letters

How to succeed in industrial research

During 41 years of working in five different laboratories, mainly concerned with applied research in electronics, I became increasingly interested in observing the careers of fellow workers. I was surprised by the number of brilliant young men whose careers petered out as they grew older and, conversely, by the number of mediocre young men who seemed to grow steadily in stature and achievement.

In trying to analyze the reasons for this, I gradually arrived at a set of guidelines for the industrial researcher. These guidelines are so obvious that I would not submit them for publication were it not for the fact that they appear to be ignored by a great number of researchers. I will begin with one major rule, which has some complex aspects, followed by some simple do's and don'ts for experimental work.

Major rule. Pick the right project! No project is 100% promising—those have been completed a long time ago! The real problem is to distinguish between the project with 10% promise of success and the one with 1% promise. There are some questions you should ask yourself before making a decision; I name just a few.

▸ Has there been a recent invention or theory or a new experimental tool that makes the solution of the problem more probable than in the past?

▸ If you should be successful, will anyone be interested in using your result? This problem does not exist in basic research when you can at least have the satisfaction of publishing your result.

▸ Have you discussed the project with those whose judgment you trust and who have had experience in the field? This is usually easier in a large laboratory.

A frequent problem is that your supervisor wants you to work on a project that you don't believe in or, conversely, does *not* want you to pursue one that you think has potential. There is no simple solution to these problems, but I can make some useful suggestions. In the first case, if you have several projects going, it is often effective to ask the supervisor which of your present projects should be dropped to make time for the new one. In the second case it sometimes pays to work on your project during evenings and weekends or when your supervisor is on vacation or in the hospital. However, I should add

COURTESY OF BELL LABORATORIES

a warning: Sometimes the supervisor is right and you are wrong! Again, don't hesitate to discuss your problem with knowledgable colleagues.

Once you have embarked on a project you must be honest with yourself and drop it if you come to a dead end. This is one of the most difficult rules to follow because it is very hard to be objective in your judgment. You are faced with two dangers: You may stop prematurely because you are too impatient, or you may continue beyond the point of diminishing returns because you are too proud to admit defeat. Cases of the second category are more serious. I have seen the best years of a man's active life wasted for this reason.

Special mention should be made of the dangers involved in research on a government contract. The procedure usually starts with a "proposal" that is phrased too optimistically in order to get the necessary funds and/or to beat the competitive bidder. Once you work on the contract you have to write quarterly reports that again have to be overoptimistic to make sure that the contract will be renewed for the next fiscal year. After having written a number of these reports you have brainwashed yourself into having much greater confidence in the project than it deserves. If you are able to remain objective and conclude that the project is not worth pursuing you must be ruthless in trying to get out of it. The government will not usually discontinue the contract because the originator does not like to admit faulty judgment. Your company is not likely to stop the research because contracts are usually for "best effort" on a "cost plus" basis. In other words, the company can never lose money on the contract and has therefore no incentive to discontinue the work. I have seen hopeless projects of this sort drag on ten or more years, ruining the promising career of a good researcher.

A final ingredient for success in picking a good project is of such a vague nature that it is difficult to put into well-defined terms. The best words are probably "luck" and "intuition," or a happy mixture of the two. Unfortunately, there are no rules how to acquire these!

Specific rules (Elementary, but often ignored).

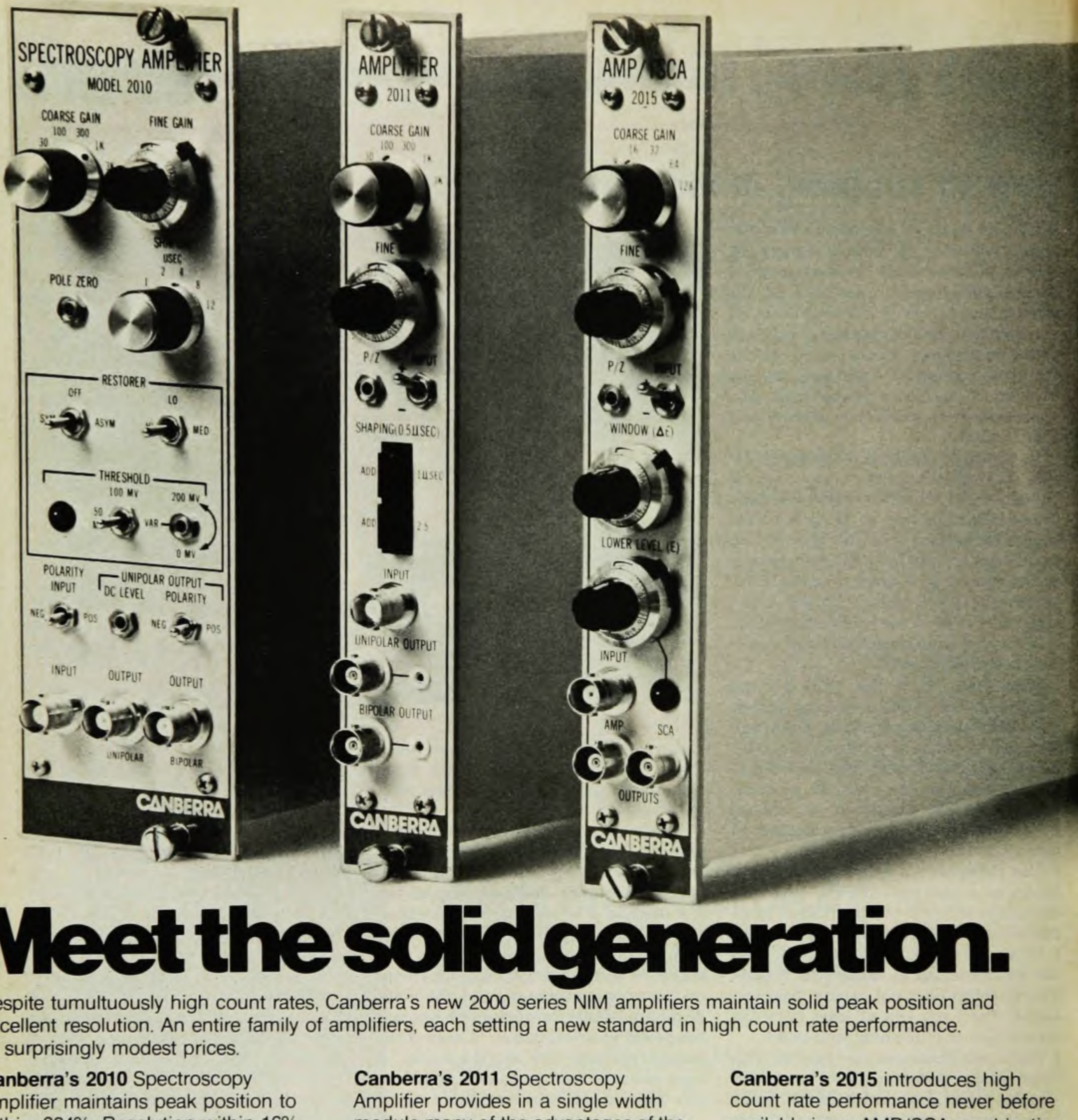
▸ Don't change more than one variable in any experiment. This is so obvious and yet is probably the most frequently broken rule.

▸ Don't draw conclusions from an experiment until you have reproduced the results at least once or twice.

▸ Don't spend time and effort on making any one measurement more accurate than the nature of the experiment justifies. If the error in one reading cannot be reduced below $\pm 10\%$ it makes no sense to measure other parameters to within 0.1%.

▸ Make your preliminary experiments with the simplest equipment ("string and sealing wax" method) and gradually add to its complexity. I have seen scientists spend a whole year, or more, on building very elaborate equipment before making any measurements only to find out that a minor point had been overlooked that made the whole apparatus useless. My favorite quotation related to this subject: "The best equipment is one that falls apart after the last experiment; any effort to make it more durable is a waste of time and effort."

▸ Work on your experiments yourself; don't ask a semi-skilled technician to take your measurements. Innumerable inventions and discoveries were the result of an unexpected observation that a less skilled worker would have overlooked or not reported—penicillin, x rays, uranium fission, and so on.


SPECTROSCOPY AMPLIFIER
MODEL 2010
COARSE GAIN
FINE GAIN
POLE ZERO
RESTORER
THRESHOLD
POLARITY INPUT
UNIPOLAR OUTPUT
DC LEVEL
POLARITY
INPUT
OUTPUT
OUTPUT
UNIPOLAR
BIPOLAR
CANBERRA
AMPLIFIER
2011
COARSE GAIN
FINE
P/Z
INPUT
SHAPING(0.5μSEC)
INPUT
UNIPOLAR OUTPUT
BIPOLAR OUTPUT
CANBERRA
AMP/SCA
2015
COARSE GAIN
FINE
P/Z
INPUT
WINDOW (ΔE)
LOWER LEVEL (E)
INPUT
AMP
SCA
OUTPUTS
CANBERRA

letters

▸ 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

•

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-