Letters

Star Wars Software

I was quite disappointed that M. Mitchell Waldrop's 9 May article "Resolving the Star Wars software dilemma" (Research News, p. 710) did not raise more questions about the assertions in the "Eastport report" or the assumptions made by that panel. The following points should be questioned.

1) The assertion that the critics of the Strategic Defense Initiative (SDI) have assumed a centralized "tightly coordinated" design. Nowhere have I made that assumption. In fact, I have explicitly discussed distributed computation. Contrary to the statements of many SDI proponents, the Fletcher panel did not propose a centralized system. Aware of the problem of survivability, they proposed a highly decentralized system. In fact, they wisely rejected a military-type hierarchy of the sort proposed by the Eastport panel because the top of such a structure would be the "Achilles' heel" of the system.

2) The assertion that the SDI critics demand "perfection." I have consistently used the work "trustworthy," explaining that what is required is a system with known, that is, predictable, effectiveness—one which we know, with great confidence is free of *catastrophic* flaws. That is far from perfection.

3) The assumption that battle stations without access to data from earlier trackings could be effective. Early studies concluded that accurate tracking and discrimination in a noisy environment would require a prior estimate of the track based on earlier observations by other satellites. The "preliminary analysis" mentioned in Waldrop's article was not published with the "Eastport report," so it could not be subject to the usual scientific scrutiny. However, as described by Richard Lipton at Stanford University, the model made the naïve assumption that the only purpose of the "coordination" was to avoid shooting at the same object twice. There are many other reasons for communication between satellites.

4) The assertion that an individual battle station in a loosely coordinated system would be simple and testable. In fact, such a station would have to perform all the functions of a centralized system. Most of the arguments made in my original papers apply to an individual independent station. The problem of controlling such a station is far more complex than the problem of building the DIVAD (Sgt. York) gun, which has been abandoned because of software problems.

5) The assertion that local errors in computer network cannot cause widespread failures. Numerous problems in existing computer networks have disproved this. Further, an error present in one station's software is likely to be present in others as well.

6) The assertion that the behavior of the network could be inferred from tests of a single element. Such an inference would be valid only if the stations had absolutely no interaction. Even if we were to design a system with zero communication between stations, experience with weapon systems shows that they interact through the effects of their weapons on sensors and targets.

In short, a careful reading of the Eastport report reveals no basis for its more optimistic conclusions. The issue of centralization is a "red herring." No serious computer system designer would consider anything other than a distributed computing system for such an application. Collections of communicating processors are known to be mathematically equivalent to a single centralized program; all the problems that can be seen in the single program can, and do, occur in the distributed equivalent. Experience has shown that distribution makes bugs harder to find. That is why computer experts consider the need for distribution to be a problem, not a magic solution to an otherwise difficult problem. The Eastport report is not a refutation of the many papers that conclude that SDI software cannot be trustworthy; it is an attempt at evasion that raises false issues and avoids the real ones.

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By identifying the strengthening of deterrence as the primary goal of Star Wars, the Eastport panel has dodged the question of whether or not a defense against ballistic missiles must actually shoot down ballistic missiles. If the goal is to develop a ballistic missile defense (BMD) system that need only have a chance of working, the engineering problem is simplified enormously. However, the desirability of a defense whose goal is to create uncertainty must then be weighed against other means to create uncertainty. For example, the Reagan Administration has noted its belief that a Comprehensive Test Ban Treaty would create uncertainty in the reliability of nuclear weapons. If this is indeed true, it would seem that a Comprehensive Test Ban would be a cheaper way of creating uncertainty in a shorter time, at lower cost, and at less technical risk than the pursuit of ballistic missile defenses.

The Eastport study overlooks a number of important points. For example, the report does not describe how to ensure that a Soviet attack conforms to a case that has been anticipated by the system designers. A "97% reliability" sounds very good, until one realizes it might refer to a defense that shoots down 100% of incoming warheads under 97% of all possible attack scenarios, and fails entirely under 3% of attack scenarios. The solid rocket boosters of the Challenger were estimated to have a catastrophic failure rate of 1 in 35; is it sound public policy to rely on a defense that is as reliable as the space shuttle?

The Eastport study places a great emphasis on simulation as a technique for validating BMD software. While simulation is a valuable tool, it is fundamentally limited. Richard Wagner (Assistant to the Secretary of Defense on Nuclear Weapons Programs) testified on 18 September 1985 to the Special Panel on Arms Control and Disarmament of the House Armed Services Committee that, while some people feel that "computer simulations and component testing" are adequate to determine the actual performance of nuclear weapons, "calculations do not suffice. There is no way to experimentally simulate the total performance of a nuclear weapon. Thus, [actual] nuclear testing contributes directly to our confidence in the reliability and operability of our nuclear deterrent by exploring the complex effects of nuclear weapons so as to insure that our systems and their basing modes do not have unanticipated or hidden flaws."

If it is impossible to simulate with confidence the total performance of a technology that is 40 years old, whose fundamental physics is well understood, and with which the United States has already conducted several hundred actual tests, how will it be possible to simulate the operation of a yet-to-be-developed technology in which the United States has zero operational experience?

The Eastport study notes that BMD software should be evolvable, fault-tolerant, testable, and so on; very few people would argue otherwise. But it makes few specific suggestions for system architecture. Those that it does make are based on unrealistic assumptions.

For example, the study notes that a 20% inefficiency results from a strategy of allocating weapons to targets on a random basis, as compared to a perfect allocation in which one and only one shot is allocated per target. This result is valid only for a leakage rate of about 70%. For a permitted leakage rate of 20%, this inefficiency jumps by a factor of 5.

The study suggests that a centralized track file containing information about all objects

(both threatening reentry vehicles and nonthreatening decoys) is unnecessary. To the extent that information about which objects are decoys is made unavailable, every object in view must be rediscriminated each time a platform is ready to fire at it. For threat clouds containing many more decoys than reentry vehicles, this seems likely to be an enormously time-consuming enterprise, and time is the one commodity in short supply during a large-scale missile attack.

Finally, the Eastport study took to task the critics of SDI software, alleging that their arguments rested on the infeasibility of developing huge amounts of perfect computer code. The critics never made such arguments. Rather, such claims came from proponents such as James Fletcher, who wrote that "Battle management for a multilayered defense is clearly one of the largest software problems ever tackled, requiring an enormous and error-free program on the order of 10 million lines of code" (*1*, p. 25).

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REFERENCES

1. J. C. Fletcher, *Issues Sci. Technol.* **1**, 15 (Fall 1984).

Response: It is apparent that Lin and Parnas, each according to his own crusade, is "responding" to his own concerns rather than to Waldrop's article or to the Eastport report.

In response to Lin's concern about "deterrence as the primary goal," the word "deterrence" appears only three times in the Eastport report, once in quoting a White House paper and twice in reference to the strategic offense. The Eastport report discusses only technical and management aspects of the Strategic Defense Initiative. It was not our charter to propose national policy. The only mention of national policy in the Eastport report is to acknowledge that the architecture and design of a strategic defense system must conform to national policies as they become established.

The only specific technical point that Lin raises is the 20% multiple shot inefficiency from a "random" allocation of weapons to targets. By some inexplicable logic, incorrect in any case, Lin states that "This result is valid only for a leakage rate of about 70%." A leakage rate of 70% could occur only in an extremely target-rich and weapon-poor circumstance, in which case the chance of more than one weapon shooting at a single target is nearly zero. Also, the weapon allocation discussed in the Eastport report is not "random," but is a comparison of allocation based on locally available versus global information. The 20% result, which is also sensitive to weapon accuracy, was obtained by several independent simulations and analyses, none of which assumed such large leakage.

Lin suggests that "the Eastport study took to task the critics of SDI software." As Waldrop reported, the panel was not shy about "taking to task" the way in which the Department of Defense procures the design of high-technology weapons. However, the report makes no specific mention of technical critics or criticisms of SDI. Thus the report could hardly be described as "alleging" (Lin) or offering "assertions" (Parnas) about what the SDI critics have been saying. The panel had its own priorities in what to study, and its main conclusions were well summarized in Waldrop's article. *Science* readers will draw their own conclusions about whether the panel's priorities and recommendations make sense.

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Today's Biotechnology

Daniel E. Koshland's invocation to the 13 June Biotechnology Issue (Editorial, p. 1313) introduced a disquieting note. Few knowledgeable scientists would dispute the promise of biotechnology, yet many of us advocate prudence in releasing engineered organisms. The world of Pasteur is long gone. Today's biotechnology researchers have vastly more information upon which to draw; they are far less constrained by equipment, methodology, and speed of communications. But the public that funds their research is anxious about new technologies, with risks that emerge mainly in hindsight, and wants assurances. Unfortunately, given the disparity in funding among the various life sciences, the ability to develop new organisms has outstripped the ability to predict the consequences of their release. Research on these consequences and improved communications among researchers in all the relevant biological disciplines are essential before the public will accept the safety of biotechnology.

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Indirect Costs and Starter Grants

The Policy Forum by R. M. Rosenzweig and P. D. Boyer (20 June, p. 1508) presents helpful views on the difficult issue of indirect costs. One matter that both authors do not discuss is that, in some universities, some of the indirect cost money is recycled to provide starter grants and facilities to new faculty members that provide a basis for winning major research grants or for more established scientists who wish to break new scientific ground. What is to happen to this pool of money in view of changes proposed to fix indirect costs? If the new policy reduces funding available for these starter grants, it could lead to critical shortfalls in scientific research that would be of major significance to the national interest.

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Sheep, Goats, and the History of Psychology

Michael M. Sokal's review (2 May, p. 664) states that familiarity with John M. O'Donnell's 1979 dissertation "The Origins of Behaviorism" has separated the sheep (who actively investigate psychology's past) from the goats (who regularly indoctrinate thousands of undergraduates with its mythology through required "history and systems" courses).

Anyone who has first-hand experience with sheep and goats or who knows about their historical association in biblical times should recognize that Sokal has mixed his species up.

Sheep are far less likely to actively investigate anything than goats, who are cursed for their inquisitiveness and ability to get into places where they are not wanted. By contrast, domesticated sheep seem only to know how to stay in a flock. Sheep are more valuable than goats for their wool and mutton. Goats can, however, serve as useful eyes and brains of the flock. Therefore, before the slaughter it is necessary to separate the sheep from the goats.

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Erratum: In Constance Holden's News & Comment article "Giving mental illness its research due" (30 May, p. 1084), a study by Otto Wahl mentioned at the bottom of column 1 on page 1085 is to be published in the *Journal of Community Psychology*, not the *Journal of Community Psychiatry*, as stated.