Letters

Sleep Research

Joseph Palca's Research News article (28 July, p. 351) reports on the third annual meeting of the Association of Professional Sleep Societies (APSS). Prominently featured are several quotes by James M. Kreuger. Discussing research strategies for the study of brain mechanisms on sleep, he is quoted as stating, "I think we're at a dead end in understanding [slow-wave sleep] in terms of neurophysiology and anatomy." He further concludes, "I really don't see where any real progress is going to be made until [we attempt to understand sleep] on a biochemical and molecular biological level." Kreuger's research focuses on the biochemistry of putative endogenous sleep factors. His work and that of others in this area represent an innovative and exciting approach to the study of sleep mechanisms. Unfortunately, his pessimism about neurophysiological and anatomical approaches is presented in an uncritical way, with no dissenting opinion offered. We are concerned that this will foster the impression among nonspecialist readers that Kreuger's remarks reflect a consensus among the sleep research community, which they do not.

The neurophysiological and neuroanatomical study of brain mechanisms of sleep and arousal is a vital and productive area of research. Recent advances include the development of a comprehensive model of brainstem-thalamic-cortical interactions which may constitute the neurophysiological basis of the transition from wakefulness to sleep (*1*), the discovery of neurons in the hypothalamus and basal forebrain which discharge selectively during sleep onset and sleep (*2*), localization of a hypothalamic target for the sleep-modulating effects of prostaglandins (*3*), improved understanding of the neurophysiological factors mediating enhanced seizure susceptibility during sleep (*4*), and investigations of state-related changes in respiratory control which are relevant to the pathophysiology of obstructive sleep apnea syndromes in humans (*5*). A survey of the abstracts submitted for the APSS meeting this year indicates that approximately 50% of the reports on basic sleep mechanisms in animals employed neurophysiological or neuroanatomical approaches. For the previous year it was 44%.

As is the case for other complex behavioral processes, it seems likely that the most profitable approach to brain mechanisms regulating sleep and arousal will include coordinated interdisciplinary studies of neurochemical substrates and of their actions on neurophysiologically and neuroanatomically defined targets.

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REFERENCES

1. M. Steriade and R. R. Llinas, *Physiol. Rev.* **68**, 649 (1988).
2. Y. Ogawa and H. Kawamura, *Brain Res. Bull.* **20**, 897 (1988); R. Szymusiak and D. McGinty, *Brain Res.* **370**, 82 (1986); *Brain Res. Bull.* **22**, 423 (1989).
3. O. Hayaishi, *J. Biol. Chem.* **269**, 14593 (1988).
4. M. N. Shouse, in *Principles and Practice of Sleep Medicine* (Saunders, Philadelphia, PA, 1989), p. 364.
5. R. M. Harper *et al.*, in *Clinical Physiology of Sleep* (American Physiological Society, Bethesda, MD, 1988), pp. 67–78; J. Orem, in *ibid.*, pp. 79–96; R. Lydic *et al.*, in *ibid.*, pp. 97–124.

"Soul-Searching" and China

It is depressing to read (News & Comment, 4 Aug., p. 461) that there is "soul-searching" among scientists as to how to react to the terrible events in China. Called to mind is the aphorism: "The hottest places in hell are reserved for those who, in time of crisis, maintain their neutrality."

Our colleagues under a despotic regime are powerless to change their system. The Tienanmen Square massacre showed that, beyond a doubt. But we (Western scientists, businesses leaders, and governments) may have that power, if we will only use it. The rulers of China are likely to allow change only if it is made too costly for them to do otherwise. That is the elementary argument for strong sanctions and noncooperation at every level, including consumer boycotts.

I am old enough to recall similar "soul-searching" about relations with the Nazi regime before the war ("we must not further hurt the victims"), and one hears the same argument now in opposition to sanctions as a means of helping to end apartheid in South Africa.

As a precondition to resuming normal contacts with China, there should be meaningful signs of change there—for example, official admission (and condemnation) of the brutal and needless killings, a public statement that the democracy movement is not "counter-revolution," cessation of the arrests, release of political prisoners, and restoration of at least the limited human rights that existed before 4 June.

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Carelessness, or Good Faith?

In Roger Lewin's article "The case of the 'misplaced' fossils" (News & Comment, 21 Apr., p. 277), I was repeatedly referred to as one of the numerous experts who had been drawn unwittingly into a possibly fraudulent affair. This is alleged to have happened, according to John Talent's accusations (*1*), when Viswa Jit Gupta provided coauthors with mislabeled fossil material, in my case with ammonoids allegedly from the Himalayas (*2*). Gupta is vigorously denying these charges (*3*), and Lewin is quite right in admitting that "no one can prove absolutely a case in which fossils said to have come from the Himalayas in fact derived from elsewhere." However, while really cogent evidence is, indeed, lacking, the circumstantial evidence assembled by Talent seems to be rather convincing.

If Gupta is guilty of deception, then Talent's whistle-blowing has its merits and certainly is justified, for scientific fraud must never be tolerated. Somewhat irritating, however, are the attempts to extend a part of the incrimination even to the unsuspecting coauthors. If deception occurred then they undoubtedly were the first victims, rather than "unwitting participants," as Talent suggests. Moreover, Talent's harsh verdict stating rigorously that "having had a sloppy approach to the primary facts, they must take mutual responsibility" appears to me definitely not acceptable. By "primary facts" Talent clearly refers to the data concerning the provenance of the material to be studied and published, but in my case there was absolutely no sloppiness in this regard. When Gupta showed me his ammonoids, I immediately recognized the striking similarity to Moroccan material, and I pointed it out to him. He, however, insisted most decidedly that he personally recovered the ammonoids at the Himalayan locality of Khimokul La. Having never been deceived during all of the four decades of my professional life by a fellow scientist, I took his word for granted. Thus, I may have acted, perhaps, in too much of good faith, but

certainly not with technical carelessness.

Incidentally, the weakness of the argument is exposed by the author himself. Publishing twice (*1*) pictures of purchased ammonoids as evidence, he stated without qualifications that they are "from the vicinity of Erfoud, Morocco." Thus, while I trusted the information coming from another scientist, Talent appears to have trusted the information coming from his shopkeeper—hardly a more cautious "approach to the primary facts."

To sum up, whistle-blowing seems to be in our days an urgent necessity. Whistleblowing should, however, refrain from overzealous exaggerations that could easily harm the reputation of marginally involved, but basically innocent, persons.

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REFERENCES

1. J. A. Talent *et al.*, *Courier Forschungsinst. Senckenberg* **106**, 1 (1988); J. A. Talent, *Nature* **338**, 613 (1989).
2. V. J. Gupta and H. K. Erben, *Paläont. Zeitschr.* **57**, 93 (1983).
3. K. S. Jayaraman, *Nature* **338**, 694 (1989).

O'Toole's Charges

Margot O'Toole (Letters, 16 June, p. 1243) states that her charges against the paper by D. Weaver *et al.* (*1*) have not changed since the inception of the controversy over that paper. However, 3 years ago in a memorandum she wrote to me setting out her original charges, she took issue only with what she saw as "serious weaknesses" in the data presented in the paper and in their interpretation. In that paper, the authors attributed the high frequency of the idiotype-positive hybridomas derived from their transgenic mice to idiotype-positive immunoglobulins encoded by endogenous genes rather than by the transgene. O'Toole, on the other hand, appeared to believe "that the observed phenomena are best explained" by three other considerations: (i) an overlooked low-level expression of the transgene in many hybridomas from these mice; (ii) a high frequency of the idiotype-positive hybridomas from normal mice of the same strain; and (iii) heterodimer formation, involving disparate classes of immunoglobulin heavy chains, one from the transgene and the other from an endogenous gene.

In evaluating the dispute I examined the data in the published paper and discussed them extensively with various colleagues. Yet O'Toole says in her letter that I "did not even look at data." It may be that this statement was made because I did not examine laboratory notebooks and O'Toole wishes to convey the impression that it is only through examination of data in the form of raw notebook entries that disagreements, like those in her original memorandum, can be evaluated. But the review of unedited laboratory notebooks is an enormous undertaking with major disruptive effects on the research activities of the laboratories under review. Though opinion may vary over when this drastic process should be applied, it seems reasonable to reserve it for situations where the charges made, such as fraud, are correspondingly drastic. Yet fraud was not suggested in O'Toole's original memorandum, and in response to direct questioning she emphatically denied making such a charge.

Although in the beginning O'Toole focused entirely on disagreements with the authors' interpretations of what she regarded as weaknesses in their data, she has recently adopted the position that there were no data at all to support some of the published results, for example, that certain hybridomas had not been "subcloned."