

representatives and make our views known. By the end of this session of Congress it may be too late.

LEWIS M. BRANSCOMB
Harvard University
Cambridge, Massachusetts

(The author is the director of the program on science, technology and public policy at the John F. Kennedy School of Government.)

Scientists Can't Afford Disinterest in US Debt

In the April Washington Reports (page 65) Irwin Goodwin presents in great detail a proposed Federal R&D budget for the next fiscal year. The news story also contains a few scattered comments that allude to fundamental problems in the Federal budget as a whole. These larger problems are so serious that the budget process described in the story sounds like a classic case of rearranging the deck chairs on the Titanic.

Goodwin makes the observation, in regard to projected interest payments on the national debt, that "such whopping payments exceed the nation's annual deficits." Well, they had better, or rather the deficits had better be smaller than the interest payments, because of a simple but little-recognized fact: In any year when the deficit is as large as the interest payment, we are borrowing all of the interest money, which causes the national debt to grow exponentially. The doubling time of the debt can be estimated from the numbers given in the story. The debt is cited as around \$4.9 trillion, with an annual interest payment of \$235 billion, which implies an interest rate of about 5%. Using the "rule of 72," the current doubling time is about 14 (72/5) years. Would we really borrow all of the interest money? Well, according to newspaper reports, deficits in recent years have actually been larger than interest payments (this situation changed just last year), and it is widely known that in a recent 12-year period the debt quadrupled, which implies a doubling time of just 6 years.

This simple picture suggests that we are in a financial state of emergency, and it may explain why the dollar has been "plunging to new lows against such strong currencies as Japan's yen and Germany's mark," as Goodwin notes. It also shows why the current efforts to balance the budget are so important. Many of us in the physics community are engaged in research that is most appropriately funded by the Federal government, so if we want such funding to continue for longer than a few more years, perhaps

our first priority should be to insist that our elected officials in Washington stop borrowing money. After all, if the budget were balanced tomorrow we would still be stuck with interest payments of more than \$200 billion every year, indefinitely. The longer we allow overspending to continue, the larger the debt and interest will grow, and the smaller the amount of money will be for R&D or any other worthwhile activity.

GARY G. GIMMESTAD
Georgia Institute of Technology
Atlanta, Georgia

Did Sagdeev Disguise Soviet System's Sins?

Richard Garwin, in his review (October 1994, page 69) of Roald Sagdeev's memoirs *The Making of a Soviet Scientist* (Wiley, 1994), presents this quotation from the book: "Many, despite the pressure of mundane life, stay firm in their selfless service to science. God help them to do so with the same grace, tenacity and integrity that distinguished that special breed of scientists, 'the keepers of the flame,' that were [Peter] Kapitsa and [Lev] Landau, [Mikhail] Leontovich and [Andrei] Sakharov." Garwin adds, "I have no doubt that Sagdeev also belongs on this list."

To place Sagdeev in the same rank as such outstanding physicists and personalities is a gross distortion of historical reality—a complete devaluation of moral standards. I worked in the Soviet Academy of Sciences for more than 50 years, meeting and talking with Sakharov, Leontovich, Landau and (less frequently) Kapitsa, as well as with many others mentioned in Sagdeev's book. The morality of the scientists and of their interrelations with the official bodies in the USSR was a significant and urgent question for me for many years, especially since 1968, when Soviet tanks entered Prague, and since 1975, as a refusenik and a participant in an unauthorized scientific seminar. This seminar was initiated in 1973 by physicists Mark Azbel, Benjamin Levich and Alexander Voronel. After their emigration to Israel, the seminar was moved from Azbel's home to the home of mathematician Victor Brailovsky. After he was arrested in 1980, the seminar operated at my home until 1987, when I and my wife, Svetlana Alpert, were permitted to leave the USSR. The seminar was attended by scientists from England, Denmark, France, Norway, Sweden, the US and other countries. In 1980—

continued on page 76

Think MagLab. Think Oxford.

Introducing the MagLab family of materials characterisation systems from Oxford Instruments - developed with experimental flexibility and reliability as the main objectives. Each system has a fully characterised sample environment. All MagLab systems are supplied with an advanced, flexible software control system for automated experimentation and data collection.



MagLab VSM

Extreme sensitivity and speed of measurement from a leading edge vibrating sample magnetometer.

- Noise base 1×10^{-6} emu p-p (2.5×10^{-7} emu RMS)
- Automatic sample positioning and 720° rotation
- Applied fields to 12 Tesla as standard
- Horizontal and vertical field options
- Sample temperatures from 3.8-300 K (300-1000 K with furnace)

MagLab Faraday

A highly sensitive Faraday balance susceptometer with fully automated measurement routines.

- Applied fields to 12 Tesla
- Sample temperatures 1.5-1000 K
- Sensitivities to 10^{-11} emu/gauss

MagLab J_c

A system for determining critical current densities.

- Applied fields to 16 Tesla
- 100 A pulsed for bulk ceramics
- 1500 A dc for testing wire and cable

MagLab Heat Capacity

A micro-calorimeter for measuring heat capacity.

- Applied fields to 12 Tesla
- Sample temperatures 0.5-200 K
- Samples up to 3×4 mm for low temperature measurements
- Extremely low measurement addendum

Call us now for a copy of our brochure "MagLab systems for materials characterisation", technical specifications and data sheets.

OXFORD

Oxford Instruments
Scientific Research Division
130A Baker Avenue
Concord, MA 01742

Circle number 11 on Reader Service Card

LETTERS (continued from page 15)

87 we had more than 300 visitors, including about 170 scientists. Five volumes of the proceedings of the seminar were published by the New York Academy of Sciences with the help and editorship of Joel Lebowitz.

It is well known how Sakharov dealt with the question of science and morality. As for his colleagues, Sakharov characterized them in 1981, in an article written while in exile in Gorki (PHYSICS TODAY, June 1981, page 29): "Every true scientist should undoubtedly muster sufficient courage and integrity to resist the temptation and the habit of conformity. Unfortunately, we are familiar with too many counterexamples in the Soviet Union, sometimes using the excuse of protecting one's laboratory or institute (usually just a pretext), sometimes for the sake of one's career, sometimes for the sake of foreign travel (a major lure in a closed country such as ours)."

In 1978 the first Joint US-USSR Workshop on Laser-Plasma Interaction took place in Zvenigorod, not far from Moscow. Two Americans, Frederick Mayer (then director of fusion experiments at KMS Fusion Inc) and Eli Yablonovitch (then at Harvard University), visited our unauthorized seminar. It was arranged that 10 to 12 American scientists were to attend the next seminar and to give a few talks.

On the arranged day, by noon some 30 Russian scientists, refuseniks and dissidents had come, Sakharov among them. Two hours later the two Americans appeared, and Yablonovitch related the following: "This morning Sagdeev summoned us and, in the presence of other Russians and American physicists, told us: 'You should not go to the seminar. There are no scientists at the seminar. Besides, to go there is dangerous.' And he added, 'Don't think that I am an anti-Semite; my wife is Jewish.'"

Mayer has recently sent me part of the 1978 report "Political Meetings" to the National Academy of Sciences about the group's meetings in Moscow. It states: "Our Soviet workshop host, S[ergey] Anisimov . . . arranged for . . . Sagdeev, together with about eight other Soviet scientists, to have a political discussion with the US delegation. . . . Sagdeev appeared to be answering questions truthfully about the numbers of 'refuseniks' . . . and the level of anti-Semitism in the USSR. We later learned he had been distorting the truth. He gave the US delegation the strong implication that if we were to visit the 'refuseniks' . . . we would be jeopardizing his and An-

isimov's careers in physics." And: "The first paper to be delivered at the seminar was a paper of Y[uri] Orlov, read by Y[uri] Golfand. [Orlov, the well-known physicist, was arrested in 1977. He was the founder of the Helsinki movement for human rights. He spent two years in a prison, seven years in the Gulag camps and two years in exile. He was deported from the USSR in 1986.] Orlov's paper was recently [that is, in 1978] published in the West. This contradicted a statement made at our meeting with Sagdeev . . . that 'Orlov was only a politician,' not a scientist. Many such contradictions came out in our discussions with the seminar attendees, making it clear that our meeting with Sagdeev had a considerable number of distortions of the facts."

An account in *Laser Focus* (November 1978, page 34) stated: "Sagdeev suggested that the visit to the refuseniks and the ensuing 'political noise' could jeopardize future exchanges. . . . He discussed the reports of a Jewish quota at Soviet universities. . . . He said there was not a quota. . . . Sagdeev was able to dissuade most of the American delegation."

Another episode bears telling. In 1983 the American mathematical physicist Norman Zabusky attended an international conference in Kiev. We arranged that on his way back to Moscow he would present a talk at the refuseniks seminar. The prominent physicist Evgeny Lifshitz invited Zabusky, his wife and his daughter to have dinner at the Lifshitz home when Zabusky came back from Kiev to Moscow.

On 1 November, soon after Zabusky's return to Moscow, Lifshitz called him to say that he had to cancel the invitation. On 2 November an Academy of Sciences official informed Zabusky that he had to leave Moscow. PHYSICS TODAY (January 1984, page 76) reported: "According to a State Department source, the science attaché at the US embassy was telephoned on 2 November . . . that Zabusky's conduct was 'inconsistent with his status as a guest of the academy.' Zabusky would have to leave by noon Saturday, 5 November." He was to deliver his talk at the refuseniks seminar in the afternoon of that day.

I learned soon thereafter that Sagdeev had visited Lifshitz and urgently "advised" him to cancel Zabusky's invitation. Thus Sagdeev had been aware of the decision of the Soviet "competent bodies" at least one day before Zabusky was told about this decision. Who asked Sagdeev to go to Lifshitz? And Sagdeev never attended our seminar. Then who informed him that "there are no scien-

tists at the seminar"?

Sagdeev wrote in a May 1992 article in PHYSICS TODAY (page 22) that the Soviet Academy of Sciences "was in the first row of political opponents to the former regime. . . . The scientists (myself among them) were bringing up all the issues of *perestroika* that would later become official policy."

However, on 27 January 1980 the newspaper *Izvestiya* published an academy memorandum stating that "the presidium of the Academy of Sciences noted that . . . academician Sakharov continues to carry out actions directed toward undermining the Soviet state system. . . . Sakharov is approving the politics of the most reactionary, aggressive, imperialistic circles, for example, of Senator [Henry] Jackson." And on 2 July 1983 four influential academicians, the Nobelist Alexander Prokhorov (director of the Physical Institute), Georgy Skryabin (the foreign secretary of the academy), Anatoly Dorodnitsyn and Andrei Tikhonov (directors of two scientific institutes), wrote in *Izvestiya*: "What kind of man is [Sakharov] to fall so low morally, to come to hate his own country and its people? . . . We know Sakharov is very popular with those Americans who would like to wipe out our country . . . off the face of the earth." This is how the Soviet Academy "was in the first row of political opponents to the former regime."

Sagdeev describes in his book his activity in the "Gang of Four" academicians (the other three were Georgy Arbatov, Evgeny Primakov and Evgeny Velikhov), referring to them as "political call girls of the Soviet delegation." Surely this group is more relevant to Sagdeev than are the "keepers of the flame."

It was not too pleasant for me to present this letter to the readers of PHYSICS TODAY, which has dealt so much with the subject of science and morality. But to have allowed the abuse of the name of Andrei Sakharov to go uncontested would have been too great a sin.

YAKOV ALPERT

Harvard-Smithsonian Center
for Astrophysics
Cambridge, Massachusetts

SAGDEEV REPLIES: Andrei Sakharov was unique—no question about it. I wrote in *The Making of a Soviet Scientist*: "The real courage . . . in the fight against the totalitarian regime and mentality was shown by . . . Sakharov, Yuri Orlov, and their friends. . . . They had burned their bridges to the system and openly declared disobedience—and even moral

civil war. I have always had admiration for people of such heroism and self-sacrifice. However, most of us at that time considered them Don Quixotes." No one is Sakharov. In that I agree with Yakov Alpert.

But a vicious personal attack on me launched, sadly, in a Soviet style of incriminations, cannot be left unanswered:

▷ First, in relation to the 1978 first Joint US-USSR Workshop on Laser-Plasma Interactions: I was one of the initiators of cooperation to keep scientific contacts alive in the political chill that settled in after the achievements of détente were almost forgotten. A group of about 12 Americans flew to Moscow to participate in that workshop as guests of the Soviet Academy of Sciences. Our side was afraid that participation of the American group (in its totality) in a seminar of refuseniks could be considered by the authorities to be a political demonstration and so endanger cooperation. In an informal discussion (in the woods, as suggested by the American guests to avoid potential tapping) I and my colleagues conveyed our concern. I understand that not everyone was happy to see that some Americans afterward did not attend the refuseniks seminar. But to take scientific cooperation a hostage in a political confrontation between the refuseniks and the regime would have been meaningless, simply because the latter did not value academic cooperation at all. As a net result many Soviet scientists, especially among the younger generation, not qualified even to become refuseniks, would have been cut off from international science. I resolutely reject the quotations Alpert attributes to me (like "There are no scientists at the seminar").

▷ Alpert apparently considers my alleged remarks about a Jewish quota at Soviet universities a big sin. I tried to convey in that informal conversation that the Soviet government avoided issuing official quotas but encouraged them indirectly. To my knowledge, the opening of the Soviet archives has not led to the discovery of direct governmental instructions concerning such quotas.

▷ Alpert's interpretation of the whole story concerning Norman Zabusky is the product of his imagination. Zabusky, my old colleague and friend, came to the Soviet Union as my guest (I was the cochairman of the Kiev conference and the director of the host institution in Moscow). The rude intervention of the authorities canceling his visa was perhaps done to penalize not only the guest but the host too. A number of scientific seminars and contacts (not only with refuseniks) also were canceled. I was indeed the first to whom the authori-

ties communicated (not without satisfaction) the news about the visa cancellations. I did not visit Evgeny Lifshitz. I phoned him to inform him about the situation, since I was to be one of the guests at his dinner. I did not "advise" Lifshitz to cancel the dinner: That was his own immediate reaction, though I did not insist that he keep his plans for the dinner. Everyone knew that Lifshitz, an outstanding theoretical physicist and the alter ego of Lev Landau in their famous course, had only recently been given a chance for foreign travel after several decades of blackout.

▷ I stand firmly by my statement that the Soviet Academy "was in the first row of political opponents to the former regime." My May 1992 PHYSICS TODAY article and similar publications of mine provide the story, well known in Russia, of how scientists (not necessarily the academicians) at the institutes of the academy began one of the first truly democratic movements in the USSR. That movement became the cradle for a "democratic Russia." The leaders of the movement were pupils and younger colleagues of Sakharov, who led them eventually to the Congress of People's Deputies, the first Soviet-era parliament that allowed democratic opposition. (At that time, in the late 1980s, Sakharov was a member of the presidium of the academy, which made that body completely different from what it was in 1980.) I will always be proud that Sakharov trusted me at that time to be his *Doverennoe Litso*, an official title meaning confidant in an electoral campaign.

ROALD SAGDEEV

University of Maryland, College Park

GARWIN REPLIES: My review of *The Making of a Soviet Scientist* conveyed my fascination with the material and style of the book and my admiration for its author, Roald Z. Sagdeev. Yakov Alpert faults as a "complete devaluation of moral standards" my putting Sagdeev in the same category as Peter Kapitsa, Lev Landau, Mikhail Leontovich and Andrei Sakharov.

I knew Kapitsa well, was familiar with the work of Landau and met Sakharov a number of times. It is often said that comparisons are odious, and they certainly cause a lot of trouble, but I will venture one more: I think that Sakharov belongs near the top of any list of saints, but I would not put Sakharov as a physicist in the same category as that great categorizer and physicist Landau. Some would place Sagdeev only on a somewhat longer list; that is a matter of individual judgment.

I have encouraged Alpert to write his reminiscences a lot more fully than can be printed in PHYSICS TODAY, because I believe that these different views of a historic time can only benefit our understanding.

As an American scientist born in 1928, I have no firsthand experience with the terror of the Soviet system under Stalin, to which the activities of Senator Joseph McCarthy and various pressures from government and nongovernmental groups in our country can hardly compare. None of us can judge how scientists should behave in a totalitarian state. Yet among American colleagues of equal courage and conviction there has been a range of judgment as to the right course of action on numerous issues, even though risks to life and career have been less here than in the Soviet Union. Some have expressed themselves fully and publicly in opposition to some government program they felt was wrong or immoral, while others have worked more quietly to move government policy in a direction they felt to be right.

To preserve a large institute as an oasis of competence, effectiveness and scientific independence, as I judge Sagdeev did when he ran the USSR's Institute for Space Science, is no small benefit, and concern for such preservation may well affect the actions of an institute director.

I have great admiration for the Soviet dissidents who risked so much to change the system, and also for those "refusenik" scientists who organized the refusenik seminars. In my many trips to Moscow I never participated in such a seminar, but I am glad that many of my colleagues did so.

Only Sagdeev can reply to the charge that he said, "There are no scientists at the seminar." But I believe that Alpert gives an incomplete view in quoting from Sagdeev's May 1992 PHYSICS TODAY article the statement that the Soviet Academy of Sciences "was in the first row of political opponents to the former regime" as if Sagdeev were concealing the often contemptible actions of the academy. On the contrary, substantial portions of *The Making of a Soviet Scientist* describe the anti-Sakharov activities of the Soviet Academy of Sciences in support of the Central Committee of the Communist Party, for instance in publishing a letter of condemnation of Sakharov signed by 73 members of the academy.

I am glad Sagdeev wrote his book, which provides substantial insights into the Soviet system. Alpert's view of events will also be valuable. Eventually, we will have both information

and examples to help us as individuals and as a society to learn to conduct ourselves with courage, effectiveness and honor.

RICHARD L. GARWIN

*IBM Thomas J. Watson Research Center
Yorktown Heights, New York*

Parapsychological Review A?

I have a serious concern that I would like to present to the physics community at large. It appears to me that there is a small but dedicated group of scientists—some with quite respectable reputations—who nevertheless dabble in things that most of us would not call science. (The terms “pseudoscience” and “pathological science” come to mind.) Occasionally attempts are made to dress up this type of work and anoint it with the trappings of “real science” and then usher it into publication in prestigious journals along with mainstream material—giving it the mantle of undeserved legitimacy.

For example, in the 1 July 1994 issue of *Physical Review A* there appeared an article by Henry P. Stapp, “Theoretical Model of a Purported Empirical Violation of the Predictions of Quantum Theory.”¹ This paper develops an *acausal* theoretical model of nonlinear quantum mechanics that is loosely based on work by Steven Weinberg. It is clear that this article was specifically created to explain the apparently anomalous results found in experiments designed to establish the physical reality of supposed paranormal phenomena: Stapp’s reference 8 is to the telekinesis experiments of Helmut Schmidt, published in the *Journal of Parapsychology*.² Schmidt claims to have demonstrated that human beings are able to use psychic powers to retroactively alter the decay rate of naturally occurring radioactive isotopes months before the actual experiment took place. Schmidt’s conclusion—which Stapp has tried to model theoretically in *PRA*—is that the test subjects used their psychic powers to alter the laws of quantum mechanics and Einstein causality.

Several scientific investigations have dismissed previous paranormal experiments by Schmidt as statistically and scientifically unsound, at the very least.³ The latest experiments—summarized in reference 2—have not been reproduced. At the very least, these acausal “telekinesis” experiments seem too controversial and pseudoscientific for there to be theories appearing in *Physical Review A* purporting to explain them. It’s

one thing for the physics community to be open-minded but entirely another for us to be supporting parapsychology and promoting pseudoscience. You can be sure that Schmidt, in his future publications in the *Journal of Parapsychology*, will reference Stapp’s paper and claim that a theory that explains all his experimental data has been published in the flagship journal of the American Physical Society.

There is another interesting point. When you read Schmidt’s paper² you find that it is not itself a report of an experimental result but rather a summary and statistical analysis of five experimental results—all by Schmidt—published in the *Journal of Parapsychology* from 1986 to 1993. Each of those experiments tested “psychic” subjects for their ability to acausally and telekinetically alter the generation in the past of random numbers based on the decay of radioisotopes. Each of Schmidt’s five corresponding papers reports data that, although suggestive, are not statistically significant. In the summarizing paper²—the one that Stapp actually refers to—Schmidt averages the results of his previous five experiments. Upon doing so he finds a statistically significant indication of acausal telekinetic activity. Each of the five experiments was carried out with Schmidt as principal investigator and first author, with the names of one or two coinvestigators appearing as second or third author. For the fifth experiment, published immediately preceding Schmidt’s summarizing paper² in the same issue of the *Journal of Parapsychology*, Schmidt’s coauthor and coexperimenter is none other than Stapp.⁴ Hence Stapp’s theory paper in *PRA*¹ is in fact a theoretical explanation for Stapp’s own experiment. It seems odd that this connection was nowhere mentioned by Stapp in his *PRA* article.

The *Physical Review* editorial board has informed me that some changes have been made in the guide to authors and referees to reduce the possibility of such papers’ being published in the future. However, it is not clear to me that this solves the problem, or even that the physics community at large is even aware that there is a problem. Should we take the extreme “open-minded” position and let such papers appear, rather than be accused of censorship? Or should we put our foot down and say, “Articles dealing with parapsychology should not be published in *PRA*—period.” (I’m not advocating that papers like Stapp’s not be published at all, but there are more appropriate forums, for example, the *Journal of*

Parapsychology.) In any case, only we as physicists can decide this. I hope that this missive will stimulate interesting public debate on this topic.

One might argue that a more appropriate approach for criticizing an article in *PRA* would be to submit an official comment to *PRA*. In fact, I and four coauthors are preparing such a comment. We will discuss our concerns about the physical theory of Stapp as well as the experiments of Schmidt. The purpose of this letter is not to discuss the nuts and bolts of a particular physical problem but to bring out into the open my concerns about the philosophy and direction of physics as a whole.

References

1. H. P. Stapp, *Phys. Rev. A* **50**, 18 (1994).
2. H. Schmidt, *J. Parapsychol.* **57**, 351 (1993).
3. C. E. M. Hansel, *The Skeptical Inquirer*, Spring 1981, p. 26. R. Hyman, *ibid.*, p. 34. J. E. Alcock, *Science and Supernature*, Prometheus, Buffalo, N. Y. (1990), pp. 81–181.
4. H. Schmidt, H. P. Stapp, *J. Parapsychol.* **57**, 331 (1993).

JONATHAN P. DOWLING

*US Army Missile Command
Redstone Arsenal, Alabama*

STAPP REPLIES: A scientist does not become “dedicated” to pseudoscience by accepting a challenge to examine purely physical facts created under highly controlled conditions. Indeed, to *refuse* to look at such physical evidence on ideological grounds would be pseudoscience.

Let me describe the special circumstances that led me to submit my theory paper to the *Physical Review*. I was approached during a conference by Helmut Schmidt, who asked me why in view of my long-standing interest in the apparent nonlocality associated with Bell’s theorem, I never referred to his experiments, which seem to indicate the existence of a similar kind of effect. I replied, frankly, that the results he claimed seemed to me so astounding that I would sooner believe in the occurrence of a procedural flaw, or even outright fraud, than in the reality of the claimed effect; since I lacked the expertise and time to do confirming experiments myself I simply remained silent.

He answered that there was a very simple procedure that I could carry out in my own office, involving only printed numbers and no dealings with human subjects, that should allow me to confirm the reality of the claimed effects (which are backed up by the claims of other “psi” researchers¹) without my having to make any assumptions at all about