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Introduction.

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Introduction

Though the papers included in this second volume were all written at later dates than those contained in the first, they are all still 'basic' in the sense that each can easily be understood by itself without any previous scientific education, particularly without any previous reading in ethology. The reader who, perhaps, is not interested in the philosophy of science, nor in the political inferences of ethology, should not be deterred by the fact that there was an ulterior motive guiding my choice of papers for the present volume: the aim is definitely different from that of the first volume, which was chiefly meant to give some idea of the lines along which ethology, as a young science, began to evolve. The present volume is intended to convey, to the interested reader, some understanding of the philosophy as well as of the theory of knowledge on which ethological approach is based. In fact, I believe that the same epistemological attitude underlies *all* truly scientific procedure. In particular, this collection aims at explaining the strategy of research imposed upon the scientific investigator by the nature of his object whenever the latter happens to be a complex system. Though the obligatory rules of this strategy have, long ago, been adequately formulated by Otto Koehler, Ludwig von Bertalanffy, Paul Weiss, Erich von Holst and others, it still seems to be unknown to many otherwise quite admirable scientists that a system can *not* be approached by operationalist methods, at least not before a considerable number of altogether different cognitive procedures have been successfully performed.

As D. S. Lehrman has aptly expressed in his contribution to *Essays in Memory of T. C. Schneirla,* the predominant opinion of the behaviourist school of American psychology is 'that questions about the processes internal to the subject that give rise to the observed behaviour are unnecessary, misleading, non-scientific and/or irrelevant to an inclusive system of behaviour analysis'. In other words, knowledge of the *machinery* of the living system, the behaviour of which we hope to understand and, if possible, to control, is deemed unnecessary to this purpose.

All the papers included in the present volume were written at a time when I had not yet fully realized how vastly different the behaviourists' attitude is, in its fundamental theory of knowledge, from that of all branches of natural science. Nevertheless I cherish the hope that these old papers explaining the strategy of research obligatory in analysing systems might suffice to make it clear to an unbiased reader why it is a vain hope ever to arrive at an understanding of behaviour without understanding the physiological machinery which causes it.

A little more, however, should be said, here in the introduction, about the behaviourists' way of thinking. Though indubitably copied from modern physics, the behaviourists' attempt to find a direct, lawful relationship between the experimenter's influence and its effects on the organism's behaviour, *without trying to analyse the causal chain leading from one to the other,* is strangely lacking in understanding of the nature of physics itself and of its relations to other natural sciences. All true science is 'physicalistic' in one sense. In all our daily work we act on the hypothesis that, if ever we should arrive at the ultimate and Utopian goal of completely understanding Nature, including our own nature, we should have explained the universe and everything in it on the basis of a number of very general laws of physics and chemistry, and *of the complex structures in which these laws are at work*. Our endeavour is to push analysis 'downwards' (that is, in the direction of more basic and more general laws) and, as investigators of behaviour, we hope ultimately to arrive at its understanding on the basis of the physico-chemical processes which take place in synapses, or at the charged cell membranes of ganglia and the like, not forgetting, of course, the immensely complicated structural systems into which synapses, ganglia and other elements are integrated. This, of course, is exactly the way in which the physicist, or, for that matter, any engineer would go about trying to analyse any complex system whose composition is unknown to him, as, for instance, a computer built by somebody else. No physicist or engineer in his right senses would, in order to get the hang of how this contraption works and how it can be used, programmatically restrict himself to studying the probabilistic relationships between input and output. He would instead take the thing to pieces and see how it is wired.

The analogous procedure, though dictated by common sense, is very far from the mind of true behaviourists. They hold the entirely unfounded belief that it is, indeed, possible to find laws which hold true for the behaviour of all animals, irrespective of whether they happen to be amoebae, pigeons, rats or men, irrespective of whether they are healthy or ill, and irrespective of the physiological processes on which their behaviour is based. They proceed as if there were no structures to be studied. They also proceed as if there were no such thing as evolution and phylogenetic adaptedness of form and function to the ubiquitous necessity of survival.

In other words, the behaviourist school treats the organism, and whatever goes on within it, as if it were as *unknowable,* as sub-atomic processes which transcend the forms of thought and of visualization with which we are endowed *a priori*. If, in this hazy borderland of that which is accessible to human knowledge, physicists resort to the exclusive application of operationalistic and probabilistic methods, they do so *only because they have to,* because our common sense and our everyday forms of thought (like causality and substantiality) as well as the corresponding forms of visualization (like space and time) cease to be applicable to that which is to be explained.

Let us suppose, by contrast, that physicists or technicians were misguided enough to apply behaviourist methods. Suppose that a team of extra-terrestrial physicists or technicians had arrived from Mars, and were, for some really unearthly reason, determined to apply nothing but operationalist and probabilistic methods to the study of (for instance) the behaviour of railway locomotives, completely neglecting - in approved behaviourist fashion - to ask what makes locomotives run. The student of steam locomotives would then quickly find himself in disagreement with the investigator of electric engines when the latter asserted that the cutting of high tension wires had a negative effect on the behaviour of trains. If ever these two, through their misguided method, should find the same lawful relationship between their randomly chosen experimental influences and their effects on the behaviour of locomotives, it would be in the cases in which the mechanisms experimentally influenced in steam and electric locomotives happened to be identical or strictly analogous. The bending of rails, for instance, would have the same effect on both kinds of engines.

Correspondingly, the behaviourist attempt to cut blindly across all species differences can demonstrate common lawfulness prevailing between the experimental influence and the-resultant changes in behaviour only in those cases in which the behavioural mechanism affected by the experiment is either identical or strictly analogous in each of the investigated species.

Indubitably the physiological mechanism which achieves reinforcement has been independently evolved in at least three phyla of animals, in Vertebrates, Cephalopods and Arthropods, and possibly, among the latter, again three separate times - in Arachnomorphs, Insects and Crustacea. The 'invention' of evolving a receptor apparatus ascertaining success or failure, and of feeding back, to the mechanisms of precedent behaviour, the reports of 'this was right, do it again', or alternatively of 'this was wrong, avoid this in the future', had, as a prerequisite, a rather complicated organization of receptor and effector mechanisms which simply do not exist in animals devoid of a central nervous system. It is, therefore, quite futile to expect, in an amoeba or a coelenterate, the capability of learning through reinforcement. Understandably, behaviourist research concentrates on animals which do possess the physiological mechanisms forming the prerequisite of conditioning by reward. If this is the case, as D. L. Lehrman pointed out, 'the same techniques, the same formulations of problems can very readily be applied, with the same quantitative methods and sometimes with the same instruments, to the studies of rats, of pigeons, of monkeys, of psychotic human beings and of normal human beings'. However, the remarkable uniformity of the behaviour of all these creatures in the operant conditioning situation depends largely upon the fact that, as Lehrman aptly puts it, 'the technique and the philosophy which it expresses, carefully avoid a good deal of what makes a guinea pig a guinea pig, and a pigeon a pigeon - to say nothing of what makes a person a person'.

Of course, this sort of comparison is a process which is altogether different from the one implied by the word 'comparative' when used as an attribute to morphology, anatomy or ethology. In these cases, the connotation of the word is that of a very special methodological procedure which, from a study of the similarities and dissimilarities of species, deduces their phylogenetic relationship and reconstructs their genealogical tree. The importance of this type of investigation is by no means purely historical. Quite on the contrary, we owe most of our insights into the causation of evolution to comparative studies: it was purely on their basis that Charles Darwin developed his theory of natural selection. If the cat has crooked, sharp claws, well-fitted for catching mice, this is a so-called *adaptation,* brought about, as we have sound reasons to assume, by natural selection which *bred* cats with that useful form of claws. If we say that the cat has such claws 'to catch mice with', we are not professing a teleological philosophy, but just stating, in an abridged way, the above-mentioned knowledge. This is the *teleonomic* aspect of the form and function of organic systems. (The term was coined by Colin Pittendrigh, who wanted to divorce teleonomic considerations from teleological ones as far as astronomy is removed from astrology.)

As has been explained, the programmatic restrictions of behaviouristic research preclude participation of any consideration of structure, or of causal chains of events. This, in turn, blocks the way to any phylogenetic approach, which can only be achieved on the basis of a thorough knowledge of structure. Avoidance of the evolutionist viewpoint makes it impossible to consider adaptation, and, consequently, the *loss* of adaptation, that is the pathological miscarrying of phylogenetically adapted structures and their functions. In other words, the behaviourists' programme excludes practically all the questions which biological science is asking, one is tempted to say all problems which are really interesting to us as live creatures and human beings.

Rejection of the teleonomic aspect of behaviour constitutes an inconsistency in behaviourist thought. It is an admitted, and even emphatically proclaimed, aim of their research to manage social relations by 'shaping' human behaviour ... by 'controlling', during education, the contingencies of reinforcement. I do not see how this kind of shaping and controlling can mean anything else than an attempt at *adapting* behaviour, nor do I see how one can hope to adapt something of which one knows neither the structure, nor the survival function, but only the changes wrought by arbitrary experimental influences. The extensions of behaviourist assumptions and techniques into the realm of pathology make particularly clear the consequences of total rejection of physiology. In the paper on inductive and teleological psychology included in the first volume, I tried to make it clear that any hope of finding the means to repair a system which has got out of order rests on insight into the causality of its 'normal' functioning, that is to say of the kind of function which provides survival value, the selection pressure of which brought about evolution of that function. There is no simpler way of defining the 'normal' and the 'pathological', and even so the borderline between the two seems disturbingly vague when we consider its immense practical, medicinal importance. Without that borderline (i.e. without the concepts of adaptation and its disturbance, of survival value and its loss), health and illness are indistinguishable from each other. At the same time, a health-preserving factor in an animal's environment cannot be distinguished from a reinforcing one, as can be demonstrated by many erroneous interpretations of deprivation experiments, in which the effects of ill-health, for instance atrophy of the retinae in an animal reared in darkness, were mistaken for the consequences of lack of conditioning.

The dogmatic restrictions of the behaviourists' research programme are psychologically amazing; it is extremely hard to understand their motives for renouncing not only quite normal, everyday functions of human cognition, but practically all sources of knowledge which, to 'nous autres', make life worth living. I have two tentative explanations of why this renunciation is so fashionable nowadays. One is that quantum or 'acausal' physics is regarded as the one and only 'exact' natural science - which it is not - and that it must therefore, at all costs, be imitated in every detail - which it cannot. The second explanation is, I am afraid, political: the fundamental negation of all that makes a person is necessarily welcome to all those who wish to *manipulate* great masses of people. The belief that a man is exclusively the product of the conditioning which he received during his childhood and adolescence encourages the attempt of 'social engineering' which Lenin proposed and which Aldous Huxley has so horrifically described in his satirical book *Brave New World* and in its sequel *Brave New World Revisited*. It is unlucky for the happiness of present-day humanity that the doctrine of the unlimited conditionability of man is untrue: One cannot *teach* a man how to be happy under the neurosis-producing circumstances which the great manipulators on all sides of all curtains are trying to force upon us. It is lucky, however, for our humanity-for everything that *does* make a man a man - that this is not so, though it will take some real men to take a stand against the deleterious influence of an ideology which, even at present, has become a world religion. Even the scientist himself, as a propagator of the behaviourist's doctrine, is threatened by this danger: As D. L. Lehrman has recently pointed out, 'it is not only the subject which is denatured by behaviourist psychology; the experimenter himself is not permitted to be entirely human'. An epistemological lobotomy which prevents an intelligent man from using the normal cognitive functions nature gave him, does indeed constitute an act of dehumanization.

Were it not for the fact that behaviouristic philosophy, by gaining political ascendancy, constitutes a real threat to the very essence of human freedom, it would be redundant to expound the philosophy of natural science. As matters stand, however, it does seem necessary to explain, in some detail, what we, the natural scientists, are trying to do, and also *how* we are going about it. Enlarging on the methodology of research seems all the more necessary, as the methods of many younger students of behaviour have undoubtedly been dangerously influenced by behaviouristic fashion. Most of them are tarred with the behaviourist brush of exclusive or predominant operationalism, some slightly, some heavily and some beyond redemption. Even among those who regard themselves as ethologists there are men who regard the experiment as the only legitimate source of scientific knowledge, and operationalistic procedure as the only way to gain it. J. P. Hailman's review of the first volume of my collected papers may serve as an example of this attitude. He 'laments the vagueness' of many of my concepts and 'the apparent lack of interest in operationally formulating and rigorously testing initial hypotheses'. He says 'Nothing summarizes Lorenz's epistemology so well as his own phrase "inductively determined facts".' In another place, arguing against 'Lorenz's conception of scientific method', he says that my joint paper with Niko Tinbergen on the egg-retrieving response of the greylag goose 'is the only one in the collection that would be likely to be called scientific by modern criteria', and this only because Tinbergen's excellent experiments form the essential part of this paper. I myself am indeed a very poor experimenter; but I hope to have shown in most of the papers included in this second volume that *induction,* the fundamental cognitive process of all science, which consists in the abstraction of a general law out of the observation of many special cases in which it prevails, is *not* absolutely dependent on the experiment: the hypothesis which allows prediction can just as well be verified or falsified by further observation of single cases which are not intentionally contrived by an experimenter. The important point is that one cannot 'test initial hypotheses' concerning complex systemic functions in any other way than this, nor can one, at an early stage of analysis, formulate any but provisional and vague concepts. To pretend that one has, at that stage of investigation, any sharply definable concepts, would be sheer fabrication. If some people, like J. P. Hailman, believe that 'Lorenz fails to convince one that he understands the cycle from observation through induction and prediction back to subsequent observation', I can only humbly hope that this second volume might convince at least some people that indeed I have! I also hope that I succeed in making it clear that operational formulation and rigorous experimental test, though desirable, are not indispensable to making a scientific legitimate assertion. Isaac Newton, for one, was in no position to test, by rigorous operational tests, the inductively determined facts which he abstracted from the observation of astral bodies (experiments on gravitational forces are extremely difficult to perform in laboratories); but I do lament a generation of scientists who deny his results the honorific attribute of being 'scientific by modern criteria'.

Considering these ulterior aims of this collection of old papers, I thought it best to make an exception from the otherwise chronological arrangement, by putting first a paper which I called 'A Scientist's Credo'. In fact, it is a paper which I have never written: some years ago, my late friend John Benjamin, psychiatrist at the University of Colorado, asked me to speak to the medical freshmen of that year about the philosophical fundamentals of natural science. I did, and he had my speech recorded on tape. In order to preserve the directness of the spoken word, he prevented me from doing any editing, and in deference to his wish I have not changed anything in what this great scientist thought worthy of taking down from that tape.

The next paper, [*Comparative studies of the motor patterns of Anatinae*] dealing with a comparative study of the innate motor patterns of ducks, is put in as a paradigm illustrating what the term 'comparative' means in biology, as opposed to its connotation in behaviouristic 'comparative' psychology. Both the publishers and myself at first had some doubts about including it in this collection, and if I finally insisted on doing so, it was because I regarded it as necessary to show to readers not familiar with the study of phylogeny what a toilsome, patient work truly comparative science is, what a hard-won accomplishment the craftsmanship of its experts is, and that the reliability of its results can actually be computed by a consideration of probabilities. The reader who is not interested in phylogeny may skip this paper, but then it is probable that he would be better off to skip the whole book.

The third paper, 'Part and Parcel in Animal and Human Societies,' was originally written in opposition to the tendency of certain social psychologists to generalize principles of gestalt psychology, particularly the ascendancy of the 'whole' over the 'parts', and to treat all living systems as if they were the results of gestalt perception. Although this error is virtually the exact opposite of the above-discussed fallacy of behaviourist thought, it results in the same disastrous conclusion that *causal* analysis is unnecessary. Both the behaviourists on one hand, who, in imitation of quantum physics, replace causality by probabilistic computation, and 'holists', on the other hand, who believe in a miraculous 'whole-producing' factor neither accessible to nor requiring a causal explanation, both unanimously hold the opinion that it is unnecessary and even unscientific to disentangle the host of causal interactions which take place between the parts, or 'sub-systems' of an organic entity, which are dependent on the structure and function of these parts, and which, in their totality, determine and *cause* the functional properties - in other words the behaviour of the whole. In this paper I have tried to explain the strategy of approach which is obligatory in the endeavour to analyse any living system. The verb 'analyse' means making the function of an entity understandable on the basis of already-known, general laws, *and of the special structure* of the system in which these laws prevail. The paper can give quite a good general idea of how far the ethological analysis of 'behaving' systems had progressed 20 years ago, and also of the role which comparative, phylogenetic considerations played in this progress.

The fourth paper [*Psychology and phylogeny*] deals with the application of phylogenetic-comparative methods in the field of psychology and behavioural study. As it touches on problems also discussed in other papers, certain redundancies could not be avoided. As in several other cases, the slowness with which I have developed certain ideas has the undesirable effect (already mentioned in the introduction to the first volume) that things that ought to have been said in the summary of an earlier paper are only explained in a later one; or, conversely, that in an early paper certain phenomena which really are essential to its understanding are only just touched upon, while their more thorough explanation follows several years later. Thus the methodology of comparison, which is not sufficiently discussed in the paper on the motor pattern of ducks, where such a discussion should be (p. 14/15), is more clearly summarized in the fourth paper [*Psychology and phylogeny*] (p. 197 etc.). The latter contains, on the other hand, some short and rather inadequate remarks on the function of Gestalt perception as a source of scientific knowledge, while this important subject is dealt with more thoroughly in the next-but-one paper [*Gestalt perception as a source of scientific knowledge*], published four years later. Another overlap, between this paper and the next-but-one, concerns the discussion of the historical circumstances which retarded the application of phylogenetical methods to the study of behaviour; these are much more clearly discussed in the sixth paper [actually the fifth: *Methods of approach to the problems of behaviour*] (which I read as a Harvey Lecture at the Rockefeller Institute, New York, 1959).

The subject of the fifth paper [actually the sixth: *Gestalt perception as a source of scientific knowledge*] included in the present collection deals with so-called *Gestalt* perception. For several reasons, this treatise is very close to my heart. For one thing, it contains a theory of cognition on which, as I have the temerity to believe, most natural scientists, consciously or unconsciously, are founding their work, and which was called hypothetical realism by Donald Campbell, to whom I owe a great debt of gratitude for editing not only the translation but actually the original, improving on it very noticeably. For another, this paper analyses a very wonderful, but by no means miraculous, form of neurosensory organization, and by this procedure may make it more acceptable to some scientists - or so I hope. It is one of the tragic-comical paradoxes of perverted scientific fashion that it is necessary to *legitimize* a procedure in the eyes of men who unavoidably use it in their daily work. Perception is the first step in any process of gaining knowledge of outer reality, and even the most 'hard-nosed' behaviourist *looks* at the graphs excreted by his Skinner-boxes, though he would (in order to de-humanize the 'scientific' procedure one step further) really love to publish them without doing so. However, even research workers who are otherwise epistemologically sound have some inhibitions against confessing (even to themselves) their dependence on their own, healthy sense organs. It is the sense organs, to which the physiological mechanisms belong, which, by complicated and non-conscious computations, abstract perceptions out of the chaos of innumerable sensory data. Much is known about these mechanisms. A long time ago, Wilhelm Ostwald had exactly the right idea of their function; Erich von Holst, Wolfgang Metzger and others have analysed them with the greatest possible success, but modern theory of knowledge does not seem to have drawn, from their results, some highly relevant inferences which, in my opinion, are inescapable. The man who is otherwise my authority in the field, Karl Popper, never mentions the cognitive functions of Gestalt perception, nor do I quite understand his rejection of *inductive* processes as means of cognition. All constancy functions, and also true Gestalt perception, perform the objectivating function of extracting, from hosts of sensory data, the lawfulness prevailing in them. In order to achieve the separation of the essential from the accidental, they need an enormous sensory input, which must be the greater, the lesser the content of essential and the greater that of the accidental. As Grey Walter said, it is redundancy of information that compensates noisiness of channel. Perception is the function of a computer which, on an unconscious level of our neuro-sensory system, achieves knowledge by an inductive procedure. On the conscious level of cognition, we never start out from the immeasurably lower level of unselected and unprocessed sensory data, but rather from the sophisticated reports made to us by our perception, particularly by our Gestalt perception. These reports do in fact already contain hypotheses, and quite well-founded ones at that. Of myself, for one, it is simply not true that my first step in approaching any phenomenon I have observed consists in creating a rather random hypothesis and subsequently trying to find fault with it. Knowing about the functions of my perception as I do, I feel inclined to suspect that the sequence of events is, at least partly, the reverse of this. I strongly suspect that, at the time when a set of phenomena seriously begins to fascinate me, my Gestalt perception *has already* achieved its crucial function and 'suspected' an interesting lawfulness in that particular bunch of sensory data. If I then spend more and more time in observation of these particular phenomena, it is already a consequence of a hypothesis which my perception has formed, though I may still be quite unconscious of it. The increased observation accelerates the input of sensory data until, when sufficient redundancy is achieved, the consciously perceived lawfulness detaches itself from the background of accidentals, an event which is accompanied by a very characteristic experience of relief expressed, as Karl Buhler described many years ago, in the sigh: 'Aha!' After this 'Aha-experience' I proceed to cast about for further observations in which the suspected lawfulness plays a role. In doing so, I tend to increase the redundancy of information, and therewith, the probability of being right. It would, however, be a falsehood to say that I am hopefully searching for evidence disproving the hypothesis suggested to me by my perception. I must never, of course, forget that there really is no such thing as verification, and that I can only increase the probability of being right by collecting more and more cases in which my hypothesis was not falsified, and by assiduously searching for circumstances under which there is maximum improbability that any other than my chosen hypothesis could furnish a satisfactory explanation. But, however conscientiously I pursue this work, it would be dishonest to deny that I am rather fervently hoping that my hypothesis will stand all these strenuous tests.

If it were not for the non-conscious neuro-sensory functions just discussed - in other words, if our conscious effort at cognition really had to start at the level of miscellaneous, unprocessed sensory data - we really should have to approach them with nothing but a consciously built-up hypothesis, as yet unsupported by any factual evidence. Inductive procedure would, I think, really be impossible and it would indeed be the best strategy of research to do one's best to disprove a hypothesis which, in this case, would be highly unlikely to contain any appreciable amount of truth. Gestalt perception, on the other hand, when based on a sufficient wealth of unbiased observation, has a way of being *right,* and if one is familiar with its occasional trick of being altogether wrong and knows when to discount its assertions, it is an invaluable and quite indispensable guide.

The next paper, the sixth [actually the fifth, *Methods of approach to the problems of behaviour*], attempts a short survey of the history of behavioural science. In 1949, at a symposium of the Society of Experimental Biology in London, I read a paper in which I analysed the shortcomings of different schools of behavioural study, of behaviourists, purposivists and of Jakob von Uexküll's 'Umweltlehre'. That paper has a disagreeable 'mother knows-best' flavour and certainly does not do justice to the merits of the scientists criticized in it. The paper presented here actually has the same subject, yet it is, in a manner of speaking, doing penance for the seeming aggressiveness of the one just mentioned by laying stress not on the errors, but rather on the lasting merits of different schools. This 'inversion of sign', far from impairing the factual content, is more conducive to a just appreciation of the historical process by which thesis and antithesis bring about a dampened oscillation of scientific opinion which, in the end, becomes stabilized at a point which represents a much nearer approach to truth than any of the opposing doctrines had achieved. This holds true, for instance, for the different views concerning instinctive and learned behaviour held by behaviourists and purposivists. It does not hold true, however, for the fundamental philosophy of behaviourism, that is to say of its attempt to circumvent the necessity of *causal* and *structural* analysis. At the time when I wrote that paper, I was not yet conscious of this fundamental and devastating fallacy. Although the reader will find that I do indeed keep harping on analysis of structure and causal interactions, he will soon realize that I still considered explanatory monopolism and atomism to be the only errors for which to reproach behaviourists.

The seventh and last paper [*Do animals undergo subjective experience?*] tackles a problem which, in my experience, regularly crops up late at night, at the end of epistemological discussions when the protagonists have become very tired: the good old, and reliably insoluble, body-mind problem. Far from attempting the impossible, I have endeavoured merely to describe functional properties of nervous processes which are correlated with subjective experience. In fact, these correlations contribute some interesting paradoxes that make the body-mind problem appear even more of a great enigma, which is impossible to solve with the cognitive functions evolution has provided us with. This seemed a tolerably good final note on which to conclude a collection of epistemological papers.

Of these, two (the first [*A Scientist's Credo*] and the sixth [actually the fifth: *Methods of approach*]) were originally written in English by myself[1] as, I am afraid, will be painfully evident to the reader, particularly with *A Scientist's Credo*. The paper on Gestalt perception was translated by Charlotte Ghurye and edited by Donald D. Campbell who, as I have already mentioned, decidedly improved on the original. For the rest of the papers I have to thank my friend Bob Martin for an immense amount of meticulous, conscientious and often really inspired work invested in his translation. One reviewer of the first volume, Arthur Bourne, has said, 'Even in translation these papers are beautifully written.' I can assure the English reader that in the German original they really are not, so the word 'even' should be deleted and tribute paid to Dr Martin.

[1] As was this introduction.