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Serge Lang's last file and the suppression of dissent in contemporary science

Serge Lang died suddenly on September 12, 2005, in his flat at Berkeley. He was 78 and had taught at the University of Yale for 33 years, where, having taken his retirement last year, he was now professor emeritus. He was still very much active, both in mathematics, and, as the visitors of the Science and Democracy web site know well, also on political, epistemological, and ethical issues.

This is not the right place for giving even an hint of the huge mathematical output –surpassing, it has been said, that of the prolific XVIII century master mathematician, Leonhard Euler – of this world-class scientist, who was among the very few in the last decades to possess a panoramic control of his science, as his many handbooks and specialized monographs are there to show. From complex analysis to elementary geometry, from differential manifolds to abstract algebra, from algebraic geometry to analytical number theory, it is hard to find a single discipline in basic or advanced pure mathematics where Lang has not left his imprint, either by proving new theorems or by systematizing the matter in one of his treatises. And there is hardly one mathematician who had his education during the last thirty years and who has not profited from poring over one or the other of Lang's books.

Teaching, research, and bureaucracies

He also published some books of mathematical dialogues with undergraduates, high school students and lay audiences. Lang's pedagogical ability and love of his science at all levels shines through for all to see in these works. Lang was also very clear as to the importance of teaching, and the fact that in the academic world all emphasis is put on research, no matter how irrelevant. In 1970 he wrote:

> In mathematics, even though we don't have the particular problem of "scholasticism", we have another similar one. Under the influx of NSF [National Science Foundation] money for the past 15 years, the total number of PhD's in mathematics in the country has jumped from 300 to 1,000 per year, thus going from a low but stable level to an unstable one, and these PhD's turn out to be too many of the wrong type of mathematicians: for the most part they succeed only in cluttering up the research journals with lousy papers. We have put a financial and sociological premium on research, mainly at the expenses of teaching. This course must be reversed. [...] Our response should be flexible and daring, and we should create an atmosphere which allows young mathematicians to feel that they can make it in the academic world without having to write one mediocre paper every year or two. The enormous rise in the number of PhD's and the shortage of good mathematicians is no more a paradox than the fact that the United States manages to have both inflation and a depression at the same time. It is a problem to adjust the relation between the total number, the type of mathematician that is produced, the needs of the country, and the tastes of the young men concerned by all this. [R, pp. 91-2]

This problem is still with us (in Italy, for instance), and the "daring and flexible" approach advocated by Lang has found very few followers. Indeed, the introduction of so-called impact factors during the last decade has encouraged the drift towards giving a greater weight to "research" in promoting a scientist's career. Needless to say, that "the needs of a country" should be taken into account when financing the work of professionals in mathematics or other sciences is something that those same professionals generally fail to appreciate or even hate to consider. However, it should be clear that it is not by enforcing an increasingly technical education in the universities that these needs will be fulfilled; as Lang explained:

> The so-called "culture" which they [the students of colleges and graduate schools] get in college appears to them irrelevant and obsolescent to a large degree, an the more professional training which

> they get in graduate schools is not only useless to them if they cannot get a suitable position in accord with his training, but also harmful to them and to society in that it has raised their expectations and makes their ultimate disappointment all the greater. [R, p. 93]

There is another fictive fashion to make the general interests to bear upon the universities, and this is by increasing the bureaucratic burden of faculty. Lang was the leader in a national campaign against Circular-21, which asked faculty to fill "effort reporting" forms, divided into a a dozen of different "activities", like "Instruction", "Organized Research", "Educational Service Agreements" etc., first in 1966 and then in 1979. In 1981 his university, Yale, turned down a NSF grant he had received because he had refused to fill and sign the effort reports; as a consequence he lost 2/9th of his academic salary (the same loss was *not* suffered by others who had followed his lead and acted similarly).[1]

Lang's determinate opposition to "bureaucratic encroachment" (cf. CC) has to be kept in mind when evaluating his overall positive opinion of some of the committees (like the Dingell Subcommittee, see *infra*) that in the last fifteen years have investigated in the United States reported cases of misconduct in scientific research.

A scientist *engagé*

As suggested above, a scientific production which could have easily filled several mathematician's lives was not enough for Serge Lang. He also felt more strongly than it was and is common among his colleagues that it was his duty to be active on political and ethical issues.

His "political consciousness" had been awakened during his sabbatical year at the University of California at Berkeley, in 1965-6; he had just published one of his most famous treatises, *Algebra*. Those were the years when Berkeley, with 27,000 students, was the epicenter of the student unrest, culminating in the rise of the Free Speech Movement (1964) and the Vietnam Day Committee (1965).[2] Lang's perception that he had to do more than just minding his own mathematical business increased "as the escalation of the Vietnam war and the domestic crises of our cities and of our minority groups were becoming increasingly alarming" [S, p. xi].

He was forty when he published his first non-mathematical book (his 15th book, incidentally!), describing the campaign for Robert Scheer, a candidate in the primary election, 7th Congressional District of California (including Berkeley and most of Oakland). Lang participated in the campaign, going as far as distributing leaflets door to door, though, he remembered, he "was still too preoccupied with academic pursuits" (S, p. xi). Scheer ultimately lost, though with a surprisingly high percentage (45% of the votes). With his book Lang wanted to leave a testimony of a genuinely grass-roots political campaign, concentrating on real issues (Vietnam, poverty, unemployment, housing, racial discrimination, police brutality etc.) and where its supporters were welcome to take initiatives without having to ask for permission.

In 1971 Lang contributed an article to an edited book, based on lectures presented at the University of California at Berkeley in the spring 1969, entitled *The social responsibility of the scientist*; his article was: "A Mathematician on The DOD [Department of Defense], Government, and Universities" and had to do with the "sad record of involvement [of U. S. universities] with institutions like CIA, IDA (Institute for Defense Analysis), DOD over the past 15 years". He abandoned his chair at the Columbia university to protest against the way its administration was dealing with the anti-war movement. He never 'repented' of his political engagement (in the wide

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[1] Lang *et al*. 1984.

[2] Draper 1965 is the classic account.

sense of the word), and in fact he went on as a critic of the political and academic establishment during all his life.

To those who found his activism surprising or strange, Lang replied thirty years later:

> As to my activism, some people have asked what it has to do with mathematics, which is my main activity in life. They seem surprised by a mathematician who shows some professional interest outside his narrower scientific commitments. But why should I not be interested in other aspects of intellectual or social activity? Why be puzzled by the disparity between a standard label ("mathematics") and the existence of another activity not closely related to the one usually associated with such a label?

Notice the careful wording: Lang is saying that, after all, there is *not* such a big "disparity" between his educational work as a mathematician and his political activism:

> There is something in me that makes me want to make others understand explicitly the assumptions under which they operate. I want to make people think independently and clearly. Is that not part of the educational commitment? [C, p. 8]

But is it not unseemly for a scientist to be "politically motivated"? To this Lang answered:

> Of course I am politically motivated! But in what sense? I define "politics" to mean in the broad sense how society is organized, how one deals with social organizations, our relationship to government, how we arrive at decisions affecting the country and the world, the way ideas and information are disseminated in the media, the role of education, the way ideas are taught in schools and universities, how information is processed (by the press, by individuals, by the educational system, by the government etc.). In understand politics in that broad sense, and in that sense I am politically motivated. But my concern for politics does not mean that I support some faction, or some wing over another wing, say the left wing over the right wing; or that I support some "ism" ideology such as socialism, communism, or capitalism. I totally reject such factionalism. [C, p. 5]

Among his peers (if this word makes any sense here) Lang was politically isolate, but less so than his enemies liked to describe him. He came to terms with having his articles systematically rejected, even by student journals of his own university, because even this, if suitably advertised, could further his political purposes. But there is no doubt that all this, notwithstanding his contagious enthusiasm, put a painful strain on him.

Philosophical background

At university Lang started following a philosophy course and then switched to mathematics, but his initial passion did not abandon him. Lang saw himself as heir to that philosophical tradition which extolled the importance of using words carefully, a tradition going from Socrates to Bertrand Russell and the logical positivists. He regarded as his main specific contribution to have brought this form of intellectual discipline to bear on the everyday practice in academia and journalism. In fact his cultural activism can best be described as an attempt at introducing the standards of factual accuracy and logical transparency into the ordinary scholarly and journalistic exchanges. He emphasized that the framing of alternatives in a public debate is a basic instrument of power. As he explained:

> When confronted with a question, the first decision you have to face is whether to accept the question on its terms, or to challenge the terms of the question. The power to impose the terms of a question, that is, to impose the way issues are formulated and alternatives are posed, is a form of control. On the whole, I find that there are very strong forces in our society which induce people to

> accept uncritically the terms imposed on them by those in power, wherever this power comes from. There are many forms of power, and many contexts, including social, political, academic, financial, and journalistic power. In my experience I also find that the educational system at all levels fails to teach properly how to respond critically to tendentious questions, On the contrary, I have found that the educational system mostly conditions students to accept unquestioningly the dominant patterns of the society around them. [C, p. 225]

The fact that Lang's writings on scientific research, journalism, and ethics properly belong in the philosophical literature has been recognized, at least, by the inclusion of a sizable portion of them in a recent textbook on the history and philosophy of science (Lauer 2003).

The "files"

Another charge he often leveled at his targets in the establishment was their inability to distinguish between a fact and an opinion or a mere state of mind. It is in this perspective that his main polemic tool, the "file", has to be viewed. As most of the confusion that plagues the public debates arises from (induced or unplanned) oblivion or ignorance of documented evidence and arguments, Lang made a point of collecting in an organized fashion the documents playing a direct role in the controversy he had entered, including the *full* correspondence between himself and the various higher-ups he was taking to task, for the members of his cc-list to evaluate at ease.

Whenever some of his official interlocutors answered by phoning him or meeting him (a common establishment technique to obstruct the compilation of a full documented story), Lang subsequently wrote them a letter describing the content of their oral exchange, so that nothing relevant to the issue at hand could be 'off record', at least as far as he was concerned.

One issue that often surfaced was that of privacy. Several of Lang's correspondents rebuked him for making public use of their letters, which were meant, they protested, as private communications. That this criticism was disingenuous was apparent both because Lang's own letters had a cc-list, meaning that they were conceived as part of a public exchange, and because the officials he addressed had no qualms in answering him. normally, on official stationery.[3]

From one of his campaign, that against the election of the political scientist Samuel Huntington to the National Academy of Sciences, Lang derived what he called "the Huntington test". It consisted in asking people to write their comments on the way Huntington, in a 1987 interview to *The New Republic*, answered those who – like Lang himself – had questioned his classifying South Africa (in the Sixties!) as a "satisfied society". The relevant passage of the interview was:

> <<Huntington says, "The term 'satisfied' has to do with whether or not there are measurable signs that people are satisfied or not with their lot. That lot may be good, fair, or awful; what this particular term is describing is the fact that the people for some reason are not protesting it. When this study [...] was done in the early 1960s, there had been no major riots, strikes, or disturbances [in South Africa]. France, on the other hand, had just been through a constitutional crisis and an attempted coup d'état".>> [Cit. in C, p. 30]

What is obvious when reading his files is that at the root of Lang's interventions was no personal animosity, but rather an intense desire to clarify issues, pointing out inconsistencies, and setting the record straight on factual questions. What is also clear and, to the newcomer, quite surprising is the utter inadequacy of most of the responses he elicited, and which were very often marred by serious intellectual and/or ethical faults, ranging from evasion of the issue and self-indulgence to sheer factual and logical mistakes.

[3] A particularly bizarre case is discussed in C, pp. 492-4.

Sometimes one can understand, though it is hard to sympathize with, the uneasiness of several of Lang's interlocutors, who were upset by his insistence on factual truth and consistency: clearly they had never suspected before that their high position in the hierarchic ladder implied a correspondingly high responsibility with respect to official decisions and statements.

In fact among those guilty of grievous intellectual sins we find presidents of Ivy League universities, editors of journals like *Science*, *Nature*, *Lancet, New York Times* etc., world famous scientists in all fields of knowledge – it is the *Who's Who* of U. S. science and journalism that comes up tarnished by Lang's circumstantial exposures. In fact Lang warned not to give an excessive weight to the honours bestowed on a scientist:

> **In any case, I urge people not to interpret membership in the NAS [National Academy of Sciences] as being more than a certification of narrow scientific contributions. Even such a certification is subject to questioning.** [C, p. 763]

After having read some of Lang's files, one is immunized forever from the temptation to rely passively on the opinion of famous pundits and scientific and academic authorities. From them one learns in a most convincing fashion that intellectual minority is not only a base condition in itself: it is also very risky.

Suppression of dissent by the establishments

Another source of surprise lies in the very content of the stories documented in the files, exemplifying a consistent pattern of stonewalling and censorship against legitimate and rational criticism. In the utterances of the science establishment there exist, side by side, big-time statements concerning the conventional standards of sciences (critical attitude, refusal of the authority principle, consistency) and an everyday practice which runs directly opposite to them. This contrast between "the rhetoric and the reality" reaches often in the documentation provided by Lang's files levels of comical evidence. Lang outlined his rich record of challenges to the establishment in a humorous fashion, but very seriously as to the gist of the question, by stating his "three laws of sociodynamics":

> *The first law of sociodynamics*
> (a) The power structure does what they want, when they want; then they try to find reasons to justify it.
> (b) If this does not work, they do what they want, when they want, and then they stonewall.
>
> *The second law of sociodynamics*
> An establishment will close ranks behind a member until a point is reached when closing ranks is about to bring down the entire establishment; then the establishment will jettison that member with the least action it deems necessary to preserve the establishment
>
> *The third law of sociodynamics*
> It's like the video games: one can't shoot fast enough

Lang's files were circulated by him to all directly involved people and to many interested scholars, so that through his mailings to dozens and sometimes hundreds of recipients a competent public was built that witnessed the development of the confrontation. But Lang made more than this, by publishing with an important international publisher two books containing material from his files: *The File* (1981, on the Ladd-Lipset survey among U. S, university professors) and *Challenges* (1998).

If our academic and media culture will some day reverse its apparent present decline, *Challenges* will be hailed as what it is – a masterpiece in the sociology of science. Alas, we are still far from that day. Lang's obituaries in the main newspapers did not even bother to mention it. Even the *Daily News* of his own university, Yale, failed to cite it and gave a confusing account of Lang's political work. Actually, when *Challenges* first appeared, very few reviews in all kinds of journals were published of this landmark work, and *pour cause*: it is the whole power system of journalism, scientific research and academia that is shown through the documents contained in it to be far below its professed standards and badly in need of reform. The force of Lang's analysis is that it does not deal in vague generalities, but concentrates on concrete examples, individual failures, specific errors, and provides a vast amount of empirical data enabling the readers to judge for themselves.

Is scientific research "basically sane"?

In several cases, and in three main ones – the Robert Gallo, the David Baltimore case, and the AIDS/HIV cases – Lang got deeply involved into issues of scientific wrong-doing by very famous established scientists. He acutely perceived and decried the drift towards legalistic or psychological notions shown in the investigations made by specifically appointed panels and boards on suspected cases of "misconduct" in scientific research; at the same time he saw these panels as the necessary and, ultimately, beneficial outcome of the consistent refusal by the higher-ups of the scientific community to face squarely the evidence of cases of serious misconduct by prominent scientists.

One typical example (endorsed subsequently by many other scientists who should have known better) was provided by the editor of *Science* in 1987:

> [...] we must recognize that 99.9999 percent of reports are accurate and truthful, often in rapidly advancing frontiers where data are hard to collect. There is no evidence that the small number of cases that have surfaced require a fundamental change in procedures that have produced so much good science. To continue the great advances that are being made, we must accept that perfect behavior is a desirable but unattainable goal. Vigilance? Yes. Timidity? No. [Cit. in C, p. 298]

This statement is to be compared with the titles of two very recent articles appeared on the open access journal *PLoS Medicine* (May 2005):

1) "Medical Journals are an Extension of the Marketing Arm of Pharmaceutical Companies"

by Richard Smith, the former editor of the *British Medical Journal*; he starts by quoting a statement (March 2004) of the editor of the *Lancet*, Richard Horton: "Journals have devolved into information laundering operations for the pharmaceutical industry".

2) "Why Most Published Research Findings Are False".

But is it not the case that to evaluate scientific research one needs to be an expert in the specific field investigated? As a matter of fact Lang was often criticized during his campaigns, with the argument that he was a mathematician and as such he could not have independent opinions on the behaviour of specialists in other areas, like biology, medicine, sociology, history etc. To this his answer was:

> To address questions of scientific responsibility does not necessarily imply that one needs technical competence in a particular field (e. g. biology) to evaluate certain technical matters. The evaluation of scientific responsibilities can legitimately be done without such technical competence. For example, at no point do I take a position as to whether certain experiments validate a theory or not, or whether the theory is valid or not; but I do take a position about the ways scientific responsibilities were exercised in raising questions or answering questions about those experiments. [C, p. 243]

“Misconduct” in scientific research and the paradox of established pseudoscience

The above-mentioned drift is clearly illustrated by the official definitions of “misconduct”. In 1989 the Federal Register (*Public Health Act*, Vol. 18, No. 30, 1 September, p. 6) defined:

> “Misconduct” or “Misconduct in Science” means fabrication, falsification, plagiarism, or other practices *that seriously deviate from those that are commonly accepted within the scientific community* for proposing, conducting, or reporting research. It does not include honest error or honest differences in interpretations or judgments of data.

In 2004 the Federal Register gives this recent reformulation of what is officially meant by “research misconduct”:

> Research misconduct is defined as fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results. Research misconduct does not include honest errors or differences of opinion. A finding of research misconduct requires that *there be a significant departure from accepted practices of the relevant research community*, and *the misconduct be committed intentionally, or knowingly, or recklessly*, and the allegations be proven by a preponderance of the evidence. [Federal Register 2004; italics added]

The second italicized condition leads the investigators to busy themselves with the intentions and other states of mind of people, not with what has been done. As a result, a panel may conclude that a researcher under investigation is not guilty of “misconduct” even though he or she has published seriously defective and misleading work. Clearly this approach is ideally suited to perpetuate the spreading of false data and results in the scientific literature, and to allow scientists to avoid public correction of unsound or just wrong claims.

On the other hand, the first italicized condition is a sociological one; as Lang sarcastically explained, with reference to the former definition, applied by the HHS Appeals Board to the Gallo case:

> According to the Board’s logic, if falsification becomes a universal practice among scientists, then it receives the legal approval of government agencies which are supposed to overview the maintenance of scientific standards for government grants and government laboratories. [C, p. 481]

To expand this objection, one can add that the use of the expression “accepted practice” leads to what may be called the *paradox of established pseudoscience*: if a variety of *pseudoscience* happens to be widely accepted within a certain research community, then to practice that pseudoscience, indeed to build one’s career on it, is no “misconduct” – and the citizens have no short-term manner to counter this phenomenon other than by direct action. Clearly to define “misconduct” this way verges on the absurdity. No scientific tenet, “commonly accepted” or not, is beyond public discussion and criticism, and no specialists should feel safe in (tacitly or openly) agreeing between them to act professionally in ways that appear to the citizens outside the agreement as irrational or unethical.

A remarkable case in point is the practice of vivisection or animal experimentation for medical purposes (cf. Ruesch 1981, Croce 1999); its departures from scientific standards are so big and at the same time so “commonly accepted”, that in my view it is hardly surprising that the most famous case of scientific misconduct in the last twenty years turned around a vivisection paper. I am referring to the article co-authored by Nobelist David Baltimore and describing immunological experiments (some of them never really performed) on transgenic mouse.[4]

Lang's criticism of legalistic definitions of "misconduct"

Lang advocated a completely different approach, based on the ascertaining of facts and full publication of official reports:

> **Rather than looking into motives and intent, and determining "misconduct" in some legalistic sense, let us raise questions about performance concerning:**
>
> **- what was achieved, when and by whom;**
> **- the accuracy, truth, or falsity of statements about scientific work or about the history of scientific work; and**
> **- the level and standards of performance in carrying out scientific work.**
>
> **I urge that questions about conduct concentrate on facts concerning performance, and not on arguments as to what constitutes "fraud", "intent", or "misconduct" and how these words are to be used. Once facts are established, the scientific community can arrive at de facto decisions: whether to tolerate certain practices or not, whether to fund certain laboratories or not, whether to rely on claimed results by certain persons or not.** [C, p. 526]

As is clear from this passage, Lang thought that, ultimately, self-policing by the scientific community was the key. He went on stating explicitly that one should not necessarily construe cases of bad scientific practice in terms of criminal law:

> **But even though one does not wish to tolerate a practice, this does not imply that the practice has to be labeled fraud or misconduct. It does not imply that the practice has to give rise to legal or quasi-legal proceedings. Rather, let us have official reports clearly informing us of the facts in the case.** [C, pp. 526-7]

Lang's intent in drawing this distinction was to make it easier for whistle-blowers to expose the mistakes and abuses in scientific research than it would be if this led automatically to a prosecution of the wrong-doers and then, necessarily, to an evaluation of the degree the accused were consciously acting against the scientific standards. The experience of the Gallo and Baltimore cases had shown that lawyers and administrators adopted an approach to the transgression of scientific standards which was bound to exculpate even authors of seriously defective works:

> Similarly, I object to tying the entire investigative enterprise to a determination of "misconduct" rather than a determination of facts in the case, with the result that if no "misconduct" in the above legalistic sense is found, then "no administrative action is needed". Linking the investigative process to a determination of "intent" or "misconduct" obfuscates the possibility of determining and making clearly known the facts of the case. Actually it has been documented to destroy this possibility in certain important aspects. [C, p. 524]

A particularly impressive example of what Lang had in mind was provided by the HHS Appeals Board in the Gallo case, which, in a document of 6 July 1993 sent by certified mail to Gallo's lawyer and to the Office of the General Counsel, ORI, stated that

> In the absence of any specific definition of scientific misconduct in a statute or regulation in effect at the time of the conduct, ORI must prove that the nature of the Respondent's [i. e. Gallo's] violation

[4] Weaver *et al*. 1986.

> of applicable standards of conduct was *such that any reasonable researcher in his position would have considered it scientific misconduct at the time*.
> [...]
> The definition [of "misconduct in science", the one quoted above] cannot reasonably be read as encompassing *falsification or any other* [sic!] *conduct which does not seriously deviate from commonly accepted practices within the scientific community* or which results from honest error or honest differences in interpretations or judgments. [Cit. in C, pp. 503, 504; italics added]

Clearly the first italicized passage asks from the judges that they accomplish a subtle and thorough sociological and historical inquiry before being able to pass judgment on the reported actions. And the second passage is even more outrageous, insofar as the writer is assuming that *also falsification* does not (or may not) "seriously deviate from commonly accepted practices within the scientific community" and therefore is not (or may not be) in need of punishment!

In the Gallo case the NIH Office of Scientific Integrity (OSI) reported that Gallo's laboratory had been guilty of practices like:

> lack of laboratory records [...] lack of attention to details which resulted in false representation [...] lack of scientific rigor [...] breached overall responsibility [...] to ensure the accuracy of the paper [...] created and fostered conditions that give rise to falsified/ fabricate data and falsified scientific reports [...]

And yet both the OSI and two out of three NIH scientific advisers concluded that Gallo was not guilty of "misconduct", though they conceded that the actions listed above "merit significant censure" (cit. in C, pp. 467-8)!

Can the scientific community police itself?

What is not very clear is what the alternative is to the judiciary inquiry. In particular, is it reasonable to hold that the only jury a scientist has ever to face for his wrongdoings *qua* scientist should comprise just some subset of his colleagues? On this issue, Lang had serious misgivings, as so much of his documentation proved beyond reasonable doubt that one cannot expect very much from scientists *as a class*. Rather, he emphasized the importance of individual sense of responsibility:

> Ultimately, to uphold the traditional standards of science, scientists cannot rely on authority, they cannot rely on panels, they cannot rely on big-time certifications such as those coming from Nobel Prizes or the National Academy of Sciences. They cannot count on the press and they cannot count on Congressional committees to bring the problems of the scientific community to their own attention, or to police the scientific community. They must rely on individual responsibility, and they must create an atmosphere and conditions under which scientists, both young and established, can exercise this responsibility without fear – fear of retaliation, fear for their careers, fear for their funding, fear for their publications, fear of the tension which come from a challenge, fear of being uncollegial, whatever. Will they? [C, p. 309]

This question mark was not rhetorical, but anguished. In fact Lang's files provide plenty of evidence that scientific researchers, particularly those at the top of the hierarchy, are all too prone, either collectively or individually, to renounce the "traditional standards of science" whenever status or money are at stake.

At the same time there were a few scientists who had acted in crucial instances in admirable ways. Lang's favourite example was physicist Richard Feynman investigating the Challenger disaster.[5]

[5] Feynman 1988.

An example: Baltimore, Dingell, and Gould.

Let us take, as an instance of the opposite kind, the Baltimore case.[6] In April 1988, the hearings of the Subcommittee chaired by John Dingell, titled "Fraud in NIH Grant Programs" began, with the aim of preventing the squandering "of precious dollars into meaningless or fraudulent work [...]"; the Baltimore case, among others, was investigated. David Baltimore, who had not been invited to testify, sent a "Dear colleague" letter a month later, where he called the hearings "totally unnecessary" and stated: "What we are undergoing is a harbinger of threats to scientific communication and scientific freedom". In 1989 the accuser of Baltimore, Margot O'Toole, introduced new damning evidence claiming that the lab notes presented by Baltimore's coauthor, Thereza Imanishi-Kari, to the NIH panel investigating the case, had been fabricated after her challenge. In April 1989, three weeks before the second Dingell hearings, the Director of the MIT center for cancer Research, Philip A. Sharp, wrote a "Dear Colleague" letter and a "Dear Congressman" letter, the first one including the following passage:

> It seems obvious that the Congressional [Dingell's] Subcommittee has decided to hassle David [Baltimore] and the other authors and this has serious implications for all of us.

The "Dear Congressman" letter said:

> It is difficult to fathom the motives behind the Subcommittee's current actions. But I believe that to continue what many of us perceive to be a vendetta against honest scientists will cost our society dearly. If scientists who have been exonerated of all wrongdoing must continue to defend themselves against vague and shifting charges, all members of the scientific community must be afraid.

The passage on Baltimore having been "exonerated" referred to the NIH panel chaired by Joseph Davie, which in January 1989 had concluded that "no evidence of fraud, conscious misrepresentation, or manipulation of data was found"; nevertheless, the *Cell* paper, according to the Davie panel, contained "significant errors of misstatement and omission, as well as lapses in scientific judgment and interlaboratory communication".

We come across once again the inconsistency of claiming that yes, very serious misbehaviors have been observed, but... no "misconduct" is detectable.

In fact the Dingell Subcommittee was widely attacked and discredited by many members of the scientific community. Typical of the obfuscation produced in the process is an article in the *New York Times* by the famous paleontologist and science writer Stephen Jay Gould in 1989, where he soberly compared Baltimore case to Galileo, and the Congressional Subcommittee to the Church Inquisition! Gould wrote his newspaper article as if the Baltimore case had to do with errors of interpretations rather than with experiments described in a scientific paper without ever having been performed:

> First, while we all accept that any beneficiary of Federal funds must be subject to the scrutiny of benefactors, what could possibly be more chilling to creativity than an office of censorship (it would have another name, but the effect is what counts) trying to impose the impossible and the inhuman – freedom from error in thought and deed? We might as well rule that any orchestra receiving a penny in state funds must employ an umpire to tap the conductor on the shoulder every time the principal French horn plays a sour note.

Nice, isn't it, this reference to the "principal French horn"; and I hope you will thank me for sparing you quite a few equally nice comparisons of scientific research to baseball games. And yet, behind

[6] What follows is mostly derived from the documentation in *The Baltimore File* compiled by Serge Lang, partially reproduced in C (see particularly pp. 275-81).

this superficially brilliant style, what a deep misunderstanding of what was at stake in the Baltimore case; what a piece of misinformation for Gould's readers. There is another interesting passage, where Gould complained that the trouble was that the public was not sufficiently aware of the purity of the scientist's soul:

> Fraud is a pathology. I doubt that nonscientists realize how concerned all scientists are to purge any detected incident.

In replying to Gould, the renowned biostatistician Irwin D. Bross wrote in a letter to the journal:

> In fact, those at higher levels of the establishment who were charged with fraud usually had numerous colleagues and high-level administrators try to cover up the fraud or dismiss it as "scientific error". This occurred, for instance, in the cause célèbre cited by Professor Gould (and in most other incidents), where few members of the establishment rushed to purge the fraud, while many rushed to condone it.

Overall, very few scientists supported Dingell. Lang was one of those who did. In 1990 Dingell commented:

> The Subcommittee expects the community of scientists to police itself. We have, of course, been severely disappointed by the response of the scientific community on a number of occasions.
>
> This disappointment extends particularly to the present instance. A number of prominent scientists, under a promise of confidentiality, examined the suspect notebook and agreed that it was obviously bogus. But these same scientists were unwilling to advance their professional opinions in public for fear of the disapproval of their colleagues. This reluctance by prominent scientists to deal fully and frankly with the problem of scientific fraud and misconduct has greatly complicated not only the present investigation, but others as well.

What is clear from this episode and many others documented by Lang is that the scientific community is far from exhibiting any special degree of ethical solidity in dealing with the bad science and bad behaviour of its members, especially the powerful ones.

Unfortunately even historians of science, instead of acting as the critical conscience of science in its making, often become the apologists of famous scientists, including contemporary ones. This has occurred, as shown by Lang, in a well-known recent book-length account of the Baltimore case (cf. K and *The Kevles File*).

Serge Lang's last file

Lang's last struggle was related to his longstanding engagement to ask the biomedical AIDS establishment uncomfortable questions concerning the rational and empirical soundness of the official belief that the astutely named "Human Immunodeficiency Virus" (HIV) is the cause of the "Acquired Immunodeficiency Syndrome" (AIDS).

On May 13 he sent two articles to the Proceedings of the National Academy of Sciences, with an accompanying review by Richard Strohman, emeritus professor of Molecular and Cell Biology at the University of California at Berkeley. This review stated that "their publication in the PNAS is not only merited, it is essential". On May 27 Nicholas R. Cozzarelli answered him by rejecting the papers, after consulting with "experts on the PNAS Editorial Board" since "Neither of them are research articles. They are instead opinion pieces". This was the whole explanation of the rejection.

In this web site the reader will find all the documentation (*The PNAS File*) to judge for themselves, and particularly to check whether the grounds for this rejection were even remotely plausible.

Lang replied in detail on June 8, by addressing himself to the President of the NAS, Bruce Alberts.

> There are indications that the orthodoxy on "HIV/AIDS" is increasingly challenged. The establishment has functioned in such a way that to raise questions about the orthodoxy amounts ipso facto to raise questions about the credibility of the establishment.

On June 22, Alberts wrote that Lang's request to reconsider his submissions would have been placed on "the agenda for the next meeting of the NAS Council, which will take place on August 7-8" ; however, for that time a new president, Ralph Cicerone, was to take his place.

Serge Lang's last letter, dated 6 September, ended by commenting that "it is highly unlikely that I shall hear from Cicerone or any other higher up in the NAS or PNAS". His main statement in this letter deserves to be quoted:

> I enclose once more the full correspondence dealing directly with my articles, including the latest letters mentioned above. Let scientific history record these dealings and the establishment's refusal to allow, let alone support, the mere existence of a challenge to the HIV/AIDS orthodoxy in a scientific context. One possible result of refusing to deal with scientist on this issue (let alone members of the NAS) is that the scientific establishment will have to deal with the media in a very damaging way – if and when the media stop repeating uncritically what is fed into them by that establishment, There are signs that the curve of journalistic criticisms of that establishment is about to shift from being slowly strictly increasing to a more substantial and rapid attack, beginning this fall. Even with what's coming this fall, it is of course not clear if and when a critical mass will be reached to topple the orthodoxy. But the scientific establishment has risked its credibility on the "HIV/AIDS" issue in a very big way.

At present, apparently, the issue is not closed. Unfortunately Serge Lang is no more here to expand his file and communicate to his cc-list its further developments.

Conclusion

Serge Lang's writings on scientific practice are arguably among the most important contributions to the sociology of contemporary science. They are at the same time a poignant testimony of the struggle of a great scientists against the forces that are stifling scientific research today – not from outside but within the scientific community itself. Lang had realized that science needs a special atmosphere for his thriving, and that the standard rhetoric of science is certainly not enough to create it.

Lang pointed out repeatedly that the proposal of solving the problem of violation of the scientific standards by introducing in the university curriculum courses in scientific ethics for the young is misplaced, as in fact the scientists of guilty such violations have been, prevalently, established scientists, not young people:

> Courses on scientific ethics are increasingly being taught, but the recommendation to have such courses by various official bodies which have refused to take position in concrete cases is to some extent hypocritical, because the evidence shows that it is not students who need such courses, but senior scientists who have provided recent examples of transgressions of the classical standards of science. The sole existence of such courses implies nothing about their effect, which depends on who teaches them, and what is covered or suppressed in them. [C, p. vi]

The contemporary scene confirms this judgment. As courses in ethics or bioethics multiply, so do also the "examples of transgressions of the classical standards of science". Lang's method of documenting and advertising these transgressions is one of the few tools which may have a chance of contributing to a substantial improvement of this lamentable situation.

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