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LETTERS

MORALITY VERSUS TECHNICALITIES

During the past year I have become increasingly dismayed to find progressively increased attention (in *Physics and Society*) being paid to the technical aspects of weapons systems development at the expense of what I believe to be the more important moral questions. In my opinion the July 1987 issue is an extreme example of the preponderance of such technical discussion.

Satisfying the "Nitze Criteria" and/or increasing directed energy beam "Brightness" are the last questions that should be addressed by a working physicist. The first questions should always involve the morality of the task assuming that it can be completed with at least partial success. Some examples of morally relevant SDI questions are: (1) To the extent that the SDI will increase our war fighting capability (at least against lesser military powers) do we as individual physicists wish to give our military and political officials such increased capabilities? (2) To the extent that a nuclear exchange without defense is MADness while a nuclear exchange with an effective use of SDI by only one side is genocidal, a fair question is do we wish to give the genocidal option to our government? (3) If the US and the USSR each have a partially effective SDI system, in the absence of strategic nuclear disarmament, what is really gained other than an escalation of unproductive technology and terror (similar to the I MIRV and you MIRV folly)?

There are many similar questions that should be asked by every physicist contemplating working in support of the SDI program. As physicists we should assume that with our help the SDI program will be at least partially successful, and then the question is do we really want such success. An analogous example is the Manhattan project. With admitted hindsight I now believe that it was immoral for a physicist to work on the Manhattan project, and I believe that the analogy can be carried over to the SDI program.

Thank you for the opportunity to present my views.

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MANAGED VERSUS UNMANAGED 7-YEAR ELECTRIC GROWTH

Evan Mills and Arthur H. Rosenfeld (April 1987) claim that "California and Texas illustrate the effects of regulation versus *laissez faire* on conservation" and that "Texas has left matters almost entirely to the marketplace." Those claims are preposterous. Are we to believe that electricity in Texas is supplied by competing firms, free to offer whatever services and charge whatever prices they see fit, free to enter and leave the market and focus their efforts on any part of it? No, the authors' later reference to the "Texas Public Utilities Commission" confirms that electric power in that state, as elsewhere, is a government-regulated monopoly. Their comparison, therefore, is not between regulation and *laissez faire,* but merely between one heavily regulated environment and another.

Mills and Rosefeld approve of government's dictating to people what kinds of buildings and electrical appliances they may build, sell, and buy. I propose the opposite: Instead of increasing coercion, let us decrease it by putting an end to government-mandated electric power monopolies and opening up the market to free competition. There is little reason why telephone poles and underground conduits cannot carry several power lines instead of just one, but such duplication is unnecessary. Competing firms could contribute to a common power grid, with communication links from individual consumers determining the output required of each firm at each moment.

Government often imposes coercive measures to deal with problems which, though blamed on the market, are themselves largely the fault of government intervention. If refrigerators have tended to be a bit inefficient, might that have something to do with government's persistent efforts, historically, to maintain artificially low electricity prices? Similarly, did not government regulation of oil and natural gas prices encourage gas-guzzling cars and thermally porous houses? Was it not government that pushed prematurely the development of nuclear power and limited the liability of electric utilities for any nuclear accidents? If new power plants are so very expensive, might that have something to do with how long and uncertain the approval process for a new plant has become? Might it also be related to the government-fostered power of construction unions to drive up costs and resist accountability for poor workmanship?

Mills and Rosenfeld have seized on a particular statistic, namely a couple of percentage points' difference in the rate of growth of electricity demand between California and Texas, in order to extol the virtures of government regulation. In so doing, they have overlooked a long, clear-cut history of government mismanagement of the energy market and other markets. Their article sheds little light on the relative merits of regulations and *laissez faire,* but it does serve to illustrate the uncritical attitude with which government coercion is so often advocated.

Allan Walstad, Physics Department, University of Pittsburg at Johnstown, Johnstown, PA 15904.

Response:

About half of Walstad's comments are about the supply side of the energy equation. Although we said nothing about the supply side, we tend to agree with his remarks about the advantages of competition and deregulation. However, it is important to keep in mind that the original impetus for the regulation of electric utilities was that they are natural monopolies and the free-market provision of power would be societally inefficient.

The other half of Walstad's comments are about regulation on the demand side, and even these are only halfway on the mark, because only about half of California's gain in efficiency is related to building and appliance standards. The rest seem to be brought about by utility pricing and incentive programs. And this is what we mean by *laissez faire*. The free market in Texas had done little on its own to promote conservation. Let us detail the most important single standard-related saving in California, the refrigerator standard.

In 1976, right after the embargo, the California state legislature passed a bill that set maximum energy consumption levels for a variety of household appliances; those standards have since been adjusted to reflect vastly improved technology. It's one of the cleanest pieces of economics around, and has had an effect both in California and around the United States.

The number of kilowatt-hours required to run a typical refrigera tor each year is being ratcheted down from 1900, where it started out in 1977, to 700 in 1993. There is no change in the service the refrigerator provides. All that we've done is double the efficiency. How much does it cost (retail) to go from the old, inefficient refrigerator to the 1993 model? The California Energy Commission estimates $100.

What does society save by that $100 investment? At 8 cents per kilowatt-hour, the less-efficient refrigerator costs $150 per year to operate and the efficient one costs about $55 per year. With those annual savings of $100, the customer can pay off the retail surcost in the first year. The refrigerator lasts 19 more years, so the savings for the following 19 years are pure profit for the consumer.

Critics have charged that these refrigerator standards are "coercing the American public." Well, yes, they do coerce the public, but not very strongly. They force the public to do things with a one-year payback, not a ten-year or thirty-year payback. Californians, at least, don't seem to mind being coerced to this extent.

These standards have affected electricity use throughout the United States, where refrigerators use the output of 50 power plants of the standard one-gigawatt size. Since manufacturers didn't bother to build new assembly lines to keep making crummier refrigerators than they could sell in California, every new production line that's been set up since the California standards were enacted conforms to those standards. There is a little "dumping" at first, but eventually all refrigerators tend to conform. By the time we get down to the 1993 standard, we'll be down to 18 gigawatts nationally. The savings, then, are 32 GW, or 32 large central-station power plants.

Evan Mills and Arthur Rosefeld, Center for Building Science, Lawrence Berkeley Laboratory, University of California, Berkeley, CA 94720.

THE SWORD OF JEHOVAH

I would like to provide some additional background and perspective on the article by Paul Craig (July 1987).

Potential offensive uses of lasers have been discussed by a number of physicists, although Craig does me the honor of referring only to my work. That article, "Nuclear Winter and the Strategic Defense Initiative," actually appeared in the January 1986 issue of *Physics and Society*, and was reprinted in *Civil Defense: A Choice of Disasters*, John Dowling and Evans M. Harrell, eds., American Institute of Physics, New York, 1987.

Craig makes the important point that high-power SDI lasers have the potential for offensive tactical use, for example against artillery emplacements. He goes on to discuss assassination of political leaders, setting oil refineries on fire, incendiary attacks against cities and forests, and destruction of crops.

The earliest paper on offensive applications that I am aware of is the informal report by A.L. Latter and E. A. Martinelli.[1] Later work has been done by T.S. Trowbridge,[2] Jurgen Altman,[3-5] and Roger Main.[6,7] During the annual meeting of the AAAS in February 1987, a symposium organized by Don Rote and Barry O'Neill was devoted in part to offensive use of SDI. Presentations were given by Harvey Lynch,[8] Peter Zimmerman, and myself. Offensive uses have also been discussed in the Soviet literature.[9]

The many potential offensive applications of SDI technology should be investigated in much more detail.

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Caroline L. Herzenberg, Technology Evaluations Group, Energy and Environmental Systems Division, Argonne National Laboratory, 9700 S. Cass Ave., Argonne, IL 60539. (Opinions expressed are those of the author, and should not be interpreted as representing Argonne National Laboratory.)

If Star Wars is set up the way Paul Craig describes it then we can see at once why Star Wars will fail: 1) Time is given in the AM-PM format instead of in the standard 24 hour form. 2) Coordinates are given in degrees, minutes and seconds, range is given in yards, and "kill diameter" is given in meters. If there are any ocean-going people in the bureaucracy they would want distances given in nautical miles. 3) In the analysis of the Soviet booster rockets, their major dimensions are given in feet and the thickness of their shell walls is given in millimeters.

Whether we are talking about peaceful commercial competition with other nations, or about Star Wars, we decimate our probability of success by our national and professional refusal to use SI units exclusively. I suspect that we physicists, each with our own petty preferences for units, are among our nation's worst offenders. Since our reluctance to move to the exclusive use of SI puts us at a great military and commercial disadvantage, one could imagine that our enemies are rejoicing over our recalcitrance.

As we ponder physics and society we can see that physicists can play a significant leadership role in helping the U.S. to the point where we can compete (peacefully or militarily) on an even basis with other nations. First we must move to the exclusive use of SI in physics. Then we must provide leadership to help the U.S. move

rapidly to the use of SI in all aspects of engineering, trade, and commerce.
Albert A. Bartlett, Professor of Physics, University of Colorado, Boulder, CO 80309.

I am not disputing the effectiveness of the laser-beam Sword. However, may I suggest that the United States of America is not Jehova; nor is Professor Craig His prophet.

Maybe our genes have doomed us to hostility. Maybe offense is a necessary evil in certain situations. But, at the very least, let's not be poetic about it! "A few hundred microseconds later the Vietnamese sky glows for a moment as Sword of Jehovah strikes...Virtually instantly five Viet Cong operating the gun emplacement, every bit of living matter, shrubbery, ants, and a few small animals are incinerated...There is no sign of life." This is not taking the bitter pill of war. This is not the way that issues should be discussed in Academe. Professor Craig is doing his armchair-killing *with gusto*.

So now we are treated to the bottom line. The Strategic Defense Initiative may not give as effective a *defense* as its supporters believe.[1] But, at least, it does provide an overwhelming *offense* !

Mr. Gorbachev knew *that* much physics all along. That's why he opposed, so vehemently, this "Strategic Offense Initiative."

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Peter Halevi, Depto. de Fisica del ICUAP, Apdo. Post. J-48, Puebla, Pue. 72570, Mexico.

Paul Craig's enumeration of some of the endless possibilites of the SDI is revealing. It shows the true motive of those pushing for SDI: to have a weapon for worldwide aggression!

Consider: we can obliterate any leader that does not suit the United States, clandestinely destroy people's crops and irradiate entire regions inhabited by people unbeknownst to them. This is stuff straight out of Dr. Strangelove. With a Death Star hovering over the Earth, humanity will have to move underground where it will be fighting over the mine shafts.

Of course people targeted by such programs may not stand idly by. It is possible that some of them who are better educated and who have better technology than we might get their own Death Star in space first and then obliterate *our* leader and destory *our* crops.
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ARTICLES

A COMPREHENSIVE TEST BAN: THE TECHNICAL AND POLITICAL CHALLENGES

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Introduction

Over the past year and a half SIPRI and the Canadian Institute for International Peace and Security (CIIPS) have been examining the prospects for a comprehensive test ban (CTB). This study, involving a panel of arms-control analysts, nuclear physicists, seismologists and political scientists, ranged over all the major aspects of a CTB and shed more light on some of the issues less commonly studied.[1] The table of contents of the soon-to-be-published study is appended below. In what follows I shall briefly summarize the conclusions of the study and add some comments of my own.

Military acceptability of a CTB

On this question there appeared to be a wide spectrum of opinion, from those unwilling to accept any further restrictions on nuclear testing to those in favour of a complete ban at an early date. Among the weapons scientists consulted, consensus was equally elusive. Full-scale explosive testing was generally held to be essential to the production of new weapon-designs, largely because of the demands of miniaturisation and/or shaping to fit specific delivery systems. The number of tests per weapon could be lowered - indeed, it was said to be the practice to keep them to a minimum - but never to zero.

In the face of this irreducible condition, it remained only to consider a) how low, in terms of yield, further testing-restrictions might go, or b) whether nuclear-weapon states would be able to agree to a freeze on new delivery systems in conjunction with a CTB. On a), the restrictions thought possible ranged fom 10-kt down to 1-kt; on b) considerable scepticism was expressed, with many weapons designers arguing, in line with the present US administration, that a CTB would over time remove the confidence in the efficiency of nuclear arsenals needed to underpin deterrence.

In the former case, the upper limit of 10-kt apparently represents the lowest level acceptable to many US weapons designers, either for testing the fission 'trigger' for thermonuclear devices, or for testing large fission weapons at reduced yield. (Into the bargain, concentration on the latter technique would mean unquantified further testing to get the extrapolations right, because the 150-kt limit of the Threshold Test Ban Treaty (TTBT) has not created great demands for such experiments hitherto). To others, the lower limit of 1-kt is quite arbitrary and mentioned solely in response to calls for further testing limitations. They do not fail to point out, however, that nuclear explosions below 1-kt are possible, as well as non-explosive nuclear experiments in the laboratory, like those connected with SDI. This leads to definitional problems of what constitutes a nuclear explosion or a weapons laboratory, problems already foreshadowed in the US-Soviet impasse over SDI.

The alternative to a low-threshold test-ban, that of a CTB to 'freeze' existing nuclear-weapon systems, with all work on new designs abandoned, met with some ambivalence on the part of the - mainly US - nuclear-weapons fraternity. Here it is less the ingenuity of the weapons laboratories which is at stake than the long-term reliability of their current on-the-shelf products. Naturally enough, there were those scientists who were prepared to claim that the bulk of existing nuclear arsenals, if retained, would still befunctional 15 or 20 years from now. Others, however, invoking faults discovered during tests, insisted on the importance of 'diagnostic' explosions and argued into the bargain that the simpler Soviet designs would 'age' better.

This assumption may, indeed, have underlain the recent Soviet initiative to persuade the other nuclear-weapon states to join in a test moratorium as an opening to negotiations on a CTB. It has also given rise to the well-publicised debate in the USA in which some dissident weapons scientists have accused the laboratories of purposely designing delicate and over-complex assemblies in order to thwart a CTB indefinitely, and to the suggestion that US designers should copy their Soviet counterparts and go for simpler models.[2] This suggestion is a practical contribution to the broader argument for a CTB to be followed by progressive reductions and, possibly, simplifications in weapons systems. Consideration was given in our study to what these might be - without much success, however. It has not been easy in the best of political climates to nominate nuclear weapons for trade-offs, and at present, with all attention focused on the outcome of SDI, traditional arms control has been made to seem increasingly irrelevant.

Verification of a CTB

The SIPRI/CIIPS study investigated all the major aspects of CTB verification, notably in-country seismic stations, a global network of stations to link up with the latter in recording and processing seismic data, and on-site inspection to follow up suspect events. Non-seismic detection techniques were also considered. The findings of the recent Natural Resources Defense Council mission to Semipalatinsk in the USSR were discussed. A low-threshold superpower agreement of limited initial duration was mooted, permitting the calibration of pre-announced tests in order to develop existing seismological detection capabilities.

The dominant concern towards which all these inquiries in some sense tended was that of being able to remotely detect and identify any underground nuclear explosion in the Soviet Union. It has been shown that exploding a device in an underground cavity is likely to dampen the seismic signal emitted, making the signal more dificult for a remote sensor to detect and identify against the background of naturally-occurring seismic events. This is known as 'decoupling' an explosion. Many opponents of a CTB, pointing to the extent of the Soviet land-mass, our comparative ignorance of its geology, and the tradition of political secrecy in the Soviet Union, argue that the Soviet leadership would be constantly tempted to evade a CTB treaty using cavity-decoupling, and thereby gain military advantage over other nuclear-weapon states.

Some of the scientists consulted in our study argued that the sophistication of today's seismological techniques, particularly as seen in the experience of the Northern European seismic arrays, weighs decisively in favour of the verifiability of a CTB. With all capabilities, extraterritorial, in-country and extra-atmospehric, brought into play, even decoupled nuclear explosions on Soviet territory could be discriminated down to 1-kt. Using remote seismological techniques alone, they could be discriminated down to 10-kt (the confidence here is based on recent progress in filtering high-frequency seismic waves). It was also pointed out that to gain a significant military advantage, a whole series of nuclear tests would have to be carried out. Preparations to do this by digging fresh cavaities would be huge and highly visible from space. Hence, with no certainity of escaping seismological detection, a potential evader would most probably be deterred from attempting clandestine test-ban evasion. Others took a less sanguine view, insisting on the temptations of cavity-decoupling to a country like the Soviet Union. The technique, they argued, is feasible, given the possiblity of disguising excavations under mining operations, and given the large number of ready-made cavities in the Soviet Union, some of them unidentified, some created by explosions for economic, as opposed to military purposes. They also disagreeed with the claims made for remote seismological detection of decoupled tests.

The suspicion inherent in these latter arguments underlines the fact that verification is a subjective, as well as a technical matter. For this reason alone, it may be wise to assume that a verifiable CTBT is a contradiction in terms.[3] At all events, it is surely better to push forward the frontiers of seismological detection, building on the considerable progress already made and increasing the stock of confidence acquired, than to abandon the initiative for further significant test-limitations.

In this respoect, the US proposal to adopt the CORRTEX yield-measurement technique in a US-Soviet 'test-exchange' is a pure political distraction from the CTB issue. Firstly, the ostensible Soviet 'test-exchange' is a pure political distraction from the CTB issue. Firstly, the ostensible US purpose is merely to assist the executive to forward the TTBT to Congress for ratification (which Congress has repeatedly requested the President to do). Secondly, the usefulness of CORRTEX in measuring low explosive yields remains uncertain (one area of consensus in our study). Thirdly, the Soviet government has since announced its own technique, thereby opening the door for Soviet scientists' counter-assessments.[4] By agreeing in principle to the US proposal, therefore, the Soviets may be preparing to catch the Reagan administration in a trap of its own making. The lesson from such experience is that nothing will ever equal the importance of improved detection capabilities using remote seismological means and in the hands of many countries. Earthquake research will continue to advance the cause of seismological verification; the involvement of ever more countries in the CTB process will enhance its objectivity.

A CTB in international relations

The political analysts on the SIPRI/CIIPS study looked at the broad strategic problems which raise obstacles in the path of a CTB. Most obviously, there is the current mismatch between US and Soviet declaratory policy on a CTB. Secondly, France and China would be unlikely to co-operate on a CTB without deep cuts in the superpowers' nuclear arsenals. Thirdly, there is a handful of states with clandestine or embryonic fission-weapon capability whose likely positions on a CTB remain totally inscrutable, particularly if, as is increasingly widely admitted, nuclear testing is no longer considered indispensable for first-generation devices. 'Horizontal' nuclear proliferation could in this sense prove to be the joker in the pack of global nuclear arms control.

All these problems suggest that much of the discussion of a CTB is too narrowly conceived. It is too often forgotten that the Non-

Proliferation Treaty (NPT) contains a commitment to work for 'the discontinuance of all test explosions of nuclear weapons for all time' - a measure called for by every Review Conference since the Treaty was signed. A CTB would, moreover, introduce less technical uncertainty into the plans of the mature nuclear-weapon states than into those of aspirants to nuclear-power status, while having a symbolically equalising effect in the international arena far beyond its practical implications. The stage would be set for the discussions of 'minimum deterrence' which logically should underpin any major reductions in the nuclear powers' strategic systems. For even if the superpowers realise the shared protection dreamed of in SDI, this would not rule out a 'minimum nuclear power' appearing elsewhere.

Conclusion

To this European observer, these are the main issues. Traditional US openness in discussions of weapon-design and its relation to testing has contributed significantly to understanding of the questions surrounding a CTB. However, these discussions also have their arcane and provincial side, not to mention the dangers of disinformation, when the discussants are judge and jury in their own cause and the concerns are those of superpowers. (It is notable thatUS openness does not extend to the yield of the X-ray laser, which raises issues of treaty-compliance as well as strategy). At the same time, one should remember that military testing is an essential preparation for war, and that weapons designers can claim to be doing no more than their professional duty in resisting restrictions on nuclear testing. To this extent, the heated debate in the US about the relative reliability of the superpowers' arsenals under a complete text-ban is a blind alley.

The values of superpowers, in short, are not necessarily those of the rest of the world; and insofar as the prospect of a CTB threatens to undermine the existing global strategic balance, it should concentrate minds on the search for lower levels of nuclear stability everywhere, as opposed to purely national solutions to the dilemmas of deterrence. Similarly, scientists contemplating the pros and cons of a CTB should place their duty to international peace above the goals of national strategy. In the immediate present, the Soviet offer of negotiations on a CTB deserves a positive response from the West.

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* The views expressed in this article are personally held and should not be attributed to SIPRI.

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4. See speech by Andranik Petrosyants, Chairman of USSR State Committee for Atomic Energy, to Conference on Disarmament: document CD/PV. 353, 3 April 1986, p. 12.

APPENDIX: Contents for NUCLEAR WEAPON TESTS: PROHIBITION OR LIMITATION?, edited by Jozef Goldblat and David Cox (to be published by Oxford University Press).

PERSPECTIVES ON RADON AND OTHER RADIATION SOURCES

By David Bodansky, Department of Physics, University of Washington, Seattle, WA 98195*

In a letter in the April 1987 issue of *Physics and Society*[1], Henry Hurwitz pointed out the inconsistency between society's approach to radiation exposures from indoor radon and its approach to exposures from other sources of ionizing radiation, in this case Chernobyl. Chernobyl is obviously the extreme example for any such comparison, nuclear weapons aside. However, the fears of radiation set in at levels far below Chernobyl (for example, they paralyze the search for a nuclear waste disposal site) and perspective is needed on a much broader array of radiation concerns. To help provide some perspective, this note will describe sources of indoor radon exposure, indicate the magnitudes of typical radiation doses from radon, and compare radon to others sources of radiation in terms of doses and current regulatory policies.

Radon, more specifically Rn-222 (half-life=3.82 days), is a noble gas produced in the decay of Ra-226. Ra-226 and its parent U-238 are contained in all soil and rocks and so, therefore, is radon. Radon in houses comes primarily from the underlying ground, and can build up to concentrations which are much greater than those in the open air. The rate of penetration of radon into a house depends on the local concentration of radium in the ground, on the permeability of the ground to the movement of radon, the availability of pathways into the house, and pressure differences between the ground and indoors.

Indoor radon concentrations vary widely from house-to-house. They also vary with changes in climate conditions and house use. Thus, estimates of the average indoor radon concentrations in the U.S. represent broad averages over location and time. Several studies indicate that the mean radon concentration for single-family homes in the U.S. is about 1.5 picocuries per liter of air (pCi/L).[2,3] About 2% of the homes have concentrations above 8 pCi/L. The U.S. record to date appears to be a house in Maine with a reported concentration of 4354 pCi/L.[4]

The immediate decay products of radon-222 have lifetimes of less than 30 minutes. When these radon daughters are inhaled they lodge in the lung, while the radon itself is quickly exhaled. The biological damage, namely cancer induction, is due almost entirely to alpha particles emitted by the daughters. The impact of radon exposures can be assessed in a number of ways.

• *By exptrapolation from the experience of miners.* There have been extensive studies which show that high radon exposures have caused an excess of lung cancer deaths among uranium and other miners. If one assumes that the number of lung cancer deaths due to radon is linearly proportional to the radon exposure, the expected number of deaths in the general population can be calculated from the data for miners. The uncertainties in the input data and methodology are large, as indicated by the range of estimates for the U.S. cited by the EPA: 5000 to 20,000 deaths per year in one formulation and about three times as many in another.[5] It seems reasonable to take 10,000 deaths per year as a nominal, although highly uncertain, value for the U.S.

•*From comparisons of lung cancer fatalities among the general population in "high radon" and "low radon" regions.* Such studies are confounded by smoking, by movements of people in and out of the regions, and by possible changes over the years in radon levels. Studies to date have failed to establish any convincing lung cancer excess in the "high radon" regions and a number of studies conclude that the cancer rates in these regions are significantly less than the rates predicted on the basis of a linear extrapolation from the results for miners.[6] However, these conclusions are controversial, and this very important appraoch has not yet yielded results which are widely accepted as establishing a breakdown in linearity for alpha particles.

• *By converting the radon exposure to the lung to an "effective dose equivalent" and calculating the resulting cancer rate.* For a given radon concentration, it is possible to convert the calculated alpha particle energy deposition in the lung into an "effective dose equivalent," a dose formulation which puts all doses on a common (effective whole body) footing. The effective dose equivalent can be used to calculate cancer fatalities or to make comparisions between doses from different sources. For typical indoor conditions, it is found that an indoor radon concentration of 1 pCi/L corresponds to an effective dose equivalent of about 200 mrem per year, with a probable error of roughly a factor of two. The average radon level of 1.5 pCi/L thus corresponds to an average effective dose equivalent of about 300 mrem per year. The calculated number of deaths for this mean dose, again assuming linearity, is consistent with the results of extrapolations from the experience of miners.

• *By comparing the magnitude of the radiation dose from radon to that from other sources.* Effective dose equivalents from a variety of sources are listed in Table 1. It is seen that the mean dose from radon exceeds those from all other natural sources, exceeds those from medical procedures, and is very much greater than the exposures received to date by the public from nuclear power operations in the U.S., including the Three Mile Island accident.

Table 1. Comparisons between radon and other sources, in terms of approximate annual effective dose equivalents. The radon dosees are calculated using the approximate equivalence: 1pCi/L —> 200 mrem per year

Source or limit	Dose(mrem/yr)[1]
RADIATION LEVELS:	
Average within 50 miles of US reactors (1981)	0.002
Three Mile Island: average within 50 miles[2]	1
Hanford reservation, maxi mum off-site dose (1985)	3
Chernobyl: 74 million in W.USSR, 50-yr mean	16
Three Mile Island: maximum off-site dose[2]	70
Natural sources, excluding radon (average)	100
Medical exposures (rough U.S. average)	100
Chernobyl: W. USSR, first year mean	200
Radon: mean for single- family homes[3]	300
Radon: upper 2% of U.S. homes[3]	1600
Radon: extreme reported U.S. case[3]	800,000
Chernobyl: workers (maximum) PROMPT	>1,000,000
REGULATORY OR ADVISORY LIMITS:	
Nuclear reactor, maximum off-site (EPA)	25
Nuclear waste repository, maximum off-site (EPA)	25
General limit on average population exposure	170
Radon: action within several years (EPA)[3]	800
Radon: recommended action level (NCRP)[3]	2000
Radon: action within several months (EPA)[3]	4000

1. Supporting references will be sent on request.
2. These doses were received over times periods << 1 year.
3. Radon doses uncertain by about a factor of two.

A major difficulty in estimating the effects of radiation exposures comes from the need to extrapolate to relatively low dose levels. The calculated deaths in the discussions above are based on the linearity assumption. The validity of the linearity assumption is uncertain. After considerable internal controversy, the NAS/NRC Committee on the Biological Effects of Ionizing Radiation, in its 1980 BEIR III Report[7], recommended using the linearity assumption for densely ionizing radiations, such as alpha particles, but concluded that the linearity assumption probably overestimates the low dose effects for sparsely ionizing radiation, such as beta particles and gamma rays. One of the most important fruits of further studies of radon epidemiology may be a better understanding of the linearity issue. If linearity fails for alpha particles, the calculated deaths for radon and from other radiation sources may be substantially overestimated.

Whatever the ultimate resolution of this uncertainty, it is hard to find a rational basis for the regulatory policies which have evolved. For indoor radon, the National Council on Radiation Protection and Measurements recommends that remedial action be taken under conditions corresponding to a radon concentration of about 10 pCi/L. The EPA has a desired target of 4 pCi/L but does not recommend prompt action at levels below 20 pCi/L. The doses corresponding to these recommendations are very much higher than the maximum off-site doeses allowed for nuclear reactors and future nuclear repositories, as seen in Table 1. In a similar vein, under its regulations for future waste disposal sites the EPA projects that the repository will "cause no more than 1000 deaths over the entire 10,000 year period...an average of 0.1 fatality per year."[8] At the lowest recommended action level for radon, several hundred thousand times as many deaths per year are predicted.

Major nuclear accidents stand in a separate category. There are no counterparts with radon to the prompt exposures of personnel at Chernobyl, which caused several hundred cases of radiation sickness and contributed to 31 deaths, or counterparts to the forced evacuation of 130,000 people, the widespread destruction of crops, and the fear engendered. However, a comparison can be made to the estimated average exposures over the next 50 years for 74 million people in the Western USSR. Their mean exposure is estimated to be about 200 mrem for the first year and to average about 16 mrem per year over 50 years.[9] These doses correspond to about 10,000 calculated cancer deaths due to Chernobyl. The median dose rate for the victims (in the "responsible" year) is well below 300 mrem per year. Thus the expected cancer deaths at Chernobyl arise primarily from dose levels less than the average from indoor radon in the U.S.

Why do we attach great significance to the projected toll from relatively low doses if the exposure comes from nuclear reactor accidents, while we essentially ignore radon deaths predicted at similar exposure levels? Some of this inconsistency, and the much greater inconsistencies which arise when other aspects of nuclear power are considered, may be due to an ambivalence in our tacit assumptions about the effects of low level radiation — in effect our "gut-level" belief or disbelief in the existence of a "safe" level. With the actual effects not known, many impulses favor assuming the worst for nuclear power and the best for radon. To the extent that there is any basis for a scientific distinction this is backwards, because radon exposures are primarily from densely ionizing alpha particles while nuclear power exposures (especially accidents) are primarily from beta particles and gammas rays. But factors of psychology and practicality favor the inconsistencies.

For one, it is relatively easy to mandate and achieve "clean" nuclear power (Chernobyl aside), but it is difficult to identify and clean up all high-radon houses and virtually impossible to address the several hundred mrem per year exposures in the average house. Further, nuclear power is an outgrowth of human activity while radon is "natural." However, the distinction between man-caused and natural radiation is blurred. House weatherization, undertaken for energy conservation, has been authoritatively estimated to increase radon levels on average by about 10% to 20%,[2] corresponding to an average dose increase (about 45 mrem per year) which is well above the maximum permitted at the perimeter of a nuclear power plant or waste repository. This is *not* an argument against energy conservation in homes. Apart from other merits, the intensive studies of building design and performance that have been carried out in the interests of greater energy efficiency can show ways to reduce the levels of indoor radon and other pollutants. However, the comparison with weatherization illustrates the selectivity of the forces which initiate concern.

In summary, radon highlights an outrageous array of inconsistencies in our responses to radiation exposures. If radon doses are taken as any sort of yardstick, comparing either to average levels or suggested action levels, then it is clear that fears about radiation from nuclear power are out of all proportion. Establishing some modicum of consistency is not just an intellectual nicety. It could improve our allocation of resources for radiation protection and our evaluation of nuclear power hazards. At present, the fear of "nuclear" radiation renders moot any serious discussion of the potential contribution of nuclear power towards relieving the risks of oil dependency and the dangers of a carbon dioxide buildup. This is a severe cost to pay for fears which often exist isolated from any scale of comparison—even to other radiation dangers.

It is hard to remove or even moderate deeply felt fears, especially when they involve unfamiliar technical considerations. However, the considerations are not particularly subtle and an informed physics community could contribute toward their clarification. A more balanced perspective will not convince even all physicists that nuclear power is "safe," but it could place nuclear power hazards more sensibly in the spectrum of contemporary hazards.

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* Many of these issues are covered in greater length in *Indoor Radon and Its Hazards*, D. Bodansky, M.A. Robkin, and D.R. Stadler, editors, University of Washington Press, Seattle (1987).

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COLLATERAL DAMAGE PRODUCED BY SUCCESSFUL DEFENSES AGAINST ICBMs: SDI DOESN'T SOLVE THE PROBLEM

Peter D. Zimmerman,*Senior Associate,
Carnegie Endowment for International Peace, and
Ruth H. Howes, Ball State University

When an American interceptor hits its target in the animated cartoons used to illustrated SDI on television, the Soviet missile vanishes in a harmless puff. In the real world, missiles do not vanish and warheads do not disappear.

The warhead of a strategic ballistic missile is a relatively small and very dense object, containing almost no empty space. It is capable of withstanding the extreme temperatures caused by re-entry into the earth's atmosphere, as well as the shocks of acceleration and vibration which are inseparable from rocket-propelled flight. Most re-entry vehicles (RVs) are aerodynamically stable so that they will point nose first when passing through the air; even if they re-enter in the wrong orientation at high altitude, they may survive. Although packed with high explosive to detonate the nuclear components, a modern warhead is nearly as inert as the same mass of lead until the specific conditions which trigger its blast are present or until it is struck directly by a projectile with relative speed comparable to that of two bullets head on. The single word which best describes a modern reentry vehicle is "rugged."

Boost-phase Intercept

We have one very graphic illustration of the fate of the components of a large rocket following a catastrophic explosion: the loss of the Space Shuttle Challenger. The shuttle itself was destroyed when the many tons of hydrogen and oxygen propellant in the external tank detonated. The first step in the accident occurred when the pressure seal at the aft field joint of the right side solid rocket booster (SRB) failed.[1] Photographs of the accident published in the Rogers Commission Report clearly show that several seconds after the explosion of the external propellant tankage the right SRB was not only intact, but under power.[2] The crew compartment of the spaceship remained intact until it finally struck the ocean although the Challenger's fuselage was far less sturdy than an RV.

The nose assembly (frustrum) of the right SRB was recovered from the ocean nearly intact, with only impact damage at its top and burns along the base.[3] The frustrum of the SRB corresponds most nearly to the bus containing the warheads of a ballistic missile. Our conclusion is that it is likely that such a warhead bus would survive the destruction of the booster which carried it, particularly since interceptors are more likely to home in on the rocket nozzle, the farthest point fom the bus.

If the bus is likely to survive destruction of its booster, so is its cargo. The question of where the surviving RVs will land thus becomes critical. If they land on the Soviet Union, there is little problem, at least for the United States. But if the RVs have the velocity to reach either North America or the territory of innocent nations elsewhere, enormous destruction might well be visited upon unintended targets. By making relatively straight-forward calculations, it is possible to estimate the dimensions of the problem.

We modelled the problem by considering the launch of missiles from the Uzhur SS-18 base located near the city of Krasnoyarsk against targets at the port and petrochemical complex at New Orleans, Louisiana. The trajectory of such a missile goes very nearly over the North Pole (New Orleans is at 90° west longitude, while Uzhur is at 89°50' east longitude), and is about 10,600 km long. We calculated the flight trajectories as if the acceleration were imparted impulsively, with the total impulse for a full range flight that needed for a minimum energy trajectory, I_{min}. For those flights where the missile was "intercepted" during boost, the impulse given the payload was taken to be $I_{min} \cdot f$ where f= (intercept time/normal burn time). This technique slightly underestimates the actual range of a missile intercepted during its boost phase.

The equations of motion were integrated using a fourth-order Runge-Kutta routine at 30-second intervals. As the warhead descended below 200 km, the integration interval was decreased to 10 seconds. The end-of-range point was determined by interpolating between the last calculated point with a positive altitude and the first with a negative one. Table 1 shows the time left in the powered portion of the missile flight at which time the ICBM is hit together with a map location reasonably close (10-50km) to the predicted impact point of the warheads. Results are shown for an SS-24/MX class solid propellant missile with a burn time of 180 seconds; similar results were obtained for an SS-18 trajectory and a hypothetical fast-burn booster. The model presented here treats one specific trajectory with many simplifying assumptions, so exact points of impact should not be taken too seriously. For example, the photograph of the SRBs leaving the Challenger explosion clearly shows that both rockets are curving away from their expected flight path. Targets hundreds of kilometers east or west of the planned flight path would also be placed at risk.

Table 1. Warhead impact point for SS-24/MX Class missiles (normal boost duration = 180 sec) intercepted during boost, as a function of intercept time.

Powered flight time remaining	Impact Point
1.0 sec	Jackson, MS
2.0	Senatobia, MS
3.0	Wappapello, MO
4.0	Medora, IL
5.0	Kewanee, IL
6.0	Reedsburg, WI
7.0	Tripoli, WI
8.0	N. Coast, Lake Superior
9.0	Holinshead Lake, Ont.
10	Pickle Crow, Ont.
15	in Hudson's Bay
20	Pelly Bay, NWT

It is very unlikely that lasers and particle beams will play any role in a deployment of strategic defenses this century.[4] Most supporters of immediate deployment advocate using space-based kinetic kill vehicles to attack Soviet boosters. Kinetic kill vehicles will be limited to a maximum "fly-out" speed of about 10 km/sec and will orbit 500 km or more above the earth in order to have a reasonable chance of surviving a defense suppression attack. If a KKV should be launched by near-perfect sensors with launchers in the right positions and a command and control system that launches interceptors the instant an ICBM leaves its silo, it is barely possible that the interceptor might reach an SS-24 by the one-hundred-sixteenth second of flight. Then the warheads of the SS-24 would fall on the Soviet Union. A hit only 30 seconds later would allow warheads to reach northern Canada. The battle management algorithm will necessarily give the most attention to the highest altitude and highest velocity missiles. Consequently, we expect late intercepts to be the rule, not the exception.

One important effect of boost-phase intercept may be to convert a well-planned attack on a few military targets into a haphazard attack which might destroy population centers which the aggressor had intended to leave unscathed or those located in a non-combatant nation. Warheads continuing in flight after the destruction of their boosters will be travelling on depressed trajectories, well below those altitudes at which mid-course defenses may be focussed. As a result, those warheads may elude detection and intercept, thus getting a free ride to their targets.

The results of successful midcourse defenses

Midcourse defenses strike RVs outside the atmosphere. Since interceptors carry little momentum, the warhead, or the debris from it, continues approximately on its intended course until it encounters the earth's atmosphere. Warheads are now well clear of the Soveit Union and may well be fused to explode in space as they are attacked, producing the raadioactive debris characteristic of nuclear explosions. The major biological hazard from high altitude explosions comes from the plutonium used in manufacturing the nuclear warheads and the radioactive Sr-90 and Cs-137 produced when they explode. For example, detonation of a hundred 500-kiloton warheads would increase the amount of radioactive strontium in the stratosphere to twice its highest level just before the Limited Test Ban Treaty went into effect.

If the warhead is fragmented by the interceptor or by detonation of the chemical explosive, debris will probably burn on reentry and be distributed as fine particles. Fine particles high in the stratosphere spread rapidly between the northern and southern hemisphere and circle the globe. A year after the particles enter the stratosphere, they begin to appear in rain that falls on temperate latitudes in both hemispheres. We know that debris from conventional explosions high in the atmosphere and from space objects that burn up during reentry behave this way. For example, the plutonium from a SNAP 9A reactor that was destroyed in a launch failure in 1964 appeared in preciptation in both hemispheres beginning a year after the disaster.[5] Plutonium from warheads used in reasonable models of Soviet attacks would cause a significant increase in global background levels which in turn could slightly increase worldwide rates of cancer and genetic defects.

The side-effects of terminal defenses

Terminal defenses attack warheads as they reenter the atmosphere above the territory of the United States and Canada. Terminal defenses planned by SDI consist of the Exoatmospheric Reentry-vehicle Interceptor Subsystem (ERIS) which will strike at altitudes of more than 100 km, and the High Endoatmospheric Defense Interceptor (HEDI) which is designed to attack warheads well inside the atmosphere.

If warheads are salvage-fuzed to explode on interception high in the atmosphere, there will be two immediate effects on the earth's surface.[6] High altitude nuclear explosions produce enormous fireballs that can damage the eyes of people and animals looking at the burst. A 500 kiloton warhead detonated at an altitude of 15 kilometers can cause retinal burns 40 miles from the burst on a clear day and 90 miles away at night. Higher altitude or larger nuclear explosions produce eye damage over greater distances. The detonation will also produce a powerful pulse of electromagnetic energy (the EMP) which can disable electronic equipment including computers, radio transmitters, telephone lines and, in an extreme case, the national power grid.

Since the heat of the nuclear fireball can overload ground and space-based infrared tracking systems, a high altitude nuclear explosion may blind the terminal defenses for tens of critical seconds while other warheads reach their targets. Satellites over the horizon from nuclear bursts have been disabled by the Argus effect, the charged particles produced by the burst and trapped in the earth's magnetic field.[7] In addition to radioactive fallout, high altitude nuclear explosions also produce large quantities of nitrogen oxides which catalyze the destruction of ozone molecules. The layer of ozone which protects the earth's surface from ultraviolet radiation lies between 16 and 32 kilometers. The ozone layer will reconstruct itself but only years after the damage has been done.

It is likely that warheads whose electronic fuzes have been detroyed by defenses will undergo chemical explosion when they strike the ground. In two bomber crashes during the sixties, warheads that were not armed experienced chemical explosions and scattered debris over areas half a mile square.[8] Either the fall of large chunks or the chemical explosion of warheads will create local "hot spots" contaminated with plutonium which will have to be cleaned up before people can live and work in them. Because plutonium burns readily, it is likely to be distributed as fine particles. In 1983, a DOE test used a chemical explosion to destroy a mock radioiosotope thermoelectric generator used to produce power for space vehicles. Debris from the explosion was so finely divided that only 70% of the original mass could be recovered.[9] Thus clean up of the "hot spots" may be impossible; it will at least be expensive. In the worst imaginable case, terminal defenses will not damage the reentering warheads but will change the angle at which they enter the atmosphere so that they detonate as they were supposed to but up to 250 kilometers from their intended targets. Areas near targets attacked by SLBMs will be at greatest risk fom warheads diverted by terminal defenses since the lower trajectories of SLBMs make them more difficult to attack in the boost and midcourse phases of their flight.

Summary

At best, defenses that are very effective against nuclear warheads carried by strategic ballistic missiles cannot protect the population of the United States or the rest of the world from the effects of a nuclear war. If one choses an optimistic scenario, the damage done to a defended U.S. would be slight compared to the damage done to an undefended country. In a pessimistic scenario, the effects of defenses would be to divert nuclear explosions from their intended military targets to random locations in the United

States and other non-combatant countries. A defense that simply deflects warheads from designated military targets toward civilian ones would turn any attack against purely military targets into a potential attack on population centers. Civil defense based on evacuation of population would be particularly ineffective in such a scenario since the deflected warheads might strike the evacuation area instead of their intended targets.

*PZ wishes to thank Amb. Robert Buchheim, who also raised the question we have tried to answer, for an interesting and useful conversation.

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REVIEW

THE MAKING OF THE ATOMIC BOMB by Richard Rhodes (Simon & Schuster, New York, N.Y., 1986)

In the nineteen thirties, scientific curiousity was impelling physicists toward the discovery of atomic fission at the same time that Hitler's fanaticism was propelling Europe toward world war. The fateful confluence fundamentally changed both science and the world. Although the story has been related many times in memoirs, biographies and histories, it has rarely been told with the scope and drama that Rhodes has accomplished in his new book. He has deftly woven together the progress of scientific discoveries with the interplay of personalities and the impact of historical events.

The first nine of the nineteen chapters chronicle the rapid succession of revelations about the nature of the atom that culminated in the discovery of fission. To follow these events chronologically, Rhodes darts from Cambridge to Copenhagen, and from Berlin to Rome, often departing from the science to introduce personally the wide cast of players—Rutherford, Chadwick, Bohr, Heisenberg, Hahn, Meitner, the Joliet-Curies, Fermi, etc. The sudden shifts make the narrative a bit choppy and at times confuse the reader. But they also reflect the rapid pace at which nuclear physics was then advancing, and given momentum to the story.

The scientific narrative is interrupted periodically — as was the scientific work itself — by the rise of the Nazis and the coming of war. Rhodes describes the creative efforts of Szilard, Bohr, Franck and many others to find opportunities outside Germany for as many of their Jewish colleagues as possible. Suddenly the physicists who had worked together as members of a single community were divided into rival teams and began to apply their science to national mandates.

The book loses some of its pace as the subject matter shifts in the next eight chapters from the quest for atomic truths to the drive for the atomic bomb. The work now involves less fundamental science and more practical engineering, although the problems are no less formidable, and the solutions — such as the idea of the implosion bomb — no less creative. Another difference is that the weapons work, and physics itself, changes scale from small to large, from intimate to institutional. Under the forceful direction of General Groves, enormous complexes and instant towns arise in desolate areas of Washington, Tennessee and New Mexico.

Bohr noted the monumental scale of the effort during his first visit to Los Alamos in 1943. Bohr had told Edward Teller in 1939 how skeptical he was that U235 could be separated from U238. When Bohr met Teller again at Los Alamos, Teller was about to claim, "I told you so," but Bohr beat him to the punch. Bohr said, "You see, I told you it could not be done without turning the whole country into a factory. You have done just that."

Rhodes treats not only the making of the bomb but the thinking about the bomb as well. We hear much about Leo Szilard, the Hungarian physicist, whose quest was once to save the world through science. Usually one step ahead of his times, Szilard was the first to recognize the potential impact of fission and among the first to recoil from it in horror. Helped by such fellow emigres as Wigner, Einstein, Teller and Fermi, Szilard relentlessly tried to persuade the US government to support work to demonstrate a nuclear chain reaction. Yet on the day that Fermi's pile in Chicago went critical, he remarked that "this day would go down as a black day in the history of mankind."

Rhodes also dwells at length on Bohr's thoughts about the bomb, which he expresses as an extension of the idea of complementarity. Bohr first introduced that term in 1927 to explain the duality between the wave and particle nature of light and matter. With respect to the bomb complementarity expressed the idea that (in Rhodes' words) "The bomb was opportunity and threat and would always be opportunity and threat — that was the peculiar paradoxical hopefulness." Bohr hoped that the bomb would "offer quite unique opportunities to bridge international divergencies" because nations would have to cooperate to forestall its threat to civilization. Yet when Bohr tried to warn the leaders of the US and Great Britain of the pitfalls of an arms race and to counsel that it might be avoided by openness, he only aroused their suspicions of his intents.

The last two chapters deal with the test of the atomic bomb and its actual use on two Japanese cities. By this time, Rhodes has carefully laid the background for understanding all the factors, events and personal viewpoints that influenced Truman's final decision to drop the bomb on Hiroshima. Among those factors were the enormous investment in the development of the bomb, the aerial bombing campaign, which by the end of the war had inured the Allies to widescale destruction of enemy civilian targets, and the suicidally stubborn resistance of the Japanese, which caused American planners to predict heavy casualties for an American invasion of the Japanese mainland. The epilogue to the book discusses the development of the hydrogen bomb.

Rhodes has done an enormous amount of research to pull together in these 800 pages so much information about this era. He conveys well the science involved. And he enlivens his book by numerous anecdotes and rich detail concerning personalities and settings. We imagine Enrico Fermi racing down a hallway of his Rome lab, carrying a short-lived sample from the irradiation room to the counting room. We envision a log on a snowy hillside about the Swedish village of Kungalv where Lise Meitner and Otto Frisch sat one Christmas eve to draw sketches of fissioning nucleii. And we walk with Fermi and Teller along New York streets in 1941 while Teller explains to Fermi why the latter's idea for a hydrogen bomb could never work, a conclusion Teller was later to overturn. It is an eminently readable book about an exceptionally important period of time.

Barbara G. Levi, Center for Energy and Environmental Studies, Princeton University.

NEWS

MINUTES OF THE FORUM EXECUTIVE COMMITTEE MEETING, 21 April, 1987

The meeting was called to order at 8:30 AM by Paul Craig, Chair.

The minutes of the previous executive committee meeting, 4/29/86 were approved as distributed to the members.

Chair Paul Craig reported to the committee: (1) The activities of the Forum are looked upon favorably by senior members of the Society administration. The Newsletter changes appear to have been very successful, both as regards the start of the creation of a needed outlet for publication and as a vehicle for the Forum to communicate with the rest of the Society. (2) It is necessary to stress at Forum-organized sessions that the Forum is the organizer—this is good for us, and might bring in more members. (3) The awards program is in good shape, the Forum and Szilard awards now having been made awards of the entire Society. In all significant regards the Forum is a full-fledged "Division" of the Society. (4) Paul thanked the Executive Committee for its help and support during his term in office.

The Secretary/Treasurer presented the financial statement for the last 12 months which indicated essentially no net inflow or outflow of funds. The Forum is financially stable, given the assumption that existing APS subsidies for the newsletter will continue. We have a significant operating surplus which can be used for special projects and studies.

Dietrich Schroeer (Vice-Chair) reported on the Panel on Public Affairs (POPA) activities. POPA not surprisingly has concentrated its efforts on the Directed Energy Study. Attempts to make a coordinated release of the Study at the April meeting had "gone up in smoke" as of the moment the Forum Executive Committee meeting began, however the SDIO's clearance of the report came just in time to permit an effective release to be made. POPA has, at present, an interest in studying physics manpower and manpower shortages. The choice and initiation of any new studies will have to wait for completion of the DEW study. As to Forum-originated studies — we are free to propose and develop an idea, but if POPA sponsors it, the Forum will lose control; Forum members of the original study may or may not participate in the POPA study.

Kenneth Ford, Divisional Member of the APS Council reported on Council actions affecting the Forum. The Council plans to release a statement on SDI but will hold it until the DEW Study is out. The question of the number of Divisions/Forum/Topical Groups to which a member of the APS may belong without charge is still under discussion. [Secy's note: APS members may presently belong to any or all subunits of the APS at no charge; each subunit receives a "dues equivalent" amount of money for each member it has ($2.00). Since the average number of subunits to which members belong is now significantly above one, this is "costing" the APS a significant amount of money.] The question of Freedom of Information seems to be easing since the Poindexter Memorandum governing, for example, data bases, has been withdrawn. The American Institute of Physics will sponsor its own Congressional Fellowship in addition to the two which the APS supports.

Art Hobson, editor of *Physics and Society* reported: The newsletter is now printed at the APS. He would very much like to have more technical articles of high quality and book reviews as well. Charges for libraries receiving copies do not seem in order; since the APS does not wish us to charge subscription fees for non-members of the Forum, the question of accepting contributions to cover our explicit handling charges arises. Discussion followed centered particularly on the question of whether it was appropriate to distribute the newsletter beyond the Forum membership. There was no resolution of the matter.

Studies: There was general discussion about the value of Forum-sponsored studies such as the one on civil defense and the on-going one on the future of land-based strategic missiles. Barbara Levi reported that it was easy to find government and independent speakers to make presentations at meetings of study groups. She requested that the land-based missile group be prepared to report at a Symposium in connection with the April, 1987 APS meeting. Ruth

Howes inquired as to the value of Forum (specifically) studies. Paul Craig responded that they were done by "amateurs" for their own education and for the education of the APS and by "professionals" to influence policy. Peter Zimmerman reponded that most who participated in the studies were not amateurs and suggested that a good study could well be sent to Council for establishment of a review committee and subsequent publication.

Fellowships: It was suggested by Fellowship Committee Chair Ruth Howes that members of the Executive Committee, and particularly the Chair and Vice-Chair, not be nominated for APS Fellowship during their terms of office. This was approved by consensus. Ruth reported the names of those nominated by the Forum to the Society for Fellowship.

Awards: Ruth Howes, incoming Awards Committee Chair, will write an announcement for *Physics and Society*. Peter Zimmerman moved that the Forum explore the possibility that we could solicit contributions for the awards funds through *Physics and Society*. The form of the award certificates was discussed. Despite the increased cost it was decided to revert to the hand-calligraphed certificates used previously and which are currently used by all other awards of the APS. It was also agreed to re-do the 1987 certificates; Ken Ford will discuss that in New York.

New business: Art Hobson asked the Forum to try to protect the policy of free membership in subunits of the APS. Dietrich Schroeer moved that the Forum Executive Committee go on record as "encouraging the maintenance of open enrollment in all divisions and the Forum without charge." Should a limitation on the number of free enrollments be imposed, Schroeer's motion continued, "that number should be no less than three." The motion carried unanimously. Peter Zimmerman moved that Paul Craig and Dietrich Schroeer be commended by the Executive Committee and thanked for their service during the last year. Carried unanimously.

The meeting was adjourned moments before the start of the Forum Contributed Papers session that afternoon.

COMING FORUM SESSIONS

Division of Plasma Physics Meeting, San Diego, 2-6 November 1987

1. ASSESSMENT OF DIRECTED ENERGY WEAPONS, Monday evening, 2 November: Richard Freeman, AT&T Bell Labs, presiding.
   - Kumar Patel, AT&T Bell Labs, Co-chairperson of APS Directed Energy Weapons Study
   - Louis Marquet, Deputy Director for Technology, SDI Organization, Department of Defense
2. STUDIES OF MAGNETIC CONFINEMENT FUSION RESEARCH Tuesday evening, 3 November: Stephen O. Dean, Fusion Power Associates, presiding
   - Gerald L. Epstein, Congressional Office of Technology Assessment, "OTA's report on magnetic fusion research and development"
   - John M. Richardson, National Academy of Sciences - National Research Council, "Outlook for fusion hybrid and tritium-breeding fusion reactors"
   - James G. Crocker, Idaho National Engineering Laboratory/ EG&G Idaho, Inc. "ESECOM I: environmental, safety and economic comparison of fusion systems"
   - Mujid Kazimi, Massachusetts Institute of Technology, "ESECOM II: comparison of fusion and fission power reactor systems"

CONFERENCE ON TECHNOLOGY AND SOCIETY

The 1987 Carnahan Conference on Harmonizing Technology with Society will be held October 1-2, 1987 in Lexington, Kentucky. The basic theme of the conference is to explore how technology can be more responsive to the needs of mankind and how technology can help humans become free and responsible members of a worthy society. It is co-sponsored by the IEEE Social Implications of Technology Society and the University of Kentucky College of Engineering. For program details write: John Jackson, Conference Director, Electrical Engineering Department, University of Kentucky, Lexington, Kentucky 40506-0046, 606/257-3926

ANDREW GEMANT AWARD: CALL FOR NOMINATIONS

The Andrew Gemant Award of the American Institute of Physics recognizes the accomplishments of a person who has made significant contributions to the understanding of the relationship of physics to its surrounding culture and to the communication of that understanding. The selection committee invites nominees for the 1988 award. Send nominations with supporting material to: John S. Rigden, Director of Physics Programs, American Institute of Physics, New York, NY 10017. Deadline for receipt of nominations is 31 Dec 1987.

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ADDRESS____________________

COMMENT

CONNECTING SCIENCE AND FEDERAL POLICY: CONGRESSIONAL SCIENCE FELLOWS

[Editors note: This article continues our series by past APS Congressional Science Fellows.]

The Congressional Science Fellowship program of the American Physical Society has existed now for nearly fifteen years. Each year, the APS selects one or two physicists who have an interest in policy to work for the U.S. Congress, as a staffer for a Congressman or Senator or for a congressional committee. Initiated in coordination with similar programs sponsored by the AAAS, ASME, and IEEE, the program has since been extended to many professional societies representing virtually the whole of mathematical, physical, biological and social sciences.

Over the course of its decade-and-a-half existence this effort has allowed over 300 scientists to be exposed to the legislative community and vice-versa. Many fellows have remained in Washington after the end of their year to continue work at the science/policy interface. Others have returned to their previous positions in academia, at national laboratories, or in the private sector. Some who remained in government service have become quite influential: ex-fellows include senior staffers on major congressional committees that deal with science issues, a former deputy director of the National Security Council, and a member of the Nuclear Regulatory Commission.

Recently, several of us ex-fellows have realized that this body of people is a potentially important resource, both for the U.S. scientific community and for the nation. This resource is not now being formally utilized, except in that several of the societies, including the APS, have begun asking ex-fellows to participate in society affairs that deal with policy matters. Further, some ex-fellows are now investigating ways of structuring an informal network or "Alumni Association".

The very existence of the fellowship program has opened links between the scientific community and Congress that didn't really exist before. These connections have become increasingly important, partly as a consequence of the growing effort Congress must devote to scientific and technical questions in fields such as health, bioengineering, environment, energy, defense, arms control, and science policy. In 1970, there were virtually no scientists or engineers among congressional staff. In fact, some Congressmen (presumably the less brilliant among them) were hostile to the idea of including "eggheads" on their staffs. This attitude has shifted significantly, in no small part because of the science fellow experience; now, scientists and engineers have a significant representation among congressional staffers.

One might consider how a network of former fellows might be employed to serve both the scientific community and the nation as a whole. A principal goal would be to improve communications between the scientific and governmental communities.

First, regarding professional societies, many of them have had a tendency to forget about their fellows after their terms expire. This is a mistake: the political experience and information that fellows gain could be important to the societies. An on-going network might provide an opportunity to tap this source of information. Another possibility would be for each society to make stronger efforts to draw on the expertise of ex-fellows.

Second, a network could form a natural, informal channel, on a personal level, for communicating to Congress the general science and policy concerns of the scientific community. Interaction between ex- and current fellows would help in this context.

Third, a network could lead to a more efficient interaction between researchers and relevant congressional committees. This would be useful, for example, in dealing with general research funding decisions. I do not mean to imply that a fellows' network should be used to lobby for funding individual projects. This would be inappropriate and, probably, self-defeating. The network might, however, be profitably employed to open communications directly between legislative staff and researchers to facilitate a broad understanding of scientific issues among staffers. Equally important, such communications could give scientists a better perspective on which research directions are perceived by policy-makers as most important for the nation, why they are so perceived, and, above all, what political and fiscal limitations might constrain federal support for science research. I feel this last point is particularly important since researchers sometimes seem out of touch with political reality.

A number of issues are now surfacing which will become increasingly important to the physics community:

- the effect of the large increase in the proportion of federally-sponsored scientific research devoted to military applica tions on current and future generations of physicists;
- the optimal mix getween "large" science and "small" science;
- the future of federally-funded civilian scientific research in the face of increasing fiscal contraints;
- the lack of adequate science and mathematics instruction in secondary schools;
- the need for an adequate science and technology base to allow the U.S. to remain economically viable in the face of aggressive foreign competition.

To help resolve these problems, the U.S. scientific community must learn to successfully make its case to Congress. Ivory tower attitudes of many scientists are fortunately being eroded, but further erosion is needed. A network of ex-fellows could help, but more vital would be the participation of as many scientists as possible in the body politic. Each individual can contribute by becoming more aware of what is going on legislatively and by developing whatever personal links with the legislative process she/he feels most confortable with. Such links could proceed via one's own professional societies (e.g., through the APS Panel on Public Affairs and the Forum on Physics and Society), through published articles in lay and semi-lay journals (including this one), through correspondence with members of Congress, and even through personal lobbying in Washington.

An early U. S. physicist, B. Franklin, once made a remark which appears appropriate, although it was made in a different context: "We must indeed all hang together, or, most assuredly, we will al hang separately."

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THE SUPERCONDUCTOR FOLLIES:

[This article originally appeard in the *The Washington Post*, 2 August 1987. The author is director of the Washington office of the American Physical Society.]

Ronald Reagan personally addressed 2,000 businessmen, engineers and scientists who gathered in Washington last week to discuss commercial applications of a newly-discovered class of superconductors. These are materials that lose all resistance to the flow of electrical current when cooled below a "critical temperature."

The president's presence symbolized the government's determination to slug it out with the Japanese in the commercialization of this dazzling new technology. That's good. But the president began with a line he has used before: "I have to confess that I am one of those people who, when the government offers to help, get very nervous."

I had exactly the same reaction when he described the "help" the administration had in mind. There was much to praise in the speech, but three things troubled me. It ignored the international nature of superconductor development. It proposed to funnel much of the government's research effort through the Department of Defense. And it raised the specter of increased government secrecy.

First of all, superconductor research has been an international effort from its very inception. It is a modern parable of the progress that can be made in the absence of nationalistic barriers. A new superconducting material was discovered by scientists in Zurich a little over a year ago. Unlike previously known superconductors, which are metals, it was ceramic—and its critical temperature was the highest ever seen. The Swiss team published the full details of their work in an international journal. Among those who read their paper were scientists in Tokyo and Beijing. Within a few months, both groups had confirmed the Zurich results. The race was on.

The next big breakthrough came a few weeks later form the Unviersity of Houston. A group of Chinese and Chinese-American scientists in Houston discovered a change in composition that raised the critical temperature higher still. People began to talk seriously about the prospect of superconductivity at room temperature. The commercial and scientific implications are staggering. The critical termperature continues to rise as important discoveries come from Berkeley, Paris, Karlsruhe and dozens of other sites around the world.

Even before the president spoke, however, a tiny cloud had already appeared over this scene of international cooperation. The president's science adviser made the decision to exclude foreign officials from the Washington meeting—though the foreign press was allowed to cover it in detail.

The gloom deepened when the president announced that DOD would lead a $150-million R&D effort in superconductors. It is no secret that this conference was motivated by the fear that the Japanese would get to the marketplace first with commercial applications. But it is thoroughly wrong-headed to rely on DOD to counter that threat.

The Japanese, after all, have no military reserach or development worth mentioning—and yet they've been eating our lunch in high-technology sales. And what about the Soviet Union, our No. 1 military competitor? The "evil empire" is a pussycat in the world's high-tech markets. There is simply no correlation to be found between a nation's spending on military research and its strength in private-sector markets.

Moreover, the president's own Commission on Industrial Competitiveness in its 1985 report concluded that DOD is a "net consumer" of new technologies. What they're saying is that military development relies on spin-offs from civilian research—and not the other way around! Our high-tech edge over the Soviets, the real basis of our national security, stems from the creative energies of IBM, Bell Labs, Texas Instruments and scores of other companies striving to make a profit in a ruthlessly competitive private-sector economy—not from coddled government contractors with their cost overruns and $600 toilet seats.

Finally, military developments, even if they have the potential to benefit the private sector, are necessarily cloaked in secrecy, delaying their use for civilian purposes beyond the period in which they might confer some advantage.

Yet the president wants more secrecy. "We must also move to protect intellectual property," Reagan said last Tuesday, "and write protections into the Freedom of Information Act for scientific and technical information generated by government laboratories." Government secrecy could strangle this infant technology in its crib.

Progress in this field has been rapid because nothing has been held back. Scientists are not even waiting for the slow pace of publication in professional journals. Long-distance phone lines around the world buzz with the results of dozens of new experiments. Express-mail packets carry samples of new materials from one laboratory to another. Foreign graudate students traveling to and fron the United States are pressed into service as couriers.

In fact, only one segment of the industrialized world seems to have been left at the starting blocks. The Soviet Union and its Warsaw Pact allies have played almost no part in what has become the most exciting scientific quest in decades. No one sought to exclude them. They are simply weighted down with travel restrictions and bureaucratic restraints on contact with foreigners and suspicion even of telephones and copying machines. Soviet scientists, who are the equal of any in the world, cannot compete in this sort of foot race.

By all means, let's take on the Japanese. There is plenty the government can do. We should, as the president proposed, reform our patent and antitrust laws to reflect the profound changes in technology and the rise of a world economy. The president reiterated his proposal to double the budget of the National Science Foundation. He might consider taking the funds from the bloated R&D budget of the Defense Department which now controls about three-fourths of all federal R&D.

But let's keep the process open. We don't do secrecy very well in this country anyway—the Soviets are much better at it. And that is why they are dead last in the superconductor race.

Robert L. Park, professor of physics, University of Maryland.

FROM THE CHAIRMAN: AN APPEAL TO FORUM MEMBERS

Over the past few years the Forum on Physics and Society has achieved a legitimate and distinguished position among the divisions of the American Physical Society.

- Forum invited sessions have brought together speakers who have contributed professional expertise to controversial topics.
- A contributed-paper session at the April 1987 APS meeting highlighted the legitimacy and professional quality of the work that some members of the Forum are doing on physics-and-society topics.
- A study, *Civil Defense: A Choice of Disasters*, edited by John Dowling and Evans Harrell, has been completed under the aus pices of the Forum (available from the AIP).
- The Forum Newsletter, *Physics and Society*, has begun to include substantive articles.
- The Forum and Szilard Awards have been made into official awards of the American Physical Society; nominations will be solicited, and awardees will be announced along with those for all other APS-sponsored awards.
- The Forum has successfully nominated persons for APS fellow ship status.

The Forum is building a network among physicists who are professionally active as science-and-public-policy analysts. They publish in the field, serve as Congressional Fellows, or work as analysts at, for example, the Office of Technology Assessment, SIPRI, IISS, ACDA, or the Carnegie Endowment for International Peace. From my own personal experience it is clear that the Forum can be very helpful by supporting physicists who do such work. Recognition and approval by peers is still a most important driving force encouraging physicists to do good and important work.

The highest recognition that the Forum can offer is through its Forum and Szilard Awards. The list of past awardees reads like a physics as well as a physics-and-society *Who's Who*. The Forum will maintain the quality of those awards. But there is the other side of becoming more professional and more "regularized." These awards must not only be in line with other APS awards, but the size of the awards should be commensurate with the honor and the recognition that they confer. In a direct sense the dollar value of the awards may not seem to matter, but indirectly the size of the award symbolizes the importance of the honor. The current award level of $250 each for the Forum and Szilard Award is simply too low. This appeal is intended to begin an upgrading of the level of these awards.

The Forum is interested in any ideas for increasing the Forum Awards Endowment. What individuals and/or institutions might be interested in contributing to that Endowment, and how might the Forum approach them? Any suggestions will be appreciated. An alternative to funding by a small number of large donors is to seek contributions directly from Forum members. Historically the Forum grew from the bottom, and remains in some ways egalitarian and grass-roots oriented. Therefore it seems appropriate to try a direct approach to all members of the Forum, to ask all of you to help us build a significant endowment for these awards. To increase the Forum and Szilard Awards, I would like to initiate a fund-raising drive by appealing directly to Forum members. Our initial goal for an enlarged endowment is to provide for two annual awards of $1000 each.

I appeal for contributions from individual members of the Forum. Please send any such contributions, made out to "APS/Forum Awards Fund," to Peter Zimmerman (Treasurer of the Forum), Carnegie Endowment for International Peace, 11 Dupont Circle, N.W., Washington, D.C. 20036. The contributions are tax deductible. I will start this drive by making the first personal contribution of $30 to the Endowment. Thank you in advance for your help.

Dietrich Schroeer, Chairman of the Forum

EDITORIAL: RADIATION HAZARDS

Keep the letters and articles coming! But keep them short, please. We don't have much space. Letters should be 0-500 words. Articles should aim for 0-2000, with 2500 an absolute maximum including the word-equivalent of the space needed for tables and graphs. I will edit longer letters to bring them into line, and return longer articles for cutting. You might think of your *Physics and Society* manuscript as a summary or extended abstract of the longer article you really wanted to write.

The July issue began a series of discussions on physics-related interdisciplinary topics that could, or should, be taught to undergraduates. David Bodansky's article in this issue focuses attention on such a topic: radiation hazards.

In my large-lecture liberal-arts physics course, this topic constitutes one in a series of a dozen lectures on nuclear physics and its social implications. We define "ionizing radiation" and we discuss the ways in which it might cause cancers or mutations, using the radiostrontium-leukemia connection and Hiroshima statistics for illustration.

To put radiation hazards into perspective, we study a table similar to Bodansky's Table 1. Points emphasized include the comparison of non-natural with natural levels, the very small average contribution from operating nuclear reactors, and the surprisingly high contribution of medical exposures including dental x-rays. My table is based on BEIR and EPA data published several years ago, before the radon hazard was recognized. I now plan to add radon to my statistics.

Since Chernobyl, I have been concluding this lecture with exposure figures, and expected radioiodine- and radiocesium-induced cancers and deaths, following that accident. We emphasize sources of information, extrapolation methods, uncertainties, and the impossibility of observing the predicted cancers in the statistics of the next few decades.

This lecture never fails to stimulate questions and discussion. Two of my favorites, always asked, are "How do you know that your sources aren't lying?" and "How do you know your predictions are reliable?" We discuss some ways of guarding against disinformation and searching out fair and careful anayses. But, as the Chernobyl example illustrates, we can never really "know" about such hazards, we can at best make informed estimates. In a way, it is the theme of the scientific method: There are no completely reliable predictions. It is a humbling thought.

Obviously, radiation hazards could be better taught by a radiation physics specialist, in an entire course devoted to the topic. But few departments have such a specialist, and few students would enroll in the course anyway. In my experience, it is possible to impart significant insights and stimulate significant thought via a brief introduction in a more general course. In fact, placing such topics into the broad sweep of the ideas and implications of physics imparts a perspective that would be difficult to attain in the more specialized course, and thus this "generalist" approach has its own particular advantages.

Art Hobson