If maximal explicitness, i.e. systematic expression of the Nominative/Oblique opposition had been an important consideration in the evolution of these forms, the Early Middle English system should have ... chosen the Dative form of the Neuter pronoun (p. 282).

Instead, the Accusative form was chosen, as shown by Erica Garcia, simply because of its greater frequency of use. This is but one example, but is illustrative of a case where we might have mistakenly thought the change was cognitive.


10. In particular, at least the following errors exist in the diagrams in chapter 7. In schema 5 there must be a connection between theory 1 and problem F, for F not to ‘discriminate’ between 1 and 2. In schema 6 there is no theory labelled 7, so there is little direct sense to Hattiangadi’s description ‘6 dominates 7’. One assumes that theory 7 must be the line which leads to problem U4.

Bibliography


MARR, N. I. *Po Etapam Jafejskoj Teorii [Through the Stages of the Japhetic Theory]* (1926).


The author responds: Mertz on parole

JAGDISH HATTIANGADI

I am lucky to find a reviewer who not only understands the central tenets of my book but feels confident enough of his understanding to want to criticize it in spite of shortcomings in its organization. The issues that he addresses are among the main deficiencies of the point of view which I attempt to explore. Mertz approaches the matter as a philosophical behaviourist, very likely as a follower of Quine. Philosophical behaviourism is a form of conceptual self-incarceration. In it one does not allow oneself to travel outside certain conceptual boundaries which are deemed secure. My book should be off limits to him. His perspective from inside does afford a certain interest to his approach, which is refreshing, and I can only be thankful that he let himself out long enough to read and comment on my thoughts on ‘language’ (where he thinks, as a behaviourist, that there is only parole).

Three issues raised by David Mertz call for some sort of comment: First of all regarding my flat, as he calls it, that knowing is not (necessarily) any sort of linguistic activity; second,
regarding how my views cohere with behaviourism; and lastly, on my alleged predilection for teleology, which Mertz thinks I must have acquired during my training as a Popperian. All three issues bear on matters of general interest, and I must thank the reviewer for his independent and critical stance, which lead him to them. I will, however, take the second and third issues before the one he raises first.

If there is one thing in my book that the reviewer notices less than I would have wished, it is that my construcions on most issues, e.g., on meanings, on problems and even on the origin of language, are meant to be resolutions of what appear at first to be separately plausible but conjointly impossible theoretical demands. In each case, the resolution is effected by a trick, a rabbit pulled out of a hat. It is important to me that I try to accommodate diverse evidence, and not just that of one kind. It is much easier to satisfy one of the several requirements, and neglect others. That requires no intellectual effort on my part, since others have worked out these views before me.

On meanings, for example, I could easily accept the behaviourist view that ‘there is no such thing as language, not if language is like what many philosophers and linguists have supposed. There is therefore no such thing to be learned, mastered or born with. We must give up the idea of a clearly defined shared structure which language-users acquire and then apply to cases’. Hattiangadi, says Mertz, admits the latter part of this quote from Davidson, but hesitates on the first. Here, as in most other places, Mertz gets me right. It is far easier to wear behaviourism as a badge than to go on to understand how it is ever possible that any two people misunderstand each other. This is the crux of the difficulty. In order to solve it I elaborate a theory of meaning which yields as a consequence the references of expressions, and also a version of logic understood, not as the structure of language, but as the evolving rules that we adopt in our efforts to avoid misunderstanding each other.

If we are merely to choose sides, it is easy to adopt one prominent point of view and to reject the alternatives as wrong-headed. It might have made for a clearer book than I have written. But if we are to understand language as we find it, and not as it suits our theoretical likes and dislikes, then we must admit that behaviourist theories cannot account for the phenomenon of misunderstanding. If I have given even a half-baked account of it, then it is more than what behaviourists can say.

Behaviourism, especially as Quine and his followers advocate it, yields the consequence that radical translation is indeterminate. What this phrase means is that on behavioural criteria alone we cannot distinguish between the philosophies of Carnap and Heidegger. Since I can, I conclude that Quine’s behaviourism is in error. If Quine will not, because he wishes to defend behaviourism come what may, I do not see to what point, given his own philosophical prejudices.

Regarding scientific knowledge, also, my views on problems need not have been so elaborate or convoluted if I were to accept the view that our beliefs are a matter of convention. No doubt some of them are a matter of convention, tradition or custom. Is all scientific truth also relative to convention, tradition or custom? If our intellectual traditions were coherent systems of thought, as Quine and others assume, then their conventionalism is unavoidable. But the plain fact is that in intellectual matters we are always disagreed. Should we come to agree on any matter, we academics turn to the next matter on which we are disagreed. My suggestion here is the somewhat Hegelian idea that a certain freedom from relativism (or a certain kind of intellectual autonomy) arises from the clash between theoretical systems, each of which is no more than an evolutionary residue of earlier competition between ideas. (Actually I prefer to think that this is inspired by Darwin, but that makes no difference to its value.)

Here Mertz notes that I agree with Quine, on the Duhemian principle that a recalcitrant fact undermines a whole theoretical system at once, and on the principle that we have the choice of modifying our theories in the system as we please, provided deductivity is respected. As a logical point this is correct. But, if I am right, then it applies only outside of intellectual traditions, and therefore does not adequately describe science. Here too my views are an attempt to concede everything that a conventionalist might wish to claim, and still deny the conventionalism or the relativism (‘internal realism’) that seems to follow. That is the ‘rabbit’ out of the hat. Assuming Quine’s conservatism, we can now show why intellectual traditions nevertheless sport ‘revolutions’ of thought. (Mertz’s reference to Hacking is interesting, but inaccurate: There is much that I agree with in Hacking’s book, but his book Representing and Intervening came long after my results were published, and moreover, what Hacking has to say is both substantially new and different from my own thoughts.)

My attempt on the origin of language is also to accomplish what appears to be impossible. My
project is this: let us assume the truth of a materialist, Darwinian (New Synthetic) picture of the evolution of living organisms. Given our recent scientific advances which supplement evolutionary biology, starting with thermodynamics and quantum physics, then crystallography, then organic chemistry and stereochemistry we emerge with an understanding of how living and, for our purposes, conscious organisms came to exist. Notwithstanding modern Analytic philosophy, Darwin may be read as having solved the mind-body problem, and it has since been confirmed. At any rate let us concede it for now.

My task then is to address this question: How did the first linguistic symbol come to be? This question bores the behaviourist, because a symbol cannot be distinguished from a non-symbol on behavioural criteria. But looking at the facts, and without regard to our intellectual convenience, we cannot deny that human beings have something that was apparently missing in the universe before - symbols. These are conventional, items of the sort that even impresses some behaviourists like Quine, so that he models all knowledge on them ('conventionalism').

Now it is an astonishing fact that one day, all of a sudden, a word came to stand conventionally for a thing, or an event or a state of affairs. How could this have happened given what we can reasonably assume to have already been there before symbols? How did symbols come to exist in a material world? My views on this, poor as they are, as designed to show how this is possible. The rabbit, taken out of the hat, amounts to this: If, in play, children actually imitate adult (apelike) communication by a mock call, then the mock call stands in a symbolic relation to the object which would normally elicit a real call. That is the trick. I cannot possibly know if this is how it happened, but I do feel pleased that I can account for the origin of symbols within living systems without assuming divine intervention, or denying, as a behaviourist would, what is plain to everyone.

If I may comment on behaviourism more generally, it amounts in part to a linguistic prescription, namely to disregard all terms that are not behaviourally defined, except as façons de parler. There is a particular irony in an investigation into language ('verbal behaviour') starting from a behaviourist point of view, because it cannot account for its own existence (since there are no points of views from that point of view). My own approach in all such matters is that one must accept all evidence, from whatever source, however expressed. We must face the difficult fact, if necessary, that we are disagreed on issues because the evidence is equivocal. In that case we must either re-evaluate some evidence; or find a trick which allows us to eat one’s cake and have it, too; or else we have to concede that the question is still open. The last attitude is the normal one. What is the use of denying some of the evidence because it is stated in a way that is unpalatable, or because it reveals an open question?

David Mertz’s criticism that my view of evolution is too teleological is telling, if true. I would certainly wish to give up the teleological interpretation of the evolution of species that Mertz detects in my Popperian training. It could be that my view has teleological consequences of which I am not even aware, having studied with Popper for a couple of hours every week for two years, as a student towards a Master’s degree. Perhaps that was too much for me to retain my independence of thought. Personally, I did not find Popper especially teleological, but this might be because I am so imbued with that Popperian spirit that I fail to see it. But if we read what I said, I think the reader will find that more teleology is read into my writing than is there (when I read Mertz’s quotations from my writings I do not see the teleological interpretation in them) - it is certainly more than I intended, if it is there.

I take it, as do most biologists, that there is in living organisms a teleonomic tendency - an appearance of teleology without there being a Grand Purpose behind it all. If individual organisms have purposes, then it is because they have evolved in response to natural selective pressures in such a way that they act in what appears to us as a goal-directed manner. Now if we deny that fish sometimes swim upstream, and other such evidence, we may commend ourselves for being anti-teleological, but we would not further the cause of understanding very much. I take it that to this extent we are agreed on teleology, and on Darwinian or New Synthetic ideas.

The difference that Mertz sees between us concerns what Mertz calls the minimal and maximal interpretation of adaptation - that is to say, between being perfectly adapted to our environment and being only slightly better than our nearest cousins at exploiting it. My own view on this is that the first view is not Darwinian at all, but Leibnizian, the view of a pre-established harmony. I call myself a Darwinian only to the extent that I accept what Mertz calls the minimalistic point of view. If I seem to have given a different impression I expressly recant it.

In my view (as for Mertz, I suspect) no organism is or can ever be perfectly or maximally adapted to its environment, if only because the other organisms which form the most important
part of the environment are themselves constantly changing. It might be that Mertz is misled about my views because I claim that to the extent that we are adapted to the environment, we act appropriately in it, and our action then can be said to contain implicit knowledge of the environment. But this knowledge exists only to the extent that we are adapted to it, which is minimally.

Buried somewhere in my book is an explicit statement that the hypotheses presupposed by our skillful actions are false if stated generally, but true if restricted to those ecological niches in which they are normally deployed. Perhaps Mertz thinks that even this is too strong. Could the hypotheses presupposed be false even there? Let us look at an example, and we will be in a better position to judge.

Newtonian theory is generally false, but 'works' in certain situations—at moderate speeds and with moderate sized objects. Even there the exact mathematical description that we have adopted in the twentieth century may not agree with the Newtonian description. But if we state Newtonian theory as an engineer might, with a + or − factor in it, then this weaker theory may be said to be true. If it is not, then there will be some consequence of it, however weak, which is true, and it is that which we can use to explain the success of the engineer's mechanics. The existence of a statement which is true of that situation is a trivial fact, arising simply from the circumstance that every consistent but false statement can always be modified or qualified to yield some true statement, it we are willing to weaken it enough.

My claim is that every statement presupposed by any organism (in its readiness to act in certain ways) is false when stated generally, which is equivalent to Mertz's claim that adaptation is minimal. These are merely two ways of acknowledging the same fact. My further claim here is that no matter how little we know our environment, to the extent that we simulate a pre-established harmony, and however imperfectly we do so, we do have some knowledge of our surroundings, however weak and however equivocal.

This conception of knowledge allows me to pull another rabbit out of a hat. All our knowledge is spurious if examined sceptically, item by item. Yet we can claim to know enough to be able to get by in the physical environment, and even to be able to test our abstract hypotheses. If we had no knowledge, none of this would be possible. But the claim that we do have this or that piece of knowledge cannot overcome the sceptical challenge. It seems impossible in either case. How can we get out if this difficulty?

In this case the rabbit out of the hat is the thesis that to the extent that we act (minimally) successfully, we presuppose false statements which have true consequences for our normal surroundings. We cannot ever be sure of any one of these consequences that it is not stated too generally, so we can always doubt its truth (the sceptical doubt). But, to the extent that we are successful in our environment, there is something that we do know; we are more likely to find this truth where we are most familiar with our surroundings, where we can routinely handle the situations that routinely arise. This is the basis of testing all abstract knowledge-claims. Here we have a foundation for knowledge without denying the sceptic's claim that any knowledge claim may be doubted.

As for society, my view coincides with that of Mertz on one matter. I, too, think that 'social customs need not be functional within society', but do not concur when Mertz carries on, 'they merely need not be so dysfunctional as to disrupt society'. On the contrary, they are sometimes dysfunctional enough to disrupt it, or else there would be no disruptions! Moreover, there is a general feature of society that I rely upon, that dysfunctional customs sometimes survive in a less disturbing form, when they are often recycled for new functions. The roots of the flexibility of grammar, which allows us to express diverse kinds of thoughts, is this: Grammar is constituted by the dysfunctional elements of language that are associated with defunct points of view which are then recycled for new and flexible functions.

In order to do all this, Mertz notices, I resort to the claim that knowledge is not (necessarily) linguistic. He is pleased to call this my fiat, and generously concedes it to me. He is more generous than most linguistic analysts, who would find my views suspect because I have this strange concept of knowledge. Mertz likes it because it resembles in some superficial way a behaviourist analysis of knowledge, in that it makes no mention of a knowing mind. Perhaps this is a good feature of it for that reason, but it is not one which impresses me.

When I ask how language is possible, it is because our many beliefs about it would seem to make it impossible, at least as we know it from our experience. Our language serves two major functions, that of communication and that of developing our knowledge of the world. Theories which describe language well under the second function, such as those of Quine,
fail on the first, where Wittgenstein (the later) does so well on the first but neglects the second. There is a reason for this tension. It arises from the fact that communication is a social or intersubjective event, but the invention of a theory is a private affair, until it is communicated. Communication puts a limit on what can be a meaningful, new idea, but someone must have an idea first if there is going to be any change in its expressive capability. If language is to be understood as we know it, then it must occasionally afford new ideas. It is this which leads to the growth, or development, of expressive capability in language. (If it could be said before, then a new location is only paraphrase. It must be incapable of expression for it to qualify as increasing expressive capability).

I conclude that it is not knowledge that presupposes language as is commonly assumed in this century, but quite the reverse, that it is language which presupposes knowledge, not only for its origin but also for its continuing development. (Incidentally, I accept Mertz’s qualification that all sorts of other kinds of change which may be called ‘semantic’ are caused by other social factors. I should have said this in the book, and neglected to do so only because I did not try to cover all aspects of language.) If all knowledge were made linguistic, by fiat, then I would simply have to invent a new word to say what I wish to say, as I do below. I can be at least as generous as Mertz. If I am right so far as this goes, then my claim that knowledge is not necessarily linguistic is not a fiat, but more like a theorem of my particular system.

My particular system proposes to take biology seriously, as opposed to most epistemological naturalists whose naturalism stops short of actually inquiring about nature. We note that adaptation, however imperfect, and however chance-like, can explain the appearance of a pre-established harmony only to the extent that organisms can in extraordinary ways anticipate features of the environment. When they do successfully anticipate something, we can read off a statement which I describe as ‘knowledge’, but if you prefer, I could argue by means of the new word ‘knowledge’ (fully definable in terms of the trigger model of knowledge and adaptation by natural selection, as Mertz has quoted me). There is a great deal that we know about the world, as living organisms which have whatever degree of success that we have in the world at anticipating nature. This knowledge is, we learn, partly encoded in the nucleotide sequences in our DNA. These determine which chemicals can be produced in our body. The enormously complex economy of chemical plants (or cells) that constitutes a multicellular organism nevertheless functions with apparent knowledge of the environment. Somehow.

Human knowledge, or linguistic knowledge, is only possible because we have knowledge of the world. Knowledge is, I suggest, only a manifestation in language of knowledge of a kind which all organisms possess. Language then is a social device which transfers knowledge in an expletive fashion. It is a social manifestation of structures in the genome. That, in the end is how, I think, most of our difficulties concerning it can be resolved. But it is a somewhat trivial point if we pay attention to it: Language can only arise and develop among living organisms. Does anyone doubt that? I have merely set out some of what we know of living organisms to fill out this picture.

Let me conclude with two somewhat unrelated points, first, a parting shot on behaviourism, and second some comments on mathematical truth.

Mertz supposes that the trigger model of action is closely allied with a behaviourist point of view, and I confess that on the face of it this seems to be true. But ideas are not always what they appear to be. Inspired by this model, Lorenz wrote The Evolution and Modification of Behaviour, which attacks behaviourism for not accounting for the fact that learning is adaptive. Moreover, the origins of this view are physiological rather than behaviourist. The two earliest versions being J. von Uexkull and Sigmund Freud, both of whose ideas were probably inspired by Helmholiz (no behaviourists). Moreover, the trigger model eschews mentalism only in the overall model, because it applies to plants as well as animals. But one of the fundamental distinctions between plants and animals is that the latter, especially mammals, are triggered by cues which are perceptual. The rapidity and variability of response in animals to cues stands in contrast with plants, and one reason for the difference is mentation among animals. (Behaviourism is a vegetative theory of animals).

If there were no mental phenomena among animals we would not have invented behaviourism. That we have is therefore good reason for its own downfall.

Mertz is not the first to find my views close to behaviourism, in spite of my opposition to it. Eight years ago Jerry Katz commented on a paper of mine on the trigger model by attacking behaviourism. At the time, I thought that this was merely an automatic philosophical response of an anti-behaviourist to certain triggering cues in my manner of expression. I see now that I was unkind. Perhaps there is more behaviourism to my point of view than I intend or wish. If so, then
Mertz has done a valuable service in raising this issue. If not I am glad of this opportunity to lay that ghost (I mean spooky behaviour, of course) to rest.

Regarding mathematics, Mertz takes me to task, in his gentle way, for not addressing the obvious question about the status of mathematics and logic given all that I say about meanings. Instead, I conclude the book with the question, which I leave unanswered. Several colleagues have chided me for this, and I believe that several prospective reviewers of the book consider this a flaw. But the fact is that I thought that what I had to say was no contribution to the subject, and so refrained from adding anything. Since then I have discovered that if I had practiced Socratic self-reflection I would have recognized that I do actually have an unorthodox philosophy of mathematics, on which I am presently writing. I shall summarize these writings for completeness.

I am a Pythagorean where mathematics is concerned, I have discovered. When Galileo, Descartes, Huygens and Newton, in their different ways, tried to describe the geometrical structure of motion itself, they were right in what they thought they were doing. It was Leibniz who challenged this point of view, claiming that space and time are relative, and internal to monads. The debate between Leibniz the rationalist and Newton the empiricist would have been clearly in Newton's favour, but for the empiricist critique of gravitation, matter, space, time and fluxions by Bishop Berkeley. His critique was that if, as Newton claims, we are to be empiricists, then we cannot empirically find that the world is not as it is revealed in our percepts. Berkeley concluded that the scientific (mathematical) world picture given us in the seventeenth century could not be true, even if it is predictively useful. Thus Newton was hoist by his own empiricist petard, and Leibniz's subjectivism in mathematics became, in one form or another, the central dogma of the Philosophy of Mathematics for two centuries and more.

Hume's conventionalism — and its modern variant, logicism — Kant's transcendental point of view, Brouwer's institutionism, Hilbert's formalism, and even modern mentalistic Platonism, as found in Godel, for example, are all variations on the Leibnizian theme.

But if I am right that our knowledge (sorry, knowledge) of the world is found in our relatively successful responses to cues, and not in the cues ("percepts") themselves, then we must abandon Berkeley's pessimism regarding our knowledge of the world. We understand the world directly, being mathematical products of a mathematical universe, more or less adapted to its local mathematical features. If my biological form of empiricism is superior to that of the eighteenth century, then we can abandon Leibnizian philosophies of mathematics as we abandon fears arising out of a bad dream. We can go back to a Newtonian conception of the world, in which mathematics is a noble science, descriptive of the world, and logic merely a theory of argument.

If I am right, then (applied) mathematics and physics are equally empirical, and synthetic. Pure mathematics is the science of mathematical technique, tested by mathematical practice, (i.e. empirically) just as physics is tested by laboratory practice (this is where I would adapt ethnomethodological studies, like Latour's and Woolgar's, for use, or misuse, in my own scheme). Logic, I am afraid, remains a theory of language, and of argument; it is either empty, subjective or, at its best, a part of social science, depending on which aspect of it one wishes to emphasize.