

2. R. B. Freeman, *Amer. Econ. Rev.* 65, 27 (1975).
3. American Institute of Physics publications R 151.12 and R 151.13.

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Pauling and Sakharov

A telephone call from a PHYSICS TODAY staff member, who asked me about a letter to me from Andrei Sakharov, has prompted me to write this statement.

I went to Moscow to participate in the International Conference on Biochemistry and Molecular Biology, sponsored by the International Union of Pure and Applied Chemistry, the International Union of Biochemistry and the USSR Academy of Sciences, and to receive the Lomonosov Medal. In Moscow I was approached by a man who spoke with a pronounced Central European accent. He gave me a letter, which he said was from Sakharov, and told me I should have it translated by some reliable person. Before I could find a reliable person I was informed by several reporters, including ABC television, that Sakharov had released the text of the letter, and that in it he had asked me to make a statement in defense of three Soviet scientists who had been sent to prison. I replied that I had signed statements and had written letters about scientists and other people whose rights have been reported to have been violated by the USSR government and other governments, although I could not remember with confidence whether or not I had taken action about these three men. I added that all governments are immoral, and cited the example of the United States government, which in 1952 refused me a passport and thus prevented me from participating in the two-day symposium in London on the structure of proteins that had been organized by the Royal Society; I was scheduled to give the opening address, and most of the papers dealt with the discoveries made by my coworkers and me.

I do not know whether my statements were published or broadcast. On my return to California, however, I found that my local paper, the *Palo Alto Times*, had on the same day as my interviews, 25 September 1978, published an article about Sakharov's letter to me, including the sentences "He urged the US chemist to come out in defense of three Soviet scientists—physicist Yuri Orlov, mathematician Alexander Bolonkin, and biologist Sergei Koralev—who have been sent to prison for dissident activities. Pauling did not make the statement requested by Sakharov, and he could not be reached for comment on Sakharov's appeal."

I consider it improper for Sakharov to have given his letter to the press before I

could get it translated by a "reliable person" and could take action on it. I think that this episode was planned to take place the way it did. I admire Sakharov, and I shall continue to support him. I feel, however, that in the future he should be more careful in his selection of advisors and agents.

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10/23/78

More on Velikovsky

In his "review" of *Scientists Confront Velikovsky* (August page 56), George Abell repeats propaganda that has often been refuted. His not realizing this could have been caused by his lack of ability to research the subject he so vehemently opposes, or by his lack of interest in obtaining the facts. The latter is probably the case, because Abell appears to believe that he already knows the "truth" about the recent history of the solar system.

It seems to make Abell and others of Velikovsky's opponents feel better about their irrationality if they convince themselves that only functional psychos would consider Velikovsky's work seriously. I have a PhD in physics and am a member of APS. I have given many talks about Velikovsky's ideas to sections of the AIAA, IEEE, SPE, MENSA, and physics departments of universities. The talks were well received, and the audience realized that Velikovsky's ideas have considerably more support than one would conclude from reading the "acceptable" journals. The range of PhD physicists actively investigating Velikovsky's work includes experts in celestial mechanics, spectroscopy, thermodynamics and plasma physics. Numerous scientists trained in fields other than physics are also investigating the ideas. Therefore, the only way Abell and others can claim that scientists consider the theory "absurd" is to define "scientist" as a person who agrees with their outdated ideas.

It is surprising that Abell only picked on Derrall Mulholland and did not mention any of the mistakes made by Sagan. His paper was so replete with distortions, misrepresentations, inaccuracies and illogical statements that surely a person with Abell's training should easily see them. In fact, Sagan's paper contained so many errors that one must conclude that either he is extremely stupid (which he obviously is not), exceedingly careless (your guess) or fraudulent. The fact that Sagan proved incompetent at refuting Velikovsky does not, of course, make Velikovsky's theory correct, but since Sagan's demonstrably false article is considered the best refutation, it does make it obvious that the subject should not be closed.

Those interested in the details about

the errors found in *Scientists Confront Velikovsky* should read *Velikovsky and Establishment Science* and *The Age of Velikovsky* (Kronos Press, Glassboro State College, Glassboro, N.J.: *The Age of Velikovsky* paperback is by Delta). The latter book also reviews the theory, some of the scientific support for it and the attempted suppression of the ideas.

It is ironic that the issue of PHYSICS TODAY with Abell's review also contained letters about continuing concern for oppressed scientists in other countries. Beginning with the origin of this concern, the freedom of dissemination of ideas has been one of the major issues. Yet, in 1971, PHYSICS TODAY wrote me that Velikovsky's ideas need no longer be discussed in that periodical. Just this year, *Science News* canceled continuing ads for *Velikovsky and Establishment Science* and *The Age of Velikovsky*. *Science News* said that they would no longer run ads on "that subject." However, the following week *Science News* ran an ad for *Scientists Confront Velikovsky*. What both periodicals obviously meant to say was that discussion of only one side of the issue would be allowed. The Soviets claim suppression of ideas protects the public from confusion. From the actions of some scientists here, one could easily conclude that they believe in the selective use of "protective" measures.

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THE AUTHOR COMMENTS: Since writing my review I have learned that the Velikovsky cult does count among its members a small handful of people with scientific credentials. Perhaps this is not surprising, considering the tens and thousands of men and women with scientific training. I understand that there are also physicians who still practice medical astrology in making diagnoses. At least it is a nearly unanimous view of physicists and astronomers that Velikovsky's ideas are not tenable in light of modern knowledge.

There is no scientific conspiracy against Velikovsky. Scientists do not generally count themselves as his enemies or "opponents;" most simply do not have time to become involved in a futile debate with committed believers, and are more likely to regard the whole matter with mild amusement. Scientific journals have not accepted papers by Velikovsky, if he has ever submitted any, because his astronomical ideas are not carefully developed scientific theories but vague speculations without quantitative justification. On the other hand, I would strongly oppose actual suppression of any views, including Velikovsky's, so long as they were not misrepresented as being those of the scientific community. But one can hardly make a case that Velikovsky has been

suppressed; he must certainly have sold more books than any physicist or astronomer, and must be one of the best-read authors of all time.

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10/2/78

Breakdown in innovation

The breakdown of US innovation has been discussed quite often recently (August editorial, page 88). In addition to the usual prescriptions of technological push, commitment to basic research, and so on, may I reiterate an old buzz word—government bureaucracy. We have all heard that the average application to FDA for approval of a new drug is not counted in pages or volumes, but is weighed in tons. It is beyond imagination how much R&D time, effort and money would be really spent in filling out (and also in reading) the tons of application forms. There are other aspects that may also be detrimental to innovation.

It is not often realized that innovation and risk may be regarded as complementary variables in the Heisenberg uncertainty principle. The bigger the innovation, the bigger the risk. We tend to remember the success stories and often forget the failures. In the decade following the Bohr atom, the list of famous physicists who worked on quantum mechanics is far longer than the few names that we remember today. Under present conditions, successful innovation brings only modest reward (for example, promotion from GS-13 or 14 to 16 or 17 or 18, a 20% salary raise), but a failure would mean a catastrophically ruined career. It is usually much safer to adhere to the established research program. But then it can be argued that an overly rigid program can lead to obsolescence or a tendency to solve the same old problem on a slightly bigger computer for a slightly more accurate answer (May, page 15).

In addition, there is the unwritten but widely believed dogma that scientific creativity declines rapidly after the age thirty. As a corollary, research directors and personnel managers are reluctant to hire scientists older than thirty. With high unemployment in physics (9% in 1974, 13% in 1975), it follows that only scientists younger than thirty can risk any possible failure. Unfortunately, the average age for receiving the PhD degree is in the late twenties.

The bureaucracy and the rigidity tend to produce fashionable research rather than innovative research. Take the war on cancer as an example. We have seen the ever-changing emphasis on radiation therapy, chemotherapy, virus origin of cancer, immunotherapy, and the current fad of environmental origin including hamburgers. In other words, everything except a cure. It is probably not sur-

prising that the war on cancer has now become known as the medical Vietnam.

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PRL versus JETP

Physical Review Letters is internationally recognized as the most prestigious physical journal. This means great honor and implies great responsibility.

PRL is devoted to "containing short communications dealing with important new discoveries or topics of high current interest in rapidly changing fields of research," obviously, no matter where such a paper comes from.

Let us analyze from this viewpoint the publications in arbitrarily chosen volume 40 of *PRL*. It contains 512 papers; only 133, that is 26% of the accepted papers, are not related to an American author or institution, and come from Europe, Japan, Canada, Australia and the rest of the world. Of course, the scientific importance of the institution influences the importance of the publication. However, the reverse may also be true.

If we look, for simplicity, only at the month when *PRL* is published and when a paper was received, the number of papers versus the time before publication is as follows:

Time (months)	1	2	3	4	5	6	7	8	9	10	11	12
Number of papers	16	131	147	88	63	28	16	10	2	3	3	3

The average time-lag is about 3.5 months; a quarter of all papers wait half a year on average before they are published! Assuming that they also meet *PRL* criteria of importance, do they meet the criteria of urgent publication?

The Soviet journal that corresponds to *PRL* is *JETP Letters*. For its (also randomly chosen) 23rd volume (in the AIP edition) the figures are the following

Time (months)	1	2	3	4	5
Number of papers	72	99	1	1	1

The average time-lag is just 1.5 months, that is less than half that of *PRL*! Only 1.7% of papers wait more than three months, only 0.5% wait five months; none of the papers wait longer.

This may be related to the system of refereeing in *JETP Letters*, which is therefore of some interest. As an author and referee of the *JETP Letters* and *JETP* for 20 years, I should like to describe it. The names of referees (2 for *JETP* papers) are confidential, as they are for *PRL* and *PR*. The referee receives

a small honorarium. The referee of the *JETP* is advised to submit his comments to the journal within 10 days or else to return the paper to *JETP*. If the comments are not received within the indicated time (usually because the referee is absent), the paper is sent to another referee. The referee rejects the paper *only* if it is not novel, or if it is trivial, or wrong. In each case he must be quite specific: give the reference to similar results; indicate how the main result, if he considers it trivial, may be easily obtained; indicate the mistake. If he cannot be that explicit, he must suggest that the editors send the paper to a referee who is expert in the field. Naturally, such a criticism is practically always accepted by the author (unless the author can indicate a mistake in the referee's comments) and precludes further correspondence. When the paper is novel and the referee sees no mistakes in it, but is unhappy with the style or the presentation, he recommends the publication of the paper, if certain places in it are revised according to his suggestions. After the revision, the paper is sent back to the same referee for his new comments. Typically this takes very little time. A misunderstanding between the referee and the author is very unusual.

It seems to me that, however high a journal's criteria, specificity of the referee's comments and distinction between the scientific value of the paper and its

style may be very helpful both for authors and the journal, and may essentially accelerate publication.

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PRL COMMENTS: We find Mark Azbel's comparisons of *JETP* and *PRL* interesting both as a comparison of journalism and as a comparison of scientific sociology in (largely) the US and Soviet Union. The publication time-lags, which Azbel discusses, can be separated, for *PRL*, into two distributions. For those articles which pass through our selection processes without delay, there is a time lag of a little less than two months between the data the article is received and the time when the journal containing the article is delivered to the subscriber's mailbox. Then there is a highly skewed distribution, which extends almost indefinitely, for those papers (a majority of the accepted papers) which are not approved unconditionally by two referees. It seems that the *JETP* is quicker than we are by nearly a month on immediate acceptances and the second set, the set of papers which