'Since the end of World War II, I have been working on the many ramifications of the *theory of messages*. Besides the electrical engineering theory of the transmission of messages, there is a larger field which includes not only the study of language but the study of messages as a means of controlling machinery and society, the development of computing machines and other such automata, certain reflections upon psychology and the nervous system, and a tentative new theory of scientific method. This larger theory of messages is a probabilistic theory, an intrinsic part of the movement that owes its origin to Willard Gibbs.

Until recently, there was no existing word for this complex of ideas, and in order to embrace the whole field by a single term, I felt constrained to invent one.' [50j, p. 15]

These words suggest that a better approach is to treat cybernetics not as a science *per se*, but rather as a *movement within science*, which draws attention to and focuses on the stochasticity of the cosmos, and on the new concepts that stochasticity allows, such as communication, information, teleological machines, regulation, intelligence, etc. (*cf. Curr. Sci.*, 25 December 1994, pp. 919–920). Calling Leibniz 'the patron saint of cybernetics', Wiener felt that all scientific attempts to absorb mind into nature, such as studies of machine intelligence (chess-playing automata), language, and animal and human intelligence, should come under the rubric 'cybernetical'. The central concepts are 'message' and 'machine' (or monad or black box), i.e. transformer of messages; 'feedback', while important, is not quite that central.

From this perspective, the lines Narasimhan has drawn between the three approaches to human understanding, viz. the cybernetic (in a narrow sense), the artificial-intelligence, and the connectionist, appear to be too sharp. These approaches should be seen as steps in an ongoing effort, that is cybernetical in the broad sense. This effort, the ultimate aim of which must be the understanding of the entire human being, should recognize that this being is 'the most complicated object under the sun', as von Neumann put it. Today this effort is rightly focused on understanding the 'Homo sapiens–faber' part of man. However, his conspicuously active but more difficult 'peccator' part should not be entirely overlooked.

1. Kalman, R. E., Falk, P. L. and Arbib, M. A., *Topics in Mathematical Systems Theory*, McGraw Hill, New York, NY, 1969.
2. Wiener, N., *The Human Use of Human Beings*, Houghton Mifflin, Boston, MA 1950, Da Capo Series in Science, Plenum, New York, NY, 1988

P. R. MASANI

Department of Mathematics and Statistics, University of Pittsburgh, Pittsburgh, PA 15260, USA.

Comments on the review of the monograph *The Scientific Methodology in the Light of Cybernetics* [*Curr. Sci.*, 1994, 67, 954–956]

Narasimhan's review contains misunderstandings as to what the term 'scientific methodology' means, and unfounded charges of omission and of exaggeration on my part.

The scientific methodology (SM) is the body of principles governing the method of inquiry used by scientists both to acquire knowledge about the world and to apply it practically. It is intended to provide the practising scientist with enough principles to pursue his craft vigorously, and to offset any doubts he may entertain as to its worthiness. The SM has, first of all, to state what it is in the world that makes science possible, and to describe its classifications and its different aspects, such as experimentation, reasoning, concept formation, mathematicization, learning from the past, and the mode of discovery. Secondly, the SM must say how the notions of message, communication, intelligence, teleology, contest, etc., which until fairly recently were left out of the scientific arena, are scientifically demarcatable. Thirdly, the SM must expose instances of faulty conceptualization. Fourthly, the SM has to say how scientific knowledge is related to the knowledge of craftsmen, philosophers and religionists. Finally, the SM must comment on how science bears on human welfare. Let us from this standpoint judge the blemishes that Narasimhan finds in his review [R] of the monograph [M] in question. They fall under 14 heads, which I have labeled I, II, ... ,XIV.

I. In [R, p. 954, para 2], Narasimhan writes that the following have not been addressed in [M]:

'(i) What is science about? (ii) What is the logical structure of science? (iii) What are the distinguishing characteristics, if any, of the activities that create science? (iv) What is the difference, if any, between scientific knowledge and commonsense knowledge? (v) Are the structures of these two kinds of knowledge similar? (vi) Do these two kinds of knowledge relate to the same world? (vii) What is the relationship between the language of commonsense and the language of science?'

Of these, (vi) is nonsensical since there is only one kind of true knowledge, that of the one world in existence. Of the rest, (i) is a little too vague to be of much significance. However, an adequate answer, to wit, *science is about understanding the world around us by the method of combining logic, mathematics and observation, which came with the Pythagoreans*, is latent in the following passage:

'The purpose of the enterprise throughout has been to understand the world around us. However, what made the work of Newton possible was the methodology of combining mathematics

and observation which came with the Pythagoreans, and the consistency with which the great successors of Pythagoras have practised it.' [M, p. 19]

The question (ii) is, of course, most germane to the SM, but it is already addressed in [M, §3]. The logical structure rests on Einstein's dictum: 'As far as the laws of mathematics refer to reality they are not certain; and as far as they are certain, they do not refer to reality', and can be summed up in the four basic principles:

PI : *All the statements of empirical* science, including statements of natural laws, are uncertain hypotheses.

PII : (a) The truths of logic and mathematics are devoid of factual content; (b) their certainty stems solely from the rules of language.

PIII : The truths of logic and mathematics provide the deductive vehicle which allows the transition from general hypotheses to empirically testable statements.

PIV : The concepts and hypotheses of science are objective in that each has a content exceeding that of observational terms or statements. But these concepts and hypotheses are not unique, and the choice of one set in preference to another is a human imposition. The imposition has been found to be inconsequential: the resulting theories most often turn out to be equipollent. Finally, the judgement of the significance of the observational errors is made by human convention.

Narasimhan does not say where these principles fall short, or what has to be added to complete them, that is not already in [M].

The question (vii), again germane to the SM, is addressed in [M, §4]. The answer in brief is: *the useful concepts of everyday language (e g. blue, hot, angry) are refined and elevated into the scientific vocabulary by a process called explication* (Carnap's term). The question (iii) is somewhat vague. Obviously, eating or sleeping are not scientific activities (although practised in moderation, they are needed to 'create science'), and, obviously, observing, experimenting, making, concept forming, deducing, and abducting are. The question (iv) has the easy answer: *scientific knowledge is a refinement of commonsense knowledge.* As for question (v) the term 'structure' is not applicable to 'commonsense knowledge', until we know what the latter means exactly. Hence, as it stands, (v) like (vi) is meaningless.

To sum up, Narasimhan is wrong in saying that the two questions (ii) and (vii), which are germane to the SM, are not addressed in [M], and is himself at fault for formulating two, viz. (iii) and (iv), which are trivial and easily answered by what is in [M], and two more, (v) and (vi), whose meaningfulness is in doubt.

II. In [R, p. 954, para 3] Narasimhan states that I do not discuss in a 'systematic manner' the relationship between *pre-scientific* and *post-scientific technologies.* But what is germane to the SM is not this relationship but the one between craft (i.e. pre-scientific technology) and science, and on this enough is said in [M]. First, the role of the craftsman is emphasized in a paragraph with the heading *The Craft as Starting Point*, wherein is emphasized the input of the craft in leading to sound concept formation and thereby to the initiation of new sciences, for 'the craftsman intelligently cooperates with nature' [M, p. 35]. Such input continues to this day [M, p. 51]. Secondly, the craftsman is invaluable to science in providing good apparatus to the experimenter. Indeed, 'for a science to emerge, either Nature has to be generous in providing enough clues to the inquirer, or else a craftsman has to give him what he may need in order to experiment intelligently' [M, p. 20]. Briefly, the SM 'rests on man being both *Homo sapiens* and *Homo faber*' and '... without the craftsman, the scientist is lost' [M, pp. 50, 51]. As to the differences between the earlier (pre-scientific) technologies and science-based technologies of the last 200 years, the answer obviously is that the latter are on the whole much superior, especially in the military field. Consequently, societies that are unable to shake off the former and shift to the latter get dominated by those that do – a fact important for human history but not directly for the SM.

III. In [R, p. 954, col. 1, para 4] Narasimhan writes that the issues discussed in [M] (summed up in para 2 of this letter) have only 'a tangential bearing on ... the SM', and in [R, p. 954, col. 2, para 2] he suggests that I have tried 'to project him [Wiener] as a man for all time'. But until Narasimhan can substantiate these charges by citation of page and para from [M], neither claim nor suggestion can be taken seriously. His remaining comments in these paragraphs as to who influenced me and what I did for 40 years are irrelevant to the SM.

IV. In [R, p. 954, col. 2, para 3] Narasimhan speaks of my problematic 'attitude to cybernetics as a framework for analysing purposeful behaviour in animals and humans' but without saying what this attitude is. In [M] the noun 'cybernetics' hardly ever appears after the Preface, although the adjective 'cybernetical' does. In the Preface [M, p. 8] the 'cybernetical perspective' is described as one in which the concepts of *message, communication* and *control* are paramount. True, that on [M, p. 82], I give the Bigelow–Rosenblueth–Wiener definition of a *teleological mechanism.* But on the same page I also point out that there is an 'important difference between engineered teleological mechanisms and living systems'. Thus, this 'attitude' is some ghost of Narasimhan's own making, which needs exorcising*.

V. In the same paragraph Narasimhan goes on to say that I overstretch the 'scope of cybernetics' and 'trivialize its technical terminology' by stating that in social undertakings 'moral evil should be construed as teleological noise'. But what is incorrect about this statement? The following illustration taken from a recent lecture makes its truth crystal-clear:

'Consider a department store as an input–output transducer, the output terminals of which are sales desks operated by well-paid clerks. The clerk weighs a pound of candy for a customer, but pilfers a few pieces while packaging under the counter. Obviously, such pilfering has a dissipative effect on the successful operations of the store, and thus plays a role analogous to that played by natural noise in, say, an electronic transducer. Unlike natural noise, however, this new 'noise' of human origin, is teleological. It is created in order to fulfil a purpose, viz. to pilfer, and this desire to pilfer is a simple example of the moral evil. The filtration of this noise, unlike that of natural noise, will involve a contest between the sales clerks and the store owner and his detectives. We are led to the following:

*In the 25 December 1994 issue, Narasimhan has written an interesting paper, but it is marred by his use of the term 'cybernetic system' without saying what it means (see the previous letter)

Triviality: (a) In social undertakings the dissipation or noise caused by moral evil is teleological; (b) the filtration of natural noise is contest-free; the filtration of teleological noise requires contest.'

These issues are clearly latent in Wiener's writings on noise and evil.

VI. In the same paragraph, Narasimhan writes that I make 'exaggerated claims' in calling the study of the genetic code, molecular biology and genetic engineering 'cybernetical territory par excellence' [M, p. 83]. But again he is wrong, for these subjects are heavily involved with communication (information), control, and (other Wiener favourites such as) teleology, Maxwell's demons, self-organizing and reproducing mechanisms, as a glance at the Contents of Monod's *Chance and Necessity* (1970), Chapters I, III, IV, shows:

I Of strange objects: The natural and the artificial ... Difficulties of a space programme ... Objects endowed with a purpose ... Self-constructing machines ... Self-reproducing machines ... Strange properties: invariance and teleonomy ... The 'paradox' of invariance – Teleonomy and the principle of objectivity.

II Maxwell's demons: Proteins as molecular agents of structural and functional teleonomy ... The enzyme proteins as specific catalysts ... Covalent and noncovalent bonds ... The concept of the noncovalent stereo-specific complex ... Maxwell's demon.

IV Microscopic cybernetics: Functional coherence of cellular machinery ... Regulatory proteins and the logic of regulations ... Mechanism of allosteric interactions ... Regulation of the synthesis of enzymes ... The concept of gratuity ... 'Holism' vs. 'reductionism'.

VII. In [R, p. 954, col. 2, last para] Narasimhan questions my assertion that with regard to classification 'the scientific tradition is close to Aristotle'. But indeed it is, for Aristotle allowed 'the natural contours within the cosmos, as perceived by the mind ... to demarcate the sciences' [M, p. 95]. Obviously, since the contours perceived by a scientific mind today will be different from those perceived by even the greatest mind in 400 BC, the demarcations must change. Furthermore, a perusal of Joachim's Introduction to the *Nichomachean Ethics* will show that Aristotle saw the need for interdisciplinary work, Hence, this issue is spurious.

VIII. The statement 'science is based on faith', which heads [R, p. 954, col. 3] is a corollary of two more basic principles of the SM:

1. Science rests on the assumptions that the world is orderly and that the human mind can comprehend this order.
2. These assumptions are based on extralogical faith; cf. [M, §1]

The faith in (2) is none other than the *primal faith* on which all life depends (cf. [M, pp. 13–14, p. 16, last para]) and the religionist's faith in God '*presupposes an orderly and discoursable world and so depends on a primal faith that such is the world we live in*' [M, p. 17]. Thus, Narasimhan is wrong in saying that I claim that the primal faith is 'of the same kind as the religious faith of medieval schoolmen' [R, p. 954, last para]. The truth [M, p. 17] is that the primal faith is *more basic than* the 'religious faith' of medieval schoolmen, Vedantists and many others, including scientists; cf. [*Curr. Sci.*, 25 December 1994, pp. 917–919].

IX. Wiener has written: 'Without faith that nature is subject to law there can be no science. No amount of demonstration will prove that nature is subject to law'. But since Narasimhan does not tell us the flaw in Wiener's full argument and that of A. N. Whitehead [M, pp. 13, 14], his skepticism as to whether 'the extralogical faith of a scientist is a faith' [R, p. 954, last para] remains to be justified. Notwithstanding his skepticism of this faith, Narasimhan attaches great importance to 'the *origin* of it' [R, p. 954, col. 1, para 2]. But once the orderliness of the world is accepted by faith, its origination, while important to the child psychologist, is of zero import to the SM, for life itself, and *ipso facto* a discussion in child psychology or in any other field, would be impossible without such orderliness. In short, *one can investigate the origin of the primal faith only by practising it.* Hence, Narasimhan's three paragraphs about Piaget are irrelevant to the SM*.

X. Regarding the notion of 'scientific revolutions' and of 'paradigm shift' (cf. [M, pp. 20–21]) Narasimhan misleads by saying 'Masani argues' [R, p. 954, col. 3, middle para] instead of saying 'Masani quotes Heisenberg, Einstein and Schrödinger to show that'. It was Heisenberg who said that the 'revolution' (*ca.* 1600) was '*a transition from the descriptive science of Aristotle to the structural science of Plato*' [M, p. 23], and Einstein who said that the relativity 'revolution' (*ca.* 1915) was '*no revolutionary act but the natural continuation of a line that can be traced through centuries*'; and on the quantum 'revolution' (*ca.* 1925) it was Schrödinger who described the wave mechanics as '*a thorough organic expansion and development, one might almost say merely a restatement of the old* [Hamilton, Jacobi] *theory in more subtle terms*' [M, p. 22]. An examination of how these great scientists do their research would show that they strive to complete and beautify the earlier mathematical equations in the light of the new experimental data, and that paradigms play no role whatever in their creative work. It is this that is relevant to the SM.

XI. Narasimhan [R, p. 955, col. 2, paras 1, 2, 4, 5] addresses the fundamental question of *abduction*, i.e. the process by which the mind is able to avoid the huge number of hypotheses that merely fit the known facts, and to hit upon concepts and propositions which yield new knowledge, i.e. *laws of nature*. All except paragraph 5 tell the true story. Paragraph 5, however, contains the incorrect statement that 'the theoretical underpinnings' (more accurately, scientific explications) of 'terms like instinct, insight, abductive skill are glossed over' in [M]. The need for such explications, far from being glossed over, has been clearly emphasized; thus:

'It remains to be discovered what factors in the human nervous system account for man's prodigious criticality in relation to learning automata, and whether Wiener's idea of grading automata in a hierarchy of types offers a fruitful approach to such study. More generally, *a scientific study of what Peirce called intunement should be on the cybernetical agenda.*' [M, p. 84]

'The far greater flexibility and criticality of biological organisms *vis-à-vis*

*In the author's earlier paper 'The illusion that man constructs reality' (*Kybernetes*, 1992, 21(4), 11–24), the obvious contributions of Piaget to child psychology are lauded, and the dubiety of their relevance to the SM laid bare Without the sun and the earth there would be no babies, thus, cosmic order first, babies second Babies do not 'construct' the world, they slowly discover its order

the metallic remain largely unexplored despite the many strides in neurophysiology.' [M, p. 84]

'*Problem*: Explain in scientific terms how the universal, i e. the pattern, first being in the mind of the creator ... , gets ingrained in an object ... and then re-enters the mind of the beholder ... [M, p. 88]

'... modern cybernetical research is still very far from solving the mind-brain problem, and ... considerable harder work lies ahead.' [M, p. 88]

This 'harder work', which must attend to Haldane's important idea of mind as quantum-mechanical resonance, belongs to *cognitive psychology*, a field which clarifies the concepts of the SM but is not the same as the SM. The cognitive psychologist needs the SM to do research. Hence, Narasimhan's remark that 'it is a pity' that I have not taken note of the recent work in cognitive psychology is misconceived. As Penrose's new book *Shadows of the Mind* shows, this field is still in too highly creative and volatile a stage to allow for a short comment. (The normal *Kybernetes* issue is under 100 pages, and the MCB Press were generous in granting me 32 more.)

XII. In [R, p. 955, col. 2, para 3] Narasimhan refers to Galileo's famous description of the Book of Nature as a 'second scripture written in mathematical language', and to my description of 'science as natural theology', but without saying how they fit into the SM [M, p. 63]. The fact is that all civilizations have felt the need for some notion of 'revealed truths' or scripture or *sruti*. These truths affect religious individuals, among them scientists, in personal and profound ways, and to them it is a matter of import that scientific truths not undermine the scriptural. A solution that ensures the impossibility of such undermining thus strengthens the role of science in civilization. Such was Galileo's solution that nature is as good a revelation of God as the holy texts, and that it is the same God speaking in different ways.

XIII. In [R, p. 955, col. 1] Narasimhan gives an accurate description of (i) the difference between animal violence and the bulk of human violence, and (ii) the ontogenetic–phylogenetic imbalance in man, evident in the havoc of human exploitation, and (iii) my attribution of (i) and (ii) to the freedom that the stochastic cosmos allows the human being, rather than to a law of nature. Unfortunately, he does not say whether my judgement (iii) is correct or incorrect. (Authors want to learn from reviewers.) Furthermore, he bypasses five important consequences that follow from (i) and (ii):

1. 'Human progress' has to be measured by the mitigation of this imbalance.

2. The evident constancy of this imbalance (small fluctuations apart) shows that mankind has not progressed during the last 5000 years [M, pp. 116–118].

3. We should not expect things to improve suddenly from now on [M, p. 118, para 2].

4. We must look upon the quest for a fully restored body-politic as never-ending, the goal being an ideal limit as $T \to \infty$ [M, pp. 118–119].

5. A viable concept of *duty* for the scientist, devoted to social prosthesis, must be premised on continual failure, and on the pursuit of goodness for its own sake [M, p. 119–120].

A trivial corollary of (1)–(4) is best stated in Narasimhan's own words with 'totally' substituted for his 'somewhat':

'It is totally simplistic to expect that what the moral teachings of these religious teachers have not been able to accomplish during the last several thousand years scientists will be able to achieve now or in the future.' [R, p. 956, col 1, para 2]

But inattention to the antecedents (1)–(4) led Narasimhan to the error of believing that I share this 'simplistic' expectation. As the quotations (3) and (4) show, I do not. He is also prevented from seeing that since 'sin grows with doing good', a scientist interested in social prosthesis, unlike the one interested in say chemistry or biology, etc., will get nowhere without the great wisdom in the Vedantic or the Greek concept of duty stated in (5).

XIV. It is well known that language and culture are important determinants in human life. But our understanding of social prosthesis will remain stagnant as long as we duck unique aspects of human culture such as the prevalence of homicide, warfare, avarice, sadism and masochism, and of linguistic misuses such as circumlocution, dishonesty, hypocrisy and treachery As for the word 'culture', unlike yesteryear, today it is a highly abused term. It is good to remember the deliberately modest title, *Notes Toward the Definition of Culture*, that T. S. Eliot chose for his book, and even more important to remember the wise words of Lord Acton with which it opens: 'I think that our studies ought to be all but purposeless. They want to be pursued with chastity like mathematics.'

P. R. MASANI

Department of Mathematics and Statistics,
University of Pittsburgh,
Pittsburgh, PA 15260, USA.

R. Narasimhan replies:

1. What is a *cybernetic system*? One can find a formal definition of a 'system' in any standard textbook on system theory. By a 'cybernetic system' what I intended was a system whose behaviour could be accounted for by use, primarily, of the concepts of cybernetics. I have listed some of these concepts in the last paragraph on the first page of my article. Can a cybernetic system serve as an adequate model of a human being? I have argued in my article that it cannot, because of conceptual inadequacies. But now Masani claims that cybernetics is not a science, *per se*, but rather a movement within science. This forecloses any further argument.

2. Coming now to Masani's monograph on scientific methodology, I think it would serve no purpose for me to answer Masani's comments one by one. Clearly, Masani and I disagree on what science is all about and the issues that a discourse on scientific methodology must grapple with. I would have to write another article to discuss my views. I would like, here, to deal with 3 or 4 points that Masani makes in his comments, to convince the reader that in my