Structural chaos in Italy

SIR — Your cover for 12 May 1983 symbolically showed Etna erupting splendidly, and your caption contained the phrase "Italy, perhaps the only country in Europe where chaos is structural"; in that issue you published a review of science in Italy under the title "Can order spring from chaos?"[1] We wish to reply that, as far as we are concerned, the answer is still unfortunately "No".

In 1982, *Nature* commented: "Italian science policy appears at last to be getting into gear . . . politicians here have reached a consensus . . . that research is necessary to pull Italy out of its economic crisis"[2]. You later reported[3]: "Scientific hearts are beating faster in Italy . . . as real hopes emerge of genuine reform of Italy's major but chaotic research council, the Consiglio Nazionale delle Ricerche (CNR)".

The CNR has in fact spent relatively very large sums of money on us (an institute for plant virus research), providing new laboratories and an excellent new greenhouse facility, and making big investments in equipment and materials. This is in line with a recent report by the National Committee for Agricultural Sciences that our level of applied and fundamental research is *ottimo* (excellent, the top rating) and that we work with *grande impegno e proficuità* (great commitment and productivity).

Nevertheless, over a period of seven years, there have been no new scientific staff appointments, despite the loss, through retirement or transfer, of four researchers and three technicians from a total of 17 research personnel. The average age of our greying scientists is 51. We are also losing bright young research workers who have trained here and abroad for up to five years, because there is no regular career structure open to them. Thus a really impressive investment by the CNR in buildings and materials has been matched by a relentless rundown in our capacity to profit from this investment.

On 20 July 1990, 10 January 1991 and 21 January, 20 March, 7 September and 15 September 1992, our director wrote to CNR headquarters in Rome underlining various aspects of the situation and repeating our requests to have at least some replacement or new blood, to match the investment in hardware, and the favourable verdict of the peer review committee. There has been no reply to any of these letters, except that the letter of 20 July 1990 was answered on 25 September 1991 (14 months later) by a request for clarification but with no response to our queries. It thus appears impossible to conduct any coherent dialogue with the CNR.

You recently published a letter from Gertrud Lund, recipient of a fellowship in another CNR laboratory, saying that the CNR had sequestered her salary and bench fee, which represent European Communities funds[4]. We can believe her story, as the CNR, for more than a year, has pocketed about 100 million lire (£44,000 or ECU64,000) that we earned through outside contracts. This money would have enabled us to support a graduate student and a technician. We suppose that the sum will eventually be released to us (without interest) but by then the party will be over, and the work, already paid for, will not have been done. We still have no idea when this money or indeed a second tranche due in 1993 will be released.

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on behalf of 32 personnel of the Instituto di Fitovirologia Applicata del CNR, Strada delle Cacce 73, 10135 Torino, Italy

1. Walgate, R. *Nature* **303**, 109 (1983).
2. Walgate, R. *Nature* **297**, 446 (1982).
3. Walgate, R. *Nature* **311**, 501 (1984).
3. Lund, G. *Nature* **357**, 355 (1992).

Procedure for gene patents

SIR — Your leading article (*Nature* **359** 348; 1992) enthusiastically applauding the rejection by the United States Patent and Trademark Office (PTO) of the Venter *et al.* gene patent application (*Nature* **359**. 263; 1992) was, perhaps, premature, and reflects an incomplete understanding of patent prosecution before the PTO.

All that has happened so far is that a lower-level patent examiner has made a routine first-round rejection of the application's claims on grounds of a lack of utility and novelty, and obviousness. First-round rejections in the PTO are made in virtually all cases, particularly in the biotechnology examining group handling this application, and particularly on grounds of obviousness (to some examiners, Noah's Ark makes obvious all subsequent patent applications claiming a boat). It is not necessarily significant. The rejections are based either on the examiner's lack of acumen or his or her wish to make the applicant actively defend the claims.

Contrary to what you say, an appeal, which is a formal proceeding before the Board of Patent Appeals and Interferences (the PTO's highest appellate tribunal), is *not* the next step. If, for policy reasons, the National Institutes of Health (NIH) decide to go ahead with the prosecution of the application, NIH's patent attorneys (usually a private sector patent law firm under contract to NIH) will request reconsideration by the lower-level examiner and will argue against (that is, will "traverse") the rejections.

Only if the examiner again rejects all (or some) claims may NIH appeal to the board to review the rejections. Even if NIH fails to persuade the board to reverse the decision on substantive grounds, or if the board makes a political rather than a legal decision, NIH has recourse to civil litigation before federal courts at three levels.

So, there is a long way to go, and your cock may be crowing prematurely.

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Conflict of interest

SIR — Having read your article about the code of practice in a case of a referee's conflict of interest (*Nature* **360**, 205; 1992), I must say that I agree with John Maddox that "lapses from strict propriety are exceedingly rare", but close examination of the problem would reveal a much bigger problem than suggested in the article. Unfortunately, there were no suggestions as to how the peer system could be improved.

I would like to suggest the installation of a feedback loop in the existing peer review system that connects the referee with the author. In such a system, the authors would have the right to have the name of the referee disclosed after an appropriate time period of perhaps 3–5 years. This would make the referee aware that the author will know his identity and push him to give a fair judgement or to declare a conflict of interest and it could restrain the referee if he/she is not just an altruistic and selfless judge.

A period of 3–5 years is long enough in modern science for conflicts of interest to be manifested in the published work of both sides, yet short enough for the case, if there is one, to be discussed with author, referee and editor. On the other hand, most rejected authors will not be interested in what happened to their manuscripts after such a long time, having published their data elsewhere.

Publications in science are too important to let the referees go 'unrefereed'.

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