Philosophia Vol. 6 Nos. 3-4 Pp. 463-494 September-December 1976

CRITICAL STUDY

THE PHILOSOPHY OF KARL POPPER
PART I: BIOLOGY & EVOLUTIONARY EPISTEMOLOGY

W.W. BARTLEY, III

THE PHILOSOPHY OF KARL POPPER edited by Paul A. Schilpp, Two volumes, Open Court, Library of Living Philosophers, La Salle, 1974, 1323 pp., $30.00.

I

The philosophical perspective celebrated in the latest member of the distinguished Schilpp series is the most radical yet presented in The Library of Living Philosophers. Radical for this simple reason: Sir Karl Popper is not really a participant in the contemporary professional philosophical dialogue; quite the contrary, he has ruined that dialogue. If he is on the right track, then the majority of professional philosophers the world over have wasted or are wasting their intellectual careers. The gulf between Popper's way of doing philosophy and that of the bulk of contemporary professional philosophers is as great as that between astronomy and astrology.*

I believe that Karl Popper is on the right track.

I am a bit reluctant to admit this. For although I spent the first ten years of my professional life in close collaboration with Popper and his ideas, I spent the second decade of my career, only recently elapsed, trying to avoid both. From 1955 to 1958 I studied Popper's work intensively at Harvard despite the warnings of my teachers there about this "difficult man". In 1958 I went to London as a kind of pilgrim to become first Popper's student and then for four

*I express my thanks to the following persons, who have been kind enough to read and comment on all or part of this first installment, and who have sometimes protested my views: Joseph Agassi, Donald T. Campbell, Marjorie Grene, Adolf Grünbaum, Jagdish Hattiangadi, Michael Haynes, I.C. Jarvie, Asa Kasher, Stephen Kresge, A.E. Musgrave, John Post, Jeremy Shearmur, Avrum Stroll, Gerhard Wassermann, J.W.N. Watkins, and J.O. Wisdom. I also acknowledge the help of grants from the Research Foundation of the California State University, Hayward.

years his colleague at the London School of Economics. In 1965 we had a row over my theory of rationality and his theory of demarcation; we have not spoken since. During these past ten years, although I have tried in a way to avoid Popper's *new* work, I have – in connexion with a study of central European thought between the first and second world wars – been investigating the origins of Popper's thought and the Viennese milieu in which he came to maturity.[1] The initial results of my study suggested to me that Popper was less original than I had at first imagined; and this "finding" helped free me from his influence.

I accepted the assignment of reviewing the Schilpp volume in the expectation that it would give me an enjoyable opportunity to criticize Popper roundly. In the course of reviewing this volume, however, I have had to revise some notions that I have entertained in recent years. Although there will be plenty of criticism in the pages that follow, this review is on the whole favourable.

II

The philosophical and scientific questions and ideas raised in the Schilpp volume for Karl Popper bear on virtually all aspects of philosophy. The work is long, published in two volumes, in a compass of 1323 pages. There are thirty-three contributions on a wide range of topics by distinguished philosophers, public servants, and scientists. These include four British knights, a British lord, and two Nobel laureates. Popper's own contributions constitute a substantial work in themselves; an intellectual autobiography of 180 pages, and a reply to his critics running to 236 pages.

One could not do justice to this work in the space of one review. I have therefore with the encouragement of the editors of this journal undertaken a survey of Popper's work based on this volume. It will be published in five installments as follows:

I. The present essay deals with biology, evolution theory, evolutionary epistemology, the "Three Worlds".

II. The second essay treats consciousness and the mind-body problem, and the relation of these to the problems of determinism and indeterminism, physics, and probability theory.

III. The third essay discusses rationality, criticism, and logic.

IV. The fourth installment reviews Popper's contributions to historical and social philosophy and to intellectual history.

V. The fifth and final installment treats Popper's background and intellectual development, his intellectual biography, and the rise of the Popper School; and it gives a summary evaluation of Popper's contributions to philosophy.

Anthony Quinton, in an interesting study of Popper's work,[2] has commented that although Popper is "the most important and

interesting of living British philosophers", there is as yet "no body of informed and serious criticism of Popper's thought to draw upon." It has been my intention in preparing this review to contribute modestly to the creation of such a critical corpus.

III

Popper's discussion of biology and evolution theory dominates the Schilpp volume as it did his most recent book: *Objective Knowledge: An Evolutionary Approach* (1972). Popper's interest in biology is of longstanding. I recall one day in the spring of 1959, as we were walking through Hyde Park together, when Popper discussed, in the most animated and delightful way, the issues between Darwin and Lamarck, and Samuel Butler's treatment of evolution in *Erewhon*. It was, so he told me then, a subject that had excited him since he was a young man.

Popper's *public* discussion of biology is, however, comparatively recent. I believe that I can date it quite exactly to the afternoon of Tuesday, November 15, 1960. On that day the members of Popper's seminar had assembled as usual around the long table in the old seminar room on the fourth floor of the old building of the London School of Economics. When Popper appeared, he announced that he would abandon the usual format and would read a new paper of his own. That new paper, which spoke of "three worlds", of biology, and gave qualified support to Hegel's theory of objective mind, took the members of the seminar off guard. The discussion that followed was more bewildered than heated; and Popper, usually one of the most persistent of men, did not pursue the matter that term. No member of the seminar, perhaps not even Popper, could have predicted that they had just heard the first note in a new development in his thought.

Present that afternoon were some of the closest members of Popper's circle, including J.W.N. Watkins, J.O. Wisdom, Imre Lakatos, I.C. Jarvie, and myself. About a dozen students, including Alan Musgrave, also attended. Ernest Gellner was no longer attending the seminars; Sir Ernst Gombrich did not attend that meeting, and Joseph Agassi had a few months earlier departed for Hong Kong. A.I. Sabra was still in Egypt; and Paul Feyerabend had been in Berkeley since 1958. Not one of these associates, neither those who were present nor those who were absent, had more than marginal interest in biology. None of them would – had be been asked to give a sketch of Popper's ideas and of his development – have mentioned biology. And Popper himself, in the autobiographical sketches that he had written for *British Philosophy in Mid-Century*, for *The Postscript* (still unpublished), and elsewhere, made virtually no mention of biology or its philosophy.

In the years since then Popper has developed his ideas on the "three worlds" and on biology, drawing in large strokes, and has thereby generalised and unified his philosophy. Although the fundamental components of his framework have not been much affected by this development, their presentation has been transformed; they are explained and in minor ways corrected. Prior to 1960 the development of Popper's thought could have been presented, however unjustly, in an incremental way: his new foundations for logic and his work in indeterminism in physics, his contributions to probability theory, all could be presented as elaborations of his early work in induction and demarcation. The new work in philosophy of biology, however, is not simply incremental: it unifies the whole.

The manner in which Popper's new biological concerns serve to integrate his thought can be seen in the new formulation of the chief problem of epistemology which he introduces in his "Replies to My Critics" : "*The main task of the theory of knowledge,*" he writes (p. 1061, my italics), "*is to understand it as continuous with animal knowledge; and to understand also its discontinuity – if any – from animal knowledge.*"

This formulation contrasts with earlier statements by Popper. In *The Logic of Scientific Discovery* (p. 15), he wrote: "The central problem of epistemology has always been and still is the problem of the growth of knowledge." Later in the same work (p. 51), he wrote that the "main problem of philosophy is the critical analysis of the appeal to the authority of experience." And in *Conjectures and Refutations* he described the solution to the problem of demarcation between science and non-science as "the key to most of the fundamental problems of the philosophy of science." (p. 42).

I do not mention these shifting estimates of centrality and importance in a carping way. Such an estimate may of course change in the course of a career, and the various problems cited not only overlap considerably: they are aspects of the same problem. I draw the reader's attention to these formulations to underscore the development that has taken place. Although Popper from time to time mentioned or alluded to animal knowledge in his earlier work, he did not give the study of animal cognition an important, let alone the central, place among the problems of epistemology. Now it is at the forefront: epistemology is chiefly concerned with the continuities and discontinuities of human and animal knowledge: indeed epistemology is to become a science of comparative cognitive apparatuses. Whereas in *The Logic of Scientific Discovery* (p. 15) Popper had maintained that the most effective way to study the growth of knowledge was to study the growth of the most advanced form of knowledge – scientific knowledge – he now also turns to prehuman forms of knowledge and to evolution for examples of the growth of knowledge.

Both scientific knowledge, as recorded in theories, and the biologically based cognitive structures of animals, can be studied objectively as products. Both are *objective* structures, the first being an exosomatic development, the second being endosomatic developments. Both, according to Popper, are produced by the same Darwinian mechanism: the highest creative thought, just like animal adaptation, is the product of blind variation and selective retention – trial and error. The same process governs both biological emergence and the growth of knowledge in science. Scorning traditional philosophical approaches to knowledge which focus on the subjective interior experience of the cognizer or "knowing subject" – his beliefs and perceptions – Popper turns to the objective products of the cognitive process, viewing cognitive structures and scientific theories alike as knowledge achievements.[3]

Just as, for the earlier Popper, the philosopher compared the content of competing theories and estimated their "verisimilitude", so for the more recent Popper the philosopher examines the entire range of cognitive structures found in the animal kingdom – the stored templates modeling the useful stabilities of the environment – and compares the "fit" between the organic system and its environment. Thus Popper significantly generalises his earlier approach: our experience is theory impregnated and structure impregnated.

IV

What is the bite behind the new approach? Before anything else, Popper is an enemy of subjectivism and idealism. And he has found in biology and evolution theory, and particularly in the comparative study of animal and human cognition, a new argument for objectivism and realism. While neither subjectivism nor idealism is an issue of central importance in theoretical biology today – with some exceptions such as Thure von Uexküll – evolution theory has in the past been used to argue in support of relativism and historicism, and contributed to the strength of these positions in the nineteenth century. Thus Popper's new biologically based argument for objectivism and realism furthers his longstanding battle against relativism and historicism. Perhaps more important strategically, it provides Popper with an independent line of argument for realism not dependent on his controversial critique of the subjectivist Copenhagen interpretation of quantum mechanics.

To gain an adequate impression of how Popper now sees these matters, it is necessary to read Popper's own contributions to the Schilpp volume in close conjunction with Donald T. Campbell's contribution to the latter. Campbell's essay, "Evolutionary Epistemology", is in my opinion, and I believe in Popper's, the most

important contribution to the volume. Popper describes it as a treatise of prodigious learning. "There is scarcely anything in the whole of modern epistemology to compare with it, certainly not in my own work," he reports. Popper describes Campbell's paper as a reliable guide to his own thinking: as the one "which shows the greatest agreement with my epistemology and .. an astonishing anticipation of some things which I had not yet published when he wrote his paper ... For me the most striking thing about Campbell's essay is the almost complete agreement, down even to minute details, between Campbell's views and my own." (p. 1059).

Campbell's paper is based on a much neglected earlier essay (1960): "Blind Variation and Selective Retention in Creative Thought as in Other Knowledge Processes."[5] Although Campbell's new paper is presented in a modest historical and descriptive style, it is densely packed with information. It is also valuable because of the level of abstraction which it attains. Popper himself reaches a new level of abstraction as a result of his interchange with Campbell – and also from his encounter with some related work by Konrad Lorenz to which Campbell drew his attention.[6] This profitable interaction says something in favor of the dialectical intentions behind the Schilpp series. Although Campbell says that Popper is the modern founder and leading advocate of an evolutionary epistemology, Popper himself had not previously put the problem in so full a context. Campbell's work enables one to see the power of an approach along Popper's lines; it opens some problems only touched by Popper; and it illuminates an area of the history of philosophy in which Popper himself has done little work.

Although much of the interest of "evolutionary epistemology" comes from the examples and analysis provided by Lorenz and Campbell independently of Popper, Campbell gives the chief credit for the emerging position to Popper – and it is indeed within Popper's account of scientific knowledge that these examples and analysis gain a context in which their full power – and their epistemological and philosophical significance – can be felt.

V

I attempt in the following to reconstruct a part of the argument that *emerges* from Popper, Campbell and Lorenz. I shall not limit myself to their presentation or examples, and shall elicit a point of view supported by their discussion.

If cognitive structures and the continuity or lack of it between animal and human knowledge are to become pieces in the debate between subjectivism and objectivism, we need to ascertain what the cognitive structures of man and various animals are like, to what extent, if any, they report a common reality, to what extent these

structures – like Kantian categories – create and define external reality and limit access to it.

The first step in satisfying these needs is to concede that part of the subjectivist contention is correct: we are indeed subject to limited and imperfect cognitive apparatuses; and these apparatuses are *essentially* limited and imperfect. Take as instructive examples – what Popper would call "rubber-stamp examples" – the microscope and the photoprint.

Consider the domain resolved with the lens of a microscope (Lorenz, pp. 112-4). The fineness of the smallest structure of the object still visible with the aid of the lens depends upon the relationship between the angle of aperture and focal length. In order for a structural grating to be seen, the first diffraction spectrum which is thrown by the grating must still fall into the front lens. When this is no longer the case, no structure is visible and one sees a smooth brown surface. *If there were in nature but a single microscope*, one might conclude that structures are only "conceivable" up to the fineness resolved by that microscope, and that to speak of finer structures is meaningless. A subjectivist conclusion about the world and the limits of human reason might thus be drawn from the examination of the apparatus by constructing an analogy between the microscope and human cognition.

A similar argument can be based on the character of the ordinary photoprint. The photoprint screen cannot produce any points of the object "represented" finer than those corresponding to the finite elements of the screen. The grain of the photographic negative permits no unlimited enlargement. Only that can be represented which can be "spelled out" on the "keyboard" provided by the grain of the print. Lest one conclude that an external world is, say, composed of squares from the observation that the grain of the photograph is composed of small squares, one must – so it may be suggested – avoid saying anything at all about an objective world independent of "squareful" representation, and speak only of different manners of arrangement in square.

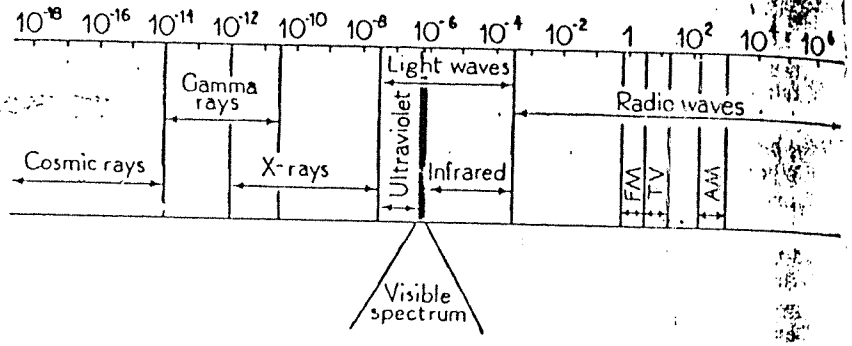
VI

In their counter-argument for realism and objectivism, Popper, Campbell and Lorenz call for a new epistemological science of comparative apparatuses. To suggest the argument involved, one needs 1) to show the vicariousness and indirectness of all cognition; and, 2) to engage in some comparative studies of cognitive structures.

The first task is as significant as the second. Whereas traditional subjectivist epistemologies begin with direct, immediate, unproblematic sense experience, Campbell and Popper want to demonstrate the

indirectness and problematicality of sense experience. Popper has of course done this for many years with his criticisms of the view that "Truth is manifest", and his thesis that all sense experience is theory impregnated.

Take the example of the electromagnetic spectrum to show the indirectness of perception.



Although the entire spectrum ranges in wavelength from less than one billionth of a meter to more than a thousand meters, the visible spectrum appears as but a tiny slice of the entire energy band: we can see in only that small section between 400 and 700 billionths of a meter. Man has no direct access to information carried within the larger part of this spectrum. Our senses do not immediately respond in this realm. Cosmic rays, gamma rays, X-rays, radio waves: we live in an electromagnetic sea, as it were, and nonetheless these do not register unassisted on our eyes, or any other sense organs. Our sensory apparatus in effect filters out all except a narrow band of light waves. Prior to the discovery of the spectrum of electromagnetic waves and prior to the invention of apparatus to tap, channel, and register X-rays, radio waves, and such like, the realms of existence and knowledge now opened by them were beyond human ken.

Why should men – and the vast majority of animals – be oriented to the external world principally through the light spectrum?

Campbell provides a simple explanation (p. 414). *Vision is the opportunistic exploitation of a coincidence*: the coincidence of locomotor impenetrability with opaqueness within a narrow band of light waves. Within this band, air and water are both transparent and locomotor-penetrable. On other wave lengths the coincidence, and

thus the cue value, disappears. As Campbell remarks, both clear glass and fog are paradoxical in this context – the first being clear but not penetrable, the second being penetrable but not clear. Modern man now exploits another coincidence of the electromagnetic spectrum – radar – in order the better to cope with fog.

Campbell's account suggests that locomotor activity is epistemologically prior to vision. The problem of the paramecium, for example, is to put itself in a nourishing and nonnoxious econiche. It solves its problem through blind variation of locomotor activity – locomotor activity which begins when starvation approaches and ends when the organism is sated – or has been killed in the search. Its exploration is – relatively speaking – direct and non-vicarious, the main ontological presupposition of its activity being the phenomenal experience of a greater spatial than temporal discontinuity: that change relevant to nourishment appears more rapidly by moving around than by standing still.

The numerous epistemologically relevant organs and activities that have been laid on top of locomotor activity in the course of evolution are *indirect* and *vicarious*. For Campbell, all knowledge processes, upon being examined in continuity with evolutionary sequence, turn out to involve mechanisms at various levels of substitute or vicarious functioning, hierchically related. At each level a process of retention operates in accordance with principles of natural selection.

The levels of vicarious and indirect knowledge processes charted by Campbell include vision, habit, instinct, visually and mnemonically suported thought, socially vicarious exploration, language, science, and others.

Vision is as vicarious and indirect as radar. Radar is used – by a ship for instance – as a substitute for locomotion. Instead of exploring its environment directly, with all the attending risks, the ship sends out radar (and perhaps also sonar). The radar beam is emitted blindly and is selectively reflected from objects, the opaqueness to the wave band vicariously representing the locomotor impenetrability of objects. Trial and error is removed from full locomotion and vicariously invested in the radar beam. Similarly with vision supported by thought: one has an environment represented vicariously – in the image in the visual cortex. This is utilised in a vicarious trial and error search or consideration of potential locomotions in thought, and functions as what Popper calls a "plastic error-eliminating control." Successful locomotions in thought may be put into *overt* locomotion.

Thought and vision may be supported by *memory*. The environment may be searched vicariously through an examination of representations held in memory, the memory operating as a vicarious criterion and substituting for the external state of affairs. Similarly

for social exploration: social forms of animal life are found subsequent to solitary forms. Within a social organisation, an individual member may – as a scout, say – have his own trial and error exploration substituted for exploration on the part of the group. The scout here is the vicar for the group. What the group learns through the vicar is both indirect and vicarious. The "ontological assumption" here is fairly definite: it is assumed that the vicar is exploring the same world as that in which his group is living, and that that common world is moderately stable – sufficiently so for the experience of the vicar to hold, vicariously, for the group.

Language also functions vicariously, enabling the results of the vicar's search to be relayed to the group without either locomotion or visual representation. Underlying it is the discovery of the representability of things and actions by words.

Science, tradition and culture are also parts of the vicarious cognitive process of biological and evolutionary importance. To explain the way in which they work Popper's idea of Objective Mind – or World 1, World 2, and World 3 – is particularly useful. This is indeed one of the most important new concepts that Popper has introduced during the past fifteen years; it dominates *Objective Knowledge*, and it plays a large role in Popper's own presentation in the Schilpp volume (although not in the discussion by the contributors, many of whom wrote their contributions prior to Popper's publication of the three worlds notion). The notion serves to convey in a nonmystical way the kind of thing that Teilhard de Chardin called the "noösphere".

Popper refers to the physical universe as *World 1*, and to the world of subjective conscious experience as *World 2*. And he uses the term *World 3* to refer to the realm of such things as the *logical contents* of books, libraries, computer memories, the logical structure of arguments, the objective problem situation at any time in a particular science. This third world – which obviously arouses Popper's chief interest – is, he contends, a "natural product of the human animal, comparable to a spider's web." This world is objective and autonomous, and exists independently of being realized in the subjective conscious experience of any human individual: it is "objective mind". The objective contents of World 3 phenomena are potentialities. "So it is," Popper writes (*Objective Knowledge*, p. 117), "with all ecological niches. They are potentialities and may be studied as such in an objective way, up to a point, independently of the question of whether these potentialities will ever be actualized by any living organism."

Those aspects of the contents of World 3 which are intended to represent the physical world (World 1) may be consulted vicariously in lieu of consulting World 1 directly. Indeed a double vicariousness and indirectness comes into play here. World 2 experience can serve

both as a vicarious representative of World 1 and as a vicarious representative of World 3, which is in turn a vicarious representation of World 1. World 2 experience can conduct an exploration of World 3 in lieu of conducting an exploration of World 1. And World 2 experience can explore World 1 in order to test World 3. In the latter case, available experimental evidence is a crucial part of those econiches to which theories adapt. As Campbell put it (1960, p. 384): "At this level there is a *substitute* exploration of a substitute representation of the environment, the 'solution' being selected from the ... exploratory thought trials according to a criterion which is in itself *substituting* for an external state of affairs." These processes are obviously vicarious and indirect, manifoldly interrelated, and rely heavily on feedback. In an econiche infused with culture – in heavy contact with World 3 – one can lead a most abstract existence – 'abstract' with reference to vicariousness and indirectness of one's contact with World 1.[7] One can use World 3 to cut oneself off from World 1, just as one can use World 3 to sharpen one's questions about and one's participation in World 1.

The examples and instances given so far indicate in broad outline, with little detail, the thrust of the argument for the importance of vicariousness and indirectness in evolutionary epistemology. Next I wish to turn to a few examples of comparative cognitive structures, before indicating how Popper, Campbell and Lorenz use this information in argument against subjectivism and idealism.

VII

Return first to the examples of the microscope and the photoprint. If there were microscopes of but one power in nature, one might conclude that structures were only 'conceivable' up to the fineness which such microscopes resolved, and that to speak of finer structures is meaningless. Once one knows of microscopes of different power, one comes to a different conclusion. Suppose, for example, that there is a less strongly resolving lens which registers brown for structures which are still visible as structures by the original instrument. One will hardly be inclined to think its power of resolution as delimiting reality! Any microscope will be limited in its achievement; even the most powerful lenses have limits as to the fineness of the structure which they resolve. There will be no reason to conclude that any particular limitation says anything about the character – let alone the conceivability – of the external world.

As to the photoprint, Popper, with Campbell and Lorenz, contends that our neural apparatus for organising an image of the world is indeed rather like a photoprint screen, and cannot reproduce any finer points of the external world than are permitted by the net which is being used. "Just as the grain of the photographic

negative permits no unlimited enlargement, so also there are limitations in the image of the universe traced out by our sense organs and cognitive apparatus." (Lorenz, p. 30). But in different organisms, this can happen in a more or less complex manner. As Lorenz puts it, "if one examines methodically what the cross-stitch representation permits to be stated about the form of the thing-in-itself, the conclusion is that the accuracy of the statement is dependent upon the relationship between the size of the picture and the grain of the screen. If one square is out of line with a straight-line contour in the embroidery, one knows that behind it lies an actual projection of the represented thing, but one is not sure whether it exactly fills the whole square of the screen or only the smallest part of it. *This question can be decided only with the help of the next finest screen.*" (p. 30).

In a parallel way, by surveying the cognitive apparatuses of animals other than humans – in effect, by consulting less fine screens – one undercuts the idea that the limits of the most recent (evolutionarily speaking) human cognitive apparatuses define the limits of the external world. It has long been known, and argued by Simmel, Uexküll, and others, that the phenomenal worlds of animals differ from one another and from man's. The boundaries separating what is experienced from what is beyond experience differ for each sort of organism. The frog provides a good example.

The vision of the frog, like radar, ignores many dimensions of the external world which are visually present to humans. An M.I.T. research group consisting of Lettvin, Maturana, McCulloch, and Pitts[8] devised an experiment in which visual stimulation could be offered to one eye alone of an immobilized frog. The frog was situated so that its eye was at the center of a hemisphere seven inches in radius. On the inner surface of the hemisphere thus created, small objects could – with the use of magnets – be placed in different positions and moved from one position to another. Microelectrodes were implanted in the frog's optic nerve to measure electrical impulses sent to the brain by the eye. In the course of presenting various objects, colours, and movements to the frog, the investigators discovered that only four different kinds of messages were sent from the retina to the brain. Regardless of the complexity and differences present in the environment, the frog's eye is equipped to transmit only a few different kinds of messages and filters out – or simply cannot register – any additional information presented.

McCulloch and his associates termed the four different kinds of visual activity registered by the frog: 1) sustained contrast detectors; 2) net convexity detectors; 3) moving edge detectors; 4) net dimming detectors.

The first provides the general outline of the environment. The

third enhances response to sudden moving shadows – such as a bird of prey. The fourth responds to a sudden decrease in light, as when a large enemy is attacking.

The second – the net convexity detectors – respond neither to general changes of light nor to contrast, but only when small dark objects come into the field of vision and move close to the eye.

As McCulloch and his group put it, commenting on the frog's resulting behaviour: (p. 231):

> "The frog does not seem to see or, at any rate, is not concerned with the detail of stationary parts of the world around him. He will starve to death surrounded by food if it is not moving. His choice of food is determined only by size and movement. He will leap to capture any object the size of an insect or worm providing it moves like one. He can be fooled easily not only by a bit of dangled meat but by any moving small object. ... His choice of paths in escaping enemies does not seem to be governed by anything more devious than leaping to where it is darker."

Thus the vision of the frog differs from that of men with respect to quantity and quality of information conveyed, not with regard to vicariousness and indirectness. The frog does not inhabit a different objective world; what he sees does include fewer details, and these are reproduced through a coarser screen. From the vantage point of our own cognitive achievements we would not take seriously the claim of an idealistically disposed frog that the limits of his experience define the limits of the world, or that it is meaningless to speak of the sorts of things which he cannot perceive.

As the visual world of the frog differs from our own, so does the spatial world of the water shrew. The water shrew masters its living space almost exclusively by path learning kinesthetically acquired through trial and error movement. Whereas a man can master a spatial problem by a simultaneous clear survey over the data, most reptiles, birds and lower mammals lack this capacity. The water shrew commands its space through kinesthetically ingrained movements known by rote so precisely and exactly that there is virtually no steering or control by optical or tactile means. The human being can approximately understand what is going on for the water shrew for he is able to behave this way himself, as for example in a strange city for which he has no map. But the water shrew, presumably, would not be able to understand the human's way of mastering space through simultaneous clear survey. As Lorenz puts it, "basically, we can comprehend only the lower precursors of our own forms of perception and thought."[9]

The spatial world of an animal may be even stranger than this example would suggest. A primitive locomotor animal might have a thirst space which it uses when thirsty, a separate hunger space, a separate escape space for escape from each predator, a mate-finding space, and so on for each utility. Only with a higher stage of

evolution does the hypothesis emerge that these spaces are the same or overlap. And this hypothesis amounts to hypothetical realism.

The white rat, the cat, the dog, the chimpanzee, all have access to this stage: spatial learning achieved in the service of one utility is available immediately for another. Accompanying this there emerges curiosity about all possible spaces, a trait with survival value. "The different *Umwelten* of different animals," Campbell concludes, "do represent in part the differential utilities of their specific ecological niches, as well as differential limitations. But each of the separate contours diagnosed in these *Umwelten* are also diagnosable by a complete physics, which in addition provides many differentia unused and unperceived by any organism." (p. 448).

VIII

The examples presented will provide the reader with some impression of the kind of argument that emerges from this discussion by Popper and Campbell. The argument provides no "knock-out" of idealism, but it does utilise scientific data in an interesting way in the service of analytical insight. It should change the context in which realism is debated. At the very least, like so much of Popper's work, this argument has the merit of setting epistemological inquiry in a context in which linguistic philosophy and commonsense realism can no longer be taken seriously.

The upshot of the argument is, I suppose, that when we consider the indirectness and vicariousness of cognition within any particular animal, and also the differences in cognitive apparatus from one animal to another, we see that the various vicars and apparatuses make no sense individually or collectively in their mutual integration, hierarchial arrangement and controls, except by reference to a *common* external world, in which they function and in connexion with which they have evolved. Each of the vicars – kinesthetic sense, vision, language, scientific representation, and the various others – has evolved separately and can be explained in terms of natural selection survival value only by reference to the others and to an external world. The different imperfections and limitations of each of these can be separately demonstrated. The way in which the various vicars complement one another, check and partly compensate for the inadequacies of one another, makes no sense apart from a common reality. From the height of our highly complicated cognitive apparatus we can understand the way in which the spatial and other cognitive equipment of various animals approximates, in however imperfect a way, to devices more elaborately and complexly developed in ourselves; and we can suppose that we and these animals have evolved in our diverse ways while coping with a common environment. We can guess at the features of this

external environment as it transcends our evidence by analysing the ontological presuppositions of the vicarious devices, including theories, used by ourselves and by animals in cognition. As Campbell puts it, "Biological theories of evolution . . . are profoundly committed to an organism-environment dualism, which when extended into the evolution of sense organ, perceptual and learning functions, becomes a dualism of an organism's knowledge of the environment versus the environment itself."

Moreover, modern science and physics do give us a standpoint from which we can even criticize and evaluate our own cognitive apparatus. Even if we assume with Kant that built-in categorical notions of space and causality determine our phenomenal experience, we can within modern physics correct for the "Newtonian bias" of our own perceptual apparatus, just as we can transcend the light spectrum by the aid of exosomatic theories and inventions which make it possible for us to tap energies and wavebands unknown to ancestors who, perfectly like us physiologically, yet lacked our theories and inventions.

IX

As will be evident from my sympathetic account of Popper's and Campbell's views in the foregoing, I have no serious objection to Popper's application of biology and evolution theory to issues of epistemology and philosophy. Much of it seems to me brilliantly suggestive, even though at this stage it is still quite programmatic. As Campbell himself summed it up: "These several disparate comments scarcely begin the task of relating the critical-realist, natural-selection epistemology to the recurrent issues in the history of the theory of knowledge. Potentially it can provide a dialectic resolution to many old controversies. But spelling out the points of articulation with the main body of epistemological concerns remains for the most part yet to be done." (p. 450)

Nonetheless, serious difficulties arise from Popper's accounts of biology and evolution, and in turning to these I must now take up a somewhat more critical stance. I wish to mention the question of the originality and adequacy of Popper's contribution to evolution theory and also his new interpretation of the status of evolution theory.

The first question is that of the originality and adequacy of Popper's contribution.

What Popper himself describes as an "important contribution to a theory of evolution of the Darwinian type" that "considerably extends" the theories of J.M. Baldwin, C.H. Waddington, G.G. Simpson, and Erwin Schrödinger, is directed to what Popper describes as the problem of orthogenesis, or spontaneous direction in evolution.

Although the way in which Popper uses this term, as well as his perception of the problem, are not common among contemporary geneticists, I shall examine his discussion on its own terms.

The problem as Popper sees it is that the idea of evolution from *random* variation – which is an important part of Darwinian and "new synthesis" evolution theory – seem to be *prima facie* almost absurd. As we have seen, Popper accepts the idea of random variation (or "blind variation", to use Campbell's terminology), and – also like Campbell – he wishes to explain how it might function despite the *prima facie* evidence to the contrary.

As Waddington, himself a Darwinian, posed the problem: "To suppose that the evolution of the wonderfully adapted biological mechanisms has depended only on a selection out of a hapahazard set of variations, each produced by blind chance, is like suggesting that if we went on throwing bricks together into heaps, we should eventually be able to choose ourselves the most desirable house."[10] It is the old problem of the monkey at the typewriter eventually typing out Shakespeare. Waddington, Popper, and many contemporary evolution theorists are unable to accept Sir Julian Huxley's contention that "The hoary objection of the improbability of an eye or a hand or a brain being evolved by 'blind chance' has lost its force" because "natural selection operating over the stretches of geological time" explains everything.[11] Quite the contrary, in the time available the adaptations required are virtually impossible on a random basis.

To use Popper's formulation of the problem, Darwin's theory appears to expect that evolutionary sequences will be of a random-walk type: an example of a random walk being the track described by a man who consults a roulette wheel at every step to determine the direction of his next step.

Yet, and here is the problem, random walks are not at all common in evolution. How then, within evolution theory, can one explain the presence of nonrandom walks?

According to Popper, the problem consists in 1) supposing that the "selection pressures" will all be external or environmental and in 2) concentrating on anatomical rather than behavioural change. Popper suggests that there are internal as well as external selection pressures, and that the internal selection pressures take the form of plastic controls on the part of the organism (itself a system of plastic controls). The chief internal selection pressure will come from *behavioural* phenomena which include preferences or aims (which are dispositional in character) of the organism.[12] These behavioural controls are given a genetic base; thus Popper postulates that different sorts of genes may control anatomy and behaviour, the latter in turn being subdivided into genes controlling preferences and genes controlling skills. These operate hierarchically in mutation.

Changes in preference structure will precede changes in skill structure, which in turn will precede changes in anatomical structure. Popper gives a number of examples – the woodpecker, an airplane with an automatic pilot, etc., – to illustrate his point, and argues – I think successfully – that were changes in anatomical structure to precede changes in skill and preference structure, the result would tend to be lethal. The preference structure is in any case, on his account, the spearhead of evolution. (This in turn helps explain the apparent phenomenon of evolution towards higher and more complex forms of life. Popper proposes that those things judged to be higher forms of life will have behaviourally richer preference structures. These preferences are dispositional and need not be conscious; they may become conscious – and this leads to another problem, that of the emergence of states of consciousness.)

This account puzzled me when I first heard Popper present it in 1961 in Oxford, when he delivered his Spencer Lecture on "Evolution and the Tree of Knowledge." It puzzled me again in 1965 when I heard him give the Compton Lecture "Of Clouds and Clocks" at Washington University in St. Louis. Now I have had an opportunity to read carefully over it again, both in *Objective Knowledge* and in the Schilpp volume, and the puzzlement remains. It is not that I disagree with what Popper says; quite the contrary. I have no idea of what Popper suposes his *new contribution* to evolution theory to be – despite reading his words over and over again with the conviction that I must have missed some crucial passage.

That behavioural and structural or anatomical mutation must be distinguished and that behaviour is the spearhead of evolution are now common contentions in evolution theory. As R.F. Ewer wrote in 1960: "Behaviour will tend to be always a jump ahead of structure and so play a decisive role in the evolutionary process."[13]

Waddington, one of the most widely read writers on these matters, has been quite explicit. He writes:

> "The general idea that the first step towards a new evolutionary change is for the animal to acquire a new habit, or a new mode of behaviour, is one of the fundamental ideas advanced by Lamarck. It goes back long before the Baldwin effect. I am sure it is a very important point, although not an absolutely general one, since it is obviously difficult to apply it to plants."[14]

Here one has, in a few brief sentences by Waddington, both what appears to be Popper's chief point, and also an objection to the generality of the theory which Popper himself never mentions. In the discussion that follows, Waddington explains, in conversation with Arthur Koestler and Paul A. Weiss, that his point is intended to help explain orthogenesis and to reduce the chance or random element – or to bring it under plastic control as it were.

Finally, if further evidence were needed, Sir Alister Hardy's Gifford Lectures, *The Living Stream*,[15] have as their chief point the thesis that behavioural change precedes structural change. Hardy's ideas were first published in 1942, and are presented most elaborately in his sixth Gifford Lecture: "Behaviour as a Selective Force."

In sum, without disagreeing with Popper's view, which he calls "genetic dualism," I find no ground for his contention that it is new. Nor can I find much support for his notion that it is a rather speculative idea, for considerable theoretical and empirical support for it is given by the various other writers mentioned.[16]

Yet it is not hard to discover why Popper should have supposed his position original. He is familiar with the studies in biology and evolution theory of his friend the late Erwin Schrödinger. In *Mind and Matter*[17] Schrödinger, like Popper, had distinguished between selection influenced by structural mutation and selection influenced by behavioural mutation. But Schrödinger argues that structural change *precedes* and is then developed by behavioural change. In challenging this last point, Popper improves Schrödinger's account – but appears unaware that other writers of distinction reached similar conclusions prior to and independently of both his own and of Schrödinger's work.[18]

X

However the issue of the originality of this aspect of Popper's thought may be decided, a more important question has to do with the *adequacy* of his account in explaining adaptation, orthogenesis and related problems. Although Popper presents his idea modestly, he evidently thinks that his position is, if correct, adequate to solve the problems of adaptation and to deal with related problems arising from explanation by random mutation. He writes of "*my solution* of the problem [of orthogenesis]".[19] While the idea that behavioural change leads structural change does help to explain orthogenesis and also – as Popper points out – explains and corrects, and simulates, other unsuccessful attempts to do so – such as vitalism – it is inadequate by itself to coordinate the doctrine of random selection and the facts of phylogeny.

To explain why Popper's theory is inadequate one must identify two separate lines of approach to the problems connected with orthogenesis. Discussion of these two approaches might be muddied somewhat due to an unfortunate terminology: both approaches are described by their proponents as being concerned with *internal* selection, as opposed to external environmental selection. But something entirely different is meant by the term "internal" in the two cases.

The first case is that just discussed, wherein writers such as Ewer, Waddington, Hardy and Popper have stressed the importance

of internal behavioural selection stemming from changing dispositions and habits of the organism. This approach can be thoroughly Darwinian: Popper has, so he boasts, kept "strictly within the logical boundaries" of an orthodox neo-Darwinism or "New Synthesis" framework.

The second case is different. Here the idea of internal selection refers *not* to the organism's internal dispositional states but rather to "coordinative conditions" (Whyte's term) of biological organisation, conditions under which life may evolve at all. These conditions restrict the range of possible mutations on the basis neither of the facts of the external ecological niche nor of the internal dispositional state but rather on pre-competitive internal genetic grounds. This kind of selection is non-Darwinian and supplements and complements Darwinian theory by adding a separate source of selection. On this account, mutations reaching the external test have previously been sifted internally. These organisational restrictions in effect define unitary laws underlying evolutionary variety. Although permitting unlimited variations, they restrict the variations to a limited number of themes, thus confining evolution to particular avenues not defined or determined by external factors. Thus there is not only selection at the phenotypic level but pre-selection at the molecular and chromosomal levels. (It is essential to the argument that this pre-selection is not random: this argument is presented in great and convincing detail by Whyte.) External factors, then, only come into play on those mutations which have passed the internal conditions.

Prominent writers on this second type of internal selection include L.L. Whyte, W.H. Thorpe, Ludwig von Bertalanffy, Arthur Koestler, Helen Spurway, and A. Lima de Faria. Popper ignores this kind of internal selection and appears unaware of its existence.

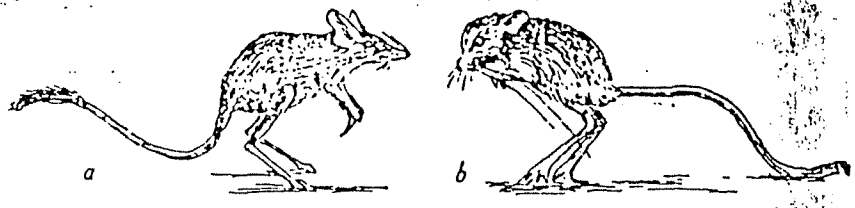
According to this second view, then, a variety of internal but non-behavioural plastic or hierarchical controls play a significant role in phylogeny. These have not yet been specified in detail and the evidence for them is, although persuasive, as yet indirect.

Some nice examples illustrate both the problem and the solution. Take the case of the marsupials, the pouched animals living chiefly in Australia of which the kangaroo is perhaps the best-known example. The marsupials differ from placentals in their method of reproduction: the marsupial embryo is born in a relatively immature state and is reared in the pouch on its mother's belly. Now most mammalians are either marsupials or placentals, the marsupials having evolved along a parallel but quite separate and independent branch of the evolutionary tree from that followed by the placentals. These two lines were firmly separated at the very beginning of mammalian evolution, in the reptilian age, and have independently developed from some common mouselike ancestral organism. Aus-

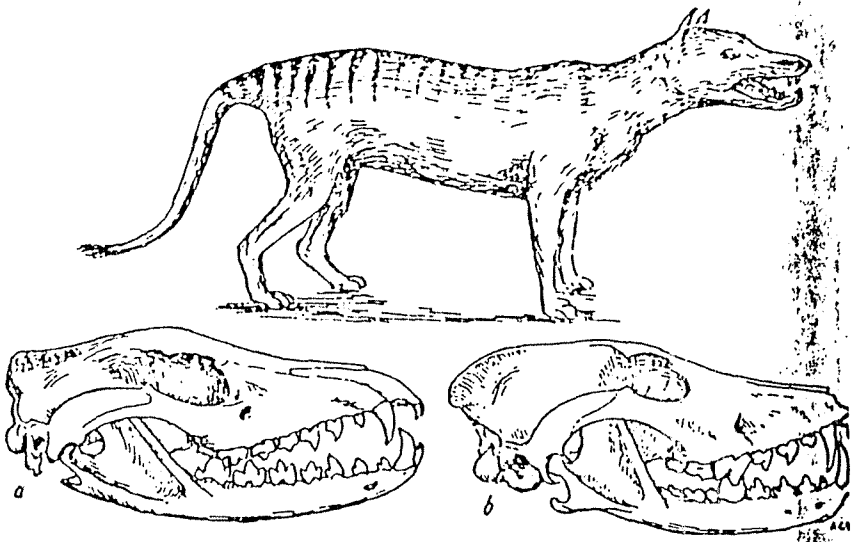
tralia was cut off from the mainland some time during the late Cretaceous; the marsupials, which are thought to have evolved earlier than the placentals, got to Australia before it was cut off, whereas the placentals apparently did not.

The striking thing is that the surviving marsupials and placentals are very similar phenotypically despite their different reproductive systems and their independent histories.

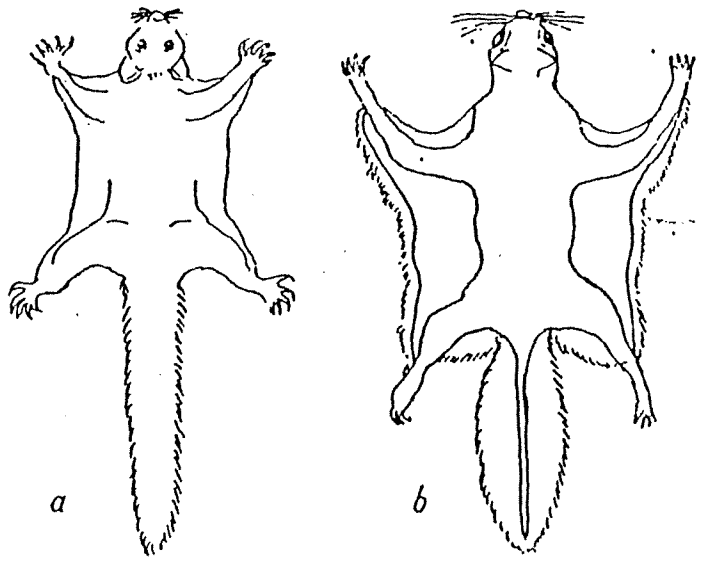
The illustrations given show the problem more dramatically than any verbal description. On the right are a series of placental mammals; and on the left are their opposite numbers among marsupials.



a, the marsupial jerboa (*Antechinomys laniger*) and *b*, the placental jerboa (*Dipus hirtipes*). After Troughton's *Furred Animals of Australia* and the *Cambridge Natural History*.



The marsupial Tasmanian wolf, *Thylacinus cynocephalus*, with below, *a*, its skull compared with *b*, that of the placental wolf *Canis lupus*, drawn to the same scale. From Sir Alister Hardy, *The Living Stream*, after specimens in the Oxford University Museum.



a, a marsupial flying phalanger (*Petaurus sciureus*) and *b*, a placental flying squirrel (*sciuropterus volucella*). After Hardy, and Lull's *Organic Evolution*.

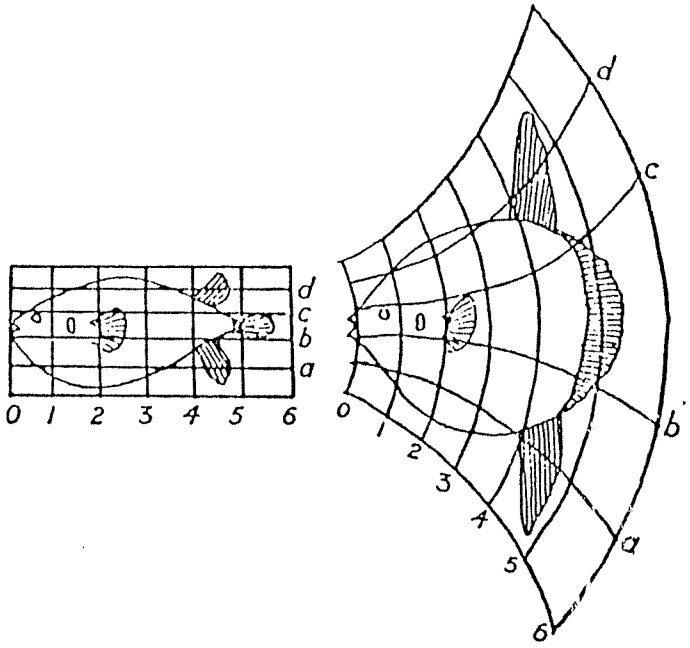
In the course of evolution, the mouselike ancestral marsupials, confined as they were to an island continent, branched out in the evolutionary tree and gave rise to pouched versions of moles, ant-eaters, flying squirrels, cats and wolves, each of them a rough copy of the corresponding placentals. There are to be sure some odd creatures on Australia: the kangaroos and the wallabies, to which there are no corresponding placentals. But for the rest, Australian fauna consist of replicas, perhaps not very good replicas, of placental types.

The idea that this parallel and *totally independent* development *just happened to occur* as a matter of random variation in the face of comparable external environments is preposterous. Adding internal behavioural pressures does not solve the problem either, although it no doubt helps somewhat.

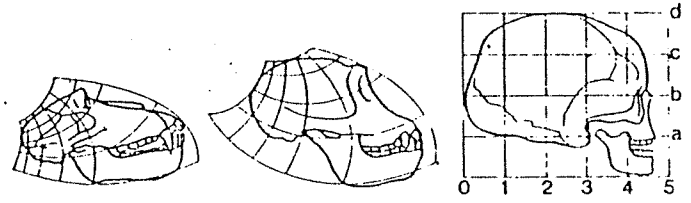
Thus this example seems to confirm the hunch that internal laws govern and limit evolutionary variety. While the case of the marsupials is perhaps the most dramatic, other examples support a similar diagnosis. There are, for instance, the quite striking geometrical relationships shown in d'Arcy Thompson's study *On Growth*

and Form (1917). Thompson discovered that when he placed Cartesian co-ordinates over the shape of one animal, and then examined the shapes of others belonging to the same zoological group, the form of one species could usually be *transformed* into that of another by a relatively simple mathematical distortion of the plan. Comparing the sun fish and the porcupine fish, as illustrated, one sees that while the pattern has remained the same it has been evenly distorted to a mathematical prescription. This phenomenon is by no means isolated, but has sweeping application, as shown in the second and third illustrations.

Similar arguments for the basic idea of a limiting internal restriction on variation can be adduced from the wide-ranging field of homology. I do not know whether one may fully explain the phenomena of orthogenesis and of structural relatedness even by combining the behavioural spearhead theory (of Waddington, Hardy, Ewer, Popper, et al.) with the theory of internal selection (of Whyte, Koestler, von Bertalanffy, et al.). These two types of internal

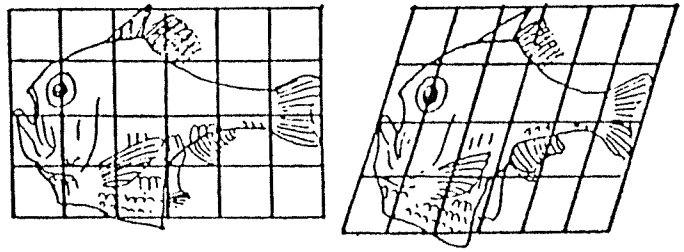


Two closely related fish are here outlined: *Diodon* (porcupine fish) and the sun-fish *Orthagoriscus*. The coordinates on the latter fish bear the same relations to its anatomy as do those on *Diodon*. After D'Arcy Thompson.



Skulls of baboon, chimpanzee and man. After D'Arcy Thompson.

selection, different as they are from one another, appear more adequate to this task together than they are separately.



Two closely related species of deepwater fish are here outlined. The first is *Argyropelecus olfersi*, and on the right is *Sternoptyx diaphana*. The oblique co-ordinates on the second fish bear the same relations to its anatomy as do those on the first. After D'Arcy Thompson.

XI

That Popper should have neglected these widely known phenomena and ideas and this alternative, and potentially complementary, form of internal selection is not entirely coincidental. For it would be difficult to assimilate such material without sacrificing his parallel between the growth of knowledge and the evolution process. This means that his new synthesis, unifying or attempting to unify the various divergent aspects of his philosophy, would come to grief at this point. Let me explain.

Popper is a *critical* Kantian, as he himself explains time and again. The word "critical" is all important. He permits no synthetic *a priori* in Kant's sense of unmodifiable structures forming experience. All knowledge structures are modifiable. Kant's error was in supposing that his categories were necessary and final, beyond modification.

But here in living forms, as in the examples just surveyed, we find some evidence of structures which are unmodifiable or radically limited in their modifiability, "biological archetypes" as Arthur Koestler calls them, whose full recognition seems to impose an *a priori* element in biological development.

To the extent to which coordinative conditions cannot be modified in biology and evolution, the evolution process is not parallel to the process of the growth of knowledge as conceived by Popper (a process wherein all structures are open to modification or revision through criticism). If so, Popper's synthesis, the bedrock of his later work, fails. The exosomatic evolution continuing in the growth of knowledge follows laws different from the endosomatic evolution from which it springs. In particular, the exosomatic process does not have the same limiting conditions as the endosomatic process. Indeed, one might speculate that just with the emergence of consciousness does life transcend its own coordinative conditions.

There is another important difference too between the growth of knowledge and the evolution process. One way to characterise the problem of Popper's epistemology is this: how to create the most lethal possible environment for ideas in which the *production* of ideas nonetheless thrives. Yet within the area charted by this problem one may sport: knowing how to create such an environment does not mean that one need do so. One may prefer to permit a fledgling idea to develop just in order to see what happens. Although one speaks of nature's sporting with organisms too, surely one sports with ideas in a freer and more deliberate way than is open to nature in its sport with organisms. On the other hand, there is no aim governing the evolutionary development of organisms in accordance with which a lethal environment need be created for those organisms! In brief, the evolutionary development of ideas is governed by a meta-aim – itself a 'plastic control' – the elimination of falsity; whereas no meta-aims govern the evolutionary development of organisms.

My remarks here are somewhat speculative: to determine the precise differences between the limiting conditions (if any) of the growth of knowledge and the limiting conditions of the evolution process is an extremely important task which is not conceived, let alone attempted, by Popper.

XII

Having argued that Popper's views on biology and evolution are neither original nor adequate, I turn to his treatment of the *status* of evolution theory.

Popper had already in his earlier work been concerned with the status of evolution theory. He had in *The Poverty of Historicism*

(1945, pp. 106ff.) criticised the notion that evolution theory constituted a universal scientific law comparable, say, to that of Newton. Conceding that it was *scientific* in character, he described it as a *particular* or singular historical hypothesis "concerning the history of the various species of animals and plants on earth." "It is of the same status," Popper explained, "as the historical statement: 'Charles Darwin and Francis Galton had a common grandfather.'"

Popper has evidently come to change his mind during the past two decades, and is now concerned to argue that evolution theory is not only not universal but is also not scientific. It embodies, rather, a metaphysical research programme, is "almost tautological," and is best understood as "applied situational logic." Popper is by no means alone in maintaining that evolution theory has tautological elements. Thus C.H. Waddington writes:

> "The general principle of natural selection, in fact, merely amounts to the statement that the individuals which leave most offspring are those which leave most offspring. It is a tautology."[20]

Popper makes the same point in similar words; and L.L. Whyte makes a related remark:

> "Owing partly to the absence of any direct test for adaptive fitness, the theory appeared to some to be capable of being adjusted to account for every conceivable kind of evolutionary change, not merely those which have actually occurred."[21]

Whyte appears to suggest that a theory that explains everything does not adequately explain anything – a thoroughly Popperian point. But he does not go as far as do Waddington and Popper; Whyte maintains that the criticism is at least partially met by mathematical developments of the theory which have been experimentally tested.

I do not believe that evolution theory is either nonuniversal or tautological or nonscientific. I understand and appreciate why it should seem to Popper and others to be so; but I suggest that this is a misperception.[22]

Popper has himself, in his work on the methodology of the physical sciences, explained how a theory which seemed to its originators and early admirers to have great content can become so familiar that later proponents regard it as definitional or tautological. Popper's own example is of Newtonian theory, which by the late nineteenth century came to be regarded by many physicists as a system of definitions rather than a theory about the real nature of the universe.

Something of the same sort may be happening in Popper's mind *vis à vis* his own theory. I have in mind here his *theory of learning* through conjecture and refutation, which he himself explains in terms of what he calls "situational logic." His learning theory is of

course closely connected with his views on evolution theory by his theory that the growth of knowledge and the course of evolution are closely parallel processes – both of them proceeding by trial and error and to be analysed in terms of situational logic.

If one is not familiar with alternative theories of learning, Popper's theory of learning by conjecture and refutation may well seem trivial. Yet his theory conflicts, both logically and empirically, with rival theories of learning, including inductivism and behaviourism. Popper's learning theory does not simply conflict with alternative *theories* (which would be compatible on his view with the suggestion that it is a metaphysical view); it also conflicts with suggested *facts* as interpreted by those theories.[23] His learning theory also incorporates some ideas about the nature of man and rejects others – such as the "bucket theory of the mind," or the *tabula rasa* theory.

By the same reasoning, the suggestion that evolution theory is empty or even "nearly empty" must also be rejected. Even if evolution *did* occur simply through the situational logic of mutation and adaptation, evolution theory would be far from empty – since Popper's situational logic of conjecture and refutation is not empty. Moreover it is far from trivial to combine the two: i.e., to claim that the evolutionary development of organisms proceeds according to the same mechanisms as does the development of knowledge!

Certainly potential falsifiers are specified in neo-Darwinian theory. It speaks not only of survival and adaptation but states quite specifically how evolutionary change may and may *not* occur. One way in which it may not occur is through Lamarckian inheritance of acquired characteristics.[24] This claim is falsifiable. For example, if Paul Kammerer's experiments with amphibians, including salamanders and the famous midwife toad (*Alytes obstetricans*), had not been discredited and indeed had been repeated, evolution theory would be in serious difficulties.[25] At the very least it would have to be radically modified. On the day that the Weismann barrier is breached, evolution theory will be falsified.

Moreover, original Darwinian theory *has* been refuted. It is not always easy to discuss this matter, since the history of the development from Darwin to neo-Darwinism or "the new synthesis" is not as clearcut as one might wish, in part because its partisans, facing ideological opposition as they did, have not been keen to bare its weaknesses to opponents from Christianity or Marxism (e.g., Pavlovians). The original idea of random mutation has been radically modified: the scope of randomness and the conditions under which it may operate are significantly restricted – as discussed above – in a way that Darwin himself would not have anticipated.

There is another point here. Although Waddington and Popper do a useful service in pointing out how potentially tautologous

elements may enter into evolution theory, it is not clear that their own examples are truly tautologous. For example, to say that "well adapted" *merely* means "has those qualities which made it survive so far" means also that "adapted" *does not mean* "fitter" or "better" in some ethical or normative sense. The history of evolutionary speculation since Spencer dramatically demonstrates that the point just made is far from self-evident or trivial.

Having indicated why I cannot accept *tout court* Popper's contention that evolution theory is not refutable or scientific and why I regard it as significantly more than "trivial," I wish to concede part of his point. He stresses that the theory contains a metaphysical research programme. To be sure, any demonstration in principle, as is Darwin's, contains a programmatic element. Darwinian theory, at least in some of its forms, suggests that it is always *in principle* possible to reduce teleology to causation by explaining in purely physical terms the existence of design and apparent purpose in the world. Yet whether this has as yet been done in even one case appears to be controversial. Popper claims (*Objective Knowledge*, p. 267): "Neither Darwin nor any Darwinian has so far given an actual causal explanation of the adaptive evolution of any single organism or any single organ. All that has been shown – and this is very much is that such explanations might exist (that is to say, they are not logically impossible)." I do not know whether Popper is right here. Many writers on evolution theory, including Sir Alister Hardy (*The Living Stream*, pp. 116ff.), take a different view, citing the experimental work of, say, Tinbergen and Bernard Kettlewell and others as evidence that actual causal explanations of adaptive evolution in certain organisms *have* now been given: the Darwinian programme has actually been carried out.

Of course one could even here reintroduce a metaphysical element. Although there are now some organisms for which direct evidence of adaptive evolution is available, there are of course many many more where there is little such evidence and where the insistence that there *must be* an explanation that conforms to Darwinism does amount to metaphysics.

However these things may be, the precise extent of metaphysical and programmatic elements in Darwinism and in contemporary evolution theory, and the balance between these elements and definitely scientific elements which also exist in evolution theory, remain unexplored ground worth the attention of the historian of science.

I should in passing like to challenge Popper's account in *The Poverty of Historicism* (an account which I presume he has now abandoned, although this is not made explicit) to the effect that evolution theory does not consist in a universal law but rather is only a *particular* historical hypothesis about the history of various

species on earth. *This must be false*, in view of what Popper himself says in the Schilpp volume and in *Objective Knowledge*: the definitions of life and the characterisations of problem-solving introduced by Popper in these volumes by way of interpreting evolution theory leave one no alternative but to interpret evolution theory as a universal theory about *how all life anywhere must evolve*. He now speaks of evolution theory as applicable to *any* world, or framework of limited constancy, in which there are entities of limited variability, wherein some of these entities will survive and others perish. In short, evolution theory applies *wherever* life has arisen, although it does not explain the origin of life itself.

To summarize this section, neither the original characterisation of evolution theory given by Popper in *The Poverty of Historicism* nor the revised characterisations of it which are presented in *Objective Knowledge* and in the Schilpp volume survive scrutiny.

XIII

Before closing this first Part of my review of the Schilpp volume for Popper, I should discuss briefly the contribution of Sir John Eccles, "The World of Objective Knowledge." It is the most extensive separate treatment of Popper's three worlds doctrine to appear in the volume.

The essay itself is an interesting one. Eccles tells how he himself, as a working scientist – he is a neurophysiologist and Nobel laureate – has benefited from Popper's views. He gives a detailed and fascinating account of how a shift from inductivism to falsificationism affected his own work.

Eccles criticizes, I believe soundly, an analogy which Popper has used to the effect that animal production of exosomatic structures – e.g., nests built by wasps and ants, spiders' webs, and such like – is analogous to human production of World 3. By way of contrasting human cultural evolution with such instinctual animal behaviour, Eccles deftly sketches the evolution of culture from the probable invention of language during the Upper Paleolithic era, around 15,000 B.C., through the Mesolithic and Neolithic ages, to the invention of writing in Sumeria around 3300 B.C. It is a sobering picture that Eccles presents, which brings home as well as any argument in the Schilpp volume the immense difference to culture made by the advent of language: the lower Paleolithic age was very long – about a half million years in length. And the men who lived throughout this vast period are utterly silent to us.

Unfortunately Eccles's essay is marred by an important error, which Popper corrects in his Reply. Both in this essay and also in his important book *Facing Reality*,[26] which devotes several chapters to

Popper's account of the three worlds, Eccles writes as if Popper restricts World 3 to the content of those cultural products which are actually *encoded* or *materialised*: e.g., the human brain (which is Eccles's scientific specialty), books, libraries, works of art. In fact, Popper's notion of World 3 is much more abstract. For Popper, World 3 *essentially* transcends its physical encoding, that part of the world in which it is materialised. A new theorem, for instance, exists in World 3 *prior to its discovery*; problems which are *yet to be discovered* are in World 3. So are theorems which are already implied by encoded World 3 products, but which have never been thought of. World 3 is for Popper a realm of meanings, objective contents and potentialities.

XIV

The only other contribution which deals with evolution or biology in an important way is that by J.W.N. Watkins. Watkins has worked with Popper longer and more intimately than any other contributor to the Schilpp volume. He became a student of Popper's at the London School of Economics at the end of World War II, shortly thereafter became a colleague teaching in Political Science, and in 1958 joined Popper's own department. He is now Popper's successor at the L.S.E.

Watkins's essay contains virtually no criticism of Popper; it is almost purely descriptive. And it is brilliant. It is the best brief comprehensive and integrated account of Popper's philosophy that I have read. Watkins has achieved his success by interpreting the problem of indeterminism as the central problem of Popper's philosophy, and organising his presentation around this key. In the course of his examination, biology and evolution play an important role. However, since the key to Watkins's discussion is indeterminism *in physics*, I shall postpone a discussion of his paper to my second instalment, which is explicitly devoted to this subject.

California State University
Hayward, California 94542
USA

NOTES

1 See W.W. Bartley, III: 'Die österreichische Schulreform als die Wiege der modernen Philosophie," in *Club Voltaire: Jahrbuch für kritische Aufklärung* (Hamburg: Rowohlt Verlag paperback; 1970); W.W. Bartley, III: "Theory of Language and Philosophy of Science as Instruments of Educational Reform: Wittgenstein and Popper as Austrian Schoolteachers," in *Methodological and Historical Essays in the Natural and Social Sciences*, Boston Studies in the Philosophy of Science, Vol. XIV, ed. Robert Cohen and Marx Wartofsky (Dordrecht-Holland: D. Reidel Publishing Company; 1974); and W.W. Bartley, III: *Wittgenstein* (New York: J.B. Lippincott; 1973, and London: Quartet Books Limited; 1974).

2 Anthony Quinton: "Sir Karl Popper: Knowledge as an Institution," *Encounter*, December 1973, pp. 33-37.

3 An extended use of the word "knowledge" is no doubt involved here. What is meant is that "any process providing a stored program for organismic adaptation in external environments is included as a knowledge process, and any gain in the adequacy of such a program is regarded as a gain in knowledge." (Campbell, 1960, p. 380).

4 See Popper's review of Thure von Uexküll, and his remarks on Jacob von Uexküll, in *Conjectures and Refutations*, Chapter 20, pp. 380ff.

5 *Psychological Review*, 1960, Vol. 67, pp. 380-400. See also Donald T. Campbell: "Methodological Suggestions from a Comparative Psychology of Knowledge Processes," *Inquiry*, Vol. 2, August 1959, pp. 152-182; and Donald T. Campbell: "Perception as Substitute Trial and Error," in *Psychological Review*, 1956, Vol. 63, pp. 330-342. This early work of Campbell does not mention Popper. Campbell appears to have heard of Popper only in the early 1960's, at which time he became a fan: he was present in 1963 when Popper was informed by Schilpp that he was to be celebrated in the Schilpp series. It is interesting that in 1960, several years before Popper began to speak of "objective knowledge", Campbell had adopted an explicitly objectivist approach.

6 Konrad Lorenz: "Kant's Lehre vom apriorischen im Lichte gegenwärtiger Biologie," *Blätter für Deutsche Philosophie*, 1941, 15, 94-125; translated into English as "Kant's Doctrine of the A Priori in the Light of Contemporary Biology," L. von Bertalanffy and A. Rapoport, eds.: *General Systems, Yearbook of the Society for General Systems Research*, Vol. VII, pp. 23-35.
See also Konrad Lorenz: "Gestaltwahrnehmung als Quelle Wissenschaftlicher Erkenntnis," *Zeitschrift für experimentelle und angewandte Psychologie*, 1959, 6, 118-165; translated into English as "Gestalt Perception as Fundamental to Scientific Knowledge," in L. von Bertalanffy and A. Rapoport, op. cit., Vol. VII. This latter paper was dedicated to Karl Bühler on his eightieth birthday.
Campbell brought both these papers to my attention in 1963, and I was struck then and later by the large measure of agreement between Popper and Lorenz, and also by the similarity of their terminology and metaphors. Lorenz and Popper knew each other as boys, and both studied with Bühler in Vienna. It is remarkable also that Lorenz and Popper, for largely similar reasons, see modern physics *and* evolution as forcing one into hypothetical realism.

7 See in this connexion Popper's picture of the 'abstract society' in *The Open Society and Its Enemies*, Vol. I, Chapter 10, pp. 174ff.

8 This example is presented in J.Y. Lettvin, H.R. Maturana, W.S. McCulloch, and W.H. Pitts: "What the Frog's Eye Tells the Frog's Brain," *Proceedings of the Institute of Radio Engineers*, 47, 1959, pp. 140-151. Reprinted in Warren S. McCullock: *Embodiments of Mind* (Cambridge, Mass.: The M.I.T. Press; 1965), pp. 230-255. See also H.R. Maturana: "Biology of Cognition," B.C.L. Report 9.0, Biological Computer Laboratory, Department of Electrical Engineering, University of Illinois.

9 There is, for example, no adequate explanation of the extraordinary migratory feats of certain birds. Night warblers reared in captivity are able to orient to the star patterns in the night sky. Such warblers have been flown in closed boxes from Germany to South-West Africa and immediately are able to orient when they encounter their new night-sky environment. Such birds seem to possess a genetically determined ability to read star patterns and steer by them, and to have some sort of time sense (the star patterns shift continually with the earth's rotation). Thus the bird appears to have a "built in" planetarium, sextant, chronometer and altimeter. With the aid of something analogous to such instruments, it is able to read and compensate for the movements of the stars around the north star, or the southern cross, rather in the way in which a trained navigator can do this. See for details Donald R. Griffin: *Bird Migration* (New York: Anchor Books; 1964), and R.M. Lockley: *Animal Navigation* (London: Pan Books; 1967).

10 C.H. Waddington: *The Listener*, 13 November 1952.

11 Julian Huxley in *Evolution as a Process*, ed. Julian Huxley, A.C. Hardy, and E.B. Ford (London: George Allen & Unwin Ltd.; 1954), p. 14.

12 Behavioural selection is not the only source of internal selective pressure serving to mitigate – while actually following – random or blind selection. Campbell lists numerous others including achieved wisdom which limits the range of trials, maladaptive restriction on the range of trials, and vicarious selection – as outlined above.

13 R.F. Ewer: "Natural Selection and Neoteny," in *Acta Biotheoretica*, Leiden 1960. Quoted in Alister Hardy: *The Living Stream* (New York: Harper and Row; 1965), p. 187.

14 C.H. Waddington, discussion remark, in *Beyond Reductionism*, ed. Arthur Koestler and J.R. Smythies (London: Hutchinson; 1969), p. 387.

15 Sir Alister Hardy: *The Living Stream* (New York: Harper and Row; 1965).

16 The evidence is of course inferior to that available for anatomical change. *For behaviour does not fossilise*, and it is therefore rarely possible to demonstrate that changes in habit preceded structural change in any particular instance. Ewer's studies of the African Suidae and other pigs nonetheless provide good evidence for the general contention. See Ewer, op. cit.

17 Erwin Schrödinger: *Mind and Matter* (Cambridge: Cambridge University Press; 1959), delivered as the Tarner Lectures at Cambridge in 1956.

18 I am informed by Mr. Jeremy Shearmur, Sir Karl's research assistant, that Popper has inserted a reference to Hardy's book in a new impression of *Objective Knowledge* and also into the next edition of the Schilpp volume.

Company; 1968) *Psychological Issues* (New York: International Universities Press; 1959); *Young Man Luther* (New York: W.W. Norton & Company; 1958); *Gandhi's Truth* (New York: W.W. Norton & Company; 1969); *Life History and the Historical Moment* (W.W. Norton & Company; 1975).

16 See James Luther Adams's comments on some of Michels's ideas in "Tillich's Concept of the Protestant Era," printed in *The Protestant Era* (Chicago: University of Chicago Press; 1948), p. 279.

17 Karl R. Popper: "Back to the Pre-Socratics," Presidential Address, *Proceedings of the Aristotelian Society*, 1958-9. See also "Towards a Rational Theory of Tradition," op. cit., and "the Nature of Philosophical Problems and their Roots in Science," *British Journal for the Philosophy of Science*, August 1952, pp. 124–56. All three essays are reprinted in *Conjectures and Refutations*. See also note 38 to Chapter 10 of *The Open Society and Its Enemies*. See also W.I. Matson: "Conford on the Birth of Metaphysics," *Review of Metaphysics*, 8, 3, March 1955, pp. 443–54.

18 See Georg Feuerstein: *The Essence of Yoga* (New York: The Grove Press; 1974), p. 25, and E. Conze: Buddhist Thought in India (London, 1962), p 19, and H. Beckh: *Buddha und seine Lehre* (Stuttgart, 1956), p. 138.

19 Oliver Wendell Holmes, Jr.: "Natural Law," in *Collected Legal Papers* (New York: Harcourt, Brace; 1920), pp. 310–11. See the discussion of Holmes in Morton White: *Religion, Politics, and the Higher Learning* (Cambridge: Harvard University Press; 1959), p. 130–1.

20 Jean-Paul Sartre: *Being and Nothingness* (New York: Washington Square Press; 1975), Part Four. These matters are treated, in a different setting, in my book: W.W. Bartley, III: *Werner Erhard: The Transformation of a Man, the Founding of est* (New York: Clarkson N. Potter, Inc.; 1978), Chapter 10.

21 See Hermann Keyserling: *Travel Diary of a Philosopher* (New York: Harcourt, Brace, and Co.; 1925), Vol. I, p. 16, and vol. II, pp. 366–7. See also Hermann Hesse: *My Belief* (New York: Farrar, Straus & Giroux; 1974), pp. 367-8.

22 Yet Popper is also no "Freischwebender," no unattached and "freely poised intelligence" in the sense of Karl Mannheim, whose book *Ideology and Utopia* (1936), Popper rather harshly attacks in Chapter 23 of *The Open Society and Its Enemies*.

23 See David E. Roberts: "Tillich's Doctrine of Man," in *The Theology of Paul Tillich*, ed. Kegley and Bretall (New York: Macmillan; 1956), p. 110. "Everyone must take a stand somewhere. . . no matter what center is chosen, it cannot be objectively demonstrated -- partly because it is the expression of ultimate concern, and partly because all value-arguments presuppose its acknowledgement before they can have any point."

24 *Outlines of Pyrrhonism* (Cambridge: Harvard University Press; 1955). See Book II, Chapter IV, 20, pp. 163–165.

25 Michael Polanyi: *The Logic of Liberty* (London: Routledge and Kegan Paul, Ltd.; 1951), p. 106.

26 Published in *The Harvard Crimson*, 22 October 1969; reprinted in *Encounter* in Sidney Hook: "The Ideology of Violence," *Encounter*, April 1970, p. 30. Italics mine.

27 The word "Sketch" is meant literally. The present section is a rational reconstruction of the problem situation. An adequate treatment would require a rewriting of the history of philosophy in terms of the emphases, doctrines, and problems on which I focus here. If my argument is correct, just such a rewriting is badly needed.

28 See W.P. Alston and Richard B. Brandt: *The Problems of Philosophy*, 3rd edition, 1978, p. 605. In the new *Fontana/Harper Dictionary of Modern Thought*, rationalism as contrasted with irrationalism is defined as denying "the acceptability of beliefs founded on anything but experience and reasoning, deductive or inductive."

29 Or as W.K. Clifford put it: "It is wrong everywhere and for anyone, to believe anything upon insufficient evidence." See "Ethics of Belief," in *The Contemporary Review*, 1876. Carnap puts the matter very clearly: "This requirement for justification and conclusive foundation of each thesis will eliminate all speculative and poetic work from philosophy. . . . It must be possible for each scientific thesis . . . The decisive factor is . . . that for the *justification* of a thesis the physicist does not cite irrational factors, but gives a purely empirical-rational justification. We demand the same from ourselves in our philosophical work." See *The Logical Structure of the World* (Berkeley: University of California; 1967), p. xvii.

30 T.H. Huxley: "Agnosticism and Christianity," *Selections from the Essays of Thomas Henry Huxley* (New York: F.S. Crofts; 1948), p. 92.

31 I am using the word "intellectualist" in the sense given it by Kant in *The Critique of Pure Reason*, final chapter on "The History of Pure Reason."

32 Descartes was expressly trying to do this. See his remarks about scepticism in *Discours de la Methode*, in *Oeuvres*, ed. Adam et Tannery (Paris: Leopold Cerf; 1879-1913), Vol VI, p. 32; and *Objectiones Septimae*, in *Oeuvres*, Vol. VII, p. 550. On Descartes see W.W. Bartley, III: "Approaches to Science and Scepticism," *The Philosophical Forum*, Spring 1969, pp. 318-331.

33 Anticipated in part, incidentally, by several other philosophers, including Jonathan Edwards. See "The Insufficiency of Reason as a Substitute for Revelation," Chapter VII of his *Miscellaneous Observations on Important Theological Subjects*.

34 It was also, less importantly and in different respects, too wide, just as intellectualism was also too narrow. The terms "too narrow" and "too wide," in this context, were introduced by Popper. See his "Demarcation between Science and Metaphysics," in *Conjectures and Refutations*.

[35] *A History of Western Philosophy* (New York: Simon and Schuster; 1945), p. 673.

[36] See Popper: *The Open Society*, Chapter 24.

[37] The common philosophical argument to the effect that one cannot persuade a man to be moral unless he is moral, or persuade a man to be logical with logical arguments unless he accepts logic, etc., are clumsy applications of the more general and correct point that one cannot argue a man into a position, including the position of listening to argument, unless he has accepted that argument counts. If both morality and immorality are arguable positions, then one can argue a man into either *if* he accepts that argument counts. For examples of this sort of reasoning see Aristotle: *Nichomachean Ethics*, Book I, section iv, and Book X, section ix; F.H. Bradley: "Why Should I Be Moral?", in *Ethical Studies*, Essay II, second edition (Oxford: Oxford University Press; 1927); H.A. Prichard: "Does Moral Philosophy Rest on a Mistake?" in *Mind*, N.S., Vol. 21, 1912, and in *Moral Obligation* (Oxford: Oxford University Press; 1949). Even the statement that one cannot argue a man into a position unless he has accepted that argument counts, however, is unsatisfactory. It is, a bit, verbal; and it is more concerned with the question of the *source* of the decision to adopt a particular position or way of life than with the more important question whether that decision and position are open to criticism. (And such questions of source blend all too readily into justificationist arguments.) Thus when one is concerned with the question of whether a decision is criticizable, it hardly matters whether that decision was originally made as a result of argument, or whether the individual in question just stumbled into it, or whether he or she decided by tossing yarrow stalks, or by some other arbitrary method. Even if the rationalist position had originally been adopted as a result of an irrational arbitrary decision, it is possible that the person who made the choice would, by living in accordance with rationalist traditions and precepts, gradually become very rational, very open to criticism, *as an unintended consequence* of his original choice. (See in this connection my: "Ein schwieriger Mensch: Eine Porträtskizze von Sir Karl Popper," in Eckhard Nordhofen, ed.: *Philosophen des 20. Jahrhunderts in Portraits*. Königstein, Athenaum Verlag, 1980). So I would suggest that important choices, such as those of philosophical positions and ways of life – even of the rationalist way of life – are very often not the result of argument, any more than scientific theories are the result of sense observation. Theories are put forward; choices are made. The question of the sources of the theories and choices is not so important. The question, rather, is whether such theories and choices are open to criticism. If they are, then they are *held* rationally, even if they were not originally *made* rationally as, for instance, the result or conclusion of an argument.

[38] *Open Society and Its Enemies*, Princeton edition of 1950, p. 416.

[39] George Santayana: *Winds of Doctrine* (New York: Harper; 1951). p. 40.

[40] Isaiah Berlin, in his Hermon Ould Memorial Lecture, "Tolstoy and Enlightenment," as reported in *The Times Literary Supplement*, November 25, 1960, p. 759.

[41] H.D. Aiken: *Age of Ideology* (New York: Mentor Books; 1956), p. 272.

[42] W.W. Bartley, III: *Wittgenstein* (New York: J.B. Lippincott & Co.; 1973), pp. 167–178; (London: Quartet Books Ltd.; 1974, 1977), pp. 120–30.

[43] A.J. Ayer: *The Problem of Knowledge* (London: Penguin Books; 1956).

[44] Karl Barth: *Evangelical Theology* (New York: Holt, Rinehart and Winston; 1963), p. 131. The italics are Barth's. See my discussions of Barth in W.W. Bartley, III: "Karl Barth: 'The Last of the Protestants,'" *Encounter*, March 1970; and W.W. Bartley, III: *Morality and Religion*, *op. cit.*, Chapter Three.

[45] Compare Putnam's "The Analytic and the Synthetic," in *Minnesota Studies in the Philosophy of Science*, ed. Herbert Feigl and Grover Maxwell (Minneapolis: University of Minnesota Press; 1962), pp. 358–397, where Putnam manages to combine circularity with an argument from authority: "Does the fact that everyone accepts a statement make it rational to go on believing it? The answer is that it *does*, if it can be *shown* that it would be reasonable to render the statement immune from revision by stipulation, *if* we were to formalize our language." See also Putnam: *Meaning and the Moral Sciences* (London: Routledge & Kegan Paul; 1978).

[46] See *The Open Society and Its Enemies*, Chapter 24, first, second and third editions. In Princeton edition (1950), see pp. 416–7. The point is repeated in "Utopia and Violence" (1948), reprinted in *Conjectures and Refutations*, p. 357. One finds a similar attitude in Sidney Hook, who writes: "To be reasonable is to be absolute about nothing except being reasonable." See his *The Paradoxes of Freedom* (Berkeley: University of California Press; 1962), p. 15.

[47] Princeton edition, 1950, p. 431.

[48] *Die beiden Grundprobleme* (Tübingen: J.C.B. Mohr (Paul Siebeck) Verlag; 1979).

It is interesting that when Popper turns deliberately to deal with the irrationalist (i.e., one who challenges the appeal to reason to settle disputes), as in Chapter 24 of *The Open Society*, he does not engage those persons who possess an argument, a "rational excuse against rationalism." Rather, he engages those who despise reason altogether, who are willing to *shoot* those who attempt to argue with them. Whereas, the sort of irrationalist with whom I am chiefly concerned in this study, is one who attempts to reply to arguments against irrationalism with *arguments* to show that the rationalist position is defective on its own terms and that rationalism suffers from those very defects which it ascribes to irrationalism. The second type of irrationalist is the stronger and the more worth debating: since a rationalist is one who aims to be moved intellectually only by arguments even if he is compelled by force to act physically

contrary to his views, a putative argument against his rationalism is far more of a threat than is force against it.

[49] At this time he rewrote the first chapter of the unpublished *Postscript*, reporting my work there. In his intellectual autobiography (Schilpp volume, p. 119), Popper makes a statement relating to this which could be amplified: "In this *Postscript* I reviewed and developed the main problems and solutions discussed in *Logik der Forschung*. For example, I stressed that I had rejected all attempts at the justification of theories and that I had replaced justification by criticism." The *Postscript* will be published in 1982, in three volumes edited by me, as follows: *Realism and the Aim of Science*, *The Open Universe;* and *Quantum Theory and the Schism in Physics* (London: Hutchinson; and New Jersey: Rowman & Littlefield).

[50] See "On Reason and the Open Society," *Encounter*, May 1972, p. 18.

[51] K.R. Popper: "On the Sources of Knowledge and of Ignorance," 1960, reprinted in *Conjectures and Refutations* (1962); *Logik der Forschung* (1934); sections 36 and 83. W.W. Bartley, III: "Limits of Rationality" (1962), *The Retreat to Commitment* (1962), and "Rationality versus the Theory of Rationality" (1964). All cited above. See also W.W. Bartley, III: "A Note on Barker's Discussion of Popper's Theory of Corroboration," *Philosophical Studies*, January-February 1961, pp. 5–10.

[52] *Enquiry Concerning Human Understanding*, Section II.

[53] John T. Kearns: "A Semantics Based on Justification rather than Truth," (Abstract), *The Journal of Symbolic Logic*, 43, 3, September 1978, p. 614.

[54] Several philosophers of science, including Carnap, Hempel, and Goodman have argued the place of a similar assumption in theories of confirmation in the natural sciences, referring to this assumption by names like "consequence condition," "entailment condition," and "content condition." My remarks here are not intended to apply only to scientific matters. For an example of the misunderstandings created by applying the transmissibility assumption or consequence condition to Popper's thought, see my "Note on Barker," as cited in Note 51.

[55] Adolf Grünbaum: "Falsifiability and Rationality," Mimeographed. Read at International Colloquium on Issues in Contemporary Physics and Philosophy of Science, September 1971; See also Grünbaum's: "Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper versus Inductivism," in R.S. Cohen, P.F. Feyerabend, and M.W. Wartofsky: *Essays in Memory of Imre Lakatos* (Dordrecht: D. Reidel Publishing Company; 1976), pp. 213–252; "Can A Theory Answer More Questions Than One of Its Rivals?" *The British Journal for the Philosophy of Science*, March 1976, pp. 1–23; "Is the Method of Bold Conjectures and Attempted Refutations *Justifiably* the Method of Science?", *British Journal for the Philosophy of Science*, June 1976, pp. 105–136; and "Ad Hoc Auxiliary Hypotheses and Falsificationism," *British Journal for the Philosophy of Science*, December 1976, pp. 329–362.

[56] That there should have been any doubt whether degree of testability is transmissible is odd. For the idea of logically deriving one statement from another not identical to it involves the notion that various statements differ in logical strength. Yet the statement which is stronger is *ipso facto* more testable — which means that its degree of testability is no more transmissible to its implicates than is its logical strength.

[57] Popper has on occasion been careless in expressing this. Thus in *Conjectures and Refutations*, p.279, note 63, he writes "confirmed" (meaning "corroborated") where he should have written "confirmable" or corroborable."

[58] See *Objective Knowledge*, op. cit., pp. 1–31; originally published in *Revue internationale de philosophie*, 95–6, 1971, fasc. 1–2. Another example of Lakatos's poor scholarship appears on p. 270 of his article, where he alleges that the "positive" solution of the problem of induction which Popper proposes in a new addendum to the third German edition of *Logik der Forschung* (1969) was written *in response to* Lakatos's article, "Changes in the Problem of Inductive Logic" (I. Lakatos, ed.: *The Problem of Inductive Logic*. Amsterdam: North-Holland Publishing Co.; 1968, pp. 315–417). In fact, Popper's positive solution appears in additions made in 1961 to the as yet unpublished *Postscript to the Logic of Scientific Discovery*.

[59] Quoted in translation from Nietzsche's Musarionausgabe by Walter Kaufmann in his *Critique of Religion and Philosophy* (New York: Harper & Co.; 1958), p. vii.

[60] See Charles Darwin: *Autobiography*, p. 123. See also W.W. Bartley, III: "What Was Wrong with Darwin?", *The New York Review of Books*, September 15, 1977.

[61] See A.A. Derksen: "The Failure of Comprehensively Critical Rationalism," *Philosophy of the Social Sciences*, March 1980, and my reply: "On the Criticisability of Logic," in the same issue. See also my "On Alleged Paradoxes in Pancritical Rationalism," Appendix 4 to the new edition, revised, expanded, and newly translated, of *Flucht ins Engagement* (J.C.B. Mohr (Paul Siebeck) Verlag; forthcoming).

[62] Agatha Christie: *Death in the Air* (New York: 1977).

[63] Jacob Friedrich Fries: *Neue oder anthropologische Kritik der Vernunft* Heidelberg: Christian Friedrich Winter; 1828 (vol. I), 1831 (vols. II and III).

[64] Whether Fries's views are rightly called psychologistic is a matter of some dispute. Leonard Nelson writes scornfully of those who "have preferred not to study his critique of reason but to parrot the traditional fable of Fries's 'psychologism.'" See Leonard Nelson: *Socratic Method and Critical Philosophy* (New Haven: Yale University Press; 1949), p. 156.

[65] In *Die beiden Grundprobleme der Erkenntnistheorie*, pp. 131–2, Popper introduced the idea of problematicality there in the sense in which I am using it here. This makes it the more puzzling that he nonetheless thinks

that something is added by requiring a decision concerning unproblematical cases – indeed he thinks that such a decision is necessary.

66 I find that my objection to Popper's views here is in general compatible with Joseph Agassi's remarks in "Sensationalism," *Mind*, 75, 1966, pp. 1–24, and in "When Should We Ignore Evidence in Favour of a Hypothesis?", *Ratio*, 15, 1973, pp. 183–205; both now reprinted in his *Science and Flux* (Dordrecht: D. Reidel; 1975), pp. 92–151. But Agassi seems to think that the key task then becomes to *explain* observation reports. Whether observation reports need to be explained will depend on circumstances and vary from one test situation to another – depending in part on whether the explanation of the observation report contributes to the explanation of the world.

67 See J.O. Wisdom: "The Refutability of 'Irrefutable Laws'", *British Journal for the Philosophy of Science*, 1963, pp. 303–6; J.O. Wisdom: "Refutation by Observation and Refutation by Theory," in I. Lakatos and A. Musgrave, eds.: *Problems in the Philosophy of Science* (Amsterdam: North-Holland Publishing Company; 1968), pp. 65–7. See also J.W.N. Watkins: "Confirmable and Influential Metaphysics," *Mind*, 1958, 345–7; "Between Analytic and Empirical," *Philosophy*, 1957; and "When Are Statements Empirical?" *British Journal for the Philosophy of Science*, February 1960. See also W.W. Bartley, III: *The Retreat to Commitment*, op. cit., pp. 85 and 159; and W.W. Bartley, III: "Reply to J.O. Wisdom," in *Problems in the Philosophy of Science*, op. cit., pp. 108–9. Joseph Agassi has drawn particular attention to the closely related question of the ways in which metaphysical theories, because of their possible conflict with scientific hypotheses, can guide and prejudice scientific research by acting as regulative principles. See his "The Nature of Scientific Problems and Their Roots in Metaphysics," in Mario Bunge, ed.: *The Critical Approach to Science and Philosophy*, op. cit. See also Rudolf Carnap: "Testability and Meaning," *Philosophy of Science*, October 1936 and January 1937. Watkins interestingly modifies his position in: "Metaphysics and the Advancement of Science," *British Journal for the Philosophy of Science*, June 1975, pp. 91–121, and in "Minimal Presuppositions and Maximal Metaphysics," *Mind*, April 1978, pp. 195–209.

68 Goodman claims that "Wolfgang Stegmüller has corrected the notion that 'anti-inductivists' of the school of Karl Popper escape the new riddle of induction." Yet curiously Goodman does not give any reference to any such work of Stegmüller in which any such "correction" takes place. See Nelson Goodman: *Fact, Fiction and Forecast* (Cambridge: Harvard University Press; 1955, new edition, 1973), pp. 74ff. I suppose that Goodman may be referring to Stegmüller's *Collected Papers on Epistemology* (Boston: D. Reidel; 1977), Vol. II, pp. 92–4. This discussion by Stegmuller shows a complete misunderstanding of Popper's position and presents no argument. See W.W. Bartley, III: "Goodman's Paradox:

A Simple-Minded Solution," in *Philosophical Studies*, December 1968, pp. 85–8; and W.W. Bartley, III: "Theories of Demarcation between Science and Metaphysics," in I. Lakatos and A. Musgrave, eds.: *Problems in the Philosophy of Science*, op. cit., pp. 40–119; and "Eine Lösung des Goodman-Paradoxons," in G. Radnitzky and G. Anderson, eds.: *Voraussetzungen und Grenzen der Wissenschaft* (Tübingen: J.C.B. Mohr (Paul Siebeck) Verlag; 1980), pp. 347–358. James N. Hullett attempts a reply to my solution in his "Discussion: On a Simple-Minded Solution," *Philosophy of Science*, September 1970, pp. 452–4, but misses the point. See also Joseph Agassi's comments on Popper and Goodman in *Science and Flux*, op. cit., pp. 165–7, 236–8, and 351.

The difference between a Popperian and non-Popperian approach to Goodman's paradox has most recently been taken up in an exchange between J.W.N. Watkins and Kurt Hübner in Gerard Radnitzky and Gunnar Andersson, eds.: *Progress and Rationality in Science* (Dordrecht: D. Reidel; 1978). Watkins misses the point of the paradox (as Hübner notices, pp. 280–81 and pp. 394–5); and Hübner contends that the paradox cannot be avoided from within a Popperian framework.

69 I owe the example of young emeralds and old diamonds to a conversation with Joseph Agassi in 1967.

70 Sir A.S. Eddington: *Space, Time and Gravitation: An Outline of the General Relativity Theory* (Cambridge: Cambridge University Press; 1920), p. 113.

71 See Sir Karl Popper: "The Aim of Science," in *Objective Knowledge*, op. cit., pp. 191–205, esp. pp. 198–203; and "Two Faces of Common Sense," in *Objective Knowledge*, pp. 102–3.

72 I owe the remark that Goodman's theory provides a routine for drawing up an infinite sequence of corroborated and competing theories to personal conversations and correspondence with Sir Karl Popper in 1978. See also Popper's new introduction, footnote 11, to *Die beiden Grundprobleme der Erkenntnistheorie*, op. cit, pp. xix–xx.

73 C.I. Lewis: "A Pragmatic Conception of the A Priori," *Journal of Philosophy*, 20, 1924; and "Logic and Pragmatism," *Contemporary American Philosophy*, Vol. II (New York: The Macmillan Company; 1930), pp. 31–51. See also Morton White: *Age of Analysis* (New York: Mentor Books; 1955), Chapter XI, p. 175.

74 See W.W. Bartley, III: *Lewis Carroll's Symbolic Logic* (New York: Clarkson N. Potter, Inc.; 1977), pp. 16–19.

75 See W.V. Quine: *Word and Object* (New York: The Technology Press of the Massachusetts Institute of Technology; 1960), p. 59. See W.V. Quine: "Two Dogmas of Empiricism," in *From a Logical Point of View* (Cambridge: Harvard University Press; 1953).

76 K.R. Popper: "New Foundations for Logic," *Mind*, Vol. 56, 1947; and "Corrections" in *Mind*, Vol. 57, 1948, pp. 69ff.; "Logic without Assumptions," *Proceedings of the Aristotelian Society*, 1947, Vol. 47, pp. 251–292; "Functional Logic without Axioms or Primitive Rules of Inference,"

Proceedings Koninklijke Nederlandsche Akademie van Wetenschappen, Vol. 50, No. 9, 1947, pp. 1214ff.; "On the Theory of Deduction: Part I, Derivation and Its Generalizations," *Proceedings Koninklijke Nederlandsche Akademie von Wetenschappen*, Vol. 51, No. 2, 1947, pp. 173ff.; "On the Theory of Deduction: Part II, The Definitions of Classical and Intuitionist Negation," ibid., No. 3, 1947, pp. 322ff.; "The Trivialization of Mathematical Logic," *Proceedings of the Xth International Congress of Philosophy*, Amsterdam, 1948, Vol. I, pp. 722ff.; "Why Are the Calculi of Logic and Arithmetic Applicable to Reality?", *Conjectures and Refutations*, pp. 201–214. See also J.O. Wisdom: "Overlooked Aspects of Popper's Contributions to Philosophy, Logic, and Scientific Method," in *The Critical Approach*, ed. Mario Bunge, op. cit., pp. 116–124. See William Kneale: "The Province of Logic," in H.D. Lewis, ed.: *Contemporary British Philosophers*, 3rd Series (London: George Allen & Unwin Ltd.; 1956), pp. 237–261. See G. Gentzen: "Untersuchungen über das logische Schliessen," in *Mathematische Zeitschrift*, Vol. 39, 1934, pp. 176–210, and 405–31.

77 Alfred Tarski: "On the Concept of Logical Consequence," chapter 16 of *Logic, Semantics, Metamathematics* (Oxford: Oxford University Press; 1956).

78 See review by H.B. Curry in *Mathematical Reviews*, Vol. 9, no. 7. July-August 1948, p. 321, and reviews in *Journal of Symbolic Logic*, vols. 13 and 14.

79 W.C. Kneale: "The Province of Logic," in H.D. Lewis, ed.: *Contemporary British Philosophy*, Third Series (London: George Allen and Unwin, Ltd.; 1956), p. 256; and W.C. and Mary Kneale: *The Development of Logic* (Oxford: Oxford University Press; 1962), p. 563. See also Bruce Brooke-Wavell: "A Generalization of the Truth-Table Method," Unpublished Ph.D. dissertation, University of London, 1958. Brooke-Wavell argues that "deducibility" is definable in terms of general tabular predicates. Together with Popper's derivation of logic from the single undefined concept of "deducibility," this would yield the derivability of logic from basic tabular rules supported by the truth-table algebra. See also J.O. Wisdom: "Overlooked Aspects of Popper's Contributions to Philosophy, Logic, and Scientific Method," in Mario Bunge, ed.: *The Critical Approach to Science and Philosophy*, op. cit., pp. 116–124.

80 Quoted in Schilpp volume, p. 1096.

81 See my exchange with Derksen: *Philosophy of the Social Sciences*, March 1980, op. cit. See also Alfred Tarski: "The Semantic Conception of Truth and the Foundations of Semantics," *Philosophy and Phenomenological Research*, IV, 1944, reprinted in *Readings in Philosophical Analysis*, ed. Herbert Feigl and Wilfrid Sellars (New York: Appleton-Century-Crofts, 1949), p. 59, where Tarski suggests that the "ordinary laws of logic" must be retained in the presence of the liar paradox.

82 In saying this I of course do not mean that we must hang on to some particular object-linguistic form of the law of noncontradiction: e.g.

(p.p.). One would, however, as Hans Lenk suggests, need to maintain a functional equivalent or analogue, which might be achieved object-linguistically with Sheffer's stroke or Pierce's operator, or metalinguistically with predications of truth and falsity, or in a variety of other ways.

83 L.E.J. Brouwer: "Mathematik, Wissenschaft und Sprache," *Monatshefte fur Mathematik und Physik*, Vol. 36, 1929, pp. 153–64; and "Consciousness, Philosophy, and Mathematics," in *Proceedings of the Tenth International Congress of Philosophy*, Vol. I (Amsterdam: North-Holland Publishing Co.; 1949), pp. 1235–1249. The interpretation of Brouwer given here was presented in my "Limits of Rationality: Critical Study of Some Logical Problems of Contemporary Pragmatism and Related Movements," op. cit., pp. 304–9. Popper includes a closely similar interpretation of Brouwer in *Objective Knowledge*, op. cit., pp. 139–40.

84 One might informally develop a number of related distinctions: there would be *argument for, argument against, argument about the truth of, argument about the conditions under which a particular statement would be false. Argument about*, as meant here, requires basic Logic I with retransmission of falsity. In *argument for*, on the other hand, one might have almost any transmission rules: one might have transmission of truth, for instance, without retransmission of falsity.

85 See W.W. Bartley, III: *The Retreat to Commitment*, op. cit., p. 173.

86 See W.W. Bartley, III: "Limits of Rationality," op. cit.

87 See K.R. Popper: *The Open Society and Its Enemies*, chapter 11.

88 On the other hand, Quine links the problem of the analytic-synthetic with an untenable empiricist distinction between logical or formative signs and descriptive signs, a distinction in terms of which descriptive signs gain meaning only on the basis of observation or sense data. Such a view requires a sharp and definite distinction between formative and descriptive signs; and if the view is abandoned, as Quine thinks it should be, it seems that the importance of the analytic-synthetic distinction diminishes too. But if descriptive words do not acquire meaning in this way, if they are *all* theoretical, all theory-impregnated, as Popper maintains, then they may or may not be used in a conventionalist way depending upon whether we wish to stick to the theories in question or to subject them to severe test. Viewed in this way, the problem does not even arise in the way suggested by Quine.

89 See my exchange with Derksen, op. cit. See also Hans Lenk: "Philosophische Logikbegründung und Rationaler Kritizismus," in *Zeitschrift fur Philosophische Forschung*, Vol. 24, 1970, pp. 183–205.

90 Jonathan Bennett: "Analytic-Synthetic," *Proceedings of the Aristotelian Society*, 1958–9. See also Bennett: "On Being Forced to a Conclusion," *Aristotelian Society Supplementary Volume*, 35, 1961. Interestingly, in the first paper Bennett cites Carroll's story of Achilles and the Tortoise in order to support his argument. See my critique of Carroll's tale, and of its use in this connection, in my: "Achilles, the Tortoise, and Explanation in Science and History," in *British Journal for the Philosophy of*

Science, 1963. See also W.W. Bartley, III: *Lewis Carroll's Symbolic Logic*, op. cit., pp. 466–470.

[91] It is interesting that Quine basically accepted Bennett's *exposition* of his position. See his reference to Bennett's work in *Word and Object*, p.68n. Another follower of Quine who writes in a way similar to Bennett is Morton White. In his *Toward Reunion in Philosophy* (Cambridge: Harvard University Press; 1956), p. 288, White maintains that an *a priori* statement is one that we believe quite firmly and therefore "make immune" to overthrow. "It is, in short, a sentence of which we say, 'This is pinned down. . . calling S an a priori statement is another way of uttering 'I accept' followed by S, followed by 'without attention to experience.'" Such an approach is deeply subjectivist, in the sense discussed by Popper in *Objective Knowledge*, Chapter 3. whereas the approach I took to presuppositions of argument was objectivist.

[92] When a philosopher attempts to defend some traditional notion, as does Bennett, with an argument that implies that the traditional notion *cannot* solve the problem it was intended to solve, what has probably happened is that the philosophical problem in question has been forgotten while the philosopher's attention has been diverted to a subordinate problem, one that is, as in this case, of a technical character, and only important philosophically in the broader connection. I am criticizing Bennet here in terms of what I have called the "check of the problem."

[93] David Hume: *An Enquiry Concerning Human Understanding*, section 12.

[94] See Nietzsche's untimely meditation on *Schopenhauer as Educator* in Walter Kaufmann's translation in *Existentialism from Dostoevsky to Sartre* (New York: Meridian Books; 1956).

[95] See my discussion of determinism in Part II of this series, and also in W.W. Bartley, III: *Werner Erhard*, op. cit., pp. 99–105. On the very important related thesis that no causal physical theory of the descriptive and argumentative functions of language is possible see K.R. Popper: "Language and the Body-Mind Problem," in *Conjectures and Refutations*, pp. 293–98. See also Popper's "Of Clocks and Clouds: An Approach to the Problem of Rationality and the Freedom of Man," in *Objective Knowledge*.

[96] For examples, see "On the Sources of Knowledge and of Ignorance, *Conjectures and Refutations*, pp. 3–30.

[97] For examples, see *The Open Society and Its Enemies*, Chapter 11, and *Unended Quest*, section 7.

[98] See *Unended Quest*, p. 27.

[99] See *Conjectures and Refutations*, p. 8.

[100] See *Conjectures and Refutations*, Chapter 4.

[101] See *Objective Knowledge*, Chapter 5.

[102] See *Objective Knowledge*, p. vii.

[103] See *Logic of Scientific Discovery*, pp. 80–82.

[104] Originally published in the *Annales de Philosophie Chrétienne*, 77th Year, 4th Series, I, (Oct. and Nov. 1905), and in *Revue de Métaphysique*

et de Morale, 12 (July 1904). Duhem's article is republished in English translation as an appendix to Duhem: *The Aim and Structure of Physical Theory* (Princeton: Princeton University Press; 1954).

[105] For Bellarmino's letter, see *Le Opere di Galileo Galilei*, XII, Barbera, Firenze, 1902. Item 1110*, pp. 171–172.

[106] *Conjectures and Refutations*, p. 254.

[107] *Logic of Scientific Discovery*, p. 55, n. 3.

[108] *Conjectures and Refutations*, p. 42.

[109] *Conjectures and Refutations*, p. 33.

[110] *Logic of Scientific Discovery*, p. 394. See also p. 108 and p. 415; and *Conjectures and Refutations*, p. 279.

[111] *Logic of Scientific Discovery*, p. 22.

[112] On such statements see J.W.N. Watkins: "Confirmable and Influential Metaphysics," *Mind*, July 1958, pp. 344–365; and J.W.N. Watkins: "Between Analytic and Empirical," *Philosophy*, April 1957, pp. 112–131.

[113] Popper's valuable critical comments on the various forms such strategies can take, and on how to avoid them, occasionally need some qualification. For instance, there is nothing intrinsically wrong about invoking a theory of the character of the theory of resistances. It may well be true that consciousness is so recent an aquisition of human nature, and still in so frail a state, that resistance may easily be aroused when the existence of an unconscious is argued. Pointing out this possibility need not necessarily be a criticism-reducing strategy: it is an important part of the psychological theories in question, and to discount it just because it can be misused would be itself uncritical.

[114] Popper's view of Freudian theory is corroborated by the recent investigations of two researchers who hold a generally favourable view of Freudian theory. Thus in *The Scientific Credibility of Freud's Theories and Therapy* (London: Harvester Press; 1977), p. 6, Seymour Fisher and Roger P. Greenberg write as follows: "The avoidance of questions of scientific validity has hurt the psychoanalytic enterprise by encouraging dogmatism. . . psychoanalytic theory has taken on cast-iron qualities. . . A false certainty is displayed in anticipation of inevitable and basically fair inquiries. . . The fact that the official psychoanalytic establishment has not admitted to any major faults in its theoretical structure for such a long time is a measure of its defensiveness. . . What changes have managed to occur have reflected the power status or persuasive fluency of individuals pleading their special views. . . There has also been an informal and unspoken attrition of concepts. Some of Freud's ideas. . . have for all practical purposes been ejected by the psychoanalytic establishment. . . without explicit testing of their validity or without a direct statement that they would no longer be given serious weight. . . the selection of what is to be considered valid or invalid has been left to a process whose nature is vague and really impossible to specify. . . has ultimately imparted a static quality to psychoanalytic writings. . . psy-

choanalysis has entrenched itself behind concepts that it is largely prepared to defend rather than lay on the line for scholarly scrutiny. Of course one should not too hastily ascribe these difficulties to the intrinsic difficulties of criticizing Freudian theory, but should also take into account that this theory was made the ideology of a profession which had a vested interest in it.

115 Thus Popper is wrong (*Logic of Scientific Discovery*, p. 84), in writing that "Only in the case of systems which would be falsifiable if treated in accordance with our rules of empirical method is there any need to guard against conventionalist stratagems."

116 While ethical theories are never empirically verifiable there are some theoretical contexts in which they conflict with synthetic scientific statements. This can occur for example in contexts where "'ought' implies 'can'" is agreed to apply to persons. In such situations ethical statements may be criticized by synthetic statements of physical impossibility. Yet it would be quixotic to call such norms scientific or synthetic. See my *Morality and Religion*, op. cit., Chapter 1; and my "The Reduction of Morality to Religion," *Journal of Philosophy*, October 22, 1970, pp. 755–768.

117 *Conjectures and Refutations*, p. 257; *The Logic of Scientific Discovery*, p. 314; *Die beiden Grundprobleme der Erkenntnistheorie*, p. 10.

118 *Logic of Scientific Discovery*, p. 37.

119 See Section XV, this paper. Popper's chief discussion of metaphysics appears in his unpublished *Postscript to the Logic of Scientific Discovery*. See also K.R. Popper: "Philosophy and Physics," in *Atti del XII Congresso Internazionale di Filosofia*, Venice, 1958, published 1960, Vol. 2, pp. 367–74. See W.W. Bartley, III: "Max Jammer on the Interaction between Science and Metaphysics," *Proceedings of the Seventh International Conference on the Unity of the Sciences* (New York, 1980). Joseph Agassi: "The Function of Interpretations in Physics," University of London Library, 1956. See also Agassi: "The Nature of Scientific Problems and their Roots in Metaphysics," in M. Bunge, ed.: *The Critical Approach*, op. cit.

120 The regulative impact of metaphysical theories on scientific hypotheses has often been illustrated. Tatarkiewicz, writing of Plotinus, has shown how such a metaphysical theory may also affect a different kind of representation: painting. Thus Plotinus's aesthetics was actually implemented in works of art which followed such principles as these: all that is the result of imperfection in the sense of sight must be avoided: e.g., diminution of size and fading of colour, deformation through perspective, alteration in appearance produced by light and shade. Things must be shown as the spectator sees them at close quarters, in the foreground, in full light, in particular colours and with all details clear. Such followed from Plotinus's theory that matter was mass and darkness, while the spirit was light, so that, to penetrate beyond matter to spirit, painting should avoid depth and shadow and present only the luminous surface of things. See

W. Tatarkiewicz: *History of Aesthetics*, Vol. I, ed. Jean G. Harrell (The Hague-Paris: Mouton-PWN (Polish Scientific Publishers, Warsaw); 1970), pp. 323–4.

121 See for instance Moritz Schlick: "The Turning Point in Philosophy," in *Logical Positivism*, ed. A.J. Ayer (London: George Allen & Unwin, Ltd.; 1959).

122 This makes it more understandable why positivists, even when they could accept the gist of Popper's objections, were not sufficiently satisfied with his "solution" to "their problem" to abandon their attempts to achieve a device – through meaning analysis – for a more radical sifting. The positivists stuck to meaning analysis in part because Popper's criterion is insufficient to get rid of long-winded nonsensical-appearing claims and other illegitimate theories. Popper showed that the specific kind of meaning analysis in which the early positivists engaged could not generally succeed, but he did not show that *no* metaphysical statements could be dealt with in such a way; Popper did not, for instance, show that *no* traditional metaphysical doctrines were analogous to category mistakes, only that *not all* were. It was, then, not unreasonable to suppose that *some* kind of meaning analysis might be useful in criticizing such views.

123 J.W.N. Watkins: "Confirmable and Influential Metaphysics," *Mind*, July 1958; "Between Analytic and Empirical," *Philosophy*, 1957; "The Haunted Universe," *The Listener*, Nov. 21 & 28, 1957; "Epistemology and Politics," *Proceedings of the Aristotelian Society*, 1957–8. See also his "Metaphysics and the Advancement of Science," *The British Journal for the Philosophy of Science*, June 1975; and "Minimal Presuppositions and Maximal Metaphysics," *Mind*, April 1978.

124 Now published in book form as Imre Lakatos: *Proofs and Refutations* (Cambridge: Cambridge University Press; 1976).

125 Adolf Grünbaum ignores Popper's reply, and repeats this worn old criticism in his "Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper versus Inductivism," op. cit.

126 Joseph Agassi: *Science in Flux*, op. cit., pp. 40–50. See also Agassi's "Corroboration versus Induction," in *British Journal for the Philosophy of Science*, Vol. 9, February 1959, pp. 311–317.

127 Imre Lakatos: *Mathematics, Science and Epistemology* (Cambridge: Cambridge University Press; 1978), p. 197n. See also Popper's reply to Lakatos on this point in Schilpp volume, p. 174 (note 226), and compare Popper on rational action, following Lakatos, in Schilpp p. 82.