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## GIDEON ROSEN ON CONSTRUCTIVE EMPIRICISM<sup>1</sup>

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Gideon Rosen's hermeneutics are so subtle and insightful, and his defences of constructive empiricism against various objections and misconstruals so skilful, that I was almost inclined to just endorse his interpretation outright. But perhaps the paper admits of other readings which might lead others to an understanding quite contrary to my own.

After Rosen's skilful hermeneutics, how can I not admit to the rhetorical element in my own writing? Yet his own paper is, it seems to me now, not entirely free of rhetorical devices, and it may be a great gain to lay those bare and open to view. Before turning to this task, let me endorse his initial point: constructive empiricism is not to be equated with empiricism. As Joseph Kockelmans put it a few years ago, *The Scientific Image* explains only what it is, according to an empiricist, to be an empirical scientist; it does not explain what empiricism is. Because I am still exploring that larger question, I shall not attempt here to answer the serious and important objections later on in Rosen's paper but address only his hermeneutical prolegomenon.

### 1. THE OVERALL ARGUMENT

At first blush, Rosen argues, constructive empiricism appears to be the assertion of a certain thesis – *call it CE* – concerning intentional aspects of science. But on inspection it turns out clearly not to be, and neither is it an exhortation to science to follow certain norms. Barring these ways of understanding the position, what can it be? Rosen's ingenious suggestion is fictionalism in philosophy: that constructive empiricism is the 'quasi-assertion' of a view about science, that is, the assertion that this thesis CE may not be true but has certain other virtues.

I will argue that Rosen has shifted attention from the thesis – which I will continue to call CE – as originally formulated to a similar but inequivalent thesis. His approach to this other thesis I consider genuinely illuminating and in the spirit of constructive empiricism, but I want to distinguish it quite sharply from the empiricist view of science as I understand it.

## 2. THE FIRST INTERPRETATION: SOCIOLOGY?

What exactly does an author do when s/he displays an interpretation of a work and then immediately goes on to dismiss it as clearly and obviously incorrect? If it was so dismissable, why is it nevertheless introduced and discussed?<sup>2</sup> The characterization of constructive empiricism and scientific realism in *The Scientific Image* (henceforth *TSI*), Rosen writes, read like “straightforwardly opposed *descriptions of the intentional features of science*” (p. 144)

A literal interpretation . . . would therefore have it that SR [scientific realism] and CE are opposing proposals about what scientists actually think. . . . But this just can't be right. If CE entails that real scientists generally don't believe what their best theories say about unobservable objects and processes, *then it's obviously wrong*. (p. 145)

The italicized (sociological!) claim at the end of this passage is of course only the introduction to a more serious reason for dismissing the first interpretation. That interpretation, according to Rosen, makes CE an empirical hypothesis, to be investigated by sociologists of science, while there is no evidence of such investigation in *TSI*. As further support for the dismissal Rosen quotes the passage in *TSI* which likens the role of empirical adequacy in science to the role of checkmate in chess:

The aim of science is of course not to be identified with the individual scientists' motives. The aim of the game of chess is to checkmate your opponent; but the motive for playing may be fame, gold or glory.

That passage includes an explicit denial that the CE thesis describes the individual scientists' motives.

Since the reasons for the dismissal are so conclusive, why is this discussion included in the paper? To echo Rosen's words, “forces of

considerable rhetorical complexity are at work” here. Interpretation 1 and its dismissal are surely included because of a certain part that is retained and which reappears later in the paper. That part resides precisely the *equation of the intentional aspects of science with the intentions and opinions of the scientists*. Since Rosen does, and I do not, accept this equation, there is for me an equivocation in Interpretation 1. To remove the equivocation I shall give distinct names to the theses which Rosen introduced here:

SR/R: (all or most) scientists aim to construct true theories, and believe the theories they accept to be true.

CE/R: it is not the case that SR/R, but (all or most) real scientists aim to construct empirically adequate theories, and believe the theories they accept to be empirically adequate.

I submit that CE is a claim about the aim of science and about the beliefs involved in theory acceptance within science. If that is not about intentional aspects of science, what is? But I submit that this claim is quite compatible with SR/R: actual scientists really believe the theories they accept (as is indicated by the passage quoted from *TSI* and made more explicit in later passages which Rosen also notes, e.g. on p. 148, in his discussion of Interpretation 2). I do not mean to radically dissociate the aim of science from the aims of the scientists but shall discuss their connection in a separate section below. Anticipating that clarification, here is how I see the matter. In his work, the scientist is engaged as participant in the pursuit of empirical adequacy. It is open to him or her, as individual, to believe that the accepted theories are true – or to qualify the acceptance with doubts about their truth without attending closely to the distinction between empirical adequacy and truth. Therefore the thesis that the scientist pursues empirical adequacy rather than truth is compatible with finding that individual scientists believe they have arrived at the truth. Given this compatibility, a sociological inquiry on this point would be irrelevant.

There is a similar distance between what the scientist pursues in his or her work and his motives or intentions in undertaking this work. Some do it, by their own testimony, in order to discover the plan of God’s creation, and some do it to discover the true laws of nature; many more today do it to discover the structure of certain unobservable entities

which they believe to exist. But the ‘it’ that they do, I claim, is work whose criterion of success in actual practice is empirical adequacy of the theories produced.

These scientists with their very different motives and convictions participate in a common enterprise, defined by its own internal criteria of success, and this success is their common aim ‘inside’ this cluster of diverging personal aim. How else could they be said to be collaborating in a common enterprise? The question is only what that defining criterion of success is.

While I will return to the distinction between CE and CE/R below, I want to note another, related distinction here. Rosen is throughout very clear on this distinction, and I thought that *TSI* was too, but there has been confusion about it elsewhere in the literature. We may draw the distinction adapting Peter Forrest’s apt term “scientific agnostic”:

*scientific agnostic*: someone who believes the science s/he accepts to be empirically adequate but does not believe it to be true;

*scientific gnostic*: someone who believes the science s/he accepts to be true.

This is a very different distinction from that between constructive empiricist and scientific realist. The latter are two types of philosopher, who have differing views of what science is, while scientific gnostics and agnostics need not be philosophers at all. The scientific gnostics’ beliefs are always changing, as science changes, but the scientific realist’s view of science stays the same throughout these changes. The two types of philosopher have corollary views about scientific gnostics and agnostics, to be sure. The scientific realist thinks that scientific gnostic truly understands the character of the scientific enterprise, and that the scientific agnostic does not. The constructive empiricist thinks that the scientific gnostic may or may not understand the scientific enterprise, but that s/he adopts beliefs going beyond what science itself involves or requires for its pursuit. As Forrest also pointed out in this connection, there is no disagreement about rationality involved here; it is not part of constructive empiricism to say that the adoption of such additional beliefs is irrational – just that it is more than what is involved in scientific theory acceptance.

## 3. THE THIRD INTERPRETATION: FICTIONALISM?

The lasting effect of reading Interpretation 3 will be, I hope, to illuminate the constructive empiricist concept of acceptance of theories. The clarification given is delicate, nuanced, and (barring to me as yet invisible pitfalls) correct. But I fear that there may be another lasting effect on the reader: to read the constructive empiricist thesis as an answer *not* to the question what science is, but to some other question altogether.

In the section on Interpretation 1 Rosen argued that constructive empiricism is not a claim about the intentional aspects of science. This prepared the reader for the possibility of Interpretation 3: that it is a claim about the public behaviour of scientists leaving out of account their private beliefs and intentions. Using the abbreviation I introduced: constructive empiricism, *pace* Rosen, does not involve the assertion of CE/R but its quasi-assertion. But exactly what is that?

Rosen immediately says that he does not mean the assertion that CE/R is empirically adequate. For that would require it to agree with all the relevant evidence, and it does not. That is the reason Rosen gives, and it will leave the reader thinking that CE/R could be empirically adequate without being true. This would be a serious mistake, in my view. It would mean that the described intentional aspects of science are unobservable. Constructive empiricism would be saddled with a type of behaviourism which I am not able to take at all seriously.

Science is an enterprise stretching over many centuries and through many civilizations, past, present, and to come. Whether or not a given person is a scientist – a participant in this enterprise – certainly depends more on what s/he does than on what s/he says or believes. But “does” is here to be taken as standing for intentional activity, not behaviour in the sense of the behaviourists.

Rosen does not say that CE/R could be empirically adequate without being true. Nor does he support the view that it is only the behaviour of the scientists, non-intentionally identified as in stimulus-response models, which accord with the constructive empiricist view of science. Yet he does construe constructive empiricism as some sort of fictionalism, an assertion that *certain aspects* of science are as if CE/R were true:

Here's my idea. For van Fraassen . . . a philosophical account of science must conform to what might be called *the phenomena of scientific activity*. I have in mind, speaking roughly, the outward activities of scientists: their sayings and doings when at work in the course of designing experiments, arguing about the interpretation of data, discussing the merits and implications of theories, and so on. (p. 152)

Scientific practice, as is easily seen in this description, is even in just its public aspects intentional activity, and not construable otherwise.

But if it is not the observable/unobservable distinction Rosen is making, what distinction does he make? Apparently it is the distinction between what counts as part of scientific activity – participation and collaboration in the common enterprise, as I would put it – and what the scientist says and does as private individual:

The phenomena of scientific activity do not include the hidden intentions of scientists . . . since these are not phenomena at all. More significantly, the phenomena in question do not include some of the things that scientists outwardly do. In his autobiography Richard Feynman notes a lamentable disease of middle age among physicists, the main symptom of which is the tendency to give public lectures on the Nature of Science. Now in these lectures the physicists may proclaim Realism – I'm sure they often do. But these proclamations, though outward, are not among the phenomena an adequate account of science must save. (pp.152–3)

As Rosen points out, everyone, both constructive empiricist and scientific realist, is here in exactly the same boat. Any philosophical view of science is to be held accountable to actual scientific practice, scientific activity – so everyone assumes some such demarcation.

But if everyone is in the same boat, and everyone assumes the same demarcation here, what happens to the opposition between CE/R and SR/R? If CE/R is correct when restricted to the scientifically relevant activity of the scientists, then what SR/R says is an addition which may be true but is irrelevant. The quasi-assertion of CE/R as now construed is the assertion that the goals in addition to empirical adequacy may be present but are not part of the scientific enterprise, and belief going beyond empirical adequacy may also be present but is not part of scientific theory acceptance. If this is correct, then what SR/R does is to conflate what is part of science, in the lives of the individual scientists, with other things that are not part of science. Since by hypothesis the

scientific realist makes the same demarcation, s/he agrees – and the debate ends with the victory conceded to the constructive empiricist.

Surely it cannot be that easy? Either the quasi-assertion of CE/R is not correct, or else the two participants in the debate disagree exactly over what is and is not relevant scientific activity. In the latter case, what the one classifies as irrelevant personal factors in the scientists' lives the other classifies as part of the phenomena of science. I cannot really speak for scientific realists, but that is how I think they will react to Rosen's suggestion. My own immediate liking for Rosen's construal, and his apparent view of the extent to which CE/R is correct, is now explained. Yes, I think he is right there; but I doubt that scientific realists will think so.

#### 4. THE LOGICAL CONNECTION BETWEEN SCIENCE AND SCIENTISTS

Certainly the thesis CE, that the aim of science is to give us empirically adequate theories, and that its aim is not to give us true theories, entails: what the scientists pursues is empirical adequacy rather than truth.

This has the form of such statements as "the whale inhabits both the Atlantic and the Pacific Ocean", and other such familiar abstract noun constructions. As this example shows, we cannot infer "all or most As are Bs" from such a sentence as "the A is a B", since most whales do not live in more than one ocean. Puzzles over this abstract form have led some philosophers to think that phrases like "the scientist *qua* scientist" or talk of natural kinds is intelligible, but as an empiricist I will of course not draw such conclusions. I make these linguistic points solely to discourage quick and easy linguistic transitions such as might lead one from CE to CE/R. The aim of science can perhaps, through a permissible *façon de parler* be equated with the aim of the scientist, but not with the common aim of all or most scientists. This point was already made in the passage of chess above, which Rosen also quoted.

Rosen of course foresaw something like this reaction from the passage with the chess analogy, and retorts:

But I wonder if this helps. Granted, there is distinction to be made between the aims of a collective enterprise and an individual's motives for engaging in it. . . . Still, the fact

remains that the proximate aim – the aim that determines what counts as success – is normally constituted by the conscious understandings of the participants. (p. 146)

We should both presumably grant that there is a strong disanalogy between chess whose rules and criteria for success are uncontroversially defined by official rule books and such large and vaguely circumscribed cultural phenomena as ‘the game of science’ (to use Popper’s and Lakatos’ term). But surely, it will be argued, Rosen’s point about action flowing from the conscious understandings of the participants remains valid for both cases?

In some sense, certainly. There is nothing *outside* the conscious understanding of the participants to define the activity. But this cannot be construed naively. It does not mean what all the participants say they are doing is what they are doing. Rosen would agree with me on this, I think, if the question before us were not *what is science?* but *what is art?* or *what is religion?* But let me take a smaller scale example than science, art, or religion: Clausewitz’ doctrine of war: [the aim of] war is the continuation of diplomacy by other means. This does imply:

the soldier’s aim, the criterion of his success, is the continuation of [his/her country’s] diplomacy.

But would Clausewitz have been refuted if all the generals canvassed insisted (in all evidence, sincerely) that their aim in war was to defend civilization, to cover oneself and one’s country with glory, or to bring about universal peace and brotherhood, while their countries’ diplomacy was clearly aimed at mercantile advantage and domination? Anyway, what about the universal soldier: the captains, lieutenants, sergeants, and grunts, the poor bloody infantry?

I do not mean to rely on this as an example of false consciousness. What happens in war flows from the conscious self-understanding of the participants. It also flows from the aims, intentions, and beliefs of the actual participants. But the two are not the same. I think that the soldiers or at least the generals understand the business of war very well. If they are sincere in their statement of their own aims and intentions, then it follows, I think, that they are convinced that pursuing those is compatible (and perhaps best combined) with the continuation of their



country's diplomacy by force of arms. (The military code of honour provides for this, I believe, in its reference to conscience, law, and morality.)

It appears from this that we must distinguish yet a third type of claim, somewhere between CE and CE/R, which I shall call CE/U (with "U" for "understanding"):

SR/U: the conscious understanding of (all or most) scientists is that the aim of science is to produce true theories.

CE/U: the conscious understanding of (all or most) scientists is that the aim of science is to produce empirically adequate theories.

We need not add a special clause concerning acceptance, I think, since to accept a theory is to take it to satisfy the relevant criterion of success, whatever that be.

Let me try to show that the \*/U theses are not the same as the \*/R theses. How should we interrogate scientists about their understanding of science? Rosen's discussion of Interpretation 1 may suggest that this is to be done by the same questionnaire that asks them for their own aims and beliefs. Let me suggest at least some more delicate probing as a little sociological experiment. Approach some scientists you know and mention some of their most valued scientific colleagues. Then tell them (taking the liberties of such empirical psychology) that as a matter of fact those colleagues are not pursuing the aim of finding true theories, but are privately concerned only to construct empirically adequate ones. Now ask them whether, with this new information in hand, they still regard those men and women as real scientists? Will they answer *No – C'est magnifique mais ça n'est pas la science?*

I am sure that you can think of variations on this game. Instead of their own esteemed colleagues you might bring in the (supposedly) great scientists in the history of science and tell the same lie (or truth, as the case may be). Indeed, if you are going to do this experiment at all seriously you had better also ask some scientists to suppose imaginatively that in the next century or so all those who continue present scientific research consciously adopt the aim of empirical adequacy. On that supposition, does science genuinely continue, in their opinion, or become a mere sham?

For the participants' understanding of their activity, as with much opinion and other (propositional) attitudes, the conditional part is by far the more significant. Speaking only of scientists in Western culture in the twentieth century, I am inclined to think that CE/U is much more likely to be true than CE/R. Indeed, I'm inclined to think that if we omit consideration of those scientists for whom the relevant questions have never taken precise form, CE/U is likely to be true not only for most of these, but for most scientists everywhere and everywhen. However, this is the sort of thing on which the facts may differ country to country, culture to culture, and epoch to epoch, exactly because philosophical education is a relevant factor therein. Neither CE/U nor CE/R is to be equated with CE.

##### 5. WHY PHILOSOPHY CANNOT BE SOCIOLOGY (AND VICE VERSA)

The danger in my reaction to these bits of putative sociology may of course be to have made CE out to be that typical metaphysical miscreant, the statement that is contingent and putatively empirical but so construed as to be beyond empirical testing. I say it is a claim about the aim of science, but do not equate the aim of science with an aim common to most or all scientists, nor even with all or most scientists' understanding of what science is. Does this not protect the claim from any possibility of falsification or even disconfirmation?

Well, if we wanted to go the way of philosophical sociology (or philosophical anthropology, a European curriculum item till recently) this danger would be even worse than the above would make it seem. For we would have to face a genuine hermeneutic circle: how can we even frame an empirical hypothesis about what scientists really do unless we have an empirical criterion whereby to identify the scientists? But is that criterion not exactly what is at issue, the answer to the question *what science is*?

Philosophy would quickly embroil itself in circularity if it tried to turn itself into cognitive science. If you pose the question *what is science?* because you are interested in scientific realism debate, then you must suspend belief in the answers CE and SR before investigating them. But in that case, what will you investigate?

The important point here is that sociology is to be itself an empirical science. Empirical science begins when the active investigator puts nature to the question: the definite question, with well-defined subject matter and precisely formulated request for information. This is what Kant called the Copernican revolution which initiated modern science (calling it by the name of one of its paradigm participants though Bacon was its prophet). So: is there a sufficiently well-defined set of scientists to be investigated by the sociologist before we settle on an answer to the question what science is?

Perhaps we could proceed as follows, as prolegomenon to the sociological inquiry: let us agree that any acceptable answer to that question will entail that our present classification of present workers into scientists and non-scientists is largely correct. This will imply that a monk in Tibet is now doing science only if he is doing Western science, but leaves open of course how to classify the activities in Tibet, or for that matter Greece, in 1500 AD, 500 AD, 500 BC, . . .

But wait: isn't there a way to set aside this obstacle? The problem, you might think, is that the above agreement on the present gives us a sample to submit to questionnaires, but without any idea of the relative size of this sample in the set of all scientists over the ages. Ignorance of this relative size prevents statistical extrapolation. Not so! There is an accidental fact that tells us something about the sample's relative size in the set of all scientists so far: the fact that there are numerically more scientists in 1993 than there were educated people in the whole world prior to 1900. It is clear from this that our sample constitutes the majority of scientists so far in the history of the world. So where is the problem?

Of course this is sophistical. A sense of the ludicrous is rapidly encroaching on us. Are we still investigating the merits of different understandings of what science is? Isn't it rather that the self-understanding of small pockets of scientists – if they really are scientists – in the 17th or 23rd century is just as closely related to the aim of science as that of the large majority in the 20th? But before we can look into this, or even frame meaningful empirical hypotheses about it, we need to know how scientists are to be identified. The identification cannot be simply through agreement on present extension. It must reach

outside the narrow ken of labels in common use in our own culture. But that requires an answer to the question what science is. And here we have come full circle.

None of this implies an obstacle to the pursuit of scientific sociology. It suffices there only to frame well-defined questions, such as whether CE/R or CE/U is true with the domain restricted to those officially classified as scientists in the year 1993.

Aristotle said of philosophy that its aim is to explain, to “remove wonder”. I think that is correct, and such contrary assertions as CE and SR compete with each other among philosophers on that basis. By taking them as central ingredients of our view of science we place ourselves in a position to *make sense of* those activities which we all agree are part of science. In our tradition this separation of philosophical from syntactically identical scientific questions has not been very popular. But the denial that there are properly philosophical questions has another side: the overcompensation by which philosophers project their own concerns and values into the enterprises they study. A main example of this, on my view, is the scientific realists’ typical insistence that science pursues explanation (as its way of pursuing truth, of course). This is but part of the mistaken but very common projection of philosophy of its own enterprise into science, art, religion, and everything else it studies. One hesitates to be uncharitable but can’t help but wonder: is this perhaps so as to be able to then claim legitimacy and worth for philosophy itself on the basis of resemblance to those enterprises? For the fashions in this projection appear to change with the way those are valued in the culture at large.

Can philosophy be stopped short by facts, in the way science can? Some philosophical enterprises cannot. In metaphysics as traditionally conceived there can indeed be setbacks, but since there are no limits to what counts as a successful repair except for logical consistency, the setbacks cannot be more than momentary. In philosophy of X, however – whether X be science in general, physics, biology, law, art, mathematics, or religion – there are facts to be reckoned with, and a philosophical view can become untenable on grounds other than consistency. As I understand it, a philosophy of X proposes an interpretation of X, and

while lots of ingenuity is allowed, it is not the case that just any interpretation can be carried through for anything. But since the enterprise of removing wonder, gaining understanding, explaining takes place within a dialogue, the successes and failures are relative to agreement among the participants in the dialogue, both with respect to classification of facts and with respect to evaluation of relevance and significance. (To some extent parallel assertions apply to science, but the analogy has severe limits.) Participants in philosophical dialogue will accordingly look for common ground and proceed from there; this common ground among them may derive largely from common views and values in the cultures(s) to which they belong. It is no dishonour to philosophy to be culturally and historically conditioned: if our aim is to understand ourselves, that is exactly what it should be.

To make this concrete, let us consider the question: suppose that CE cannot be carried through as an interpretation of science with respect to what we all come to agree on classifying as science – what should a constructive empiricist do then? It is a very difficult supposition for me to make, since so much of science seems to me to make sense on CE and not on SR. But in a purely logical sense I can suppose this, and it is clear what my choice would be then. I could then either say that it had turned out that science does not exist in my culture, or else that I had had a very wrong idea about what science is. It would be a hard choice. For saying the former, I would cut myself off from discussion of this enterprise in which we all have much practical interest. Saying the latter, on the other hand, I would disconnect the reference of “science” from the object of my admiration. Empiricists admire science, but of course they admire science as they conceive it – how else? That object of admiration is an intellectual enterprise subject to strict empirical discipline, with room for scientific agnostics and scientific gnostics alike. Admiration for science goes by the classification of what counts as science, and not by what are classified as incidental concomitants.

In conclusion then, I agree to what Rosen says at the end of his section II, with one change – minor typographically, but perhaps not minor with respect to content. “The aim” of attempting to carry through the constructive empiricist interpretation of science

is to show that even though he sees no reason to believe what they say, the [scientific agnostic] need not be driven out from the paradise that Boyle, Newton, Mendel, Rutherford, Bohr and the rest have created. (p. 156)<sup>3</sup>

What hinges on this for empiricism is that this will allow what it takes science to be and what it takes as paradigm of rational inquiry can be one and the same.

#### NOTES

<sup>1</sup> All page numbers cited refer to Gideon Rosen (1994) 'What Is Constructive Empiricism?' *Philosophical Studies* 74, 143–178 (this issue). I wish to thank Gideon Rosen, Peter Forrest, and Martin Jones for helpful discussions.

<sup>2</sup> Rosen's and my own reaction to the idea of constructive empiricism and scientific realism as rival sociological hypotheses, about what scientists actually believe and strive for, should, however, be compared to the papers in the special issue of *The Monist* (sched. Jan. 1994) devoted to science and realism. This includes the paper by Peter Forrest cited below.

<sup>3</sup> Where I have "[scientific agnostic]" Rosen wrote "constructive empiricist".

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