

Michelson-Morley experiment implies the speed of light is universally constant in all inertial frames.

History also bears witness against the inductivist position. If we read carefully the text of the speech from 1922 and take into account the words of Robert S. Shankland [American Journal of Physics 31, 47 (1963)]: "When I asked him [Einstein] how he had learned of the Michelson-Morley experiment, he told me that he had become aware of it through the writings of H. A. Lorentz, but only after 1905 had it come to his attention," we see there is no immediate connection between the Michelson-Morley null result, or similar experiments, and P2.

According to Zahar, the invariance of  $c$  could already be deduced from the Lorentz theory before 1905. But even the Lorentz system was not constructed to explain the Michelson-Morley result. All the formal apparatus developed by Lorentz, very useful for Einstein, had nothing to do with the Michelson-Morley experiment. The Lorentz transformation was originally only a mathematical tool. Namely, the Lorentz transformation appeared to reduce the form of the equation of electron motion in electric and magnetic fields from an arbitrary moving frame of reference to the frame at rest with respect to ether.

In a paper in 1895 Lorentz tried to give a physical interpretation of his own transformation. One step in that direction was the Lorentz-Fitzgerald contraction hypothesis. Another one was the Theory of Corresponding States. The final form of the latter theory given by Lorentz in 1904 (the paper unknown for Einstein before 1905) might be turned into a theory observationally equivalent to special relativity: from a formal point of view, Einstein differs from Lorentz in that he regards the "effective" Lorentzian variables in a moving frame of reference as the real ones and presumes only the Lorentz transformation as that which relates inertial frames.

Zahar claims that "it was not Michelson, the experimentalist, but Lorentz, the theoretician, who played a considerable role in the genesis of Special Relativity." The new heuristic point of view taken by Lorentz as well as mathematical tools developed by Lorentz taught Einstein to deal with the formalism of electrodynamics of moving bodies.

Independently of questions related to the Michelson-Morley experiment, one can ask why P2 is one of the two axioms of SRT and not for example P3 "The Maxwell-Lorentz equations express a law of nature." Of course, the latter was Einstein's starting point, histori-

cally and epistemologically. Moreover, the formulation of P2 is in disagreement with Einstein's heuristic requirement of coherence.

Zahar resolves the question as follows: P2 is the weaker formulation than P3; it is implied by conjunction of P1 and P3. The postulates P1 and P2 are sufficient to generate a new kinematics and to derive the Lorentz transformation; Einstein probably did not want to make his space-time theory dependent on a macroscopic theory (Maxwell electrodynamics).

The above remarks are only a brief digest of the very deep study given by Zahar. He treats not only the foundations of special relativity but also the foundations of the general theory. His work deserves to be better known among physicists.

T. GRABIŃSKA

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12/82

THE TRANSLATOR COMMENTS: Contrary to T. Grabińska's comment, my intention in translating Einstein's lecture at Kyoto in 1922 is fulfilled; this speech has indeed thrown some light on the current controversy as to whether or not Einstein was aware of the Michelson-Morley experiment when he proposed the special theory of relativity in 1905. The letter of Grabińska is one example of a proof.

Since my paper is essentially a translation of Einstein's lecture with an introduction explaining the background matter of the lecture, I did not try to discuss how the Michelson-Morley experiment influenced Einstein's thought in developing the special theory of relativity. In dealing with the point mentioned above, Einstein's recollection in 1922 at Kyoto should be taken into account.

YOSHIMASA A. ONO

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1/83

## Systems of units

Having received my *Physics Vade Mecum*, it was with pleasure that I noticed the imperial system of units has at last been reduced to 4 pages of conversion tables. I received my university education using imperial units backed up with cgs, mks and even made-up units to try and achieve consistency within each system. Now that Australia is one of many countries that have adopted the SI system of units, I find it convenient not to buy books that are cluttered with data and formulae in units other than SI units. SI units are so simple and consistent that problems of distinguishing between force and mass and deciding on what length unit is required for a particular calculation no longer exist—except in American text-

books (even though I understand the US is adopting the SI system).

It was thus with disappointment that I noticed cgs units intruding into the *Physics Vade Mecum*. It was with shock I noticed centimeters appearing nearly everywhere in the book. The SI system has a preferred ratio of  $10^3$  between units with different prefixes, and so cm is not a preferred unit. Neither are dynes, bars, or even hp appearing on page 64 with rpm. The unit of surface density  $\text{oz/yd}^2$  appearing on page 65 would have to result in acoustics appearing to be witchcraft rather than physics if only because the unit appears to be so antiquated.

May I suggest that in any future *Vade Mecum* the editor should adopt one system of units, namely SI, saving space and confusion with a much more useful publication.

DAVID EDEN

Eden Dynamics  
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1/83

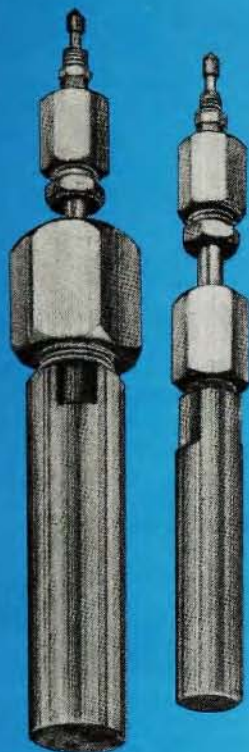
THE AUTHOR COMMENTS: We appreciate David Eden's view that it would be very nice if there were only one system of units. We could then do away with conversions and conversion tables altogether, saving space and confusion, as he points out.

We considered the question of units very seriously when we were composing the *Vade Mecum*. The problem is that only a fraction of the physicists who write in the AIP journals use SI units. In fact, in *Physical Review* and *Physical Review Letters*, the most important journals read by physicists, SI units are rarely used. It isn't even true that cgs units are universal. Theorists prefer natural units,  $\hbar = c = 1$ .

It seemed to us that physicists will use the units they are most comfortable with and that will best express the sense of magnitude they are trying to convey. We decided that our handbook would be most useful if it were written using the language and units prevailing at this time. Each editor made the choice appropriate to his own field. The advantage of this became apparent when it came to reproducing the tables of data which the *Vade Mecum* has in abundance. We saved the labor of converting the tables and the pain of introducing errors.

As a matter of fact, we felt we had gone as far as we dared in the *Vade Mecum* to advance the cause of SI units. In many cases, we went to extra space and trouble to give expressions in both cgs and SI units side by side. An effort was made to include the prescription for conversion to SI units in the table headings.

Those who favor SI units should credit David Lide of the National Bureau of Standards, Chairman of the *Vade Mecum* Committee, for having pushed us as far as we went in this



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direction. Increasingly, American physics texts are adopting SI units. As more students, trained in their use, move on to use them in the journal articles they write, their prevalence will expand. Future editors of the *Vade Mecum* will undoubtedly take note.

HERBERT L. ANDERSON  
Editor-in-Chief

2/83

AIP Physics *Vade Mecum*

## PhD for teaching

As a positive response to Paul Smith's letter (January, page 116) I would like to make a radical proposal for preparing prospective undergraduate physics teachers. I challenge graduate physics departments to initiate a teaching-track program leading to the PhD in Teaching (PhD-T) with a status equal to that of the standard research-track degree (which would be renamed the PhD in Research).

Students in the PhD-T program would have basically the same course requirements as their research-track peers and would be required to take a candidacy examination that emphasized physics through the first half of the PhD program of courses. As an option, a student in the program could take a limited number of courses in education or the philosophy of science.

The heart of the teaching-track program would consist of substituting supervised teaching for the traditional dissertation research. Students would teach lecture sections in general physics and two of the following major courses: mechanics, electricity and magnetism or modern physics. In addition, they would teach a general laboratory course and at least one advanced laboratory. The best and most experienced teachers among the physics faculty would supervise the teaching and give support to the PhD-T candidates.

In place of the traditional dissertation defense, PhD-T candidates would take an examination on the historical development of physics.

A teaching-track program similar to the one outlined above would, in my opinion, prepare undergraduate physics teachers of whom the physics community would be proud.

BYRON C. HALL JR  
Cincinnati, Ohio

2/83

## Teaching without a PhD

A recent letter from Byron Hall (February, page 110) dealt with his opinion of whether or not it is necessary to have a PhD to teach college physics. My own recent experience as a faculty member

prompts me to support Hall's conclusions.

First, let me state that my MS is in radiation science and not "pure" physics. I completed all the coursework for my PhD but was unable to obtain sufficient funding to complete my thesis. At the behest of the chairman of the physics department at my undergraduate alma mater (a four-year private community college), I filled in as instructor of physics. This was two days prior to the start of the new academic year when the original person (who had a PhD) requested to be released from contract.

During the academic year, I taught a non-calculus general physics course, modern physics, quantum mechanics, the associated laboratory courses and a survey course. The survey course is a science-related topic specially designed for non-science majors; the one I taught was entitled "Radiation and Radioactivity in Our Environment." The enrollment for this course was 37 students for the regular semester's offering and 45 for a special three-week spring-summer intersession.

The college also maintains a subcritical nuclear assembly for student training. Thus it is mandatory to maintain a strict regulatory and dosimetry program. Part of the requirements for my MS included knowing the Federal and State Nuclear Regulatory Codes, so I became licensed with the NRC and was responsible for all nuclear safety. Furthermore, I spent the three weeks of the winter vacation disconnecting and cleaning the pump, filters, and plumbing of the reactor to remove the crud built up through prior neglect. I was not reimbursed for this time nor was a student allowed to assist me, since there was not enough work-study funding available.

I encouraged the students to work on extra-credit projects. The culmination of one such project was the interface of a TRS 80 microcomputer to a student-grade multichannel analyzer.

My reward at the end of the year was being replaced by a recent PhD graduate (in theoretical physics) with little or no undergraduate teaching or laboratory experience. I had had a year of undergraduate teaching, plus graduate teaching experience during my teaching assistantship.

Currently I am employed at a national laboratory, where I hope my work can be used as a thesis project for my PhD. Maybe then I can return to teaching.

I pose the following question: Did it make any sense to replace a "seasoned" teacher who had demonstrated skill and devotion with a novice just because of a piece of parchment?

MARWIN RAPKIN

3/83

(Address withheld by request)

## More on ice age in physics

I want to comment on the letter by Thomas Phipps (February, page 15). I was brought up believing that physical science, because of its logical character, was self-correcting. What, then, is it about the present scientific climate that seems to perpetuate "bad" ideas and to prevent the appearance of "good" ones; why, then, has fundamental theory ceased to be self-correcting for most of the century?

A major question that needs consideration is how to tell "good" ideas from "bad." In retrospect, it is clear that Copernicus' Sun-centered planetary system was a good idea while the Ptolemaic Earth-centric system was a bad one, although each described the planetary motions quite well. There are many other notable examples throughout the past three hundred years leading to our present interpretation of nature. The history of the physical sciences shows clearly that the best ideas were those which simplified, unified and allowed the science to progress. One basic ingredient throughout the progress of physics since Newton has been a continuing development of physical intuition by succeeding generations of scientists. This was needed before a successful mathematical theory could be produced. Thus, to distinguish "good" from "bad" ideas requires a well-developed physical intuition on the part of physicists. Apparently, physical intuition has not been part of the development of physicists over the past fifty or so years, due primarily to relativity and quantum theory and the non-intuition-developing interpretation they inspire. In fact, it has become accepted dogma that the substance of twentieth-century physics is beyond human intuition and, consequently, we are toying with the idea that the physical world is not "real" after all. What we seem to have done is to back ourselves into a philosophical corner because of a succession of bad ideas.

As for the "Ice Age" concept itself, quite obviously there are not that many members of the physics community who accept that there is an "Ice Age"; otherwise, there would be no point to Phipps' letter. This would not be a problem to us who believe that physical theory is currently in a state of stasis if the journals, which are the usual sounding boards of scientific enterprise, accepted for publication alternative ideas to those presently in vogue. However, such is not the case. The APS journal editors, for example, and their coterie of referees appear to be acting as a bureaucratic filter system to eliminate all alternative ideas and theories, no matter how carefully the consequences of these proposals have