

DISCUSSION PAPER

on

Gerard Radnitzky's

**THE EVOLUTION OF SCIENCE AS AN ELEMENT
IN THE "RISE OF THE WEST"**

by

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At the time when I first settled down to study Professor Radnitzky's paper its concluding section, on Euroscclerosis, could scarcely have been more topical. For M. Jacques Delors had just delivered at the latest Trades Union Congress a rapturously received address on how he and his fellow Commissioners in Brussels were planning to add to the soon to be established European common market what he cryptically characterized as "a social dimension". Aware that 'social' here was, as so often, a code word for 'socialist' the union barons got the message; abruptly abandoning their long time opposition to British membership in the European Economic Community. It was, of course, a similarly immediate appreciation of the - in every sense! - burden of the missionary message of M. Delors which provoked our Prime Minister to make the Gaullist speech which so offended the socialists of all parties, both at home and abroad.

But what we have to discuss today is not, or not so much, that concluding section but all the earlier sections upon which it depends. I have to confess at once - albeit, as my sometime Supervisor Gilbert Ryle used to say, "not very shamefacedly" - that I myself find Professor Radnitzky's paper both profoundly sympathetic and extremely exciting. So my contribution cannot be more than supplementary and scrappy. It will in fact consist of peripheral addenda.

(1) The first concerns the REMM model. Radnitzky needs, I think, to be a little more careful than perhaps he sometimes is both about self-interest and about maximizing. No doubt it is true, almost if not quite necessarily true, that all of us always are - in the never to be forgotten words of so many characters in the Broadway stories of Mr. Damon Runyon - "doing the best we can"; in the sense that all of us all the time are trying to get the most we can of all the things which we most want.

If however, this statement is to be true it can only be by our taking care to make it almost if not quite necessarily true. To do this we have to insist on construing 'self-interest' as embracing all the preferences and desires which anyone may have - including preferences and desires for the welfare or, for that matter, the illfare, of other people. In this understanding, and it is only in this understanding that the REMM model can be fitted to the familiar facts, even the most unselfish actions must be allowed to be, yet should not be discredited for being, expressions of the purest self-interest.

We also need to introduce a similar flexibility into our usage of the word 'maximizing'. For it would be as false to assert that everyone is forever eager to increase their satisfactions, and alert to every possibility of so doing, as it would be to claim that all business managements are equally profit-oriented and equally devoted to improving the balance sheet. Everyone should know that neither proposition is true,

and that many people most of the time are content if they can coast along gently. They are moved to strenuous activity, if ever, only when they see a prospect of really massive improvement or a threat of painful decline.

Finally, under this first head, we must note the claims that "One of the human universals is self-interest: if given a chance to express his own decision, [a man] will not irrevocably and unconditionally delegate decisions to others. Self-interest has survival value, which makes us suppose that it is the result of evolution..." (p.11).

Certainly, Radnitzky and I will not. But in those now far gone days when no Jesuits were Leninists men used, on joining the Society of Jesus, to put themselves irrevocably and unconditionally at the disposal of their superiors. And self-interest has survival value for the self-interested individual only in so far as it is self-interest in necessarily selfish sense which we have to reject if we are to make the REMM model fit the facts.

(2) The second thing which I want to do is to defend what Radnitzky calls "cognitive traditionalism" against (what I see as) his Popperian paradoxes: "Cognitive traditionalism upholds an end-state conception of ideal knowledge: genuine knowledge should be proven to be true" (p. 26). This, Radnitzky contends, is all wrong:

This type of legitimizing (of) secondary theories has impregnated Western philosophy and shaped the latent epistemology that underlies ordinary language: ordinary language is impregnated with justificationist philosophy. (For instance, "I know that p, but I can only conjecture that p" would be considered self-stultifying) (p. 27).

This is indeed all quite unexceptionable. For it would indeed (be ordinarily accepted to) be improper to claim to know p unless, either you had sufficient (evidencing as opposed to motivating) reason or reasons for believing p, or else you were in some other way in a position to know that p.⁽¹⁾ But what is wrong is to go on to think that, because 'If I truly know then I can't be wrong' - because, to transpose this from the misleading Material into the Formal Mode of Speech, it is true that from 'Descartes knows p' it follows necessarily that 'p is true' - therefore knowledge, true traditional knowledge, is vouchsafed only to the infallible; and hence - since no mortal is infallible - that there is no such thing.⁽²⁾

It is, presumably, because he is inclined to accept this invalid argument - from (traditional) knowledge to the necessary infallibility of any (traditional) knower - that Radnitzky commends his own Popperian conception for taking adequate

account of our universal fallibility:

The epistemological climate that is propitious for science favours a process conception of knowledge: the process of research is open-ended, and scientific progress is possible in spite of the (in-principle) fallibility of all human knowledge (pp. 26-7)

(3) Radnitzky thus implicitly accuses "cognitive traditionalism" of presupposing that knowers must be infallible, and therefore incorrigible. This accusation, as we have just seen, is groundless. For the ordinary claim to knowledge is not a claim to any sort of infallibility, however limited and qualified. Furthermore, ^{whereas} ~~where~~ if I know p it follows necessarily that p must be true (though, of course, not necessarily and by the same token a necessary truth); every actual and particular claim to knowledge is, and cannot but be, made by some fallible human being. So all such claims remain always, in principle, corrigible. If any claim to knowledge, however properly grounded, is proved to be mistaken; then it follows that it was not, after all, knowledge.

But Radnitzky has another accusation against "cognitive traditionalism"; to which the cognitive traditionalist must - albeit again, "not very shamefacedly" - plead guilty. For

we do insist that if anyone is properly to claim to know p, then they must, either possess sufficient reason for making that claim, or be in a position which in some other way entitles them to make it. Precisely this is the 'justificationism' which, as a Popperian, Radnitzky abhors: "A scientific theory cannot be 'justified'" (p. 29).

Presumably this abhorrent rejection implies that there cannot be good, or even any, evidence for its truth. So much for Lyell's The Principles of Geology: So much for The Origin of Species: Such consequences are, surely, grotesquely and intolerably paradoxical?

In objecting, in his Note 3, to all claims to knowledge, "in the sense in which that word is used in ordinary language", Radnitzky maintains that "If scientists had listened to the ruminations of philosophers like Descartes, (science) could not have developed" (p. 03). But it is Popper, following Hume, rather than cognitive traditionalists who would appear to have been led astray by the ruminations of Descartes.

For Popper - like, and indeed consciously following, Hume - seems always to have taken as "the obvious dictates of reason", which "no man who reflects ever doubted", three assumptions which Descartes reached as interim conclusions in the first two paragraphs of ^{Part IV of} A Discourse on the Method.⁽³⁾ First, in the order of appearance in Descartes, comes the complex idea that ^{empirical} knowledge is possible only where it is not of material things but - as the

old-timers used to say - of sense-data. But his acceptance of the first is Popper's reason, and presumably also Radnitzky's reason, for rating Hume's treatment of argument from experience a decisive "refutation of induction". Given this, then they have to conclude both that "A scientific theory cannot be justified" and that there is no such thing as knowledge; at least as traditionally conceived.

About and against these altogether unbelievable conclusions I have had much to say on other occasions; but here and now must say no more.

NOTES

- (1) To appreciate the difference between evidencing and motivating reasons, consider Pascal's Wager; the one classical argument for the existence of God eschewing reasons of the first in favour of the second sort.
- (2) For a development of this point - in which John Henry, Cardinal Newman is deployed to correct John Stuart Mill - see my An Introduction to Western Philosophy, Chapter IX, Section 4. Indianapolis, and London: Bobbs-Merrill, and Thames and Hudson, 1971. This is shortly to be reissued in a revised edition by Thames and Hudson only.

- (3) The contention that these, as he said, "obvious dictates of reason" were indeed Hume's philosophical starting point is developed in my David Hume: Philosopher of Moral Science. Oxford, Blackwell, 1986.