PHYSICS AND SOCIETY

Volume 17, Number 2 **April 1988**

Physics and Society is a quarterly newsletter of the Forum on Physics and Society, a division of the American Physical Society. It is distributed free to members of the Forum and to physics libraries upon request. *Physics and Society* presents letters and refereed articles on the relations of physics and the physics community to government and society. It presents news of the Forum and of the American Physical Society (335 East 45th Street, New York, NY 10017) and provides a medium for Forum members to exchange ideas. Opinions expressed are those of the author only and do not necessarily reflect the views of the APS or of the Forum. Contributions should be sent to the editor: Art Hobson, Physics Department, University of Arkansas, Fayetteville, AR 72701, (501) 575-5918. Editorial Assistant: Leonora Hermann.

The American Physical Society
335 East 45th Street
New York, New York 10017

LETTERS

SDI NEAR-TERM DEPLOYMENT

Colonel Worden (January 1988) begins by stating, "In order to persuade the Soviets to abandon their first-strike offensive warfighting approach, they must be faced with the likelihood of defenses which can deny that offensive strategy." But what about the Soviet's perception of our first-strike offensive capability? Doesn't this make any defensive deployments on our part (especially a unilateral deployment in the absence of some kind of arms-control agreement) inherently destabilitzing? And how could Worden or anyone know how the Soviets would respond if we attempted such a deployment? Does Worden interpret the current Soviet intelligence estimates of our strategic forces to preclude the possibility of an American first-strike capability?

How can we believe that the Soviets will remain in their static positions while we seek to outflank them with early unilateral deployment? Couldn't they redress by a combination of additonal force deployments and space-based countermeasures? If Worden is correct in his analysis, the Soveits could not begin to accept any strategic force reductions in the face of a threat to deploy SDI.

I am not much more comforted by Benoit Morel's (January 1988) arguments. Certainly the Soviets have a vigorous program of defensive weapons and they must be moving forward in their development of hardened, fast-burn boosters. Thus, isn't there a danger that if we devote our resources to the development and delayed deployment of DEWs that we may expose ourselves to an evolving Soviet missile force?

There is a veritable chasm separating Worden's and Morel's economic analyses which is rooted in antagonistic assessments of how effective an interim deployment must be to be acceptable. Morel states that, "...there is no basis to claim that the deployment of SBKKVs would dramatically reduce the lethality of a Soviet first strike," and that such deployment would not "enhance deterrence" but would, "...make the Soviets more nervous about the credibility of their retaliatory force."

There is thus a fundamental disagreement among the two authors of what deterrence is. In Morel's vocabulary, early deployment does not merely diminish Soviet first-strike capabilities but further reduces Soviet retaliatory capabilities. For Worden, early deployment is targeted against first-strike forces only and should not be deemed destabilitizng. This does not appear to be a realistic perception.

But what about the Soviet view? It seems to me that Morel's exposition comes closest to expressing Soviet fears of American capabilities should SDI be deployed.

Until and unless we can find some way of assuaging Soviet fears about SDI, I don't see how we can move ahead with both strategic force reductions and defensive deployments. Neither do I see how, now that Pandora's box has been opened, we can back away from SDI.

--Irving A. Lerch, NYU Medical Center

To quote Worden's article, "The US strategic analysis proceeds from an exchange calculation, " and "If both sides have similar defenses, then this analysis tends to conclude that the second-striker is in worse shape." Since US policy is based on a second-strike capability, it would seem that he is arguing against SDI.

Concerning SDI survivability: It is likely that the USSR could develop a ground-based capability to destroy space platforms. Furthermore, the exchange calculation becomes very favorable to the Soviets when interceptors are launched against the space platforms from the ground. For example, using interceptor and space platform costs from the Marshall Report, the publication of the Washington-based Marshall Institute that first suggested a near-term deployment, the exchange calculation becomes 30:1 against us. This report suggests that the interceptors would cost $1.5M each, whereas the cost of a space platform carrying 10 interceptors would be $4.5M.

The case SDI proponents make for space defense survivability, as outlined in the Marshall Report and supported in an unpublished paper by Edward Teller and Gregory Canavan, is that space platform countermeasures and penetration aides make the system survivable. In particular, they claim that the platforms may maneuver to avoid interception or they may deploy hundreds of lightweight balloon decoys to reverse the exchange calculation. According to Canavan, the intercept probability against a maneuvering, tactical aircraft is 5% based on the US experience in Vietnam. He expects the same probability for ERIS intercept of maneuvering space platforms. We are reminded of the effectiveness of Stingers against MIGs in Afganistan. It is unlikely that an IR homing missile could be outmaneuvered by the 100 times heavier space platform.

On the other hand, space platform decoys are more difficult to dismiss. The situation is similar to the use of decoys during midcourse to prevent the interception of ballistic warheads. SDI has had to resort to "interactive discrimination concepts" to solve this problem. Beam weapons such as lasers and neutral particle beams need to interact with the decoys to identify them. It is not yet known if these concepts are feasible. However, we suggest that interactive concepts will be far easier to implement using ground-based lasers and microwave radars than using space-based beam weapons. The ground-based laser already deployed in the USSR may even be adequate. The relatively small cost of ground-based weapons and sensors again is a disadvantage to space defense.

But is it not time to give up on mathematics and use some common sense? An SDI system would at most give us only a few brief years of increased deterrence because the Soviets would soon build one too. In the long run it will weaken our deterrent capability and create a much more dangerous world. In 1972, we had the opportunity to eliminate MIRVs by treaty. Is it not time to consider arms control as an alternative strategy?

--Richard Ruquist, Visiting Scientist, MIT Program in Science and Technology for International Security, Cambridge, MA 02139.

Col. Worden made the following challenge: "The astro-shield is not and never was the goal of the SDI and I defy anyone to find statemetns of responsible government officials to the contrary."

Then-Secretary of Defense Caspar Weinberger appeared at the Kennedy School of Government on 5 September 1986 to participate

in the symposium, "Beyond Deterrence: Avoiding Nuclear War in the Longer Run." As reported in the *New York Times* on 6 September 1986, and in the Spring 1987 Bulletin of the Kennedy School of Government, Secretary Weinberger gave the following response to a questioner who accused the Reagan Administration of using "bait and switch" tactics in its promotion of the SDI as a population defense: "There is only one SDI. It is exactly what he (the President) says it is...What we want to do is put a tent over the Soviet Union."

--Art Charo, Research Fellow, Harvard University Center for Science and International Affairs.

Let me call Worden's bluff with two examples that he no doubt hopes have been forgotten.

Consider the following remarks by Secretary of Defense Caspar Weinberger on "Meet the Press" (27 March 1983) soon after the original Star Wars speech: "The President's whole motive here is to try to remove the fear of just threats of retaliation by developing a system of defensive work that will ensure that no missiles could get through... [T]he defensive systems the President is talking about are not designed to be partial. What we want to try to get is a system that is thoroughly reliable and total... And I don't see any reason why that can't be done."

Using more vivid imagery, President Reagan himself made the following well-publicized remarks to the graduating class of Glassboro High School in Glassboro, NJ three years later (19 June 1986): "Even now we are performing research as part of our Strategic Defense Initiative that might one day enable us to put in space a shield that missiles could not penetrate- - a shield that could protect us from nuclear missiles just as a roof protects a family from rain."

While Worden may try to argue these and other examples away on technicalities, the intent of such statements is clear: to build popular support for the SDI program by holding out to the public the highly appearing image of an absolute technical fix to the threat of nuclear annihilation. The repeated use of such imagery by the government's most prominent spokesmen is irresponsible and dishonest. These images are not only pure fiction, they directly contradict statements by White House and Pentagon scientific experts, as well as by those directly involved with the SDI program.

Perhaps that is why Worden, being aware of such examples, wisely covered his bets by including the word "responsible" in his challenge.

--David Wright, Dept. of Physics, Clark Hall, Cornell University, Ithaca, NY 14853-2501.

Response by Benoit Morel:

In reply to Dr. Lerch:

We should not be worried by the Soviet efforts on strategic defenses; the laws of physics apply to them the same way as to us; the countermeasures which would be lethal to SDI would also be lethal to their defense system. But before moving to implement these countermeasures, let us see what kind of defense system the Soviets have in mind; the countermeasures are much easier and faster to implement than defense systems to deploy. For the same reason it would be silly for the Soviets to redesign their missiles at high cost before we embark on the even more onerous course of deploying a strategic defense system.

Even if the Soviets were dumb enough to base their nuclear strategy on a vain attempt to prevail, we should not try to beat them at that game; keeping a reliable survivable retaliatory strike capability is all that is needed to deny them any supremacy or control. There is plenty of evidence that the Soviets have also realized that.

Let me encourage Lerch to acquire the paper from which I quoted the cost estimates.* It is not against early deployment of SDI, but using vastly optimistic efficiency assumptions of the components, it shows how limited the resulting defense would be against the *existing* Soviet arsenal.

*C. Cunningham, E. Gerry, P. Duffy, Near Term Ballistic Missile Defense, Lawrence Livermore National Laboratory.

--Benoit F. Morel.

Response by Col. Worden:

The responses to my article have missed the point.

The SDI argument concerns three competing approaches to deterrence: 1. Mutual assured destruction — minimum deterrence based on a specified damage to the sides' societies. 2. Nuclear warfighting — each side strives to deter by presenting the other with a prospect of military defeat. 3. Non-nuclear — wholly non-nuclear forces form the basis of military deterrence.

Both the United States and USSR operate under variants of approach 2 — nuclear warfighting. The purpose of SDI is not to strengthen this approach, but to move us toward approach 3. SDI can do this if it can make alternative 2 untenable. The SDI initial deployment is thus designed to disrupt analyses supporting alternative 2 strategies. Both Irving Lerch and Richard Ruquist have missed this point — they act as if I was trying to fit SDI initial deployments into current strategy.

U.S. strategy is based on a single exchange calculation. It is U.S. strategic analysis, Dr. Lerch, and not me which is targeted against first strikes. If we can destroy 60% of the Soviet military in a retaliation our current requirements can be met. Equal defenses make this hard, and incidentally give the first striker a minor advantage. Dr. Ruquist seems titillated that an SDI supporter admits this — but he ignores that I feel that the whole U.S. first strike analysis is irrelevant for a non-nuclear world.

Unlike Lerch's charge, neither I nor the Soviets consider solely first strikes. The Soviet Correlation of Forces (COF) and other similar analyses are concerned with a side's ability to dominate the military situation — before anyone strikes and during protracted warfare. Since the Soviets consider that they would be the victim of a first strike, they strive to have forces which can dominate despite such a strike. The addition of the defenses I propose does not give the United States the ability to successfully carry out a first strike — rather it drives the COF to near unity where neither side can dominate even with a first strike. Lerch is correct in his concern whether the Soviets could simply recover their COF advantage. In fact, detailed analyses show that the best response for the Soviets is massive deployment of their own defenses. The end result of this move is a defensive arms race — the United States could not preserve its 60% damage expectancy in this situation, nor could the Soviets maintain a COF advantage. If this happens, my goal of denying each side's nuclear warfighting strategy would be met.

Dr. Charo should read what Weinberger said. At no point did

he say that defenses must be impenetrable. Indeed, he specifically said, as quoted in the New York Times, that defenses considerably less than perfect could meet our goals. If Charo had paid attention to the context of the Secretary's remarks, he would have noted that Weinberger was rejecting the ground-based missile defense which supports nuclear warfighting (alternative 2) strategies in preference for a space-based defense (a "tent" over the USSR) which does maximum distruption to Soviet warfighting analyses.

David Wright should also read the entire speeches from which he extracted the President's and Weinberger's quotes. In both cases the complete protection alluded to is presented in an arms control context. Arms control reduces nuclear weapons to such a low level that essentially perfect defenses against residual threats are possible. This is not a sudden rationalization but was a concept emphasized in the President's original 23 March 1983 speech where he predicted that the SDI "could pave the way for arms control measures to eliminate the (nuclear) weapons themselves."

If there was an attempt to fool the public on the issue of defense effectivenes it has apparently worked only on those like Dr. Wright — people so wedded to deterrent alternative 1, Mutual Assured Destruction, that they would oppose defenses even if we could have perfect ones tomorrow for free. Public opinion polls repeatedly show that Americans are well aware that SDI's technical goal is not impenetrable defenses. Moreover, the overwhelming majority support SDI's real goal.

--Simon P. Worden.

PERSPECTIVES ON RADON

David Bodansky (October 1987) attempts to provide some perspective on relative radiation doses and on improving regulatory consistency. I suggest that on the latter point he fails. The fact that the acceptability of radiation doses, and in particular what consitutes an acceptable dose, differs with circumstances is not of itself evidence of inconsistency. Thus, when Bodansky compares the dose limitations recommended for radon with those imposed on nuclear waste depositories (or other nuclear facilities) and says, "it is hard to find a rational basis for the regulatory policies which have evolved," he chooses to ignore that one would *expect* different dose limits based on differences in benefits, settings, and responsibilities.

For virtually *any* environmental insult, limitations are most severe when an industry is contaminating a common good, such as outdoor air or water, and thus causing exposures to members of the public who do not derive any benefit directly from the emission at fault. Thus, levels of "criteria" pollutants in outdoor air are limited to significantly lower concentrations than those found in many homes. And contol of hazardous chemicals is much more severe for community water supplies and (again) outdoor air than it is for exposures occurring in factories or homes. Those present in the last two settings thereby receive direct benefits (and can make choices), substantially altering the acceptability of associated risks, as compared with outdoor exposures that are externally imposed by an industry.

The same must be expected of radiation exposures. Hence, in simply comparing two different exposures and railing about "outrageous...inconsistencies," proponents of nuclear power can mislead themselves and others substantially. It may very well be that the differences between acceptable levels for different settings are too extreme but, if so, the nuclear industry probably has no different a complaint than, say, the chemical industry. A decade's research on indoor pollutants suggests that indoor exposures of various types — radon, organic chemicals, asbestos, and tobacco smoke, to name a few — can cause estimated risks comparable to those associated with aspects of lifestyle such as driving a car or even smoking cigarettes. It is evidently necessary that, in treating indoor radon, organics, etc., we re-examine the premises for exposure limitation, and build a fuller perspective on risk in different settings, as a basis for sensible control strategies (1). Such a consistent approach would provide a reasonable way for deciding what indoor levels are acceptable but would, no doubt, leave outdoor standards much more stringent, because of differences in the cost-benefit balance. And building such a perspective will also probably help to allay overly-large public fears about environmental pollutants, radioactive or chemical. However, I believe that this increase in understanding — on the part of both scientists and the public — will, if anything, be delayed by simplistic comparisons and complaints.

1. See, for example, A.V. Nero, "Elements of a Strategy for Control of Indoor Radon," Chap. 12 in W.W. Nazaroff and A. V. Nero (Eds.), Radon and Its Decay Products in Indoor Air, Wiley, New York, in press (for January 1988).

--Anthony V. Nero, Jr. Lawrence Berkeley Laboratory.

Response:

Of course, one expects and wants some differences in protection criteria depending upon circumstances. However, this does not free us from the need to ask about the reasonableness of the differences in specific cases. In the present case, I do not see how general considerations of the sort cited by Dr. Nero can justify differences of the magnitude which currently exist in the protection criteria for radon and nuclear power, in the public concern over actual and potential exposures, and in the resources allocated for study and mitigation. It does not help matters if the inconsistencies are as bad for the chemical industry as for nuclear power.

Dr. Nero almost grants my basic point when he says that "...it may very well be that the differences between acceptable levels for different settings are too extreme..." However, he suggests that it is too soon to reach a conclusion and instead that we should wait for the development of a comprehensive overall approach to the broad problem of environmental insults. I disagree, because I think that this development, while desirable as a long-term goal, is not necessary in this instance. With the disparities as great as they are (e.g. "acceptable" casualty rates more than 100,000 times greater for indoor radon than for waste repositories), it seems simple to pass judgment.

The need to look at radiation hazards on a common footing has been recently illustrated by calls, in Great Britain and elsewhere (1,2), for reduction in the maximum radiation dose allowed for workers in the nuclear industry. The suggested reductions are to annual dose levels which, for example, are below the U.S. National Council on Radiation Protection and Measurements suggested action level for indoor radon. As pointed out in an editorial in *Nature* (1) on this subject: "...the interests of public health would probably be better served by the urgent investigation of a related problem

whose importance has come to light only recently—the unexpectedly high concentration of radon in some poorly ventilated modern houses." Among other merits, such investigations can help us learn more about the hazard from chronic low-level exposures.

The problem, of course, is one of achieving balance—both in assessments of radiation hazards and in allocation of resources for investigation and mitigation. There is no good reason why we cannot have the best of a number of worlds: energy efficient housing with low radon levels and nuclear power without excessive radiation exposures. Exaggerated fears of radiation have crippled, at least for the moment, our expanded use of nuclear power, and indifference to radon is allowing high levels to go uncorrrected in some homes. These are not forbiddingly subtle matters and it strikes me as timely to address them now. Better perspective can be achieved by addressing them jointly.

1. "Hard battles on radiation safety," Nature 329, 185 (1987).
2."Radiation Dose Limits," Science 238, 1349 (1987).

--David Bodansky,

NUCLEAR POWER FEARS

One of the founders of probability theory (I believe it was Laplace) noted that the general public has a reasonably good grasp of elementary probability. That is, they understand that the odds on a tossed coin should be even, and so on. However, he went on to observe that people will make bets known to be mathematically unfavorable if the rewards of winning are sufficiently high. Anyone who questions this statement need only consider the multitudes who play the New York State Lottery.

Some part of the public's fear of radiation can be explained by the mirror image of this observation. People will not make a bet which is mathematically sound if the cost of losing seems too high. If you tell a member of the public that a radiation accident which causes 10,000 casualties may be expected to occur with a probability of only 1 in 10 million for any given year (numbers pulled out of the air), you have not helped to make him a proponent of nuclear power—probably just the reverse.

--J. Samuel Smart, Croton-on-Hudson, New York

PHYSICS AND EVOLUTION

I wish to comment on the letter from Chris Orr (January 1988) on your editorial (July 1987).

Scientists/physicists do not use concepts appropriate to the study of immaterial reality. Theologians do. As scientists we would be remiss if we were to introduce concepts such as angel, transubstantiation, God, and so on, onto our science classes. Of course, we are not pure scientists; we have many aspects to our being. As other-than-scientist we can see the world in models that are not scientific. *I believe* that God created the Universe. I also believe that He used evolution to do it.

To suggest that "evolution" and "theory of evolution" are synonymous is a serious error. Just as gravitation is a fact, so too is evolution a fact. The various theories of gravitation are and will be tested and testable as also will be the various theories of evolution. No theory can ever be established as true; theories are always temporary.

There are no "unsettling parallels between the Genesis acount and evolution's early chronology." The two accounts have completely different conceptual frameworks, purposes, and modes of presentation. They are not comparable. Extracting creationism from Genesis is not only poor science but also poor theology. It only arises from a poor understanding of the Bible and its purpose.

"Believing in creationism" is quite distinct from "believing in creation." As a theory, creationism has been disproved. There is no evidence supporting it. Just try and find the "evidence" in the thousands of books and articles on the topic and you come up with nothing, not even a conceptual framework.

I do not know who teaches the theory of evolution as fact. I suspect that no scientist does. The suggestion that anyone does is a straw man.

--John L. Hubisz Ph.D., B.Th.,Physicist/Clergyman.

I have written on this topic in *A Physicist's Guide to Skepticism* to be published in the spring by Prometheus Books. Without going into all the arguments, I would simply like to point out that creationism, far from being a science on the same level as evolution, is not even an empirical theory. If you start with the premise that a supernatural being created the universe 6,000 years ago, and that the fossils of prehistoric animals were placed in the rocks for reasons known only to the creator, then you can prove anything you want.

Since, according to the creationists, creationism must always be true, there are no tests that can be applied to prove it false. Thus, it is not an empirical theory. As Karl Popper has put it, empirical theory must be falsifiable. Chris Orr gives the game away when he says, "...a catastrophic creation cannot be disproved." The nature of empiricism and its status in science should be taught to our students from the beginning.

--Milton Rothman, Physicist (retired)

The time has come to clear up some muddy thinking regarding the discussion of evolution among scientists so that when we are asked to give our views to laymen we are clear on the basics.

Chris Orr's confusion arises because he does not (as many others do not) make a clear distinction between evolution as a natural phenomenon and any particular theory of evolution devised to explain certain indisputable observations.

Evolution is a fact. There is ample evidence in the geological as well as the genetic record to show that various species have evolved. For the most part the evidence is clear and convincing. Sometimes there are gaps and missing pieces just as in any other scientific data set.

There are several theories of evolution. One, usually called *the* theory of evolution, is Darwin's Theory of Evolution. This theory has been very useful over the years and is in accord with most of the observed facts of evolution. Other theories have been proposed in an attempt to encompass more and more of the observations and explain some apparent conflicts and discrepancies. This is fine, this is how scientists arrive at the truth. It may even be necessary to abandon Darwin's theory at some point but up to now it has had the most success in explaining the most observations so it is what is taught.

When Orr says, "data supporting evolution is good but not conclusive" we see that he should have said that data supporting this or that theory of evolution is good but not conclusive. On the other hand, I know of absolutely no data in support of creation (in fact there is ample evidence to the contrary) and I was not even aware that there was a theory of creation.

If we are "true scientists" we should not present views which are our personal opinions regarding matters about which we have not a shred of evidence, that is, matters of faith and belief. We should have enough respect for those that we teach that we do not promise more than we can deliver.

Finally, to be quite frank, I find it appalling that a distinguished journal such as yours should give ignorance such a forum.

--Mark H. North, Dept. of Physics, Mich. Tech. Univ., Houghton, MI 49931.

I used to have an office next to our department office. It was trying because I got more than my share of non- physicists who wanted to prove the theory of relativity to be incorrect. Usually these attempts were torturous calculations that claimed to deduce contradictions from the Lorentz transformations. I tried to explain that that was impossible and had no relevance to the theory of relativity. This response was never satisfactory and I was requird to find the elementary algebraic mistake in a morass of frequently illegible calculations. The discovery of a mistake in their own work did not faze the authors. They promised to come back with a new proof and frequently they kept their promise.

A common feature of these people was their inability to grasp what a scientific theory is. I tried to explain to them the accomplishments of the theory of relativity. They would have none of it. They knew it was wrong and they were not interested in what could be explained by the theory or how it guided research.

Creationists remind me of those who insisted that relativity was wrong. They are not interested in the accomplishments of evolution or the guidance and coherence that it gives to biology. It offends them and they intend to show that it is wrong. The creationists fail to understand what a scientific theory is. It is not a hypothesis that can be proved to be true like the detective's suspicion in a crime novel. A scientific theory serves to correlate existing experimental facts and to direct furtehr research.

Creationism is not a theory. It does not correlate evidence except to say it is all correlated. It does not stimluate research. It should be ignored by scientists and it should not be taught in schools.

--Francis Halpern, Santa Rosa, CA.

It should be recognized that Darwinian evolution and biblical creationism are both theories, the basic difference between them being that evolution is a scientific theory, whereas creationism is a non-scientific theory which could be called "The Genesis Theory" or "The God Theory."

It is natural that the scientific theory gets higher priority in science courses, where creationism should also be discussed, primarily to explain that it is unscientific, and therefore necessarily arouses skepticism among scientists.

It would be incorrect, however, to dismiss Genesis as a mere notion or a delusion, because it is a belief which has been held by too many people for too long a time to be taken lightly. In that sense, it is strongly favored by Darwinism, as a theory which is clearly fit to survive!

The Bible could be considered the original or prototype science text, as well as the original text on ethics and humanities. The science part needs revision, so scientists can take it literally, instead of having to regard it as an elegant collection of poetic metaphors, where, for example, God is a metaphor for the basic mechanism of the universe, or "7 days" is a quaint metaphor for 7 epochs, in which the number 7 may be arbitrary. It's not bad, but it could be improved! It needs some imaginative re-thinking.

--Kenneth J. Epstein, Chicago, IL 60640.

With regard to the plea of Orr that we should admit our biases when we teach, of course he is right. And the principle applies not only to evolution, but also to ESP and flying saucers, and even to special relativity, quantum mechanics, and magnetic poles, on all of which there has been disagreement.

Many people assume that the only possible explanations of origins are evolution and supernatural creation. Although I don't have any others to offer, there is no *a priori* reason why other explanations might not be possible. Arguing against one explanation is not the same as arguing for the other, unless one can prove that there are only the two posssibilities.

Biological, geological, and astronomical evolution are full-blown scientific theories, consistent with each other and with a lot of data. Supernatural creation, on the other hand, is just a hypothesis, not a theory.

There are two big problems with the supernatural creation hypothesis. One is that there is no evidence to support it. Even more important, though, is that it is scientifically useless, since nothing follows from it: it is sterile and a dead end.

Various religions have added more hypotheses to supernatural creation, but these are simply additional hypotheses, and not at all demanded by the original one. For example, if someone should find a piece of very old wood on Mount Ararat, this might be construed as evidence for a Great Flood hypothesis, but it would mean nothing at all relative to the hypothesis of supernatural creation.

The supernatural creation hypothesis arose thousands of years ago not because there was any knowledge that suggested it, but because there was almost no knowledge at all in biology, geology, and astronomy. On the other hand, the evolutionary hypotheses in these fields were suggested by evidence, and theories built on them can be compared with new evidence that comes in.

--Edgar Pearlstein, Professor of Physics, University of Nebraska.

It is hard to know what to make of Chris Orr's statement that "facts are facts when they are conclusively proven by data." Since we can never prove anything with 100% certainty with scientific inductive reasoning, Mr. Orr must believe that there aren't any facts. Certainly the evidence for evolution by mutation and natural selection from comparitive anatomy, from the fossil record, and recently from molecular biology, is overwelming.

--Robert Joel Yaes, University of Kentucky.

Response:

The response has been similar to the numerous letters published

in *Physics Today* a couple of years ago concerning evolution and creation.

I purposely did not reveal my particular bias, which makes the responses all the more interesting, since most seemed to assume that I have a bent toward a Creator.

Hubisz, Rothman and North are correct in saying that evolution is fact, as far as species evolving to adapt to their particular environments (in a broad sense). Usually, this has been expanded to include astronomical evolution, and it is also expanded to include species evolving to a higher form.

Rothman appropriately points out the error in my statement that "a catastrophic creation cannot be disproved." It would be more correct to say that it cannot be proven at this time. It is pretty well accepted that there was some sort of catastrophic creation. But what was the nature of the "trigger mechanism" for that creation? There had to be some catalyst, some force. That force is not known at this time. However, for a person who believes that a Creator was that force, at least a force is identified. Of course, there is the question "who created the Creator?" On the other hand, we really cannot say that at $t = 0$ something was created out of nothing. That requires more faith than does belief in a Creator. The bottom line is whether the initial force can ever be identified.

Hubisz asserts that there is no parallel between Genesis chronology and evolutionary chronology. When plotted on a time line, ignoring exact dates, they match up pretty well. The unsettling aspect is that match, given the assumed ignorance of the biblical writer 5000 years ago. Odds of such a good match are not very high.

Most respondents indicate that no work has been done to present scientific evidence to support a supernatural creation. This is largely due to the fact that it is nearly impossible to get anything on the subject published in mainstream journals.

Hubisz does not know who teaches the theory of evolution as fact. Unless one lives in a vacuum, a casual survey of classroom texts and course examinations indicate that they teach evolution as fact. Since we are involved in a forum on Physics and Society, we should take reponsibility to see what is really being taught.

I enjoyed the assertions that creationists are not interested in the evolutionary process (Halpern, Rothman and North).

My enjoyment peaked at North's comment that discussing teaching is giving a forum to ignorance, and that it is appalling to discuss it in a distinguished journal. On the contrary, we should be aware of the growing narrow-mindedness in our classrooms. There is a strangulation of creative thought and scientific investigation in areas that go against currently accepted thought. I would rather see an open minded, fully informed scientist than one who wallows in preconception.

Arguments and narrowmindedness will continue on both sides until a real solution to the question of creation is found.

--Chris S. Orr.

ARTICLES

A CRITICAL LOOK AT LAND-BASED MISSILES

This issue of *Physics and Society* contains a brief preview of some of the work of the Forum Study Group on land-based missiles. Although the study is not yet complete, enough material of interest has accumulated to warrant sharing it with you. The first paper describes the framework adopted by the Study Group to evaluate the different options for land-based missiles. In essence, it stresses that the characteristics desired in a missile system depend on the role that the missile system should serve. It then sets forth a list of charactertistics. The next two papers describe and briefly evaluate two missile system options — the land-mobile Midgetman and either Midgetman or MX missiles deployed in superhard silos. Evaluations of other land-based missile options will appear in the next issue.

--Barbara Levi, chairwoman and editor of the Forum's land-based missile study.

INTRODUCTION TO THE LAND-BASED MISSILE STUDY

Leo Sartori

The role of nuclear weapons in defense policy

Three questions underlie any analysis of possible new ICBM deployments: (i) What is the mission of our nuclear forces? (ii) What attributes of nuclear weapons are relevant to the fulfillment of that mission? (iii) What deficiencies in the present force motivate new deployments? The main body of the study will assess the pros and cons of specific potential deployments.

Discussion of the mission of nuclear weapons invariably focuses on *deterrence*. It is generally agreed that the primary function of our nuclear arsenal is to dissuade anyone from initiating a nuclear attack against the United States, through the threat of overwhelming retaliation. This is the most elemental definition of deterrence, though not the only one in use. By enlarging the set of provocations to which a nuclear retaliation is threatened, one can define various types of "extended' deterrence. The expanded set can include nuclear or even conventional attacks on an allied nation.

A critical component of any deterrent strategy is its credibility: the would-be aggressor must be persuaded that the threatened retaliation is likely to be carried out. Inasmuch as nuclear retaliation carries grave risks, credibility is far from assured. It stands to reason that credibility diminishes as the set of provocations to which the threat is directed grows. There is widespread doubt, for example, as to whether the United States would risk all-out nuclear war in response to a conventional attack on one of the NATO countries.

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Fortunately, successful deterrence does not require that the potential aggressor be certain the threatened response would materialize. Even a small risk of initiating all-out nuclear war can be expected to deter under all but the most exceptional circumstances. In this sense deterrence can be regarded as a bluff, but one that the adversary can call only at grave peril.

The basic problem of strategic planning is to decide what force will provide the most effective, most reliable, and most credible deterrent. The view prevalent today is that the United States must be capable of "flexible response" to a spectrum of possible attacks. Some assert that we must be prepared to wage a limited but possibly protracted nuclear war. That doctrine goes beyond deterrence and purports to address the question, "What if deterrence fails?" Its proponents argue that the capability to wage nuclear war "successfully" is itself the most effective deterrent. Others strongly disagree, contending that a nuclear war-fighting capability is unatainable and that its pursuit could have disastrous consequences. Such doctrinal questions have obvious implications for the character and size of the forces required.

Counterforce, the capacity to destroy military facilities including hardened targets such as missile silos, is perhaps the most controversial aspect of nuclear weapons. Some argue that counterforce has no place in a pure deterrent strategy because a retaliatory strike would be aimed at civilian/industrial (countervalue) targets that are not hardened. The opposing view posits that any Soviet first strike would attack US strategic forces while avoiding civilian targets. A countervalue response to such an attack would be suicidal, it is argued, because it would inevitably elicit a devastating second-wave attack against American cities. Inasmuch as a threat to commit suicide has low credibility, a strategic force with only counteralue capability has little deterrent value. A counterforce response would be far more credible since it would be less likely to trigger a disastrous exchange of attacks on population centers. Critics of this view point out that counterforce targets are so numerous and widely distributed on both sides that even a "pure counterforce" strike would cause heavy civilian casualties and might be hard to distinguish from an all-out attack.

The paradox of counterforce is that any force capable of carrying out an effective counterforce second strike can *ipso facto* carry out an even more effective counterforce *first* strike. Acquisition of a powerful counterforce capability by either superpower is therefore bound to be viewed by the other as an intolerable threat to its security, to which a response is mandatory. The growth of Soviet counterforce during the 1970s was in fact so viewed by the United States and provided justification for the large military buildup carried out by the Reagan Administration in the 1980s.

Understandably, each superpower tends to take the position that "our counterforce is good (i.e., defensive) but theirs is bad (i.e., aggressive.)" Doctrinal considerations aside, it is a fact that US counterforce capability has been increasing over the past decade; proposed new deployments will continue that trend.

Attributes of nuclear weapons

The following are the principal attributes by which specific weapon systems and total force structure are judged. Depending on the intended mission for the weapons system, each attribute would be more or less valued.

Survivability. The deterrent value of a weapon system vulnerable to surprise attack is small. It is generally assumed that the United States would ride out a Soviet first strike, at least until nuclear weapons had detonated on US soil and the scale of the attack had been determined. Under a launch-on-warning strategy, survivability is less important than a reliable early warning system and the ability to launch quickly. But LOW is generally held to be a reckless strategy because it risks initiating nuclear war as a result of accident or of a false alarm. Without LOW, survivability is a primary criterion in evaluating any weapon system; it is the sole motivation for all mobile missile basing modes.

Accuracy is the principal attribute that determines counterforce capability. In order to produce the high over-pressure needed to destroy a hardened target, a weapon must detonate very close to the target. The accuracy required depends on the hardness of the target and on the nuclear yield of the weapon. The newest missiles have accuracies of the order of 100 meters.

Speed is also relevant to counterforce. A weapon with very long delivery time allows the opponent to take defensive measures including launch of the threatened forces. Ballistic missiles are fast, while bombers and cruise missiles are much slower.

Security of command, control, and communications (C^3) is particularly important for a second strike mission. If the United States is to ride out an attack and retaliate effectively our forces must be able to receive commands and respond reliably in the highly stressed environment that would follow the detonation of large numbers of nuclear weapons over US territory. This requirement calls for a highly robust C^3 system, including back-up modes for vulnerable components. A first striker need be less concerned over C^3 because his attack would be launched in a relatively calm environment.

Arms control considerations. Since no treaty limiting offensive strategic systems is now in effect, each side is free to deploy new weapons of whatever type and in whatever quantity it pleases. In the START negotiations, however, each side has proposed substantial reductions from present force levels, with ceilings on various types of weapons.

The possibility of a START agreement affects deployment decisions in two ways. First, whatever weapons are selected for deployment should be ones that the United States would plan to retain as part of a reduced force. Second, any proposed deployment would affect the verifiability of a treaty. The basic verification task in any prospective treaty will be to count deployed missiles and/or warheads. Weapons that are difficult to count would complicate verification and might jeopardize the successful conclusion of a treaty. This is a particular concern for mobile deployments.

Redundancy. A major tenet of US policy is to maintain a variety of strategic forces so that a decline in reliability of any one component does not jeopardize the deterrent. Strategic planning has long been based on the triad of ICBMs, submarine-based missiles, and bombers, whose differing capabilities and vulnerabilities provide a synergistic effect. The number three, however, is not God-given. The possibility of moving to a modified dyad is one of the options analyzed in our study.

Crisis stability depends on the over-all character of the forces on both sides. Stability is high if there is no incentive to strike first in a crisis. Possession by both sides of strong counterforce capability, particularly if the forces are highly MIRVed, is destabilizing in that it appears to confer a substantial advantage to the side that strikes first.

Cost is of course always an important consideration. For example, Midgetman will cost significantly more, per warhead, than MX in silos, and this might ultimately be a deciding factor in the deployment decision.

Do we need new missiles?

The present US ICBM force is entirely silo-based; the first 50 MXs are being deployed in modified Minuteman silos. This force has low survivability: it is in principle vulnerable to attack by accurate Soviet SS-18s, SS-19s, or the new SS-24s and SS-25s. This is perhaps the major rationale for new deployments. In order to improve survivability, the Pentagon is proposing to deploy additional MXs and/or the new Midgetman ICBM in mobile basing modes. It should be noted that the SLBM force continues to be highly survivable; bombers are survivable if given adequate warning time.

Survivability can be improved without deploying new missiles, for example by superhardening the silos. This is one of the options analyzed in our study.

The proposed new deployments would also substantially increase US counterforce strength. Minuteman III, long the mainstay of the US ICBM force, has only modest counterforce capability. MX, with improved accuracy, is definitely a counterforce weapon; Midgetman would have similar accuracy. A force of 100 MX (or of 50 MX and 500 Midgetmen) would carry 1000 warheads, not enough to threaten all 1400 Soviet ICBMs; in conjunction with SLBMs, however, it would place all Soviet silo-based ICBMs in jeopardy. (The Trident D-2 SLBM, with accuracy comparable to that of MX, will be a potent counterforce weapon.)

Whether the additional counterforce strength is needed is a controversial question, as discussed above.

An argument in favor of Midgetman is that it would contribute to improved crisis stability. Any attack on a force of single-warhead missiles would necessarily expend more warheads than it destroyed; in this respect, there would be no advantage to striking first in a crisis. Evolution toward non-MIRVed forces is held by many to be a desirable goal.

Among the problems raised by the proposed mobile deployments are verification, high cost, and the difficulty of maintaining secure C^3. These problems are examined in detail in later parts of our study.

LAND-MOBILE MIDGETMAN

Art Hobson

The land-mobile Midgetman has been dubbed an "arms controller's missile." When the Scowcroft Commission proposed this missile in 1983 (1), stability was its main justification: its single warhead would make it a less lucrative target and its mobility would make it a less vulnerable one than the silo-based, multiple-warhead Minuteman and MX missiles. Midgetman should also be a more survivable missile, although it costs more, complicates command and control and makes verification of numerical limits more difficult. We review all of these characteristics here.

Description

Because it is mobile, the Midgetman must be short (16m) and light (17 tonnes). Like the MX, it is boosted by 3 solid-fuel stages plus a post-boost liquid propellant stage for final targeting adjustment. It will carry the same re-entry vehicle as the MX, with a 300-475 kiloton warhead, and it will probably be guided by a modified version of the MX's inertial guidance system. Its accuracy and lethality will thus rival that of each MX warhead.

The missile will be carried in a hardened mobile launcher (HML) that is towed by a tractor designed for roads and limited off-road travel. Together, the tractor, missile, and missile carrier weigh 107 tonnes. Scale-model tests indicate that, when lowered and anchored to the ground in its streamlined configuration, the HML can withstand a blast overpressure of 2 atm (30 psi), compared with 0.5-0.7 atm for a modern battle tank and 200 atm for a hardened missile silo. A HML can withstand the airburst from a 500 kiloton SS-18 warhead detonated 1.5 km away. The HML's maximum road speed is 80 km/hr but mobility testing over typical operating terrain indicates an actual average speed of about 45 km/hr. Before launch, the 2-member crew lowers the carrier to the ground and drives away in the tractor. A remote launch-control center then fires the missiles (2,4).

Two mobile basing modes are under study: *Random mobile*, in which HMLs move randomly over 5 large tracts of military land in southwestern U.S., and *dash mobile*, in which HMLs remain parked at Minuteman silos in the northern midwest, ready to dash on warning onto surrounding roads and farms.

In the random-mobile scheme, HMLs would move periodically over the deployment area to deny an enemy's knowledge of their locations. In peacetime the missiles would roam over 10,000 km² near the perimeters of these tracts. During periods of "increased tension" (strategic warning), the HMLs would expand their operating area to include also the full interior of these tracts, an area of 20,000 km². "In the event of imminent attack" (tactical warning), the HMLs would dash off of the DOD land onto adjacent roads and open country, expanding the area of dispersal still further.

In the dash-mobile deployment, HMLs would be based in soft bunkers at some of the 1000 Minuteman silo sites, with two HMLs located just a few meters away from each selected silo site. Upon tactical warning, the HMLs would dash onto adjacent roads, dispersing a safe distance from the silos within the flight time of an attacking re-entry vehicle, which presumably means the 30-minute flight time of an ICBM.

Estimates of the number of deployed Midgetmen vary from 150 to 1000 but the most commonly discussed number is 500. Present plans call for an initial 150-250 missiles in the dash-mobile mode at Malmstron Air Force Base, Montana, in 1992 (3).

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Survivability

The most plausible attack on mobile missiles during this century is a barrage attack of the entire operating area. Beyond that time frame, targeted attacks on individual HMLs, using directed homing warheads, may also be feasible. Figure 1 summarizes my calculations of Midgetman survivability under barrage attack. The graph shows the number of Soviet SS-18 *missiles* needed to barrage deployments of 500 Midgetmen on 5 reservations, as a function of dash time (the area barraged by a given missile is independent of its number of warheads). Under these barrages, the Midgetman force would have a survival rate of 10-20%.

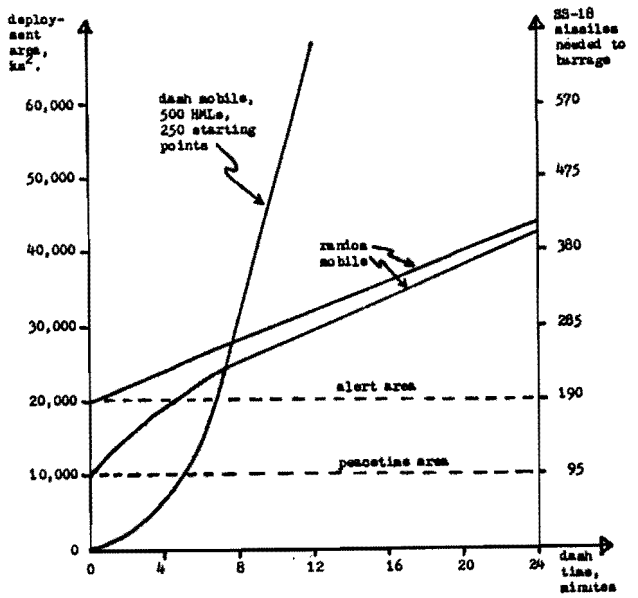

Figure 1. Deployment area generated, and number of SS-18 missiles needed to barrage that area at a survival rate (fraction of HMLs surviving) of 10-20%, as a function of dash time, for the dash-mobile and random-mobile basing modes. The attacking missiles are assumed to be air-burst at the optimum height for maximum destruction. The assumed dash speed is 45 km/hr (28 mph). "Dash time" means the time actually spent moving at this average speed. The total time to deploy, including warning, communication, start-up, and deployment in hardened configuration, would be at least several minutes longer.

The calculations shown in the figure assume that the dash begins when an enemy missile is launched. Realistically, however, the time available for dashing is decreased by the time to detect an attack, communicate the dash order, start up, and dig in.

Figure 1 indicates that the current Soviet deployment of 308 SS-18 ICBMs would probably not be sufficient to barrage a random-mobile deployment of Midgetmen, because of the dash time allowed by the 25-minute ICBM flight. Even if the Soviets supplemented their SS-18s with other existing ICBMs, they would have to spend many more warheads (at their current levels of MIRVing) than they destroy.

However, SLBMs rather than ICBMs would probably be used against Midgetman. Figure 1 illustrates a new feature of the Midgetman system not present in silo-based missiles — their great sensitivity to warning time. Like the U.S. bombers, their main threat would come from submarine launched ballistic missiles (SLBMs), which can be launched from just off the coast to cut flight time about in half. The Air Force plans for the quick-reaction portion of the bomber force to leave the bases with 3-10 minutes warning.

Plans to move HMLs onto a larger alert deployment upon strategic warning helps reduce but does not eliminate its vulnerability. There may not be sufficient strategic warning, or the President may hesitate to go to high levels of alert. Note that Midgetman deployment could create a tension between survivability and stability. Calling for alert status, so essential to survival, is a provocative and destabilizing act.

SLBMs are not currently accurate enough to destroy silo-based ICBMs, but they can barrage Midgetman deployment areas. Each of the modern Soviet SLBMs (SS-N-20 or SS-N-23) has a barrage effectiveness of about 1/3 that of the SS-18. Thus multiplying the number of missiles in Figure 1 by 3 gives the number of SLBM missiles for the same barrage. For example, with 5 minutes delay and 13 minutes SLBM flight time, the Midgetman force could be destroyed by about 600 missiles. The Soviets have more than this number of SLBMs but they would have to sneak more than 30 submarines close in off the U.S. coast.

Even if they succeeded, an attack today by SLBMs would provide warning to the silo-based ICBMs, which could then be launched before enemy ICBMs arrived. Thus, currently, timing problems inherent in an attack with both ICBMs and SLBMs would prevent U.S. ICBMs from being attacked at the same time as bombers and HMLs.

However, the most critical strategic problem in the 1990s may be the simultaneous vulnerability of ICBMs and bombers to a single SLBM attack. The Soviets are likely to achieve the same hard-target kill accuracies for their SLBMs as those expected for the U.S. Trident II missile, to be deployed in 1989. Because of its own vulnerability to short warning times, the Midgetman cannot help rectify this vulnerability problem. The dash-mobile scheme is especially vulnerable to short warning times. HMLs start their dash from Minuteman silos so that a surprise attack could destroy them with only a few Soviet SLBMs beyond those already needed to destroy the colocated Minuteman or MX missiles.

The Soviets currently have enough SLBMs to barrage the Midgetman force and also to attack the entire Minuteman/MX force plus the bombers if they can catch the HMLs on their alert areas. In the case of 50% reductions now being discussed Soviet SLBMs could attack random-mobile HMLs only in their peacetime dispersal, and may not have enough missiles left over to attack the silo-based ICBMs and the bombers. However, the dash-mobile deployment would be more vulnerable to the attack on ICBMs and bombers. If both sides reduced to "finite deterrence" levels (500 ICBM and 500 SLBM warheads), the Midgetmen could be threatened only if several hundred warheads had yields on the order of 10 Mt.

Thus mobility alone cannot provide absolute protection for land-based missiles. The Midgetman may remain relatively invulnerable only if coupled with arms control measures to contain quantitative and/or qualitative improvements.

Stability

A major rationale for the Midgetman has always been its single

warhead. The attacker can never do better than to destroy one warhead for each warhead its spends. By contrast, an attacker can (with high probability) destroy the 10 warheads on one MX missile or the 3 warheads on one Minuteman missile for every two warheads it spends. The mobility of the Midgetman further enhances its stability: If the Midgetman is deployed in a random-mobile mode, the price to destroy one Midgetman warhead is about 6 warheads from a 10-MIRVed SS-18. In a crisis, the Soviets are less likely to feel they have something to gain by a preemptive strike against Midgetman forces than they would if the U.S. had all silo-based MIRVed missiles.

The stabilizing effect of Midgetman's single warhead and mobility needs to be balanced, however, against the destabilizing effect of its highly lethal warhead. Midgetman can destroy hard targets, and thus threatens silo-based missiles. If the Soviet planners make the same worst-case assumptions as DOD planners, they will estimate that each Midgetman has a 95-99% chance of destroying a Soviet ICBM in an attack with one warhead on each silo.

At the same time, however, if the Soviets continue their current trend of deploying road-mobile SS-25s or rail-mobile SS-24s, the accuracy of the Midgetman may become relatively unimportant.

Cost

The lifecycle cost of a force of 500 mobile Midgetmen may be about $50 billion, compared to $21 billion for 500 MX warheads on 50 missiles based in Minuteman silos (4). The high price has dampened some of the enthusiasm for this missile. The price per warhead would be reduced if the missile carried two or three warheads. However, the additional weight decreases the missile's mobility, so there are no current plans to add warheads.

Command and control

Land-mobile missiles cannot be connected to their launch control centers by permanent hardened land lines. Instead they must rely primarily on radio communications. Presumably the receivers designed for the Milstar systems, particularly for use on bombers, would function equally well for mobile missiles, and the launcher could carry a deployable antenna which would allow the crew to utilize information received via the GWEN network. Airborne launch control centers could also launch the missiles using line-of-sight UHF communications.

The ability to control the missiles depends critically on whether the missiles are to be used only in a reflexive deterrent fashion, or in a more active war-fighting role. In the deterrent mode, the control system is pre-programmed to carry out any one of several possible nuclear attacks, and the attack order is chosen form one of these plans. Pre-programming places minimal demands on the control structure. But protracted nuclear war-fighting requires such capabilities as retargeting of nuclear weapons during a war, necessitating in turn a far more sophisticated and survivable control system. The low data rates of the GWEN communication system and the questionable survivability of HML crews make it unlikely that adequate control can be maintained during a protracted war. Mobile missiles are also more vulnerable to accident and to terrorist attack, and will require adequate security systems.

Verification

The very feature that makes the Midgetman survivable — its mobility — is the feature that makes it difficult to count for arms control purposes. The SALT treaties relied almost exclusively on remote observations to check that the other side was not cheating on the treaty limits. By contrast, the Intermediate-range Nuclear Force (INF) treaty, which deals with mobile missiles, imposes unprecedented intrusive verification measures, including on-site inspections of missile bases and the permanent presence of inspectors at production facilities. It is likely that a treaty limiting the numbers of strategic mobile missiles would require verification measures at least as intrusive. Restrictions of deployments to well-defined bases, a practice usually dictated in any case by operational necessity, may help each side to keep track of the adversary's missile deployments. Monitors at production facilities could provide additional checks against illegal deployments. These verification procedures make it less probable, although never certain, that the other side will not cheat.

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SUPERHARD SILO MODE FOR MX OR MIDGETMAN MISSILES

John R. Michener

In this deployment mode the missiles - either the 10 warhead MX or the single warhead Midgetman - are deployed in exceedingly strong silos placed in very hard rock formations. It is expected that such silos can survive peak blast pressures of about 7000 atm (100,000 psi). Such silos have appreciable steel volume fractions and exhibit substantial ductility when subjected to such air blasts.

The high hardness of the silos results in substantially reduced kill radii of the attacking warhead. The kill radius of a ground-bursting 500 kt warhead against a 7000 atm silo is about 75 m, a factor of 4 smaller than the typical 130 atm hard silo. While such silos are vulnerable either to the current generation's high-yield ICBM's or to the next generation's highly accurate ICBM's, they are not

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vulnerable to submarine launched missiles of the current or the next generation utilitzing ground-burst warheads. These silo designs are potentially vulnerable to earth penetrating warheads. The explosion of a moderate yield warhead at a depth of some tens of meters results in a ground shock far more intense than the ground shock of the same weapon detonated at the surface. The ground shock has a longer time constant than the air burst and subjects the silo to far higher bending and lateral stresses than a surface burst. These effects should result in a silo-killing effectiveness of about 30 times that of a equal yield groundburst. (The yield of a penetrator warhead would probably be substantially less than that of an equal-mass ground-burst warhead because of the hardening necessary to enable the warhead to survive an encounter with solid rock at about 4 km/sec.) While neither the US or the USSR currently have earth penetrating warheads, such warheads are in the early phases of development in the US, and presumably the USSR. The development and deployment of such weapons would substantially increase the silo kill radius of the attacking warhead. The timed detonation of warheads emplaced in the ground would allow the attacker to avoid fratricide of multiple attacking warheads and allow for the coherent interaction of separate expanding blast waves to subsurface Mach fronts and stems, further increasing the damage to the attacked silos. A ban on the development of such weapons, which could be accomplished by banning the testing of nuclear weapons of yield greater than 1 KT, would substantially increase the survivability of this deployment option.

The crisis stability of this mode depends upon the missile protected: The MX carries 10 warheads and is a very valuable target, while the Midgetman missile carries 1 warhead and is correspondingly about a tenth as valuable a target. While the USSR would be likely to view the destruction of an MX missile to be worth 2 high yield warheads, it is uncertain that the destruction of a Midgetman would be viewed as being worth such an expenditure.

Superhard silos are eminently verifiable. No other construction remotely resembles a superhard silo. They are easily counted from orbit. The construction of a silo field and its associated launch control centers cannot be hidden or concealed.

This deployment mode allows time-urgent control of the missiles. Since they are emplaced in close communication with their launch control centers they can potentially be retargeted easily and launched whenever desired.

Both these missiles are designed to be highly accurate and have hard target kill capability. The launching position is known accurately, as are the trajectories that would be used to attack chosen targets. Both missiles carry warheads with yields of several hundred kt with CEP's on the order of 90 m. The MX can carry a single warhead with a yield of about 20 MT.

While I have requested economic information from the Air Force upon the cost of constructing superhardened silos for the MX and Midgetman missiles, I have not received any information at this time. The MX has been in production for several years now. As such, its development costs have been amortized. The cost of producing additional missiles is < $100 million each. The Midgetman missile is still in the development stage and therefore still requires development and testing expenses. Due to these factors the Midgetman missile is substantially more expensive than the MX missile if only a few hundred are produced. Its costs drop rapidly if large numbers of these missiles are produced. The cost of the silos for the missiles is far higher for the MX than it is for the Midgetman. The mass of the MX silo will be at least 10X that of the Midgetman silo and the components are heavier and more difficult to handle (it should be noted that for an equivalent throwweight 10 times as many Midgetmen will be needed). Estimates that I have heard for the cost of MX silos exceed $100 million. If Midgetmen were deployed in large numbers it would be feasible to build a factory to produce large steel castings for the construction of Midgetman silos. Such a construction technique would allow substantial economies of scale and labor savings for the Midgetman program. Similar savings would be much harder to realize with the MX silos.

Operational doctorine has a substantial effect upon the relative cost effectiveness of these missiles. The MX missile is a cheaper delivery vehicle than the Midgetman if it is launched before an attack lands. In such a case there is no need to harden the silos at all, but there are associated problems of first strike vulnerability and crisis stability. The Midgetman missile is more expensive than the MX, but it is a far less valuable target than the MX. The destruction of a Midgetman force would require a far greater allocation of high lethality Soviet ICBM's than would be required for a similar destruction of MX missiles. If it is desirable that the missiles ride out a first strike, the Midgetman is clearly superior to the MX. If it is expected that the missiles will be launched under attack, the MX is a more cost effective option.

REVIEW

RADIOACTIVE RADIATIONS AND THEIR BIOLOGICAL EFFECTS by Peter Lindenfeld (American Association of Physics Teachers,College Park,Maryland, 1986)

Nearly a decade ago, AAPT organized a committee to generate a series of "Issue-Oriented Modules" designed to be used by students in traditional physics courses. *Radioactive Radiations and their Biological Effects,* by Peter Lindenfeld of Rutgers University, is a very useful product of that effort. Those who know Lindenfeld will not be surprised by the gracefulness of the booklet's prose and its logical organization.

The discussion begins with a description of background radiation and its measurement. The dose concept is defined and related to biological effects. Worked examples and problem sets accompany the text. Suggestions are made for possible student projects and discussions. An appendix provides a short but readable overview of the fundamentals of nuclear physics.

Of course, this booklet was written before the Chernobyl accident occurred. People using this book now will surely want to add a discussion of the projected health effects of that accident, and questions related to it that have yet to be answered.

More undergraduate physics majors need opportunities to discuss pressing issues that cut across disciplinary lines. Nuclear power is only one example of an issue where a knowledge of physics is essential to informed discussion. This objective and interesting treatment could be read with profit by majors and non-majors alike. It is highly recommended.

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NEWS

COMING FORUM SESSIONS: BALTIMORE MEETING, 18-21 APRIL 1988

The spring meeting, or "Washington Meeting" (being held in Baltimore this year!), is the big meeting of the year for the Forum. We have several invited paper sessions, the awards session, a contributed paper session (our only contributed session of the year), a business meeting, and short courses at this meeting. We hope that the physics-and-society community will try to attend this meeting this year and every year.

SHORT COURSE: NUCLEAR ARMS RACE TECHNOLOGIES IN THE 1990s
Saturday and Sunday, 16 and 17 April (the days before the Baltimore meeting) at The George Washington University, Washington, D.C. See announcement below for details.

FORUM AWARDS SESSION, Dietrich Schroeer, Forum Chairman, presiding.
Monday 18 April, at 20:00. The awards session will be preceded by a business meeting at 19:30, all Forum members invited.

- Ashton Carter, Harvard University, "Technical aspects of the implementation of the ABM treaty"
- Robert Williams, Center for Energy and Environmental Studies, Princeton University, "The coming age of gas turbine power"

See announcements below for the award citations.

INVITED SESSION: LAND-BASED MISSILES IN AN ERA OF STRATEGIC REDUCTIONS, Barbara Levi presiding.

- U.S. Air Force, "Rail garrison MX"
- U.S. Air Force, "Midgetman"
- John Steinbruner, Brookings Institution, "Force reductions and the principles of stability"
- Gerald Marsh, Argonne National Labs

(See the meeting program in the APS Bulletin for further details.)

INVITED SESSION: THE OZONE HOLE, Ruth Howes presiding.

- Mark Schoeberl, NASA Goddard Space Flight Center
- Steven Fels, National Oceanic and Atmospheric Administration, Geophysical Fluid Dynamics Lab
- James Anderson, Department of Earth and Planetary Science, Harvard

(See the meeting program in the APS Bulletin for further details.)

INVITED SESSION: THE SAFETY AND FUTURE OF DEFENSE PRODUCTION REACTORS, Anthony V. Nero, Jr., presiding.

- Theo G. Theofanous, University of California, Santa Barbara, "Safety Issues and fixes for DOE production reactors"
- Herbert Kouts, Brookhaven National Lab, "Institutional aspects of safety of DOE production reactor
- Bruce Twining, U.S. Department of Energy, "Considerations in decisions on future production reactors"
- David Albright, Federation of American Scientists, "A View of future requirements for nuclear weapons materials"

CONTRIBUTED SESSION: FORUM, Robert Hallock presiding. There will be one session featuring contributed papers on the application of physics to problems of social concern. Try to attend, and join in the discussion! This session is an annual event. Next year, contribute your own paper on physics and society.

SHORT COURSE: NUCLEAR ARMS RACE TECHNOLOGIES

There will be a short course on Nuclear Arms Race Technologies: The 1990s, to be held 16 and 17 April 1988, in Washington, DC, sponsored by the Forum and by the American Association of Physics Teachers.

This short course will follow the tradition of the 1982 and 1983 courses. These resulted in the AIP Conference Proceedings #104: *Physics, Technology and the Nuclear Arms Race,* edited by D. Hafemeister and D. Schroeer, who are also organizing the course for 1988. The course will inform physicists who teach about the arms race or who want to study these issues more deeply. The talks will emphasize the technical aspects.

There will be 15 one-hour talks by experts, including for instance Edward Luttwak of the Center for Strategic and International Studies, Spuregon Keeny, Jr., President of the Arms Control Association and former Deputy Director of ACDA, and P. Bescond, President of the SPOT Satellite Corporation. Topics include environmental effects of nuclear war, proliferation of nuclear weapons, the Strategic Defense Initiative, treaty compliance and verification, the strategic triad, and strategic policy.

The short course begins at 14:00 on Saturday, 16 April, and ends in the evening of Sunday, 17 April. These two days precede the APS general meeting in Baltimore. The short course will take place at George Washington University in Washington, DC.

The registration fee is $60, which includes the cost (about $50) of the resulting 500-page AIP Conference Series book. Send registrations to Professor Dietrich Schroeer, Department of Physics and Astronomy, 278 Phillips Hall-CB#3255, University of North Carolina, Chapel Hill, NC 27599-3255. If you don't have time to preregister, you can register at the meeting by 14:00 Saturday.

1988 SZILARD AND FORUM AWARDS

SZILARD AWARD: To Robert Williams, Senior Research Scientist at the Center for Energy and Environmental Studies, Princeton University, "For applying physics to end-use energy efficiency and educating physicists, members of Congress, and the public, on energy conservation issues." The American Physical Society's Szilard Award is given annually to an individual or group who has constructively applied physics in the public interest.

FORUM AWARD: To Ashton B. Carter, Associate Director of the Center for Science and International Affairs, Kennedy School of Government, Harvard University, "For his clear and lucid exposition of the physics issues in the nuclear arms race and his unique ability to combine his physics background and good judgement to clarify the technical parameters of these important public policy issues." The American Physical Society's Forum Award is given annually to an individual or group who has promoted the public understanding of the relation of physics to society.

The Awards Session will be held at 20:00, Monday, 18 April, at the Baltimore meeting.

NEWLY ELECTED FELLOWS OF THE AMERICAN PHYSICAL SOCIETY, FORUM ON PHYSICS AND SOCIETY

Robert J. Budnitz, Future Resources Associates, Inc.: "For leadership in applying physics to issues in environmental and energy policy and careful studies of the technology of nuclear reactor safety."

Jack M. Hollander: "For founding and directing research programs on energy and the environment and for taking a leading role in the study of global energy resources and requirements."

Anthony V. Nero, Lawrence Berkeley Laboratory: "For his leadership in the study of radon and indoor air quality and assessment of risks associated with nuclear, geothermal, and fossil fuel generation of electric power."

Richard A. Scribner, American Association for the Advancement of Science: "For leadership in applying physics to arms control problems and for developing the Scientific Congressional Fellowship Program. Your coordination of the efforts of the scientific societies which participate in this program enable these societies to provide important scientific input to the public policy debate."

FORUM ELECTION RESULTS

Our new vice-chairman is Richard A. Scribner. Our new Forum councilor is David Hafemeister. Our new secretary-treasurer is Henry Barschall. Barbara Levi, our present vice-chairwoman, automatically becomes our new chairwoman.

HIGHLIGHTS OF THE APS COUNCIL MEETING

Items of interest to Forum members from the November APS Council meeting in San Diego:

Directed Energy Weapons Study. Council expresssed great satisfaction with the quality of the DEW Study report and the extent to which it has stood up over the six months since its release. It has clearly been influential. Criticism, although strident, has been limited to just a couple of sources. (See the debate in the November *Physics Today* between critic Gregory Canavan and Study Co-Chairs Nicolaas Bloembergen and Kumar Patel.) Council noted with gratitude the enormous effort expended by Bloembergen and Patel not only during the study but since its release. Several other members of the study panel have also devoted much time to follow-up activity.

New APS Vice-President. Eugen Merzbacher, the newly elected APS Vice President, is known for his balanced concerns for resarch, education, and public affairs.

Industrial Advisors to Council. The terms of the two Industrial Advisors to Council—Chuck Hebel of Xerox and John Hulm of Westinghouse—were extended by one year. Council plans to decide whether to continue such advisors on a permanent basis. They were named on an interim basis two years ago because the normal procedures of nomination and election did not appear to result in adequate industrial representation on the Council.

"What's New". Council approved the inclusion every month in the Bulletin of the American Physical Society of a page of highlights from "What's New," the weekly Newsletter prepared by Bob Park of the APS Office of Public Affairs.

Forum Newsletter. Council endorsed the free distribution of the Forum Newsletter to interested parties outside the Forum and also endorsed the idea of soliciting voluntary contributions from non-Forum readers.

Possible move of some AIP operations to Washington. This topic stimulated considerable debate, especially on the questions of the AIP-APS relationship and the appropriate role of AIP in public affairs. A task force to be appointed by APS President Val Fitch will examine the issue and report back to Council in January.

--Kenneth W. Ford

"WHAT'S NEW"

Robert Park, head of the APS Office of Public Affairs in Washington, continues to produce the readable, informative (and sometimes provocative) weekly newsletter "What's New." It is posted every Friday afternoon on Telemail and PI-NET, and is available free to anyone who accesses either of these services on line. For instructions on access, call the Washington APS office: 202-232-0189

SCIENCE AND SOCIETY AJP REPRINT BOOK

Ten reprints of calculations on the arms race, energy, and environment, from the American Journal of Physics, by Dave Hafemeister (88 pages, non-profit). Send $3.70 plus $1.70 (shipping) to El Corale Books, California Polytech State University, San Luis Obispo, CA 93407.

PROFESSIONAL ETHICS REPORT

The AAAS Committee on Scientific Freedom and Responsibility and the AAAS Professional Society Ethics Group is introducing a new quarterly newsletter on professional ethics. To fill a need not readily met by journals that publish scholarship related to professional ethics, this newletter is devoted to publishing timely information on professional ethics issues and activities that affect a wide range of professions. The *Professional Ethics Report* will foster interprofessional dialogue on professional ethics by reporting on news and events, describing programs and activities, reviewing various resources, and publishing letters. For information about subscriptions or other matters, write to the Office of Scientific Freedom and Responsibility, American Association for the Advancement of Science, 1333 H. Street, NW, Washington, DC 20005.

NATIONAL ASSOCIATION FOR SCIENCE, TECHNOLOGY, AND SOCIETY

A new professional society, the National Association for Science, Technology and Society (NASTS) was inaugurated on 6 February 1988 at its Technological Literacy Conference in Washington, DC. The new President is Rustum Roy, former Director of the Materials Research Laboratory at Penn State and now Director of the STS Program. The new society is sponsored by a grant from the Carnegie Corporation. The Conference itself was sponsored by AAAS, ASEE, and other national organizations.

The founding of NASTS offers scientists concerned with moral, ethical, and societal issues raised by technology and science an opportunity to join their peers in the social sciences, philosophy, religion, pre-college teaching, government policy, and public interest groups, for discussion, debate and learning on topics as diverse as:

- aci rain politics and technology;
- role of science in shaping women and minorities;
- technological literacy of the non-technical 95% of the population; and
- value changes in containment of health care costs.

Members share the common concern that society must come to terms in a wholly new way with the impacts of science and technology. Members will get the NASTS newsletter; the "Bulletin of Science, Technology and Society," a journal of thought-provoking articles covering issues of concern to the field; the annual Technological Literacy Conference Proceedings; and special rates on other STS publications.

Address inquiries to: Science, Technology & SOciety Program, 128 Willard Bldg., The Pennsylvania State University, University Park, PA 16802. Phone (814) 863-1173.

JOIN THE FORUM! GET THE NEWSLETTER!

If you are an APS member it is easy, and free, to join the Forum and receive our newsletter. Just complete and mail (to the editor) the following form, or mail us a letter containing this information.

I am an APS member who wishes to join the Forum and receive the newsletter:

NAME(print)_____

ADDRESS_____

COMMENT

ATMOSPHERIC THREAT

I am writing to alert the physics community to an activity now going on that might become a serious threat to the earth's atmosphere.

In August 1987, U.S. Patent 4,686,605 was granted to Dr. Bernard Eastlund for a "Method and Apparatus for Altering a Region in the Earth's Atmosphere, Ionosphere, and/or Magnetosphere."

The idea of the invention is to generate a beam of radio waves of enormous intensity and direct this toward the upper atmosphere. At certain altitudes, electron cyclotron resonance heating of existing electrons would cause further ionization of the neutral particles of the atmosphere. Among the intended uses of the invention are to "distrupt microwave transmissions of satellites," or to cause "even total disruption of communications over a very large portion of the earth" Other intended uses include weather modification, lifting large regions of the atmosphere, and intercepting incoming missiles.

This 'skybusting' concept may sound like a tall order, but look at the power levels that will be used - 10^9 to 10^{11} watts! This is equivalent to the output of ten to 100 large power generating stations.

I have talked to the inventor, Dr. Bernard Eastlund, and he was also interviewed by Alex Chadwick recently on National Public Radio. He has carefully thought through the technical questions involved. I am not a specialist in this field, but my guess is that what he proposes is technically feasible.

I bring this to the attention of the physics community because highenergy experiments pose a danger to the upper atmosphere. In the radio interview, Dr. Eastlund said that a secret military project is already underway to study and implement the invention. Tests of this kind could cause irreversible damage. Trace constituents in the upper atmosphere can have a profound effect. We have seen this with the ozone layer. A minute concentration of ozone absorbs dangerous ultraviolet radiation. A small quantity of manmade chloroflurocarbon compounds is enough to destory the ozone layer and increase the incidence of human skin cancer, worldwide. After long negotiations, the federal government has joined in an international treaty to protect the ozone layer. A few tests of the Eastlund invention might undo all that we have accomplished with the treaty.

Effects in the upper atmosphere cannot be localized. In 1982, a Mexican volcano, El Chichon, in an unusually strong eruption, injected large amounts of volcanic material directly into the stratosphere. Satellite observations followed the material as it spread. Within a few weeks, a line of material extended around the globe. In less than a year, the stratospheric cloud had blanketed the entire northern hemisphere. The language of the patent indicates that it is clearly intended to produce effects on a similar global scale.

Besides the patent cited above, there are two other patents, classified secret, that concern other uses of the invention. For the time being, any tests will be done under Defense Department secrecy rules. If this happens, we will not know what is going on. What we do know is that secrecy always lowers the standards of environmental accountability.

With experiments on this scale, irreparable damage could be done in a short time. The immediate need is for open discussion. Any plans for tests should be publicly disclosed and the environmental consequences fully discussed. To do otherwise would be an irresponsible act of global vandalism.

Richard Williams, Physicist
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