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plete and correct definition of a planetary nebula, and ended up by saying that a planetary nebula is an object listed in a catalogue of planetary nebulae, but most definitions of them stress their morphology and origin. In any case, the Crab Nebula is not included in any modern catalogue of planetary nebulae, and so far as I know no serious astronomer or astrophysicist has considered it a planetary nebula since the work of Mayall, Baade and Oort in the years around 1940. Surely Steven Weinberg can't have written that caption for figure 1 of his article in the June issue (page 33).

DONALD E. OSTERBROCK Lick Observatory Santa Cruz, California

EDITOR'S COMMENT: Reader Osterbrock is certainly right when he says that Weinberg could not have written the erroneous caption. It was written by a member of the PHYSICS TODAY staff on the basis of information given in the Catalogue of Photographs and Slides of Hale Observatories, which supplied the photo.

Measuring loudness

Although Edgar Shaw has given an overview (January, page 46) of the problem of quantitatively evaluating noise pollution, this reader wishes to dispel any impression that the problem is solved by any of the completely empirical numerical measures discussed in the article. Even the fundamentals are debatable. For example, according to current orthodoxy, a 10-dB reduction in sound-pressure level corresponds to halving the loudness. This result was derived from subjective loudness judgment, or psychoacoustic, tests.1 However, the most recent, extensive (1320 subjects) and unbiased psychoacoustic tests indicate that a 6-dB (not 10) reduction corresponds to half loudness.2 This result receives strong support from mathematical analysis of neurophysiological data.3

Shaw has used A-weighted, sound-pressure level (dBA) as his basic subjective measure of noise. Of the available measures of loudness, this one is probably the best.⁴ Noisiness and other subjective quantities are too complex to be evaluated at present.⁵ In any event, for a subject in a given emotional state, loudness may serve as a satisfactory indicator of relative noisiness. Unfortunately dBA is not a very good measure of loudness. For example, the loudness level of a steady sound at constant dBA may vary up to 15 phons (decibels) depending on the bandwidth of the sound.

The broader the bandwidth, the louder the sound. This is certainly a significant effect. A new measure of loudness is called for.

Basic studies of human response to sound as related to noise pollution seem to have been dominated by psychologists. However, an understanding of the connection between sound and loudness requires knowledge of intermediate operations of the auditory system, that is, of human physiology, especially neurophysiology. Ultimately the data collected by psychologists and physiologists can best be correlated into mathematical theories of the sensations by physicists, since the system must obey physical principles. For this reason the still unsolved problem of quantitative evaluation of noise pollution offers a fertile field for the physicist.

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WALTON L. HOWES Middleburg Heights, Ohio

THE AUTHOR COMMENTS: To cover the entire field of noise pollution in a single article was no easy task. The point mainly at issue was touched on in the single clause "a 10-dB increase in sound level (more properly, loudness level) is perceived as a doubling of 'loudness' " (page 46 of my article). This, I am assured, correctly reflects a well-established scientific consensus in an area of psychophysics fraught with difficulty. The subject of loudness is of course, like all scientific questions, open to further enquiry. I am indebted to Howes for drawing attention to noise pollution as a fertile field for the physicist. It is a point I intended to make.

EDGAR A. G. SHAW National Research Council Ottawa, Canada

The 20-micron window

In my review on "The Past and Future of American Astronomy" (December, page 23) I refer to the work of H. Rubens and E. Aschkinass, in the course of which I stated that "in essence they also found the 10- and 20-micron windows in the Earth's atmosphere." The phrase "in essence" is—at least for the 20-micron window—too strong. Ru-

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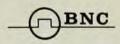
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bens and E. Nichols2 had earlier found only small absorption at 24.4 microns for small paths of carbon dioxide and water vapor. Rubens and Aschkinass accordingly suggested that this might be a region of relative transparency in the Earth's atmosphere. They failed, however, to find sunlight in this wavelength region but, repeating the earlier laboratory experiments, confirmed the relative transparency of CO2 and H2O here. They proceeded to test with longer paths and discovered a perceptible absorption by water vapor for a 40-cm cell-one of the earliest findings of the pure rotation spectrum of water. Rubens and Aschkinass then erroneously concluded that the terrestrial atmosphere is "wholely opaque" between 12 and 20 microns and at 24.4 microns. It seems possible that their failure was due to the poor sensitivity of their detectors. Their initial hunch was valid. However, the first direct demonstration of transmission of sunlight in the 20micron region was made at Lowell Observatory by Arthur Adel.3 I am happy to acknowledge his priority in the matter, but remain impressed by the near miss of Rubens and Aschkinass.

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Ivory tower dreams?

The recommendations on employment problems in astronomy from the National Academy of Sciences (May, page 70) are certainly based on futile dreams in an ivory tower. Its authors need to realize that: (1) Many apprentices to research are neither temperamentally nor intellectually suited to teaching in junior colleges; (2) Faculties that have no separate astronomy staff seldom have funds or political support to consider new appointments; (3) Replacement of teaching assistants (an endangered species) with more expensive qualified staff is against the current of belt-tightening in all universities.

Astronomers and physicists must stop blaming political leaders for the decline in prestige of our disciplines. As teachers we need to spend enough time with our students to convey not only the excitement of discovery, but also respect for precision in both verbal and numerical concepts. The community can well afford to support scholarly inquiry, but the fruits of new knowledge need to be shared more widely in every-

day language as well as in the exclusive jargon of a privileged club.

WILLIAM BUSCOMBE Northwestern University Evanston, Illinois

Reactor safety defended

I have read the letter on the AEC reactor-safety study by Herbert Malamud in the February issue (page 71), and I think that a reply is very much in order.

When the original article by David Burnham appeared in *The New York Times*, I wrote to their editor protesting the misrepresentation implicit in the article. My best answer to Malamud is to quote the text of my 12 November 1974 letter to the *Times*.

"Since I am the 'Mr Smith' identified as one of the participants in the WASH-740 revision featured in David Burnham's article castigating the Atomic Energy Commission (Nov. 10), I feel that I must comment on his serious allegations.

"Mr. Burnham was quite correct in stating that Brookhaven National Laboratory reviewed the question of power reactor safety in 1964-65, and my calculations did indicate that an area the size of Pennsylvania could be involved, provided the maximum hypothetical accident were actually to happen. What he ignored completely is that the scientists working on this project considered these calculations as the absolute limit of conceivable results, not as an event that had any likelihood of happening. We were firmly convinced that the probability of any power reactor accident causing serious injury to the public is infinitesimal.

"I might add that most current critiques of reactor safety, by environmentalists, the Environmental Protection Agency and even by the Atomic Energy Commission, are in my opinion ultraconservative to the point of absurdity. I have been associated with radiation safety problems throughout most of my career, and I see no reason to treat them with yardsticks totally different from those applied to other human activities.

"The point is that neither I, nor any of my former Brookhaven colleagues as far as I am aware, felt that this study revealed any hazard worthy of genuine public concern, and I certainly felt no qualms of conscience when a final report on our work was never completed.

"I trust you will extend the minimum courtesy in publishing this letter since Mr Burnham has been given the front page to accuse the Atomic Energy Commission of sweeping a major safety problem under the rug, and to give the totally erroneous impression that we are all exposed to great danger."

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