Letters

Frequency or Wavelength?

There is a rational way of plotting spectra and spectral functions that is full of physical meaning and correspondingly useful; and there is an irrational way of plotting them that has no direct physical meaning, and that distorts their shapes. The first way is to plot them on a scale of frequency or its equivalent; the second is to plot them on a wavelength scale. By a curious historical mischance, this second procedure has become a habit, in part among physicists, and almost universally among chemists and biologists. The habit has become so deeply ingrained that to break it now will demand a great effort and, however consistently that effort is maintained, will take a stretch of years. One of the greatest embarrassments is the degree to which that habit has been implanted in the design and construction of our measuring instruments.

Nevertheless the time has come, and is indeed long overdue, to make that change. Spectra, and spectral variations of every kind, are acquiring more and more concrete physical meaning, and all that meaning is in terms of frequency scales. To give spectra physical meaning, one has as a regular thing nowadays to transpose them laboriously from the wavelength scale in which they are commonly plotted to a frequency scale. Whatever time and effort it may take to revise our present practices will save vastly more time and effort in the end.

A frequency scale has many advantages:

1) The energy of a photon is directly proportional to the frequency ($E = h\nu$). A frequency scale therefore represents directly the scale of energy relations.

2) In many types of investigation these energy relations are of direct concern. In photochemistry and photobiology in particular, one is primarily interested in the energy content not of a photon but of one mole of photons, a so-called Einstein ($E = Nh\nu$). When spectral variations are plotted on a frequency scale, there is no difficulty in transposing them directly into such energy units as kilocalories, electron volts, or watt-hours, which could be designated directly on a parallel scale.

3) Plotted on a frequency scale, the absorption bands of molecular spectra tend to have the shape of normal distribution curves, whereas on a wavelength scale they are skewed in shape through compression at the shorter wavelengths.

4) Absorption bands that have the same width on a frequency scale have very different widths on a wavelength scale, those that lie at shorter wavelengths appearing narrower than those that lie at longer wavelengths. This of course makes an altogether spurious impression.

5) The areas under absorption bands plotted on a frequency scale have a real and very useful physical meaning, since they represent transition probabilities. This meaning is lost on a wavelength scale.

6) Not only the areas under such bands but their half-widths have physical meaning on a frequency scale. They are an approximate measure of transition probabilities and a measure of so-called oscillator strengths. None of this is apparent on a wavelength scale.

On grounds of usefulness and meaning, therefore, a frequency scale has all the advantages. Things that are the same look the same on a frequency scale but look very different on a wavelength scale. Aspects of curves that have physical meaning on a frequency scale lose this meaning on a wavelength scale. . . .

The proposal to go over generally to a frequency scale raises the problem of units and of the direction of the scale. I would favor an ascending scale of frequencies reading from left to right. I am told that this may introduce some inconvenience in the recording of infrared spectra, in which the principal interest often involves the higher frequencies, which one might therefore like to record first; but that could be taken care of by recording such spectra in reverse, though they would still read on an ascending scale from left to right.

Obviously a scale of frequencies (c/λ) involves inconveniently large numbers, going also far beyond the number of significant figures in measurements. Also, the transposition from frequency to wavelength is inconvenient. For these reasons a scale of wave numbers ($1/\lambda$) in cm^{-1} is more convenient. In the visible spectrum such a scale involves five digits, whereas three or four would be better for most purposes. For this reason wave-numbers in mm^{-1} should be considered.

A third possibility involves the so-called Fresnel unit, the frequency $\times$ 10^{-12}. The special convenience of this unit is that the numbers are manageable—three digits between 300 and 3000 $m\mu$—and all significant. It so happens also that a wavelength scale from 400 to 750 $m\mu$ becomes a scale of 750 to 400 Fresnel units.

It will take some time to change people's habits, to go through the discomfort of dealing with two kinds of scales, one in the old literature, the other in the new, and most of all to get our instrumentation over into the new form. Obviously, this is not a reform to undertake individually. The heart of the business is to get the editors of scientific journals to insist upon the new mode of presentation. That will probably demand affirmative action by the governing boards of sponsoring societies and by the appropriate committees of such organizations as the International Union of Pure and Applied Chemistry. Once such a change as this is agreed upon, we should look forward to a period in which all spectra are presented on both scales, but drawn linearly with frequency, and with the frequency scale as abscissa below the graph, a nonlinear wavelength scale appearing above the graph. Eventually it should be possible to drop the wavelength scale entirely, and use a scale of wave numbers or Fresnel units alone.

The substance of this letter was discussed informally at the October 1964 meeting of the board of directors of the Optical Society of America, and has been commented on editorially by S. S. Ballard in *Applied Optics* (Feb. 1965), p. 219. Informative discussions of these issues will be found in G. H. Beaven, E. A. Johnson, H. A. Willis, and R. G. J. Miller, *Molecular Spectroscopy* (Heywood, London, 1961), pages 13–15, and in A. E. Gillam and E. S. Stern, *Electronic Absorption Spectroscopy in Organic Chemistry* (Arnold, London, 1958), page 14.

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Skeptic

E. G. Sherburne, Jr., in editorializing (17 Sept., p. 1329) on TV coverage of the Gemini program, expresses confidence in the television industry as a competitive enterprise. He expects that TV coverage of this area of technology and science will improve because "the networks which excel in their scientific homework [and hence, presumably, in their performance] will excel in the marketplace."

This is a rather remarkable conclusion for someone to reach—unless, of course, he spends little time watching commercial television.

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Mass Extinctions of Mesozoic Biota

My brief summary of speculation on the subject of mass extinctions of Mesozoic biota (25 June, p. 1696) was published largely with the hope of evoking critical evidence (pro and con) before pursuing further any hypothesis that may prove too improbable. The vulnerability or the needed documentation of several points was, I hoped, made evident. My brevity, however, may account for some misunderstanding indicated by comments in a letter by Newell (27 Aug., p. 922) and in personal communications from others. Points considered "vulnerable" by Newell seem to require additional comment for their more adequate consideration.

Newell's evaluation of the supply of nutrients by run-off from the land to the oceans considers only the annual contribution. That this is almost negligible as compared with the upwelling nutrients from the ocean reservoir seems well recognized in my statement, "The volume of nutrients in the depths of the vast oceanic reservoir might appear nearly inexhaustible to the biologist, but it appears that the supply of nutrients from the ultimate source on land decreased over some millions of years." Considerable (if inadequate or unconvincing) support for this statement formed a large part of my paper. Criticism of this would seem pertinent, rather than of what would have been an obvious inadequacy if Newell's point had not been recognized or had been questioned.

Important, although still inadequate, data from geochemists on the residence-time of inorganic elements in the oceans are now well known and were not reviewed in my brief paper. Such data are even less satisfactory, however, on organic constituents among the nutrients. The nutrient requirements of various groups of microplankton under diverse conditions involve many complications, but both the organic and inorganic substances must have the land surface as their principal original source. Hutchinson, in a paper in the just published *The Scientific Endeavor* (Rockefeller Inst. Press), makes the interesting statement regarding the open oceans that "it is possible that iron, which is almost insoluble under oxidizing conditions in inorganic aqueous systems, usually limits the amount of living matter. . . ." This may prove especially significant under my suggested rather long-term conditions in the oceans.

However, even those nutrients that are most effectively recycled through upwelling and other ocean currents are partly lost to the bottom sediment in the process—especially to the relatively rapidly accumulated hemipelagic and nearer shore bottom sediments. Subnormal replenishment of the reservoir involving a geologic time of some millions of years seems expectable from the indicated conditions on land of that time, and thus any of many critical substances needed by phytoplankton could have become inadequate.

The importance of ocean currents, and especially of upwelling, is so well known that it could hardly have been overlooked by one associated with an oceanographic institution, but some evidence suggested that their intensity and effectiveness may have decreased under the conditions of that time. Indeed, it was this result from the warmer, more uniform, and perhaps thicker, surface water—making the conditions somewhat more comparable to those of laboratory cultures—coinciding with a then "deficient diet" in the deep ocean reservoir, that might best account for the worldwide destruction among marine populations.

In a personal communication Roger Revelle has commented that the long-term and widespread stabilizing effect of more marked stratification, deterring upwelling currents, might have been a more important and immediate factor than an impoverished reservoir in profoundly affecting marine life. This may well be, although without the additional factor of a considerably depleted reservoir it would seem to me probable that some large regions would have had sufficient current movements for adequate nutrient supply. Under the latter conditions alone, a continued or perhaps increased "geographic speciation" might be more expected than the wholesale and worldwide extinctions of so many previously thriving populations that are recorded. In any case, the relative importance of the two factors (and other, perhaps related, ones) seems more difficult to test and evaluate than whether or not a partially depleted ocean reservoir could have been a significant factor in the event.

There seems little question on the less pronounced or abrupt effects upon land plants at that time, and I will not here attempt additional discussion of the land animals. Perhaps these land animals indicate more profound and abrupt destruction of many thriving populations than my limited information would indicate. Certainly there were important evolutionary changes during that time, and perhaps one of the "explosive evolutionary periods." Newell is in a position to obtain more complete information on this than am I, and its presentation would permit a better consideration of whether or not abrupt *extinctions* on land were comparable to those in the open oceans.

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Extrasensory Induction of Brain Waves

Duane and Behrendt believe they have demonstrated "extrasensory electroencephalographic induction between identical twins" (15 Oct., p. 367). If