BOOK REVIEWS

BOSTON STUDIES IN THE PHILOSOPHY OF SCIENCE, VOL. XI. PHILOSOPHICAL FOUNDATIONS OF SCIENCE. Proceedings of Section L, 1969, American Association for the Advancement of Science. Edited by R.J. Seeger and R.S. Cohen, Synthese Library Vol. 58, Dordrecht-Holland/Boston-U.S.A., D. Reidel Publishing Co., 1974, ix + 545 pp.

The twenty-eight papers included in this collection cover a wide range of topics, from Leonardo Da Vinci's paintings to a different style of "highly condensed object" – the so-called "white dwarfs" of astronomy. I imagine that few people will have the interests and the competence to take them easily through this book. It is therefore not clear to me that these proceedings are very usefully published together. There is no obvious connecting theme, except insofar as in every case some aspect of either science or a scientist (e.g. Leonardo) is dealt with. The title of the volume is misleading, for these are by no means all philosophical papers or all of special relevance to philosophy; moreover on the whole the philosophy that there is is unimpressive. In particular, many points are too unclear or too sketchily made to be very exciting. This is therefore not a book I am inclined to recommend highly either as a whole or in part to a philosophical audience. And in general it seems to me that these papers are too brief for both depth and clarity. However, particular people might find certain pieces useful. In what follows, therefore, I note the contents of the book section by section and give brief accounts of some somewhat arbitrarily chosen individual papers.

1. Perhaps by way of encouragement to the reader, the book begins with five papers which in effect celebrate the versatility of one man, Leonardo da Vinci, on the occasion of the four hundred and fiftieth anniversary of his death. This part forms a little 'book' of 115 pages and 55 plates of paintings, technical drawings, and doodles. The papers include "An Aspect of Leonardo's Painting" (John Shapley); "On the Physical Insights of Leonardo" (Raymond J. Seeger); "Leonardo as Military Engineer" (Bern Dibner); and "Leonardo da Vinci and the Beginnings of Factories with a Central Source of

Power" (Ladislao Reti). In the fifth paper, "Leonardo da Vinci and the Sublimatory Process", Raymond Stites critically considers Freud's judgment on Leonardo that "so universal in his many interests, (he) avoided all investigations in the realm of psychology". Stites argues that Leonardo had deep interests in and insights into psychology. We are referred to Stites's translation of the *Codex Trivulzianus* which includes forty-five "word lists with strongly affective or emotional overtones". Stites infers that Leonardo "observed and used what we today call chain-association, thereby inventing for himself a kind of self-psychoanalysis with art-therapeutic overtones."

2. In Part II, "Physics and the Explanation of Life", Eugene Wigner's paper expresses the views on the mind-body problem of an eminent physicist. Perhaps his most intriguing claim here concerns physics. While many philosophers now are busy arguing away the 'contents of consciousness' in favour of the 'physical', Wigner claims that "modern microscopic physics at present uses 'observations' or 'perceptions' as primitive concepts." Wigner has argued this claim elsewhere; his remarks here are tantalizingly brief. As he states it

> "the basic principles of physics, embodied in quantum mechanical theory, are dealing with connections between observations, that is contents of consciousness . . . the necessity of the formulation in terms of perceptions, and hence the reference to consciousness, is characteristic of quantum mechanics compared with classical physics." (p. 123)

Wigner is again concise on the ability of physics to explain 'contents of consciousness.' As I understand him, he believes that physics may well be adequate one day to explain situations involving life and consciousness: but this physics will be a transformed one. Just as the most recent extension of Newtonian physics – quantum mechanics – uses 'observations' as 'primitive concepts', so a further extension of physics may be hoped to give deeper insights into mental processes; laws *of a kind presently unknown* will be formulated for 'observations', which will no longer be the primitive terms of the theory.

In the other paper in this part, "New Concepts in the Evolution of Complexity", J. Bronowski counters certain arguments for vitalism, the belief that "the laws of physics which hold in the inanimate world will not suffice for the explanation of life." A number of Bronowski's claims are of the form "we do not at present know enough about . . . to say whether it can be explained by – ".

The 'new concepts' alluded to in the title are "stratified stability" and "unbounded plan". Evolution is an "open and unbounded plan" in which the errors which destroy the individual are also the origin of species. Polanyi has argued that the increasing complexity of species in the course of evolution supports vitalism. Bronowski attempts to explain this increase in terms of two empirical principles, (1) natural selection in favour of genetic variability, (2) stratified stability: "the building up of stable configurations does have a direction, the more complex stratum built on the next lower." Against the vitalist, Bronowski argues that chance is 'constrained to work' precisely by moving from simple to complex structures. Finally, he considers how it can be possible for complex arrangements to establish themselves at all, given the Second Law of Thermodynamics.

3. Part III consists of one essay in the history of science: Martin Klein's "Boltzmann, Monocycles, and Mechanical Explanation", the 1969 George Sarton Memorial Lecture. Professor Klein counters Whitehead's description of the last quarter of the nineteenth century as "an age of successful scientific orthodoxy, undisturbed by much thought beyond the conventions". He notes that the "tradition of mechanical explanation" was coming to an end in those years; a number of debates among scientists reflected difficulties in this tradition. "Questions of fundamental principle drew an unusual amount of attention in this stodgy era". Ludwig Boltzmann figures as an ardent spokesman for the dying tradition.

4. Part IV is devoted to "Current Problems of Cosmology". The 'laws' of physics, already given a temporary and limited look (as they now stand) in Wigner's paper, are 'attacked' from another point of view in Peter Bergmann's "Cosmology as a science". Bergmann argues that whereas studies focussing on the nature and history of the universe may extrapolate from "laws of nature that have been obtained on the terrestrial scale", they may also serve to cast doubt on the finality of those 'laws'. A series of brief glimpses into contemporary cosmology are given in these papers, which will be opaque in parts to those knowing little physics.

The other papers are "Open or Closed?" (Philip Morrison); "Cosmic Evolution" (David Layser); "Highly Condensed Objects" (Fred Hoyle); "The Case for a Hierarchical Cosmology . . ." (G. de Vaucouleurs); and "From Mendeleev's Atom to Collapsing Star" (J.A. Wheeler).

5. Part V is entitled "Objectivity and Anthropology", and the five papers are roughly on this one theme, though they do not refer to one another. In the first paragraph of her "Objectivity in the Social Sciences" Judith Buber Agassi notes and deplores a trend current in American sociology, rejection of "the basic condition for objective social science", that is, a rejection of "the assumption of the unity of mankind – both intellectual and moral". According to the view she opposes "nothing can take the place of first-hand experience: only women can understand women's problems" (and so on). Now, Agassi spends no time investigating what may lie behind slogans of the type cited. Suppose they express the view that if one wants to know what it is like to be a person of a certain sort one must either be a person of that sort or at least be dependent upon such people for one's knowledge, for instance one must ask them about their experiences. If this were the point of such slogans in their context, then they would not seem necessarily to conflict with the idea of the "intellectual and moral" unity of mankind. For given that there is 'privileged access' of a sort to some experiences and that a scientist may have to ask the 'privileged' about their lives and learn from them, a single set of moral and intellectual standards could still be *applicable to* these experiences. Agassi does not sufficiently clarify the nature of the view she is attacking or the view she is defending, or explain the relationship between them. Nor does she mount a sustained attack, but in the bulk of her paper presents a brief historical sketch of attempts in American sociology to "solve the problem of value", citing once again at the end the current trends she deplores. This paper is sensible and serious in tone but, as I have indicated, lacks depth or precision in argument.

I.C. Jarvie (in "On the Objectivity of Anthropology") states plainly that objectivity "consists in placing checks on bias" and claims that the best way to do this is to live up to Popperian canons of scientific method: to subject our ideas to "the standard of intersubjective and repeated testing". Jarvie asks whether it is true in anthropology, as in natural science, that "to all intents and purposes" the same descriptive material could have been collected by anyone. According to Jarvie, most anthropologists would say no. This at first struck me as an unlikely claim about most anthropologists, but I note that Jarvie's question seems likely to attract opposite answers, depending on which of two possible construals is made. Thus consider: (1) Was the same material (e.g. social structure, laws) there to be discovered by whoever was adept or curious or cour-

ageous or charming enough to discover it? (2) Is it the case that anyone at all could have had enough wit, courage, etc. to discover the social structure, and so on? I would myself expect most anthropologists to say 'yes' to (1) but 'no' to (2). That Jarvie in fact has construal (2) in mind is perhaps evidenced by his later claim that *machines* could not do social science, because they cannot enter into social relationships like talking. A negative answer to (2) would still indicate a "profound personal equation" in anthropology, if not so profound as a negative answer to (1). (In contrast, physicists hardly need to ingratiate themselves with the particles they are investigating.) Jarvie would seem to be right in averring that the "personal equation" of anthropology as expressed in a negative answer to (2) at least does not preclude its objectivity in his sense.

Towards the end of his paper Jarvie juxtaposes two claims rather paradoxically. First, he indicates that objectivity is attained once the best that can be done by way of testing claims has been done. This seems fair enough, given his account of objectivity. But, second, he notes with equanimity that little is in fact ever done to test the accuracy of anthropological field reports. He does *not* say that such tests are, either in principle or in practice, impossible, but justifies the lack of checks in a very peculiar way. Checks, Jarvie claims, would harm a fieldworker's dignity, and moreover (and I quote) "if he lies or misperceives, so what?" One would have thought that if the importance of anthropological truth has to be impugned, there will not be much dignity left in any anthropological field work. The importance of anthropological truth aside, are objectivity and lack of testing really opposed, as Jarvie's drift seems to have it? Perhaps at least checking by *another* anthropologist is not necessary to free results from bias, perhaps even the replication of an observation of one's own is not essential to objectivity. Then we could conceivably have truth, objectivity, and the 'dignity' of not being checked on all at once. These suggestions are implicit in Anthony Leeds' paper " 'Subjective' and 'Objective' in Social Anthropological Epistemology".

Leeds argues that, according to one conception, the subjective-objective dichotomy is that between unique and replicable sensations, but that it is better to see all sensation and experience as 'objective', in that "something is observed by someone". Anthropology, at any rate, accepts dreams and visions as real data. The locus of 'subjectivity' – as something problematic – is not the irreplicable sensation but "the act of giving meaning, especially evaluative meaning, to experience and observation". It turns out that

this is not such a problem in anthropology, Leeds claims: anthropologists, immersing themselves in exotic cultures, tend to avoid ethnocentrism and "axiological infusions" in their results. It looks as if Leeds disagrees with Judith Agassi on the 'moral unity of mankind' and disagrees with Jarvie as to the importance to objectivity of some measure of replicability of the data used. But it is hard to say since none of these authors make their central ideas and theses sufficiently clear and precise. The other papers in this rather disappointing section are Jacob W. Gruber, "Acquired Models and the Modification of Anthropological Evidence" and Paul W. Collins, "The Present Status of Anthropology as an Explanatory Science".

6. Part VI, "Comparative History and Sociology of Science" includes "Scientific Concepts and Social Structure in Ancient Greece" (K.H. Niebyl); "Algebre et Linguistique: l'Analyse Combinatoire dans la Science Arabe" (Roshdi Rashed); "On pursuing the unattainable" (J. Agassi); and "Sciences and Civilizations, 'East' and 'West': Joseph Needham and Max Weber" by Benjamin Nelson. There is also a paper by Stephan Toulmin with a comment by Ernan McMullin.

In "Scientific Strategies and Historical Change" Toulmin argues that the history and sociology of science are inseparable from the philosophy of science. At the same time he characterizes philosophy of science as being concerned with "scientific rationality". He links the two theses as follows. The striking occasions for the exercise of scientific rationality are "cloudy" situations in which new concepts or approaches are established. Two things are required in order that new concepts be established: a person with some "authority" or a "reference-group" – and a "judgment" made by that person or group as to what new concept or approach should be adopted. Such judgments, though not logically derivable from "standards available" at the time, are none the less rational. An account of scientific rationality must give an account of the emergence of 'authorities' and their 'judgments'. But then it must be concerned with history, sociology, and psychology, and not be 'pure' philosophy.

McMullin notes that Toulmin has argued for such views at much greater length in his book *Human Understanding*. In the article under review, at least, one important issue is glossed over: whether or not the influential judgments of 'reference groups' are ever criticizable. Is the 'winning" judgement or decision in a 'cloudy' situation *always* a rational one? If a 'wining' decision is *by defini-*

tion rational, then apparently a rational decision need not be a *justified* one. (Thus McMullin, in his thoughtful commentary, says that Toulmin runs the risk of "separating rationality and justification".) If, on the other hand, influential judgments *are* criticizable, then one might expect a philosophical investigation of 'scientific rationality' to be concerned with canons of criticism rather than with psychological, sociological, or historical explanations of authority and the actual judgments particular scientific authorities have made.

7. The book concludes with Part VII, "Unity of Science", containing two papers. Kenneth F. Schaffner writes on "The Unity of Science and Theory Construction in Molecular Biology" and Lawrence Sklar on "The Evolution of the Problem of the Unity of Science". Sklar's paper is as much philosophical as historical. It is also brief and programmatic. He takes it that the current idea of "The Unity of Science" is the idea of "the ultimate reduction of all theory to some most basic theory." Some claims about reduction raise difficult questions. For instance if one claims that xs are reducible to ys insofar as they can be identified with ys, the issue arises "what is an identification?" Such questions are "straightforwardly metaphysical". Sklar suggests that questions of this kind will arise in the context of all "models of reduction". Thus if the "Unity of Science" thesis in philosophy gained impetus from an anti-metaphysical logical positivism, it now finds itself enmeshed in metaphysics. Here then are some philosophy of science questions which –*pace* Toulmin perhaps – seem to be purely philosophical.

As I said at the beginning, I cannot recommend this book as a whole very highly. I hope, however, that I have given a fairly clear idea of the nature of its contents. Incidentally, the book contains rather a large number of misprints.

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WITTGENSTEIN'S LECTURES ON THE FOUNDATIONS OF MATHEMATICS, CAMBRIDGE 1939, edited by Cora Diamond, Ithaca: Cornell University Press, 1976

I have grave doubts as to whether this book – if one can call it that – was worth publishing. It is yet another collection of lecture notes taken by students who were present at Wittgenstein's lectures, this time on the foundations of mathematics, at Cambridge in 1939. Cora Diamond has assembled a text based on notes taken by R.G. Bosanquet, Norman Malcolm, Rush Rhees, and Yorick Smythies. Malcolm wrote of these lectures: "They were given without notes or preparation. Each lecture was new philosophical work. Wittgenstein's ideas did not come easily. He carried on a visible struggle with his thoughts."

In her preface, Prof. Diamond acknowledges the difficulties of such an undertaking. For example, we are told that Bosanquet's notes were the fullest but were edited and rearranged "for his own purposes" (p. 7). Rhees' and Malcolm's notes had a "minimal degree" of editing and interpreting, but Malcolm's were often the briefest (p. 8). Smythies' notes followed the form of the original lectures but were "sometimes barely legible" (p. 8). Further, none of the four versions covered all of the 31 lectures. And in many cases, there are discrepancies which are "more or less considerable" (p. 8). Prof. Diamond states that her aim was to produce, from these four sets of notes, a version which is both as "readable and accurate" as possible (p. 8). "No single version was taken as *the* basic text. Rather, each passage is based on a comparison of all of the available versions of that passage. Where two or more versions agreed in some point, I normally took them to be correct in that respect." But: "As a consequence, there are sentences to which nothing in the four versions corresponds" (!) (p. 8). And she confesses: "The accuracy of the text varies and depends to a certain extent on the accuracy of my ear." Also: "Many passages could have been handled differently" (p. 8). As a result of all this, we are told: "A great deal of caution must . . . be used before anything in the text here can be

taken as 'giving Wittgenstein's views' or even as giving good evidence for some interpretation of what he says elsewhere" (p. 9).

When difficulties were encountered, Prof. Diamond occasionally calls attention to them via brief footnotes. Let me quote a few of them which illustrate some of the problems.

> The versions of this passage in B [Bosanquet] and S [Smythies] are quite different. The text here is based on both; it could have been done very differently. (p. 20)
>
> B's version of the second way Lewy [a member of the class] goes wrong is entirely different. The text here is based on what is given only very sketchily in S. (p. 20)
>
> This sentence is doubtful. The sentences in B and S from which it is constructed make slightly different points. (p. 21)
>
> This paragraph, based on B and S, is quite doubtful. (p. 28)
>
> This sentence is a combination of two quite different ones from M [Malcolm] and S. It is very much a guess. (p. 31)

It would be tedious to cite any more. The point should be obvious. What about the lectures themselves? They are extremely dull and tiresomely repetitious. And most of the comments on the issues with which they are concerned are either trivial or are presented in a confusing and sketchy manner. Most are dealt with in a far better (if not entirely adequate) form in the *Remarks on the Foundations of Mathematics* (and to some extent, other works). Among them are: the application of mathematics, calculation, counting, criteria, definition, experiment and mathematics, "foundations" of mathematics, grammar, inference, logic and logical laws, meaning and use, measurement, necessity, numbers, proofs, experiental and mathematical propositions, rules, tautologies, and mathematics as a technique. Among the mathematicians and philosophers who are criticized, we find (as expected): Hardy, Hilbert, Frege, and Russell – the last two at great length. Since Wittgenstein's views on these issues and figures are well-known from the *Remarks* and other works, I shall not take time to re-hash them here.

The approach which Wittgenstein takes in the lectures is also one which is familiar to readers of his later works. He begins by asking the question: How can he (or anyone who is not a mathematician) talk about the foundations of mathematics? He says that he does not wish to "interfere" with the mathematicians and that all he is going to do is talk about a new interpretation of mathe-

matical symbols. He states that as a philosopher he can talk about mathematics because he will only deal with misunderstandings and "puzzles which arise from the words of our ordinary language, such as 'proof', 'number', 'series', 'order', etc." (p. 14). What sort of puzzles and misunderstandings? Those which "arise from a tendency to assimilate to each other expressions which have different functions in the language" (p. 15). He claims that he will try to draw attention to a certain investigation – one which might be called an investigation into the meanings of certain words. Or better yet: "The investigation is to draw your attention to facts you know quite as well as I, but which you have forgotten, or at least which are not immediately in your field of vision. They will all be quite trivial facts. I won't say anything which anyone will dispute. Or if anyone does dispute it, I will let that point drop and pass on to something else" (p. 22). They are, indeed, quite trivial. And (again) since most of what he says is found in the *Remarks* and other works, I shall not waste space by elaborating on them. But they concern the topics which I enumerated above.

Wittgenstein seldom discusses any difficult or complex aspects of mathematics (or logic) nor does he say much concerning the foundations of mathematics. Hence, the title of the lectures is misleading. In response to the view that he was going to lecture on a "branch of mathematics" known as "the foundation of mathematics" he says: "I am not going to lecture on this, I know nothing about it" (p. 14). In the last half of the text, however, he does discuss Russell's view that logic is the foundation of mathematics and argues against it. The foundation of mathematics does not lie in the "cold and austere" logic of Russell but in certain "empirical facts" which involve us in a language game (p. 280). Both mathematics and logic are "part of the *apparatus* of language, not part of the application of language" (p. 250). And both mathematics and logic serve merely to make transitions from one "material proposition" to another (p. 267). Logical and mathematical propositions facilitate calculation or inference (p. 282).

There is one moderately interesting feature of the book. The editor has included comments, questions, and answers to questions which were made by members of the class which include: Lewy, Prince, Watson, Wisdom, Gasking, Malcolm, Rhees, and, above all, Turing, who heroically attempts to raise objections, provide answers, etc., – only to be (often) put down by Wittgenstein, usually in a cursory manner: Here are a few examples:

Wittgenstein: Suppose that when we counted the squares we always got different results. Would the figure still be called a proof?
Turing: It would be a bad proof.
Wittgenstein: A bad proof of *what*? It would not prove what result we should get if we counted the squares on a certain occasion. In fact, the figure would be useless for physics. What would it prove? Nothing. (pp. 37–38)

Wittgenstein: Suppose we place a yardstick against a metre rod, glue the two together and then cut off the metre rod where the yardstick ends . . . Can we say which is being measured by which?
Turing: Probably the yardstick is being measured by the metre rod, as the metre rod is cut off where the yardstick ends.
Wittgenstein: Oh, come now – then we will cut it off somewhere in the middle. Now you cannot tell which is measured by which. (pp. 117–118)

Wittgenstein: If a contradiction may lead you into trouble, so may anything. It is no more likely to do so than anything else.
Turing: You seem to be saying that if one uses a little common sense, one will not get into trouble.
Wittgenstein: No, that is NOT what I mean at all. (p. 219)

One final remark. The volume contains an excellent and thorough index. Alas, this reader – and doubtless many others – will seldom have occasion to use it.

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WITTGENSTEIN'S DOCTRINE OF THE TYRANNY OF LANGUAGE: AN HISTORICAL AND CRITICAL EXAMINATION OF HIS BLUE BOOK, by S. Morris Engel, The Hague: Martinus Nijhoff, 1975.

Wittgenstein's later philosophy has often been held to be completely original, without historical antecedents, or even a repudiation of past philosophy. Among some expositors it has been seen as one strand in the development of twentieth century British philosophy. In his study, Engel challenges these common views. Of course, several commentators (Anscombe, Stenius, Toulmin, and others) have already questioned or opposed these prevailing opinions. Engel acknowledges their efforts but seeks to go further in tracing the historical roots of Wittgenstein's work. The significance of this aspect of Engel's book is emphasized by Stephen Toulmin in his introduction to the volume. However, it would be misleading to say that Engel's book is simply an historical study. It is rather an effort to illuminate various aspects of Wittgenstein's thought and its relation to certain movements within contemporary philosophy as well as an effort to trace historical influences. The book, in Engel's words, "grows out of an attempt to understand how some philosophers within the linguistic movement have come to say the very strange things now so familiar to us. Having for a long time been puzzled and intrigued by them, I finally determined to discover what some of their origins were and what about them was so puzzling and intriguing to me. Although I soon discovered these origins in Wittgenstein's *Blue Book*, the tensions and contradictions which I found in this work, as well as Wittgenstein's differences with his disciples, proved to be themselves very puzzling and I set about to trace their roots as well" (p. xi).

Relying upon Warnock's exposition (in *English Philosophy Since 1900*), Engel briefly describes the impact which G.E. Moore had upon contemporary philosophers. He states that Moore must be credited for being the first to ask why philosophers say such strange things as "Time is unreal," etc. He claims that Moore did not answer this question but that Wittgenstein did, and he states that he wishes to trace the effect which Wittgenstein's answer had upon those who

came under his influence. Among the latter, he distinguishes two groups – those who hold that the nonsensical utterances of many philosophers are a function of language per se (the linguistic wing) and those who maintain that these strange assertions are manifestatons of deep psychological needs (the clinical wing). According to Engel, although both of these "wings" stem from Wittgenstein and both are connected with Wittgenstein's central thesis regarding the way we are often led into confusion "by means of language," nevertheless, both have misrepresented and misunderstood Wittgenstein. Hence, he proposes to examine Wittgenstein's theory regarding language and maintains that, since the source of its "stresses and strains" is in the *Blue Book*, his first task will be to examine that work.

In a chapter entitled "The Dilemma of the 'Blue Book'," Engel presents an exposition of some of the main themes in the book. Among them is Wittgenstein's view that philosophical questions – unlike empirical ones – are not about an "external" reality but rather are "either about the conceptual schema (or language) we use in organizing that external reality or are by-products of the friction brought about by conflicting schemata – that they are, in either case, only about language" (p. 30). Engel presents some brief criticisms of Wittgenstein's views but holds that these are all "overshadowed" by a certain major internal difficulty or contradiction in the *Blue Book*. And what, precisely, is the dilemma of the *Blue Book*? Apparently this. There are (according to Engel) two "strands" in Wittgenstein's thought. According to the first, the confusions which are found in the writings of philosophers have their origin in language. The philosopher, in this view, is one who, having been misled by language, propounds in perplexing ways linguistic propositions as if they were empirical or scientific ones. He is led to make this confusion and to utter strange things by certain misleading words and expressions. "In order to dispel this confusion and relieve the puzzlement, what we must do is to destroy 'the outward similarity' between the two propositions that are wrongly assimilated. This can be done by seeing 'how the words *are actually used in our language*' and thus how and where they mislead" (pp. 41–42). But although this seems to be the dominant strand in the *Blue Book*, Engel finds in it another strand which is in direct conflict with the first. "What this new strand seems to assert is that philosophical puzzlement, far from being the effect of linguistic confusion is, on the contrary, itself the very cause of it!" How so? "Certain deep

dissatisfactions with language . . . lead the philosopher to revise it in ways more congenial to him" (p. 42). According to Engel, Wittgenstein is not clear as to just what these dissatisfactions are. But what is clear is that "language alone is now no longer the sole villain. Something else, something obviously deeper and more intractable is apparently responsible" (p. 42). But if Wittgenstein is unclear as to what these deep dissatisfactions are, Engel seems equally unclear. His treatment of this issue is one of the most disappointing features of the book. Since he attaches such importance to this second strand, one expects a much more searching analysis or probing.

Engel then turns to the question of the historical parallels and sources of Wittgenstein's work – sources which have led to the above-mentioned stresses and strains of Wittgenstein's views. Engel finds these to lie chiefly in the works of Kant and Schopenhauer, and he devotes a chapter to each. Engel's main thesis here is: Wittgenstein failed to resolve the tensions and contradictions in his thought because they stem from and have their antecedents in the historical sources from which he "gained his inspiration." The presence of these influences upon him drew him in conflicting directions, leading him to believe that, on the one hand, philosophical puzzlement is a product of confusion (Schopenhauer), yet on the other hand that its source lies deep in our nature (Kant); that what lies beyond the area amenable to skill is nonsense (Schopenhauer), yet that it is an important kind of nonsense (Kant), and so on (p. 97). Engel chides Wittgenstein's followers for having failed to see that "the Wittgensteinian system" was a product of these "two diverse literary traditions between which it tried to maintain a delicate balance" (p. 98). The chief defects of Engel's work seem to me to be in these two chapters devoted to Kant and Schopenhauer. His case for Wittgenstein's indebtedness to Kant is rather flimsy. He spends over ten pages in pointing out that *other* writers have not shown it. But he does not adequately show it himself. He puts quotes from Kant and Wittgenstein side-by-side and (for the most part) lets the reader see the point. Often it is difficult to see. Similarly, it seems to me that Engel stretches his case in his comparison of Wittgenstein and Schopenhauer. The citing of somewhat similar quotations is not enough. More elucidation of them is needed. Nevertheless, in both cases some interesting hints are given which may provide spurs for further research.

In the last part of his study, Engel turns to a certain theme which appears in some of Wittgenstein's works which were written

shortly after the *Blue Book* – a theme which "promises a way out of the dilemma posed by that book" (p. 102). According to Engel, "the view which seems to emerge from these writings is that the mind is held captive and in bondage not by language, pure and simple, but rather by 'pictures' which language has a tendency to generate" (p. 103). Many words tend to arouse pictures of what they represent. In most cases, this is helpful since it guarantees that we will all use the words in the same way. But in other cases (e.g., 'particle') the pictures are misleading. When such words are put to new uses instead of being used in their ordinary ways, the opposite effect occurs. These words are misleading because the pictures which they arouse in us lead us to expect the wrong things. Since an image is misapplied, the end result is puzzlement. Confusions arise when we use familiar words in unfamiliar ways. The pictures aroused by them are misleading, and from these "most of the problems of philosophy arise" (p. 109). Engel illustrates this theme by citing numerous passages from unpublished works as well as from the *Investigations, Foundations,* and *Brown Book.* And how does Wittgenstein's later theory of pictures (not to be confused with the picture theory of the *Tractatus*) provide a way out of the dilemma of the *Blue Book*? In Engel's view, Wittgenstein's earlier theory (in the *Blue Book*) does not adequately explain why language tends to have such a "dominating and perverse effect" upon us. It does not provide answers as to how "language manages to exercize such a tyrannically bewitching power over our minds and what about it enables it to continue to deceive us even when its deceptions are brought to light" (p. 120). The later theory is said to make up this lack. Unfortunately, Engel does not show just how this is achieved but is content to make brief remarks such as: "This tendency of language to generate pictures is not only something universal about language but may very well be conceived as precisely the sort of fault in it that his theory here requires" (p. 102).

In his final chapter, Engel attempts to show that Wittgenstein may be regarded as a type of existentialist and that his "existentialism" and the direction to which it points lie, much more than what is commonly referred to as existentialism, in the direct line of development of western philosophy. Engel tries to establish this claim by such means as the following. He remarks that the concept of the Absurd is common to existentialists. And he cites some of the later passages of the *Tractatus* as evidence of Wittgenstein's having held this concept. But he seems to interpret that

claim as being equivalent to the thesis that Wittgenstein held that the world we experience depends to a great extent on the conceptual system which we use in organizing it for ourselves and that in this enterprise "we are under a kind of human and universal deception" (p. 125). Just why these two claims are held to be equivalent is not made clear. Nor is the transition from such matters to Engel's conclusion: "In stressing this aspect of our human condition (an aspect which in its modern version is the all-embracing theme of Existentialism) Wittgenstein shows himself to be part of this great philosophical or metaphysical tradition" (p. 125). This is surely a provocative thesis, but I find Engel's support for it to be unconvincing.

In spite of the difficulties which I have mentioned, Engel's book deserves serious study. I believe that he has not successfully achieved his main task. But he has at least made a start toward that end. In the words of Toulmin, "Professor Engel claims only to be opening up some of the questions which we shall all be having to pursue in the years ahead" (p. xii).

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PERCEPTION, EMOTION AND ACTION, by Irving Thalberg, Oxford: Basil Blackwell, and New Haven: Yale University Press, 1977, 142 pp.

The five essays in this book are united by a common methodological theme – what Thalberg calls a 'component approach', Two main advantages claimed for it are that it is compatible with a variety of metaphysical positions, and that it provides a way of breaking various long-standing philosophical deadlocks, twelve of which are discussed in the volume.

What is meant by a component approach (or 'analysis') can best be illustrated by example, since Thalberg eschews any abstract characterization of his methodology. Consider the event of seeing an X. Such an event, Thalberg argues, has various ingredients – e.g., the X; light waves traveling from the X to the perceiver's eye; neural activity; beliefs acquired by the perceiver; and perhaps even sense-data, depending upon one's philosophical proclivities. The event in question *consists of* such components, and it would be an error either to identify the event with a sub-group of its components (e.g., neural impulses) or to speak of a causal connection between the event and any of its components – even though causal connections may well exist between the components themselves. This result is counterintuitive, however, since it does seem clearly correct to say that the light's traveling from X to my eye is a cause of my seeing X. And there is a similar consequence for a component analysis of emotion: we will be debarred from saying, e.g., that a traveler's belief that he has mislaid his passport causes him to be anxious about that matter (33-4). (I shall reserve until later an application of the point to action-theory.)

Thalberg provides no necessary and sufficient conditions for componency, claiming that (a) we can generally identify components easily enough by appeal to common sense, scientific investigation, and the meanings of key terms; and (b) his program will be vindicated if it offers a significant alternative to other philosophical outlooks. But if 'being a component of X' is incompatible with 'being a cause of X' (as Thalberg usually maintains, but see p. 29 for a contrary insinuation), our intuitions about causes may clash

with or prevent any clear intuitions about components. Thus, in the absence of something like 'the shopworn apparatus of logically necessary and sufficient conditions' (115), the significance of Thalberg's alternative to adversative views is difficult to assess.

The need for a criterion of componency also emerges from Thalberg's component approach to action. Among other things, he wishes to show that his view can accommodate a causal account of intentional action, and this requires him to handle counter-examples based on 'deviant' causal chains. Consider Donald Davidson's story of the mountain climber who holds a rope from which his colleague is dangling helplessly. The climber is tormented by a desire to let go of the rope, all the while aware of the fate that his partner will suffer if he does so. These thoughts, however, so unnerve him that he lets go of the rope after all. Intuitively his letting go does not seem to be an intentional action, but this creates a problem for causal theorists who analyze intentional action as bodily movement produced by intentions and desires.

The main elements in Davidson's story, Thalberg suggests, are these causally connected items (61):

(i) the alpinist's longing to perform the action of relaxing his grip;

(ii) his belief that if he does so, the rope and his partner will fall;

(iii) his paroxysm of guilt in response to his desire and belief;

(iv) the uncontrollable weakening of his fingers.

Although Thalberg has several things to say about this case, I shall focus on just one of them: the question of which items on the list are components of the climber's action (i.e., loosening his grip). Thalberg argues that (iv) is not a component of that action, since 'The bodily movement reported by Davidson seems too much like twitching, shuddering, tremors and other movements which occur whether or not one wants them' (61). Hence, if some version of a causal theory of intentional action is to survive, (iv) must be replaced by:

(iv') the relaxing of the fingers.

This is said to differ from such things as shudders and twitches in that it is 'supposed to be a movement within the person's repertory, or at least not one that may elude his control' (61). And now at least we see the climber doing the very thing he wanted to do,

so Davidson's story no longer obviously describes a non-intentional action. Despite Thalberg's claim to the contrary, however, the component account now appears circular. For what is a 'movement within a person's repertory' or 'one not eluding his control' except a controlled movement or action? Clearly, however, it is not enlightening to say that one mark of an action is a bodily movement which is an action. Thus, Thalberg's causal account is left with the problem of specifying what *sort* of bodily movements are components of action.

In Thalberg's view actions (or more precisely, basic actions) are never anything over and above their causally connected constituents. (*Non*-basic actions for him may consist of components, some of which are not causally connected, but that is irrelevant for the present.) Intentions and desires (perhaps identical with some neural processes), along with neural feedback mechanisms, bring about bodily movement; together these items constitute an action. But such a deterministic scheme, it has often been argued, threatens freedom of action. Thalberg's initial response to the determinist is as follows:

> If what we do is to result – inevitably – from electro-chemical processes within our nervous system, then it must be a separate occurrence. The movement of the rest of our body is easy to distinguish from events in our cerebral cortex and its peripheral extensions. But if we introduce a constituent analysis, our action will encompass both a bodily movement and neural processes. The whole action cannot therefore result from one of its ingredient events. Consequently, we will be unable to raise the specter of our own brain processes making our actions unfree. (74).

This reply, however, shows at most that our actions cannot causally or inevitably result from those neural processes *which are their constituents.* Now, let A be an action (e.g., moving my finger) whose constituents are $c_1 \ldots c_n$. Presumably these constituents are part of a longer causal chain whose earlier members are not constituents of A. Since these earlier members are causally responsible for $c_1 \ldots c_n$, and hence for A, it looks as if there is no room for free (uncaused) action. So far, then, Thalberg's contention 'that no problem arises if we regard some concurrent brain processes as neural components of what a person does' (71) has not been vindicated. The only other argument he offers on the subject is this:

> We simply disavow any claim to have analysed basic action – or any other kind of action – in a way that yields the 'essence' of agency . . . In a sense, the agent himself does not figure in our narrative of what goes on when he acts. So he is neither active nor passive in the proceedings. (128)

Notice, however, that here the problem of free *action* has given way to a problem about the freedom of *agents*. And while I agree that an account of action need not yield an analysis of agency, this concession will not circumvent deterministic arguments against free action.

Another goal of Thalberg's component approach is to put to rest a dispute in action-theory between Reductive Unifiers, who identify basic actions with their attendant results, and Pluralists, who see basic actions as numerically distinct from their generated consequences. Accordingly, he accepts the following propositions (cf. p. 120):

(1) Basic actions are actions;
(2) Non-basic actions are actions;
(3) They are not the same action;
(4) They are not different actions.

Although propositions (3) and (4) seem mutually exclusive, Thalberg's strategy is to assert that (4) is nothing more than a denial of the Pluralists's outlook. There is still room to maneuver, he submits, between that denial and an acceptance of the Reductive Unifier's position. The main idea is that basic actions are components of non-basic actions; and when X is a component of Y, the disjunction 'Either X is identical with Y, or else X and Y are numerically distinct' fails to apply. This is an interesting proposal, one which does suggest a useful alternative to the views represented in the aforementioned dispute. Nevertheless, it must face two difficulties. First, the examples of componency which Thalberg provides do not yield an account of the relation 'is a component of' clear enough to warrant an authoritative evaluation of the proposal at hand; and the revelation that there can be components of action which are not causally connected with one another (89-92) makes several of his analogies rather dubious. Second, an unwelcome consequence of the component approach here is that it forces us to forego many of our usual causal explanations of action. For Thalberg, (1) a desire to smoke a cigarette, may cause (2) a variety of bodily movements which result in (3) a

lighted cigarette's being in my mouth; but since (1)–(3) are components of the (non-basic) action (smoking a cigarette), it follows that my desire to smoke cannot be a cause of my smoking. (The same point applies to explanations of basic actions.) This consequence can be avoided by Pluralists like Alvin Goldman – Thalberg's primary Pluralist adversary – but unfortunately, Thalberg argues much more effectively against Reductive Unifiers than de does against Pluralists.

In spite of the reservations recorded above, I do think that the component approach deserves elaboration and refinement (perhaps unavoidably along the traditional lines of necessary and sufficient conditions). Moreover, many of Thalberg's arguments against other philosophical views are persuasive, and his style is lively and free from unnecessary technical detail. Taken in conjunction with some traditional literature on these topics, *Perception, Emotion and Action* would make a useful text in an advanced undergraduate or graduate course. The book contains numerous typographical errors but none of them affects its intelligibility.*

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* This review was written while I was on sabbatical leave from The University of Michigan–Flint, during which time I held a Charles Phelps Taft Postdoctoral Fellowship at The University of Cincinnati. I am grateful to both institutions for their support.