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Essay Review

Trouble-Shooting Creativity: A Critical Appraisal of David Bohm and F. David Peat's *Science Order & Creativity*[1]

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The difference between pure or basic science and applied science is that the former aims at understanding reality, while the latter endeavors to improve it. Both represent intense goal-directed trouble-shooting activities; each constantly seeking to unearth, and subsequently to solve problems. But the types of problems they seek to solve are different. A pure, or basic scientific problem arises when a theory, an account of nature, is found in some sense inadequate; when *science* is thought to fail. A problem in applied science arises, on the other hand, when nature itself (as described by pure science) is found allegedly incapable of realizing certain practical objectives; when *nature*, rather than science, is thought to fail. Needless to say the two projects are closely related. The relationship, however, is not entirely symmetrical. Applied science provides valuable testing grounds for the claims and theories of pure science. But pure science is quite capable of flourishing even in areas devoid of practical applications. On the other hand, applied science is applied *science.* Devoid of theoretical insight, the applied scientist is blind.

All of this is equally true of the philosophy of science. Most contemporary philosophy of science is 'basic', seeking primarily to describe and to understand science rather than to improve it. Pure or basic philosophy of science takes science as it finds it. It ponders its theory structures, its test procedures, the nature of its decisions, its language(s), its logic(s), its modes of reasoning, its institutional and communal life, the complexity of its relationship to its wider social,

[1] London: Routledge, 1989 (first published by Bantam Books, 1987).

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political and cultural context(s). It is seldom explicitly normative. For most professional philosophers of science, science is considered a curious and fascinating phenomenon demanding of explanation. Science thus largely stands to basic philosophy as literature to literary theory or as music to musicology. Basic philosophy of science is ideally an interpretative rather than a constitutive, or even a corrective enterprise. As in basic science, the type of problems raised and addressed by basic philosophy of science pertain to the shortcomings and apparent inadequacies discovered in its own second-order theories *of* sciences. Trouble-shooting is here also a *reflective* activity, directed primarily towards the self-improvement of philosophy. Basic philosophy of science, in short, seeks to expose and rectify apparent failures on behalf of philosophy to understand science rather than apparent failures of science to do its job.

But not all philosophy of science 'basic'. Joseph Wayne Smith's *Reason, Science and Paradox*, to take one extreme example, urges philosophers of science to 'take up once more the enterprise of *philosophia perennis*'. Rather than view philosophy as a merely curious bystander, or, worse, 'as a mere underlabourer of the sciences', writes Smith[2] a 'metaphor involving prison guards poised with machine guns may be more appropriate'. The problems Smith undertakes to expose attest not to faulty second-order philosophizing, but to alleged first-order scientific failure and confusion. His aim as a philosopher is not merely to understand what scientists are up to, so much to apply such undestanding in order to expose their apparent failures to achieve their own objectives. Smith's self-appointed corrective philosophical project for the sciences is as thoroughly negative as it is antagonizing. 'Philosophy', he maintains 'Can be viewed as a *sui generis*' knowledge producing enterprise in a number of respects, one of the most important being the establishment of essentially negative results such as the contradictoriness of theories and the intelligible limits of cognitive enterprise'.[3] The concluding paragraph of his chapter on probability theory summarizes his approach: 'It is not my aim in this work to suggest new alternatives to defective positions, but merely to engage in a much needed slash-and-burn exercise. Perhaps from the ashes of this scepticism the search for a new non-probabilistic, non-formalist approach to the foundations of statistical inference m[a]y grow'.[4] The slashing and burning once completed, it is left to the now duly humbled and closely invigilated scientist to attempt to put things right.

[2] J. W. Smith, *Reason, Science and Paradox: Against Received Opinion in Science and Philosophy*, London/Sydney/Wolfeboro/New Hampshire, Croom Helm, 1986: 3.

[3] *Ibid.*

[4] *Loc. cit.* p. 149.

David Bohm and David Peat's *Science, Order & Creativity,* is also a work of applied philosophy. Like Smith, they forcefully raise problems that attest to scientific rather than to philosophical failure. Their style and rhetoric is decisively less obnoxious, but their critique of science is equally devastating. Here, however, all resemblance between the two books ends. In fact, one could say that for Bohm and Peat, Smith's very undertaking is centrally symptomatic of the main problem that in their view besets science today. *Science, Order & Creativity* is not at all concerned with the conceptual coherence or logical consistency of this or that scientific theory. On the contrary, according to Bohm and Peat, the problem with science owes, at least in part, to a lack of playful conceptual and logical laxity on behalf of scientists. Science, they argue, has become conceptually rigidified to the point of fragmentation. The situation is so far gone, their book implies, that professional philosophy of science can no longer help. It is no longer a question of understanding science better for what it is, as basic philosophy of science would have it, or of pointing aggressively, as does Smith, to conceptual or logical lapses in specific areas of scientific reasoning. For science to (re)take its place as 'the key to increasing progress and the betterment of life' (p. 15), they submit, it needs to be (re)rendered creative. In order to do so, they claim, science is required no less than 'to operate in a radically new way, in which fundamentally different ideas are considered together and new perceptions made between them' (p. 63). The problems divulged, analyzed and allegedly solved in *Science, Order & Creativity* are not scientific problems. They attest to a fundamental failure of science but not to scientific failure *per se*. Bohm and Peat's meta-scientific undertaking cannot afford, therefore, to remain negative. However, neither science itself nor current professional philosophy are capable of the radical positive rethinking required, in their view in order to restore and ensure scientific creativity.

Bohm and Peat's prognosis for the sciences is bold, but perhaps not quite as radical as they make out. The truly radical critics of science, notably Bacon and Descartes in their time, and perhaps also Alfred North Whitehead in this century, called emphatically for the entire scientific enterprise to begin anew. Bohm and Peat do nothing of the sort. Their complaint hardly touches on the content of current scientific theories. What they find deeply disturbing has little to do with what scientists actually say or even how they say it, but with the rigidity by which their views are held and treated thereafter. In this respect *Science Order & Creativity* is a timely and thought-provoking book that raises a cluster of important philosophical problems seldom addressed by philosophers. Its philosophical radicalism, however, even within its limited domain, is injustified. Their book is vexingly out of touch with much of latter-day history and philosophy of science, from which, as I shall argue, it might have greatly benefited in both its negative and positive claims. Written out of it as they are, it is none the less a book that professional philosophers should take seriously.

The problems dealt with in *Science Order & Creativity* converge on what Bohm and Peat term 'the fragmentary approach of science to nature and reality' (p. 15). As opposed to the perfectly legitimate and natural division of an area of knowledge into particular fields of specialization, science, they submit, all too often exhibits a tendency 'to impose divisions in an arbitrary fashion, without regard for any wider context, even to the point of ignoring essential connections to the rest of the world' (pp. 15-16). While the first type of division normally reflects true scientific progress, the latter, they urge, is frequently detrimental, if not fatal to scientific advance. The difference between a natural and fruitful division of an area of knowledge and one which is arbitrary and fruitless is not sufficiently clarified by Bohm and Peat, but the general gist of their complaint is clear. Fruitful divisions normally result from the need to delve deeper into specific issues without losing sight of the larger picture. Scientists often chose to concentrate on specific aspects of a problem to the temporary deferment of others. One does not have to endorse all of Nancy Cartwright's thesis as to 'how the laws of physics lie', for instance, to appreciate both the extent and value of self-consciously employed *ceteris-paribus* clauses in physics.[5]

Fragmentation, by contrast, attests to a far more arbitrary, unreflective and *prima facie* unjustified bracketing off of major theoretical premises. More times than none, submit Bohm and Peat, such moves are symptomatic of a tacit 'defense-mechanism' that serves *ad hoc* to insulate the hard-core of today's scientific consensus by redirecting the thrust of seemingly problematic new findings to the periphery. And thus, although 'the modern mind' no longer considers the recognition of 'absolute truth' as a viable scientific objective, 'and scientists have become accustomed, at least tacitly, to accepting the need for unending change in their basic concepts', many of them continue 'to defend the tacit infrastructure of the whole of science with great energy' (24-5). The result is lamentable. Novel insights tend to be automatically set aside, or at best relegated to the role of mere instrumental moves valued for their formal utiliy rather than for their conceptual content.

[5] N. Cartwright, *How the Laws of Physiucs Lie*, Oxford: Clarendon Press, 1983, especially pp. 46-7.

True challenges to the consensus seldom receive a fair hearing. Regrettably, note Bohm and Peat, Kuhn's description of a paradigm-governed 'normal science' is true of much of today's physics.

However, rather than treat Kuhn's widely read book as a disturbing diagnosis of the way scientific institutions are run, most scientists have come to accept it as the norm. Kuhn's model itself, one might say, now functions as a second-order paradigm, informing the interiorized self image of the modern scientific mind. 'Paradigms', write Bohm and Peat,

> especially after they have been established for some time, hold the consensual mind in a 'rut' requiring a revolution to escape from. Such excessive rigidity amounts to a kind of unconscious collusion, in which scientist unconsciously 'play false together' in order to 'defend' the currently accepted bases of scientific research against perceptions of their inadequacy (p. 61).

Bohm and Peat would have benefited at this point from Imre Lakatos's influential 'Methodology of Scientific Research Programmes', which, far more than Kuhn's seemingly detached description of normal science, endeavors to prescribe an outspokenly normative model of scientific research. Lakatos's model advocates (rather than merely points to) the existence of rigid 'hard cores' insulated by 'protective belts' of 'auxiliary hypotheses' actively and deliberately defended, at times even, 'pig-headedly' by 'negative heuristics' designed intentionally to shield them from disconfirming findings. One important difference between Kuhn's and Lakatos's accounts of science would seem highly relevant to Bohm and Peat's concerns. While Kuhn depicts exemplar mature normal science as governed by a single paradigm, Lakatos envisions science at its best when different 'research programmes' compete for the same ground. Though governed by different paradigms (as Kuhn would have it), Lakatos's competing research programmes are not incommensurable, at least not in Paul Feyerabend's crippling sense of the term. Their accumulating track records, are comparable, according to Lakatos, regardless of whether or not the actual theories each of them generates are mutually translatable. Lakatos's second-order methodology (facilitating choice between rival programmes as opposed to first-order theory choice within a particular programme) is grounded upon an analysis and evaluation of what he loosely terms 'problem shifts'. A research programme is regarded 'progressive' and worth further pursuing, if the theories it generates remain over time a step ahead of the facts as it were; if they entail novel empirical predictions, of which some at least are later borne out by experience. Conversely, a programme will be considered 'degenerative', and a candidate for the scrap heap, if over time its theories remain persistently a step behind the facts, having repeatedly to accommodate new findings by *ad hoc* modifications.

At first blush Lakatos's account seems to fare well with Bohm and Peat's main examples – e.g. the alternative to Newtonian mechanics suggested by the Hamilton-Jacobi theory which, in their opinion, never received a fair hearing despite it being at the time as 'progressive' as Newton's better established approach, or Heisenberg's and Schrödinger's contradictory though equally 'progressive' approaches to the quantum theory. In this respect Lakatos's model certainly seems a step up from Kuhn's. But Bohm and Peat's vision of scientific creativity aims at considerably more than the type of alleged second-order pluralism granted by Lakatos. It makes little difference whether mutually exclusive paradigms are adopted in succession by the same community *a là* Kuhn, or entertained concurrently by different communities *a là* Lakatos. In either case the paradigms in question will be equally entrenched and likewise rigidly defended. And the fact that Lakatos's model endeavors to provide rational criteria for paradigm choice is at best beside the point. The fact that two research programmes are conceived at any one time as mutually exclusive should *not*, in Bohm and Peat's view, inevitably lead to an either/or decision, nor even to a 'pig headed' decision to defer judgment temporarily for the sake of giving the currently less successful one a second chance. What is required, they urge, is for the *same* scientific community to be able tentatively to set aside the apparent contradictions between rival theories, and to seriously explore novel ways of playfully relating them through creative metaphor. Within Lakatos's framework trans-programmatic trading and exchange is wholly confined to the level of empirical finding. Confirmed predictions of a progressive programme become the explanatory problems of its rival, now required to accommodate them theoretically. (And should it be found to do so repeatedly only by *ad hoc* modification, it will be ruled degenerating and eventually abandoned). Rival hard cores, as their name implies, remain for the Lakatosian, in principle, rigidly insulated, sheltered from criticism and subsequently unmodified. Lakatos's philosophy of science encourages and fosters fragmentation almost as a matter of ideology.

If the conservatism that characterizes the works of Kuhn and Lakatos (despite their differences) was indeed representative of the current consensus in philosophy of science, Bohm and Peat's radicalism would be justified. There is surely little sense in seeking the aid of a philosophical system in order to combat the very ideas it sanctions or allegedly presupposes. But the Kuhn/Lakatos approach is not representative of all current philosophy of science. Karl Popper's work stands in stark contrast to both the implied conservatism of Kuhnian normal science and the motivated conservatism of Lakatos's Methodology of Scientific Research Programmes. Bohm and Peat's failure to appreciate the full force of Popper's innovative approach not only deprives their diagnosis of an important ally, but, as I shall argue shortly, renders questionable the main thrust of their prognosis.

Bohm and Peat envisage a truly creative scientific community as a well-informed, knowledgeable and open-minded group of researchers playfully uncommited to any one theory. A creative community will encourage and reward the construction of bold new theories, preferably those that challenge the common view most profundly. As a matter of policy, such theories will be granted long periods of gestation, during which they are 'played true' in conjunction with their better established rivals.

> Fundamental ideas need to be sheltered for a while in a spirit of creative 'play'. This should be acknowledged within the scientific community as being a necessary period in which the new idea can be discussed openly and refined... Theories need no longer be considered as rivals, and the problem of determining criteria for choosing between them becomes less urgent. It is even possible that the same scientists may entertain several alternatives in the mind at once and engage in a free creative play to see if they can be related, perhaps through a creative metaphor (pp. 59-60).

Underlying the methodology implied in *Science, Order & Creativity* is the idea best described as an attempt to redefine the nature of scientific queries. If T_1 and T_2 are two partially co-referring and contradictory theories, the first question to be asked should not be: how can we best determine which of them to retain and which to reject, but rather: whether it is possible to modify and adjust our fundamental concepts so as to retain them both. The very existence of a theory considered fundamentally at odds with the accepted view should first be regarded as a potential falsifier of whatever is thought to deem them incompatible. Our first reaction to theoretical disparity, argue Bohm and Peat, should be to suspend judgment, to doubt the disparity itself rather than to take it at face value and to act upon it. Perhaps it is possible to view the situation anew in ways that render the incongruity but apparent. Bohm and Peat thus envision scientific creativity at play primarily at the deeper and fundamental level of second-order metaphysical trans-theoretical conceptual schemes, rather than at the level of first-order operational theory. 'Science will flourish in a more creative way', they write, 'if it allows a diversity of different theories to flourish. When communication between these different points of view is free and open, so that a number of alternatives can be held together at the same time, then it is possible to make new creative perceptions within science'(p. 83). For Lakatos, by contrast, all metaphysics, properly considered within the realm of scientific contemplation, belongs within the hard core of particular research programmes – duly unquestioned, and actively sheltered from modification. Popper's theory of science is on this point decidedly different from Lakatos's, and in important respects closer to Bohm and Peat's. In the face of problems no area of knowledge should be sheltered. And since apparent problems are our main, if not only real source of knowledge, the bolder the conjecture, the better.

Bohm and Peat, however, limit their discussion, almost exclusively, to the problem of theory-choice (or suspension of choice). They largely ignore the more conventional areas of scientific trouble-shooting, such as those involved in the straightforward empirical testing, interpretation, or the mathematization of single theories, despite the fact that they undoubtedly represent the main bulk of day by day science. In a sense their omission is understandable. Such run-of-the-mill scientific research, conducted in the light of one particular theory, bears less directly perhaps on the problem of inter-or trans-theoretical fragmentation with which their book is primarily concerned. But then *Science, Order & Creativity* aspires to treat the wider and more general issue of scientific creativity. In this respect their omission is less understandable.

Creative scientific thinking does not begin at, and is not confined to the research situations focused upon by Bohm and Peat, namely, those in which several alternative, seemingly incompatible though equally plausible theories are already available. Interesting as it may be to the philosopher, the current state of quantum mechanics, that furnishes their main case-study, is hardly typical. Moreover, by ignoring the process(es) by which the various theoretical alternatives came to be proposed in the first place, even the partial picture of science they attempt to paint is considerably distorted. As they aptly note (p. 101-2), there is usually more to a scientific theory than the observations it aspires to accommodate and the mathematical formalism it elects to don. Theories, proposed as explanations or interpretations or classificatory schemas of phenomena, normally premise and reflect, if only implicitly, deep metaphysical commitment[6]. However, scientific

[6] Cf. J. Agassi, *Science and Society: Studies in the Sociology of Science*, Reidel/Dordrecht/Boston/London, 1981, Chapters 18, 19 and 21.

theories are seldom born of purely metaphysical deliberation. Rather, as Popper persistently urges, they are best described and treated *as proposed solutions to scientific problems*. And it is primarily *qua* solutions to the problems they were initially intended to solve that theories enter scientific debate, including at the advanced level dealt with by Bohm and Peat. However, when properly couched in the processes of problem-seeking and solving that initially begot them, alternative scientific theories are not as easily reconcilable as Bohm and Peat envisage, even from the point of view of a philosophy that fully endorses their basic anti-Kuhnian approach. This, in my opinion, is their most serious oversight, as well as an interesting problem in its own right. But to appreciate it as such, a little more needs to be said about the nature of scientific problems in general.

Pursuing a staunch realist agenda, Popper himself, as is well known, limits scientific problems to empirical disconfirmation. A theory is deemed scientifically problematic, according to Popper, if (and one is tempted to add: only if), in conjunction with backgound knowledge, it logically entails predictions that are incompatible with whatever are considered the facts at the time. But it is doubtful whether this particular type of problem truly exhausts the full range of scientific perplexity. Various theoretical proposals are frequently suggested and seriously entertained by scientists with a view to overcome a variety of 'non-Popperian' obstructions, ranging from conceptual imprecision to mathematical inelegance. And whether or not we happen to regard them as difficulties worth tending to is beside the point. I suggest, therefore, to retain Popper's basic problem-oriented approach to science, but to treat the notion of a (scientific) problem far more generally.[7]

A basic scientific theory may be broadly described as a particular type of goal-directed system; a structured conceptual scheme or model designed, depending on one's philosophical persuasion, as a means to describe, interpret, explain, formalize or efficiently classify certain classes of phenomena. (The list of course, may be extended at will). Such a system will be regarded problematic exactly if there exists a discrepancy between what it is, in principle, capable of accomplishing and what it is desired, or expected to accomplish. Viewed thus, as disparities between the capacities and objectives of goal-directed systems, problems are rendered epistemically objective. A system may or may not be problematic regardless of anyone ever being aware of the fact. Subsequently, our accounts of problems may be true or mistaken, closer or

[7] Cf. M. Fisch, 'Towards a Rational Theory of Progress', *Synthese*, 99, 2 (May, 1994).

further from the truth. Broadly speaking, an apparently problematic system may be rendered less problematic in one of two ways: by either improving its performance in relation to its original goals – in which case the problem will be said to have been fully or partly *solved* – or by suitably modifyng its original goals – in which case the problem will be said to have been *dissolved* or *mitigated*. A scientific theory found incapable of accommodating the full range of phenomena to which it was originally meant to apply, for example, may be rendered unproblematic by an appropriate restriction of its original domain of application (as, for example, the restriction of Newtonian mechanics to relatively low velocities). But we would not consider this a solution of the problem of accounting for the kinds of phenomena excluded by such a move. A rational inquirer will, therefore, normally opt for the second option only as last resort. Scientists should not compromise their theoretical objectives unless they are convinced (a) that the problems that beset the theory designed to attain them are insurmountable as it stands, *and* (b) that there exist good reasons for salvaging as much of the problematic theory as possible.

Thus construed, science is rendered sufficiently multi-purposed to be regarded as problem-oriented in a manner acceptable to all. It progresses by persistent trouble-shooting, by contrived and ordered cycles of problem-seeking and problem-solving, geared to the continuous amelioration of its theories, models, mathematical apparatuses and experimental set-ups, in relation to whatever objectives they are intended to achieve. Creativity is required in the course of both the problem-seeking and problem-solving phases of any scientific inquiry. Open-mindedness, and a capacity imaginatively to perceive familiar situations in new and different ways are as important to the effective disclosure of problems as they are to solving them. And in either case rigidity, excessive pre-commitment, the motivated or subliminally entrenchment of hard-cores or paradigms are all major hindrances – all of which would seem to sit comfortably with Bohm and Peat's emphatic critique of the paradigm-infested state of current scientific thinking, debate and communication. But it does not. Interestingly, the class of scientific problems that they discuss is not easily accommodated even by the liberal erotetic model of scientific progress outlined above.

To view a basic scientific theory erotetically is to view it as both the product and subject of tenacious trouble-shooting. Basic scientific theories, certainly complex highly mathematicized ones, such as those discussed in *Science, Order & Creativity*, are seldom born of idle curiosity. A certain amount of playful tinkering is definetely involved, but for their larger part, they represent years of meticulous, convoluted, stubborn, and, more times than none, frustrating hard work in the face of pressing problems. A new theory is normally prompted into existence, tested, modified and assessed in relation to problems found to beset its competitors and predecessors. At this stage it will be presented and upheld as a comparatively better option than its rivals – namely as a better means to the achievement of the goals it is expected to achieve. Once established as a serious contender, the focus of critical scrutiny will gradually turn inwards by both friends and foes – the former, in order to further improve it, the latter, with a view to expose it flaws in order to promote and improve their own alternatives. Bohm and Peat are right in claiming that in 'Kuhnian' communities that are overly committed to a fundamental conceptual infrastructure, this type of rivalry will inevitably lead to fragmentation. A novel theory that challenges the accepted conceptual scheme will be deemed problematic merely for doing so, its other qualities notwithstanding. To cast their claim in erotetic terms, such communites assume *apriori* that adherence to the norm in this respect should be counted first and foremost among the desired goals of any new hypothesis. Failing to do so, novel conjectures will be rejected, or at best relegated to the role of a mere formal instruments thus blocking the possibility of ever creatively rethinking the science's tacit conceptual foundation. This is precisely the condemnable attitude Bohm and Peat dub 'playing false'. Within the type of 'Popperian' community described above, where, in principle, conservatism is not considered a virtue, nor hereticism problematic, there is no such danger. So that even if we grant Bohm and Peat their point that today much of science is in fact tacitly conducted along conservative Kuhnian lines, their additional tacit assumption that all current philosophy of science is similarly disposed is simply wrong. In *Science, Order & Creativity* Popper's theory of science is briefly acknowledged only to be criticized for laying excessive stress on falsifiability. Its effect on the infrastructure of science, claim Bohm and Peat, has been not to stimulate, but rather to further 'discourage the mind from free play with ideas' (59). The (contrasting) approaches of Kuhn and Popper, they imply, combine, in the infrastructure of science, to stifle all chance of theoretical innovation. Even when proposed within the conservative conceptual limits tolerated by the Kuhnians, Popper's influence on science has been to imply that 'without the possibility of some immediate 'crucial experiment', [a new] theory is looked on as being 'just metaphysics' and without any particular importance for science' (*Ibid*). Bohm and Peat are undoubtedly better acquainted with the current climate of opinion within physics than the present reviewer, so I shall not challenge the descriptive sections of their book. But the remedy they propose is a different story.

Insofar as a theory is seriously proposed with a view to advance science a step forward, it is born testable, at least with respect to its intended goals (which, as noted, may very different from those proposed by Popper himself). But rather than insist that theories, however outlandish, be 'played true' – that they be taken at face value and rigorously examined to determine whether they are indeed capable of fulfilling their anticipated promise – Bohm and Peat contend that, in the interest of scientific creativity we are better off *playing down* the differences between them and their rivals. Faced with two incompatible theories, the Kuhnian elects to reject the most outlandish one regardless of their relative success. The Popperian insists that neither of them be deemed problematic unless proven so in accord with their professed goals, which in any case should not include conformity to an *apriori* conceptual scheme. Bohm and Peat suggest, contrary to both schools of thought, that their evident disparity be thought of, in the first instance, as indicative of a potential deficiency on behalf of their shared conceptual foundation.

The Popperian, in short, advocates setting aside all metaphysical commitment (unless challenged directly by new findings) and to play true at the level of theory. Bohm and Peat, by contrast, demand that we set aside first-order problems and rivalries, and play true at the level of metaphysical foundations. The two strategies are obviously at cross purposes (because to play true on one level requires playng false on the other, and *vice versa*), and cannot be jointly endorsed *simpliciter*. On the other hand, neither party can easily ignore the concerns of the other. The Popperian certainly objects to *apriori* conceptual insulation of any kind, and although Bohm and Peat steer wide of first order scientific research, it is hard to imagine that they deny it. They do not address the problem explicitly. Here and there their text seems to imply that they are aware of it and that they seek to avoid it by advocating a kind of 'Doublethink'. The recurring phrase they employ is 'unity within diversity' which may be taken to denote an ability playfully to move from one level to the other while performing incredible acts of forgetting. This is extremely hard to conceive, and in any event is not developed further.

This is the interesting problem *Science, Order & Creativity* leaves us with. The need to meet the challenge of the conservatism inspired by the works of Kuhn, Lakatos and latter day relativist is an urgent one. Bohm and Peat, not sufficiently aware perhaps of the full range of their philosophical adversaries, rightly and forcefully diagnose it as a real threat to the very prospect to scientific creativity. Paradoxically, the philosophical solution they propose is plagued by the type of fragmentation their book so aptly laments, remaining by the end of the day unnaturally disconnected from the larger part of scientific life. And as if to prove their prognosis, the fragmentary nature of their own analysis owes to a lack of attention on their behalf to an important philosophical alternative – that of Karl Popper and his school. For them the problem requires attention but is considerably less acute. Popper himself aknowledges the importance of metaphysics for science, but largely ignores the possible feedback effect of science on metaphysics. Joseph Agassi tackles in head-on[8] within a Popperian framework. Metaphysical systems, he argues, can interact with theories both ways prompting mutual revision when necessary. When a theory is modified in the face of a problem, it may require us to rethink our deeper conceptual frameworks, and *vice versa*. When creative thinking is induced by a truly open and critical attitude, it may well go all the way down to fundamentals. To do so there is no need for the type of motivated conceptual laxity advocated by Bohm and Peat.

Despite its obvious shortcomings, *Science, Order & Creativity* is a valuable and timely book. The problems it raises intentionally and unintentionally are urgent ones. Its bold and detailed analyses and proposals for the reform of the language of science, the running of scientific institutions, and their implications for scientific research and teaching, are insightful and valuable. This is a book philosophers and scientists alike should take with utmost seriousness.

[8] J. Agassi, 'The Methodology of Research Projects: A Sketch', *Science and Society: Studies in the Sociology of Science* (footnote 6), pp. 273-82, esp. § 6.