degree of caution as our universities use, and perhaps more. The time has come for commercial vendors to stop selling supercomputer time to just anyone who can afford the $1000-an-hour tab.

At the end of May the House Committees on Foreign Affairs and on Science and Technology held a joint hearing on international science and technology policy. One of the proposals discussed was the establishment of a new executive-branch interagency committee for international science, engineering and technology. I believe this committee should be established, and that it should move quickly to resolve the important questions concerning a uniquely valuable yet vulnerable technology—our nation's supercomputers.

TERRY L. BRUCE
US House of Representatives
19th District of Illinois
6/86 *Washington, DC*

Strategic Defense Initiative

It is more in sadness than in anger that I feel compelled to respond to Richard Garwin's allegations in his letter published in the March PHYSICS TODAY (page 9). Because Garwin singled me out in his letter, I will respond on my own behalf. I would like to address two issues: the circumstances surrounding the Department of Defense's request that the Office of Technology Assessment background paper *Directed Energy Missile Defense in Space*[1] be withdrawn, and my opinions concerning submarine survivability.

On 4 June 1984 the Department of Defense asked (in a letter from W. H. Taft IV, deputy secretary of defense, to John Gibbons, director of OTA) that the OTA background paper be withdrawn because it contained "serious errors." In his March letter Garwin misrepresents the facts in this incident. He states that I managed the preparation of the "critique" done by the Department of Defense on the OTA background paper, that according to Major General John C. Toomay this critique was not competent, and that OTA had an "independent" committee review this issue. These allegations are all false.

Following the release of the OTA background paper in April 1984, the Department of Defense had it reviewed by Lawrence Livermore National Laboratory, Los Alamos National Laboratory and several SDI systems-analysis contractors. All agreed the OTA background paper was seriously flawed. As a result, the Department of Defense prepared[2] a press release on 8 May 1984. It appears that this press release was the only document Garwin read, because that is the document that was criticized by Toomay, a member of the 1983 Fletcher study, which set the baseline technical plan for SDI. At the time that Toomay wrote to OTA about his concerns, he also wrote to me about those same concerns with the press release, but asked for the detailed technical backup for the Department of Defense points. These were supplied, and Toomay has not communicated any further concerns to the SDI Organization.

The critique actually supplied to OTA was several documents, none of them prepared by me. Based on technical inputs from the national laboratories and other systems-analysis groups, the Sparta Corporation, a major SDI systems-analysis contractor, prepared[3] a consensus classified critique of the OTA background paper in May 1984. This critique was sent with the 4 June 1984 DOD request for withdrawal of the paper. A separate paper representing[4] the official position of Los Alamos was also sent to OTA. OTA was asked (in a letter from Taft to Gibbons) by the Department of Defense on 20 June 1984 to set up a mutually acceptable independent panel to review the issues. On 27 June 1984 OTA refused to consider this request and refused (in a letter from Gibbons to Taft) to reveal to DOD who the "distinguished experts" were to whom OTA had referred the matter. On 13 July 1984 OTA sent (in a letter from Gibbons to Taft) a single-page summary of their review that provided no technical rebuttal of the points raised in the DOD and Los Alamos critiques. Furthermore, the OTA reviewers turned out to be individuals who had previously raised objections to SDI on political or strategic grounds. In comparison, three distinguished experts—Robert Jastrow, former director of the NASA Goddard Institute for Space Studies; William Nierenberg, director of the Scripps Institution of Oceanography and head of Jason, a defense scientific study panel of which Garwin is a member; and Frederick Seitz, former president of the National Academy of Sciences and former president of Rockefeller University—wrote a letter (on 11 June 1984) to Senator John Warner, chairman of the Senate strategic-forces subcommittee, substantiating the errors in the OTA background paper. The SDI Organization has repeatedly tried to get OTA to address the technical issues, and has provided[5] briefings and numerous other technical analyses supporting the DOD conclusion. Even OTA has admitted (in a letter dated 31 July 1984 from Gibbons to Lieutenant General James A. Abrahamson, director of the SDI Organization) that "the issues

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with the Background Paper and critique are very much open."

Garwin originally coauthored[6] a report that stated that 2400 "standard" platforms (fixed-performance lasers) would be needed to defeat the *current* Soviet missile force of 1400 ICBMs. Garwin has since changed[7] his estimates for this problem to 79 standard lasers. He has also verified that the number of standard lasers needed scales as the square root of the number of offensive missiles added to the current Soviet offensive force. The latter fact was one of the points with respect to which the OTA background paper erred. I fail to understand why the Department of Defense is vilified as "incompetent" when all of the errors have been on the side of SDI critics.

I was appalled by Garwin's statement in his letter that "SDI's Worden has often invoked submarine vulnerability in support of SDI." I have never said that the current submarine deterrent is vulnerable. Indeed, my public statements on this issue say quite the opposite, as should be well known to Garwin. In a debate with him in Colorado Springs, Colorado, in November 1985 I said:[8]

> Today we have about 40 very expensive missile subs forming our survivable offensive deterrent. These are the forces Dr. Garwin and his colleagues love. If the Soviets could figure out some way to destroy a large percentage of these, we would be in real "hurt city." In this case, the Soviets do have something equivalent to the "space mine"—it is called the attack sub. The Soviet idea is to have the attack subs follow along with our ballistic-missile subs, and the moment the Soviets choose to strike, the Soviet attack subs would presumably destroy our ballistic-missile subs. The fact of the matter is that we are not very worried about this scenario.

As Garwin surely recalls, I went on to explain why the ballistic-missile subs are not vulnerable today. It is curious that Garwin claims I said something exactly opposite from my words. Conversely, Garwin knows that the Soviets are working very hard on antisubmarine warfare, and that national security demands that we take seriously the possibility that they might succeed in their efforts at some point in the future.

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SIMON P. WORDEN
Lieutenant Colonel, USAF
Special Assistant to the Director
Strategic Defense Initiative Organization
Department of Defense
Washington, DC

4/86

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According to Richard Garwin (March, page 9), General James Abrahamson and I err when we say that my calculations on SS-18 shielding in *How To Make Nuclear Weapons Obsolete* are correct and Garwin's calculations are wrong. Now the Martin Marietta Corporation has entered the lists against Garwin. The chart on page 91, taken from a Martin Marietta report, "Laser shielding of Soviet ICBMs: Performance implications" (unnumbered, issued April 1986), confirms the accuracy of the calculations reported in my book. Contrary to Garwin's statement, 8 tons spread over the surface of an SS-18 would cost the Soviets, in the words of the Martin Marietta report, "virtual elimination of mission capability"—that is, 9.2 warheads (7100 pounds) out of 10 according to the Martin Marietta analysis.

Details of the Martin Marietta analysis are not included in the report. My analysis used the following parameters for the SS-18: area of the first stage, 175m^2; area of the second stage plus bus, 125 m^2; mass of the first stage, 160 000 kg (initial) and 15 000 kg (final); mass of the second stage, 40 000 kg

continued on page 90

letters

continued from page 15

(initial) and 4000 kg (final); mass of warheads, 350 kg each (10 warheads carried on vehicle); exhaust velocity of first stage, 2300 m/s; exhaust velocity of second stage, 2800 m/s; velocity at second-stage burnout, 6700 m/s. With these parameters, the rocket equation is

$$6700 = 2300 \ln\left[\frac{160000 + 40000 + P + S_1 + S_2}{15000 + 40000 + P + S_1 + S_2}\right] + 2800 \ln\left[\frac{40000 + P + S_2}{4000 + P + S_2}\right]$$

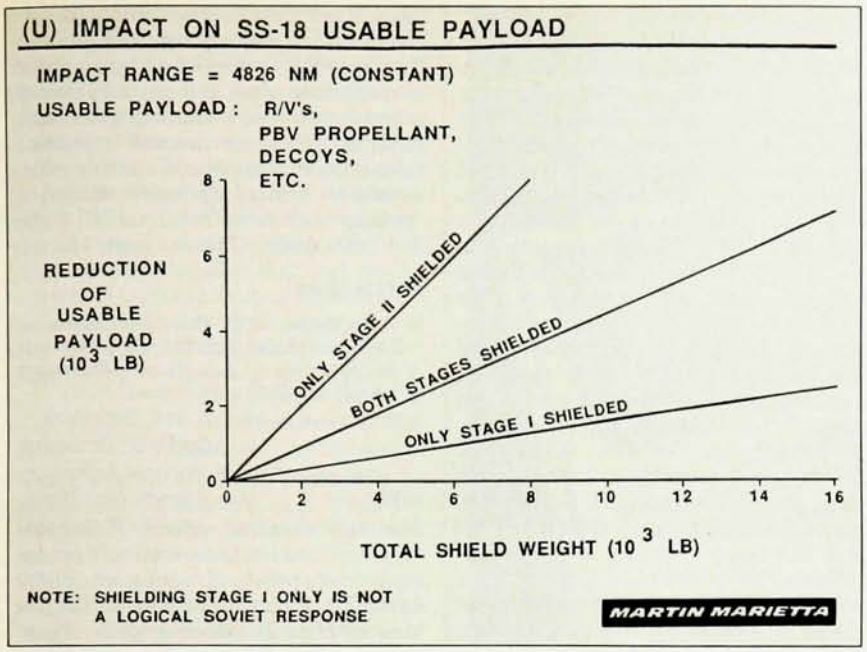
where $P(S_1,S_2)$ is the payload and S_1 and S_2 are the masses added to the first and second stages, respectively. For $S_1 = S_2 = 0$, the rocket equation yields $P = 7650$ kg, or 16 750 lb, more or less the accepted value for the SS-18 payload. For 8 tons, or 7300 kg of mass, added to the rocket skin ($S_1 = 4250$ kg, $S_2 = 3050$ kg), the equation yields $P = 4100$ kg, or 9050 lb, a reduction in payload of 7700 lb, or 10.0 warheads.

In fact, if Garwin's own analysis and parameters, as given in his PHYSICS TODAY letter, are used to do the problem, they yield essentially the same result: The Soviets must offload 10.3 warheads under these conditions.

So now three independent analyses—that by Martin Marietta, mine and Garwin's—agree on the following proposition: Eight tons spread over the skin of an SS-18 would cause the offloading of all or virtually all its warheads. How then does it come about that Garwin finds himself in conflict not only with me, but with the entire analytic apparatus on which the Defense Department relies for its information in such matters?

The answer is that Garwin has changed the conditions of the original problem in a way that yields an answer closer to his assertion. The change he introduced consisted in calculating the offload of warheads produced by spreading 8 tons *over the first stage only* instead of spreading it over the whole rocket. But this is a very different problem. Because weight added to the first stage does not affect the performance of the second stage, 8 tons on the first stage produces a relatively modest penalty for the Soviets of only 1.2 tons, or three warheads, as Garwin's PHYSICS TODAY table indicates.

Garwin's review of my book in PHYSICS TODAY (December, page 75) is so constructed that the casual reader may not notice he has switched calculations. In the review he first gives the quote from *How To Make Nuclear Weapons Obsolete* that refers to 8 tons spread over the entire SS-18, and then immediately after gives a description of his



own calculation based on 8 tons spread over the first stage only, as if these were equivalent propositions. In his response to the criticisms of his review he did the same thing. I leave it to the reader to judge the intent behind these quick switches in wording. I would call them prestidigitation.

One other point warrants mention: Shielding only the first stage, while it produces an offload penalty closer to Garwin's desideratum of one or two warheads, would be an inane act for the Soviets. Putting the shielding only on the upper stage, as I did in my calculation for the AAAS meeting, makes some sense because for several reasons there is a fair chance the first stage would burn out before our defense could engage the Soviet missile. But putting the weight on the first stage and leaving the upper stage and bus exposed to our attack would be, in the words of the Martin Marietta report, "not a logical Soviet response."

ROBERT JASTROW
Dartmouth College
4/86 *Hanover, New Hampshire*

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The political and ethical problems raised by the proposed Strategic Defense Initiative are very important, but I wish here to comment on a technical aspect of SDI that has not received enough attention.

SDI will require energy sources that must be far more compact and powerful than anything used in space so far. The notion of SDI implies a system that can very rapidly fire hundreds of energy beams powerful enough to disable armored warheads thousands of miles away; that can be fired repeatedly at will; and that is so cheap to produce and to install in orbit that a high degree of redundancy can be maintained, and also cheap enough that the nation is not bankrupted in the course of its construction. All of this seems to imply that the primary energy sources must be several orders of magnitude more powerful than chemical sources; in other words, SDI must go nuclear.

Theoretically the most attractive power source would be fusion. The mere fact that all approaches to controlled thermonuclear fusion are visionary should be no objection to including it within the purview of SDI, because SDI itself is visionary in the extreme.

It seems very shortsighted to me that many energy-related projects, including most fusion projects, have been cut back, while extravagant support goes to SDI itself (PHYSICS TODAY, April 1985, page 59). Surely a major component of SDI should be research into novel approaches to controlled thermonuclear reactions, as well as into the development of advanced fission reactors and space-based solar power. Otherwise, we might ultimately suffer the fate of the unfortunate Professor Langley, who is said to have built a workable aircraft some years before the Wright brothers but could not find an engine capable of powering his machine.

KENT PEACOCK
University of Toronto
10/85 *Toronto, Ontario, Canada*

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Robert Jastrow's response (January, page 9) to Richard Garwin's book review is strong, but not persuasive because of the possibility of continual changes by the offense.

It is not important whether any *particular* changes become important: The point is that the defense can never be sure that the offense will not find some new device to defeat it. Therefore research and development can never end. There will be no such thing as a final deployment of strategic defense. Instead the system will have to be continually upgraded to meet newly perceived threats. System maintenance, which is difficult even for a static system of such complexity, is likely to become impossible. Software may never get to the "maintenance" stage at all, requiring continual redevelopment.

Progress toward an ever receding goal of strategic defense implies a piling of weapon upon weapon, sucking in an ever greater share of resources economic, scientific and technological.

MICHAEL E. GREEN
City College of the
2/86 *City University of New York*

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It was kind of Richard Garwin to give us the rocket equation and the parameters he used in his March letter, as this allows us to run our own checks in the debate on shielding versus warhead offload. Using Garwin's assumption of 8 tons of shielding on the first stage only, with no shielding on the second stage and bus, we find that one must offload 1.14 tons, or three warheads, to obtain the final velocity of 7000 m/sec that checks with Garwin's table.

However, Robert Jastrow's calculation spreads the 8 tons over the whole missile (first stage, second stage and bus). Using the relevant surface areas (first stage, 176 m^2; second stage, 50 m^2; and bus, 72 m^2, which values may be verified simply by looking at a picture of the SS-18 and doing a little geometry using a total length of 32 m and a diameter of 3 m) we find that 4.72 tons of shielding will be on the first stage and 3.28 tons on the second stage and bus. Plugging these into the rocket equation we find that 3.98 tons must be offloaded, or all ten warheads, as Jastrow states in his book.

Why has Garwin criticized Jastrow's book with a calculation that is clearly not the one that Jastrow does? Why did Garwin phrase his March response so that if one did not run through the numbers oneself, one would think he had definitively shown Jastrow to be in error? The only plausible reason would be if there were in fact some reason to shield only the first stage. But the first stage is burning when the missile is in the densest part of the atmosphere, where it is difficult for lasers to penetrate. If anything, it would make more sense to shield only the second stage and bus. If one does this, the equations trivially indicate that one must offload 1 ton of payload for each ton of shield-

ing added: Jastrow strikes again. The Garwin calculations are simply irrelevant.

PERRY LEE ANTHONY
SAM PARK
Massachusetts Institute of Technology
4/86 *Cambridge, Massachusetts*

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One enters into debates on the Strategic Defense Initiative with some trepidation because each side appears to be right when it accuses the other side of engaging in a political and emotional discussion rather than in a scientific debate. However, the correspondence in the March issue of PHYSICS TODAY following the January letter (page 9) from Robert Jastrow commenting on Richard Garwin's December review (page 75) of *How To Make Nuclear Weapons Obsolete* requires comment.

Garwin's calculation of the payload penalty exacted by an 8-ton shielding applied to the first stage of a rocket is correct as far as it goes, but for no good reason ignores the shielding required by the second stage. Shielding applied to the last stage of a rocket really does reduce the payload ton for ton. A rough calculation suggests that 8 tons of shielding distributed between two stages cuts the payload by between 3 and 4 tons. This is not the total mass of the shielding, as Jastrow in his book and General James A. Abrahamson in his letter seem to suggest it ought to be, nor is it the whole payload unless the weight of the bus is assumed to be immutable and weapons must be offloaded first. But neither is it a negligible penalty, as Garwin apparently would have us believe, and it is in fact near the 4 tons Jastrow claims in the letter that Garwin attacks when he does his calculation.

As Garwin asserts, Gregory Canavan for a long time persisted in the error of using the average kill rate rather than the average kill time to gauge the efficacy of a single satellite. It is easy to see from the Schwartz inequality that the product of these averages is always greater than 1, so that Canavan's error was favorable to the defense. However, if the laser can always be aimed at a part of the target perpendicular to the line of sight, the error seldom exceeds a factor of 1.2 for the threats in constellations of defense satellites generally discussed. It never comes near 3. Garwin's recollection that Canavan's error was a factor of 3 is due to the earlier use of a model, now apparently abandoned, in which it was assumed that the booster surface was approximately horizontal, so that the cosine of the angle between the booster skin and the laser beam was the ratio of constellation height to slant range. In that case the error is much larger.

Hans Bethe and I, in an unpublished paper, using equations with which we believe all parties (especially Canavan and Garwin) agree, calculated[1] the logarithmic derivative of required constellation size with respect to threat rate. If this has the value $\frac{1}{2}$, the constellation grows as the square root of the threat, a situation favorable to the defense. If it is 1, the constellation and the threat are proportional. We found that the derivative increases with threat size and with the overhead due to height and retarget time. In case the parameters used[2] by Garwin apply, it has values in the ranges 0.6–0.7 for a 0.1-sec slew time, 0.8–0.95 for 0.5 sec and above 0.99 for 3 sec. In each case, the smaller number is appropriate to the smaller threat (1400 boosters) and the larger number to the larger threat (3000 boosters). Canavan's (private) reaction was that this result was closer to square-root scaling than anything he had calculated for a while. Garwin (March, page 144) says that square-root scaling is "a 'virtue' propagated worldwide by the efforts of the SDI Office, even though its derivation was faulty and it holds in no relevant parameter regime." It all depends on how close to $\frac{1}{2}$ one perceives 0.6 to be, and on whether or not one happens to believe that a 10-m-diameter beam can be retargeted (not necessarily by reorienting a 10-m-diameter mirror) in a tenth of a second.

Jastrow claims (January, page 11) that fast-burn boosters are not a suitable countermeasure because the bus must wait to leave the atmosphere before dispensing its reentry vehicles. This makes a fast-burn booster wasted effort only if the bus is as vulnerable as the booster, which seems unlikely. One can even conceive of advantages to burnout within the atmosphere, such as aerodynamic steering of the multiple reentry vehicles, which would eliminate the need for bus engines, fuel and so forth, but require guidance on each RV and make it difficult to deploy decoys.

Jastrow also points to new developments in rocket design that promise to reduce the cost of sending mass into orbit by a factor of ten. He asserts in his January letter that this reduction, together with the transatmospheric supersonic ramjet, will "irrevocably tilt the balance of cost in favor of the defense" without explaining why the offense will not benefit from at least the first of these advances as well.

Now, of course, if one is reviewing a book, one is obligated to call attention to errors one finds in it. But otherwise, instead of making, and calling attention to, bloopers of various sorts, such as errors of factors of 3 or 30 or 1600 that have been corrected or refer to abandoned models, descriptions of diffraction that make one cringe and are illustrated by examples off by an order of magnitude, and so forth, can we not attempt with good will to get the facts straight? Must we precede irrelevant calculations of payload penalties with carefully crafted phrases such as "murky trail from error to SDI-certified orthodoxy" (March, page 15)?

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ALBERT G. PETSCHEK
Los Alamos National Laboratory
5/86 *Los Alamos, New Mexico*

JASTROW REPLIES: Albert Petschek's very civilized letter unwittingly propagates one point of confusion introduced by Richard Garwin in the review of *How To Make Nuclear Weapons Obsolete.* That book and General James A. Abrahamson in his letter do not state that 8 tons of shielding on an SS-18 will eliminate 8 tons of payload. Their statement is that this amount of shielding will eliminate all the warheads on the SS-18. These weigh *in toto* about 3500 kilograms. That is, as Petschek notes, the correct answer to the problem.

The other technical points in Petschek's letter are very interesting. Regarding the fast-burn booster, Petschek says it can still be a useful Soviet countermeasure if the bus is harder than the booster. But in fact a bus is as soft as a booster, if not softer. A bus is essentially a scaled-down rocket stage with a thin-walled, fragile construction. Furthermore it has panels that must open to let the warheads out and that can easily jam under attack. Finally, it has electronics, very vulnerable to our defensive weapons, that control the timing, magnitude and direction of the impulses imparted to the warheads—all essential for accuracy. And if you try to harden the bus with shielding, it costs you heavily in lost payload—pound for pound, as Petschek observes.

If the Soviets attempt to restore the usefulness of the fast-burn booster by deploying its warheads shortly after burnout while the warheads are still in the atmosphere, they must, as Petschek notes, place guidance and control on each warhead to maintain accuracy. This makes every warhead a mini-bus. That costs more than a factor of two in useful payload. Even worse, deployment of warheads in the atmosphere eliminates the use of decoys, leaving a bare threat that enormously simplifies our defense.

Regarding the description of diffraction in *How To Make Nuclear Weapons*

Obsolete, which appalled Garwin and makes Petschek cringe, the problem here is that the layman, having seen searchlights in the night sky, has an impression of a beam of light as having a sharply defined edge between light and no light. But Maxwell's equations do not allow field discontinuities except at a boundary. Referring to this situation in terms of light "spilling over" the edge of an obstacle may not be to Petschek's taste, but it is an accurate description, in layman's language, of the behavior of the fields and the broadening of the beam by diffraction.

ROBERT JASTROW
Dartmouth College
6/86 *Hanover, New Hampshire*

GARWIN REPLIES: First I respond to Lieutenant Colonel Simon P. Worden's remarkable challenge that three allegations he extracted from my letter (March, page 9) "are all false."

▶ Did Worden "manage" the Department of Defense critique of the Office of Technology Assessment background paper?[1]

On 4 June 1986 I called Wayne Winton of Sparta Corporation (the "major SDI systems-analysis contractor" identified by Worden as preparing the critique) and asked him, "Who managed for the Defense Department the preparation of the critique of Ashton Carter's OTA background paper of 1984?" His reply: "I think it was Pete Worden, Major Pete Worden." When Worden says of the several documents of the critique, "none of them [was] prepared by me," he is hardly refuting the claim that he "managed the preparation" of this critique.

▶ Did Major General John C. Toomay (deputy director of the Fletcher committee) judge that this critique was not competent?

In a telephone conversation on 4 June 1986, Toomay told me that he has no reason to believe that his letter of 22 June 1984 to John Gibbons, director of OTA (judging that the "DOD comments . . ."[1] contained "falsehoods, irrelevancies and misinterpretations"), was in error; he reported that he "had seen no reason to change" his statement. Toomay called the DOD document incompetent, not the entire DOD.

▶ Was the 8 May 1984 press release the only document of the DOD critique that I had read?

Released to the press, this was the formal public response of the Department of Defense. However, beginning 18 June 1984, I have studied the DOD comments transmitted to OTA on 4 June 1984 (some 60 pages largely abstracted from the accompanying Los Alamos paper[2]), as well as OTA's responses to the DOD critique.

▶ Did OTA have an "independent" committee review the question as to whether the background paper should

letters

be withdrawn?

Lieutenant General Glenn A. Kent, William J. Perry and Charles H. Townes are certainly independent of OTA. They are individuals of unquestioned competence, honesty and service to DOD; any reservations they may have had about SDI would not affect their technical review of the request for withdrawal of the background paper.

What, then, remains of Worden's statement, "These allegations are all false"?

▶ Have "all of the errors . . . been on the side of the SDI critics"?

Let's look at the 8-page official "DOD comments . . . ."[1] The report states: "Thus, forcing the Soviets to go to fast-burn boosters is the same as deploying a highly effective boost-phase system." Toomay, in his letter, calls this "making an absurdity out of a respectable analytical technique."

The report states: "Technologies and techniques have been identified which will insure survivability [of satellites]." Toomay's letter says this is "simply untrue."

The report states: "The primary cost for an ICBM is the booster and the silo." However, Toomay's letter says: "A booster is a contained quantity of propellant; a silo is a cylinder of reinforced concrete. The cost of an ICBM is not primarily in these two entities."

The report states: "Second, the Fletcher study concluded that effective discrimination of decoys from warheads could be accomplished in midcourse." In his letter, Toomay says, "This statement is false."

Here is what Worden says[3] about himself and the early SDI claim that ten million lines of error-free code are necessary and feasible for a defensive system:

> Dr. Garwin raised the issue of error-free software as cited in an article by Dr. Fletcher. I must confess some guilt in those statements, as I wrote much of the material Dr. Garwin has cited. I have been taken to task for those statements by certain software engineers. One of them has said that I must have been crazy or drunk when I wrote those statements. I was probably both!

To repent of error is laudable, but it does conflict with the assertion of infallibility for oneself and one's colleagues.

▶ When I wrote that Worden had "often invoked submarine vulnerability in support of SDI," I meant precisely that he has often argued that SDI is necessary to compensate for *potential* vulnerability of our strategic submarine force. But if it is easier to *prevent* submarine vulnerability than to build

an SDI (and that is the end to which I quoted Walter Munk), or if small subs by the dozens or hundreds and Midgetman missiles in silos are the way to a survivable force, then it impairs our security to assert that an SDI of unknown feasibility, cost and survivability is the way to solve our hypothetical problem.

What Worden has not done is to provide factual information and analysis that the citizenry, Congress or the Administration would find useful in assessing the SDI program. The Carter background paper is more than two years old; there is nothing of that scope and quality available from SDI. If SDI knows no more than did Carter when he wrote the background paper in early 1984, it has been wasting our money. If it does know more, then it is not carrying out its responsibility to report to the nation. On 16 July 1984, I wrote Lieutenant General James A. Abrahamson as follows:

> Nevertheless, it would be extremely useful for the SDI to make available all of the technical information which can be provided in an unclassified fashion, so that those who wish to understand the SDI have a better chance of doing so.... Perhaps as a first step one might create an SDI reading room, while an SDI contractor wrote an analogue to the Ash Carter paper.
>
> Will you help us with this effort?

The reader may judge how much time it would have saved in this dispute about hardening if Abrahamson had made available to the readers of PHYSICS TODAY the unclassified material that the SDI Organization bought with our money, some of which information I am able to provide below.

My March calculations on payload–offload are highly relevant as a tool for evaluating lasers for boost-phase intercept. Means of calculation should be distinguished from fact, and fact from assumption. I was addressing means, not ends; let's look at the record.

At the 28 May 1984 AAAS press conference and panel discussion, Robert Jastrow asserted that "shielding an SS-18 with 4 tons of ablative material would reduce the 8-ton payload by 50%." He then "explained" this in a letter to Herb Lin (the italics are mine):

> Dr. Garwin stated that the weight was 300 kg during our panel discussion and explained the difference as being due to the fact that I had forgotten the first stage and everything smeared on it are thrown away after the first-stage burnout. However, his reasoning was incorrect. The basic rocket equation shows that one must *subtract from the payload all weight that has been added to the first stage, even though the first stage is discarded after burnout*.... Assuming this covering is spread over the upper *two-thirds* of the booster, its mass is ... 4400 kg = 4.84 tons.... Leaving the lower part of the missile uncovered is "conventional wisdom" based on the idea that the first-stage period of the flight will be in the lower atmosphere and beyond the range of our defense. In fact, the first stage of the SS-18 flight lasts long enough to carry it well out of the lower atmosphere, and there is no reason to leave the first stage *or any part of it* uncovered. I followed the conventional wisdom in the calculation reported to the AAAS.

In his current letter Jastrow says, "Putting the shielding only on the upper stage, as I did in my calculation for the AAAS meeting ..." but that is directly contradicted by his statements at the meeting and in the letter to Lin quoted above.

On 11 April 1986 I requested, and received, from the SDI Organization five unclassified charts by the Martin Marietta Corporation on "Laser shielding of Soviet ICBMs: Performance implications." (SDIO writes that the request to Martin Marietta "was verbal, so I don't have anything in the way of a 'request.'") One of these charts is shown with Jastrow's letter. Another, the "SS-18 surface area distribution," shows stage 1 (including "interstage") as 66% of the area, and stage 2 (including payload, post-boost vehicle and fairing) as 34% of the area. The final chart reads:

Interpretation of Usable Payload Reduction
Example shielding case
- Total shield weight = 16 000 lb (8 tons) distributed evenly over entire booster

Net reduction of usable payload = 7100 lb (portion of nominal stage-2 throw weight)
Simple R/V offload
- For individual R/V weight = 770 lb (350 kg)
- Payload reduction equates to 9.2 R/Vs
- Virtual elimination of mission capability

Soviet alternative
- Modify PBV configuration
- Shorten payload fairing (saves shielding weight)
- Offload PBV propellant (for missing R/Vs)
- Retained mission potential: 4–5 R/Vs (estimated)

Removing only warheads to compensate the mass of hardening is "not a logical Soviet response." Indeed, every reader can wonder whether the SDI Organization has explored an even better "Soviet alternative," which is to retain the PBV propellant and to burn it in the PBV engine to compensate speed loss due to hardening. This is preferable to propellant offload if

$$(1 + fM_2/P) < V_{2e}/V_{Pe},$$

where fM_2 is the second-stage empty

mass, V_{2e} and V_{Pe} are the rocket exhaust velocities for the second stage and the PBV ("bus"), respectively, and P is the mass of the PBV including warheads and fuel. The stage-2 velocity gain from offload of a small mass ε from the bus would be

$$\Delta V_o = \varepsilon \times V_{2e}/(fM_2 + P),$$

but PBV velocity gain from retaining and burning that fuel would be

$$\Delta V_P = \varepsilon \times V_{Pe}/P$$

More than payload–offload calculations is involved in designing a hardened missile. What is required for a uniform coat to withstand 20 kJ/cm^2 of laser heat? For a coat that can absorb the heat of vaporization of carbon, only 0.4 g/cm^2 is needed—1200 kg to cover the entire missile surface. An asymmetrically hardened missile could fly with the greater hardness toward the closest laser, and with much less hardening below, where there is no laser threat.

In preparing this response I reviewed my original chart (available upon request) presented on 29 May 1984 at the AAAS panel, which I prepared in response to Jastrow's statement at the AAAS conference that the weight of hardening anywhere on the missile must be subtracted from the payload. Because the analysis is not rigorous, I used a numerical calculation in my March letter. The model used for the chart deals not with an SS-18, but a generic ICBM, with three stages of equal velocity gain 2.3 km/sec. But perhaps because "3" is both the number of stages and the exhaust velocity in km/sec, I derived a stage-mass ratio r of 10, as befits a velocity gain in *one* stage of 7 km/sec. I took a tapered rocket like the Soviet SS-X-25, and determined the stage-area ratio to be 5. I apportioned the total shielding (Jastrow's assumed "4 tons") according to the stage areas, and maintained constant total speed by offloading a payload mass equal to the shielding mass on a stage divided by the appropriate power of r: r^2 for the first stage and r for the second stage (r assumed large). My blunder in stage-mass ratio invalidates the result of May 1984; the error is my own. The approach is invalid unless r is large, and so can't be used with the true r. I must also confess that I sometimes call 1000 kg a "ton" instead of a "tonne."

In response to Albert Petschek, I would say that only under very artificial assumptions—an infinite and uniform distribution of boosters, a flat Earth, laser battle stations deployed at ground level (or not higher than the height of booster burnout) and zero laser-retarget time—would the required number of laser battle stations grow as the square root of the missile threat. (The latter two restrictions were not explicitly mentioned[2] in the derivation.) But see how this is distorted by Worden in his letter: "He [Garwin] has also verified that the number of standard lasers needed scales as the square root of the number of offensive missiles added to the current Soviet offensive force." I did no such thing!

My *Nature* paper shows[4] clearly that " 'square-root scaling' can enter only at low satellite density or low booster density." In its analysis of distributed boosters, it relaxes only the requirement of zero retarget time, and that is why I say that my derivation (and, *a fortiori*, Canavan's) is faulty for finite deployments and "holds in no relevant parameter regime." For the real distribution of Soviet boosters, square-root scaling assumes that satellites outside the deployment area are not permitted to shoot in. Furthermore, a nation would be responding no more logically to a large deployment of laser battle stations by deploying additional MIRVed, long-burn boosters distributed over its territory than it would by hardening only the first stage.

Like Petschek, I would like to avoid adjectives or who said what and when, but the reader must judge the illumination cast by this exchange.

References

1. *Strategic Defense and Anti-satellite Weapons*, hearing report, Committee on Foreign Relations, US Senate (25 April 1984). The background paper is printed on pp. 259–338. The "DOD comments . . ." are pp. 350–353. An OTA response is found on pp. 353–355.
2. G. Canavan, H. Flicker, O. Judd, K. Taggart, *Comments on the OTA Paper on Directed Energy Missile Defense in Space*, P/AC:84-43, Los Alamos National Laboratory, Los Alamos, N.M. (8 May 1984).
3. S. P. Worden, debate with R. L. Garwin held 22 November 1985, Colorado Springs, Colorado, proceedings to be published by US Space Foundation.
4. R. L. Garwin, *Nature* **315**, 286 (1985).

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6/86

Apartheid

The propriety of publishing advertisements for faculty positions in South African universities has recently been questioned by several correspondents (October, page 148). Attention is drawn to the small-print condition regarding equal opportunity at the beginning of each advertisement section in PHYSICS TODAY.

It is not so long ago that apartheid existed in certain parts of the United States, but the propriety of publishing advertisements from universities in the deep South was never questioned. Also, most editions of PHYSICS TODAY include advertisements from employers (including the US government) who claim to be offering equal opportunity but who require candidates to be American citizens! Different degrees of equality are implied, perhaps?

I do not support apartheid but I do not support double standards either. I believe that all forms of discrimination are to be deplored everywhere, not just in South Africa.

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11/85

Third atomic bomb?

Erich Hutzler suggests in his letter (December, page 13) that in a television interview Senator Barry Goldwater may have inadvertently revealed a third combat use of nuclear weapons, in Southeast Asia.

I feel a more plausible explanation is that the senator was referring to the 15 000-pound bomb, the largest convention munition in the Vietnam-era inventory. This bomb was too large to be carried by existing bombers and was dropped from C-130 transports. Its principal use was the demolition of trees to clear landing spaces in the jungle.

In the conversation the senator did not explicitly state that the weapon used in Vietnam was nuclear; however, he did state that it was used for jungle clearing. It would appear reasonable to assume that he was discussing the 15 000-pound bomb and nuclear weapons as being the largest munitions used by US forces.

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1/86

Kondo problem

I am writing to correct an important omission from the subsection entitled "Solution of the Kondo problem (at last)" that appeared in the condensed-matter-physics section of *Physics News in 1985* in the January issue of PHYSICS TODAY (page S-22). In referring to earlier work on exact solutions of aspects of the Kondo problem, I omitted a reference to important work by Soviet colleagues. An appropriate addition to reference 1 would be the review article of A. M. Tsvelick and P. B. Wiegmann, *Advances in Physics* **32**, 453 (1983). I regret any distortion of the description of the progress made on the Kondo problem that resulted from its omission.

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