

several orders of magnitude, as it is on Jupiter. Applications of hot hydrogen organic chemistry to the atmospheres of Jupiter and the other Jovian planets have also been published (3).

The claim (1) that these results are relevant to the interstellar medium is, however, extremely dubious, if it is gas-phase interactions that are being invoked. Even though hydrogen atoms produced in such experiments will be substantially superthermal in the interstellar medium, the three-body reaction rate coefficients are so low that no reactions of any importance can proceed until the gas densities are already so high that we are considering a collapsing interstellar cloud, on its way to forming stellar or planetary systems (4). This objection applies equally well to some other experiments purportedly of relevance for interstellar chemistry (5). However, in the near-ultraviolet irradiation of frozen ices—including frozen HCHO, an observed interstellar constituent—hot hydrogen atoms are produced and there is sufficient mobility at low temperatures to initiate further organic synthesis (6). Thus the production of hot hydrogen atoms on interstellar grains or cometary surfaces may be of relevance to interstellar organic chemistry.

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There are several considerable differences with respect to the reports of Hong *et al.* (1) and Sagan and Khare (2). Among these are the following. The production of protein amino acids as well as other amino acids was authenticated by gas chromatography–mass spectrometry by Hong *et al.* (1). Furthermore, some additional protein amino acids not reported by Sagan and Khare (2) were found (1, 3). Still other organic compounds were produced and

verified by gas chromatography–mass spectrometry by Hong *et al.* (1), and some of these in turn were used as intermediates for the production of amino acids. The one-carbon-atom substrate CH<sub>4</sub> was found to successfully give amino acids (and other organic molecules), and hydrolysis of a polymer was not believed to be a major source for the production of amino acids (1). Quantum yields were experimentally determined by Hong *et al.* (1).

The results reported by Sagan and Khare (2) indicate the strong possibility of mercury-sensitized reactions occurring with traps at temperatures as high as –23°C and even at –64°C unless true equilibration with the walls has been attained, especially with the use of a mercury line resonance source (3). Experiments 4 and 5 of (2) are not clear since, if Pyrex excludes ultraviolet light in experiment 4, it should also exclude it in experiment 5. In addition, all reactions in which amino acids were obtained (except experiment 5) were conducted at very elevated temperatures where reactions of thermal hydrogen atoms can become important.

There is no way to resolve such questions as whether HCHO and CH<sub>3</sub>SH were abundant in the atmosphere of the primitive earth. Furthermore, these compounds in themselves are not important; their significance arises from the fact that they are representative of hot hydrogen atom sources. It is worth pointing out that in many instances the production of some of these compounds is the key issue; for example, C<sub>2</sub>H<sub>5</sub>OH has recently been found in interstellar space.

In (4) Khare and Sagan do not discuss reaction products from frozen ices as arising from the reactions of hot hydrogen atoms. Indeed, until it

can be shown that the products do arise in this way by independent known hot hydrogen atom reactions, not by thermal hydrogen atom reactions, and, importantly, not through formyl radical reactions, such a proposition would be purely speculation.

Three-body collisions are not required for the formation of reactive radicals via hot hydrogen atoms. At least in abstraction reactions, such radicals are commonly formed “cold.” Even if these radicals and others were not formed “cold,” they could become “cold” by radiative processes or collisions such that, upon reaction, any translation energy and enthalpy of reaction could be taken up by nondissociative internal modes in the molecule. If the lifetime of the metastable molecule is long enough, radiative deactivation would stabilize the system. Also, a second two-body collision of the metastable molecule with a molecule, notably H<sub>2</sub>, would stabilize the system (5). It was never our intention to exclude from consideration three-body collisions with dust as a third body, and indeed this type of collision is a strong possibility (5).

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## Erroneous Date for Chilean Glacial Advance

In their report Mercer and Laugenie (1) discussed the implications of an age of about 36,000 years (sample I-6348) for a tree pushed over by advancing ice in south-central Chile. The Quaternary Isotope Laboratory at the University of Washington has recently dated a sample of this wood and obtained an age of 56,000 ± 2,000, –1,700 years (sample QL-61).

The counter size and the background counting rate determine the maximum

age range that can be measured in a specific counter. The maximum age range of the counter for which the 36,000-year date was obtained appears to be about 39,000 years. Evidently the 36,000-year date was either the result of statistically unexpectedly large variations in counting rate that resulted in the reported finite age, or it was caused by a less likely contamination of the sample with about 1 percent of recent material.

The maximum age that can be measured in the larger counter at the Quaternary Isotope Laboratory is about 61,000 years. Because a 56,000-year-old sample has retained only 0.1 percent of its original  $^{14}\text{C}$  activity, this age has to be considered minimal until further experiments have been carried out. Although the new age is a lower limit only, it is clear that Mercer and Laugenie's conclusions based on the 36,000-year date are invalid. However, they interpreted the wood and the underlying interbedded peat, ash, and clay as interstadial, not interglacial, on account of the stratigraphic position as well as the apparent age of this nonglacial sequence. Reexamination of the site in September 1974 by Mercer and Moreno supports this interpretation, the strongest evidence being the slight weathering of the diamicton, thought to be till, immediately below the nonglacial sequence, compared to the intense weathering of all known examples of till

of the penultimate glaciation, even where buried by younger glacial deposits. The new radiometric age determination would require that the interstadial occurred early in the last glaciation. However, the evidence for this chronostratigraphic interpretation is not conclusive, and the nonglacial sequence may date from the last interglacial.

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## Dimensions of Olfactory Quality

Schiffman (1) has recently applied nonmetric multidimensional scaling techniques to the problem of the dimensions of olfactory quality. Her reanalysis of data collected by Wright and Michels (2) and Woskow (3) yields similar two-dimensional solutions, one dimension clearly having to do with pleasantness and the other being more opaque but apparently relating to spiciness or sharpness. Showing that new techniques allow an approximate replication of this configurational space using weighted physiochemical properties, Schiffman (1) argues that such methods hold promise for discovery of underlying olfactory receptor mechanisms. In so doing, she fails to take into account central processes that mediate judgments concerning the pleasantness dimension. Research workers in olfaction have had to labor under the hardship of not beginning with a fairly good idea of what the relevant dimensions were. By analogy, let us imagine that psychophysics had begun without any notions of the relevant dimensions of vision and audition. Some sort of multidimensional approach wherein subjects were asked to rate or classify a miscellany of objects and events—such as girls, chairs, whispers, rainbows—might seem to give promise

of yielding relevant dimensions. But would we emerge from our analysis with dimensions such as hue, brightness, size, shape, and pitch? Osgood, Suci, and Tannenbaum (4) in asking subjects to rate words were essentially asking them to rate mental images varying along the basic visual, auditory, and other dimensions. They, of course, obtained factors of evaluation, activity, and potency. Now the first two are the same dimensions that Schiffman obtained. But for the fact that it was known in advance that concentration or strength was a relevant dimension and must be controlled, a potency factor as well would probably have emerged. Even if we could replicate Osgood's semantic space, using some set of physical properties of the rated objects, we would not search for pleasantness receptors in the retina or cochlea since we know that connotative meaning is a central and not a peripheral phenomenon.

Pleasure would seem to be a central process based upon the arousal potential or impact value of stimuli. As such, it can be induced by variations along a number of stimulus dimensions. Wundt (5) first presented the hypothesis that hedonic tone is an inverted-U function of stimulus intensity, with stimuli of

medium intensity being felt as pleasant, those of greater intensity as unpleasant, and those of low intensity as neutral. Subsequent research has confirmed this notion as well as suggesting that pleasantness is related in a similar inverted-U fashion to variables such as meaningfulness, complexity, and novelty.

If such pleasure dimensions as the one obtained by Schiffman are based upon central processes, then they cannot be connected in any direct way with receptor mechanisms. There are two reasons for this. First, since compounds may gain their arousal potential by simultaneous variation along any of a number of fairly independent dimensions, compounds that are similar in their pleasantness are not necessarily similar in their positions on the basic dimensions. The more diverse or "representative" the stimuli being judged, the less likely it is that similar pleasantness derives from similar positions on the underlying dimensions. The method, then, actually confounds the basic olfactory dimensions rather than disentangling them to the extent that it is applied to judgments of heterogeneous sets of stimuli. Moreover, only some of these stimulus dimensions, such as molecular weight and Raman intensity, may be of relevance to olfaction. Others, such as familiarity and stimulation of trigeminal nerve endings, may not be. These dimensions are not equally represented or controlled in the stimuli used by Schiffman. Some of her stimuli are believed to stimulate not only olfactory but also trigeminal receptors (6). Optical antipodes, such as *d*- and *l*-carvone, which have the same molecular weights and vibrational spectra but different odors (7), are not included.

Second, because of the curvilinear relation between arousal potential and pleasure, the method systematically obscures the relevant dimensions. For example, in Schiffman's dimensional space the light carboxylic acids are unpleasant while a heavy, less volatile one is pleasant. If we assume that arousal potential varies directly with volatility, then we can predict that an even heavier one would fall between these groups in the neutral area because of its even lower arousal potential. The three groups as ordered on the pleasure dimension would be out of order on the more basic volatility dimension, and it is the latter which would be expected to relate in a

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