

Spanish "toxic-oil" syndrome

SIR — In their dual aetiology hypothesis for the Spanish toxic oil syndrome¹, Root-Bernstein and Westall fail to distinguish between theories and scientific facts. This is precisely one of the major problems we are encountering in Spain when dealing with the several aetiological hypotheses which were put forward in the aftermath of this tragic event.

Infection by *Mycoplasma pneumoniae* was the leading working hypothesis during the first 4–5 weeks, but diagnostic investigations carried out in different laboratories, including the Center for Disease Control in Atlanta in the United States, apparently exclude *M. pneumoniae* as a common infectious agent in the population affected by the toxic syndrome. In fact, only in 2.5 per cent of the investigated cases was *M. pneumoniae* isolated²; furthermore the serological study of 500 paired sera (acute and convalescent) revealed that 2 per cent developed antibodies against *M. pneumoniae*³. There was also no ultrastructural evidence of *Mycoplasma* in post mortem studies of the lung and other organs (M. Rubio, Instituto "Jaime Ferran", CSIC, Madrid, Spain, personal communication). Meanwhile the "toxic-oil" hypothesis was building up on the basis of epidemiological facts and the finding of fatty acid anilides (up to 2,000 p.p.m.) in samples of adulterated cooking oil being used by the affected families.

Fatty acid anilides behave as neurotoxic xenobiotics in rabbits when given orally at a daily dose of 0.01 mg per kg body weight (letter to *Lancet*, in the press) which is one or two orders of magnitude below the expected daily human intake (on body weight basis) from an adulterated oil containing 1,000 p.p.m. fatty acid anilides. In addition, the immunogenicity of fatty acid anilides to rabbits has been established by intradermal challenge in anilide treated animals, solid phase radioimmunoassay of their sera and by the immunofluorescent detection of "anilide specific antigens" in tissue slices of anilide treated animals, incubated with immune serum and fluorescein labelled goat anti-rabbit serum⁴.

Root-Bernstein and Westall suggest that anilides might be acting as "adjuvants" for a hyperimmune response to some mycoplasma. Our experimental findings unambiguously show that anilides alone behave as immunogenic and neurotoxic to experimental animals. A dual hypothesis might not be necessary to explain the Spanish toxic-oil syndrome.

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2. J.G. Riagau-Perez & Winkler, *W.G. Lancet* ii, 724 (1982).
3. Grupo de Investigación Microbiológica, *Boletín Epidemiológico Semanal* (Ministerio de Sanidad y Consumo) 1485, 153 (1981).
4. A. Marquet *et al.*, presented to the "CSIC workshop on the Toxic-Oil Syndrome", Madrid (Spain), March 14–16, 1983.

Soviet scientists under pressure

SIR — In *Nature* of 24 December 1981 (294, 688) you published a letter entitled "Voices from a wilderness". It described the plight of scientists in the Soviet Union who have requested permission to emigrate, including their loss of employment and the revocation of their scientific degrees.

We have recently returned from a visit to the Soviet Union where we met several of the scientists whose names appear on the published correspondence. The purpose of our visit was to deliver offers of visiting professorships to seven of the scientists. Representing the faculty of the University of Southern California, we wanted to express our concern for their rights and academic freedom and to create the opportunity for them to resume research and teaching.

We think that the editor and the readership of *Nature* will want to know of recent events in connection with the December 1981 correspondence. When we were meeting with the Soviet scientists, we learned that the persons whose names appeared on the *Nature* letter were being summoned to the prosecutor's office for intense interrogation. Each was called separately as a "witness" and shown a copy of the *Nature* letter, but with his own name missing from the list of signers.

We were told that one of two goals were intended: one or more of the scientists would agree to be a witness against the others, who would then be prosecuted; or, all the scientists would deny connection with the letter and the authorities could then publicly claim the *Nature* letter was a fabrication of the West. Thus far, we have also been told, those whose names appeared in the *Nature* letter have foiled these intentions. It appeared to us likely they will remain united and continue to refuse to answer the prosecutor's questions.

These scientists have been and are courageous. They face a mounting campaign by the Soviet authorities to intimidate them and their families. A protest by *Nature* and its readership over the Soviets' response to the publication of the letter in *Nature* would, we think, be appropriate.

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Intersexual selection

SIR — Ornithologists for over a century have been fond of criticizing the Darwin-Fisher concept of female choice as a factor in the evolution of extreme sexual dimorphism. The loudest criticisms seem to result from a lack of proper experiments, coupled with a puzzling resistance to attempting them^{1,2}. Andersson's³ study beautifully illustrates the power of experiments in a natural population to clarify age-old disputes in behavioural ecology. However, the report, like many others in the recent literature on sexual selection, continues a curious usage of a term that does not mean what it says, namely "intersexual selection". According to Darwin⁴, sexual selection depends "on a struggle between the individuals of one sex, generally the males, for the possession of the other sex". Therefore, regardless of whether aggression or choice is prominent, sexual selection must always by its very nature be intrasexual; it can never be intersexual.

Although the term "intersexual selection" was not actually introduced by Huxley⁵, his introduction of "intrasexual selection" for aggressive struggles between males for females strongly implied that the principal alternative, "epigamic selection"⁶, must be "intersexual selection." Certain recent textbooks⁷ have unfortunately preferred Huxley's terms to Darwin's. Some recent authors^{8–10} have simply substituted "intersexual selection" for "epigamic selection". The behavioural interactions between the female and male are, of course, intersexual, but the evolutionary selective process, even in mate choice uncomplicated by male-male aggression, is inherently intrasexual. The males compete among themselves for the female's preference and as a result of female preference one genotype of male may increase relative to another.

For Darwin's distinction between preference and aggression as factors in sexual selection, the terms intra- and intersexual selection are at best misleading. Both should be abandoned. In their place the English language provides adequate material with which to make the distinction between sex-ual selection based on aggression and that based on preference. Darwin did not need jargon for this distinction.

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