▸ Keep up-to-date with relevant literature by regularly scanning journals such as *Current Contents, Chemical Abstracts*, and so on. Most of this reading may appear wasted time. However, if you find only *one* reference a year that gives you a new idea, or shows you that someone else has done work on your project before, you may save years of effort.

One final rule: You must work hard! Every famous scientist has achieved his pre-eminence by the combination of a brilliant mind and hard work. On the other hand, many brilliant men have never "made it" simply because they were lazy.

ALFRED H. SOMMER
Wellesley, Mass.

New generation motivations

Michael Moravcsik has raised some interesting points with his survey of the motivation of physicists (October, page 9). It is useful to know the extent that physicists are motivated by "internal" criteria such as "Release of innate curiosity," as opposed to "external" criteria like "peer recognition" and "financial advantage." I would, however, wish to point out one apparent defect in this particular survey.

Moravcsik describes the respondents as "virtually all personally known to me." These respondents were asked to describe their attitudes "When you decided to become a physicist, when you received your PhD, 15 years ago, and now." This would suggest that although no selection by age was made deliberately, nevertheless most of the persons surveyed were of Moravcsik's own generation, who entered physics in the period from slightly before to slightly after World War II, and that the survey sample contained very few people who have held their PhD's for less than 15 years. This survey thus could not detect any difference in motivation between younger and older physicists, a difference that could have a profound effect on the future of our profession.

As the social context in which physics research is done has undergone a drastic change in the last 30 years, there are ample theoretical grounds for expecting such a difference. The availability of additional financial rewards in the form of "summer salaries," consulting fees, and so on, might have attracted externally motivated persons to physics, in recent years, who would have otherwise gone elsewhere. On the other side of the coin, many younger physicists may have been forced to change their attitudes by the recent dramatic downturn in research funding. While older professors may enjoy the luxury of being motivated by "innate curiosity," their younger colleagues *must* concern themselves with the immediate question of professional survival.

If younger physicists are indeed more externally motivated than their predecessors, this fact could go a long way towards explaining both the increase in quantity and the deline in quality of research publications in recent years. It could also explain the propensity of younger physicists to hop aboard whatever bandwagon happens to be passing by, be it "Polywater," A_2 splitting, U(12), dual-resonance models or charmed colored quarks, as that is where they perceive the "peer recognition" and consequent "financial advantage" is to be found. Since the great discoveries of the past have been made by individuals who were indisputably "internally motivated," this kind of attitude on the part of younger physicists, if it exists, would not justify much optimism concerning the future of physics.

ROBERT J. YAES
Memorial University of Newfoundland
Newfoundland, Canada

THE AUTHOR COMMENTS: Yaes is correct in assuming that in my sample the youngest generation of physicists (around 30 years old) were underrepresented, though they were *not* unrepresented. The question of whether motivations change from generation to generation is in fact an interesting one, which my original survey was not intended to shed light on. This question, however, cannot be decided by *ex cathedra* statements like those contained in Yaes's letter. Instead, they should be explored by surveys, similar to mine, structured specifically to yield information about this specific problem. I would therefore like to urge Yaes, or anybody else interested in this subject, to participate in information gathering instead of mere guessing.

MICHAEL J. MORAVCSIK
University of Oregon
Eugene, Oregon

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Moravcsik's survey (October 1975, page 9) on the motivation of physicists is interesting, but I wonder if his findings are so readily interpreted. Consider, in particular, the finding that most people seem to be physicists for the personal esthetic satisfaction, innate curiosity, urge to convert talents into achievements, and satisfaction of making a discovery. I certainly share such sentiments, and I am not surprised that a majority of others do too. On the other hand, it must also be realized that physics students are *taught* that these are the respectable peer goals in physics. It is difficult to imagine a colleague gaining much approval by avowing that he or she is really out for power and money. The student almost never learns anything of the financial or be-

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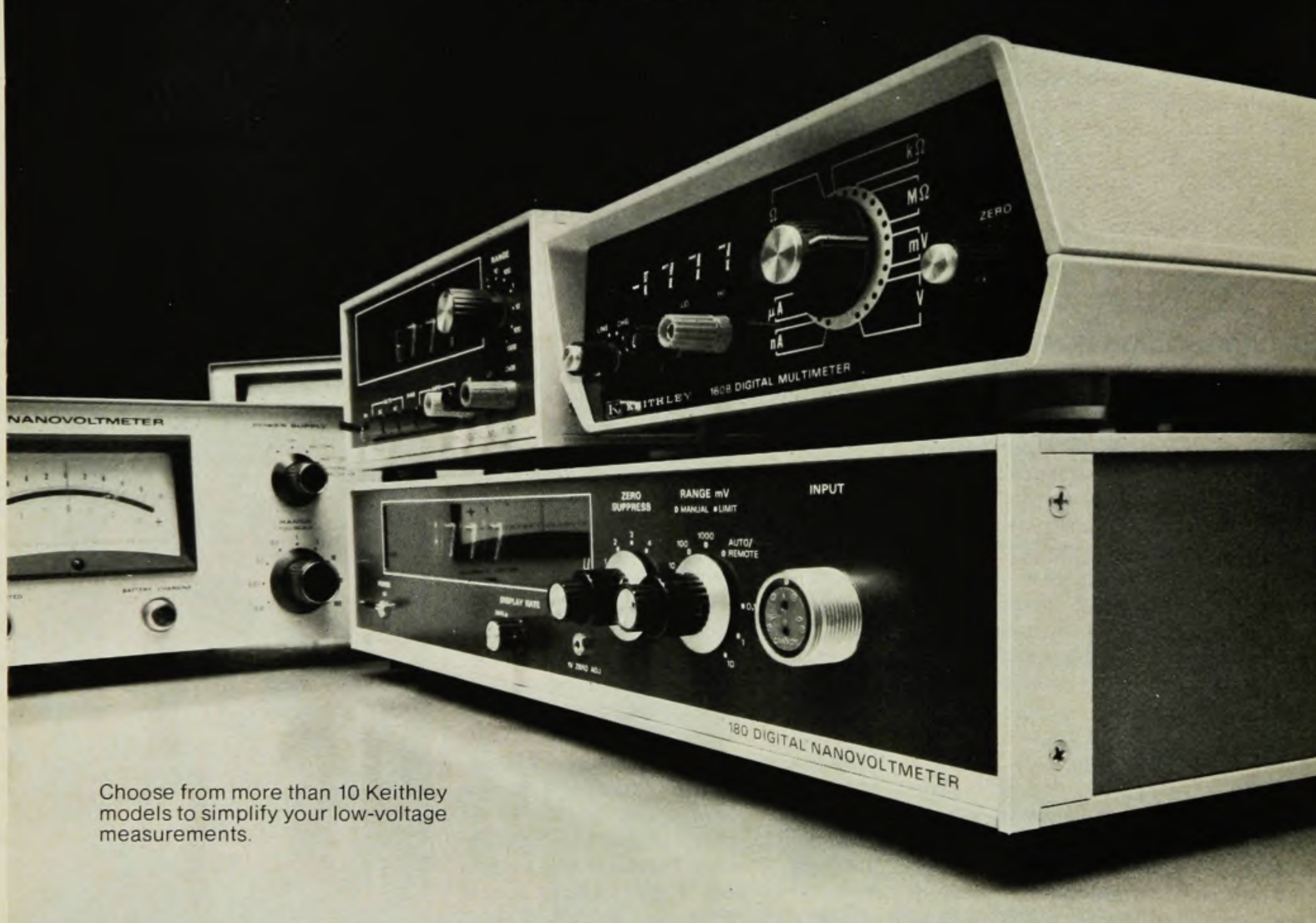

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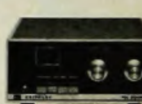
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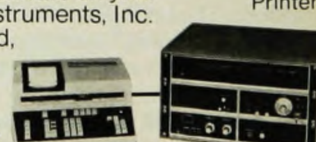
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havioral backgrounds of the great heroes of physics. Indeed, it would be considered impolite for one to note some who eagerly and successfully grasped for fame, contentedly took credit for the work of people under them, or reaped fat profits from commercial exploitation of their discoveries. I suspect that Moravcsik has actually succeeded in gauging what physicists think they should think about why they are in physics.

F. CURTIS MICHEL
Rice University
Houston, Texas

THE AUTHOR COMMENTS: Curtis Michel's critique is one that can be heard often in connection with motivational surveys, including the presumably more "sophisticated" ones in which the respondent is asked to make decisions in hypothetical situations. It is a variant of the statement that an object which looks, sounds, feels, and tastes like an apple is really something else but perfectly disguised as an apple. It is sometimes thought that the difficulty can be resolved by studying what respondents actually do rather than what they profess, but that method is shaky also because the response in an actual situation is determined by which motivation is most seriously challenged or endangered and not by which is strongest.

While such a critique is logically unanswerable (see hidden variables), it appears to me to have little functional significance. In practice, the best one can do is, a) not to be submerged in a preconceived view of what the motivations are, even if such a view is attractive on account of the simplicity or ideology of a certain model, b) to carry out as many different types of studies as possible, in the hope that they eventually point at some consensus of views which then can and must be used in practical decision making in science policy until something better comes along.

MICHAEL J. MORAVCSIK
University of Oregon
Eugene, Oregon

Spin and relativity

George Uhlenbeck in his charming reminiscences of the discovery of the electron spin (May, page 43) mentions two contexts in which special relativity played an essential role: (1) coupling between spin and orbital motion, and (2) Thomas factor.

Point (1) arises from the force resulting from Lorentz transformation to a coordinate system where the electron is at rest; point (2) is based on the fact that general Lorentz transformations with non-parallel velocities form a group only after adjoining rotations. Belated recognition of (1) caused delay in the acceptance of electron spin by Wolfgang Pauli and Niels Bohr. Belated recognition of (2) caused delay in the final acceptance of electron spin by Pauli. Both Pauli and Bohr were, of course, familiar with Lorentz transformations. However, Pauli was skeptical of the whole concept of electron spin, because of his emphasis on the "classical two-valuedness" of the fourth quantum number. Bohr, on the other hand, was prejudiced by his previous introduction of a "non-mechanical strain" to cause (1). Both Bohr and Uhlenbeck and Samuel Goudsmit learned from Einstein the basis for (1). But even the latter was surprised by (2), as Uhlenbeck notes.

Surprisingly, a survey showed that none of the great mathematicians Poincaré (who named the Lorentz group), Minkowski, Klein, Herglotz or Weyl stated explicitly the above property of the Lorentz group. Nor is it contained in Pauli's book. Equally surprisingly, however, Silberstein's *The Theory of Relativity* [MacMillan, London, 1914 (!)] does contain an explicit statement (pages 167–170), which is also contained in the second (1974) edition of his book. This fact, of course, does not the least affect the great merit of L. H. Thomas, who not only noticed the above property of the Lorentz group independently, but also showed how it led to the factor ½. A more detailed discussion, including the essentially quantum nature of the spin-orbit interaction, will be given elsewhere.

EUGENE GUTH
University of Tennessee
Knoxville, Tennessee

Lunar studies continue

George Siscoe's review of *Lunar Science: A Post-Apollo View*, by S. Ross Taylor (in your February issue) contains one possibly misleading statement: "A program of intensive lunar study started in 1964 . . . The program was completed in 1972."

Although the Apollo program itself ended with the flight of Apollo 17 in December 1972, NASA's continuing lunar-science program is alive and flourishing. Over a hundred investigators are conducting studies of the 343 kilograms of lunar rocks and soil returned by the Apollo missions. Many of the scientific instruments placed on the Moon by the astronauts are still transmitting useful data and will continue to do so for a few more years. Ground-based laser ranging to the four retroreflectors placed on the Moon is producing important results in celestial mechanics, relativity and terrestrial geodynamics. There is also a vigorous data-synthesis program in which the whole body of geophysical, geochemical and geological information about the Moon is being analyzed to produce co-