

Motivations of physicists

The interpretations of the history of science and analyses of the sociology of scientists depend crucially on an assessment of the motivations of scientists. It is not easy to generate hard information on such motivations. There are several conceivable ways of conducting a study of them, but none are free of methodological objections.

The purpose of this note is to present the results of a survey carried out to gather information on the motivations of physicists. The method used in the survey is a rather straightforward one and its virtues and shortcomings are likely to be evident.

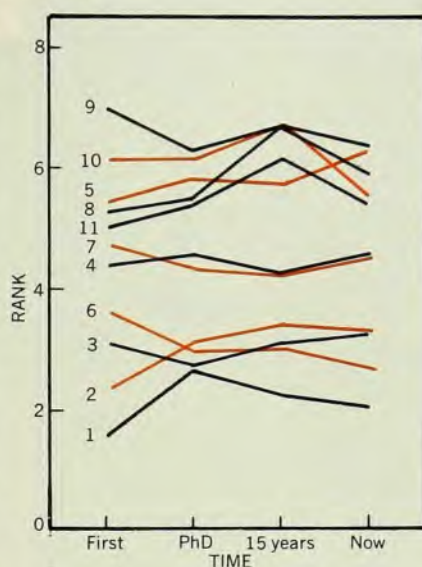
The survey consisted of a letter and ballot sent to 100 physicists, all working within the United States. Their specialities were in experimental or theoretical nuclear or particle physics. No selection by age or other criteria was made, except that virtually all of the addressees were personally known to me. I believe this played an important role in producing a rate of return unusually high for a survey: 61 responses were obtained on the strength of the original letter and one brief reminder note.

The original letter explained the arrangements for anonymity; 13 respondents did not use the opportunity for anonymity.

Instructions on the ballot stated: "Please indicate your motivations to do science by writing '1' for the strongest motivation, '2' for the next strongest, and so on. If a stated motivation does not apply to you at all, write '0.' If a column does not apply to you, leave it blank. Feel free to write in additional items for motivations if they play a significant role in your case."

Below these instructions there were 14 rows and four columns. The columns were headed "When you decided to become a physicist," "When you received your PhD," "15 years later," and "Now." The 14 rows had the following headings:

1. Personal esthetic satisfaction derived from learning about nature.
2. Release of innate curiosity.
3. Urge to convert your talent into achievements.
4. Urge to be the first ever to discover something (i.e. striving for priority.)
5. Urge to compete.
6. Personal satisfaction obtained from



Average ranks of the eleven motivations at various points in the scientific career. The ranks are averaged over all questionnaires in which that motivation is classified with numbers between 1 and 11. The numbers next to the curves correspond to those in the list of motivations shown in the text.

- making a discovery (regardless of priority).
7. Peer recognition.
8. Social prestige in society as a whole.
9. Opportunity to have influence.
10. Financial advantage.
11. Service to humanity.

Rows 12, 13, and 14 had no headings, and were intended for "write-in's. Ten of the returned questionnaires elaborated on the categories or commented on the response. Eight of the questionnaires expressed doubts or problems that arose in responding to the survey. The texts of these comments and doubts are available from me on request.

The results of the survey are shown in the figure. The average ranks of the eleven motivations are plotted against the four column entries. The rank is averaged over all questionnaires in which that motivation is classified with numbers between 1 and 11.

For a given motivation, the average rank does not depend very strongly on time. In particular, no motivation changes by more than 1.5 units. Motivations 2, 5 and 8 increase their average with time, while 6 and 9 decrease theirs.

On the whole, however, rankings do not change much.

In terms of importance, there appear to be at least two rather distinct groups. Motivations 1, 2, 3 and 6 appear to be considerably more important than the others. Among the latter, perhaps 4 and 7 form another group by themselves, but the distinction here is not so clear. The same features can be observed in the data if one replaces the abscissa of the figure by the number of "0"s marked against a certain motivation, summed over all four columns of all questionnaires.

The result can be summarized by stating that the *internal* motivations (satisfaction from learning, release of curiosity, conversion of talent, and satisfaction from discovery) play a considerably more important role than the *external* motivations (priority, peer recognition, competition, prestige, influence, finances, or social service).

As a result of the weak dependence on time observed, the relative importance of various motivations hardly changes at all with time. The relatively somewhat decreased role of esthetic satisfaction at the time of obtaining the PhD might be due to a temporary satiation with work. Indeed, this motivation reoccupies its lead at later times.

Finally, the survey appears to indicate the motivation of scientists in doing scientific work is a rather complex superposition of many different components. This would suggest that models that assume one single dominant motivation predestining the history of science or the sociological structure of the scientific community are likely to be grossly in error.

MICHAEL J. MORAVCSIK
University of Oregon
Eugene, Oregon

Lights in the sky

Harley Rutledge's letter "Light Flashes in the Sky" in the September issue (page 11) suffers from deficiencies often found in reports of UFO sightings. It is frustrating that no mention is made of the criteria used to estimate the quoted distance and altitude limits. While these criteria are pedagogically useful in estimating linear sizes, one is left to conclude that they are merely guesses. This is indeed unfortunate for Rutledge.

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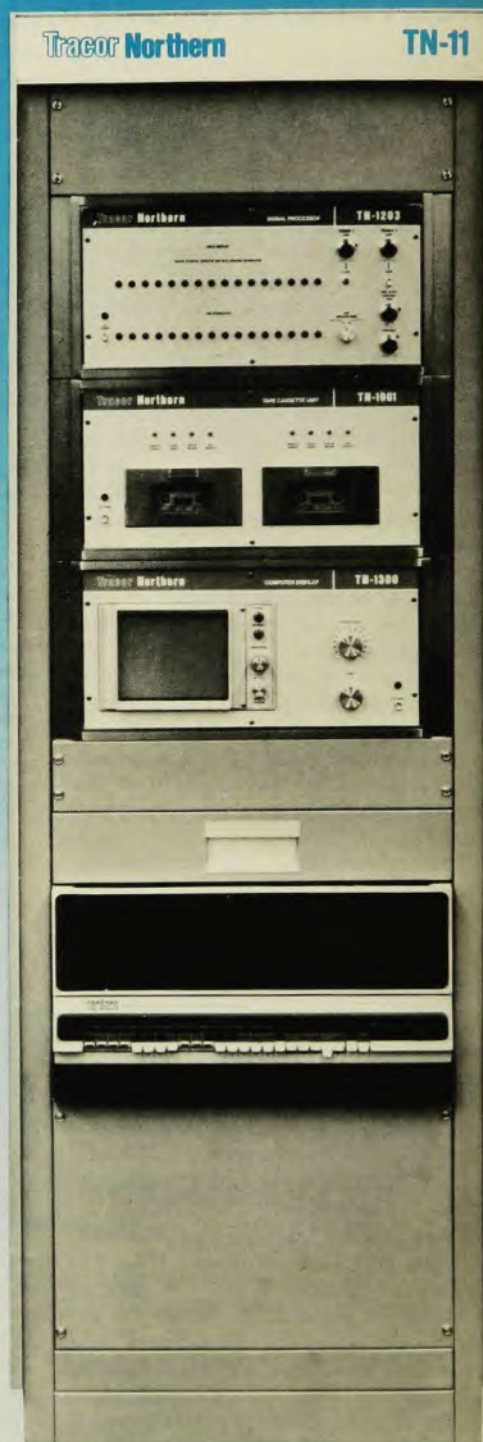
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ge's "some 100 anomalous events" because of the opportunity provided by three observation points apparently established solely to search for more examples of the phenomenon. Simple theodolites would have provided rough triangulation estimates for these quantities. Perhaps this was the case, but it usually is not.

UFO's are difficult to study quantitatively, precisely *because* they are unexpected and transient. Care must be taken to establish patterns indigenous to those objects. Rutledge might have obtained temperature and dew-point temperature profiles so as to identify the cloud height that he mentions. Or, in other circumstances, limits to the altitude might be established by noting disappearance behind an aircraft or mountain top whose heights can be found. Better use must be made of these fleeting opportunities, but this is often not the case. Then the sighting becomes simply one more interesting bit of folklore.

There are instances where simultaneous observations have been made by radar and visually of unidentified objects. It has always struck me that a great opportunity existed therein to select between crude models for these objects. With observations by two radar units operating on different frequencies, it may be possible to discern whether or not the object is solid. Alternatively, should an object turn out to be an electrified air pocket, its plasma frequency might be identified. With this in mind, I was quite interested in Rutledge's comments concerning a 37.1-MHz transmission. But his reasons for mentioning that circumstance were unclear. Did he feel that his actions were correlated with the object's disappearance?

It is unfortunate that observers who have the chance to gather these data do not take advantage of their situation. This is especially true when unusual objects recur and are specifically being searched for. The purpose of this letter is not to denigrate Rutledge. On the contrary, it is to point out circumstances, perhaps taken for granted, which limit the effectiveness of reports such as his. With this in mind, his future observations may be even more revealing.

HAROLD I. HEATON
University of Massachusetts
Amherst, Mass.

In the event that the letter of Rutledge is intended to be taken seriously, it should be pointed out that the phenomena he reports are well known entoptic events that may be observed as well in a darkened laboratory as in the twilight

sky. The thin streaks and punctiform flashes have been attributed to the stimulation of retinal receptors by individual red blood corpuscles, probably in releasing oxygen in their transit of the retinal capillaries.

It is probable that the binoculars (10×35) served only to limit the field of view and to reduce the solid angle of visual search involved. This effect is known to be powerful in the occasional reports of sighting stars (more probably planets) from the bottom of a well or chimney. But if the events are indeed entoptic, Rutledge's binoculars could as well be replaced by empty cardboard tubes.

Finally, it is important to dispel the myth that "the angular resolution of the eye is one second of an arc," or, for that matter, any other single value of angular subtense. The measured value of visual resolution, or visual acuity, depends upon the conditions of test, nature of the test object, and, most importantly, upon the contrast in luminance between object and background. It is obvious that angularly tiny objects of sufficient contrast are easily seen (how many seconds of arc are subtended by a star?), and that the detection of such objects requires only that sufficient flux is available at the retina for stimulation of its receptors. In a perfect eye this limit is set by considerations of diffraction; in an ordinary one the image is further modified by errors of refraction.

JOHN H. TAYLOR
University of California
San Diego

THE AUTHOR COMMENTS: I appreciate Harold Heaton's concern for our experimental technique. A full disclosure of our investigation of 1.5 years would reveal that we have met most of the objections raised in his letter. Perhaps his comments are premature.

In response to the comments by John Taylor, perhaps a rebuttal will be of didactic value. A careful reading of my letter vitiates the first two paragraphs in his letter. It is inconceivable that I and my co-workers would spend nearly 120 nights in the field observing "stimulation of retinal receptors by individual red blood corpuscles"! It seems unlikely that several observers would give similar descriptions of a simultaneous sighting if the effect was a physiological phenomenon. Certainly no signals would have been recorded by our instrumentation.

It is ironic that a "research psychologist" on the East coast responded with a description of a sighting similar to those described in my letter. An erratic flight pattern was observed and confirmed by his son. The psychologist reported that there were ample points of

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reference in his field of view during the sighting.

Taylor's objection to my estimate of the "angular resolution of the eye as one second of arc" is correct. The statement should have read "one minute of arc" and a quick calculation proves my intent. It was simply a typing mistake. I chose a value of one minute because it is easy to extrapolate to larger angles. I am aware that four minutes, for example, would have been a more realistic choice. I might point out that the value "one minute" appears in many elementary physics texts.

HARLEY D. RUTLEDGE
Southeast Missouri State University
Cape Girardeau, Missouri

Novelty vs. profundity

As a physician who has actively worked in theoretical biophysics for approximately 15 years, I would like to respond to Carlton Frederick's letter (May, page 15). I believe today's physicist is brighter than he has ever been. Both the subject of physics and the total culture, particularly our own, have had an immense increase in complexity since the 1930's, and simply meeting this greater demand has pushed (or pulled) intellectual development further. One has merely to read the influential texts of this earlier period and note their simpler, more pedestrian outlook to see this. What has happened, I agree, is that this complexity has fostered the over-specialization of career and placed the super specialist in a position of excessive influence by rewarding novelty on an equal footing with profundity. Since it is much easier to achieve a result with some novelty than one that is both novel and profound, the novelists proliferate at a far greater rate than the founders.

In the past the physicist of note was almost invariably a man who had mastery of the entire subject, and either a profound innovator or one who had every possibility of being such. These men set the tone and character of physics forty and more years ago and continue to inspire the student of today. The contemporary physicist of this stature, however, may very well be a "profounder profounder," that is to say profounder squared.

ARTHUR GROPPER
Los Angeles, California

Materials science courses

The fine summary of the Conference on "Tradition and Change in Physics Graduate Education" held on our campus last summer, failed to make one point which is of importance to the academic physics community: The mis-

match in the thesis topics with employment fields was clearly identified. However, not enough attention was paid to the fact that physics faculties must face the issue of defining the optimum course work for undergraduate as well as graduate students. As it turns out now, a typical student in electrical engineering or materials science has a good grasp of the fundamentals of quantum mechanics and solid-state physics, while their colleagues in physics have typically not had a single course in materials science or electrical engineering. Since the materials field (broadly defined) is likely to employ perhaps half the physicists produced in the near future, I believe that the report of the National Academy of Sciences Committee on Materials Science and Technology¹ bears serious consideration by every physics department, when it says:

"We believe that materials science will play an increasingly significant part in the education and work of physical and life scientists, as well as of engineers and technologists."

A 3-6 credit requirement in materials science (or biophysics or geophysics) may be the test of the physics community's commitment to change and the translation into real life of its concern for "applied physics."

References

1. Materials and Man's Needs. Summary Report of the Committee on the Survey of Materials Science and Engineering. N.A.S. Washington, D.C. 1971.

RUSTUM ROY
Pennsylvania State University
University Park, Pennsylvania

Predicting earthquakes

Your April issue (page 74) carries a review by Bruce Bolt of *The Jupiter Effect* by Stephen Plagemann and myself in which, following a sound precis of our arguments, he notes that "California records . . . indicate that 1803 or thereabouts was not specially active" in seismic terms, although that was the last occasion of the planetary alignment discussed in our book. This is incorrect; since the book appeared, I have visited the region of the San Andreas just south of San Francisco, and this visit included the mission of San Juan Bautista, which suffered greatly during the 1906 earthquake. The mission records describe a series of major tremors in that area in the early 1800's, and although this proves nothing it is clearly of interest to our hypothesis. Plagemann and I have included a discussion of these records in the paperback edition of the book, which should now be available, and the discussion will also appear in future hardback editions.

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