

Questions of Scientific Responsibility

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Review of Serge Lang, *CHALLENGES*
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Serge Lang is a distinguished mathematician of French extraction, who has made important contributions to several branches of his discipline (particularly to algebraic geometry and number theory), and who is also known as a prolific author of treatises and handbooks. Some of his books have been used by generations of students and researchers. This is the case of his *Algebra* (Addison-Wesley, 1965) and of the more elementary *Linear Algebra*, the first edition of which has an Italian translation (Borlinghieri, 1970). Even the Italian general reader has been aware of (has had the possibility to appreciate) Lang's exceptional skills both as teacher and as popularizer, thanks to three brilliant 'lectures-dialogue with the public' included in the recent *La bellezza della matematica* ['The Beauty of Mathematics']¹. Yet it is likely that most of his colleagues who are not American, and the public in general, do not know much of another role Lang has carried out since the Sixties, with intelligence, commitment, and (partial) efficacy: that is to say the role of critical conscience of American academia. The book under consideration offers extraordinary evidence of a very significant part of the activity related to that role. One also has to be thankful to

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¹ Published by Bollati Boringhieri, Torino 1991. The original title *The Beauty of Doing Mathematics* expresses better the basic idea of this text.

the publisher for a volume so easily consulted (thanks also to several indices), and very pleasant typography.²

Some of the general themes tackled in the book are: the resistance of the ruling orthodoxy to accept, or even only accept to discuss, critics' opinions; the introduction of spurious quantitative methods as a weapon used by some students of socio-political issues to confer an illusory scientific authority to their claims; the bias that informs questions asked in certain opinion polls; the obstructionism of the most popular scientific journals toward heterodox theories, manifesting itself through censorship, biased editorials, refusals of the right of reply; the press's role in the twisted portrayal of certain scientific controversies and in the defamation of individual 'troublesome' scientists.

These themes are not discussed in the abstract, but through the circumstantial examination of some exemplary cases. The files presented in *Challenges* deal with some episodes "in the area where the academic world meets the world of journalism and the world of politics" (p. 5)³: the proposal for election to the *National Academy of Sciences* (NAS) of the political scholar Samuel P. Huntington; the opinion poll of American university professors by Ladd and Lipset; the affair which broke out in connection with an article written by the Medicine Nobel prize laureate David Baltimore and collaborators; the charge against Robert Gallo of having used, without proper acknowledgement, biological material coming from the *Institut Pasteur* in Paris, for the isolation of the retrovirus HIV; the issue of the causal connection between HIV and AIDS; and the controversy aroused by the political pamphlet *Russophobia* by the great Russian mathematician Igor Shafarevich.

Lang's method, for the presentation of these cases, is to let the personalities involved speak as much as possible for themselves, through letters and different kinds of writings, thus providing a very extensive first hand documentation (partially relegated to appendices) with which readers can form their own opinions. Given the difficulty of finding a lot of that material, this book will become an almost indispensable reference for all interested people. Moreover, with a happy methodological decision (documented briefly but keenly [pp. 15-16]), Lang draws attention to facts (documentable) more than to motives and intentions, or to alleged plots. Thus, he succeeds in keeping the debate on a high plane, without coming to sterile denigration of individuals (a practice which several of the aforementioned personalities have indulged in, as opposed to him), or leaving room for gratuitous fictional reconstructions of alleged backstage dealings⁴. With regard to his aims and the method followed, Lang writes:

²However, for a book which will probably be in its readers' hands for a long time, a paperback sewn in signatures, rather than merely glued, would have been a better publishing choice.

³All the page references, unless otherwise specified, are to the book under review.

⁴With this I do not intend to suggest that I agree with the view, as naive as it is popular among newspaper "opinion columnists", according to which one should refrain

My principal interest was to make people think independently and clearly, and to put scholarship in the service of corrective action. My method was to start with one specific case which I found important, and to pursue it wherever I was led. [...] I don't go along with the tendency which necessarily dissociates "academic discussion" from individual responsibility and action. If some people call my approach *ad hominem*, so be it. [pp. 196-197]

To give an idea of the book's content, I will linger a little more in detail over three 'cases', pointing out, however, that in a review one could hardly do justice to the plethora of material presented in more than eight hundred pages.

1. The proposal to elect S. Huntington to the NAS in 1986 was opposed by Lang with the charge, directed at a significant part of his work, of "passing off political opinions as science" and denying known and very significant facts. Huntington is a figure who has been (and is) in great favor with the highest levels of American politics and academia, having been a consultant for the State Department and for the CIA, and in the quinquennium 1981-1985 "the most quoted political scientist in the field of International Relations" (p. 31). Following Lang's example, we give a sample of his scholarly style. In a book of his from 1968, used as a text at Yale for many years⁵, Huntington classifies South Africa as a "society with low systemic frustration" (referring to the definition of "frustration ratio" given by I. K. and R. L. Feierabend in an article from 1966), and so no less than a "satisfied society". (Note that at that time, South Africa was at the height of apartheid, with 20 million Blacks victims of racial segregation.) Lang has a field day in showing the pseudoscientific and intimidatory nature of the equations included in Huntington's book (involving "quantities" such as "social mobilization", "political institutionalization" and the like), and the silence on riots and repression, which were mentioned even in newspapers in the decade preceding the book's publication. Lang circulated his analysis among all members

from any reconstruction of historical events that can't be supplied step by step with documentary evidence. This approach, if consistently followed, would inhibit every serious attempt to understand history. The crucial point is, instead, that one shouldn't ever lose sight of the difference between what can be proved by documents and what can not be: conjectures must be admittedly used, but to orient documentary research, not to replace it.

⁵*Political Order in Changing Societies* (Yale University Press, 1968). Robert D. Putnam, chairman of the Government Department at Harvard, wrote about this book that it "merits its reputation as one of the true classics of modern political science." Another Huntington's work picked on by Lang is *The Soldier and the State* (Belknap, Harvard 1957), which, in conformity with what the author himself wrote twenty years later, presented "an unabashed defense of the professional military ethic and rejection of traditional liberalism".

of the NAS and in other mailings going to hundreds of individuals.⁶ Huntington does not answer directly, but in a magazine interview, he asserts - without any concern for the "frustration ratio" - that a "satisfied society" has for him the following peculiar technical meaning: whether the lot of the people is "good, fair, or awful [...] the people *for some reason* are not protesting it." Huntington also states that "when this study [...] was done in the early 1960's, there had been no major riots, strikes, or disturbances [in South Africa]."⁷ As a matter of fact, the *New York Times*, to mention only one example, reported numerous disturbances, some of which resulted in the massacre of demonstrators.

Reactions of the academic establishment to Lang's campaign are those which one can imagine: Lang is accused of passing judgment on a field in which he does not have enough expertise; he is criticized for a presumed aversion, as a mathematician, toward the more fluid (or rather, "ambiguous") social sciences; it has been insinuated that he has a score to settle with Huntington, and of course he has been ascribed a 'leftist' political position, to which is attributed the real explanation for the controversy he aroused. Distinguished academics such as Yale's Provost William Nordhaus, and Paul Samuelson intervene in Huntington's favor; Herbert Simon even writes an essay in which he tries to justify Huntington's "mathematics"; the biologist Jared Diamond writes an article in *Discover* in defense of "soft sciences", as they are called in derogatory terms (as if Lang had leveled a general accusation against them!); and so on. In the book under consideration these authors, as well as many others, are quoted and commented on extensively, and the poor quality of their arguments, which at times borders on inconsistency, is striking⁸. Particularly noteworthy is the appeal to collegiality which Lang supposedly violated in attacking a member of the academic community.⁹ Despite all of that, Lang's tenacity obtains the result of non-election, both a first (1986) and a second (1987) time.

The trend exposed by Lang has a considerable impact. The attempts to create mathematical models of social or historical phenomenon are not, in themselves, to be condemned, but one must not cherish the illusion of extending to social science the credit enjoyed by physics simply by introducing numbers and equations into

⁶On the issue of the costs born for these mailings, see the interesting correspondence between Lang and the Provost of his university (Yale), pp. 189-196.

⁷Quoted p. 30. Italics added.

⁸See for example Samuelson, quoted on p. 81.

⁹The accusation of having used a "tone [...] anything but collegial" is leveled against him by the well-known sociologist of science Robert Merton (p. 201). In June 1987, Nordhaus, who together with Samuelson is author of a famous text of general economics, popular also in Italy, writes to Lang: "We need to muster all the strength we have to combat the ignorance and superstition that prevails without our walls. Our mission as an institution for the precious nourishment of ideas and scholars is badly bruised when we turn upon our own, when we withhold that extra ounce of trust and forgiveness"(p. 38).

political or sociological analysis¹⁰. For instance, the fact that it is possible to define the 'gross national product' of a country and to associate a number with it, doesn't reduce at all the political nature of opinions to the meaning of that concept, and thereby to the circumstances that its numerical measure decreases or increases. To mask political options with the seeming neutrality of percentages and technical terms is a trick against which real scientists have to warn the public.

The other most frequently used trick is that of deceptive terminology. Huntington's proposal to define as above a "satisfied society" is a typical example of a "persuasive definition" in the sense envisioned by C. L. Stevenson (1938), that is to say "one which gives a new conceptual meaning to a familiar word, without substantially changing its emotive meaning, and which is used with the conscious or unconscious purpose of changing the direction of people's interests".¹¹ Obviously, according to Huntington's proposal, a police regime which is sufficiently repressive is enough to create a "satisfied society". It is not difficult then to understand what kind of political perspective his readers had to be 'persuaded' to adopt.¹² Someone might hold that in the social sciences the intrusion of evaluative elements is unavoidable. The question remains how it is possible to write "one of the true classics of modern political science" (see footnote 5) having such low standards of rigor and making such serious mistakes, if aptitude for propaganda rather than for science were not an important selective criterion for career advancement in certain sectors of academia.

2. The vicissitude in which David Baltimore has been involved is already the theme of a vast field of criticism concerning "scientists' ethics" or, vice versa, "scientific fraud".¹³ In May 1986, an American researcher, Margot O'Toole, began to have strong suspicions about the data presented in an article published the previous month in the prestigious journal *Cell* under the name of various authors, among them Thereza Imanishi-Kari, Baltimore himself and David Weaver. She asked to consult the laboratory notebooks and found out, in the seventeen relevant pages, that the obtained results did not justify certain claims included in the article. Imanishi-Kari told her that she wasn't able to provide the notes corresponding to another experiment whose data are included in the same

¹⁰See the article, to which Lang refers as well, by N. Koblitz: "Mathematics as Propaganda", in *Mathematics Tomorrow*, edited by L. A. Steen, Springer Verlag, 1981.

¹¹M. Warnock, *Ethics Since 1900*, Oxford University Press, 1968, p. 71

¹²An example, unfortunately more topical, of the same rhetorical maneuvering is to define the concept of humanitarian intervention in such a way that can be applied even to a brutal bombing campaign of civil targets.

¹³A brief but useful report can be found in F. Di Trocchio, *Le bugie della scienza* ['The Lies of Science'], Mondadori, Milan 1993, pp. 70-79. By the way, it's in this book that I happened to read for the first time about Lang's 'second' activity.

article which would have been enough to validate the questionable conclusions mentioned above, because she couldn't find them. In short, O'Toole was able to find out that "a whole series of experiments, described in the article, and on which the main thesis was based, were not even carried out" (pp. 248). Scientists to whom she (O'Toole) turns, including her doctoral adviser, advised her to let the matter drop. During a meeting, Baltimore declared himself against publishing a retraction (of the controversial parts) of the article, adding that if she presented the correction to *Cell*, he would submit a note contradicting her.

What we have said so far is based on the testimony given by O'Toole in May 1989 to the Congressional Dingell sub-committee. Ever since 1986, several parliamentary and scientific institutions have been engaged in the case, in several committees. Part of the impetus was given by two scientists already known as skilled 'fraud investigators', Walter Stewart and Ned Feder, both of the National Institutes of Health (NIH). By and large, I would say, in agreement with Lang, that the evidence which came out during the various congressional hearings and in other committees (and also with the secret services' help!) has demonstrated the reliability of O'Toole's account. Still, official opinions underwent several revisions, more or less 'political'. At first (1991) Baltimore went so far as to ask for the article's recall, but in 1996 a verdict on appeal, this time favorable, allowed him to obtain a partial public rehabilitation, followed the next year by his election as president of the *California Institute of Technology*. It's worthy to note that O'Toole, on the contrary, lost her job and has remained unemployed for several years.

Readers who are already acquainted with the main aspects of the Baltimore case will be able to expand and enrich their knowledge of it through the very numerous primary sources (letters, newspaper articles, depositions) extensively quoted by Lang, and will be able to form an opinion on the responsibilities of the scientists directly involved. However, I think almost everybody will draw the same conclusions as to the behavior, by and large, of the scientific establishment and of the press, specialized and not: some of the greatest authorities, both scientific and institution, engaged in systematic obstructionism: journals like *Nature*, *Science* and *Cell* refused to publish the critical research by Stewart and Feder (which at last appeared in an abridged form in *Nature* in 1991, after a report of NIH confirmed the suspicions); even the NIH hesitated a long time before giving Stewart and Feder (as employees) the authorization to submit the article to a journal; several famous scientists intervened with poor ethical sensitivity in defense of the 'reputation' of science and of their colleagues; and so on. In short, everything possible was done to favor the cover-up of the investigation. In my view, the repeated appeal to the principle of authority by Baltimore and his colleagues is particularly interesting, in a context in which the very least one should have granted to critics was an open debate and full access to the data necessary for a fair judgment (particularly those included in the laboratory notebooks), which instead were, as much as possible, denied (see pp. 258, 264 et sequ.).

3. The case of the supposed causal relationship between HIV and AIDS (a hypothesis which is even today the orthodox conception on that score) is the object of another fascinating inquiry of Lang. In the essay "HIV and AIDS: Questions of Scientific and Journalistic Responsibility", published initially in 1994 in a journal of Yale University, he discusses the way in which the doctrine of HIV's role in etiology spread among both researchers and the general public, despite the lack of convincing proof. (According to Lang, the date from which one can place the inception of this idea's diffusion is 1984, shortly after the beginning of the Gallo-Montagnier controversy on the priority of isolating HIV.) Some criticism of that premature development of consensus appear to be very strong.¹⁴

In the first place, the very definition of AIDS in official texts and documents is circular with regard to the above-mentioned relationship: in fact, one speaks of AIDS if certain symptoms are found (general wasting away, weakness, etc.) and moreover if HIV is present.

In the second place, the possibility that HIV is an opportunistic virus, that is to say one which invades an already sick organism, has been disregarded, confusing between correlation and fortuitousness.

Finally, whoever has expressed doubts about the orthodox theory has been attacked in the scientific (and non-scientific) press, with the accusation of putting public health in danger, thereby considering the matter settled before even discussing it. The main alternative theory, associated with the name of the virologist Peter Duesberg, that AIDS might be caused by drug abuse, was considered to be 'finally' refuted on more than one occasion, without giving to Duesberg or to other skeptics, the possibility of responding further to the 'refutations'. (Among those skeptics, a special place must be given to the Nobel prize winner Kary Mullis, the discoverer of a reaction which allows one to test more efficiently the HIV infection.) Duesberg in particular was denied funds for experimental research proposed in order to ascertain or refute the connection which he presumed to exist between drugs (especially nitrite inhalants) and AIDS.¹⁵

¹⁴On this subject, are available in Italian: the assessment by Duesberg, *AIDS, Il virus inventato*, Baldini & Castaldi, Milano 1998 (first American edition: *Inventing the AIDS Virus*, Regnery, Washington 1996), and the book-survey by M. Bucchi, *La scienza imbavagliata. Eresia e censura nel caso AIDS* ['The Silenced Science. Heresy and Censorship in the AIDS case'], Limina, Arezzo 1998, which includes interviews with several protagonists of the controversy (Lang included, pp. 85-86). A good deal of interesting material (for instance the documents on the relationships between Duesberg and *Nature*) is available at the following internet address: <http://www.duesberg.com>.

¹⁵However, as Lang emphasizes, there is no reason to polarize the debate on AIDS's etiology in such a way: it is possible that more than one causal factor come into play (pp. 707-708). Nor, obviously, if Duesberg is right, would it follow that "every public

The analysis of the role journals have played in the case is, as in the other episodes presented, enlightening. The orthodox press, and in the first place widely circulated scientific magazines such as *Nature* and *Science*, stand out for their propagandistic tone and censorial attitude; specialized journals, such as *The Lancet*, have denied the right of reply, and decided arbitrarily when the discussion was to be considered closed. They have made sure - no one knows thanks to what kind of divinatory power - that the controversy's outcome could have only one possibility.¹⁶

Special emphasis is rightly given to the editor of *The Lancet*, Richard Horton, and to his long article-review published in the *New York Review of Books*, as well as to two consecutive exchanges of letters with Duesberg.¹⁷ As a matter of fact, despite the appearance of balance and objectivity¹⁸, Horton's paper personalizes the issue too much, and most importantly, doesn't answer the objections which were put forward. In the first exchange of letters, there is a further deterioration of the debate's tone, with the provocative suggestion that Duesberg should test HIV's harmlessness by injecting himself with the virus; the article ends with Horton invoking "censure"¹⁹ on him for not taking into account experimental data which were not even available at the time of the review. This answer is without any doubt a very good example of the rhetorical technique of "evading previous objections and introducing new material" which has not yet been "subjected to scientific scrutiny." (p. 705). This technique is stigmatized by Lang more than once (but I am not taking a big risk predicting that it will be applied in reviews of this book!) Lang sent to *New York Review of Books* a commentary in the shape of

health injunction about the need for safer sex becomes meaningless" (R. Horton, part II of the article "AIDS: truth and heresy", in *la Rivista dei Libri* (the Italian version of the *New York Review of Books*), October 1996; the passage is discussed and quoted by Lang p. 704). From the whole book it is possible to extract an impressive collection of paralogisms authored by famous personalities from the world of Anglo-Saxon science and university.

¹⁶This is what the *Senior Editor* writes on 15 January 1996 to a professor of Public Health, refusing to publish a brief letter in which, among other things, he demanded an answer in the journal to Duesberg's objections: "We have received many letters on this topic and after an initial round we decided to close this debate in the pages of *The Lancet* for now. *No doubt in time Duesberg will be proved wrong.*" (p. 695, italics added.)

¹⁷See for the translations *La Rivista dei Libri*, "AIDS: verità e eresia", quoted in footnote 15; and for the first exchange of letters, the section "Letters" in the issue of November 1996.

¹⁸I myself had the opportunity to talk about the "precise and balanced article by R. Horton" (*La Rivista dei Libri*, March 1997, p. 42). The information contained in Lang's book made me change my mind.

¹⁹Correctly rendered in Italian by 'biasimo', not by "censura" as in the already cited Italian translation, which primarily means 'censorship'.

an article, approximately the same length as Horton's, but was turned down;²⁰ nor did he have better luck sending the reply directly to *The Lancet*.

A disquieting conclusion that one unavoidably draws from reading this part of Lang's book - whatever the future developments of AIDS research should be - is that the running of the scientific community, as it is organized nowadays, allows a united orthodoxy to form, in a very short time, around one of the possible opinions, long before solid evidence in its favor is available. This orthodoxy defends itself against critics with the weapons of rhetoric, of censorship, and of marginalization (from conferences and funding).²¹ It's obvious that the very considerable amount of money invested in this kind of research and the heavy economic involvement of several top scientists in the medical industry are partially responsible for these dynamics.²²

A few more words in conclusion. Remarks such as the preceding ones (and others inspired by the reading of the often disconcerting stories Lang reconstructed)²³ are sometimes received as if they threaten the foundations of the trust one must have in science and instigate a dangerous "irrationalism". But the conclusion is not valid: one might trust science as an ideal of public and objective knowledge - and that is certainly the case with Lang²⁴ - without having the same

²⁰Eventually he obtained the publication of a letter, in noteworthy circumstances (p. 709; see also Bucchi, quoted, p. 86).

²¹However, one must acknowledge that Horton, editor of one of the most influential medical journals at the international level, has written without fudging in the above cited article, that "*the ideological assassination that Duesberg has undergone will remain an embarrassing testament of the reactionary tendencies of modern science*" (italics added).

²²Part of an interview given by Duesberg in July 1994 (quoted by Bucchi, p. 173), effectively addressed this point: "Look at all my colleagues who work in the AIDS field. They are all on the board of biotechnological companies or they work for these companies developing vaccines, antiviral drugs, AIDS-tests; they contribute to selling them and promoting them on the market. In some cases, they even own these companies." And, after some comments on Robert Gallo and David Baltimore (who "a few years ago sold for 30 million dollars a company that worked in the AIDS field"), concludes: "If you have twenty or thirty people who work with you in a company which has a turnover of a couple million dollars a year, it's very difficult to show up in front of those people and say: 'Hey guys, we are on the wrong track. Let's drop this stuff'."

²³See the half-serious "three laws of sociodynamics", on p. 797.

²⁴Besides, the model of scientific integrity indicated more than once by Lang (pp. 207, 266-267, 390-392, etc.) is the great physicist Richard Feynman (1918-1988) - everything but an "irrationalist" or an "enemy of science" (on the issues discussed by Lang, see *Cargo Cult*

degree of trust in the capacity of the scientific community to live up to that ideal. As a matter of fact, it should be obvious that as much as scientists show their open-mindedness in admitting malfunctions and faults emerging in particular circumstances, and in working to neutralize their causes, the trust of everybody - citizens and researchers - is bound to increase in the way that science is done and in the results it achieves. Instead, reluctance to question itself is perceived from the outside (but also among scientists) as if the world of research wanted to keep its malaises concealed. In order to react effectively to these suspicions, which as a matter of fact are very widespread, it will be necessary to pursue a tighter correspondence between science's reality and its ideals (absence of dogmatism, fair comparison of ideas, repetition - and not sheer repeatability - of experiments, accessibility of data), even though sometimes that necessitates the suspension of judgment on crucial social and economic issues on the part of scientists and scientific institutions. Of course, this is something that the present media system tends not to accept from scientists, since it is not useful to achieve influence on public opinion, which is one of that system's main goals. But the most certain way to damage, in the medium run if not in the short run, both scientific research and its public image is to answer suspicions of crisis by simply denying them; that is to say, maintaining that scientific research 'enjoys excellent health',²⁵ and that the institutional processes which control them effectively guarantee the right to critique and correct representations of scholars' opinions. For those who are persuaded that is how things stand, I couldn't suggest a better antidote than Lang's lucid and passionate book to wake them up from their dogmatic sleep. This book should become standard reading also for journalists, sociologists and philosophers; at least if we want the public debate on science, ideology and power structure to reach a new level of seriousness. Because, as another mathematician has written, "if science is the pursuit of truth, don't we also have to be truthful regarding the way it is done?"²⁶

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Science, pp. 338-346 by R. Feynman, "*Surely You're Joking, Mr. Feynman!*", Norton, 1985, and his report on the inquiry on the Challenger disaster, in the second part [pp. 113-237] of "*What Do You Care What Other People Think?*", Norton, 1988.)

²⁵Or that the only problems which might arise are related to the shortage of financial resources.

²⁶D. Ruelle, *Hazard et Chaos*, Odile Jacob, Paris 1991, p. 8