

Thunderstorm Activity

Wilcox *et al.* (13 Apr., p. 185), show a convincing relation between the solar magnetic sector structure and the earth's meteorological activity as indicated by vorticity at the 300-millibar level. Is it possible that this interesting correlation may in some way be related to Markson's observation (1) that there is a maximum in thunderstorm activity when the earth is at the leading edge of a negative solar structure?

B. VONNEGUT

*Atmospheric Sciences Research Center,
State University of New York,
Albany 12222*

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The Laboratory Frog

As a coauthor with E. L. Gibbs and G. W. Nace of a number of articles dealing with frog health and having worked with the tetracycline treatment (1) for the common frog diseases, I am in full agreement with Papermaster (Letters, 6 Apr., p. 10) that the treatment represents an excellent stopgap solution to the complex and serious problem of laboratory frog disease (2).

I agree with Papermaster's suggestion that laboratories refuse to pay for dead frogs, but I seriously doubt whether this will lead to improved preventative measures. Most suppliers already issue credit for frogs that are dead on arrival.

Gibbs, Nace, and I discussed in an article entitled "The live frog is almost dead" (3) the complex reasons for the poor condition of the average laboratory frog. Speaking as a supplier seriously concerned with the quality of his product and the quality of science, I suggest that greater progress toward an improved laboratory frog will be made most quickly if researchers are willing to pay slightly higher costs for specially handled animals.

This is not a sales pitch aimed at developing larger profits; it is simply a reflection of the fact that most of the time you get what you pay for, especially in a competitive market.

MARVIN B. EMMONS

*Science Education Division,
Nasco,
Fort Atkinson, Wisconsin 53538*

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2. *Amer. Zool.* **13**, 71 (1973).
3. E. L. Gibbs *et al.*, *BioScience* **21**, 1027 (1971).

The Born-Einstein Letters

The correspondence between Albert Einstein and Max Born, published in 1969 in book form in the original German (1), is a record of "much that will prove invaluable source material in the history of science" [Bertrand Russell in the foreword (2)]. It therefore seems important to correct an unfortunate slip in the English translation of the letters (2) which makes it incorrectly appear that criticism of Fritz Haber, in which Einstein joined Born, applied to Kasimir Fajans.

One reads in Einstein's letter of 27 January 1920 (2, p. 21): "Haber is complaining bitterly about Fajans. You have described the latter very well. He is unaware of the number of arbitrary assumptions he makes and vastly overestimates the value of consistent results. . . ."

The German original (1, p. 32) reads: "Über den Fajans schimpft auch Haber tüchtig. Letzteren hast Du sehr gut gekennzeichnet. Er merkt nichts von der Zahl seiner willkürlichen Annahmen und überschätzt deshalb den Wert der gefundenen Übereinstimmungen masslos. . . ."

In the English translation, the sequence of the two names was changed from that in the German original, but the word "latter" was not correspondingly replaced by "former." Clearly, in the German original, it was not Fajans, but Fritz Haber whom Born "described very well" with criticism which Einstein then amplified.

The error affected a man of outstanding merits. Fajans' part in laying the foundations of classical radiochemistry is history [for example, the Fajans-Soddy displacement law, which was formulated by Fajans independently and published in 1913, slightly before the paper by Soddy (3)]. Hardly less fundamental are his contributions to the theory of chemical binding [the Born-Fajans-Haber correlation (4), the "Fajans Rules" on polarity, and the comprehensive experimental studies and interpretation of mole refraction as a guide to an understanding of chemical interaction].

While no explanation is found for Haber's "bitter complaint" about Fajans,

Einstein's opinion of his friend Haber is illustrated in an earlier letter to Born, of 9 December 1919 (2, p. 19). Citing his "forceful methods for trying to wrest truth from nature," Einstein calls Haber "a kind of raving barbarian, but very interesting all the same." Privileged by long friendships both with the late Fritz Haber and with Kasimir Fajans (now professor emeritus at the University of Michigan), I agree with Fajans' characteristically objective comment to me that Einstein's criticism of Haber "is too sharp and too general."

It seems to me that Haber might well be judged a "romantic" scientist under the scheme once proposed by Wilhelm Ostwald (5), who classified great scientists depending on their "psychography," or work style, into two types: "romantic" and "classical." Fajans seems to fit the "classical" definition.

MAX A. BREDIG

*Oak Ridge National Laboratory,
Oak Ridge, Tennessee 37830*

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2. *The Born-Einstein Letters*, translated by I. Born with commentaries by M. Born (Walker, New York, 1971).
3. K. Fajans, *Radioelements and Isotopes: Chemical Forces and Optical Properties of Substances* (McGraw-Hill, New York, 1931), p. 26, especially footnotes 1, 2, and 6; F. Soddy, *Nobel Lectures, Chemistry, 1901-1921* (Elsevier, Amsterdam, 1966), pp. 371-399, especially pp. 387 and 397.
4. D. F. C. Morris and E. L. Short, *Nature* **224**, 950 (1969).
5. W. Ostwald, *Lebenslinien* (Klasing, Berlin, 1927), vol. 3, p. 115.

The Fudge Factor

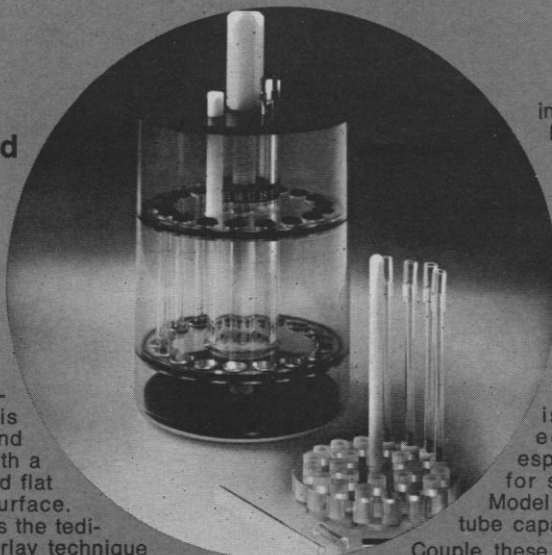
Richard Westfall (23 Feb., p. 751) describes the more or less arbitrary nature of Newton's corrections to his calculations of the velocity of sound in air, the acceleration of gravity at Paris (from the moon's motion), and the precession of the equinoxes. The term "fudge factor" is applied to these corrections. However, in fairness to Newton, one should also point out that the process of reasoning backward from an experimental result to correct an inadequate theoretical result is not necessarily dishonest. It may in fact be wholly scientific when used to extract information about probable side effects; and if the theory-minus-side effects is assumed sound, then the calculation becomes a demonstration of the magnitude of these perturbing effects.

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discussion, Newton apparently postulated some physical effects in air which might account for the inadequacy of his calculations of the velocity of sound. Such hypotheses were not in themselves unjustified. Scientific progress often takes its beginning from inspired guesses. The burden of disproof rests upon the experimenter. Wrong is only done if the calculator pretends to assign quantitative exactitude to each of several hypothetical effects which together are invoked to account for a total discrepancy. Newton's analysis merely suggests that the individual effects may have magnitudes of the order he assigned, and it should not be read in any other light. Considering the primitive state of the knowledge of thermodynamics in the late 17th century, his speculations about the velocity of waves in damp air were probably the best that could be done.

There is nothing unsound in making hypotheses to bring theory and experiment into agreement; but the hypotheses should be tested out by further investigation, and not presented as the last word.

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Readers who found Westfall's article "Newton and the fudge factor," of great interest, as I did, might like to look at what one man, at least, took to be some fudging in Newton's development of calculus—or "fluxions," as he called it. I am referring, of course, to George Berkeley's *The Analyst* (1734) and the additional writing to which it led. This can be found in any collection of Berkeley's works. Not only is it of historical interest, but there are points raised which even now a teacher of introductory calculus, in particular, might do well to ponder.

Incidentally, is there not a moral to be found in all of this? Is it not likely that a time will come when some of the work of which we are now most proud will be seen to contain outrageous fudges? Nor is it too hard to see some places in which this might come about. Our "renormalizations," for instance, may some time be called by a much less charitable name. It seems now to be agreed that some of the early papers on relativity contained actual mistakes, which had the same effect as fudges.

In one sense, we need not make too much of all this. It would appear that

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"to fudge is human." But because of that very fact, we should be ready to admit the possibility that we may, perhaps almost unconsciously, have committed a fudge, or overlooked one by someone else. Recently, when Dingle (1) and others suggested that there is something wrong—call it a mistake or call it a fudge—with the theory of relativity, what they encountered could scarcely be described by any term other than "persecution." Earlier, O'Rahilly, who had rather similar experiences, said that the heretic is treated worse in physics than in theology. We should remember that a theory which is to a great extent true may still be fudged here and there; those who point this out are in the same position as physicians who tell us that, while we are mainly healthy, we have such and such an illness, which fortunately can be cured.

H. L. ARMSTRONG

Department of Physics,
Queen's University,
Kingston, Ontario, Canada

Reference

1. H. Dingle, *Science at the Crossroads* (Martin Brian and O'Keeffe, London, 1972).

While reading Westfall's article I was reminded of an anecdote told by the late J. C. McLennan during a lecture at the University of Toronto about 40 years ago.

McLennan said, as I remember, "One time I remarked enthusiastically to Nils Bohr, how wonderful it was that his equations yielded such an accurate value of Rydberg's constant. Nils said to me 'Of course, McLennan, I made it come out that way.'"

McLennan then said to us, "Perhaps that is the difference between Nils Bohr and me."

ARTHUR H. BOULTBEE

70 Bush Avenue,
Greenwich, Connecticut 06830

In reply to McHugh, I am unable to find in Newton's language any grounds for the assertion that he offered the two corrections to the velocity of sound as hypotheses to account for the discrepancy. He did not present them in such a manner. Moreover, he assigned "quantitative exactitude" to them, and did so without any evidence external to the calculation that such "side effects" even exist.

RICHARD S. WESTFALL

Clare Hall, Herschel Road,
Cambridge, CB3 9AL, England

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Effects of Marijuana Use

John Kaplan's review (12 Jan., p. 167) of the recent American and Canadian government-sponsored reports (1, 2) on cannabis does justice to neither.

Like the British Wootton Report of 1968 (3), these North American studies did, indeed, recommend a more humanitarian approach to the legal issues. What Kaplan fails to mention is that the reports contain much cautionary clinical material which led both the American commission (1, p. 134) and the Canadian commission (2, p. 301) to conclude, as had the British committee (3, Section 71), that the use of marijuana was to be discouraged for various individual and public reasons.

These reasons are perhaps best stated by the Canadian commission (2, p. 274):

To sum up, then, it seems to us that there are at least four major grounds for social concern: the probably harmful effect of cannabis on the maturing process in adolescents; the implications for safe driving arising from impairment of cognitive functions and psychomotor abilities, from the additive interaction of cannabis and alcohol, and from the difficulties of recognising or detecting cannabis intoxication; the possibility suggested by reports in other countries and clinical observations on this continent, that the long-term heavy use of cannabis may result in a significant amount of mental deterioration and disorder; and the role played by cannabis in the development and spread of multi-drug use by stimulating a desire for drug experiences and lowering inhibitions about drug experimentation.

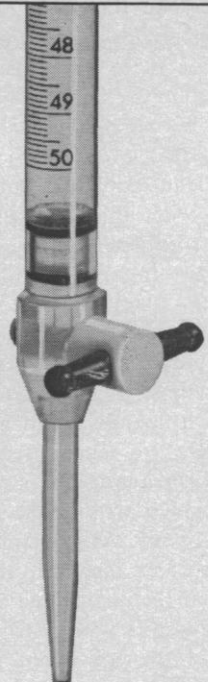
A number of people have discontinued the use of cannabis because of these and other problems, and even continuing users are becoming more willing to admit that—as Gabriel Nahas demonstrates in his excellent coverage of the subject (4)—marijuana is a "deceptive weed."

CONRAD J. SCHWARZ

Student Health Service and
Department of Psychiatry,
University of British Columbia,
Vancouver 8, Canada


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