pertaining to specific laws) regularity patterns in the empirical domain under investigation. [11] That such patterns may be wrong is no argument for their absence or dispensability. Fallibilism does not entail anarchy in knowledge. The problems of uniformity and projectibility, about which Adler has some interesting things to say, should be approached along these lines too.

3. Concluding Remarks

As I said in the beginning, the implicit view underlying this discussion is that induction is a multidimensional affair, and that most of these dimensions are not by themselves inductive. I take this to be the main lesson of the methodological turn. A strong prejudice that tends to obscure this lesson is that induction is a simple inference or computation. This is the rationalist ideal of the inductivist. In demolishing it Hume had perceptively shown what ontological assumptions, if independently vindicated, would bring us close to the ideal. Although still obsessed by the ideal, our inductive interests are more regional and dependent on the type, sophistication, temporal stage, and aims of a scientific enquiry. Even when so constrained, an inductive strategy relies on a prior model of the domain of that enquiry. I take this to be the main lesson of the conceptual turn. Such models will themselves be regional and dependent in the earlier sense, and in turn will make different inductive strategies possible. That the probability calculus applies or that statistics takes over completely is only a tribute to the strength of the model and of its associated assumptions. That, when this happens, we let either of them measure support, etc., and guide our degrees of belief, is not so much, or not primarily, an indication of rationality as it is a commitment to what made such measurement and guidance possible in the first place.

I do not want to conclude without mentioning that the tension between the conceptual and the methodological turn is an old story in the philosophy of induction. Jevons, for instance, was an optimistic conceptualist who based his reduction of induction to probability on a model according to which 'nature is to us like an infinite ballot box, the contents of which are being continually drawn, ball after ball, and exhibited to us'. The same model was contemplated by Peirce before seeing, somehow in the spirit or our earlier transcendental exercise, what assumptions go with it, and turning methodologist. And so on. The story did not change this century, both before and after Carnap. Are we then destined to go through the whole thing again and again? Yes, I am tempted to say, if we want either to capture induction in a probability model alone or to disregard such models altogether. Both positions are extreme and invite cyclical counterreactions. No, if we take a closer look at models, methodology and the various parameters of scientific theorizing and see a local interplay at work. The key to a good inductive logic may be found in the overall interaction of these elements rather than in any particular one. [12]

Notes

1 See his *Logical Foundations of Probability* (1950), sees. 44A, 48, and 49.

2 The ideology is exemplified elsewhere as well. Popper's critique of inductive logic is an instance of it, and so is the recent historico-critical reconstruction of science. Lakatos's own view tries to bring all these together.

3 Carnap, 'Replies and Systematic Expositions' in P. A. Schilpp (ed.), The Philosophy *of Rudolf* Camap (1963), p. 977.

4 This is a position taken by Ian Hacking. See his The *Emergence of Probability* (1975), ch. 15. Carnap's view is that such assumptions are generally methodological.

5 Wesley C. Salmon, 'Partial Entailment as a Basis for Inductive Logic' in N. Rescher et al. (eds.), Essays in *Honor of Carl G.* Hempel, (1969), pp. 47-82.

6 Significantly, this is Harold Jeffreys's view in his Theory *of Probability* (1939), sec.7.6.

7 R. Carnap, *Logical Foundations of Probability,* sec. 49B.

8 See Jaakko Hintikka, 'Carnap versus Essler versus Inductive Generalization', Erkenntnis 9, (1975), 240-1.

9 ' Imre Lakatos, 'Changes in the Problem of Inductive Logic' in 1. Lakatos (ed.), *The Problem of* Inductive Logic (1968), pp**.** 373, 407.

10 Lakatos, op. cit., p. 401.

11 I have briefly dealt with this in 'Hume and the Problem of Local Induction' in R. J. Bogdan (ed.), *Local* Induction (1976). See also Roger Rosenkrantz, Inference, *Method, and Decision* (1977). ch. 4.

12 "Some of the ideas presented here were discussed with Ian Hacking, Jaakko Hintikka, and Paul Humphreys. Their reactions and suggestions were very helpful, and thev will find here my warm thanks. I also want to thank Jonathan Adler for the excellent and stimulating interaction we had while I was preparing this paper.