

Realisms and Some Parallel Problems of epistemology and Methodology¹

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“All that is old is not good for this very reason; a work is not flawless because it is new; the wise chooses the one which stands the test of reason; only fools go by what others say” Kalidas.

“A really good scientist is one who knows how to draw correct conclusions form incorrect assumptions” Otto Frisch, *What I Remember* (1979)

“Logic books are divided into two parts: in the first part on deduction the fallacies are described, and in the second part on induction they are committed.” Attributed to Morris R. Cohen.

For years now Professor V.N.Jha, among very few scholars, has been urging, through various activities, people to reappraise the long Intellectual tradition of Indian culture in general and the philosophy of Nyaya, which is acknowledged to be one of the best examples of this cultural trend, in particular. Rationalism consisting mainly in the attitude of criticism including self-criticism has always been the very strong point of Indian culture. But today very few people in India or outside try to understand Indian culture in terms of her own indigenous concepts and categories deriving them from the vast literature of classical India available primarily in Samskrta language. To people like him it strikes as an irony of fate that this Indian culture is known today even in its homeland as primarily a mystical culture. What, apart from Orientalist narrative, has contributed to this is the reconstruction of Indian History in general and history of Indian science in particular by some modern historians and historians of science with known leanings towards Marxist ideology. Many of these scholars work with a conception of rationality or rationalism that is informed by modern European science sans its religious and capitalist background. It is in this context of reconstructing the classical Indian culture (at least in its medieval period) as marked by anti-rationalistic

¹ This paper is largely based on the material presented in the international conference on ‘Karl Popper and Beyond’ organized by the Department of Philosophy, Jadavpur University in December 2002]

trend or by the decline of rationalism that I wish to dedicate the present paper to Professor Jha.²

In a number of publications and lectures over last two decades or so I discussed subjects central to Foundations and methodologies of Theoretical sciences in the Indian Tradition and how to rediscover and correctly project (in terms of indigenous concepts) India's theoretical cultural identity. In all these discussions I defended a version of realism. Popper is also known for his preoccupation with scientific theory and his preference for realism. Of the two major groups of scientists and philosophers of science – reductionists and realists – Popper belongs to the latter. So what I propose to do here can be described as examining how realism-_{PK} stands related to realism-_{KP}. The former version of realism is, as far as I can see, an authentic reconstruction of Nyaya realism which, taken as epistemology, can be best described in the way I did before, that is, as *expansive empiricism*³; and as an approach to science it is best described as rationalism. It reminds me of one of the motivations of Karl Popper to turn his attention to the question of methodology of science. He wrote “A very serious situation has arisen. The general anti-rationalistic atmosphere which has become a menace of our time, and which to combat is the duty of every thinker who cares for the traditions of our civilization, has led to a most serious deterioration of the standard of scientific discussion. It started with the brilliant young physicists who gloried in their mastery of the tools and looked down upon us amateurs who had to struggle to understand what they were doing and saying. It became a menace when this attitude hardened into a kind of professional etiquette” (*Quantum Theory and Schism in Physics*, Vol. III of the *Postscript*, Hutchison, London 1982 p.156). This reminds me of the passage I quoted from an article of Norwood Russell Hanson in my lecture “The Present State of Physics and Philosopher's Worry”⁴. It is this. “It is reasonable, therefore, to suppose that their [informed critics'] dissatisfactions with orthodox quantum theory and with the Copenhagen interpretation of it may reveal weakness in the standard position..... [But] the popular pronouncements of the

² One may profitably consult in this connection the first half of the paper, produced jointly by the author, author, “Creation of Mater: Traditional Indian Thinking and Modern Science” in the journal *Science Philosophy Interface* Vol. No.2 1996 and his paper “History of Science and Two metamorphoses of Mind” published by PHISPC, Delhi.

³ See author's *Indian Realism: A Rigorous Descriptive Metaphysics*, Ch.1 and page 54

⁴ This lecture was first delivered in the Indian Statistical Institute, Kolkata, and later in Jabalpur University.

Copenhagen theorists have often seemed calculated to stifle all opposition as uninformed or archaic, or worse”.

While Sharing some of the major concerns and views of Popper I, as an upholder of a certain version of Indian realism, though not only on that account, have, some serious problems regarding some of his stands. One such concern has just been noted. Popper’s concern at the decline of rationalistic approach among the scientists reminds me that I have been protesting against some of the most widely known views of one of India’s most popular and self styled historians of science, Debiprasad Chattopadhyaya, who seems to swear on rationality. He has said most forcefully in a number of places that the Brahminical sub-culture and some of the philosophies aligned with it are responsible for the decline of science in India; that Indian science was defeated by the capitalist conservative Brhaminical forces. He, with his known and acknowledged Marxist leanings, found Buddhism to be more acceptable than any of the Brahminical philosophies.⁵ Thus he prefers Buddhism to Vedanta idealism (and possibly realistic systems of thought like Nyaya). Joseph Needham has suggested, in his Foreword to the first volume of Chattopadhyaya’s *History of Science and Technology in Ancient India*, that it had proved over simplistic to think that capitalism with its tolerance of religion was inimical to mankind, which needed to be saved by science and atheistic Marxist ideology. I also tried to argue before that Chattopadhyaya’s speculations about the cause of the decline of Indian science are not corroborated by the facts of history of the development of science or the epistemology of modern (Western) science. But today I cannot fail to contrast the Marxist leaning and anti-realism of this historian of science who swears by whatever rationality he adheres to with the three important strands and commitments of Popper: His early disillusionment with Marxism, his commitment to realism and his rationalism as exemplified in his concern at the growing anti-rationalistic attitude in the world of science. And these are among some of my major interests in Popper. I will begin with the last of these points and eventually make some comments on Popper’s views on development of science and his criterion of falsifiability. Both these are somehow related to the question of epistemology and methodology of science. But of all that I say what is directly related to Popper are presumably well known and hence my

⁵ I wish he knew how much Buddhism was indebted to the veda as has been very convincingly demonstrated by Mm Yogendra Nath Tarka Vedanta Tirtha.

endeavour will be to draw the attention of the reader to some relevant aspects of Indian thinking which are either not noticed or misunderstood.

Growth of knowledge is as much a concern of the scientists as of the philosophers. However the first thing that I would like to say is why Popper's view about growth of knowledge seems unacceptable to me. It is not simply because as an Indian I belong to a culture of which, belief in the theory of evolution does not form an inalienable part. Even so the unacceptability has something to do with Popper's reference to evolution in his treatment of the subject in question. The account of knowledge, which Popper as well as Toulmin gives uses evolution as the model. (Popper, K., 'Evolution and the Tree of Knowledge' in *Objective Knowledge*, Oxford, 1972, pp256-84 and S.E.Toulmin, *Human Understanding*, Oxford, 1972, pp136-42). But they are not the only ones or the first ones to say this. Even earlier T.H. Huxley, Herbert Spencer, Ernst Mach and Teilhard de Chardin held similar views. Huxley said in 1880 that is just after 21 years of the publication of Darwin's *Origin of Species* "The struggle for existence holds as much in the intellectual as in the physical world. A theory is a species of thinking and its right to exist is coextensive with its power of resisting extinction by its rivals". And after eighty years Popper said in his 1961 Spencer lecture that the growth of our knowledge "is the result of a process closely resembling what Darwin called 'natural selection'; that is, *the natural selection of hypotheses*: our knowledge consists, at every moment, of those hypotheses which have shown their (comparative) fitness by surviving so far in their struggle for existence; a competitive struggle which eliminates those hypotheses which are unfit". Popper went further and it has been said, "such talk, Popper emphasized, was not meant to be taken metaphorically". Even apart from my culturally determined opposition to evolutionism this view of Popper is unacceptable on various other grounds. But before I discuss some of these, I will note another important difference between the two intellectual cultures in question.

Popper belongs to a culture that is marked by a belief in the opposition between science and religion, which contributed to the growth of modern philosophy or the central part of its epistemology or first philosophy. Strangely enough this civilization is also marked by another belief regarding conflict between science and philosophy as one of the pseudo sciences. When philosophy of science and methodology separated themselves from general epistemology the second of these conflicts was the matter of primary

concern. But in India not only sharp dichotomy between science and philosophy was never drawn but also fact or thought of such conflicts were unknown till at least the advent of the West. So one should not be surprised if methods of philosophy and science, if there were such things, were, conceptually speaking, more alike in India than in the West. And it may be argued that even if some difference is there that has been overemphasized in the West.

Let us turn to the question of the evolutionary model of growth of knowledge. It has been said that “It is difficult to conceive of a more wildly inappropriate manner in which to talk of the survival of theories and hypotheses.” In case of biological species we can hardly make sense of such remark as species which did not in fact survive should better have survived. And in case of certain organ which a certain species developed in course of time we cannot make sense of the remark that it does not make the species any the more fit. For survival and fitness are coextensive in the realm of biological evolution. But the same does not hold in case of theories. “Theories differ. They may survive for social reasons, through propaganda, censorship, and other political tools. Alternatively, they may die out not because they are unfit, but through stupidity, prejudice, and corruption. It is legitimate to say of a hypothesis that, though it has survived a long time, it should have been discarded long ago; equally, we can say of a theory which failed to survive, that it should never have been rejected. Less, if any, sense is however made of the claims that the trilobites should have survived, and lizards should have long been extinct. The truth is that fit theories can die and unfit ones survive. The same cannot be said of organisms within the confines of Darwinianism” (Gjertsen, D., *Science and Philosophy* etc. pp. 137-8). Secondly while species that are extinct cannot be revived there are innumerable instances of loss and recovery or rediscovery of theories. Just two examples may be given. The theory of heliocentric planetary motion proposed by Aristarchus and others was “found under consideration 1500 years later by Copernicus” Keeping in mind how Popper was impressed by certain remark of Einstein, which however he seems to have over interpreted, I would like to quote a passage from a paper I jointly authored in 1996. (“Creation of Matter” etc.) “Historians of science know that there are too many examples of such loss and recovery of insights and theories. This also strengthened the practice of not claiming finality in favour or against any theory. History shows us that it is always risky to reject or accept any theory finally, for the future is unknown and science is progressing. How the particle theory would be judged on the

basis of current state of knowledge 50 or 100 years hence nobody can judge today. Classical habit and practice was to model matter in terms of particles. Around 1960 an ad hoc alternative model was proposed; matter could now be modeled in terms of string. (Superstrings theory may be traced further back to 1919 and to Theodore Calisaya) The limitation of this model was known even before 1974 but in this year string theory ceased to be of interest even for modeling some of the strongly interacting particles. The string theory or the very idea of string model would have died if some small group of physicists did not continue to work hard with this model ignoring suggestion of many that they were merely wasting their time. But now, thanks to the work of those few determined researchers, string theory in its modern form – or superstrings theory – commands attention of a sizable section of physicists.” Thus while it is all right to say about a certain species that it was unfit to survive, we should exercise more prudence when we speak of theories. The latter, unlike species, can be dormant. Some times a third consideration is also advanced. For one who accepts the perspective of evolution nothing is entirely novel. Species have ancestries; they do not emerge from nothing. Sometimes theories are and can be absolutely first – completely original, unanticipated and without ancestors

Lastly, Popper was misled by his adherence to the evolutionary model to think that as in the case of the biological species so in case of theories competition leads to improvement and enhances chance of survival. But the fact is otherwise. When theories conflict it is not always the better that survives or the weaker discarded. History shows that the better theory of Aristarchus remained ignored for nearly one and a half century after which Copernicus rediscovered it. Ptolemy’s inferior theory ruled for over 1500 years before it was finally discarded. “Galileo lost to the Church, and generations of past anatomists have argued that black men and all women of any society are the intellectual inferiors of white men.” Again competition may fail to resolve the issue of survival. ‘One niche one species’ rule holds for species but not for competing theories. “Today belief in astrology is held as widely and as firmly as belief in astronomy. Indeed they have happily coexisted for well over two millennia.”

It cannot be that these historical facts are not unknown to Popper. But what can he then mean by survival of competing theories or evolutionary model of growth of knowledge? Before answering this question I would like to revert for a while to his concern for rationality. I would like to first supplement Popper’s remark in this regard by noting what two other scientists have to say in this matter. They also note with dismay

the decline of the culture of rational approach among the scientists and categorically say that irrational or non-rational factors contribute to the actual survival or extinction of scientific theories. One of these scientists is J.V. Narlikar, a famous astro-physicists of India. I had occasion to contest some of his views on historical and cultural matters. In that connection I noted some frank confessions he made about the state of rational culture among modern scientists. I found that the situation was better in classical India or classical Indian theoretical culture. Incidentally my paper under reference was written partly as an answer to Narlikar who questioned the value of studying the views of past thinkers such as classical thinkers of India. While defending in that article the need for studying past wisdom, I also emphasized that the coexistence for centuries of competing philosophical systems in India is to be authentically understood as a commitment to norm of rationality. But I will return to this later. First we would like to note what Narlikar said about the decline of rational culture. He said “Scientists do not know the answers to puzzles like these which may call for radical rethinking. Indeed, howsoever objective they may call themselves, the practitioners of science tend to be reluctant to face new evidence that forces them to revise their ideas. Thus many of the puzzles are simply wished away.” I refer to another physicist, F.Sellari who is among few working scientists who have extensively and approvingly quoted Popper and welcomed his anti-dogmatism, anti-positivism, anti-subjectivism and pro-rationalistic stand. Selleri in a paper published in 2000 writes “it is difficult to be optimistic about the near future because the scientific community has become very conservative as far as the theories of modern physics are concerned”. He has given many examples to show how powers that be in the scientific community did not allow experiment or theory against the established and official theory.

Now the moot question is what Popper could have meant by saying that among the competing theories some survive by what may be called natural selection? Since he knows the historical facts to the contrary to which we made reference above, we should not, I suggest, take him to be making a factual point. His point seems to be normative. In an ideal condition where extraneous consideration or irrational factors do not operate the survival value of a theory depends on its power to withstand competition or crucial test. This brings us to the question of epistemology and method. And here too I will have to

state a few known things in order to make my case. And I begin from the second difference noted before between the concerned intellectual cultures.

If in India quest for spirituality and darsan developed simultaneously and in relation to each other [which should not be interpreted as mystifying knowledge but as rationalizing spiritual quest or even religion], in modern Europe science and philosophy developed together. Among the trinity of modern Western or European philosophy logic is itself a science and epistemology is in service of science. Though epistemology is contemporaneous with modern science yet philosophy of science or methodology of science separated itself from general epistemology quite late. And even after the death of this epistemology was declared and the monolithic conception of knowledge as science was replaced by the conception of holistic nature of human theoretical enterprise, philosophy remained in the service of science. If only critical philosophy was found to be unobjectionable philosophy which clarifies the concepts of science, logicians are said to be active in disclosing the theoretical underpinning of science. Epistemology or modern epistemology at any rate started its career as a project to demonstrate that modern science generated knowledge proper and ended with the grand conclusion that knowledge and science are interchangeable and that science constituted the whole of human knowledge.

It was came to be believed around this time that behind the success of science is the method it employs: the scientific method. In formal sciences it is strictly the deductive demonstration and in empirical science it is observation and experiment along with inductive justification. [We will speak of induction and reason later]

Though today there are many who do not believe either that science is the totality of human knowledge or that there is any such thing as *the* scientific method yet that did not affect much the continued interest in induction among philosophers. The problem of induction and that of scientific method are closely connected with the possibility of science and its distinction from pseudo science. And this is a problem of abiding concern for the scientists, as well as the philosophers and finally of the general literati of the modern age and culture of science. We can even say it is still an obsession with the European majority.

The problem can be situated or even formulated in different ways. In one way science is not only a certain body of established knowledge at any given time it is also a way of generating (ever) new knowledge or expanding man's knowledge. Thus knowledge from mathematics or logic to astronomy and from astronomy to mechanics

from mechanics to biology to psychology and economics scope of science is ever expanding. What science risks in this expansion is its rigour; on the one end of this expanding horizon of it or, better, of human experience, there is pseudo science. Thus the problem of demarcating science from pseudo science is not a onetime job. But the question is whether in different times it is a matter of repeated application of the same criterion or is it that the criterion itself changes? Is there any prospect of Phrenology becoming a science in future, or astrology or parapsychology? The problem here should ideally be a real difficulty to strike a balance between the rigour and scope. But not infrequently it turns out to be a conflict between credulity of the masses and the indefensible bias on the part of scientists who otherwise claim to be committed to the norm of rationality. We have witnessed in recent times strong opposition to include astrology in the university syllabus by the faculty to whom psychoanalysis is well accepted as a course of study, though these two (perhaps along with Marxism) have been found by Popper to fail equally the falsifiability test of science. Again Kant maintained in his *Metaphysical Elements of Natural Science* that empirical psychology could never be included among science proper. It may be thought that psychology is an ambiguous expression; psychology, which has since received the prestige of science is experimental psychology which did not exist in Kant's time. So it is believed that experiment is all what makes the difference. We will come back to this presently. But first I would like to say what decides is often the chosen paradigm or sheer politics or authority. Thus it has been said that there "is a sort of paradigm of 'the scientific method' (a paradigm which itself is pretty... vague) which one occasionally finds pretty well exemplified, especially in Physics. But even in physics there is a great deal of knowledge which doesn't, and shouldn't, fit the paradigm. I don't believe that there is *really* an agreement in our culture as to what is a 'science' and what isn't. Any university catalogue claims that there are subjects called 'Social Sciences', and that Sociology and Economics are sciences. But I would bet if we asked anyone in the Physics Department whether Sociology is a science he would say 'No'". (Putnam). As to how Peers community (authority), politics or power plays a role in deciding what is and what is not a science has been discussed in many works including the articles by Selleri and Narlikar to which I referred above. But we have already said whatever the fact of history may be we are concerned with developing a theoretical criterion to distinguish between science and pseudo science. And Popper found it to be the criterion of falsifiability and not verification. But for situating properly

Popper's position one needs to discuss Popper's view of induction. Fortunately most of the relevant points are well known though need always remains to discuss in any given occasion even the otherwise known things in respect of the special relation they bear to a group of other concepts relevant in that context.

II

What I will do in this second part is an extension of what I said in 1984 in the third chapter entitled 'Sense Science and Universals' of my book *Indian Realism: A Rigorous Descriptive Metaphysics*. The point simply is this. Just as Descartes created the problem known as the problem of the knowledge of the external world which is also a problem of the possibility of science, so also and unrelated to it Hume's empiricism and his view about induction created the problem of possibility of empirical science. Incidentally as we will see these two problems of possibility of science and demarcation between science and pseudo science have become closely connected in the thoughts of Popper. In any case, since there is no generally accepted solution of the problem of induction or answer to Hume's critique of induction, the problem of the possibility of science remains still unresolved. It is to be noted, however, that the working scientists do not seem to be aware of or bothered about this problem. How otherwise a lot of contemporary scientists beginning with Ernst Mach could consciously align themselves with Humean empiricism and phenomenalism which the realists so forcefully denounce? Add to this the fact that science has been making phenomenal progress even as the philosophers are debating over its possibility. This situation has been aptly formulated by C.D. Broad in his remark that "induction is the triumph of science and disgrace of philosophy." Obviously Broad takes induction to be the effective method of science. Be that as it may, this remark of Broad should appear to philosophers very difficult to swallow. But it is equally difficult to legitimize the conceivable stand which, if we take, may disprove the conjunctive statement in which Broad formulates his remark. We may formulate the stand weakly or strongly by combining the following two statements disjunctively or conjunctively as the case may be. And the two statements are: first, that induction is behind the possibility and progress of science is a myth, and the second, now at least we have solved the problem of induction. The problem about the first statement is that not only there are many inductivist among scientists and philosophers of science but

also or because of that even if false the myth of inductive method has become firmly entrenched in the intellectual culture of modern Europe or, under its influence, of the whole world. So far as the second statement is concerned two different claims re there that pertain to two different solutions, which are claimed to have been found, to the problem of induction. If Popper and his followers claim to have found negative solution to the problem that there is no such thing as induction, Carnap and his followers claim to have found a positive solution of the problem in that Carnap has developed his own (version of) inductive logic. Thus the problem of induction will continue till the dispute between these alleged alternative solutions is resolved. Another point should be noted. The disagreement between Popper and Carnap relates to induction understood as a method of justification of already available hypotheses. But they seem to agree on the point that there is no inductive discovery procedure. I would like to discuss later, if possible, with reference to analogous problem in Indian realism that from their pramana-theoretic approach the realists believe that there is both a logical discovery procedure and that there is a logical method of justification of general proposition.

To return to Popper; he thinks that the difficulties pointed out by Hume regarding induction is fatal. We should take Hume to have shown conclusively that neither of the two claims – that induction is a method of discovery or that it is a method of justification – can be rationally upheld. Popper has gone a little further (or so Thinks Stegmuller, W., *Collected Papers etc.*, Vol.II. D. Reidel 1977 p.76.) “According to him Hume has conclusively proved that there can be no rational justification of induction and, furthermore, that the concept of inductive inference is a pseudo concept: induction is an illusion.”

The denial of induction, formulated in this way, may invite the question ‘how this denial can itself be is justified’? In the realm of formal theory there can be demonstrably indemonstrable statement. But the denial under question is supposed to be empirical general statement: All inductive inferences fail to satisfy the criterion of inference. We cannot narrow down the scope of Hume’s or Popper’s denial. “In order to correctly assess the meaning and significance of Hume’s *problem of induction* it must first be formulated quite generally as has been done for example by Wesley Solomon. This means that it must not be prematurely narrowed down to particular forms of ‘non-demonstrative inference’ such as, for example, induction by enumeration; for in the case of any specialization there lurks the potential danger of being accused of failing to take

sufficient notice of such and such non-demonstrative inference; one is then told that *true* induction lies in the very kinds of inference he happened to neglect” (Stegmuller. P.69). If for the moment we decide not to follow this line of argument against Popper’s denial of induction and if further for the moment we ignore the rival claim of Carnap and his followers then since Popper has shown that there is no induction Broad’s remark stands refuted. The so-called problem of induction is solved.

But Popper’s problem begins just here. Popper’s problem is not just the problem of induction though it results partly from Hume’s solution or dissolution of the problem of induction. Induction may be a pseudo concept but science for Popper is not a pseudo concept. Not only there is such a thing as science but also we find phenomenal progress which science has made in recent times. And largely such progress has been made possible by rejecting and selecting hypotheses at each stage. How is this done? There must be some method. We have seen above some of the scandalous ways in which such selections are actually made. But we also remarked there that Popper’s problem is not historical or theoretical but normative. The question is not how actually selection is made but how it can or should rationally be made. And Popper will agree that even if there is no induction there must be a method by which scientific hypotheses can be tested so that in virtue of the result of such test they can be either rejected or at least provisionally accepted, as the case may be.

Scientific hypotheses to be justified can be either law-hypotheses or statistical hypotheses. We need to distinguish between these two types of hypotheses since a failure to do so has led many people to misunderstand the nature of corroboration. Be that as it may since there is no induction the law-hypotheses cannot be justified inductively. And unless we assume the universe to be finite and the possibility of expansion of quantifiers we cannot deductively justify law hypotheses. For being universal of unlimited scope they cannot be derived from any number of singular statements. They cannot be conclusively verified. In other words, scientific law-hypotheses cannot be empirically verifiable “since the experiential basis at our disposal is, and will always remain, finite. Down to the end of time mankind cannot definitively establish the truth of even a single scientific hypothesis. If the expression ‘knowledge’ is taken in the sense of definitive knowledge (*episteme* in contrast to *doxa*) then there is no knowledge in the empirical

sciences” Stegmuller P.76). Or, as Popper said, “We do not know; we simply guess.” (L.F.3rd ed. 1969 p.223).

But then the problem of demarcating science remains unsolved for metaphysical statements are equally empirically unverifiable and hence by the same logic they are also conjectures. How to distinguish empirical scientific conjectures from metaphysical conjectures? A likely reply is that metaphysical conjectures will ever remain conjectures since they will not or cannot be put to any sort of empirical test. Scientific law-hypotheses however can be falsified; “they can be refuted by particular facts.” “The *potential empirical falsifiability* or empirical refutability is thus the sole criterion by which empirical hypotheses are demarcated from metaphysical conjectures.” Popper said, “It must be possible for an (empirical) scientific system to be refuted by experience.”

Two questions remain. First is what exactly is the actual procedure of this sort of empirical test. It cannot be the procedure of showing the falsity of the statement, which can be got by the rule of universal instantiation; but the falsification employs still the deductive rule of inference called modus tollens. Another question is why we cannot say that the falsity of one hypothesis confirms or verifies deductively the opposite hypothesis if it is opposite? The other question is more important we have here a way of proving the scientific character of a certain hypothesis but what about the question of justification of hypothesis in the sense of rationality of choosing one among many others? All scientific hypotheses are potentially falsifiable. We can also find one that is actually falsified; but then there would always remain many unfalsified ones. Are they for that account not scientific and therefore not to be counted among competing hypotheses? But that will be unrealistic, to say the least. So we must follow as criterion of falsifiability and not actual falsifiedness. But how to choose in a particular domain of objects one particular unfalsified but potentially falsifiable hypothesis from among other such competing hypotheses. The question of choosing the actually falsified hypothesis does not arise. If now we cannot choose any one of the falsifiable hypotheses then we cannot rationally select. Are we to go then by non-rational factors like prejudice, power etc.?

Popper needs a concept of deductive confirmation in addition to the concept of empirical falsifiability. His concept of corroboration is just such a concept. But this corroboration is not just a matter of positive test result. If a prediction or prognosis on the basis of certain hypothesis together with initial conditions is realized, that is, as it is

usually said, if the test result is positive why can we not say that the hypotheses is confirmed?. We know that Popper looks at the situation or interprets it in a different way. But no clear or good answer is given why he prefers to do so. Let me quote from an article, which is to my mind the most insightful, detailed, careful and clear account of the problem of induction with reference to Popper and Carnap. “If what is predicted does take place, one usually says that the test has *turned out positive*. This manner of speaking tends to create misunderstanding for it suggests we have here found a *positive instance that supports the hypothesis*; such an idea then leads immediately to the further idea that given an increasing number of these positive instances the hypothesis will be increasingly supported and confirmed. Popper rejects such a line of thought: the so-called positive outcome of a test is in fact nothing else but an unsuccessful attempt at falsification; we have not succeeded, he says, in contradicting the hypothesis; we have failed in our attempt at having the hypothesis falsified by experience.” But in passages like this we are told what Popper thinks and says but not why he does so or what justification he has. At this point I would like to return back to the Indian realists, with whom I align myself, and digress to some relevant views of them.

I have repeatedly said in many places that Indian theoreticians were not found seized with the idea of absolute generality or certainty. They go by optimality rather than unlimited generality. Any explicit statement to this effect is hard to come by in the literature of classical Indian philosophy or science. But if we look at the structure of theories that are actually there and in which their conception of theory is enshrined one can hardly avoid such a conclusion. This structure clearly shows that they realised and not simply recommended that theories are open. Two of the most rigidly formulated Indian theories are the linguistic theory of Panini and the metaphysical theory of category of the Nyaya- Vaisesika philosophy. With reference to both these and many others it is possible to substantiate the view that Indian theoreticians were not seized by the idea of unlimited generality and absolute certainty, at least, in empirical matters. Their theory remains open keeping room for future growth and progress. But still in their theory of inference or inferential justification the Nyaya philosophers face the problem that is similar to the problem of induction at least as they are usually formulated.

(1) What is the principle of induction (or if one believes in pluralism then, what are the rules of inductive inference)? (2) How can the principle of induction (the rules of induction) be justified?

If we replace in these two interrogative sentences the word induction by simply inference then we get the questions with which the realist logicians of India were challenged. In this context at least they are found to acknowledge that however great may be the number confirming instances or positive test results they cannot confirm the general principle of inference. But such a principle and for the matter of that any general empirical statement or belief can be falsified by one single falsifier which therefore outweighs any number of positive instances. It is in this connection that they came up with an answer to the question why one should not interpret a positive instance as partially confirming but only as, as Popper says, an abortive attempt to falsify the general hypothesis. The answer in brief is that no sense can be given to the concept of collective confirmation. Positive instances do not add up to increasing support of the hypothesis or general statement being tested. If Popper could say some such thing then he could justify his way of interpreting the positive test results. It may seem that it should be rather possible for Popper to admit additive value of positive test results since he does not think that empirical science gives us definitive knowledge. For him science yields only conjectures, may be rational conjectures; besides he believes in probability as per his own propensity theory. Indian realists not only believe in the possibility and actuality of definitive knowledge they also hold that inference does yield definitive knowledge (though occasionally it yields 'definitive' falsehood also). Thus if they do not admit of additive value of supporting or positive test results that has nothing to do with emptiness of the concept of definitive knowledge. It is for this reason that the attempt to treat Indian realist's inference as inductive is as much misconceived as to treat it as syllogistic. Whatever that may be they envisage and entertain what may be called the problem of justifying the principle of inference corresponding to the problem of justifying the principle or principles of induction; and since such a principle is also formulated as empirical universal statement it raises the usual question about the very possibility of such justification. And Indian realists agree that even a single falsifier can falsify such a statement. Negative instance is so far stronger than any number of positive instances. What is interesting is that these realists also hold that it is possible to confirm such a universal statement. We will presently attempt a short exposition of their notion and requirement of confirmation of such statements. But before that we may as well return briefly to Popper's actual position.

In continuation of his own way of interpreting positive test results Popper solves the problem of choosing from among possible unfalsified hypotheses by introducing his concept of corroboration. This has been variously criticized. Some have found that with the introduction of this notion Popper brings back covertly which he overtly denounces, namely, induction. But as is well known his notion of corroboration which essentially rests on the notion of content is in reality a matter of deductive confirmation or better non-inductive confirmation. What he means is this. Objective choice from among many possible hypotheses is made possible by the fact that these hypotheses differ in respect of their empirical content. Greater the class of basic sentences, the possible falsifiers, a theory precludes greater is its content. Thus content of theory is determined by the extent of the class of falsifiers. "Greater the content, the lesser the logical probability of the theory." More contentful a theory is it is more vulnerable. And this fact acts as objective ground for choosing a theory not in the first instance for accepting it but for putting it to strictest possible test. If the theory fails the test or gets falsified then we reject it; if, however, it survives the test then it is provisionally accepted or corroborated. So in Popper's system there is no such thing as absolute acceptance or closed theory. In the Indian realist's scheme of things though there is such a thing as proving a theory or a general statement they admit almost as a rule that every acceptance is a provisional acceptance. If this amounts to saying that the proof is not deductive then that is welcome or at least not a matter of concern. We are forced to accept a general principle of inference because we can perceptually discover it, we can force its critics to accepting it, and finally show that there is some practical necessity. Highly technical and detailed discussions of each of these points are available in the literature. But it is not possible to reproduce them here. The major point is that for (positive) instances or test results to have additive value it should be possible to combine them. But though we can observe in succession a good many number of positive instances we can observe only one at a time and experiences are short lived and cannot combine. It is not therefore possible to have many confirming instances in the required sense of the term or we cannot make any good sense of the claim that in some cases we have more number of positive test results than in some other cases.

It is to be noted that Popper's notion of corroboration is in need of fuller and clearer explication, which it unfortunately lacks. Whereas in Indian realism the criterion of non-inductive acceptance has been worked out in greater detail. But unfortunately we

cannot go into the details of this. Instead we should take note of the exposition of scientific method as developed by classical thinkers and philosophers of India and discussed by some of the modern Indian scholars. The following passage, we quote from the work of a great scholar, is perhaps the briefest and most comprehensive description of the method that the Indian scientists and philosophers used to employed in their theoretical enterprise.

“The scientific methods already noticed – the Joint Method of Difference (the *Pancakarani*) and the Joint Method of Agreement (*Vyaptigraha with Upadhisankanirasa and Tarka*) – are not the only methods of ascertaining causality or concomitance, or establishing a theory (*Siddhanta*); nor are these Methods always practicable. Very often we reach the explanation of a fact (*Upapatti*) by means of a hypothesis (*Kalpana*) properly tested and verified (*nirnita*) A legitimate hypothesis must satisfy the following conditions: - (1) the hypothesis must explain the facts (*drstasiddhi or upapatti*); (2) the hypothesis must not be in conflict with any observed facts or established generalizations (*drstasiddhaye hi adrstam kalpyate, na drstavighataya-Jayanta..*), (3) no unobserved agent must be assumed where it is possible to explain the facts satisfactorily by observed agencies (*yadi adrsatmantarena drstam na siddhati kamamdrstam kalpyatam, anyathapi tadupapatau kim tadupakalpanena* (ibid), (4) when two rival hypotheses are in the field, a crucial fact or test (*vinigamaka, ratio sufficiens*) is necessary; the absence of such a test (*vinigamanaviraha*) is fatal to the establishment of either; (5) of two rival hypotheses, the simpler, i.e., that which assumes less, is to be preferred, *ceteris paribus* (*kalpanalaghava versus kalpanagaurva*); (6) of two rival hypotheses, that which is immediate or relevant to the subject matter is to be preferred to that which is alien or remote (*pradhanopasthitattva*); (7) a hypothesis that satisfies the above conditions must be capable of verification (*nirnaya*) before it can be established as a theory (*siddhanta*). The process of verification of a hypothesis consists in showing that it can be deduced as a corollary from (or is involved by implication in) some more general proposition which is already well established (cf Vatsyana’s exposition and illustration of verification, *nirnaya* – including both deductive Method and colligation)”.(Brojendra nath Seal, *Positive Sciences of the Ancient Hindus*)

The reason why I quote the above passage is quite obvious. Both the content and the wordings made the passage most relevant. Even so there are points where I do not agree with the author’s interpretation of some of the passages of theoretical

literature of classical Indian thought or his selection of English expressions. Further the quoted passage should be taken only as supplementing some of the things I have written above. But it does not note, let alone shed new light on, the most crucial point of additive value of positive test results. But Seal had no knowledge of Popper and he did not mean to write the above passage as a response to the position held by others in similar matters. The other dismaying point is that Seal did not think of coming out more clearly on the issue that it would be a matter of forced interpretation if one chooses to present the inference or logic of the Naiyayikas to be either deductive or inductive. But it is unmistakable that Seal had an insight into India's indigenous notion of rationality, which he describes in the above passage with reference to the parallel notion of modern Europe. My purpose has been to identify certain definite problem which the intellectual culture world over faces today and show that we can derive some useful insight from the literature of the intellectual tradition of classical Indian culture that may prove effective if one wants to in certain to the treatment of which some useful insight which an

Anyway, India has her own indigenous notion of rationality, which is not derived from the modern European science that has a history of hardly four centuries. But nowadays scholars of India and outside have become conditioned to think that and also conclude it is pathetic to see today that India, the inheritor of the legacy of a great rational culture, has so much distanced herself, owing to historical contingencies as are well known, from her own tradition of intellectual culture that she that of rational culture.

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