IEEE spectrum

features

THE INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS, INC.

the cover

Standing on the tongues of flame and exhaust gases that thrust it skyward at 13000 km/h, the Spartan missile shown undergoing test launching in this U.S. Army photograph is one component of the much-disputed Safeguard ABM system. More information on this and other components of the system, and on the dispute itself, is available beginning on page 24.

departments

THE INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS, INC.

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Spectral lines

The road to recognition. We noted here a month ago that many electrical engineers regard themselves as belonging to an inadequately recognized profession, and that they take unionism to be inconsistent with professionalism. The road to recognition as a profession is well traveled and well marked.

To avoid emotional involvement, consider not engineers but mudpuddlers. If mudpuddling has been going on for a couple of generations and the mudpuddlers feel that their status as professionals is not adequately recognized, their first move is to form an *ad hoc* group that writes down a Code of Ethics for mudpuddling. The second step is to set up a Board of Certification, which gives examinations to confirm the competence of practicing or aspiring mudpuddlers. This differentiates In mudpuddlers from others. To avoid Outcry, it is usual to have a "grandfather clause," permitting any then existing Out to become an In upon payment of a fee. The results are:

(1) a body of Certified Mudpuddlers subscribing to a Code of Ethics;
(2) an Out group consisting—at the moment—of practically nobody.

The next step, taken after a decent pause, is to promote legislation that protects the public by making it illegal for mudpuddling to be done by anybody but certified Mudpuddlers; certification is thereby converted into a license to practice, and Outs are well and truly out. Thereafter—for the sake, naturally, of improving the protection of the public—the examinations may gradually get harder. To convince everybody that mudpuddling is an exacting profession (and by this time there is no doubt that mudpuddling does rank as a profession) the prerequisites for the examination are gradually lengthened. After the examination, there may be a period of internship. When the training period for mudpuddling is nearly as long as the training period for feesicians, simple justice demands that the financial rewards of mudpuddlers—including the grandfathers—be raised to a realistic level.

Along this road, electrical engineers have traveled farther in some countries than in others. In the U.S., many states provide examinations and licensing for the status of Professional Engineer. These examinations, though doubtless well suited to testing the competence of a mechanical or civil engineer, examine mainly on matters irrelevant to electrical engineering; they are not meaningful tests of competence in our field. In some states, an electrical engineering speciality is recognized. The trend is in the right direction, but the present situation is unsatisfactory. For what it is worth, my opinion is that the Board of Directors of IEEE should work vigorously for the establishment, in all U.S. states, of a status as Professional Electrical Engineer, to be awarded on passing an examination that is strictly relevant to electrical engineering.

The IEEE does now advise on the setting of examinations for the title of Professional Engineer, and it might well contribute, in larger measure, to setting the standards for the proposed status of Professional Electrical Engineer.

We have yet to take the customary first step toward higher prestige: adopting a Code of Ethics. As noted by Alger and Holt in this issue, a code intended to be suitable for all engineers has been drawn up by the Engineers' Council for Professional Development, and it has been subscribed to by many of the engineering professional societies, but not by IEEE. This code has been considered at length by the IEEE Executive Committee, which objects not only that is it unenforceable, but also that it promises performance that an employed person may not be able to deliver. In principle, of course, if a man's employer should make demands that are inconsistent with the proposed code (possibly through "buying" work by bidding less than will cover the cost) the man can quit the employer. That's fine, but if the employee has such economic strength that he can quit at will, then he needs no apparatus to support his professionalism. It is the people who cannot just quit that need the organization. Here, in my opinion, we have the real reason that electrical engineering is not recognized on all sides as being a profession. When you get down to fundamentals, a professional in the traditional sense is typically not dependent for a long term on a single employer. Ideally, he is in business for himself—perhaps with one or two partners—and works for fees. Most electrical engineers are on salary, and most of them very much hope to remain so. It might be helpful if this fraction of the profession would stop looking wistfully toward the American Medical Association, and would consider the benefits that have been garnered for salaried members by the American Association of University Professors. The status of Professional Electrical Engineer is desirable, but it seems destined to be useful principally to those who work for clients rather than for long-term employers.

J. J. G. McCue

As a service to the members of IEEE, and
as a service to the larger society of which they are also a part,
IEEE SPECTRUM presents a debate and staff report on

The antiballistic missile

Against the ABM, page 40 *George Rathjens* *Massachusetts Institute of Technology*

For the ABM, page 46 *Donald Brennan* *The Hudson Institute*

The ABM controversy is without parallel in the annals of modern weapons systems. And it is not really about ABM. It is about the U.S. way of life and about the grim facts of life and death. Not all of these facts are technical ones, but technical men—and others—must assess all of them in any case, for technology has created a precarious, but not hopeless, situation for which human history has provided no precedent. Technology has created a world in which men must soon decide whether they will live together or die together. There is no longer any middle ground.

Report on the ABM

Seymour Tilson *Staff Writer*

By the time you read these words, the United States Senate, after a spring filled with much preliminary debate, probably will have settled the latest round in the unprecedentedly acrimonious national controversy over the antiballistic missile. The Senate must decide on the Nixon Administration's request for $800 million this fiscal year with which to begin deploying an ABM system named Safeguard. Its decision will not end the debate. Like most previous decisions in the difficult arena of national security—whether made by Congress, the Defense Department, or the President—this one too will leave the losers alive and determined to fight on. Whether the nuclear war, that sane men on all sides of this continuing controversy hope to avoid, would leave even the winners in such relatively satisfactory shape is a moot point. But it goes to the heart of the controversy.

Because this question of "Who and what would survive a nuclear war?" has remained unanswerable to such untutored minds as mine since the end of World War II, until rather recently I, like most of you, no doubt, paid only passing attention to the great ABM debate.

The end of a peaceful editorial spring

I had ignored the ABM debate despite the fact that the implications of the Safeguard decision were, and continue to be, grave. Involved are fundamental and much-overdue questions about the long-accepted strategy of mutual assured destruction as a deterrent to nuclear war. The decision also raised fundamental questions about the allocation of U.S. resources, about the way in which crucial decisions are made in a democracy, and about the role of the technical community in contributing to these decisions. Yet despite my deep personal interest in such weighty and fateful questions, my editorial lack of interest in the ABM hassle persisted well into the spring just past.

After all, I must have reasoned subconsciously, if the Safeguard decision would in fact enhance my security as its proponents assured me it would, I could succumb to spring fever with greater peace of mind than ever. (And what a lovely spring it was!) If the decision was wrong, if it moved us all a little closer to doomsday as its opponents assured me it would, all the more reason to enjoy to the full the gift of one more spring.

Besides, I didn't have to pay attention. My income was relatively independent of the contractual ebb and flow associated with the development and deployment of complex weapons systems. Many of yours are not. This fact suddenly became important as a political consensus of sorts began to emerge from the facts and fantasies unveiled to public view as the ABM debate intensified.

together former Government leaders, foreign policy scholars, experts on weapons technology, economists, Senators and Representatives to investigate the actual enormity of that "misplaced power" of which President Eisenhower warned.

. . . the national security bureaucracy . . . is composed of the Armed Services, the Central Intelligence Agency, the National Security Agency, the Atomic Energy Commission, and other bodies provided for in the National Security Act of 1947, and it is closely linked to the aerospace and armaments industry, segments of the labor movement, and a new middle class of scientists, engineers, businessmen, and universities with defense research contracts. This complex is not a conspiracy; it is an enormous, self-perpetuating institutional organism. It receives such a disproportionate amount of Federal funds that there is no effective counterbalance to it, and such decisions as those on Vietnam and ABM are generated from institutional momentum rather than conscious policy decisions . . .

The urgency of our concern is underscored by the critical juncture at which we stand in the development of nuclear weapons. The reason we called for the postponement of ABM deployment, a moratorium on testing of MIRV (multiple individually-targetable re-entry vehicle), and immediate commencement of strategic arms talks with the Soviet Union is that the time for such talks may soon pass the point of no return. Because of the impossibility of detecting the number of warheads inside the deployed missiles we will reach a stage in a few months when neither nation will be able to accept a limitation on its strategic force. The Soviet Union has been pressing for such talks, and we have been putting them off while we complete testing.

A profound indication of the acceleration of the arms race is the fact that our own military strategists are presently engaged in a debate to shift the basic question of defense policy from preventing nuclear war to surviving it . . .

So said Senators George McGovern and Gaylord Nelson, and Representatives George Brown, Jr., Philip Burton, John Conyers, Jr., Don Edwards, Donald Fraser, Robert Kastenmeier, Benjamin Rosenthal, and William F. Ryan. The Congressional Conference on the Military Budget and National Priorities of which they spoke was sponsored by 53 Congressmen—14 Senators and 39 Representatives—of both parties, and support has grown.

Other, equally patriotic and deeply concerned Americans, including the President, acknowledged the existence of the military–industrial complex and came to its defense. One of its defenders was Senator Barry Goldwater, a member of the Armed Services Committee and a retired major general in the Air Force Reserve. He took the Senate floor on April 15 to defend the size of the U.S. military–industrial complex and the scientific, academic, and economic communities that assist in military work. Excerpts from his address were published in *Science*, the weekly journal of the American Association for the Advancement of Science:

. . . I am greatly interested in the growing preoccupation of some groups and individuals these days with the so-called military–industrial complex in the United States. Indeed, if I were a psychologist, I might be tempted to the conclusion that the left wing in American politics has developed a "complex over a complex." . . .

Rather than deploring the existence of a military–industrial complex, I say we should thank heavens for it. That complex gives us our protective shield. It is the

Enter, the military–industrial complex

Ten United States Congressmen—two Senators and eight Representatives—writing in the June issue of *The Progressive* (a monthly that was founded in 1909 by Robert M. LaFollette, Sr., the Wisconsin champion of liberal causes) described this developing political consensus and its relationship to the ABM issue:

On March 28, 1969, two separate but ironically related events occurred which insistently pointed to the most urgent public issue of our time: the role of the military–industrial establishment in the United States.

The first event of that day was the death of Dwight David Eisenhower, himself a hero of the American military heritage. As a departing President he had startled the nation with his Farewell Address, in which he warned of a military establishment supported by an immense arms industry which "has the potential for a disastrous rise of misplaced power." Eight years later his words take on new import—after at least $500 billion dollars sunk in military expenditures, a disastrous war in Vietnam, a senseless intervention in the Dominican Republic, more than forty-two treaty commitments to as many countries to intervene "in case of aggression"—all this while acute poverty and distress persist within the United States itself.

These misplaced priorities were the setting for the other event of March 28, the Congressional Conference on the Military Budget and National Priorities, which brought *bubble under which our Nation thrives and prospers. . . .*

What is more, I believe it is fair to inquire whether the name presently applied is inclusive enough. Consider the large number of scientists who contributed all of the fundamental research necessary to develop and build nuclear weapons and other products of today's defense industries. Viewing this, should not we call it the "scientific–military–industrial complex"?

By the same token, do not forget the amount of research that has gone on in our colleges and universities in support of our defense-related projects. Maybe we should call it an "educational–scientific–military–industrial complex." . . .

What we are talking about, Mr. President, is an undertaking which grew up from necessity. . . . Its ultimate aim is peace in our time, regardless of the aggressive, militaristic image which the left wing is attempting to give it. . . .

Many other Congressmen, and other prominent Americans, also came to the spirited defense of military research, development, and deployment. They saw such efforts as peace-keeping necessities and as guarantors of our security against threats posed by foreign enemies. Nevertheless, it became disturbingly clear in the course of the ABM debate that, to increasing numbers of other Americans and their Congressional representatives, you—the "amoral" technician—may be the enemy most to be feared. Whether or not it is a rational response to the decidedly noninfinitesimal cost-calculus of the nuclear arms race, to these Americans of all political persuasions, you, and the men in the industrial and military establishments of all nations who buy your technical talent and count on your moral neutrality, appear to be the sole beneficiaries of the world's unending preparations for the war that may indeed be fought, but almost certainly never won.

The history and budgetary aspects of the ABM debate could lend support to either point of view.

A capsule history of twenty years

The record of the last two decades is rife with escalating defense costs and studded with systems having a high rate of technological obsolescence, though many technical people would assert that at least these efforts have bought peace in our time, precarious though it may be.

Although Russia and the United States through this "peaceful" period have continued to base their defensive strategies on the supposed deterrent power of their mutually massive retaliatory nuclear capability, both sides have sporadically explored the possibility of developing an effective missile defense system. The immediate objective of these explorations, to reduce casualties should war come, is enormously attractive in its own right. On the strategic level, moreover, an ABM system that could nullify a substantial portion of a potential enemy's nuclear striking power might serve to tilt the world balance of power. Yet the record of attempts to develop a nuclear defense is not especially encouraging. In the 1950s he United States spent $30 billion on bomber defenses that were later acknowledged to have been of only very uncertain effectiveness. In the 1960s the United States spent another $20 billion on ABM research and development, but in every instance—until now—it abandoned the idea of deploying the defensive systems spawned by the R&D process, when it became obvious that, years before they could have been deployed, they would have been rendered obsolete by expectable Russian advances in offensive missile technology.

This expenditure of $50 billion may have accomplished something, however, because despite these apparent failures, pro-ABM pressure on Congress and the White House continued to increase through the past decade prompted by the belief held by many that now, finally, we might have a system that would work. It was this pressure that led not only to the decision to deploy Safeguard, but to what may prove to be, over the long term, the most significant result of the antimissile debate—a widespread public and Congressional backlash to the power, imagined or real, of the military–industrial complex.

The genesis of an IEEE Spectrum story

Concern over this aspect of the rapidly developing trend toward assessing the consequences of technological development in our open society wasn't the only reason why, when Jerry McCue (the Editor of IEEE SPECTRUM) called my attention in mid-April to the news release from the American Institute of Physics here reproduced as Fig. 1, I told him I had already seen it, and had marked it, as you can see. Another reason for my interest, one shared with Jerry as it turned out, was that I was more than academically interested in seeing how a responsible professional group like the American Physical Society attacked the problem of providing both its members and the U.S. public with impartial insights into controversial political problems with high technical content. (Indeed, a few weeks earlier, at the IEEE International Convention, I had wondered at the absence of such a timely topic as ABM, which was already making headlines in the general press, from the program of a professional society whose membership includes some of the most technically knowledgeable people in the field.)

"Technical Aspects of the Antiballistic Missile System" was the straightforward title of the AIP press conference scheduled for the afternoon of Tuesday, April 29, and of the full-dress APS technical session scheduled for later that evening.

Think of it! In one afternoon and evening of listening to the experts, pro and con, I could catch up on a debate I had managed to ignore for years.

Yet none of these was my real reason for going, or Dr. McCue's real reason for thinking along a parallel track. The compelling reason was that the debate could be ignored no longer.

Following President Nixon's decision to proceed with a slightly altered version of the previous Administration' Sentinel ABM system (more about both systems presently), the debate had escalated in intensity to the point where much information hitherto unavailable because of security restrictions was being released and used by both sides. Such diverse publications as *Foreign Affairs*, *The New York Times*, *Science*, *Scientific American*, and the *Bulletin of the Atomic Scientists* were covering the techn. cal and political charges and countercharges with regularity and responsibility. Indeed, as far back as last November, even *Congressional Digest* had featured the controversy over the U.S. antiballistic missile system.

IEEE SPECTRUM is the journal reaching those members of the technical community most intimately involved i· developing the equipment designed to serve as the ner· vous integument and brain behind the nuclear punch carried by ballistic missiles and countermissiles. Could we

NEWS FROM THE AMERICAN INSTITUTE OF PHYSICS
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OFFICE OF PUBLIC RELATIONS
Eugene H. Kone, *Director*
Audrey Armstrong, *Assistant Director*

FROM: PRESS ROOM, SPRING MEETING OF THE AMERICAN PHYSICAL SOCIETY
ADAMS ROOM
Sheraton-Park Hotel, Washington, D. C.
Telephone: (202) CO 5-2000
Meeting Dates: 28 April through 1 May 1969

PRESS CONFERENCE SCHEDULE

MONDAY, APRIL 28

10:00 A.M. Session BA1-BA3--"The Moon," Dr. Charles Townes, University of California, Berkeley, Chairman.
BA1 "The Apollo VIII Lunar Flight"--Col. Frank Borman, NASA.
BA2 "Lunar Gravimetrics, Mascons and Possible Implications"--Paul Muller, Jet Propulsion Laboratories.
BA3 "Apollo XI Laser Ranging Retro-Reflector Experiment: New Precision in the Study of the Earth--Moon System"--Carroll Alley, Jr., University of Maryland.

TUESDAY, APRIL 29

10:00 A.M. Session DC1,2,4--"Space Astronomy," Dr. George F. Pieper, NASA Goddard Space Flight Center, Chairman.
DC1 "Long-Wavelength Radio Astronomy"--Robert G. Stone, NASA Goddard Space Flight Center.
DC2 "Far Infrared Detection of the Galactic Center"--William E. Hoffmann, Goddard Institute for Space Studies.
DC4 "Microwave Emission from NH_3 and H_2O in the Galaxy"--David M. Rank, University of California, Berkeley.

11:30 A.M. Reception and Luncheon (Continental Room), Washington Group, National Association of Science Writers. Presentation of AIP-U.S. Steel Foundation Science Writing Award in Physics and Astronomy to Walter Sullivan, Science Editor of The New York Times. Address by Dr. Myron Tribus, Assistant Secretary of Commerce for Science and Technology on "Toward a Livable Man-Made World."

3:00 P.M. Session 1-4 (Continental Room)--"Technical Aspects of the Antiballistic Missile System," Dr. Luis W. Alvarez, Chairman.
1 "Technical Aspects of ABM and Penetration Aids"--Hans Bethe, Cornell University.
2 "Defense Versus Retaliation"--Eugene P. Wigner, Princeton University.
3 "ABM and the Dynamics of the Arms Race"--George W. Rathjens, Massachusetts Institute of Technology.
4 "In Defense of Missile Defense"--Donald G. Brennan, Hudson Institute.

Member Societies: *American Physical Society* • *Optical Society of America* • *Acoustical Society of America* • *Society of Rheology*
American Association of Physics Teachers • *American Crystallographic Association* • *American Astronomical Society*

FIGURE 1. This news release from the American Institute of Physics triggered our coverage of the ABM debate. At the 3 P.M. press conference, speakers 3 and 4—George Rathjens and Donald Brennan—previewed arguments they offered later that evening at the American Physical Society's formal session; evening talks provided the articles on pages 40 and 46.

remain aloof any longer?

I went to the Physical Society meeting, carrying a portable tape recorder.

Jerry and I had tentatively agreed with George Rathjens and Donald Brennan, opposing speakers numbers 3 and 4 at the Physical Society press conference and technical session, that we would tape-record their remarks. Our collective hope was that Rathjens' and Brennan's edited versions of the transcribed tapes would provide IEEE SPECTRUM's readers with a pair of articles that would provide a full and balanced review of all elements of the ABM controversy. These hopes were largely fulfilled, as the articles beginning on pages 40 and 46 attest.

However, as I began to learn at the APS meeting, and as I learned in tracking the debate subsequently, there was more—much more—to the story.

Context of the current controversy

The Nixon Administration had described the Safeguard system as limited, effective, and nonprovocative, and had asserted that its phased deployment beginning at this time was essential to the nation's continuing security in the nuclear age. Its total cost of installation was estimated initially at $6.6 billion over the next several years. Between March and May of this year, however, this preproduction estimate nearly doubled—it is now about $10.8 billion.

In deciding to proceed with the long-deferred deployment of an ABM system, President Nixon responded to increasingly grave intelligence assessments of the nuclear attack capabilities of the Soviet Union and China. But an additional objective of deployment, he said, was to provide the people of the United States with "protection against the possibility of accidental attacks from any source." The increasingly grim facts of international life appeared to justify this additional, but not entirely new, concern. Other nations were nearing nuclear readiness.

Item: Luther J. Carter, Science, December 6, 1968

When the nonproliferation treaty (NPT) was signed on 1 July [1968] by the United States, the United Kingdom, and the Soviet Union, the prospects for arms control had seldom looked better. As this important first step toward ratification of the treaty by its sponsors was being taken, President Johnson announced that the United States and the Soviet Union had agreed to begin, in the near future, talks on limiting and reducing strategic arms, including both offensive weapons and antiballistic missile (ABM) defense systems. The nonproliferation treaty itself, besides forbidding the nonnuclear states to receive or manufacture nuclear weapons, forbids the nuclear powers to assist such states in acquiring nuclear arms and, further, pledges these powers to negotiate to end the nuclear arms race.

Today, less than a half year later, the NPT itself may be in danger and the prospects of the United States and the Soviet Union's undertaking productive arms control talks seems highly uncertain. Although almost 80 nonnuclear nations have signed the treaty, only two of these signers—Canada and Sweden—are among the half dozen or so nations having a scientific and industrial base strong enough to give them the option to become members of the nuclear club. Japan, West Germany, Italy, Israel, and India, for example, have not yet signed.

Luther Carter's reportorial perspicacity and President Nixon's responsible concern about the possibility of accidental attacks from sources other than Russia and China were both underscored by a short notice that appeared in *The New York Times* on April 1, 1969.

Item: Philip Shabecoff, The New York Times, April 1, 1969

TOKYO, March 31—Japanese nuclear scientists have developed their own techniques for the production of enriched uranium, which in sufficiently high concentrations can be used for the manufacture of nuclear weapons.

Because of Japanese sensitivity to anything having to do with nuclear energy, today's disclosure—by the Government's Science and Technology Agency—has aroused considerable excitement in this country.

Japan has not yet signed the treaty to halt the spread of nuclear weapons and there is a hard core of opposition to the treaty in Tokyo. . .

The President's fears were focused on far horizons when he decided to deploy Safeguard. The eyes of Safeguard's opponents were also fixed on the developing dimensions of the international arms race as the year unfolded. Safeguard's opponents had not only described the system as open-ended and ineffective, but as an extremely provocative step to take at this particular time, when assessed in conjunction with impending dramatic improvements in our offensive capability and in the context of long impending talks with the Soviet Union aimed at limiting or ending the by-now increasingly worldwide race to possess nuclear arms.

Safeguard's opponents argued that in addition to complicating arms-control talks that already promised to be complicated enough, expanded but still ineffective versions of the initially limited Safeguard system could eventually cost $25–100 billion, or more, and could lead to what some of them called a "national security state," in which the psychological preoccupation with war would not only preclude paying attention to pressing domestic problems but would make war more likely.

New technological developments that first came to widespread public attention as the ABM debate grew more intense, in fact made it seem likely that any state seeking security in the nuclear age might have to preoccupy itself with little else.

Before the historic vote on the nominally defensive ABM measure was taken this summer, this unprecedented technical–political debate had widened in scope to include the testing and deployment of offensive devices known as MIRVs.

New doomsday device, or an added deterrent to war?

With MIRVs (the acronym stands for Multiple Independently-targetable Reentry Vehicles), a single intercontinental ballistic missile would be capable of carrying and firing anywhere from three to a dozen or more thermonuclear warheads at as many separate targets. Figure . shows in a schematic way how MIRV might work. The MIRV issue became intertwined inextricably with the ABM issue for a very simple reason. The United States started developing MIRV in a serious way several years ago, when it began to suspect that the Soviet Union was developing an ABM system to defend its cities. At present the United States is developing a three-warhead MIRV for its land-based advanced Minuteman III ICBMs, and a 10-warhead MIRV for its submarine-launched Poseidon missiles.

As a justification for proceeding with Safeguard, ABM proponents pointed to the possibility that the Soviet Union might be developing more or less comparable MIRVs for its large SS-9 intercontinental ballistic missiles (Fig. 3). This concern, that the Russians might in fact tr to overcome our nuclear deterrent by placing MIRVs on their SS-9, was a large factor in the Nixon Administration's desire to deploy the Safeguard system, in which the initial deployment of countermissiles would provide some close-in protection for Minuteman bases, the presumptive "first-strike" targets for Soviet MIRVs rather than for cities. The same concern assertedly unde lay the Administration's desire to develop MIRVs, to ensure that U.S. missiles surviving an attack by the Russians could overcome such defenses as they had deployed and penetrate to their targets, presumably Russian cities.

Safeguard's opponents countered these arguments b turning them around. They pointed out that, from th Russian side, our long lead in MIRV development could be interpreted to mean that we were already well on the way to obtaining the first-strike capability we feared they might someday obtain. Safeguard's opponents also argued that if MIRV development by both the United States and the Soviet Union could be stopped or postponed by impending arms negotiations, the urgency for either side developing an ABM system would diminish.

This argument suffered a potentially severe blow in mid-June. Until then it was generally assumed that the Soviet Union was testing a less sophisticated version of multiple-warhead missile than we were. It was believed that theirs carried a three-part warhead, all three elements of which would land near one another in a fairly tight pattern focused on only a single target. This buckshot approach to overwhelming a city's nuclear defenses carried the acronym MRV. It lacked the lethal "I." But in mid-June a new analysis of the Soviet test program, made primarily by intelligence personnel in the Pentagon, suggested that multiple warheads now being tested by the Russians may in fact be capable of being guided accurately to three separate targets—that they might be MIRVs instead of MRVs, in brief. These projections of potential Soviet capability also surmised that the Soviet MIRVs might be powerful enough to destroy the blast-hardened underground silos that contain the fixed, land-based portion of our diversified attack-missile force.

Arms control implications of ABM and MIRV

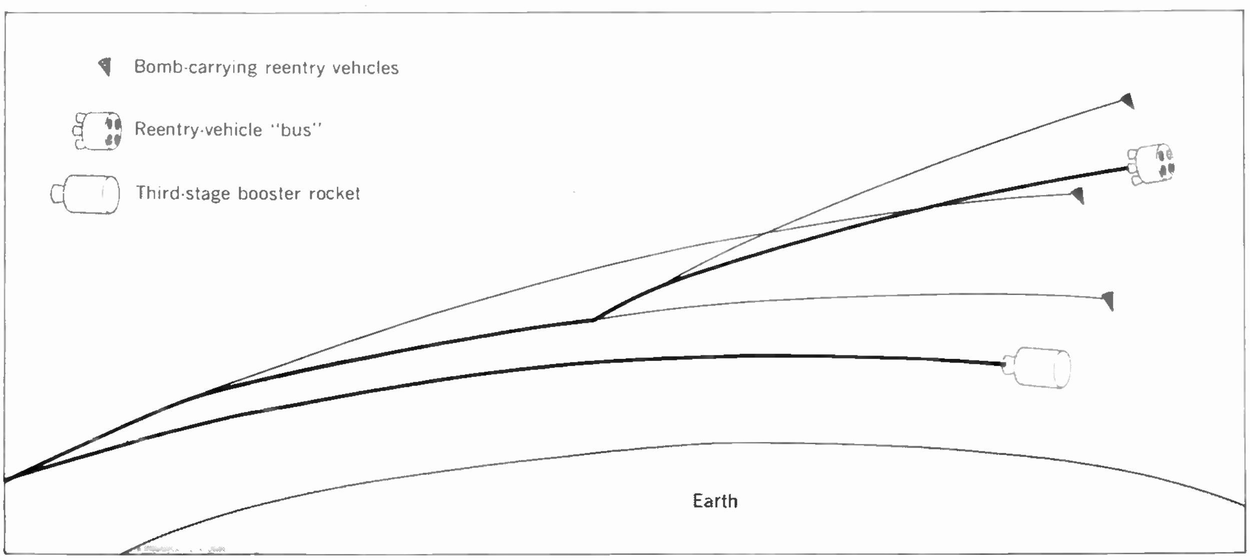
The MIRV component of the ABM dispute raised the possibility also that fears of a runaway nuclear arms race, leading to only one foreseeable end, might indeed be justified. Arms-control proponents, including those opposed to and in favor of the Safeguard ABM, reasoned that as long as missiles were armed only with single warheads, it would be relatively easy to check any missile limitation agreement unilaterally, with reconnaissance-satellite photography. This would eliminate the formerly pressing need for on-site inspection, the issue that has proved a formidable stumbling block in arms-limitations talks for many years. But when and if MIRVs were deployed, satellite pictures would not suffice; they could not reveal how many warheads might be mounted on an emplaced ICBM. On-site inspection would be the only way to check this in any arms-control arrangement—an inspection method that could prove to be as politically objectionable to the United States as it always has been to the Soviet Union. If the development of MIRV was to be stopped, therefore, arms-control advocates in both the Administration and among its critics felt it would have to be done while MIRVs were still undergoing testing. Using various optical and radar tracking methods, both the U.S. and the U.S.S.R. could unilaterally assure themselves with reasonable certainty whether the missiles being fired in their respective ocean test ranges carried multiple warheads.

This concern that it might be too late to stop MIRV and ABM if the arms talks with the Soviet Union that have been impending since last year were delayed for very much longer, led Republican Senator Brooke of Massachusetts and forty other Senators to introduce a bipartisan resolution early in June calling upon President Nixon to suspend MIRV testing and enter into prompt arms negotiations with the Soviet Union.

By mid-June, officials in the Pentagon, State Department, and White House were suggesting that it might already be too late to keep the MIRV genie bottled up. Then up popped the genie.

Item: John W. Finney, The New York Times, June 27, 1969
WASHINGTON, June 26—Without any public announcement the Air Force last week ordered the controversial

FIGURE 2. Multiple, independently targetable, reentry vehicles—MIRVs—that would render each ballistic missile capable of carrying anywhere from three to a dozen or more nuclear warheads to as many separate targets, threatened to upset the strategic balance of power on the eve of impending arms-control talks between the U.S. and U.S.S.R. The Nixon Administration wanted to proceed with initial deployment of ABM at two U.S. ICBM bases because it feared that by mid-1970s Soviet MIRVs might threaten the underground portion of our retaliatory attack-missile force. ABM opponents asserted we were so far ahead of Russians both in MIRV development and in diversification of our retaliatory force that ABM was not needed at this time. Arms-control proponents, some for and some against the deployment of ABM now, feared that because MIRVed missiles couldn't be detected unilaterally by satellite reconnaisance photography, arms agreement would be harder to arrange.



> *multiple warheads for its Minuteman 3 intercontinental missiles.*
>
> *The Air Force awarded an $87-million contract to the General Electric Company for production of 68 missile re-entry vehicles. . . It will be the first production run in an Air Force plan to equip 500 Minuteman 3 missiles with multiple warheads.*
>
> *The contract was awarded last Thursday, the same day that President Nixon told a news conference that the Administration was considering the possibility of a moratorium on MIRV testing as part of an arms control agreement with the Soviet Union.*
>
> *To opponents of an American antiballistic missile system, however, the clear implication of the production contract was that the situation was passing beyond the point where deployment of the multiple warheads could be readily controlled by a moratorium on testing.*

Among the opponents of ABM, *The New York Times*, which is editorially opposed to the deployment of both MIRV and Safeguard at this time, was unusually caustic in its assessment of the production contract award. It asserted that "Given this situation, the country is entitled to an explanation from the President of his intentions in the Soviet-American missile-control talks, which he has personally held up for more than seven months." The *Times* went on to say (June 26, 1969) that "Fuel has now been added to a long-smouldering suspicion that a major reason for delaying the talks has been to flight-test MIRV to operational confidence first."

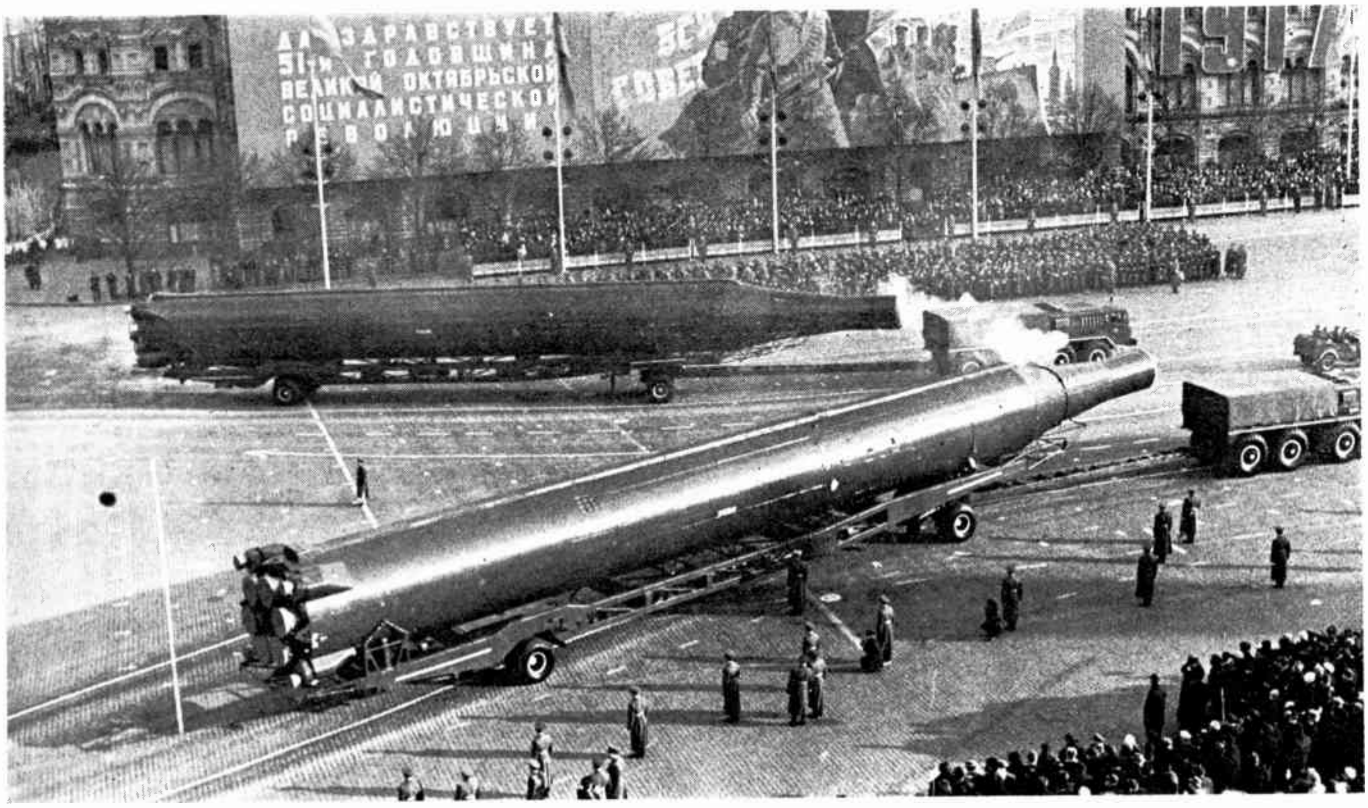
Widening ripples and intelligence gaps

The intelligence estimates of Soviet capabilities and intentions that figured so largely in the Administration's case for Safeguard themselves came under direct attack as the ABM-MIRV controversy intensified throughout the spring. At several points in the debate, critics of the Safeguard plan charged that the Pentagon was distorting intelligence estimates to exaggerate the dimensions of the Soviet threat. These charges centered on apparent discrepancies between national intelligence estimates prepared under the direction of the Central Intelligence Agency, and the far grimmer projections of Soviet military potential offered by the Pentagon in defense of the need to deploy Safeguard. According to ABM critics, this "intelligence gap" went to the heart of the argument over whether it was, or is, an urgent military necessity to start immediate deployment of the Safeguard system. Indeed, at one point in the acrimony-filled spring, scientific and Congressional critics of Safeguard accused the Defense Department of changing its figures and of intentionally misstating intelligence estimates in order to justify deployment. In the midst of this intelligence brouhaha, one relatively cool voice emerged amidst all the clamor. Its accent, as you might expect, was British.

Item: Anthony Lewis, The New York Times, April 11, 1969

> *LONDON, April 10—The Institute for Strategic Studies estimated today that by the middle of this year the Soviet Union would have more intercontinental ballistic missiles deployed than the United States did.*

FIGURE 3. These Soviet SS-9 intercontinental ballistic missiles became embroiled in the ABM debate when the Administration expressed fears they would be equipped with bomb-carrying, multiple independently targetable reentry vehicles—MIRVs—similar to those now being developed and produced for our ICBMs and submarine-based missiles. ABM opponents disputed Pentagon intelligence estimates, however, and sought a moratorium on all MIRV testing and deployment pending long-delayed arms talks.



This development was not viewed by the institute as bringing a shift in the strategic power balance. The institute noted that superiority in other delivery systems planes and submarine missiles—would still give the United States "a lead in total numbers of nuclear weapons."

The institute, in its annual strategic survey, said the Soviet parity in long-range missiles would improve the chances for successful arms limitation talks. President Nixon has indicated that talks may begin this summer.

"Such discussions," the report said, "may be more meaningful than on previous occasions, when acceptance of a freeze or reduction would have perpetuated a Soviet numerical inferiority."

The report said the U.S.S.R. had "deployed close to 1000 I.C.B.M.s" by last September, including some with solid fuel. The report also spoke of Soviet advances in testing new nuclear delivery systems. Estimates and speculations of this kind about Soviet strategic weapons have abounded in recent months. Those from the institute may carry particular weight because of its high reputation as an independent center of research on defense and security.

The institute's report will provide little backing for either side in the debate now going on in the United States over the Soviet Union's intentions in its missile program.

What were the policy roots of this extraordinary battle between groups of Americans, each devoted to its own vision of where security might be found in a world balanced on the brink of disaster for a generation?

Policy roots of the ABM debate

The current phase of the defense debate began in earnest nearly two years ago, on September 18, 1967, when the former U.S. Defense Secretary reversed the policy of the previous decade and announced plans to deploy an ABM system known as Sentinel against the possibility of an attack by China. This announcement set off a widespread public debate. It blossomed after several years of quiet, though equally vehement, technically oriented debate within government circles on the feasibility of an ABM defense. In these circles, however, the McNamara announcement of President Johnson's decision was immediately preceded by several months of still more intense debate, instigated nominally by Soviet plans to deploy a light ABM defense around Moscow following the game of nuclear "chicken" played by the U.S. and U.S.S.R. during the Cuban missile crisis. American disquiet was also increased by evidence of unexpectedly rapid Chinese progress in the art of thermonuclear war.

The original proposed Sentinel deployment called for a "thin area defense" of U.S. cities against the relatively unsophisticated, not too heavy sort of missile attack the Chinese might be capable of launching by the mid-1970s. An area defense requires relatively fewer interceptor sites than a point defense, since it depends upon intercepting incoming missiles with nuclear-armed, long-range countermissiles at altitudes well outside the earth's atmosphere. A point defense, on the other hand, whether of specific cities or of our own intercontinental attack-missile sites, is designed to intercept incoming warheads late in their trajectories, within the atmosphere, with a barrage of short-range, high-acceleration countermissiles. A single interceptor-site complex can thus attempt to defend only a limited area.

The Sentinel proposal in its original form did not require placing nuclear-tipped ABMs and the radar facilities upon which they depend near U.S. cities. The Safeguard system advocated by President Nixon and by now probably acted upon by Congress, reverted to this element of the original plan after a public furor developed over changes in the plan, proposed by the Army, which would have located radars and their defending interceptors near densely populated areas. The Safeguard system, moreover, instead of being deployed at all sites at once, would be installed in phases. According to the Nixon plan, the first installations would be located to protect two ICBM bases in Montana and North Dakota. These bases contain perhaps 350 Minuteman ICBMs in "hardened"—i.e., presumably blastproof to all but a nearly direct hit—underground silos scattered across the western prairie.

Neither the Nixon nor the Johnson Administrations believe that the bulk of the American population can be protected against the kind of massive, sophisticated attack the Soviet Union is already capable of launching.

Human implications of the Safeguard system

Pictorial illustrations can do justice to neither the technical characteristics of an ABM system nor the human implications of the ABM controversy, though the latter might be more easily illustrated than the former. If IEEE SPECTRUM weren't a family magazine, for example, the human implications might be simply illustrated with a pair of contrasting photographs. One would show the residents of a typical large city—in the U.S., the U.S.S.R., wherever—going about the business of living, the situation that would pertain if ABM systems indeed helped to prevent nuclear war or indeed served as an effective shield should one occur. This possibility, that defensive systems might work well enough to offset the threats posed to the earth and man by the nuclear megatonnage already poised in ready-to-fire position, is a tantalizing one. It suggests that the question of who and what might survive a nuclear war is no longer as unanswerable as many, I among them, for many years have thought. Against this sanguine possibility, however, editorial impartiality would require us to print the contrasting photograph. It would show one—just one—burned, irradiated, emotionally numbed "survivor" of Hiroshima or Nagasaki.

One other human implication of the ABM issue must be mentioned. Dr. Ernest J. Sternglass, professor of radiation physics at the University of Pittsburgh School of Medicine, raised it in a letter to *The New York Times*. He believes that the vast amounts of strontium 90 released into the atmosphere by an all-out nuclear exchange—or by an "effective" ABM defense—could have genetic effects that would end the existence of mankind. "The unanticipated genetic effect of strontium 90," he wrote, "presented at the June meeting of the Health Physics Society, follows both from an increase of infant mortality along the path of the fall-out cloud from the first atomic test in New Mexico in 1945, and from a detailed correspondence of state-by-state infant mortality excesses with yearly changes of strontium-90 levels in milk . . . The computer-calculated infant mortality was found to have reached close to one excess death in the U.S. per 100 live births due to the release of only 200 megatons of fission energy by 1965." To Dr. Sternglass, "This indicates that a release of some 20 000 megatons anywhere in the world, needed in offensive warheads for an effective first strike or in the thousands of defensive ABM warheads required to insure interception, would lead to essentially no infants surviving to produce another generation." Presto! Population problems are solved.

Technical features of the Safeguard system

Figure 4 shows, in a crude way, the full eventual possible deployment of what its opponents have come to call the "Sentinel-Safeguard" system. They gave it this name because they believed that the chief, thinly concealed option in President Nixon's phased deployment plan was in fact the option eventually to deploy a somewhat strengthened version of the original Sentinel system.

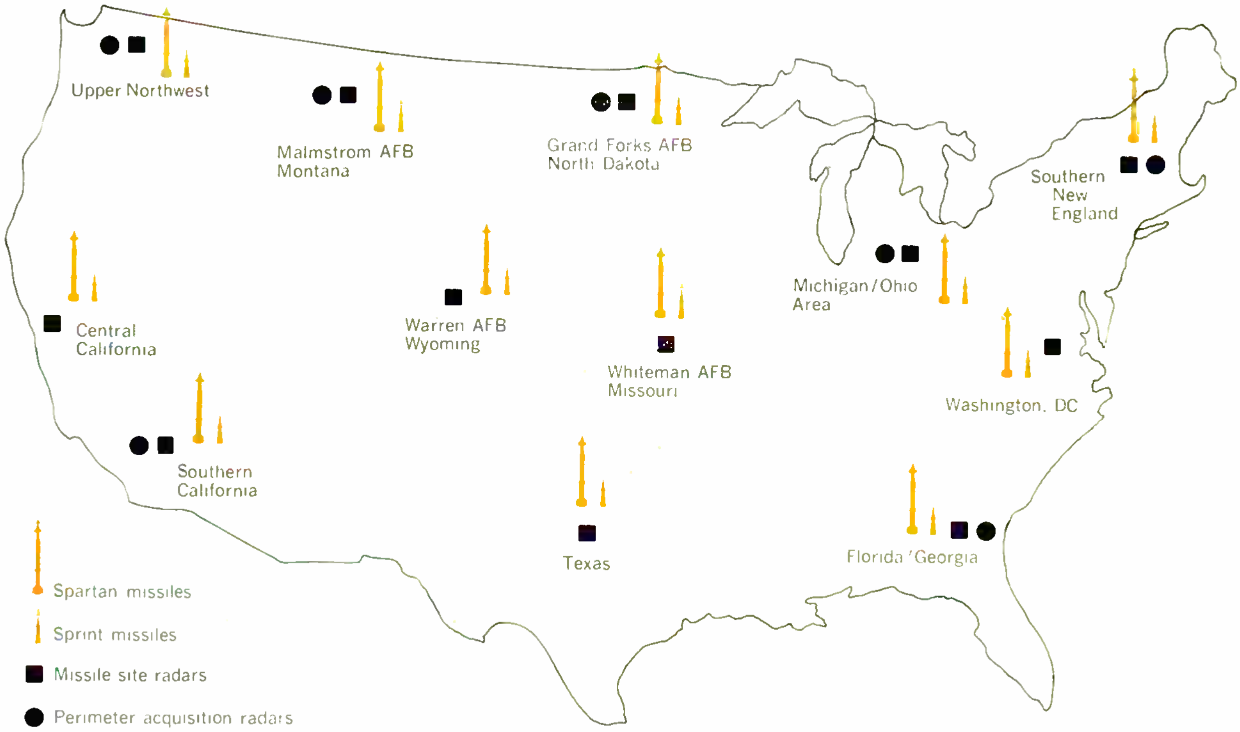
Be that as it may, an ABM system, by any name, is not a simple weapon. It is a presumably well-coordinated array of nuclear warheads, missiles, radars, computers and computer programs, communications and command and control systems, with one human boss—the President.

Two different missiles, whose gross configurations and elementary characteristics are shown in Fig. 5, carry the nuclear counterpunch. One, the long-range Spartan, is a three-stage, 18-meter-long, solid-fueled rocket. It is designed to engage incoming missiles while they are still in space, above the earth's atmosphere, and perhaps several hundred kilometers (and of the order of five minutes) from their intended targets. The Spartan carries a 2-megaton thermonuclear warhead equivalent in destructive power to 100 Hiroshima bombs. The other missile, the short-range, high-acceleration Sprint, is a two-stage, 9-meter-long, solid-fueled rocket with a 20-kiloton, Hiroshima-level, warhead. Sprint is a last-ditch device, designed to intercept those attacking missiles that elude or survive the disabling effects of the X rays and neutrons sprayed across the near-vacuum of space by the higher-yield explosion of Spartan's more powerful nuclear warhead. Sprint is designed to operate well within the atmosphere, at altitudes far enough below 35 km for orthodox blast pressures to be effective, but high enough so that explosive effects of the attacking missiles and countermissiles would be mitigated at ground level. In the atmosphere, X rays released by a thermonuclear explosion are rapidly absorbed by atoms in the air. Thus this technique for disabling attacking missiles is of only limited value. The Sprint warhead, therefore, is presumably designed to achieve its close-in defense objective by a combination of neutron-flux (heat-generating) and conventional blast effects. Conventional blast effects, of course, are not transmitted through space at the airless altitudes at which Spartan is designed to operate.

One ground sensor for the Spartan missile is a long-range detection and tracking radar—the so-called Perimeter Acquisition Radar (PAR). This is a phased-array system whose scanning beam can be electronically switched from one direction to another in a few millionths of a second. The PAR is designed to detect attacking missiles essentially as soon as they rise above the earth's horizon, at a distance of about 4000 km—which, at the speed of an ICBM, corresponds to about 20 minutes of warning time if the attacking missiles have been launched on normal minimum-energy trajectories. PAR then must track them for a minute or two, to establish their trajectories, and then feed these data to the computer system that calculates nominal interception points and that, through a complex command chain, triggers the dispatch of Spartans to the indicated rendezvous.

A critical role in this command chain is played by a lower-power, shorter-range radar detection system—the

FIGURE 4. Configuration of the Safeguard ABM system as it might look when—and if—fully deployed in the U.S., excluding Alaska and Hawaii. Protection of Montana and North Dakota ICBM bases is focus of current funding controversy over President Nixon's phased-deployment plan.



Missile Site Radar (MSR). These enter the fray prealerted by data transmitted from PAR, and are designed to perform surveillance and detection, target-track, Spartan missile-track, and command and control functions for the backup Sprint countermissiles as well.

Escalation of the technical debate

The vulnerability of the MSRs to nearby nuclear blasts became one focus of the technical criticism of Safeguard. But supporting the radars and Spartan-Sprint missile subsystems, and tying them together into an integrated defensive system, is a highly complex data-processing system. It is this computer system that receives and interprets data from the radar systems and that dispatches and instructs the missiles to those rendezvous upon which human destinies depend. It is not surprising, therefore, that the computer system became another focus of the technically based controversy about the potential effectiveness of ABM.

Item: Philip M. Boffey, Science, May 16, 1969

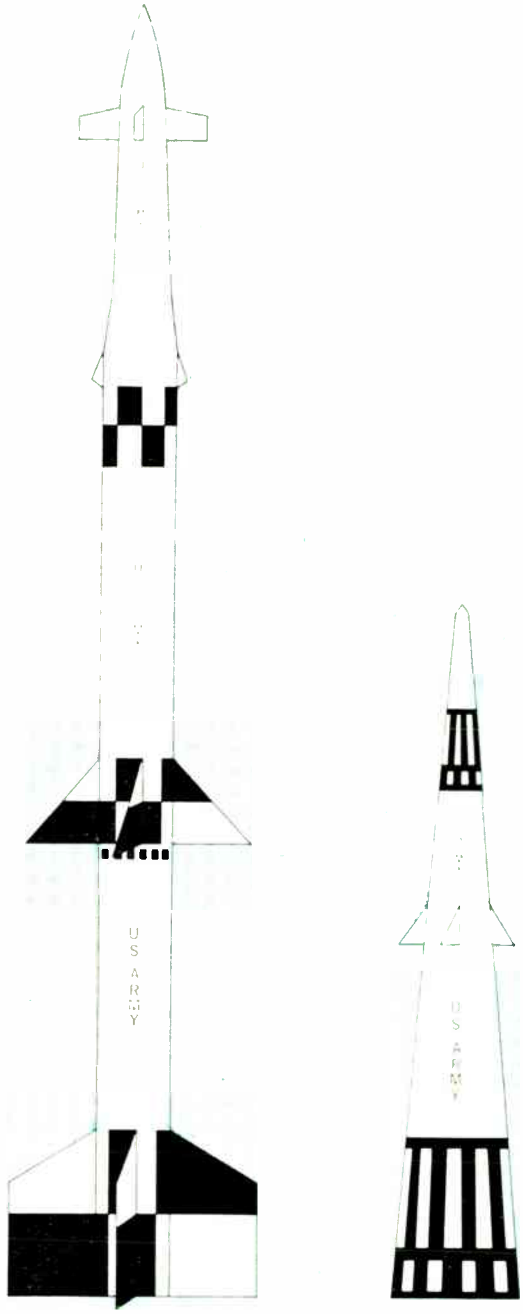
The technical and political arguments over the proposed antiballistic missile (ABM) system crystallized last week with the release of an unusual document—a sort of "Summa Theologica" of the anti-ABM forces. The document—a book-length criticism of the Nixon Administration's proposed Safeguard ABM system—was prepared for Senator Edward M. Kennedy (D—Mass.) by a group of eminent scientists, academicians, and public figures, including two former presidential science advisers, a Nobel Prize winner, and high officials in recent Democratic administrations. It is believed to be the most voluminous and comprehensive public attack on a major weapons system ever made by prominent members of the scientific community.

The study was commissioned by Senator Kennedy last February in order that Congress and the public might have a "non-Pentagon report" to weigh against the official administration reports justifying the ABM. The authors attempted to develop what they call the "other" side of the argument, and their conclusions, not surprisingly, are diametrically opposed to the Pentagon's reasoning. Their report asserts that the ABM is not technically capable of performing the missions assigned to it, that it is not needed at this time, and that it would probably accelerate the arms race, thus decreasing national security rather than increasing it.

These conclusions are given weight by a roster of distinguished names. The report was prepared under the direction of Jerome B. Wiesner, science adviser to the late President Kennedy, and Abram Chayes, former legal adviser to the State Department. It was reviewed "for factual accuracy" by George B. Kistiakowsky, science adviser to the late President Eisenhower, and Paul Doty, of Harvard, both chemists. And it contains separate chapters by such notables as Arthur J. Goldberg, former U.S. Ambassador to the United Nations; Theodore C. Sorensen, special counsel to the late President Kennedy; and Bill Moyers, special assistant to former President Johnson. Scientific contributors include Nobelist Hans Bethe, of Cornell; Leonard S. Rodberg, of the University of Maryland; Jeremy Stone, of Stanford; and George Rathjens, Steven Weinberg, and Bernard Feld, all of M.I.T. The separate contributors took responsibility only for their own chapters, but Wiesner, Chayes, Rathjens, and Weinberg authored the section of the report setting forth the overall argument and conclusions.

. . . The report states that each of the system's components—missiles, computers, and radars—"is at the extreme

FIGURE 5. Two missiles carry the nuclear counterpunch in the Safeguard ABM system. The long-range Spartan is designed to attack incoming missiles above the earth's atmosphere with a disabling flux of X rays and heat-generating neutrons that are sprayed across the near vacuum of space from the 2-megaton warhead it carries. Sprint, a short-range, high-acceleration missile, carries a 20-kiloton warhead, which is designed to disable attacking missiles by orthodox blast effects and neutron heating at < 35 km.



SPARTAN
Height: 16.7 meters
Range: 640 km
Speed: 13 000 km/h
Warhead: ~1 megatonne

SPRINT
Height: 8.2 meters
Range: 40 km
Speed: Unreleased
Warhead: ~1 kilotonne

of sophistication for its type." It also notes that the system requires "extraordinary coordination" among these elements during the 20 minutes or so that are left between the time an incoming warhead is spotted and the time it must be destroyed.

The report particularly singles out the computers as a likely source of trouble. It says the computers would be "the largest and most complex ever built" and that the programming would have to be "more sophisticated and complex than any accomplished so far." Leonard S. Rodberg, a University of Maryland physicist and former chief of the science office at the Arms Control and Disarmament Agency, states flatly: "Many computer engineers currently involved in the project profess uncertainty as to whether they will even be able to design the software, much less assure that all sources of potential failure have been removed." Rodberg says the ABM computer system would use a new "time shared" approach that is "in its infancy." He says the first practical time-sharing system became operative only four years ago, and that our largest computer firms have encountered "severe difficulties" in developing reliable small-scale systems for commercial use.

Inevitably, and with remarkable rapidity, this anti-ABM report commissioned by Senator Kennedy, nominally for the edification of his senatorial colleagues, appeared shortly afterward as a book. It bore the title, *ABM: An Evaluation of the Decision to Deploy an Antiballistic Missile System*, and on June 3, 1969, was projected at the by-now widely perturbed public in a two-page advertisement in *The New York Times* that announced its simultaneous availability in hardbound (Harper & Row, $5.95) and paperback (New American Library, $0.95) editions.

The advertisement restated the technical essence of the anti-ABM argument in terms designed to make sense—and to exert an emotional impact—on the layman. It reasserted the report's argument to the effect that the ABM (Sentinel-Safeguard) is probably the most complicated electronic system ever attempted, and it again stressed that each of its elements—missiles, computers, radars—is at the extreme of sophistication for its type. Moreover, it said, "the computer programming alone, for example, presents problems not yet solved even on the theoretical level." And, it warned, because of time and complexity constraints, human intervention once the President had "pushed the button" was impossible. The computer had to check itself. As if this weren't scary enough, this abridged version of the anti-ABM argument went on to its more general conclusion. "Well," it said, "if everyone knew for sure that Safeguard would work, then there might be some (shaky) confidence about turning our lives over to it." But, it went on to ask, what about the gap between expected performance and actual performance in the case of systems many times less complex than this one? "Performance is nearly always below promise, even when there is plenty of time and the possibility for testing." But Safeguard can't be tested fully.

With the appearance of this technical broadside by anti-ABM forces, the technical battle was now joined with a vengeance.

The Pentagon counterattack

Several weeks before this anti-antimissile missile hit the bookstores, in fact immediately upon its appearance in Senator Kennedy's office on May 6 as a hastily printed, 344-page, loose-leaf monograph, the counterattack began. John Finney reported this opening shot of the Pentagon counterattack in *The New York Times*, May 7, 1969: "... Obviously concerned about its potential political impact, Defense Secretary Laird promptly set up a Pentagon team to analyze the report's conclusions.

"Dr. Foster [Dr. John S. Foster, director of defense research and engineering] called a news conference at 1:30 P.M.—five hours before the report was officially released by the Kennedy office—to issue a statement castigating the report in its conclusions and methodology.

"Dr. Foster said he and his staff had found the Wiesner-Chayes report 'of little help' because 'we find nothing in the report that has not been analyzed in depth by the Department of Defense and the technical community over the past ten years.' 'In fact,' he said, 'because it contains a number of errors and is internally inconsistent, it could add to the confusion of the very people whom the authors want to help on this issue.'"

Foster continued to spearhead the Pentagon's counterattack on the now clearly defined and highly visible non-Congressional critics of ABM the following week.

Item: W. Beecher, The New York Times, May 13, 1969

DAYTON, Ohio, May 12— ... In a speech frankly designed as a partial rebuttal to the report challenging the Administration's proposal to build a missile defense system that was prepared at the behest of Senator Edward M. Kennedy, Democrat of Massachusetts, Dr. Foster declared: It is untrue, as stated in the report, that it would take at least two warheads to assure destruction of one Minuteman missile in its silo.

Contrary to the report's assertions, missile defense is not more costly than missile offense; rather, it is roughly equal.

It is wrong to suggest that the Safeguard missile defense system could not be given a good enough computer to handle the vast amount of data necessary for its assignment. A mammoth computer currently in operation and similar in design to what will be used with Safeguard contains almost one million different instructions.

In a direct reference to those who had contributed to the anti-Safeguard report put together by Dr. Jerome Wiesner of the Massachusetts Institute of Technology and Abram Chayes of Harvard, Dr. Foster said: "There are some eminent scientists, who for one reason or other, claim it won't work ... They have offered no problem which we have not long since addressed and resolved ... I want to point out that one does not obtain a meaningful technical judgment by taking a vote of the scientific community or even of Nobel laureates."

How, then, does one obtain a meaningful technical judgment? Perhaps the knowledgeable readers of IEEE SPECTRUM have some valuable opinions on this. It is one of the overridingly important methodological questions raised by the ABM debate.

Pentagon opinion was read into the public record at greater length later that week when DOD research chief Foster's and Deputy Defense Secretary David Packard's additional comments were reported in *Science*.

Item: Philip M. Boffey, Science, May 16, 1969

Foster described Safeguard as a "forgiving" system which "doesn't have to know everything that could happen to it to make it work." He said Safeguard could miss some attacking warheads, but still achieve its purpose if it destroyed enough attacking warheads to pre-

serve a substantial part of our "deterrent" ICBM force, thus making it clear to the Soviets that a surprise attack would result in disastrous retaliation.

On the question of reliability of components, Deputy Defense Secretary David Packard, who conducted a review of the ABM program for the Nixon administration, has testified that all components are "sound and feasible technically." Packard said he particularly investigated possible computer software problems, and concluded: "This data processing job is a large one. It does not involve any new technology. It's simply a large system involving data processing." Similarly a ranking Pentagon ABM scientist told Science *that "while the computational job is very difficult, I see nothing in it beyond the state of the art."*

This same scientist also said that ABM missiles may actually be more reliable than existing ICBMs because the ABM doesn't need "a fancy inertial guidance system" and doesn't have to hit a target 5000 miles away.

As to the Wiesner Chayes recital of poor weapons performance in the past, the Pentagon tends to dismiss the examples cited as irrelevant or exaggerated, and points instead to such American technological successes as the Apollo moon program as evidence that big systems have worked in the past.

Apollo's achievements have been extraordinary, but then there was that well-advertised error that omitted a hyphen from the computer program on the Apollo 10 descent.

Non-Pentagon support develops for ABM

Earlier in the month, in a development that received scant attention in the press, a rebuttal of sorts—in the form of a 60-page booklet supporting deployment of the ABM—was issued by a group of prominent scientists and others associated with the American Security Council, an industry-supported group dedicated to "meeting the Communist challenge to world freedom." The report (which is available for $1.50 from the American Security Council, 1101 17th Street, NW, Washington, D.C. 20036) was prepared by a committee of 31 headed by Nobel Laureate Willard Libby of U.C.L.A. The group included such luminaries as William J. Thalor (physicist, Georgetown University), General Nathan F. Twining (former chairman of the Joint Chiefs of Staff), Edward Teller (physicist, the Lawrence Radiation Laboratories), and Eugene P. Wigner, a Princeton physicist and, like Libby, a Nobel Prize winner.

Later in the month, the thinking of still another prominent pro-ABM civilian received greater attention from the press.

Item: W. Beecher, The New York Times, May 26, 1969

WASHINGTON, May 25—One of the country's leading nuclear strategists has told Congress that many arguments of scientists opposed to a missile defense system range from mistaken to "plainly absurd."

Dr. Albert Wohlstetter of the University of Chicago, the man credited with having led the way toward the concept of protecting strategic forces in order to deter an enemy from staging a surprise attack, has issued his rebuke in the form of a 21-page analytical report to the Senate Armed Services Committee.

When the Wohlstetter Report is released by the committee, it is expected to further intensify the already warm debate surrounding the Administration's proposed Safeguard missile defense system.

At least in part, the report amounts to a stern rejoinder from someone outside the Nixon Administration to the recent book-length criticism of the Safeguard system that was written at the behest of Senator Edward M. Kennedy, Democrat of Massachusetts.

Dr. Wohlstetter, who has spent the last two decades concentrating on arms control and on protecting strategic forces, chided his colleagues on the other side of the antimissile argument for either not having done their homework or for having done it sloppily.

He spoke with the confidence of a man who was once described by Dr. Henry A. Kissinger, the President's National Security Adviser, as the man who more than any other "provided the intellectual impetus for the recasting of American military strategy in the 1960s." . . .

Dr. Wohlstetter chided Dr. George Rathjens, a former disarmament official and one of the authors of the Kennedy-sponsored attack on Safeguard, for computations that led to his conclusion that an all-out surprise attack by Soviet missiles on the 1000 American Minutemen missiles would destroy only 75 percent of the targets. The "correct" number is 95 percent, Dr. Wohlstetter insisted. . .*

In spite of this carefully measured rebuttal by Wohlstetter, ABM critics no doubt comforted themselves with the fact that Henry Kissinger, now the President's National Security Adviser, was the same man who, just before coming to Washington, had written these words†:

Throughout history, military power was considered the ultimate recourse. Statesmen treated the acquisition of additional power as an obvious and paramount objective . . . The nuclear age has destroyed this traditional measure . . . No foreseeable force-level—not even full-scale ballistic missile defenses—can prevent levels of damage eclipsing those of the two world wars . . . The paradox of contemporary military strength is that a gargantuan increase in power has eroded its relationship to policy . . . The capacity to destroy is difficult to translate into a plausible threat even against countries with no capacity for retaliation . . . Slogans like "superiority," "parity," "assured destruction," compete unencumbered by clear definitions of their operational military significance, much less a consensus on their political implications.

Comforting as this Kissinger opinion might have been, anti-ABM partisans later took less comfort from the reappearance of Wohlstetter, the arms strategist once described in such superlative terms by Kissinger, in two additional contexts. Wohlstetter turned up in a group, headed by former Secretary of State Dean Acheson, which, on the day following announcement of the Wohlstetter report, called for a more "reasoned" public debate on such defense issues as ABM. And Wohlstetter's name turned up, along with that of Donald Brennan‡ and those of several other authors also associated with the Hudson Institute, a think tank deriving most of its support from the Defense Department, in an announcement by the Pergamon Publishing Company of an impending book that bore all the earmarks of being the definitive intellectual riposte to the anti-ABM forces. Entitled *Why ABM? Policy Issues In The Missile Defense Controversy*, the book was expected to become available by the end of July. Although this would in all likelihood be too late to influence public opinion in time to affect the current

* IEEE SPECTRUM's anti-ABM author.

† In a Brookings Institution report published in 1968 by Doubleday as a book entitled *Agenda for the Nation.*

‡ IEEE SPECTRUM's pro-ABM author.

(by now possibly concluded) battle over ABM appropriations, in the "battle of the books," at least, the pro-ABM forces had drawn even—and there would be future appropriations battles.

Looking beyond the current battle also, no doubt, was the Acheson group, of which Wohlstetter was a member. It announced plans to establish a bipartisan committee of prominent citizens to argue such major questions of national security as ABM and MIRV.

Item: W. Beecher, The New York Times, May 27, 1969

WASHINGTON, May 26— . . . Proposing to call the group the Committee to Maintain a Prudent Defense Policy, Mr. Acheson sent letters today to more than two dozen prospective members, including several former senior Government officials and a number of academicians. Along with Mr. Acheson, who was Secretary of State in 1949–53, the co-founders of the new committee are Paul H. Nitze, Deputy Secretary of Defense in the Johnson Administration, and Dr. Albert Wohlstetter of the University of Chicago, a leading nuclear strategist . . . "On an issue of this kind, connected as it is with avoiding nuclear war and nuclear coercion, it is all too easy to stimulate an emotional rather than a reasoned public response. But these issues are too grave to be governed by emotion," the letter said . . . Mr. Nitze, in an interview, said the three men decided to form a body to discuss the ABM . . . and related issues because of their strong concern that the argument has been largely "one-sided." They wanted, he said, to contribute to a "balanced debate."

There has been a "tremendous effort on the other side" of the issue, Mr. Nitze said. "It's awfully hard for the public to find a well-reasoned argument."

That's what I was looking for, too—a well-reasoned nonemotional argument—when I embarked on this hegira a few months back, at the American Physical Society meeting at Washington's Sheraton-Park Hotel.

The battle of the Sheraton-Park

The tension and sense of personal involvement that the ABM issue had generated in the scientific community was immediately apparent as I entered the lobby of the Sheraton-Park Hotel. Its formidable floor space was swarming with ebullient physicists—recognizable by their convention badges—who had just returned from their lunch hour; and visible on scores of lapels were bright red buttons in the octagonal shape of a traffic stop sign. These buttons bore, in white lettering, the unambiguous message "Stop ABM."

Before I could reach the hotel registration desk, which is only a few dozen feet down the lobby from the entrance doors, I was accosted by a leggy, lovely young lady in a miniskirt. Her blouse also bore the convention badge and the exhortatory red and white button. She was one of several similarly attired young ladies—they were usually accompanied by bearded young men also wearing the convention badge—who were circulating through the lobby, soliciting signatures to anti-ABM petitions.

Though I declined to sign the physicists' anti-ABM petition on several grounds—among them that I was not a physicist, and, more to the point, that I didn't know enough about the issue to register an opinion—I did accept the fistful of mimeographed materials the earnest young lady pressed upon me. The leaflets and their young, energetic distributors made it clear that the physicists' discussions of ABM that week in Washington were not going to be restricted to either the formal technical session or the formal press conference, to both of which I would presently make my way.

These unprogrammed parts of the meeting were coordinated and staffed principally by members of Scientists for Social and Political Action, a 500-member group of mostly younger activist types among the physics community. Though not a part of The American Physical Society, SSPA was formed at *ad hoc* meetings during an earlier (February) meeting of the APS in New York. The SSPA activities included not only the leafleting and petition-gathering to which I had already been exposed, but also a poll of physicists on the ABM Safeguard issue, a "march" on the White House, lobbying visits to Senatorial offices, and, at the Sheraton-Park, the hectic sort of lobbying and press conference activity that one associates with political conventions rather than technical meetings.

Background and foreground of the APS debate

During the course of the week-long meeting, SSPA presented a petition signed by 1100 physicists to President Nixon's science adviser Dr. Lee DuBridge, urging that plans to deploy the Safeguard ABM system be withdrawn. The petition was presented at the conclusion of a two-mile anti-ABM protest march from the convention hotel to the White House, in which the perhaps 100 physicists who covered the route on foot were joined at the White House by another 100 or so more sedentary activists who arrived by bus. SSPA delegations also met with some threescore Senators or Senators' aides to register their opposition to Safeguard and to deliver copies of the petition signed by 1100 physicists. The group also released the results of a poll of 1216 physicists taken before, during, and after the April 29 evening APS meeting on "Technical Aspects of the Antiballistic Missile System." At that session itself, SSPA members registered their objections to the structure of the formal program with the arguments set forth in the leaflet reproduced here as Fig. 6. In their poll (the ballot form for it is reproduced here for your edification as Fig. 7), 21 percent of those polled supported Safeguard, 76 percent opposed it, and 3 percent had no opinion.

Like 3 percent of those polled by SSPA, I also had no opinion when I proceeded to the formal APS press conference. Here the opposing Nobel Laureates in physics—Hans Bethe of Cornell and Eugene Wigner of Princeton—and the opposing arms control strategists—George Rathjens of M.I.T. and Donald Brennan of the Hudson Institute, our two authors—ran through a preview of the arguments they intended to offer at the technical session.

Penetration aids can confuse the defense

The Bethe–Wigner debate was only the latest in a series of technically based but nevertheless essentially political disputes in which prominent physicists have engaged since they opened the Pandora's box of nuclear armaments in World War II.

Item: John Walsh, Science, March 28, 1969

Over the years there has been a certain continuity in arguments and in personalities. Hans Bethe, Nobel-Prize-winning theoretical physicist, played a key role in the work of American scientists mobilized during World War II and was a dominant figure among those who argued that it was possible to develop a detection system adequate to police a nuclear test ban. And it was Bethe, collaborating with physicist Richard L. Garwin, who produced an article, published in the March 1968 issue of Scientific

American, *which provided a prime public source of information for opponents of the ABM. Bethe and Garwin discussed in detail offensive tactics and aids to penetration of the putative "thin" ABM shield, and thus markedly raised the level of sophistication of subsequent debate.*

Drawing upon his 12 years as a member of a White House panel concerned with the evaluation of decoys and other devices and penetration aids for overcoming missile defenses, and upon essentially the same arguments he had advanced in the *Scientific American* article, Hans Bethe asserted that the multiple stratagems available to an attacker could make it almost impossible for the defender to design an adequate ABM defense. The attacker, first of all, will try to design his weapons to withstand the three sources of potential damage from defensive missiles—neutrons, X rays, and, within the atmosphere, blast.

Above and beyond this, however, the attacker can employ a wide variety of penetration aids to interfere with the operation of the defender's ABM system. One general scheme is to confuse the radar and computers of the ABM system by concealing the actual attack vehicles in a mass of objects that superficially resemble them. These objects can be fragments of the launching vehicle or deliberately designed decoys. The latter might be nothing more than balloons with an internal wire mesh to reflect radar waves. As long as they are outside the atmosphere, balloons will travel as fast on ballistic trajectories as heavy metallic objects. Although the defensive system can wait for the atmosphere to sort out real bomb-carrying reentry vehicles from balloon decoys, the time for interception is then much shortened. Moreover, more complex decoys can be designed to reenter the atmosphere in a manner that makes them not easily distinguishable from bomb-carrying vehicles.

Radar blackouts can blind the defense

A second general stratagem open to the attacker is directly to nullify the effectiveness of the defender's radar system. This can be done, for example, by dispersing a myriad of tiny wires (chaff dipoles) that can reflect radar signals over such a large part of the sky that the bomb-carrying vehicles cannot be picked out. Another approach is to design some decoys to generate radio noise in the frequency range of the defender's radar—to use electronic countermeasures, in other words.

The defender's radar can also be neutralized—rendered blind for greater or lesser periods of time—by the electron clouds created by nuclear explosions. Such explosions can create electron clouds in two ways. A "fireball" blackout results when the intense flux of heat from a thermonuclear blast strips electrons from atoms and molecules of air. A so-called "beta" blackout results when the radioactive debris produced by the explosion releases beta rays (high-energy electrons) and these in turn ionize the air they traverse, freeing additional electrons. The areal extent of the fireball blackout depends on the size of the explosion and the altitude at which an attacker might set it off. The areal extent of the beta blackout also depends on these factors but, since beta rays released at high altitude travel along the earth's magnetic lines of force ionizing air molecules as they travel, a properly produced beta blackout could in effect create an artificially intense—and radar-opaque—ionosphere at an altitude of 50 km over an area whose radius was perhaps 100 km.

The electron clouds generated in these ways would absorb and refract the lower-frequency, longer-range radar waves of Safeguard's perimeter acquisition radars (the PARs) with especially great effectiveness, and for perhaps as long as several minutes. The shorter-wavelength, shorter-range missile site radar (MSR) waves, however, would be attenuated only briefly. Thus, although a long-range defense system is more vulnerable to radar blackout, a terminal system is at first glance less vulnerable. But even at lower altitudes, of course, the defender must contend with the blackouts caused by his own defensive nuclear-weapon blasts.

Weighing the cost and practicality of such penetration aids and the problems they could produce for the defense, Bethe concluded that the overwhelming advantage lies with the designer of offensive weapons systems. The defender's best hope is to develop "terminal" systems—systems that work as close to the defender's cities and military installations as possible. The latter is what the Safeguard system is designed to do. To protect large sections of the country with such a system, however, would be exceedingly costly (of the order of $50 billion).

Since defensive systems are doomed to failure—the offense can always overwhelm the defense with penetration aids and additional attack missiles if he wishes to spend the money to do so—the real question, according to Bethe, is how many casualties are you willing to accept? This becomes an economic question, in part, because the cost of escalating the offense to achieve a given casualty level is only about one quarter of the cost of the defense necessary to offset it. (Brennan disputes this interpretation of the so-called "exchange ratio" however; see p. 47.)

In Bethe's opinion, the long-range Spartan component of the "Sentinel-Safeguard" ABM system, which is designed to intercept incoming missiles above the earth's atmosphere, can work well enough for an attack force made up of 50–100 simple (no penetration aids) missiles. If the same number of attacking missiles were equipped with penetration aids of the sort Bethe described, however, the number of exoatmospheric interceptions Spartan would be capable of would drop "drastically."

What about the short-range, high-acceleration Sprint missiles, which, in the Safeguard system, are designed to provide closer-in, within-the-atmosphere defense of our land-based retaliatory force of Minuteman ICBMs but not our cities? Bethe thinks interception in the atmosphere is possible; the atmosphere will separate the decoy "sheep" from the nuclear "goats" to some extent. But he argued that such a costly system was unnecessary at this time, and he reiterated former Defense Secretary McNamara's vigorous statement about the folly of thickening what was then (September 18, 1967) the "light" but countrywide Sentinel system into a "heavy" system: "It is important to understand," McNamara said, "that none of the systems at the present or foreseeable state of the art would provide an impenetrable shield over the United States.... Let me make very clear that the [cost] in itself is not the problem: the penetrability of the proposed shield is the problem."

It was to this disturbing problem that Dr. Wigner, Bethe's opponent, directed his remarks.

Wigner agrees with Bethe, but...

Wigner's knowledge of the ABM, he said, was derived primarily from a major civil defense study he conducted

APS MEMBERS - YOU HAVE A RIGHT TO RAISE THESE QUESTIONS

The council of the APS has gone on record as not taking political positions. This session was intended as a technical discussion. But is it purely technical as set up?

We are unconvinced because:

1. The speakers have obviously been chosen to be equally divided in their political stand on the ABM.

2. The titles of some of the talks (particularly the last) make clear that the emphasis is political.

Because of this we want to emphasize that the political spectrum of the speakers is dangerously narrow for two reasons.

1. Two of the speakers are "hawks" on the ABM and other defense issues, whereas the ABM opponents do not include pro-disarmament speakers.

2. The even split can be taken by the press and the public to indicate the split of informed opinion within the APS; whereas, in fact the vast majority of those with strong, informed opinions oppose the ABM.

Voice your disapproval of this symposium set up.

Sign the anti-ABM petition.

Join the anti-ABM march on Wednesday.

SSPA

FIGURE 6. Scientists for Social and Political Action, the activist group that spearheaded anti-ABM activities at last spring's Washington meeting of the American Physical Society, voiced its disapproval of "narrow" APS arrangements for the ABM "technical" debate with this leaflet.

FIGURE 7. SSPA also polled physicist opinion during the week-long APS meeting. Of 1216 who voted—on the ballot shown below—71 percent opposed Safeguard, 26 percent favored its deployment, 3 percent had no opinion. If, after reading our staff report and debate, IEEE members care to express their opinions, they are invited to clip ballot and mail it to Editor.

POLL ON THE ANTI BALLISTIC MISSILE SYSTEM
(taken of physicist at the Washington American Physical Society meeting 28 April-1 May)
Please check one box for each question.

1. I support deployment of the present Safeguard Antiballistic Missile system. [ ]
I am opposed to deployment of the present Safeguard Antiballistic Missile system. [ ]
I have no opinion on this matter. [ ]

2. Deployment of Safeguard is likely to lead to an increase in the arms race. [ ]
Deployment of Safeguard is likely to have little effect on the arms race. [ ]
Deployment of Safeguard is likely to lead to a reduction in the arms race. [ ]

3. I am in favor of a thicker Antiballistic Missile system. [ ]
I am opposed to any deployment of an Antiballistic Missile system. [ ]
I am undecided about future Antiballistic Missile systems. [ ]

4. Further comments on Antiballistic Missile systems, technology, politics, economics, etc.

for the government a few years ago. This exercise in the destructive potentials of nuclear weaponry made him a determined advocate of a large-scale bomb shelter program to protect the civilian population in the event of nuclear war. He agreed with Bethe's analysis of the technical and economic factors in the ABM dispute, and argued that these dismal facts made the case for a civil defense program, in addition to ABM, more imperative than ever. Wigner's calculations led him to believe that, in the event of an all-out Soviet attack on U.S. population centers, casualties could be reduced to less than 20 percent of the population at a cost in civilian defense of about $21.2 billion. This figure would be for a shelter program only; it would not include the cost of an ABM system heavy enough to stop some significant number of incoming missiles in such a city-centered attack. Divers estimates of the cost of such a system range upward from about $25 billion to as much as $100 billion or more.

In support of his impassioned and somehow touching plea for an all-out casualty reduction program going far beyond Safeguard, Wigner quoted a number of belligerent statements by Soviet military men as evidence that the Russians might attack, despite the reasons advanced by many ABM opponents to the contrary. He also quoted Soviet Premier Kosygin's reply to a query about the Soviet decision to deploy ABM defenses around Moscow. He answered to the effect that defensive systems are not the cause of the arms race but a worthwhile expenditure to preserve human lives. Continuing in this vein, Wigner also briefly described the great steel sliding doors that are built into some Moscow and Leningrad subway stations; when closed at each end of the platform, he pointed out, the doors could convert these 60-meter-deep subway stations into capacious bomb shelters. And, in describing an "elaborate" Soviet plan for an evacuation system to clear their citizens from the cities, he argued that only a nation contemplating a nuclear first strike could profit by such a scheme. To this, Dr. Bethe replied that evacuation could not precede a sneak attack because it would alert the other side, and the short time remaining between such a sneak attack and the inevitable retaliatory attack would preclude evacuating the cities.

And so it went, each side straying ever farther from the narrow "technical aspects of the antiballistic missile" in order to score debater's points in which inanity was piled on top of inanity in what seemed a futile quest to read other men's motives if not their minds. A social psychiatrist listening to the debate might have concluded, as I no doubt presumptuously did, that these men of undoubted good will on both sides—on both sides of the divided world and on both sides of this debate—were doing little more than looking into mirrors. Perhaps, being human, no other course is possible. If so, Balzac's human comedy was assuming all the aspects of a tragedy.

Wigner concluded his appeal to the assembled physicists by displaying his version of what has, in this grim business, become known as the "numbers game." To buttress his interpretation of Soviet motives as sinister, he cited figures showing that Soviet missile strength exceeds ours by 5:1 in total explosive power, though the total area of potential destruction covered is about the same for the U.S.S.R. and the U.S. because our bombs, though individually smaller, are nevertheless more numerous. In number of warheads in place and ready to go, we and the Russians are about equal, according to Wigner.

The applause for Bethe and Wigner was also about equal; no basis for a technical judgment there.

Exodus before the main event

At the conclusion of the Bethe and Wigner talks many in the audience of 2000 departed, convinced apparently that they had seen the main event. Many, no doubt, had come in the first place chiefly to see and hear two of the giants of 20th century physics have at each other; to these physicists the issue may have been of subordinate interest. They may have been a part of the "silent" vote in the SSPA poll. The hour, in any case, was late. At least 1500 remained, however, to hear nonphysicists Rathjens and Brennan pick up where Bethe and Wigner left off.

Both Rathjens and Brennan are representatives of that new, younger breed of "defense intellectuals"—men mostly in their late 30s and early 40s who, over the past decade, have preempted the "high-priest" roles so frequently played in defense matters by physicists since World War II. This succession was in part due to the normal toll of World War II scientists taken by time, but it also reflects the inevitable growth of a body of strategic doctrine attuned to the sometimes unreal-sounding realities of life in the nuclear age. Among these absurd but harsh facts, physics is only part of the story.

Ladies and gentlemen. In this corner...

George Rathjens and Donald Brennan are particularly well equipped to deal with the strategic elements of the ABM dispute. Each in his way has contributed significantly to attempts to break the impasse that the combined talents of nuclear physicists, electrical and electronics engineers, and rocket engineers have confronted us with since the end of World War II. Interestingly enough, the ABM, MIRV, SS-9, strategic arms negotiations, and the Nuclear Non-Proliferation Treaty—among other things you have already read about or will be reading about in the following pages—could contribute to breaking this impasse. Almost everyone involved in any way in the ABM dispute agrees that the nuclear overkill standoff between the superpowers must be ended. The real question that breeds the profound disagreements we have discussed is, how shall it be ended?

Discounting the opinions of those with little stamina—and less sanity—who would bomb the other side now, at the one extreme there are those—on both sides of this perhaps hopelessly divided world—who hope, or believe, that some combination of offensive and defensive capability can be developed that will give their side the undisputed upper hand. At the other extreme, again on both sides, there are those who see no way out other than immediately scrapping all strategic nuclear attack systems already in existence and seeing to it, somehow, that they stay scrapped and that no new ballistic missile systems come into existence anywhere in the world.

Rathjens and Brennan occupy neither of these poles. They are what others have called the "rational" or "realistic" men in the middle. To modify the phrase once used by Herman Kahn, Brennan's colleague at the Hudson Institute, they are not only among the men who think the unthinkable—they are among the men who helped invent it and helped nurture it to its present formidable proportions. Having thought about the unthinkable, for several years, they are now, I suspect, as spooked by it as we who have chosen to ignore it until the last few months.

George Rathjens speaks—

Against the ABM

Viewing the ABM in the context of the dynamics of the arms race, Rathjens concludes that our deployment of it at this time may be premature and provocative. It will stimulate the Soviet Union to respond, he thinks, both offensively and defensively, just as they—and we—have responded in the past to real or imagined threats. And we will again respond to their real or anticipated response. He argues that the endlessly squandered resources of a runaway arms race will buy neither nation the security it seeks, and in our case, at least, could better be used to ameliorate threats to our society arising from problems at home. Rathjens' remedy? An arms freeze, and negotiations now, while our strategic posture is stronger than it is likely to remain should the arms race continue, and while the Russians may be willing to talk seriously for the first time from a position that approaches parity with us, rather than from a sense of inferiority that may have kept them from the conference table until now.

Fear is the dynamo, justified or not

I spent most of last week facing Senators and Congressmen. I feel more at home here, at the Physical Society meeting, perhaps because I was trained as a physical chemist, but also because you can't ask questions, or, if you do, I don't have to answer them. I did last week.

First I'd like to discuss the ABM question generally, with emphasis on the role of ABM in population defense. Then, in the second half of my talk, I'll deal specifically with the President's planned Safeguard deployment.

First, let me argue that each side's—the Soviet Union's and the United States'—decisions regarding strategic forces is based largely on what the other does or what each fears the other might do.

On the U.S. side it's clear that our very rapid buildup in land-based ICBMs and submarine-launched missiles (SLBMs) during the mid-1960s was a direct result of fears that arose in this country at the time of the so-called missile gap crisis. The fact that the missile gap never really materialized (and that our intelligence was bad) was irrelevant. We built up our forces because of our fears that there might be a very large Soviet force. We are now deploying MIRVs on our missiles, multiple individually targetable warheads, in a direct response to the fact that the Soviet Union has deployed something of an ABM system around Moscow and the fact that it may, we fear, deploy a larger ABM system around all of the Soviet Union. In the closing year of the Johnson Administration, the decision to go ahead with the Sentinel ABM program was directly responsive to our fear that the Chinese might deploy an ICBM system. And now we have Safeguard, which is directly responsive to Secretary Laird's, and presumably President Nixon's, fear that the Soviet Union may greatly enlarge its missile force, particularly its SS-9 force, and may include or may use multiple warheads with those missiles. I think there is no doubt that on the United States side, at any rate, we do react, and we react often before the fact, often before the threat develops.

Whether the Soviet Union does likewise is perhaps more debatable. I can give you some examples that I think may partially support the thesis that they do. I would argue that the very rapid growth in Soviet ICBMs and sea launch missiles, which Dr. Wigner has just spoken of, is in a sense a response to the very rapid growth of our own force during the mid-1960s. They are not yet quite caught up. The Russians have deployed what we thought for a while was an antiballistic system—we now believe it is an air defense system—at a time when enlargement of air defense might not have made much sense. There is a common feeling, though, that that defense system, the so-called Tallinn system, was a Soviet reaction to the possibility that we might go ahead with the B-70 bomber. The timing of the Soviet decision seems to fit this hypothesis. The Moscow ABM defenses were no doubt a response to our ICBMs and SLBM deployment. Finally, I submit that the Soviet decision, if there has been such a decision. to employ multiple warheads on its SS-9 can just as rationally be ascribed to a concern that the U.S. might deploy an ABM defense as to any other cause. We have been talking about ABM for years, and both before and after the Sentinel decision there was pressure in this country to build a large-scale ABM system designed to blunt a massive Soviet attack. As Hans Bethe has pointed out, multiple warheads are among the most effective of all penetration aids against the ABM system. The timing of the recent acceleration in SS-9 production and the observation of tests of multiple warheads carried by SS-9s seem to correlate well with the Sentinel decision and the pressure that developed last summer to expand it.

Momentum of the action/reaction phenomenon

Now it has been the prevailing view in the U.S. that this action/reaction phenomenon, where one side reacts to the other's decisions, is a very real thing. It applies to both sides. Each Secretary of Defense in this country (and the President in each case, starting with President Eisenhower) has refused to go along with deployment of a massive ABM program that might blunt a Soviet attack simply because such a large-scale defense would buy us very little. The Soviet Union would simply react by improving its offense to offset whatever effect our new defensive deployment might have. There has been dissent from this point of view. The Joint Chiefs for years, I believe, implicitly dissented; they supported a large-scale ABM. There was feeling there, and one can read it in their Congressional testimony, that we ought to go ahead with an ABM deployment because it would save lives. Implicit was the idea that the Soviet Union would not react by improving its offensive forces to the extent that the Secretaries of Defense and the President have believed. I happen to believe that this action/reaction phenomenon works both ways. There are others who do not. (I believe you'll find when Don Brennan talks to you that he has some doubts about whether Soviet decisions are reactions to ours.) It is a fundamental point, for, if you accept the belief that the Soviet Union will not react, I think most of the arguments against a large-scale ABM disappear. If you believe as I do that they do react, I believe you have to accept the judgment that President Nixon reached—that it would be futile to build a large-scale ABM system.

The tragedy of the action/reaction phenomenon is that modern weapons require long lead times. And people are conservative. This means that we react not to what the other fellow is doing but to our worst fears of what he might do, and we have to react long before he has done it. Several of these examples illustrate that. We are reacting now to the Soviet SS-9 threat, not because there is a threat, but because it might develop. And the same thing applied to Sentinel. The same thing applied in the missile gap period that I mentioned earlier, when we started turning out Minutemen at the rate of one a day because we feared that the Soviet Union might do so too, or might be ahead of us in fact, when they were not.

The other tragedy is that it is very difficult to turn these machines off. Even when the intelligence changes, and it is apparent that the threat that you feared is not materializing, it has proved impossible to stop the build up in forces. We have seen that happen many times. We saw it here last August when Sentinel went right ahead despite the fact that revised intelligence indicated that the expected Chinese ICBM threat wasn't developing. We are going ahead full speed with our multiple warhead programs despite the fact that, somewhat to our surprise, the Soviet ABM deployment has halted.

The problem of long lead times and effectiveness

Well, of all the systems that can stimulate this action/reaction kind of phenomenon, I believe that an ABM system is the most worrisome that we have yet had to consider. There are fundamentally two reasons for this. One is that ABM systems have such a very long lead time. That means one must make a decision long before it is clear what the other fellow is up to, and our decisions with respect to Sentinel and now with respect to Safeguard are illustrative of that. We never expected a Chinese threat before about 1975, and we do not, I think, realistically expect a threat to our Minuteman force from Soviet SS-9 until about the same time period, if then, but we find it necessary to make the decision now. That is one very unfortunate quality of ABM systems. A far more serious one is that very great uncertainty is associated with any estimate of the effectiveness of an ABM system.

I am going to spend some time on this matter of effectiveness because I think it is a matter of the greatest importance and it will perhaps be understood better in an audience of physicists than it would in any other I have talked to. I have tried to make the point elsewhere with I think indifferent success. Perhaps it will be understood best of all by experimental physicists, people who have tried to make complicated systems work. Let me illustrate this point about uncertainty by comparing ABM systems with some other complicated kinds of equipment that we have built in recent years.

First, what about ICBMs? As you know we have built many of these. We believe they are reasonably reliable. By that I mean somewhere between 50 and 100 percent of those we have would get off the launch pad and go to about where we wanted them to go. That was not always true during the early days—the percentage was much lower. But I submit that an ABM system is a very much more complex system, and one in which one can have only very much lower confidence than one can in an ICBM system. Let me tell you why I believe this. First of all an

ABM system—unlike an ICBM—has to work when the adversary decides that it must. It has to work when the adversary's warheads arrive, and never at any other time. For an ICBM system that is not true—at least not with the kinds we have. Our ICBMs can ride out an attack and if they are not immediately operational, if there is a black box that needs changing, you can change it; that can't be done with ABM. It has to be ready to go.

Against it, the adversary has an enormous range of attack options to use. Hans Bethe has discussed many of these penetration techniques; they pose very formidable problems for the defense. For the offense the problem is very different. We know what Moscow looks like and that's what the target is. We know what other installations in the Soviet Union look like and they are targets for our ICBMs. So the target is a simple thing for us, in contrast to the kinds of targets the ABM system has to deal with. The environment for an ABM system is totally unknown, or almost totally unknown. We cannot remotely simulate that environment in a test program. We can't simulate many nuclear explosions going off at the same time, and yet the effectiveness of the defense may well be seriously degraded by that, and in my judgment, it will be, much more so than that of the offense.

Then there is the requirement for success.

If an ABM system is deployed to defend a large city and a single ICBM gets through, that is the end of the city. One needs essentially 100 percent effectiveness. That's not at all true of offensive forces. If 30 or 40 percent of the attack force survives to retaliate, that's good enough. So there is a very different order of requirement placed on the confidence in performance.

Finally, the defensive system involves more interacting components. So I argue that an ABM system is much less likely to work than an ICBM system.

Reaching for the moon is infinitely simpler

In every Congressional hearing that I've been at in the last few weeks, people have suggested that if we can make Apollo work we ought to be able to make an ABM system work. And I'd like to take a minute to discuss that. I think the Apollo problem is a much simpler one. First of all, the time for launch of Apollo is set well in advance. We know when it has to go—you don't know that with the ABM system. I don't know of any Apollo shots that have gone yet, although there may have been some, that did not have some "holds" during the launch sequence when something required checking. You can't do that with an ABM system. In the case of Apollo we're dealing with a nature that we hope is not malevolent. When you're talking about an ABM system you have to assume that the other fellow is.

We also are running our Apollo program with very competent men who are dedicated to it. I submit the kind of performance that you can expect there is rather different than you can expect of a bunch of draftees and junior officers who are in for a couple of years; and they have to run and maintain the ABM system.

Finally, I'd like to point out that Apollo really has not been a complete success. We did have a tragic failure, the fire, and it happened unexpectedly. We never would have expected, I think, before it happened, that the first fatalities in the Apollo program would occur on the ground in a simulation of a launch. Nor do I believe that in the case of the Soviet program anybody would have expected that the first failure would have been because a parachute failed to open. Now that's a very important point. Until you can test the system fully, you just can't have confidence that these very unexpected things will not foul you up. And that may well happen with ABM.

Now with this kind of a problem, with these illustrations, I think it's easy to understand why there are such extreme variations in the estimates of ABM performance. This extreme variation can be illustrated in the case of the Sentinel program. Defense Department spokesmen, and knowledgeable ones, including the present Director of Defense Research and Engineering, have claimed that the Sentinel system would be so effective that it could "deny" damage to the United States in the event of a Chinese attack. That means nothing gets through. On the other hand, you've heard Dr. Bethe and many other people argue that there is a very good chance that the other fellow may use decoys or other penetration aids that will easily overwhelm the system. And I submit there is also a substantial probability that the system may just fail catastrophically when it's turned on for the first time.

High- versus low-confidence penetration aids

This reminds me that I ought, perhaps, to say a word about low- and high-confidence penetration measures. It has a bearing on one's assessment of how an ABM system will work. If one has to get through and can afford the price, one will use very-high-confidence penetration techniques. In the limit, one will use a warhead for every interceptor that the other fellow has and thus exhaust his defenses. But a poor country like China may try other techniques—blackout, for example. Bethe mentioned some of them here. Such techniques will not permit one to have as high a confidence of penetration, but they may be just as effective, and before they are tried perhaps neither side will know. That's another consideration that I think bears on the extreme variability in the estimates of how well ABM systems will perform and how effective they will be relative to the offense.

Hans Bethe has quoted a figure to the effect that the Soviet Union could offset an ABM deployment by us by expending about one fourth as much on improvements in their offensive forces as we would have spent on our ABM. That calculation was based, incidentally, on very-high-confidence penetration techniques—essentially on exhausting the interceptors. I would argue that the Soviet Union might use another penetration technique costing 1/40 as much and they still might get through, or maybe not. Certainly they could not have as high a confidence of doing so. But there are these very large uncertainties and they are terribly important. With them there is a propensity on the part of both sides to overreact to ABM deployments. If you don't know how well the other fellow's system is going to perform, you are going to overdesign your offense—your're going to overbuild it—to make sure it will get through. On the other hand, if the defense has grave doubts about how well its ABM system will work, it will try to beef it up.

I think it's apparent that the worst kind of an arms race must result when one has both ABM and offense systems on both sides.

The Moscow defenses, according to the recent announcement by President Nixon, have 67 interceptor missiles. We could exhaust that defense and get through simply by expending at most 67 of our warheads, to use

up the interceptors. That would be the end of the defense. There may be other lower confidence, cheaper penetration techniques that also would be effective, but that one would certainly work. Now we are, as I mentioned earlier, putting multiple warheads on most of our submarine-launched missiles and on many of our ICBMs. When that program is completed, each of our Poseidon submarines will carry about 160 warheads. In other words, less than half of a Poseidon boat load would exhaust the Moscow defenses.

To convert a Polaris boat costs us probably $80 million; throw in a few more millions for the missiles, and I think you end up with a figure of the order of $100 million to offset that Moscow defense. The Moscow defense probably costs, incidentally, something like 500 million or a billion dollars. You can see how bad the defense looks in that case. On the other hand let me cite what we have done in a response to the Moscow defenses. We are not converting one Polaris boat or one half of a Polaris boat. Rather we are converting 31 out of the 41 we have, and a very large fraction of our Minuteman force as well.

We are spending many billions to overcome that defense, a 2-order-of-magnitude overreaction. Now what will happen to Moscow as a result of this reaction? Moscow is going to be very heavily targeted. According to the Director of Defense Research and Engineering, and I agree with him this time, it is likely that Moscow will be more heavily damaged than it would have been had it had no defense. We will overtarget it, giving the defense every benefit of the doubt; the defense will not work as well as we fear it might; more of our warheads will get through than conservatively planned upon; and the target will be more heavily damaged than had no defense been put there in the first place.

The hour grows late. What about Safeguard?

Let me now, in view of the lateness of the hour, turn to specific discussion of the Safeguard program.

Hans Bethe has mentioned that the technical problems for the defense of missile sites are different than for cities.

First of all, a Minuteman site can be defended with local defenses alone. We don't have to defend really large areas and this means we can rely largely on low-altitude defense. The penetration aid problem therefore, if it doesn't go away, at least becomes much simpler from the point of view of the defense and much more difficult from the point of view of the offense. That makes hard-point defense of missiles technically easier than defense of cities. For ICBMs, a *very*-low-altitude defense is feasible. One can tolerate near misses in the case of a Minuteman site. If an enemy warhead goes off a mile or so away, you probably won't have to worry very much about it, whereas if it went off a mile away from where we're standing now, and with a reasonable yield, we wouldn't be here. So that makes the fundamental problem easier.

An ineffective defense of Minuteman is entirely acceptable whereas one has to have a very high confidence of defense of a large city. Now that may seem like a strange thing to say, but I believe it. If one is relying on the Minuteman as a deterrent, it doesn't matter if some small fraction is destroyed. It probably doesn't matter if one third are destroyed. It doesn't matter even if a very much larger fraction is destroyed, just so a few survive. Their defense doesn't have to be very good.

You don't even have to know that it will work as long as the other fellow thinks it might. That might suffice for deterrence!

And finally, a preferential defense is possible. After deploying your interceptors and your radars, you can still decide which ICBMs to defend; the other fellow won't know where your defensive effort will be concentrated, so much of his attack will be wasted. If he is attacking your cities, however, and you are interested in a very high level of defense, you've got to defend them all, and he decides which few he is going after and concentrates his attack there. In that case the options lie with the offense.

Well, you may ask, then why do I oppose the Safeguard decision since it is now oriented to defense of our ICBM and since defense of ICBMs seems so much easier than defense of cities? I'd like to tell you. First of all, I believe no defense of our ICBMs is needed at this time. Second, if our retaliatory forces are in jeopardy, I believe we have better options available for removing that vulnerability than defense of Minuteman. Third, I believe the present plan is a poorly designed one, on technical grounds. Fourth, it is very expensive and we have other places for our money at this time. Fifth, I believe the Safeguard deployment will prove to be a stimulus to the arms race; and finally, I believe that it will be an impediment to negotiating an end to the arms race. Now let me deal with each of these in a little bit of detail, or at least with most of them.

We don't need Safeguard now

I don't believe we need defend our retaliatory forces at this time. We now can deliver something like 4000 warheads against the Soviet Union. Four hundred of those would destroy 75 percent of Soviet industry and something like 30 percent of their population. Even 100 warheads—just 4 percent of our total force of one-megaton weapons, if you like—would destroy something like 50 percent of their industry and perhaps 20 percent of their population. That is more than enough deterrence.

And our force is going to grow. It's going to grow to about 10 000 warheads with the introduction of multiple warheads.

Dr. Wigner has argued that that actually will result in a diminution in our capabilities because the warheads will be much smaller. I don't believe that at all. He has cited Paul Nitze's paper on this subject to illustrate his point, and I'd like to just quote from that paper since I happen to have it here. Mr. Nitze attempted to measure the relative effectiveness of one 10-megaton warhead against ten 50-kiloton warheads. The idea there was that one could trade these off one against the other.

You will find that, according to Mr. Nitze, ten 50-kiloton warheads are only five sixths as effective as a single 10-megaton warhead in destroying a city of two million people. But, also according to Mr. Nitze's figures, they are 3½ times as effective in destroying cities of 100 000, and there are many more cities of 100 000 in the Soviet Union than there are cities of two million.

Further, we will maintain in our inventory enough megaton-range warheads to target all the large cities, and we'll have these small ones available for cities of 100 000. We'll have these "small" ones available for cities that are so small that a 50-kiloton warhead, and mind you that's 2½ times Hiroshima, would grossly overkill the small city. In that case, of course, ten 50-kiloton warheads would be ten times as effective as one 10-megaton warhead. So I would argue that as we increase the size of our strategic force from 4000 to 10 000 warheads we're going to have a greatly enhanced retaliatory capability, and that for the Soviet Union seriously to contemplate a preemptive attack against this country would require that they have very high confidence of being able to destroy at least 95 percent and more likely 98 percent of those retaliatory weapons. (Frankly if I were making the decision, the confidence would have to be more than 99 percent before I'd consider such an attack.)

Now that's a tall order. That requires that they be able to knock out our ICBMs and our Poseidon fleet at essentially the same time—simultaneously. They can't start picking them off or we'll presumably retaliate with the residue. In addition, they've got to have an air-defense capability that will take care of our bomber fleet, which is being improved with the addition of new kinds of air-to-surface missiles. It won't do to try and get those bombers on the ground because an attack cannot be coordinated to get both the bombers and the ICBMs. I think it's just impossible. If you knock out the bombers first, that gives you the warning to let the ICBMs go. If you attempt to deliver an attack simultaneously on the two, you get about 20 or 30 minutes' warning that their ICBMs are attacking ours, and with 20 minutes warning you can get a very large fraction of the bomber force in the air. So they've got to deal with the bomber forces through air defenses.

Then they've got to deal with the tactical forces we have in Europe. We have about 7000 warheads there. Not all of them can get to the Soviet Union, and many of them are of a smaller yield, but it is an additional component of our retaliatory force. Now they have none of the capabilities required to destroy these forces at this time. We don't know how to do such things either, nor do we see how to develop the capabilities on the time scale that we're immediately concerned with. So what one is talking about here is a combination of a number of very improbable events before one need be really concerned.

Finally, even if they had all these capabilities, could a Soviet leader contemplate attacking this country in the expectation that we would withhold our Minuteman force until it was destroyed, recognizing that we would have 20 to 30 minutes' warning that a very large number of ICBMs were on their way? The Minuteman force is designed to ride out an attack, and I think it's wise that that is so, but I would think that one would hesitate to attack it on the assumption that it would be withheld.

For all these reasons it seems to me that the possibility of the Soviet Union developing a capability for knocking out, as I say, 98 percent or 95 percent of our strategic force by, say, the mid-70s is so small that it can almost be neglected—almost, but not quite.

Now what if it should happen—what if they should begin to develop such capabilities? I submit that we will have adequate warning of the necessary developments so that we can take appropriate countermeasures to assure that a significant fraction of our retaliatory force would survive a Russian attack. We don't need to act now.

The vulnerability of Minuteman under Safeguard

Let me talk now about the adequacy of the proposed defense for Minuteman. (And I might say parenthetically here that I've already mentioned that a large fraction of our bombers can be, I think, put into the air in adequate time, and for that reason the part of Safeguard that may be deployed to defend bomber bases is in my judgment a total waste. If the bomber bases should be attacked, possibly by Soviet submarine-launched missiles, say, they'd get the bombers, but we would have 1000 ICBMs which we could then launch if we wanted to do so. Since an attack against our bombers would be a foolhardy thing to do, that part of the Safeguard defense is wasted.)

But what about the part designed to defend Minutemen? Well, first of all, that part is poorly designed and it is not surprising that this is so. It was designed originally for an entirely different purpose. The components that are being used for Safeguard were designed when we were still thinking of defending all of the United States against a massive Soviet attack. They are the same components, with one minor modification, that were developed for the so-called Nike-X system. They were designed primarily to defend soft cities, not hard Minuteman sites. And there is a very great difference, as I mentioned earlier.

You can tolerate a nearby nuclear burst in the case of the Minutemen that would destroy a city. Any rational defense should take account and advantage of the difference in the requirement, and this one does not do that.

The radar is particularly bad. You see, the radar—a phased-array radar and Hans Bethe talked about this—is terribly vulnerable. That wouldn't make much difference in the case of the city defense because it's very much tougher than the city is. But it's very much softer than the

ICBM sites and therefore it becomes the target of choice for any attacker. The Soviet Union does not need to use SS-9s to knock out our radars. They can knock those out with cheaper, smaller, lower-yield, less accurate missiles. And the defense is dead. There is only one radar per site. That is not surprising; they cost $150 million a copy and each is protected by only a few dozen interceptors that can be easily exhausted—and then there is no defense.

When one goes through the simplest kind of calculations, it turns out that with this deployment we would be spending perhaps $25 to $100 million per ICBM saved, at a minimum. They cost perhaps $4 million to buy. Now we could do far better by putting in more Sprint interceptors, but even then a limiting calculation—I won't go through it now—suggests it still is not very satisfactory.

We could do better with other components designed for the purpose, and I personally would favor research to develop those components, but not deployment. But even an optimized hard-point defense system, one that uses the right components, looks bad to me compared with some of the other options we might have if the threat ever should develop.

To mention just one option, with the Poseidon program we are extending the range of our sea-launched missiles. That makes a much larger fraction of the ocean available, and that makes the other fellow's ASW program more difficult. We can build other still less vulnerable submarine forces; these are in the works, at least in a preliminary way.

The all-important, nontechnological option

Finally, there is the possibility of arms-control talks with the Soviet Union, and to me that's the most important possibility of all. Our present strategic posture is a very good one. I think the best thing we could do now is freeze it where it is. It is argued that we may be at a disadvantage if we talk now, while we don't have an ABM system and the Soviet Union has a small one. But I would think that the advantage we have in a very superior submarine force and a very superior bomber force far more than offsets that small advantage—and I think it's a very small one—that their Moscow ABM system gives the Soviet Union. The best thing we could do is to stop now. Soviet forces are growing; things will get worse if we wait. So now is the time in my view to go ahead with the talks. I think that's the highest priority of business we have, and we shouldn't let the ABM or anything else stand in the way.

The Soviet Union seems ready to talk

In this connection I'd point out that, though it has taken a long time, the Soviet Union is now apparently ready to talk. We're the ones that are holding things up. They may not be willing to talk seriously, but we ought to try. I think there are two reasons why they are ready. For the first time they can deal with us from a position that approaches parity. Dr. Wigner has said that we may be in an inferior position. The fact is that we can both inflict about 120 million fatalities on the other, or more. To me that's close to parity, close enough. The other reason I believe they may be willing to talk is that they may have reached, or come close to reaching, the same conclusions we have about the futility of ABM, and about the desirability of not going on with it. They have stopped their ABM system, and they're willing to talk. I think there's a convergence of views.

Well, let me conclude by saying a word about the costs of ABM. They are not only economic. I think there will be a very large cost if we go ahead with even the Safeguard system, a large dollar cost, but I believe beyond that that the decision would provide a great impetus to an arms race—though it wouldn't be as bad as the old Sentinel program would have been. It wouldn't be quite as likely to stimulate an improvement in Soviet offensive capabilities, but I believe Soviet offensive capabilities will be improved as a response anyway.

The Safeguard system uses the same components that could be used for defense of our cities, or, if you like, for putting in jeopardy the Russian retaliatory capability, and I believe the Russians must see it that way. Considering the long lead times that I have talked about, and the fact that our defensive deployment will reduce the lead time for a full-scale U.S. defense, I can't see how they can do otherwise. If we were to see a Safeguard system deployed in the Soviet Union I do not believe that we would disregard it. I really do believe we would begin to greatly improve our offensive capabilities. I believe the Soviet Union will do likewise if Safeguard is built.

So I think we are confronted not with a $6.6 billion expenditure here—that figure is probably low anyway for Safeguard—but whatever the figure is, it will be just the first increment. There'll be a further expansion in the arms race and we will spend a great deal more. So will they. Such expenditures will buy no increase in security for either side. Meanwhile, there are threats to our society here at home, I believe, on which such money could be spent more wisely.

George W. Rathjens, the first of our contributors as he was the first of the nonphysicist speakers at the APS meeting, is visiting professor of political science at the Massachusetts Institute of Technology. After his graduation from Yale University in 1946 he received the Ph.D. degree from the University of California at Berkeley in 1951. He taught chemistry at Columbia University from 1950 to 1953. From 1953 to 1958 he was with the Weapons Systems Evaluation Group in the Department of Defense. After a year as a research fellow at Harvard University, he returned to Washington as a member of the staff of the special assistant to the President for science and technology. In 1961 he was chief scientist in the Advanced Research Projects Agency of the Department of Defense, becoming deputy director of the agency later that year. From 1962 to 1965 he held various administrative posts with the U.S. Arms Control and Disarmament Agency, and from 1965 until he went to M.I.T. he was director of the Weapons Systems Evaluation Division of the Institute for Defense Analyses. Although his article was developed from the tape transcription of his talk before the American Physical Society, much of the material in it also appears in *The Future of the Strategic Arms Race: Options for the 1970's*, a pamphlet by Rathjens that was published earlier this year by the Carnegie Endowment for International Peace, and in "The Dynamics of the Arms Race," an article that appeared in *Scientific American* in April of this year. As the deadline for this issue of *IEEE Spectrum* approached, there was some concern over whether Rathjens' edited version of his tape transcript would arrive in time. The delay was engendered by his heavy late-spring schedule of appearances before Congressional committees considering the ABM. Along with such prominent figures as Jerome Weisner, the M.I.T. electrical engineer now provost of M.I.T., who was science adviser to Presidents Kennedy and, briefly, Johnson, Rathjens has been serving as sort of a scientific-strategic "field marshal" for the opposition to ABM.

Donald Brennan speaks—

For the ABM

Like Rathjens, Brennan is for negotiating with the Russians. Beyond that, their disagreements are basic. Brennan thinks that the Safeguard system, which he is in favor of seeing deployed, is insufficiently substantial. He favors the deployment of considerably more massive defensive systems that, unlike Safeguard, would be designed to save lives instead of the retaliatory missiles that hold other human lives as hostages for our own. He thinks that both we and the Russians should deploy such massive, life-saving systems in preference to any further escalation of offensive forces by either side. It is in our common interest, he stresses, to limit damage on both sides should war occur—and war, he emphasizes, can indeed happen; we've just been lucky so far. Brennan thinks that ABM is a protective umbrella that we can well afford, and that the choice to be made in allocating our resources is not between defensive missile systems and other national priorities, but between such systems and offensive ones—a proposition he thinks the Russians would agree to, to judge from their historic preoccupation with defense over offense. His truly novel assertion, however, is that appropriate defensive systems might also provide the political-psychological umbrella under which the people of both nations could feel reasonably secure while negotiated reductions in offensive force levels were taking place.

I am going to emphasize my own thinking...

I should begin by emphasizing that I'm not going to speak only about the Safeguard system. I am not here tonight, on this or on any other platform, simply as a spokesman for the Administration. As many of you know I have studied strategic and arms-control matters for many years, and have studied the problems of ballistic missile defense, in particular, from various points of view for several years—sometimes opposing, sometimes supporting such defense. I have during this period developed my own rationale about these issues, and I am going to emphasize my own thinking much more than the official rationale for the Safeguard program.

I do happen to believe that the Safeguard program is sensible and I believe that many of the justifications for it that have been advanced by the Administration are sensible. On the whole I'm not going to discuss those with you. I think you may have heard enough of some of those arguments from other spokesmen in other settings, and I would rather talk about some of the issues that have been rather less discussed.

In particular, I shall not much discuss the protection of offensive forces tonight, although that is one of the declared objectives of the Safeguard program. I believe it is a sensible objective. I would comment on one point that Dr. Rathjens made. He said that the components that are being used for the Safeguard program were not designed for protection of the offensive forces. That is substantially incorrect. Actually the Nike-X technology has been thought of as having dual missions for quite a few years, and much of the development has been carried out with the objective of dual missions. Some of you may recall that when Secretary McNamara first spoke of the Sentinel deployment in his September 1967 speech in San Francisco, he said at that time that some of the deployment would be intended to add protection to the Minuteman base system. That objective dropped out of the discussion shortly thereafter, and the Sentinel program discussions through the 1968 period focused much more on city defense against light attacks. But the technology had been developed with the objective of force protection in view and it might well have been scheduled as early as 1967.

Let me pass on to the type of objectives I think have been rather less emphasized in public discussion of the possibilities of defense. Here I am going to talk to you about substantial defenses for protection of cities, mainly for the protection of cities against Soviet attack.

The fact is: war can happen

The basic problem that concerns many of us who are interested in the possibility of defense of cities against Soviet attack is the fact that war can happen. You know we have lived through two decades of the nuclear era so far, and have had no nuclear war since Hiroshima and Nagasaki, and many of us carry around a lot of confidence that probably it won't happen. Nevertheless, anybody who is really confident that it cannot happen is, in my judgment, overconfident. There is a real chance that some crisis in the Middle East, in Cuba, in Berlin, or wherever it may be, can escalate to a nuclear war. And if we have an all-out nuclear war with prevailing forces, and no defenses whatever, the consequences would be utterly catastrophic.

Attacks with prevailing levels of Soviet forces against the U.S. could result in 120 million fatalities; 100 to 120 million fatalities is not at all unrealistic for the scale of damage that could occur. And, of course, you'd probably lose half or more of the industrial capacity of the country. Probably more than half, because much of it would be colocated with the larger cities, and without defenses you could probably expect to lose most of the larger cities. So you would lose probably more than half the country, in terms of both populace and industry.

The kinds of defenses that can be deployed for costs in the range of, say, 15 tc 25 billion dollars would reduce that level of damage from something of the order of half the country to something of the order of 10 or 15 or perhaps 20 percent of the country, if the Soviets do not make substantial increases in their offense force. I'll come back to that hypothesis a little bit later on. For the moment I do wish to emphasize that you're talking about making an enormous difference. The United States after an attack in which more than half the country was lost has very poor prospects of anything resembling recovery. You can change this to a situation in which you may have a major disaster—no one would say that losing 10 percent of the country is anything other than a major disaster—but one which the country as such might very well survive. And you might thereby be saving directly say 60 or 80 or perhaps as many as 100 million lives. That's a lot of lives to be saved. That's a lot of insurance value for the country, if it works, and I would argue that in all probability it can be made to work.

Now that may sound, at first hearing, as if I differ with some of the cost estimates Dr. Bethe quoted. In actuality I think we're talking about some of the same estimates. He said that the Soviets could offset such a defense, a defense of the level that I'm speaking about here, for an expenditure of the order of one quarter of the cost of the defense, if they are interested in achieving something like a 20 percent fatality level. He was in a sense focusing on the hole in the doughnut. Let me tell you what the rest of the doughnut is like and then you will understand, I think, why one can look at the same information from two quite different perspectives.

Substantial defenses are not easily offset

My contention is that a substantial defense of the sort that I was speaking of is, in fact, hard to offset even if the Soviets choose to do so. They would have to spend a lot of money, time, and trouble to do it. Suppose we deploy such a defense, one that would reduce the level of fatalities to something like 10 or 15 percent if they did not increase their offensive forces in immediate response. If they wish to restore the level of potential fatalities that would have prevailed in the absence of a defense, that is to say something like 100 or 120 million, I think it is generally agreed by most people who have studied these matters closely in late years, and I emphasize "in late years," that it would require an expenditure on the part of the Soviets of something like the same amount as the cost of the defense in order to neutralize it; and if they wish to do it with confidence, quite likely more. Dr. Rathjens correctly pointed out that these kinds of estimates are based on high-confidence penetration tactics, but if one is talking about preserving something like "assured-destruction" capabilities, and the usual argument is that the Soviets would be bent on preserving assured-destruction capabilities, they are not going to do it with low-confidence penetration aids, and it would then require something like an expenditure on the level of the defense.

Now suppose instead of wanting to get 100 or 120 million fatalities the Soviets were content to raise the level of estimated American fatalities to something like 40 million from the 20 million that might have obtained if they didn't increase their offensive forces. They would not need, of course, to spend as much on their offensive forces. Obviously, if they add just one missile they'll do something more to the United States, at least in an expected sense. If they wish with fairly high confidence to bring the level of expected fatalities from 20 million back up to 40 million, they must add to their offensive forces an increment costing about one quarter as much as our defenses would cost in the first place.

This is the number that Dr. Bethe was speaking of, and that's why it is looking at the hole in the doughnut to say, as he did, that it's going to be easy to offset defensive spending. I would observe that that cycle of U.S. defense that would be one quarter offset by the Soviets, one quarter in dollars, would have resulted in a saving of 60 million American lives if you'd had that war. That partial Soviet offset is by no means going to nullify the insurance value of your defense. Many of us would argue that the Soviets would be most unlikely to do that even if you do not discuss the matter with them.

You might ask how stable are these technical prospects that I am speaking of here. I claim that today it looks as if it's fairly hard for the Soviets to neutralize a substantial defense of the kind that I am speaking of. You might say, well suppose that is true today, what kind of assurance do you have that by the time you get the system deployed in, say, 1975 or 1977, the Soviets won't have found some cheap and reliable means of penetrating your defense? Or perhaps even the Chinese will find a way of penetrating it. Well no one, of course, can give a dogmatic answer to a question of that form. However, I'll give you a parallel that some of us find quite persuasive.

Penetration aids can't be both cheap and reliable

I heard Dr. Rathjens this afternoon, in a preliminary discussion of these matters, refer to our ASW [antisubmarine warfare] system and say that it was pretty worthless. I think this points to a judgment that many people share, people who have looked at the general problem of attacking submarines and who have looked in particular at the problem of attacking Polaris submarines. This judgment is that prospects for successfully attacking Polaris submarines seem very poor. And more precisely what I mean by that is that it looks as if a reliably successful attack on Polaris submarines would be very expensive; short of very expensive systems it does not seem as if there are any reliable ways of attacking.

Now that gives us a lot of confidence in the Polaris submarine force, not unlimited confidence but a lot. Whence does all that confidence derive? If you think about it for a while you will decide it is precisely because we have spent approximately one-half billion dollars a year for something upwards of ten years in research and development on antisubmarine warfare. And with something like ten years of major research and development in antisubmarine warfare nobody has found a cheap and reliable means of attacking Polaris submarines. It's only from that negative fact that you have some confidence, fairly good confidence I would argue, that the Polaris submarines are reliable components of the deterrent force.

Now I would suggest that a like fact is becoming true in regard to missile defense. It hasn't been nearly so widely noted, perhaps because of the intense level of controversy surrounding this subject. But, in fact, the kind of technology that would be used for an effective defense of this kind has been visible and under study since about 1963. People have been making experiments with it since about 1963–1964 and for about the past five years we have had a major research and development program, at a level of perhaps one third of a billion per year, on methods of penetrating one of those effective defenses. And to the present time, with all that expenditure on research and development, nobody has found a cheap and reliable means of penetrating a substantial missile defense. You can find expensive ways of doing it, with high-confidence devices like multiple warheads, or you can find cheap ways that may do something but more likely will not.

I would argue from this parallel that one should correspondingly begin to have some confidence that the technical effectiveness of a defense is beginning to look pretty reliable, at least for the substantial kinds of defenses that I am speaking about here.

Strategic implications of an effective defense

Now let me mention an often neglected, but important, strategic interaction resulting from defense. The deployment of a defense, in addition to making war less damaging if it occurs, is in fact likely to make the war less likely in the first place. Some people occasionally argue that if you reduce the fatality level to about 20 or 30 million or some number of this kind by deploying such a defense, then people will become button-happy, and you're more likely to have a war. I think it takes very little serious reflection about either U.S. or Soviet decision-makers to dispel the idea that if the fatality levels will be "only" 20 or 30 million, then people are going to be wild about pushing buttons—they just aren't. Nobody is going to be crazy about having that nuclear war, even in that world. No more than they were quite a few years ago, when the offensive forces were at much lower levels.

I would argue instead that the kinds of uncertainty that will be introduced by a defense are going to be such as to make the initiation or the planning of any attack very much more problematical. It is hard to figure out what is going to happen, especially with light attacks of the sort that might reasonably constitute the first step of a strategic escalation. If you try to escalate, by degrees, some sort of tactical nuclear-war situation into a situation involving limited use of strategic missiles, you will find that even a light defense will complicate the situation very considerably. And from this point of view I would say that the impact of 67 interceptor missiles (67 as of last week or something like that, not necessarily 67 next year) around Moscow, which incidentally can probably reach throughout the whole of European Russia, is quite a bit more than the mere number 67 suggests. They really constitute something of a complication.

What about the Russian reaction?

Let me then pass on to what many critics, and I would say probably rightly, consider to be the major potential problem associated with defenses of the kind I'm discussing. Both of my opponents tonight, for example, were heavily concerned with the possibility of stimulating an offensive-force response from the Soviets if we were to deploy such a defense. There is a widespread concern that if the United States were to build a substantial defense, it would only require the Soviets to increase their offensive forces in response. I would argue that arms-races responses of that kind are not necessary, they are not even likely, and you can certainly try to control them with discussions with the Soviets, which I favor at least as much as anyone else speaking on this platform.

If one looks at the attitudes that the Soviets have exhibited in respect to strategic forces, as in Kosygin's statements for instance, you can find many statements from the Soviets, both official and unofficial, that suggest the Soviets are much more interested in a defensive emphasis in their strategic posture than an offensive emphasis. Historically they have outspent the United States absolutely by factors like 3 or 4 to one in their expenditures on strategic air defenses. And, of course, in the recent past they have been outspending us infinity to one in strategic missile defense. Well, if you look at the evolution of the Soviet attitudes, which have not suggested that they are bent on some kind of pure offensive-force deterrent posture, it is very easy to believe that even if you did not have serious discussion with the Soviets, the deployment of an American defense might well result either in no Soviet response at all, or, if you got one, that it would more likely be a defensive one. They might very well increase their own defenses in response.

An assured destruction posture is slightly insane

I would argue that some of the prevailing attitudes in the United States that emphasize a posture of assured destruction are themselves a little bit insane. We have no major vested interest in being able to kill 100 million Russians. We do have an important interest in maintaining the United States' strategic posture in a good relationship with the Soviets. For all kinds of reasons I'll not attempt to spell out at length here, I do not believe that we have anything like a fundamental requirement of being able to destroy 50 or 100 or any other fixed number of millions of Soviet citizens without regard to what they can do here. And furthermore, if the Soviets wish to build defenses I think the United States should be building defenses, and not offenses, in response. We have a greater stake in live Americans than in dead Russians.

I'm not the only one who believes this, by the way. Some of the best-known critics of missile defenses have gone on record as saying that if you could hold down the escalation of offensive forces by adopting a defensive posture, then maybe missile defense would be a good buy. And now I'm arguing, of course, that you should hold down any offensive-force response. I argue that you probably would not get a major offensive-force response from the Soviets even if you didn't discuss the matter with them. But, of course, I argue even more that we ought to be seriously engaged in discussions with the Soviets attempting to limit any offensive-force responses that might otherwise occur, and I believe we would find a very ready audience for that purpose. And if you carry out such a program you would establish an environment in which you might have both an offensive-force ceiling and the possibility of deploying defenses on both sides to limit the possible damage should war occur.

Can offensive forces be frozen or reduced?

I'll quote to you what one substantial critic of missile defenses has said about the possibility of such a program. He said: "There are people who say that it is better to spend your money on ABM than on more destructive power. If one could do this, that is, freeze the offensive power on both sides and build defenses—this might make ABM a good thing. If Congress, the military and manufacturers were happy to build only defenses and did not pressure to add to the offensive forces, maybe ABM would be a good buy." That quotation happens to be from Jerome Wiesner, who has said something of this form in several contexts. And he is by no means the only critic of missile defense who has taken some position of this form.

Wiesner went on to say that he does not believe you could get a ceiling on offensive forces, but he made that judgment on the basis of what he thought was a fixed U.S. requirement in the minds of many people, not himself, that we would have to be able to kill 100 million Russians. Well I have taken something like a small poll in the Pentagon, among other places, and I don't find anything like a fixed requirement, even among people in uniform, that we should be bent on our being able to kill 100 million Russians. I do find a very substantial willingness to cut back on offensive forces if we could build up defenses. And this, I argue, is what we should be doing.

The heart of arms-control doctrine

I would say that we and the Soviets have an important common interest in limiting deaths in the event of a war. This is basically the heart of arms control. You know the strategic offensive forces have been built up largely out of an arms-race response. The Soviet offensive forces are in response to ours, and increases in ours have been responses to Soviet offensive forces. This is how the threat has arisen, almost purely out of an arms-race situation. Therefore, I would argue that we have an important common interest in limiting the risks from these offensive forces. Many people have understood for many years that arms control was intended to limit the likelihood of war and to limit the scope and violence of a war if it occurred. I repeat, we have a major common interest with the Soviets in limiting this damage. We have no common interest with the Soviets in *facilitating* the damage of a war. And that is exactly what you would be doing if you decided deliberately to abstain from any and all defenses.

If you are interested in facilitating the damage that a nuclear war might cause, you could find much cheaper strategic postures to pursue that would do that quite reliably. We could agree to mine each others' cities with bombs planted under the major cities and secure firing crews and arrangements to set them off. It would save you all kinds of worry about vulnerabilities and things of this kind. Some of you may know that systems of this kind were discussed some few years ago. Now you could go back to programs of that kind if you think the country would buy one, if all you want is to facilitate the damage of a war between the United States and the Soviet Union.

Back to the question of costs

Let me say a little bit about the subject of cost. It is an important point here and I wouldn't, by any means, minimize the importance of cost as an item in this debate. I myself have been mainly talking about larger programs than the Safeguard program. I don't believe that you need to go as far as 50 billion dollars, which Professor Bethe had on one of his charts. In some recent correspondence with him I thought he mentioned a somewhat lower number as plausible for a heavy defense. It might in actuality be 30 billion dollars or it might be 15 or it might be 35 for a heavy defense. Very few people are willing to talk seriously about defenses outside of this range.

Well, even if you accept that it might cost 25 billion dollars, that's a lot of money. I agree it's a lot of money, and I think anyone would agree it's a lot of money. How do you get some perspective on such a cost? There are two ways that I think one should consider. First, one might compare it with what has actually been spent by the country on air defense. I wonder how many of you realize that the United States' expenditures for air defense total something like 50 billion dollars since World War II. That's adding up the dollars as they were spent; if you look at the investment cost for the present system in 1969 dollars it is probably a number of that order. And we are still spending close to 2 billion a year on operation and maintenance of that system, which buys a lot less insurance for the country than some of these proposed missile defense systems would. So from the viewpoint of what we have traditionally spent on defense against attack, and incidentally I would emphasize again that we have spent only about one third as much as the Soviets have in this area, by that standard a number like 15 billion dollars, or perhaps 30 billion at worst, is not a wildly extravagant expenditure for a country with our resources to contemplate.

Allocating the nation's resources

Now look at this matter of cost from a quite different perspective, the question of allocation of resources within the society. There's been some rather strange discussion of the allocation issue. We have not traditionally taken a look at any particular expenditure and asked what would that do when transferred to some other area, and is that particular other application more important to us than the thing that we were thinking of spending that money on in the first place. For example, I have a number of friends in the high-energy physics business, some of them more or less close to the Weston accelerator, which has an estimated cost of 300 million dollars. I wonder how many people who have been concerned with that accelerator ever thought, "Well, you might with that money buy 30 or 50 quite respectable schools in Harlem." Now I don't suggest that's actually an appropriate comparison. I would suggest it is equally inappropriate to say that money to be devoted to missile defense should rather be used for this particular other purpose, or for that particular other purpose. Traditionally, we judge these needs in relation to some sort of unspecified general pressure, and we try to judge them one by one.

I would suggest to you that an appropriate ground rule for deciding what it is reasonable to spend in this area is that the United States strategic force budget should not be substantially less for any substantial period of time than the Soviet strategic force budget. I do not insist that it should be twice as large because our gross national product is twice as large. I'm willing to settle for something comparable to the Soviet strategic force budget, averaged over some period of time. Within that ground rule I think it is very easy to finance a defense that would make good sense for the country. You can, if you like to think of it so, divert that expenditure away from your offensive forces. I'll say again, we have much more interest in live Americans than in dead Russians. It seems to me entirely proper to cut back on offense to some degree as you're building up on defense, because you're basically shifting in some degree the fundamental basis of your security policy.

How to accomplish strategic disarmament

I want to point out as a final observation here, actually something more than an observation, that by following programs of this kind I know how to do strategic disarmament. I do not think any of the substantial critics of missile defense have any program in view for doing anything like strategic disarmament. Professor Bethe spoke of maintaining a posture of pure destruction, as if this were an objective that he wished to continue. Speaking for myself, or rather speaking actually for quite a few others, I do not want forever to live under a nuclear sword of Damocles. I think we have a long-term interest in getting out from under postures of that kind because if you remain under them, sooner or later there will be accidents and you will have a catastrophe.

The only way that I can foresee of achieving some major reduction, and perhaps elimination, of the U.S.–Soviet strategic nuclear threat, is by the evolution of defenses. By no other route can I foresee a political willingness to reduce allowed offensive forces to a level at which the plausibly possible clandestine missile forces could compare with the allowed missile forces.

With the presence of defenses, we might be able to accept a program in which offensive forces are reduced to numbers like 100 missiles on each side, or conceivably to even no strategic missiles on each side. If you do not have defenses and wish to reduce the scale of damage that the country might suffer in a major war to the level that could prevail in the presence of heavy defenses, then you would be obliged to go down in offensive forces to numbers like 50 missiles—perhaps 100 missiles, perhaps 20, depending on their size and characteristics—but in any event it would be a small number, if one is talking about a maximum fatality level of something like 20 million.

I know of no serious student of these matters who believes that within anything like the next decade, for instance, you could find a way of reassuring the United States that the Soviet Union does not have clandestine missile forces of the level of 20 or 50 missiles, or whatever it be. Yet such a force would be very significant if you were to reduce offensive forces directly to such levels. On the other hand, if you start with a program of at least medium defenses, then under the cover of those defenses it would be quite possible to reduce strategic offensive forces to levels at which the damage that could occur would be greatly reduced, relative to current possibilities.

This summary of the issues is too short to do full justice to the subject. But I hope it will indicate some of the considerations that move many of us to support the deployment of defenses.

The defense rests.

Donald G. Brennan, Rathjens' opponent, was president of the Hudson Institute—a strategy-generating think tank—from July 1962 to May 1964. Prior to joining Hudson Institute, where he now concentrates on research, Brennan put in nine years as a research mathematician and communication theorist at the Lincoln Laboratory, M.I.T. This particular intellectual stronghold of the military–industrial complex has, strangely enough, nurtured many "amoral" technicians now numbered among those who are most strongly opposed to ABM. Although Brennan is for the ABM, and for defensive systems in general, his background is equally unusual and interesting. Through some mysterious chemistry, for some inexplicable reason, this professionally successful mathematician, this man whom the uncritical might seek to dismiss as a slave of his military–industrial masters, began to devote much of his time at M.I.T. to arms control and national security problems, while continuing his technical research. His professional preoccupation with arms control didn't begin until 1957, however, when he organized a study group that led to the 1958 Summer Study on Arms Control held in Cambridge under the auspices of the American Academy of Arts and Sciences. This exercise was repeated in 1960.

Since then, Dr. Brennan has served as consultant to the Department of State, the Department of Defense, the Arms Control and Disarmament Agency, the Executive Office of the President, and has become a member of a task force of the Defense Science Board. He is editor of the well-known anthology, *Arms Control, Disarmament, and National Security* (New York, George Braziller, 1961), and guest editor of its predecessor, the special (Fall 1960) issue of *Daedalus* on "Arms Control." He also is editor of the new international journal *Arms Control and National Security*. He has contributed articles on arms control to a number of journals, and has lectured at Harvard, M.I.T., the University of California, and defense study centers in London, Bonn, Paris, and Oslo. He also has given seminars on arms control in Moscow.

Brennan received the B.S. (1955) and Ph.D. (1959) degrees in mathematics from the Massachusetts Institute of Technology. Before that he was engaged in radio engineering as a registered professional engineer in Connecticut. He is a Senior Member of the IEEE and a member of Sigma Xi, the American Mathematical Society, and the Institute of Strategic Studies in London.

The distributed-lumped-active network

Its application to filtering problems

The circuit designer faced with the present complexity in analyzing and synthesizing network realizations can look forward to more practical methods as well as more frequent use of the digital computer

Lawrence P. Huelsman *University of Arizona*

The development of modern integrated circuit techniques has added a new and important network element to the list of those usually considered by the filter designer—the distributed RC network. This article discusses the properties of such networks and shows how they can be combined with lumped passive elements and active elements to produce a class of distributed-lumped-active (DLA) networks. Such networks can be applied to a wide range of filter requirements and have many advantages. For example, DLA realizations usually require fewer components than comparable realizations using lumped elements. Because partial differential equations are required to model distributed components, many new techniques of analysis and synthesis must be used in the design of DLA networks. A discussion of these techniques, including many that may be implemented on the digital computer, is presented and examples of some typical DLA realizations are given.

The classical techniques of electric-filter design are based on the use of passive lumped-network elements; namely, resistors, capacitors, and inductors. Discussions pertaining to these elements form the bulk of currently available text material devoted to circuit analysis and synthesis. These are also the elements usually treated in handbooks of filter design. In the last decade, however, additional degrees of freedom have been provided for network designers by the inclusion of active devices in the catalog of practical network elements. The list of such active devices not only encompasses the more traditional ones such as operational amplifiers and controlled sources, but also includes the more modern ones such as the gyrator and the negative-immittance converter.

In network synthesis, the inclusion of active elements has provided a rather striking revolution. For one thing, it has been found to be possible, both in theory and in practice, to dispense with inductors in many network realizations. This is especially true in the lower frequency ranges. The class of networks consisting of resistors, capacitors, and active elements (the active *RC* class) not only realizes all the network functions realizable by circuits containing inductors, but it also realizes functions that are not realizable by the passive *RLC* class of networks. In addition, the synthesis techniques for many classes of active *RC* networks are far simpler than the synthesis techniques for corresponding passive *RLC* realizations. Moreover, such specialized techniques as the state-variable synthesis approach may be used to provide high-Q realizations with extremely low sensitivities.[1] Thus, for the filter designer who chooses to use active instead of passive realizations, in the applicable frequency ranges, there are many potential advantages and relatively few disadvantages. Note especially that, with relatively minor modifications, the user may continue to apply all the conventional tools of network analysis with which he has become familiar and comfortable.

With the advent of integrated-circuit and thin-film technology a further addition to the filter designer's catalog of elements has appeared. The basic manufacturing processes associated with these techniques readily produce a network element characterized by the word "distributed." In its usual form, such an element is referred to as a distributed *RC* network, since it consists of layers of resistive and dielectric material (Fig. 1); thus, resistive and capacitive paths exist between the various terminals, as indicated by the symbol in Fig. 2. In the preceding paragraph, we pointed out that the introduction of various active network elements extended the capability of the filter designer, simplified his synthesis procedures, and in general produced many advantages with few disadvantages. The distributed network, however, in an almost cataclysmic step has managed to undermine all the accepted and conventional methods of network

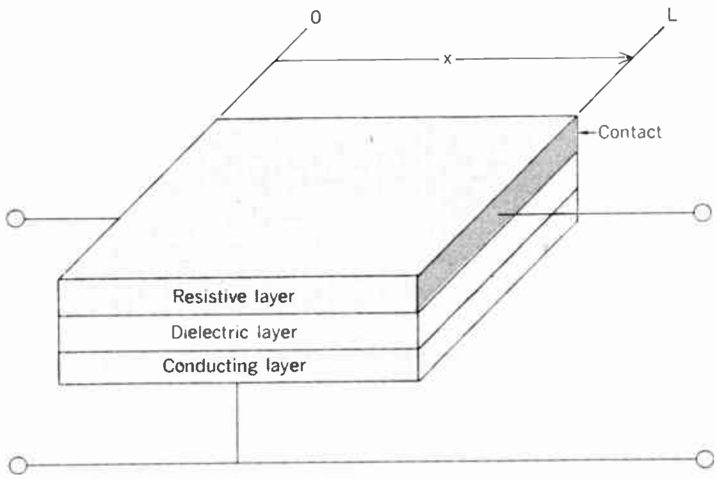


FIGURE 1. Example of a distributed RC network.

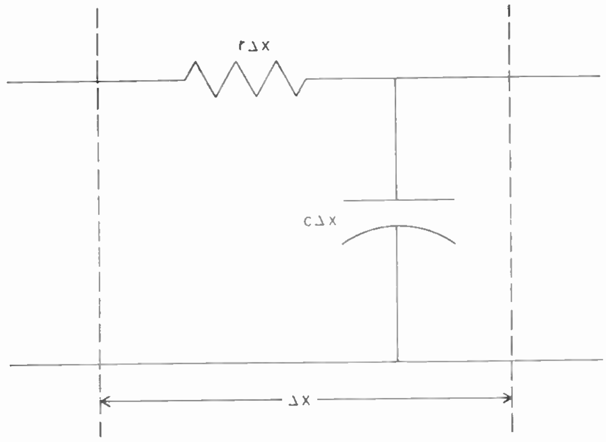


FIGURE 3. Transmission-line model of a simple RC network section.

FIGURE 2. Schematic diagram for the network of Fig. 1.

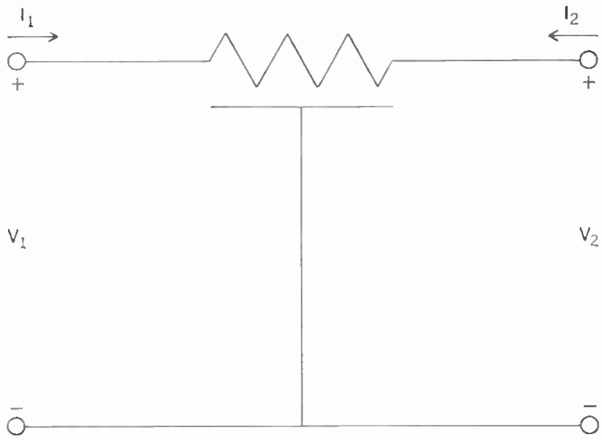
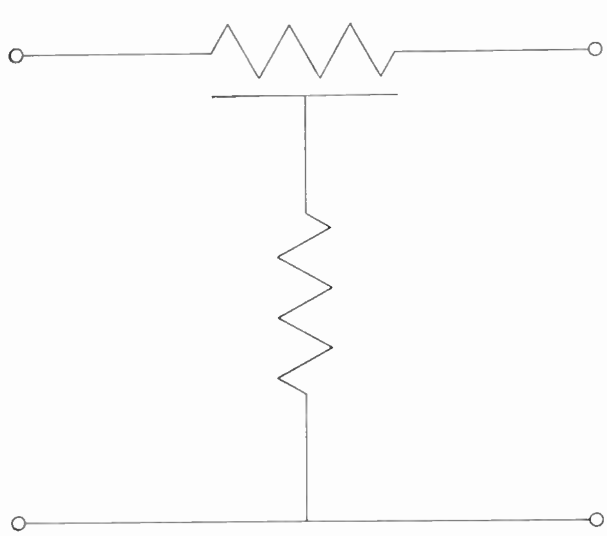


FIGURE 4. Distributed RC network in which a series lumped resistor is employed.



analysis and synthesis. The result has been wild threshings on the part of network theorists to develop new techniques that are applicable not only to distributed networks, but also to the broader class of networks that includes distributed, lumped, and active elements. These techniques have covered a wide scope, ranging from the intensely theoretical to the extremely practical. This article will describe some of these techniques and, in general, discuss the problems inherent in the analysis and synthesis of DLA (distributed-lumped-active) networks.

Properties of distributed RC networks

As an introduction to a study of the DLA network, this section will consider the properties of one of its components—the distributed *RC* network. To emphasize the significant points of our discussion, we shall assume that the network has a simple rectangular shape with contacts extending completely across the ends as indicated in Fig. 1, and that the resistive and insulating materials are completely uniform. Thus, the equipotential and electric flux lines in the resistive layer will be, respectively, at right angles and parallel to the coordinate x shown (we will also ignore fringing effects at the edges). The result of these simplifying assumptions is to permit the use of a standard one-dimensional "transmission line" analysis for the network, as opposed to the more general case, in which variations of potential in two dimensions would have to be considered. If we let the length of the line be L (see Fig. 1), then a section of the line of incremental length Δx may be modeled according to Fig. 3. In the limit, as $\Delta x \to 0$, the equations for this section become

$$\frac{\partial v(x,t)}{\partial x} = -ri(x,t) \qquad \frac{\partial i(x,t)}{\partial x} = -c\frac{\partial v(x,t)}{\partial t} \tag{1}$$

where r is the resistivity per unit length, c is the capacitance per unit length, and $v(x, t)$ and $i(x, t)$ are, respectively, the voltage and current at the point x in the line. Combining the relations given in Eq. (1) we obtain the well-known parabolic second-order linear partial differential equation

$$\frac{\partial^2 v(x,t)}{\partial x^2} = rc\frac{\partial v(x,t)}{\partial t} \tag{2}$$

This type of equation is also referred to as the heat or diffusion equation. A solution for Eq. (2) may be found by various techniques. For our purposes, let us take the Laplace transform of the variables $v(x, t)$ and $i(x, t)$, which gives the variables $V(x, p)$ and $I(x, p)$, respectively, where p is the complex frequency variable. Assuming zero initial conditions, we may write Eq. (2) in the form

$$\frac{d^2V(x, p)}{dx^2} = rcpV(x, p) \tag{3}$$

Standard mathematical techniques may be applied to solve this equation and the corresponding relation for the current variable $I(x, p)$, and to apply boundary conditions to define the voltage $V(0, p)$ (the transformed voltage at $x = 0$) as the usual two-port input voltage $V_1(p)$, the voltage $V(L, p)$ as the two-port voltage $V_2(p)$, etc. Applying these techniques we obtain the following equation that defines the z parameters for the uniform distributed RC network[2,3]:

$$\begin{bmatrix} V_1 \\ V_2 \end{bmatrix} = \frac{R}{\sqrt{pRC}\sinh\sqrt{pRC}} \begin{bmatrix} \cosh\sqrt{pRC} & 1 \\ 1 & \cosh\sqrt{pRC} \end{bmatrix} \begin{bmatrix} I_1 \\ I_2 \end{bmatrix} \tag{4}$$

where $R = rL$ is the total resistance between the input and output terminals, and $C = cL$ is the total capacitance (as measured by a high-impedance source).

At this point of our development, we make our first unpleasant discovery about distributed RC networks; namely, *their parameters are expressed as hyperbolic functions of the square root of the complex frequency variable.* In general, such functions have an infinite number of pole and zero locations. This is certainly a far cry from the simple algebraic polynomials that characterize lumped networks (even lumped networks including active elements), and obviously will introduce many complications into the analysis process.

Now let us see what happens if we permit the network to be tapered. In this case, the resistance per unit length r will not be constant; that is, it must be written as $r(x)$. Similarly, the capacitance per unit length will be $c(x)$. The solution of the partial differential equations for this case rapidly becomes a formidable hurdle. For an exponential taper defined by $r(x) = r_0e^{ax}$, the solution still is not too involved[4]; however, for anything else we rapidly descend into a morass of mathematical complexity. For example, although a solution exists for a linear taper, it requires the use of Bessel functions.[4] For the more general case, various authors have predicated the use of matrizants (infinite matrix series of multiple integrals),[5,6] generation of an eigenfunction expansion by the use of a Bubnov Galerkin technique,[7] and many other proposals all well-calculated to alienate the practicing engineer. Despite the sophisticated appellations carried by these techniques, their results may be considerably in error because of the basic assumption of a one-dimensional model for the network. As an example, one experimental study showed that a "notch" produced by a circuit including an exponentially tapered RC network showed discrepancies of over 10 percent between the experimentally observed results and the theoretical results predicted by a one-dimensional model for the distributed network.[8]

We have just discussed a problem encountered in finding a suitable set of parameters to represent distributed RC networks. A further problem is encountered when one desires to interconnect such elements with lumped and/or active elements and investigate the properties of the overall network. The resulting expressions contain transcendental and irrational terms of the general form shown in Eq. (4), as well as algebraic expressions of the type normally characterizing lumped and active elements. As a simple example of this problem, consider the case of a distributed RC network with a series lumped resistor as given in Fig. 4. Such a network has a zero transmission notch in its sinusoidal steady-state voltage transfer function $V_2(j\omega)/V_1(j\omega)$. The frequency at which this notch occurs, however, must be found by solving the transcendental equation[9]

$$\tanh\sqrt{\omega RC/2} = -\tan\sqrt{\omega RC/2} \tag{5}$$

Solving this equation for the value of ω at which the notch occurs is a nontrivial task, especially for the engineer accustomed only to finding the zeros of rational functions.

From these remarks, it is easy to appreciate the fact that distributed networks usually are not amenable to description in terms of their pole and zero locations. For some purposes, however, it is possible to model such networks in terms of their *dominant* poles. For example, consider the open-circuit voltage transfer function $V_2(p)/V_1(p)$ for the uniform distributed RC network of Fig. 2. It may be shown that the network function has an infinite number of poles p_n on the negative real axis. The location of these poles is given by the relation[10]

$$p_n = \frac{-(2n+1)^2\pi^2}{4RC} \qquad n = 0, 1, 2, \cdots \tag{6}$$

The zeros of the transfer function are all located at infinity. For some applications, it may be sufficient to consider only the dominant poles close to the origin, such as the ones at $-\pi^2/4RC$, $-9\pi^2/4RC$, and $-25\pi^2/4RC$, to give an accurate representation of the network function. Another possibility is to replace the dominant poles with a single effective dominant pole, using that to characterize the distributed networks.[10] The determination of the location of such a pole, however, though straightforward for the uniform network and for simple tapers such as the exponential taper, becomes a formidable task for the case of arbitrary tapers.

Because of the difficulties encountered in determining the characteristics of distributed networks over the entire complex frequency plane (e.g., finding the locations of the poles and zeros that determine their performance), considerable attention has been given to representing these elements in terms of their behavior under conditions of sinusoidal steady-state excitation. One advantage of such a representation is that the characteristics of a given distributed element are easily combined with the characteristics of lumped and active elements to yield an analysis for a specified DLA network structure, as discussed in the following section.

The preceding observations have been pessimistic in the sense that they have stressed the problems encountered in attempting to use distributed RC networks. There are some compensating advantages to be gained from using

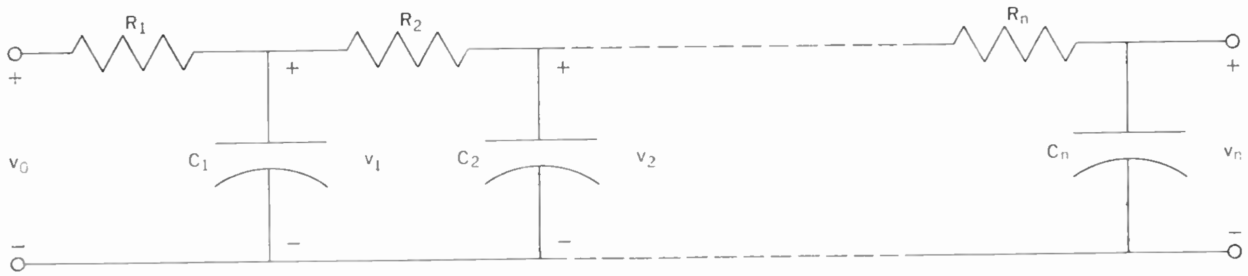


FIGURE 5. Cascade of lumped RC sections.

these elements, however. For example, consider the open-circuit voltage transfer function, under conditions of sinusoidal steady-state excitation, for the simple distributed *RC* network shown in Fig. 2. For large values of frequency, the expression for the magnitude of the transfer function becomes[11]

$$\lim_{\omega \to \infty} \left| \frac{V_2}{V_1}(j\omega) \right| = 2 \exp\left(- \sqrt{\frac{\omega RC}{2}} \right) \tag{7}$$

Hence, this network provides an attenuation that is an exponentially increasing function of frequency, a greater attenuation than is available with lumped *RC* networks. Similarly, for large values of frequency, the phase of the transfer function becomes[11]

$$\lim_{\omega \to \infty} \operatorname{Arg} \frac{V_2}{V_1}(j\omega) = \sqrt{\frac{\omega RC}{2}} \tag{8}$$

Thus, phase increases indefinitely; that is, a single distributed *RC* network can provide as much phase as desired, a property not found in lumped *RC* networks. As a final illustration of the utility of distributed *RC* networks, note that only two components are needed in the notch-producing network of Fig. 4. Thus, in this application, a single distributed element and one lumped resistor can be used to replace the six elements required in a comparable lumped realization, such as a twin-*T* network, providing an obvious saving in network complexity.

Analysis of the DLA network

It has been pointed out that it is quite difficult to characterize distributed *RC* networks by any means (such as pole and zero locations) that readily permit the analysis of DLA networks—i.e., networks containing distributed elements as well as lumped and active elements. If it is desired to analyze DLA networks only under conditions of sinusoidal steady-state excitation, many of these difficulties can be surmounted. The process generally requires the use of digital computation facilities.

There are several techniques that may be used to model the characteristics of a distributed *RC* network under conditions of sinusoidal steady-state excitation. For example, if we visualize the network as consisting of a cascade of lumped *RC* sections (Fig. 5), then in the limit as the number of sections becomes large, it may be shown that the resultant two-port parameters for the overall cascade converge uniformly to the parameters of the actual distributed network.[5,6] The transmission parameters for such a cascade are easily found. For the network of Fig. 5, the transmission parameter matrix for the entire distributed network easily is seen to be

$$\begin{bmatrix} A & B \\ C & D \end{bmatrix} = \prod_{i=1}^{n} \begin{bmatrix} 1 + j\omega R_i C_i & R_i \\ j\omega C_i & 1 \end{bmatrix} \tag{9}$$

Such a procedure is clearly applicable to distributed networks of arbitrary taper. Thus, for a specified value of frequency, we can obtain the complex values of the transmission parameters for a distributed *RC* network.

A representation of the form given in (9) provides a means of producing a sinusoidal steady-state analysis of a DLA network. The procedure may be outlined as follows:

1. Choose a value of frequency.
2. Use Eq. (9) to determine the complex-valued transmission parameters of the distributed networks at the specified value of frequency.
3. Convert the transmission parameters for the distributed networks to *y* parameters.
4. Determine the complex-valued nodal admittance matrix for the lumped parameters of the DLA network at the specified value of frequency.
5. Add the *y* parameters of the distributed network to the appropriate elements of the nodal admittance matrix.
6. Reduce the order of the admittance matrix to include the constraining effects of any sources (voltage-controlled voltage sources).
7. Solve the nodal admittance matrix to determine the desired transfer or driving-point function.
8. Repeat steps 2 through 7 for all other frequencies.

Although this procedure is straightforward, the sheer detail of the calculations suggests the use of a digital computer program to produce readily usable results. Such a program has been described in the literature.[12]

The model for a distributed network (Fig. 5) may also be used for time-domain studies of DLA networks. For such an application, state-variable techniques may be used to describe the distributed network model. In general, the state-variable formulation requires that all the equations describing the network be put in the form[13]

$$\mathbf{x}' = \mathbf{A}\mathbf{x} + \mathbf{B}\mathbf{u} \tag{10}$$

where $\mathbf{x}$ is the column matrix of state variables (which, in most cases, consist of the voltages across capacitors and the currents through inductors), $\mathbf{x}'$ is the column matrix of time derivatives of the state variables, $\mathbf{u}$ is a column matrix giving the excitations to the network, and $\mathbf{A}$ and $\mathbf{B}$ are the matrices characterizing the network. Such a formulation is easily made for networks that include distributed network models of the form given in Fig. 5, since the relations of the state-variable formulation of Eq. (10) for all interior nodes of the model will have

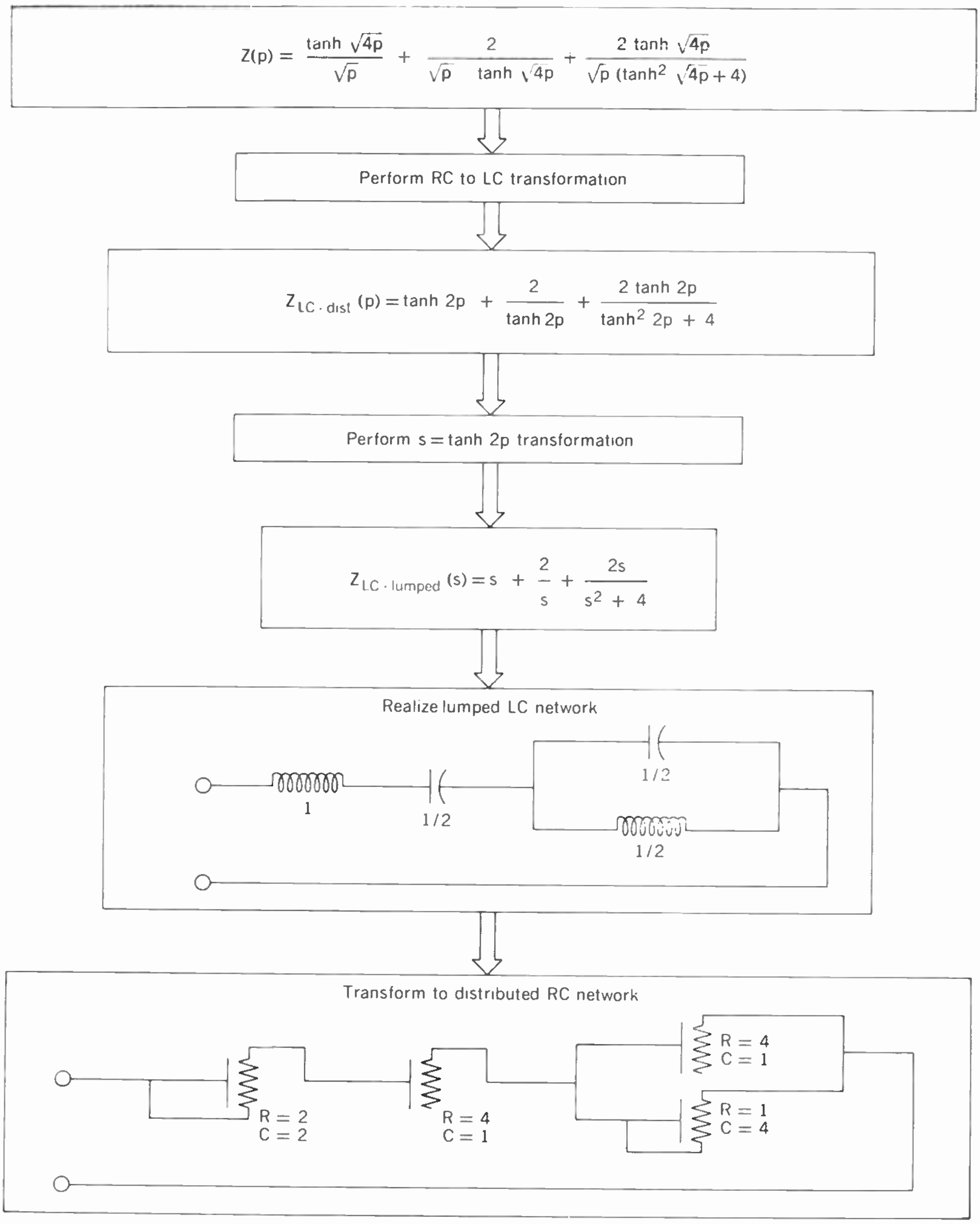


FIGURE 6. Synthesis of a distributed RC network.

the form

$$C_i v_i' = \frac{v_{i-1} - v_i}{R_i} + \frac{v_{i+1} - v_i}{R_{i+1}} \qquad i = 1, 2, \cdots, n-1 \quad (11)$$

Thus, these relations are easily generated by a mathematical algorithm and combined with the relations defining the remaining state variables of the overall DLA network. The resulting set of first-order differential equations may be solved by any of the usual matrix differential-equation solving techniques, such as a Runge-Kutta method, to find the output time response of the DLA network for an arbitrary input excitation. Thus we see that digital computation techniques may be used to provide a characterization of time-domain response as well as frequency-domain (sinusoidal steady-state) response for the DLA network.

Synthesis of DLA networks

It has been described how digital computation techniques may be applied to the general problem of analyzing DLA networks in the frequency domain (sinusoidal steady-state response) and in the time domain. Frequently, however, a topic of more interest to the potential user of DLA networks is the *synthesis* problem—that is, the question of how to specify and interconnect various distributed, lumped, and active elements in order to realize some specified network characteristic.

First of all, let us consider networks consisting only of distributed *RC* elements. The problem of realization of specified network functions using only distributed network elements has been considered by several authors. In general, their results are characterized by the same problems discussed in the analysis of distributed elements—namely, the difficulty of working with the transcendental relations describing the elements. Since, in general, synthesis techniques are more involved than analysis techniques, these difficulties become even greater in the synthesis problem. As an example of the difficulties, assume that a desired driving-point function can be expressed in the form

$$Z(p) = \frac{k_\infty}{\sqrt{p}} \tanh\sqrt{pRC} + \frac{k_0}{\sqrt{p}\tanh\sqrt{pRC}} + \sum_i \frac{k_i \tanh\sqrt{pRC}}{\sqrt{p}(\tanh^2\sqrt{pRC} + \Omega_i^{\,2})} \qquad (12)$$

To synthesize such a function, we may proceed by first transforming the function using an *LC–RC* transformation,[14] thus changing the problem from the synthesis of a distributed *RC* network to that of a distributed *LC* one. The second step consists of employing a transformation of the complex frequency variable of the form $s = \tanh 2p$, where p is the original complex frequency variable and s is the new complex frequency variable.[15] Such a transformation effectively changes the problem from the synthesis of a distributed *LC* network to that of a lumped *LC* one. The driving-point function in s may then be realized by any of the well-known Foster or Cauer forms. Finally, any inductors in the lumped *LC* realization can be replaced by short-circuited distributed *RC* elements, and any capacitors by nonshorted ones. The process is illustrated schematically in Fig. 6.[16] Although such a procedure theoretically is quite acceptable, there are few network designers who think in terms of network functions of the form given in Eq. (12). Thus, in practice, such techniques are of limited usefulness. The same observation generally applies to many of the other synthesis techniques that have appeared to date.

A second approach to the synthesis of distributed networks entails the modification of the shape and taper of the distributed element in such a way as to meet some desired network characteristic. As an example of such an approach, it has been shown that the driving-point and transfer characteristics of a distributed *RC* network can be made to approach those of rational network functions for specially modified distributed elements.[17]

An entirely different philosophy of synthesis consists of the employment of optimization strategies used in conjunction with analysis techniques of the type described in the preceding section. Such strategies may be applied to determine the configuration and the element types and values of a DLA network so as to realize some specified network performance criteria. This approach has the merit that it may take advantage of the considerable wealth of information concerning presently developed optimization techniques, which may range from such a basic approach as a steepest-descent algorithm to the fairly sophisticated calculus of variation techniques.[18]

An interesting extension of the usual optimization approach is the use of optimization techniques to maximize the magnitude of the network function describing a DLA network at some point on the complex frequency plane. This procedure has the effect of producing a pole of the network function at the specified point, thus it may be used as a synthesis method.[19] A general disadvantage of all optimization methods is that their implementation requires considerable experience on the part of the user. In addition, such methods predicate the availability of a sophisticated DLA analysis program, and hence are of little use to the network designer who has an immediate application for DLA networks.

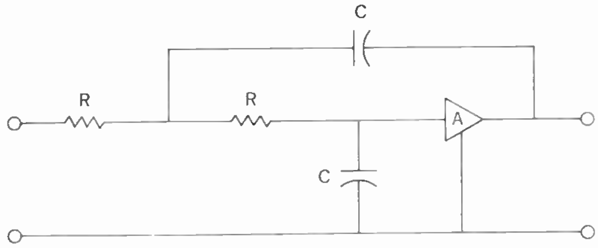
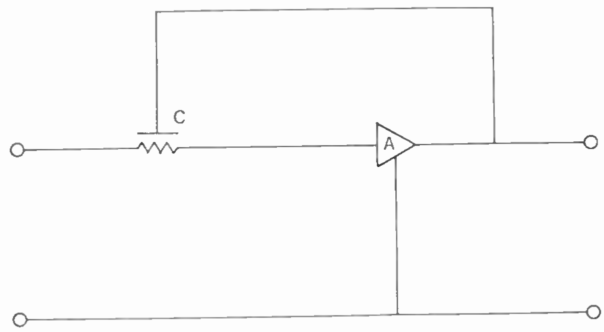


FIGURE 7. Example of a low-pass network.

FIGURE 8. Network of Fig. 7 with the lumped elements replaced by a distributed element.



A practical approach to the use of DLA networks

We have pointed out some of the difficulties attendant to implementing the synthesis problem for DLA networks. Although such difficulties provide formidable obstacles to the general application of DLA networks, considerable alleviation of these difficulties is being produced by the appearance of relatively easy-to-use design information covering many practical filtering situations. In this section, we shall discuss one such set of design information.[20,21]

In our example of the application of design information to the realization of a DLA network, we shall assume that it is desired to realize a low-pass open-circuit voltage transfer function. Let us first consider an active *RC* circuit (with lumped elements only), which will provide such a realization. A well-known configuration for a low-pass network function is shown in Fig. 7. The triangle shown in this figure represents an amplifier (voltage-controlled voltage source) of relatively low gain. Design information for this circuit has been tabulated, giving the values of the four passive elements and the value of the amplifier gain in terms of the desired positions of the two complex conjugate poles produced by the network.[22] Since the realization is low-pass, all the transmission zeros are located at infinity. An examination of the configuration discloses that a purely resistive path exists from the network input terminal to the input of the amplifier. Similarly, a capacitive path exists from the network output terminal to the junction of the two resistors. A consideration of such connections might lead us to attempt to replace the lumped elements of Fig. 7 by a distributed element as in Fig. 8 (because the distributed network has an infinite number of poles, the lumped capacitor shown

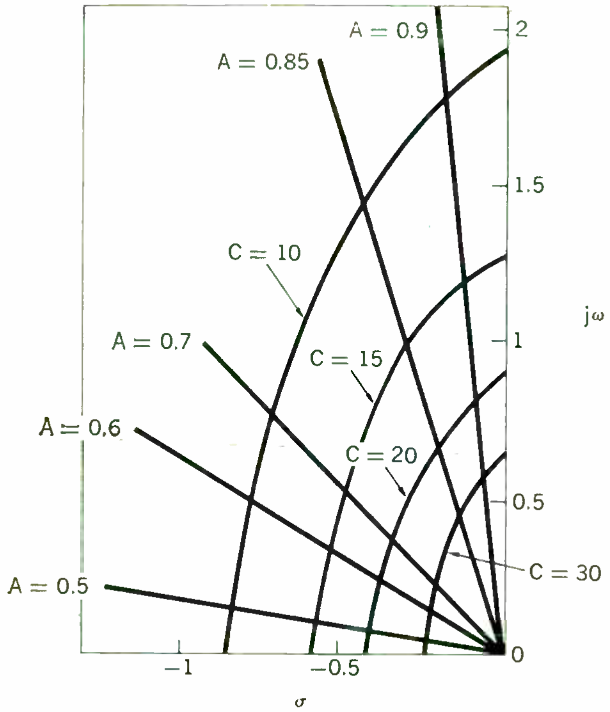


FIGURE 9. Computerized analytical results showing the upper left half of the complex frequency plane.

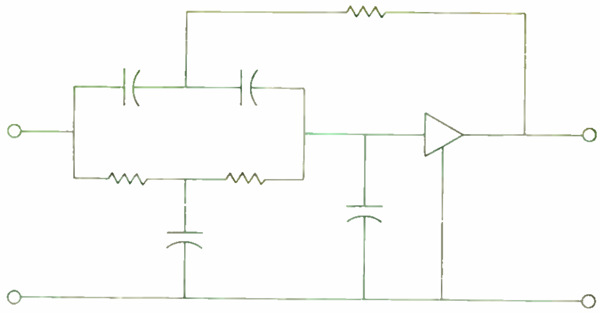
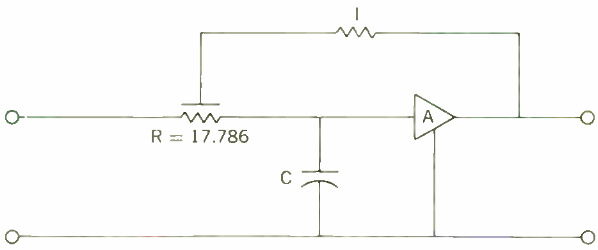


FIGURE 10. Active RC realization of a voltage transfer function.

FIGURE 11. Network of Fig. 10 utilizing a new passive configuration.



across the input of the amplifier in Fig. 7 is no longer required).

An analysis of the sinusoidal steady-state behavior of this circuit indicates that it produces a low-pass network function. If a digital computer analysis is made of the properties of this network for a wide range of element values and frequencies, the network characteristics may be directly compared with those of a two-pole low-pass voltage transfer function, and the results used to define a single complex pair of equivalent dominant poles for the DLA network. The results of such an analysis are plotted in Fig. 9. This illustration shows the upper left half of the complex frequency plane. The radial lines indicate values of amplifier gain A, and the curved lines indicate the total value of capacitance C in the uniform distributed network. For convenience, the resistance of the distributed element has been normalized to unity. Obviously, Fig. 9 may be used as a design chart to synthesize low-pass networks in which a single distributed element replaces the four lumped elements normally required for a similar realization. In addition to requiring fewer elements, it should be noted from Fig. 9 that the amplifier gain of the DLA realization is always less than unity. This is of considerable advantage when transistors are used to realize the amplifier. Finally, note that this circuit may easily be used as an oscillator. With the input grounded, the gain required in this case is approximately 0.925.

The same technique may be applied to other common network functions. For example, consider a voltage transfer function with a pair of conjugate zeros on the $j\omega$ axis, and a pair of complex conjugate poles. Such a function will have the form

$$\frac{V_2}{V_1} = \frac{K(p^2 + b_1)}{p^2 + a_1 p + a_2}$$

An active RC realization for such a network has the form shown in Fig. 10.[23] (This realization assumes that the poles are closer to the origin than the zeros.) The theory of the design of such a network is based on the fact that the passive network must provide a transmission zero at $p = j\sqrt{b_1}$ from the input of the overall network to the amplifier input (when the output is short-circuited). Such a transmission zero results from the twin-T configuration of the passive network elements. In the section on properties of distributed RC networks, however, it was pointed out that a distributed RC network with a lumped series resistor (as shown in Fig. 4) also provides such a transmission zero. Comparing other characteristics of the passive network configuration of Fig. 10, we are prompted to explore the properties of a new passive network configuration consisting of a distributed RC network, a single lumped resistor, and a capacitor (Fig. 11). If we assume that an impedance normalization is made for the network such that the series lumped resistor has unity value, then the total relative resistance of the distributed element required to produce a notch [and to satisfy the relation given in Eq. (5)] is 17.786. If we choose the total capacitance of the distributed element so as to locate the transmission zeros at one radian per second (thus providing a frequency normalization), then only C, the value of the lumped capacitor, and A, the gain of the amplifier, remain free to be chosen. Using the same digital computer techniques as were applied to the low-pass case, we find that a design chart similar to the one shown in Fig. 9 may be constructed.[20, 21] This type of chart specifies the equivalent poles of the DLA network as a function of the parameters C and A. Thus, we have a design procedure for another very useful class of networks. The technique is obviously extendable to other network situations, and there are many variations that may be applied to the basic approach. As an example, the use of phantom zeros has been shown to be effective in minimizing the sensitivity of the network to changes in the gain of the active element.[24]

Conclusion

In some ways, a parallel relationship exists between the present status of the DLA network (i.e., the network containing distributed, lumped, and active elements) and the status of the passive *RLC* network a few decades ago. At that time, many synthesis techniques for passive *RLC* networks had been discovered and reported in the literature but, owing to their complexity, such methods were limited to use by a relatively small portion of the total potential users. Considerable alleviation of this problem occurred as design tables based on various synthesis techniques were developed and published. With such tables, the filter designer who had a relatively straightforward filtering problem could easily find a realization. For less-common filtering problems, even today, the user finds that tables are not available to satisfy his requirements. Therefore, he must learn and apply the basic methods in order to be able to solve the problem.

For the DLA network, a similar situation currently exists. The complexities of analysis and synthesis of such realizations provide a very real barrier to their effective and frequent usage. Already, however, some practical and useful design information has appeared. More will undoubtedly follow. Hence, the network designer shortly will find available an even broader range of techniques that may be used effectively to realize many network characteristics.

The general area encompassed by DLA networks is just beginning to be explored. Many of the topics introduced in this article will merit increased study. This is especially true of the use of optimization techniques to determine topological structures and element values for DLA networks. Another interesting research area is the design of multiterminal distributed elements in which the terminal configurations themselves are shaped and located so as to produce desired network characteristics. Further, it appears that, owing to the intractability of the mathematical relations describing distributed networks, the digital computer will play an ever-larger role in the treatment of the DLA network.

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The new responsibility of the engineer

In the "us" world in which we now live, the engineer too has a new responsibility—to contribute his technical knowledge and his skill in teamwork to the wider field of deciding public policies

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E. Howard Holt *U.S. Army Electronics Command*

Engineers and scientists are responsible for the technical progress that has been made and they have expert knowledge of what can be done next. For this reason alone, they must take part in deciding public policy. However, the modern technical man has a further contribution to offer—the ability to understand the views of others, and to incorporate these views into the best possible group decisions. This article points out that this is an ethical concept that can be spelled out in an extension of the ECPD Canons of Ethics and puts forth for consideration three proposals for strengthening the Canons.

Arnold Toynbee wrote recently: "Our greatest challenge today is the morality gap between our cumulative accelerating advance in science and technology and our appalling failure in our relations with each other."[1] At their 1968 annual meeting, ASME's President-elect Marlowe said: "The engineering profession must bear (or share) the responsibility for the shortcomings as well as the accomplishments of today's world."[2] And at the IEEE Convention in March, several speakers urged engineers to contribute to the making of decisions affecting the public, pointing out that to withhold our views may result in uninformed, and so unsound, public policies.

Before making a decision, an engineer or a scientist collects all the data and weighs them carefully. In contrast, nontechnical people, and the public in general, tend to bias their decisions emotionally and to personalize issues in terms of heroes or foreign devils. Businessmen generally make decisions on factual grounds, but both engineers and businessmen are strongly influenced by economic factors—the pressures of competition and the availability of resources.

To reach sound public-policy decisions, it is essential that the values of conservation, safety, quiet, and beauty, of aiding the poor, and of helping other countries, be weighed along with the economic data. We must look to the well-being of people and to future needs rather than merely to financial gain. As President Hollomon of the University of Oklahoma remarked at the Highlight Session at the March IEEE meeting,[3] we now live in an "us" world, and the ethics of the "I" world that we lived by before are no longer adequate.

Engineers and scientists are responsible for the technical progress that has been made and they have expert knowledge of what can be done next. For this reason alone, they must take part in deciding public policies. And in addition to their technical knowledge, engineers can make another vital contribution to policy making.

What engineers can do

Many years ago Herbert Spencer wrote that the basis for ethics is the ability to sympathize—to understand and share the feelings of others—that has developed over thousands of years because of its survival value. Civilization depends on the division of labor, with each man working in his own field of competence and relying on others to provide the things he does not make himself. A complete trust that each member of the organization will do his share of work for the common good is an essential factor in the success of modern corporations and of governments. Their immense, widespread, and long-term projects could not be completed without honesty, fair dealing, and good ethics.

Engineers and scientists, as members of the technostructure[4] that carries forward large-scale projects, understand the necessity for cooperation and have become skilled in its practice. Each person in the group has expert knowledge in some fields but is inadequately informed in others. He thus has learned how to be a loyal member of a team, and as such he and his teammates know the art of listening, weighing, and compromising before a group decision is reached. The concept of a Henry Ford or an Andrew Carnegie making all the decisions for a company is out of date. It is the existence of the technostructure of many experts, interacting through committees and other exchanges, that enables the modern corporation to succeed and grow.

The ability to sympathize with and appreciate the views of others, and to incorporate these views into the best possible group decisions, is the further contribution that the modern technical man has to offer. The obligation to contribute both his technical knowledge and his skill in teamwork to the wider field of the decision-making councils of the nation is the new responsibility that has been laid upon the engineer.

This is an ethical concept that may well be spelled out as an extension of the Canons of Ethics promulgated by the Engineers' Council for Professional Development.

A stronger code of ethics

Traditionally, professional men have recognized the importance of ethical values by the adoption of codes of ethics. The ECPD Canons therefore have a long and honorable tradition, and it is appropriate that they should now be reviewed and strengthened.[5] When they were last revised, in 1963, changes were made to recognize the problems of the employed engineer, but the Canons continue to be solely guides to individual conduct, primarily in private practice. In their present form they do not fully meet the needs of employed engineers, nor do they prescribe any rules for the conduct of engineering societies. It is the purpose of this article to put forward for consideration three proposals for strengthening the Canons.

First, we propose that a fourth fundamental principle be added to the present three (see Appendix), namely:

IV. (The Engineer) Will join, and earnestly support, a professional society in the field of his competence.

The technical societies devote their efforts to education, technical standards, and the advancement of the art, and take no part in seeking personal benefits for their members—in strong contrast to the practice of unions. Thus, supporting a technical society is rendering public service, with one's only reward the distinction gained by contributing a technical paper or serving on a committee. The ethical basis for this proposal is that the engineer is obligated to serve society, and that he can give more service as a member of a team of specialists than he can by working alone. After an engineer has earned the respect of his fellows for his technical competence, he should also join a professional society such as the National Society of Professional Engineers, and in this wider role he may promote the economic aims of engineers. The difference in concept between the technical societies and the NSPE is shown by the tax exemption granted to the former and denied to the latter.

The purpose of adding this principle is to ensure that there will be an active society for every major branch of engineering, one that is devoted to the progress of the art and is ready to help in the solution of important technical problems.

Our second proposal is that each engineering society accept responsibility for discovering, inspiring, bringing forward, and supporting those of its members who can contribute most effectively to the formation of sound public policies in the technical fields of their competence. This proposal would extend the ECPD Canons to provide guides for the conduct of engineering societies.

How to bring qualified engineers forward, and enable them to contribute to public decision making, is a problem to be solved. One way to accomplish this is for such engineers to be elected to membership in the National Academy of Engineering—and for the Academy to be recognized as the spokesman for the entire engineering profession. The Academy chooses its own members, based on the recommendations of a nominating committee. It seems desirable for the technical societies to recommend their most qualified members for NAE membership, and it is likely that the Academy nominating committee would welcome such additional nominations, which would bring with them the assurance of proved ability and broad support.

To carry out this second proposal, it is suggested that each major engineering society appoint a Public Affairs Council, with two duties: (1) to survey technical problems of national importance and designate those problems with which members of the society might be helpful; (2) to select from the society's membership a few of the best-qualified leaders and urge them to study the chosen problems and to air their views publicly. In speaking out, the members would do so as individuals. On no account should a technical society consider or vote upon social, political, or other controversial topics. Its efforts in these wider fields should be confined to inspiring individual members to consider the problems, as a means of rendering public service. However, through its committees the society may gather data or have research done so that the service can be more effective.

Those individual engineers who make real contributions in this way may then be recommended for membership in the NAE. With this assurance of support by the profession, and of attracting as members well-qualified individuals who are willing to serve, the Academy surely will receive additional backing from foundations and the government for the sponsorship of long-range and extensive studies of such technical problems as pollution control, transportation, water supply, and atomic power, as has already been done in the field of automobile traffic safety. If these matters are reported and hearings held on them, the public can be kept better informed and the conclusions reached will be more generally accepted than is true now.

Proposals along these lines—for engineers to take part in civic affairs on a local level—have been spelled out previously.[6] It is appropriate for each local chapter of an engineering society to encourage its members to serve on school boards, planning commissions, etc., and even to run for public office. In this way younger members can gain public respect and political wisdom, thus enabling them to rise to prominence in national affairs.

Our third proposal is further to revise the Canons dealing with the problems of employed engineers. The hard facts of business survival require that business decisions be based on relative costs and on the conditions prevailing in a particular field. Business customs vary widely in different lines and in different countries. Thus, although the decisions of management may not be in accord with the ideals of an employed engineer, he must recognize the ethical requirements of loyalty to his employer and as a member of a team, as well as the precepts of the Canons. How to solve the problems that arise in this situation should be considered in the revision of the ECPD Canons. We do not spell out here how this may be done but ideas to this end are being formulated.

One important way for the employed engineer to promote good business ethics is to develop and apply quality-control and evaluation procedures that will secure the desired quality level, and measure the risk of failure, of his products. The engineer, and his employer as well, should be bound by ethical principles to make sure that publicity claims can be proved by test. Both the customers and the general public should be informed of the consumer's risk that is associated with every product that is subject to variations in manufacture and use. In this way the customer is made to realize that when he accepts a lower-priced product he can normally expect a shorter life and a greater risk of failure.

Through the engineering societies much has been done to develop life and acceptance test procedures. Every engineer should try to develop or improve such procedures for his own products so that high quality can be more clearly demonstrated and the products' value recognized.

Conclusion

It is hoped that a session at the 1970 IEEE International Convention can be devoted to consideration of the foregoing and related proposals. The Institute has a unique opportunity to play a leading role in the development and acceptance of a strengthened ECPD Canons of Ethics, a set of ethics that will reflect the needs of IEEE members and of the engineering profession as a whole.

Appendix. Canons of Ethics for Engineers

Fundamental Principles of Professional Engineering Ethics

The Engineer, to uphold and advance the honor and dignity of the engineering profession and in keeping with high standards of ethical conduct:

I. Will be honest and impartial, and will serve with devotion his employer, his clients, and the public;
II. Will strive to increase the competence and prestige of the engineering profession;
III. Will use his knowledge and skill for the advancement of human welfare.

Relations With the Public

1.1 The Engineer will have proper regard for the safety, health and welfare of the public in the performance of his professional duties.
1.2 He will endeavor to extend public knowledge and appreciation of engineering and its achievements, and will oppose any untrue, unsupported, or exaggerated statements regarding engineering.
1.3 He will be dignified and modest in explaining his work and merit, will ever uphold the honor and dignity of his profession, and will refrain from self-laudatory advertising.
1.4 He will express an opinion on an engineering subject only when it is founded on adequate knowledge and honest conviction.
1.5 He will preface any ex parte statements, criticisms, or arguments that he may issue by clearly indicating on whose behalf they are made.

Relations With Employers and Clients

2.1 The Engineer will act in professional matters as a faithful agent or trustee for each employer or client.
2.2 He will act fairly and justly toward vendors and contractors, and will not accept from vendors or contractors, any commissions or allowances, directly or indirectly.
2.3 He will inform his employer or client if he is financially interested in any vendor or contractor, or in any invention, machine, or apparatus, which is involved in a project or work of his employer or client. He will not allow such interest to affect his decisions regarding engineering services which he may be called upon to perform.
2.4 He will indicate to his employer or client the adverse consequences to be expected if his engineering judgment is over-ruled.
2.5 He will undertake only those engineering assignments for which he is qualified. He will engage or advise his employer or client to engage specialists and will cooperate with them whenever his employer's or client's interests are served best by such an arrangement.
2.6 He will not disclose information concerning the business affairs or technical processes of any present or former employer or client without his consent.
2.7 He will not accept compensation—financial or otherwise—from more than one party for the same service, or for other services pertaining to the same work, without the consent of all interested parties.
2.8 The employed engineer will engage in supplementary employment or consulting practice only with the consent of his employer.

Relations With Engineers

3.1 The Engineer will take care that credit for engineering work is given to those to whom credit is properly due.
3.2 He will provide a prospective engineering employee with complete information on working conditions and his proposed status of employment, and after employment will keep him informed of any changes in them.
3.3 He will uphold the principle of appropriate and adequate compensation for those engaged in engineering work, including those in subordinate capacities.
3.4 He will endeavor to provide opportunity for the professional development and advancement of engineers in his employ or under his supervision.
3.5 He will not injure maliciously the professional reputation, prospects, or practice of another engineer. However, if he has proof that another engineer has been unethical, illegal, or unfair in his practice, he should so advise the proper authority.
3.6 He will not compete unfairly with another engineer.
3.7 He will not invite or submit price proposals for professional services which require creative intellectual effort, on a basis that constitutes competition on price alone. Due regard should be given to all professional aspects of the engagement.
3.8 He will cooperate in advancing the engineering profession by interchanging information and experience with other engineers and students, and by contributing to public communication media, to the efforts of engineering and scientific societies and schools.

(Approved by Engineers' Council for Professional Development, September 30, 1963)

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The early history of electronics

IV. First radiotelegraphy experiments

The source of the mainstream of the development of radiotelegraphy is the work of Marconi, but many other experimenters took a hand very early, notably Russia's Popov; the exact priority later became the subject of much controversy

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In our last installment, we saw that among the many experimenters who drew inspiration from the Hertz memorial lecture given by Oliver Lodge in 1894 was Augusto Righi, then professor of physics at the University of Bologna.[1] He included some of the material in lectures that were audited by Guglielmo Marconi, who was fascinated by Hertz's experiments and determined to find out whether a telegraphic system could be based on what had been accomplished. He could hardly believe that no one had grasped the commercial possibilities of Hertzian waves, especially since the principal components of such a system—the transmitter, the antenna, the resonator, the coherer, and the automatic decoherer—had already been invented and were presumably available to engineers much better qualified than a privately educated Italian youth barely out of his teens. When he first thought of using the waves for radiotelegraphy in 1894 on his father's estate at the Villa Griffone near Pontecchio, he wrote later, he could "scarcely conceive it possible that their application to useful purposes could have escaped the notice of eminent scientists."[2]

He began experimenting in the spring of 1895 with an ordinary spark induction coil and homemade coherers of the Branly type. The spark gap was a four-ball arrangement similar to one first proposed by Lodge and afterwards improved by Righi, who had found that tarnishing of the balls owing to continuous sparks could be reduced if they were surrounded by a parchment bag containing a mixture of Vaseline and Vaseline oil. To turn the discharge on and off Marconi inserted a telegraph key in the primary circuit of the induction coil and was thus able to cause short or long trains of sparks to jump across the gap according to whether he held the key down for a short or a long time. He could soon detect these dots and dashes all the way across the room. By the summer of 1895 he had moved his equipment to the garden and had begun experimenting outdoors. In searching for methods of increasing the capacitance in the transmitter, he quickly hit upon the phenomenon that Dolbear, E. Thomson, and others had noticed before him[3]: Connecting one terminal of his transmitter to an elevated metallic object and the other to a grounded metal plate not only increased the capacitance, but also the distance over which the radiation could be detected.

This "grounded antenna," the elevated portion of which consisted of a tin can atop a pole, enabled Marconi to signal across the entire length of the garden, and he found that there was a direct relationship between the height of the pole and the distance attainable. As a receiver, he used a similar arrangement with a Branly coherer inserted in the circuit.

Marconi made important technical improvements in all the components of the system, notably in the design of the coherer. He used a much shorter tube, reducing the effective length to about 2 mm, and determined that the best metal for the filings was a mixture of 96 percent nickel and 4 percent silver particles, sifted to a uniform size. Air was exhausted from the tube and it was sealed off. In series with the coherer Marconi placed a relay that fulfilled two functions: it actuated a tapper (which was simply an electric bell with the coherer itself taking the place of the bell gong) and worked a telegraphic printing instrument to record the received signals. Chokes were inserted in the relay and tapper circuits to prevent the sparks produced by these components from reacting on the coherer. The entire apparatus, together with the batteries needed to work the relay and the tapper, was mounted on a board and placed in a metal box to screen it from stray sparks or oscillations. In some experiments, Marconi placed the spark gap of the transmitter along the focal line of a cylindrical parabolic metal reflector, much as Hertz had done.[4] But even without the reflector he managed to receive signals at a distance of 2.4 km, or about a mile and a half.

At that point, Marconi packed up his apparatus and traveled to Britain, arriving there in mid-February 1896. He had chosen to exploit his invention there because Britain was a maritime nation whose dependence on overseas communications seemed to offer the greatest scope for developing the commercial possibilities of his system. Another important reason for his decision to seek his fortune abroad was the fact that his mother, who was of British ancestry, had devoted herself to securing financial and other support for him. As a result of her efforts, a meeting was arranged with W. H. Preece, the engineer-in-chief of the government Telegraph Service,

(Marchese) Guglielmo Marconi (1874–1937) was born in Bologna of an Italian father and an Irish mother and was educated privately. His technical contributions to radiotelegraphy were matched by the scarcely less important contribution represented by the energetic manner in which he promoted his system and made it the basis of a vigorous commercial establishment, which stressed marine applications of radio communications from the first. He continued to occupy himself with the technical aspects of radio; toward the end of his life he interested himself particularly in microwaves and in diathermy. He received the Nobel Prize for Physics in 1909 (jointly with K. F. Braun, the inventor of the cathode-ray oscillograph and of a rival system of radiotelegraphy, to be described in our next installment), as well as many decorations and honorary degrees. He received the Italian knighthood in 1897 and was created a marquis in 1929. (See Dunlap, O. E., Jr., "Marconi: The Man and His Wireless." New York: Macmillan, 1937. Also, Marconi, Degna Paresce, "My Father, Marconi." New York: McGraw-Hill, 1962.)

who had long interested himself in "wireless telegraphy" (i.e., induction telegraphy). On June 2, 1896, Marconi applied for his first patent,[5] and in the same year he demonstrated his apparatus to Post Office officials over a distance of 100 meters. Soon after that he established communication on Salisbury Plain over a distance of several miles in the presence of military and other government representatives, including Capt. H. B. Jackson of the Royal Navy, who had been experimenting with "wireless" on his own.[1] In May 1897, Marconi repeated this experiment across the Bristol Channel and Preece was convinced. He decided to set aside his own system of induction telegraphy and to place the powerful support of the British Post Office at the young inventor's disposal. Radiotelegraphy had acquired a powerful patron.

In June 1897, Preece lectured at the Royal Institution on "Signalling Through Space Without Wires."[6] It is to his everlasting credit that even though he had himself experimented with alternate schemes, Preece was quick to admit the superiority of Marconi's method. "In July last," Preece said, "Mr. Marconi brought to England a new plan ... Mr. Marconi utilises electric or Hertzian waves of very high frequency ... He has invented a new relay which, for sensitiveness and delicacy, exceeds all known electrical apparatus." Anticipating that Marconi might be criticized for seeming to claim novelty where there was none, Preece added: "He has not discovered any new rays; his transmitter is comparatively old; his receiver is based on Branly's coherer. Columbus did not invent the egg, but he showed how to make it stand on its end, and Marconi had produced from known means a new electric eye more delicate than any known instrument, and a new system of telegraphy that will reach places hitherto inaccessible."[6]

Engineering comment was, on the whole, favorable. One journal pointed out the practical uses of research in a self-righteous editorial: "We wish Mr. Marconi, his apparatus and his experiments, all possible success, if only because the evolution from Maxwellian equations and Hertzian vibrations of a thoroughly practical system of telegraphy will prove an excellent object-lesson in the value (in the City sense of the term) of pure research."[7]

On hearing Preece mention a "new plan," Lodge waxed sarcastic: "One of the students in Prof. A. Righi's class at Bologna, having heard that Professor lecture on the production and transmission of Hertz waves across space, and their detection by the cohesion which they caused in a group of metallic filings, and, being gifted, doubtless, with a sense of humour as well as with considerable energy and some spare time, proceeded to put a coherer into a sealed box and to bring it to England as a new and secret plan adapted to electric signalling at a distance without wires. Being influentially introduced to the chief engineer of the Government Telegraphs, who, presumably, was too busy to remember what had recently been done in the Hertz-wave direction, the box was announced as containing 'a new plan' which had been 'brought to England.' "[8] Lodge was particularly irked at the attention that the foreign upstart was receiving: "Every daily paper now has bulletins concerning the progress of the practical application of the invention, except in so far as it is still being privately worked at by uninfluential individuals." (Both biting passages were included when the article containing them became part of the second edition of Lodge's booklet, *The Work of Hertz and His Successors*, but were omitted from subsequent editions.[8])

But in Germany, Prof. Slaby, who had witnessed Marconi's experiments across the Bristol Channel, commented afterwards: "The generation of Hertzian waves, their propagation through space, the sensitivity of the electric eye—all that is said to have been known. Very true, but with these known means one got just as far as 50 meters and no farther."[10]

Perhaps the final word on the relative importance of Marconi's contribution was given in the opinion of a U.S. judge, W. K. Townsend, in an action that upheld the validity of Marconi's original patent: "It would seem to be a sufficient answer to the attempts to belittle Marconi's great invention that, with the whole scientific world awakened by the disclosures of Hertz in 1887 to the new and undeveloped possibilities of electric waves, nine years elapsed without a single practical or commercially successful result... Marconi was the first to describe and the first to achieve the transmission of definite intelligible signals by means of the Hertzian waves."[11]

Marconi's fears that someone would forestall him were well justified. Laboratory experiments with Hertzian waves were being conducted in several countries and sooner or later someone else would have been sure to see the commercial possibilities of radio waves. The much-publicized lectures of Lodge gave fresh impetus to the efforts of his colleagues in several countries. In a description of some outdoors experiments he had made, Lodge had mentioned in his lectures that he could detect "signals" at 40 yards, adding that in his estimate "something more like half-a-mile was nearer the limit of sensitiveness for this particular apparatus as then arranged. However, this is a rash statement not at present verified." Three years later, Slaby took him to task for even mentioning numbers; Lodge, wrote Slaby in his textbook, "designates half an English mile (800 m) as the maximum achievable distance without, however, having tried it in practice."[10] Other critics gleefully pointed out that Lodge's prediction was manifestly wrong in view of Marconi's successes, which brought a sharp rejoinder from Lodge in the form of a footnote when the book containing his lecture came out in its third edition: "This statement has been absurdly misunderstood, as if it was a prediction of what would always be the limit of sensitiveness for any apparatus and any sized sender. Nothing of the kind was in my mind. Such predictions are always preposterous, and I am not obliged to those who imagined that I had been guilty of one of them."[9]

Such later disclaimers apart, the statement originally published seems to have challenged others to see whether they could do better. In addition to Jackson and Righi, Slaby had experimented with Hertzian resonators and parabolic mirrors at the Technische Hochschule in Charlottenburg near Berlin, but the best he had been able to do was to perceive the oscillations across the length of a corridor. When the first reports of Marconi's demonstrations began to appear in newspapers, Slaby quickly saw that Marconi must have introduced some new factors to be able to telegraph across several kilometers. Slaby packed his bag, traveled to England to witness the demonstrations, and was duly impressed. Marconi had indeed done something new: Using known components, he had created a system, one that comprised an elevated antenna, a ground connection, an improved coherer, and a well-engineered spark-gap circuit. Slaby returned to Germany, encouraged by what he had seen to continue his own experiments, which were ultimately to lead to the development of a rival system of radiotelegraphy and the foundation of the German electronics industry.

(Sir) William Henry Preece (1834–1913) was born in Wales and educated at Kings College in London. He worked for a time under Faraday at the Royal Institution. He was active as a telegraph engineer from 1853 to 1870, when he joined the Post Office. He became successively electrician-in-chief (1870), engineer-in-chief (1892), and consulting engineer (1899), a post that he held until his retirement in 1904. He became a Fellow of the Royal Society in 1881 and during 1898–1899 he served as president of the Institution of Civil Engineers. He was knighted in 1911.

Another early investigator who was inspired by Lodge's lecture was Popov, an instructor at the Naval School at Kronstadt. He experimented with Branly coherers, set up a receiver with a protruding wire in 1895, and read a paper about it, "On the Relation of Metallic Powders and Electric Oscillations," at a meeting of the Russian Physico-Chemical Society on April 25, 1895.[12]

Popov had seen an account of Lodge's lecture and set up a circuit very similar to that of Lodge, with a coherer tube placed above the hammer of an electric bell. By means of this apparatus, Popov could register electric disturbances, including atmospheric ones, and in July 1895, a similar instrument with an ink recorder was in fact installed at the Meteorological Observatory of the Institute of Forestry in St. Petersburg. A one-paragraph summary appeared in the minutes of the meeting, and a more complete account was published in January 1896.[13]

Adolf Carl Heinrich Slaby (1849–1913) was born in Berlin and studied at Jena, where he received the doctorate in 1873. He was professor of electrical engineering at the Technische Hochschule in Charlottenburg near Berlin for 30 years. His lectures on "spark telegraphy" were published as "Die Funkentelegraphie" by Leonhard Simion, Berlin, in 1897, a title that is probably responsible for the persistence of the "spark" in German radio parlance to this day; it was the first textbook on radiotelegraphy published anywhere. (Extracts from the book also appeared in English in "Century Magazine," vol. 33, pp. 867–874, 1898.) Slaby's student Georg Wilhelm Alexander Hans Graf von Arco (1869–1940), who served as his assistant during 1896–1898, afterwards played an important part in the development of the German electronics industry.

There is no doubt that Popov was aware of possible radiotelegraphic applications of his apparatus: His paper ended with the words, "In conclusion I may express the hope that my apparatus, when further perfected, may be used for the transmission of signals over a distance with the help of rapid electric oscillations, as soon as a source of such oscillations possessing sufficient energy will be discovered."[13]

On March 12, 1896, Popov gave a demonstration before the Physico-Chemical Society of which no verbatim record survives; the minutes merely state, "A. S. Popov shows instruments for the lecture demonstration of the experiments of Hertz. A description of their design is already in the Journal."[14] However, the claim was subsequently made that during this demonstration, Popov actually transmitted a message. This is how an eyewitness, Orest Danilovich Khvolson (1852–1934), recalled the occasion: "The transmission took place in such a manner that the letters were transmitted in the Morse alphabet and the signs were moreover clearly audible. The President of the Phys. Society, Prof. F. F. Petrushevsky, stood at the blackboard holding a paper with the key to the Morse alphabet and a piece of chalk. After each sign was transmitted he glanced at the paper and then wrote the corresponding letter on the blackboard. Gradually there appeared on the blackboard the words *Heinrich Hertz*, moreover in Latin characters. It is difficult to describe the joy of the many members of the audience, and the ovation given to A. S. Popov, when these two words were written down. The meeting took place at the beginning of 1896, but I cannot establish the exact date."[15]

This account has met with some skepticism on the part of non-Soviet observers, notably George William Osborne Howe (1875–1960), who wondered why the first account of this transmission did not appear until 30 years after the event.[16] According to Soviet sources, the reason was that Popov's work was under the control of the Naval authorities and Popov did not want any account of the application of his storm detector to radiotelegraphy appearing in print, although there was evidently no objection to demonstrating such an application to a large public gathering. At any rate, beginning with the 50th anniversary of Popov's 1895 lecture, the Soviet authorities formally declared that he was the "inventor of radio," a claim that they now began to press with considerable conviction.

I have considered the question of priority in considerable detail elsewhere[17] with the help of the original sources and have concluded that the claim made on Popov's behalf may be considered on one of two bases: (1) priority of publication (i.e., public disclosure in print), which is by far the more generally accepted *sine qua non* of priority of invention; and (2) historical research. On the basis of printed publication, Popov cannot be said to have "invented radio" since he did not describe in print his use of his equipment for the transmission of intelligence before Marconi's patent application of June 2, 1896. According to this generally accepted criterion, Marconi is the "inventor of radio" (i.e., radiotelegraphy).

On the basis of historical research, there is indirect evidence that Popov demonstrated the transmission of intelligence to others on March 12, 1896. There is comparable evidence that Marconi demonstrated the transmission of intelligence at an even earlier date, though admittedly not to a scientific audience. (It is incontrovertible that he had brought his apparatus to a degree of perfection that justified the considerable expense of a journey to London, where he arrived a month before the "Heinrich Hertz" demonstration.) According to this criterion, Popov's work was *at the most* contemporary with Marconi's and probably later.

All credit should go to Popov for independently evolving the same practical receiver design from Lodge's first suggestion as Marconi did, a similarity that Popov pointed out in a letter written to a British journal.[18] Popov should also receive credit for his later experiments, carried out in the face of substantial obstacles. He managed to keep his laboratory up to date in the light of subsequent radiotelegraphic developments. From a tour of radiotelegraphic installations in Germany and France, Popov could write to his assistant Piotr Nikolaevich Rybkin (1864–1948) with justifiable pride in 1899: "I have seen and learned everything possible and I have spoken with Slaby and seen his apparatus, and visited Blondel at the station in Boulogne. In a word, I have learned everything possible and I see that we have not fallen much behind the others."[19] But it was evidently uphill work in the Russia of the Tsars. According to a

Aleksandr Stepanovich Popov (1859–1905) was born in the Urals and was educated at the University of St. Petersburg, graduating in 1882. He taught at the Torpedo School at Kronstadt and also taught electrical engineering and physics at the Naval Engineering College there between 1890 and 1900. His design of a receiver with an elevated antenna brought him a measure of contemporary recognition: he received a premium from the Russian Technical Society in 1898 and a diploma at the Fourth International Technical Congress in Paris in 1900, and he was elected vice president of the Russian Physico-Chemical Society in 1904. He died without receiving the full international recognition that continued participation in the early years of radio development would have brought him, but later his achievements were brought to the attention of the world through the efforts of his countrymen.

Soviet source, "a shop established by Popov in 1900 at the Kronstadt port for the repair and manufacture of wireless-telegraph apparatus had neither adequate equipment nor sufficient personnel to suppy the Russian Navy with radio stations. On the eve of the Russo-Japanese War of 1904–1905, the Naval command was forced hurriedly to supply its ships with German-made radio stations."[20] This apparatus was derived from the designs of Marconi, whose merits were well appreciated in Russian official circles: in 1902 he had been made a Knight of the Order of St. Anne of Russia.

There is every indication that Popov's subsequent work was likewise of the highest caliber; had he had the opportunity, he would have doubtless continued to make important contributions. The Russians have good reason to be proud to have produced a pioneer of Popov's rank; but the officious Soviet campaign to denote him as the "inventor of radio" and to enlarge his reputation out of proportion with his achievement amounts to a deviation from objectivity that must be deplored by all historians of technology who remain untouched by chauvinistic considerations.

As we shall see in our next installment, the mainstream of technological development in radio communications proceeded largely from the efforts of Marconi and his followers and scarcely at all from those of Popov—not even (as we have seen) in Russia—with one possible exception. The famous Paris instrument maker, Eugène Ducretet (1844–1908), always insisted that his designs were based on the radiotelegraphic receiver of Popov rather than that of Marconi.[21] Ducretet may have been influenced in his attitude by the fact that Marconi had by then taken out a patent, whereas Popov had not.[22] In any case, the receiver designs evolved by the two men were nearly identical, as Popov pointed out in his letter to *The Electrician*[18]—a development that is far more common in the history of technology than is supposed.

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Charles Süsskind's biography appeared on page 33 of the August 1968 issue.

Pulse trimming of thin-film resistors

Thin-film resistors of chromium–silicon oxide (Cr-SiO) can be trimmed precisely by a technique involving a heating and annealing process controlled by short pulses of the proper power, duration, and frequency

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Resistivity decreases can be induced in Cr-SiO films by current pulses of short duration. Precision resistance measurements between pulses permit trimming of film resistors to 0.1 percent in less than one second, provided pulse power, duration, and frequency are adjusted properly. A trimmer system that contacts several resistors through multiple probes and controls the process through a paper-tape reader is described. Pulse trimming has been applied to film resistors ranging from pure Cr up to Cr–40 mole % SiO. After annealing at 400°C, additional resistance decreases of at least 20 percent are possible with all compositions containing SiO. The interval between 15 and 30 mole % SiO is most suitable because substantial resistance changes are obtained at pulse powers well below the limits at which resistors burn out.

Microminiature thin-film resistors of Cr-SiO were developed by the use of vacuum evaporation and subtractive etching processes several years ago.[1,2] Analysis shows that the resistor tolerances are limited to about ±10 percent if dimensions of the order of 1 mil (25 μm) are involved. Absolute tolerances of ±5 percent are contingent upon developing the techniques of photographic mask fabrication and photoresist etching beyond the present state of the art. Resistors of greater precision approaching ±1 percent require individual trimming, but because of the small resistor sizes, trimming techniques based on altering the thickness or geometry of the current path[3] are not practical. However, the alternate possibility of inducing resistivity changes by heating of individual resistors can be used regardless of size.

Thermally induced resistivity changes

The effects of heating on the resistance of thin metal films are related to various types of structural change.[4] In oxidizing ambients, electropositive metals form surface or grain-boundary oxides, which constrict the current paths. Therefore, these films, of which tantalum films are typical,[5] show resistance increases upon annealing. If oxidation is prevented, postdeposition annealing causes resistance decreases in most metal films.[6,7] Irreversible changes of this type occur because freshly deposited thin films are highly disordered, and on heating tend to assume a state of greater order with less free energy. At temperatures offering sufficient atomic mobility for ordering processes to occur, electron scattering in thin films is reduced and the mean free path is increased. This has been demonstrated by Hall measurements on mechanically deformed metal foils.[8] Typically, the resistance of thin films decreases in several steps if the annealing temperature is raised uniformly. To explain resistance changes that occur over wide temperature ranges, it has been proposed that the corresponding decay processes of point defects are associated with a spectrum of activation energies.[9]

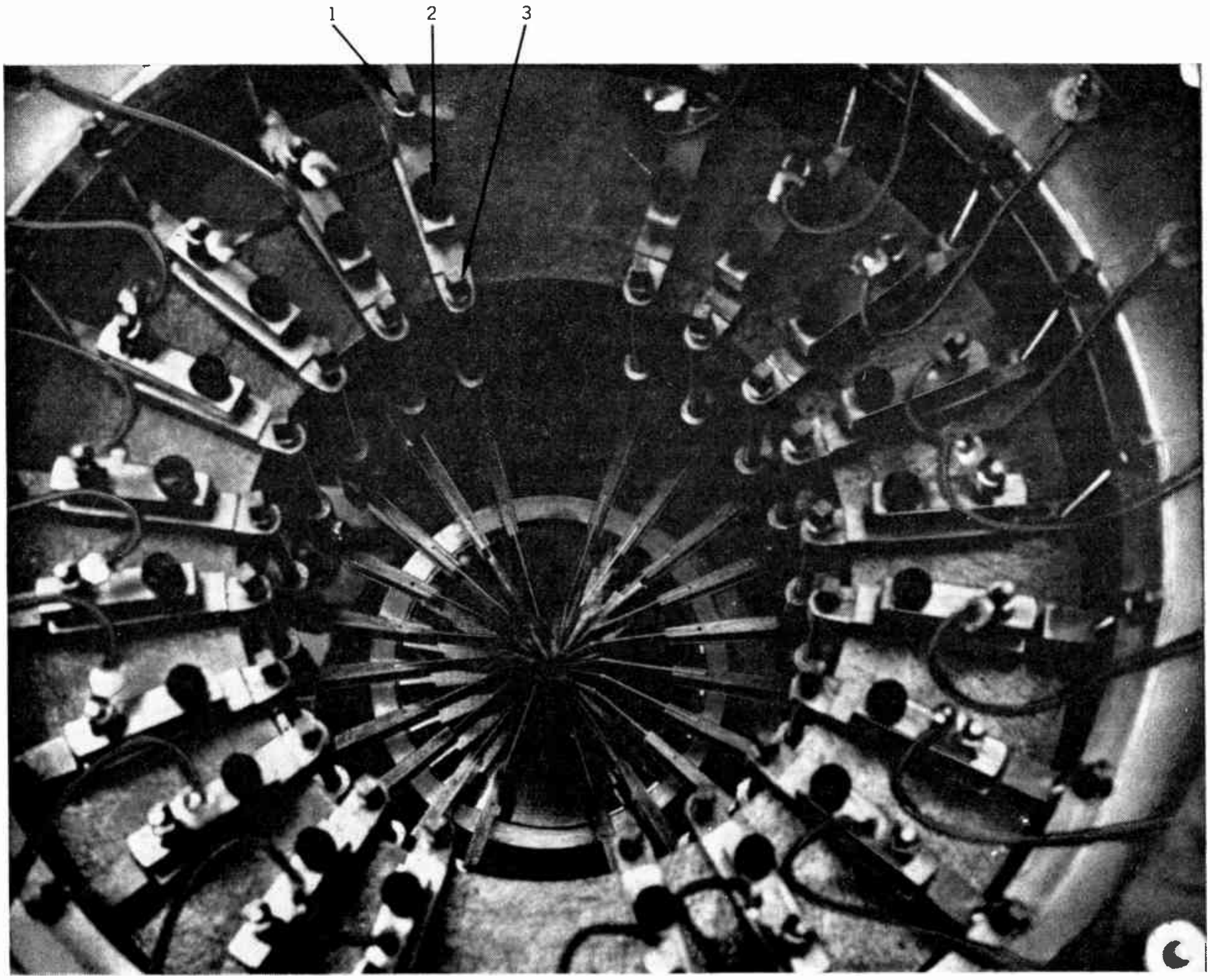
In the case of certain alloy[10] and metal–dielectric films,[11,12] the irreversible resistance decreases induced by annealing are larger than in metal films because they reflect transitions from amorphous to crystalline structures. Cr-SiO films fall into this category, with the additional complication that one of the constituents is the compound Cr_3Si, which forms by solid-state reactions.[13] Consequently, the films consist of partially crystalline Cr and Cr_3Si regions separated by SiO_2. Their resistivity can be reduced significantly by annealing, which leads to segregation of Cr_3Si so that more conductive bridges between metallic regions are formed.

The first attempt to utilize thermally induced changes for individual adjustment of small film resistors was made in 1962 by Maissel and Young,[14] who subjected tantalum film resistors to current pulses generated by capacitor discharges of about 1-ms duration. The resulting resistance changes were positive because the predominant reaction induced was oxidation. Although the technique yielded resistors within ±0.1 percent of the desired values, upward trimming has certain disadvantages in that film areas having higher than average resistance receive a proportionately greater fraction of power. Consequently, these areas become hotter and increase faster in resistivity than the rest of the film. As a result, local deviations of film resistance become enhanced, and the risk of destroying the resistor by concentrating the power in "hot spots" is high. In downward trimming the situation is reversed and high-resistance areas decrease first because they receive more power and become hotter. Local "hot spot" formation is therefore self-limiting, and resistors of greater uniformity are obtained.

Realizing the advantages of downward trimming offered by Cr-SiO, Bullard *et al.*[15] developed a dc method in 1963 that proved highly successful. With power inputs of the order of 1.55 $\mu W/\mu m^2$ (1 mW/mil^2) of resistor area, decreases of 20 percent or more of the annealed value were obtained on SiO-coated film resistors in air. Without SiO coating, the same technique yielded resistance increases up to about 40 percent. In both cases, the duration of the direct current was limited to avoid destruction of the devices; trimming times of less than one second were typical. The absolute tolerances of the trimmed resistors were ±1 percent or better. Since the resistors were monitored while the trim current was applied, it was necessary to use empirical corrections for reversible resistance changes after trimming.

Recently, a similar dc technique was described by Drukaroff and Fabula,[16] who induced up to 40 percent resistance decreases in evaporated Pt-Rh film resistors at power levels of about 4.7 $\mu W/\mu m^2$. Accuracies of ±1 percent or better are claimed, but repeated power application is required to correct resistance changes during cooling. Another technique that yields about 40 percent resistance decreases, as well as increases, employs laser pulses to heat Cr-SiO resistors without affecting adjacent

FIGURE 1. Multiple probe assembly for pulse trimming of film resistors.



1. Radial adjust screw 2. Height adjust screw 3. Left/right adjust screw

areas.[17] The approximate power densities are of the order of several milliwatts per μm^2 for millisecond pulse lengths. The pulse power must be increased repeatedly as trimming proceeds, and the resistance decrements appear to be somewhat irregular. Tolerances of ±0.1 percent seem feasible, but one second is considered to be the minimum trimming time per resistor. At about the same time, Braun and Lood[18] reported a method of adjusting Cr-SiO film resistors by electrical pulses with energies of 1.55 mW/μm^2.

The trimming technique described in this article uses the same principle as that reported by Braun and Lood. It employs downward trimming, which is very effective with Cr-SiO films, and can be performed in air, provided the Cr-SiO resistors are protected against oxidation. The trim energy is supplied by microsecond pulses of 1.55 mW/μm^2 peak power. This technique confines Joule heating to the resistor area alone and permits measurements between pulses when the resistors are at or near room temperature. As trimming proceeds, the resistance decrements become smaller, so target values can be obtained with an accuracy of ±0.1 ohm. The net trimming time is less than one second per resistor. The technique is suitable for automation and permits as many resistors as can be contacted with multiple probes to be trimmed in rapid sequence without mechanical adjustments.

Trimming system

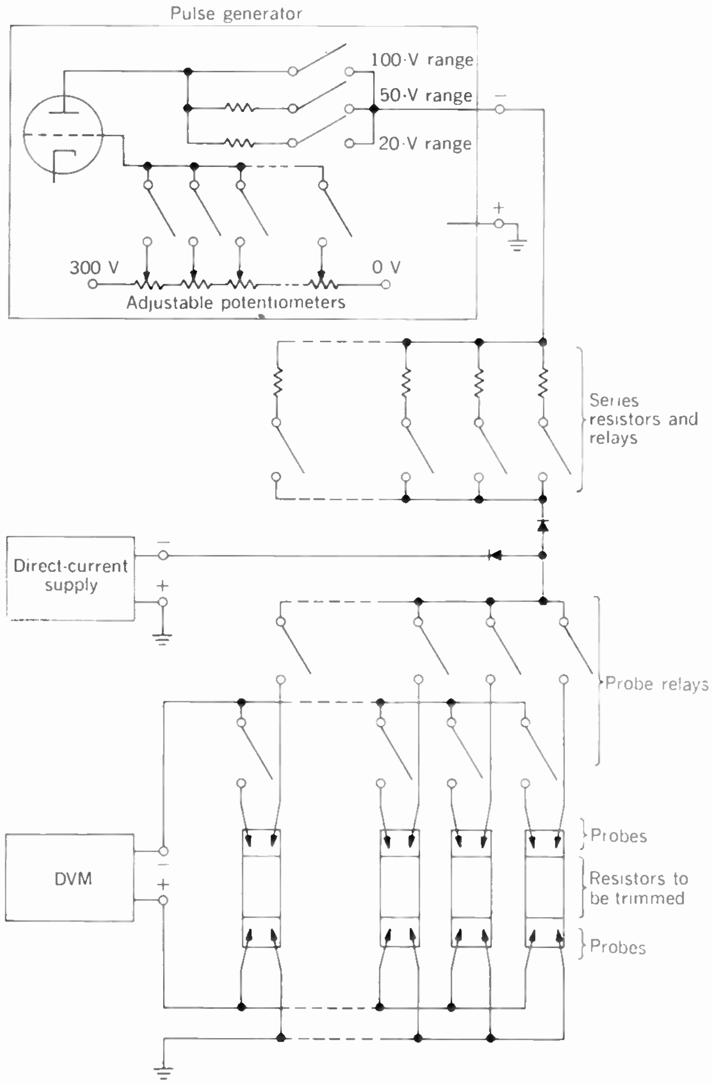
Multiple probes. For trimming of microminiature resistors, multiple probes capable of contacting several devices simultaneously are most economical because they reduce the frequency of mechanical adjustments. To achieve the desired precision, four-probe resistance measurements are necessary; that is, each resistor terminal must be contacted by two probes. For resistor land dimensions of at least 250 μm, Beckerman and Bullard's fixed multiprobe assembly[19] is a satisfactory solution. However, contact areas of this size would defeat the purpose of miniaturizing resistor dimensions down to 25 μm. In this investigation a multiple probe developed by Graner[20] was used; see Fig. 1. It consists of pointed contact blades with shafts sliding in precision sapphire bearings. The individual probes have limited adjustment in all three directions and rest on the film lands under their own weight. Since 20 probes are available, it is possible to contact either five separate resistors or up to nine resistors with one common terminal. To relocate the probes from one set of resistors to another, the pedestal supporting the substrate may be lowered and moved horizontally in both directions. The increments of travel are adjustable, and the probe setting may be initiated by push buttons or fully automatically.

The minimum contact area required for reliable positioning of two probes per land is a 150- by 150-μm square or a circle 150 μm in diameter. In situations where contact areas of this size cannot be tolerated, trimming may be performed through temporary lands.[21] These are etched in sufficiently large size out of a metal film deposited over the protective SiO_2 coating and connected to the resistor lands through holes in the oxide. After trimming, the temporary lands may be reduced in area by subtractive etching. This method has been applied successfully with temporary lands made from Al or Cr-Cu-Au films.

The trimming circuit. Figure 2 shows the trim circuit in schematic form. The pulse generator supplies current pulses to the film resistor being trimmed through a series resistor and one pair of probes. The resistor also receives a continuous direct current of 1 mA, the supply for which is insulated from the pulse generator by diodes. The dc signal is picked up by a second pair of probes and measured during the time intervals between pulses.

The circuit is multiplexed as indicated in Fig. 2 by several sets of selectable relays. One group of relays serves to connect the probes contacting the resistor to be trimmed to the current supplies and to the digital voltmeter (DVM). Since the resistors to be trimmed may have different values and require pulses of different power, the pulse generator was converted into a programmable pulse amplitude instrument. This was accomplished by substituting 12 potentiometers and relays for the amplitude vernier control, which determines the grid voltage of the power amplifier. Furthermore, the internal range switch of the generator was replaced by relays so that three different output voltage ranges could be selected. These modifications provided 36 possible pulse amplitudes, which could be chosen by activating one potentiometer relay and one voltage range relay. Finally, a number of adjustable series resistors also selectable by relays

FIGURE 2. Diagram of the multiplexed trim circuit.



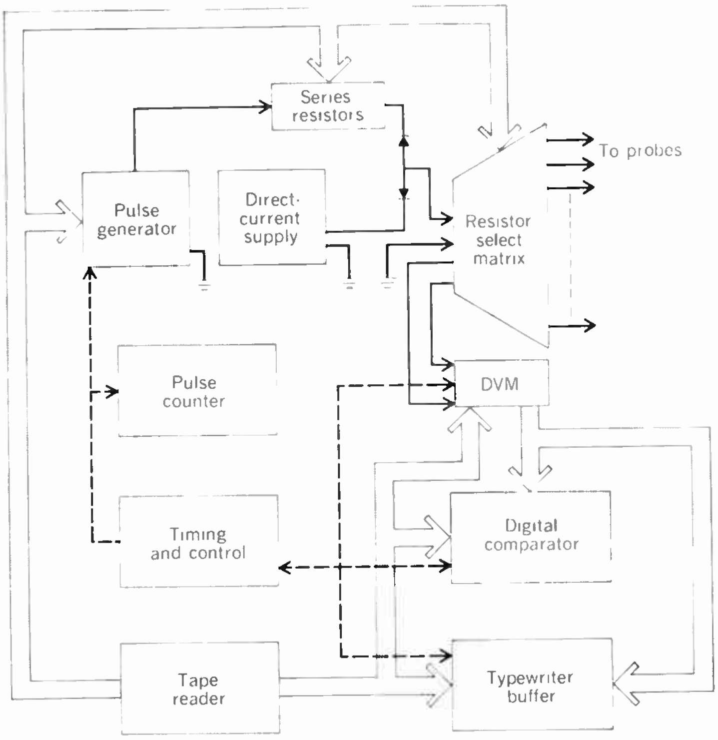


FIGURE 3. Block diagram of the trimming system.

were installed so that the total external circuit resistance could be chosen independently of the value of the resistor to be trimmed.

System control and operation. A block diagram of the trimming system is shown in Fig. 3. The resistor value as displayed by the DVM goes to a digital comparator, which also receives inputs from two sets of switches with four binary-coded decimal digits (BCD) each. The two groups of voltage levels generated by the BCD switches will be referred to as the stop value and the lower limit. During trimming, the comparator subtracts the digital output of the DVM (resistor value) from the stop value. If the result is negative, the trim process continues. The comparator also subtracts the lower limit from the DVM output. If this difference is negative, the resistor is rejected as overtrimmed. If both results are positive, the resistor has been trimmed successfully to a value that lies between the stop value and the lower limit. The number of pulses required to accomplish this result is displayed on a pulse counter.

The typewriter buffer stores the value of the resistor prior to trimming. When trimming is completed, the buffer decodes the input signals representing final resistor value and pulse count, activates the solenoids of an electric typewriter, and the initial and final resistor values as well as the number of pulses are printed. For high-speed trimming, data logging can be restricted to recording only rejected resistors.

Automatic operation of the trimming system is made possible by a paper-tape reader. This unit reads a block of 8 × 32 bits for each resistor to be trimmed and thereby provides the logic levels that represent the stop value and the lower limit. It also completes the trim circuit by actuating the relays that determine the pulse amplitude, series resistor, and probes to be used. Furthermore, the tape controls the typewriter carriage return and selects the range of the DVM. Additional bits in the tape block carry information to identify the first and the last resistor to be trimmed in a cycle.

The timing and control portion of the system determines the sequence of events in the trimming process, as shown in the flow diagram in Fig. 4. After a trim pulse has been released, the process is delayed to allow the resistor to cool. Within a few milliseconds, the resistor approaches room temperature and the DVM can be triggered to read the new value. The second delay is of the order of 100 μs and represents the resolving time of the DVM. Depending on the results given by the comparator, the process is repeated or terminated by either rejecting or passing the resistor. To avoid prolonged and unsuccessful trimming, the comparator may also reject a resistor if it has received a predetermined number of pulses without reaching the stop value. This pulse-count maximum is dialed manually into a register attached to the pulse counter.

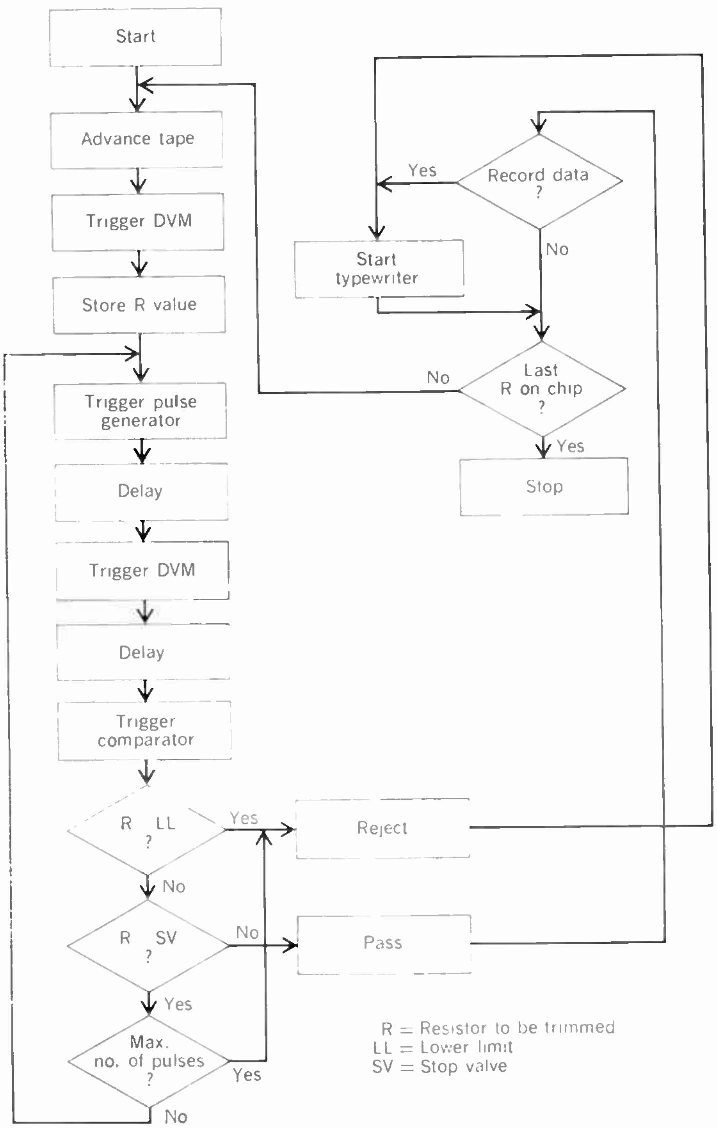


FIGURE 4. Flow diagram of the trimming process showing timing and control portion of system.

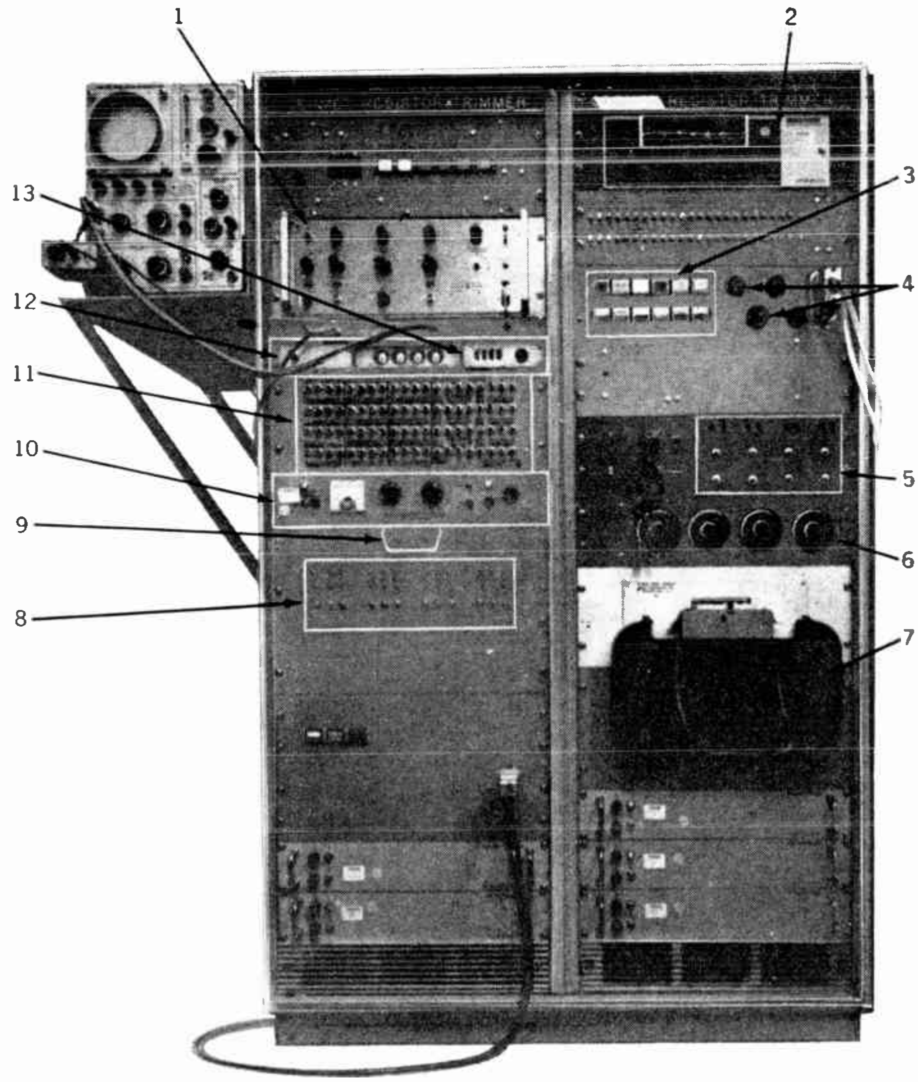


FIGURE 5. Resistor pulse-trimming system.

Legend:

1. Pulse generator
2. DVM
3. Operating controls
4. Pulse-frequency control
5. Series resistors
6. Substitute decade box
7. Tape-control unit
8. Pulse-generator potentiometers
9. Pulse-generator range switches
10. Direct-current supply
11. Switches for manual operation
12. Pulse counter
13. Pulse-limiting register

In the trimming system, shown in Fig. 5, all controls normally activated by the tape reader can also be operated manually through toggle switches. The pulse repetition frequency is set manually, since it does not require in-process adjustment, and the trim pulse duration can be varied with a control knob on the generator. A decade resistance box may be substituted for the resistor to be trimmed to close the circuit. This arrangement allows pulse amplitude, duration, and frequency to be adjusted without resistance changes while these parameters are observed on the oscilloscope screen.

Trimming parameters. The resistor trimmer permitted adjustment of the electrical parameters over a wide range. Initially, this flexibility was necessary to determine the effects of pulse repeat frequency, duration, and amplitude on the trimming process. The test samples were etched Cr–20 mole % SiO resistors with Al contacts coated with SiO_2 and annealed at 400°C for one hour.[2]

Pulse frequency. To maximize trimming speed, the greatest possible pulse frequency is desirable. However, the sequence of pulses must be slow enough to permit heat dissipation so that the resistor value will be measured near room temperature. To allow as much time as possible for resistor cooling, the trigger pulses that initiate the resistance measurements were set to occur near the end of a cycle. For example, if trim pulses were released at 0, 10, 20 ms, resistance measurements were triggered at 9, 19, 29 ms, respectively.

To determine the maximum pulse repeat frequency, several groups of 25- by 250-μm resistors were trimmed against a stop value of 300.0 ohms. The initial resistor values ranged from 325 to 380 ohms. With the pulse amplitude and duration chosen, 40 to 60 pulses were required to reach the stop value. A few seconds later, the resistor value was remeasured to determine whether complete cooling had caused any further resistance changes. The results of these experiments are shown in Table I.

Since annealed Cr-SiO resistors have small negative temperature coefficients of resistance (TCR), high repeat rates cause overtrimming because of premature resistance measurements. At 1000 Hz, the TCR effects approach 1 percent of the resistance value; the scattering of the final resistance values also increases for the higher pulse frequencies. Therefore, 100 Hz was chosen as the standard operating frequency.

Pulse duration. The pulse duration and amplitude were adjusted simultaneously so that the 25- by 250-μm resistors reach the stop value of 300.0 ohms with 30 to 40 pulses. The resistor values after complete cooling to room temperature are shown in Table II.

The resistor values obtained with pulses of more than 10-μs duration indicate overtrimming as the result of insufficient heat dissipation between pulses. Comparison of the pulse energies shows that comparable trimming requires more energy the longer the pulse duration. Hence, long pulse durations cause large amounts of energy to be dissipated into the substrate area surrounding the resistor—an undesirable condition. Conversely, peak pulse power increases as the pulse duration becomes shorter. However, operation at short pulse durations demands relatively high current amplitudes, which may be beyond the output capability of the pulse generator if large-size resistors are to be trimmed. A pulse duration of 5 μs was chosen as a suitable compromise.

Number of pulses. The number of pulses required to trim a resistor depends on the size of the desired resistance decrease and on the peak pulse power employed. Figure 6 illustrates the decrease of 25- by 250-μm resistors for pulses of different peak power. Each curve represents one resistor as trimmed initially by single pulses and later on by groups of several pulses at repeat rates of 100 Hz. With increasing peak power the total resistance decrease obtained with 100 pulses increases, and the initial resistance decrements become larger. As trimming proceeds, the resistance decrements per pulse decrease and the residual resistance asymptotically approaches the lowest value that can be obtained at the peak power.

For practical purposes, the number of pulses must be limited so that the trimming time per resistor does not exceed an economical maximum. A period of one second was chosen arbitrarily as the maximum trimming time. Consequently, the peak power must be sufficiently high that even the highest resistors do not require more than 100 pulses to reach the stop value. On the other hand, each resistor should receive a sufficiently large number of pulses to approach the stop value in decrements that are not greater than the permissible deviations from the design value. Therefore, the peak power must not be too high either. Most of the 25- by 250-μm resistors, for example, varied initially between 340 and 375 ohms. To reach the design value of 300.0 ohms, minimum and maximum decreases of 12 and 20 percent, respectively, are required. According to Fig. 6, a decrease of 20 percent with 100 pulses is obtained at a peak power of about 8.5 watts. At this power level, the minimum decrease of 12 percent requires about 20 pulses, and the decrement of the 20th pulse is estimated to be approximately 0.6 ohm. Hence, if the trim process is stopped at a value of 300.3 ohms the last pulse applied may at worst produce a resistance of 299.7 ohms. Experience has shown that tolerances of ±0.1 ohm can be obtained for resistors of less than 100 ohms if the peak power is adjusted correctly. For resistors between 100 and 1000 ohms the absolute deviations are somewhat larger, but they are still of the order of ±0.1 percent of the resistor values.

Resistors of different sizes. It is to be expected that resistors of different sizes will have different peak power requirements, which must be determined separately. The resistors used for trimming experiments were of rectangular shape with dimensions varying from about 12.5 to 250 μm.[2] The different sizes are available on the same substrate wafer and therefore experienced identical deposition and annealing conditions. They were also trimmed under identical conditions except that the peak power varied. Instead of terminating at a certain stop

I. Average resistor values and spreads for various pulse repeat frequencies*

Pulse Repeat Frequency, Hz	Resistance Values After Cooling, ohms
100	300.0 ± 0.1
150	299.9 ± 0.1
200	299.8 ± 0.1
250	299.7 ± 0.3
300	299.5 ± 0.4
400	299.4 ± 0.3
500	299.3 ± 0.3
600	299.2 ± 0.3
700	299.1 ± 0.3
1040	297.9 ± 0.2
1190	297.0 ± 0.2

* Constant operating conditions: pulse duration, 5 μs; stop value, 300.0 ohms; peak pulse power, 8.7 watts.

II. Resistor trimming with pulses of different duration*

Pulse Duration, μs	Pulse Amplitude (at 300 ohms), amperes	Peak Pulse Power, watts	Pulse Energy, μW·s	Resistance Values, ohms
2.5	0.22	15.0	37	299.9 ± 0.1
5	0.17	8.7	43	299.9 ± 0.1
10	0.14	6.0	60	299.9 ± 0.1
20	0.12	4.3	85	299.7 ± 0.3
40	0.115	4.0	160	299.5 ± 0.5

* Constant operating conditions: stop value, 300 ohms; 30–40 pulses at 100-Hz repeat frequency.

value, the resistors received 100 pulses and thus reached different final values depending on the power applied, as illustrated in Fig. 6. The average resistance decreases for a given resistor type (100 pulses) were then plotted against the respective peak powers. From the resulting curves, the peak powers needed to induce relative decreases of 15 and 30 percent with 100 pulses were derived for each resistor size. The peak powers are shown in Fig. 7 as functions of the film resistor areas.

The graph demonstrates that the peak pulse powers required to produce comparable decreases in resistors of different sizes are directly proportional to the areas covered by the resistors and independent of the length-to-width ratios. Therefore, the peak power needs to be determined empirically only for one resistor type. The peak pulse powers P_i for all other resistors on the same substrate can be calculated according to the relation

$$P_i = k w_i L_i \tag{1}$$

If the resistor dimensions w_i and L_i are given in micrometers, the proportionality constant k is of the order of 1.55 mW/μm^2. Variations of k for Cr–20 mole% SiO films from different deposition runs are insignificant since the film composition is tightly controlled; therefore, few if any power adjustments are necessary for different resistor substrates.

Constancy of trim power. Since the peak power affects both trimming rate and maximum resistance decrease, it is important to prevent power fluctuations due to differences in initial resistor changes during trimming. The methods of maintaining constant peak power depend on the output characteristic of the pulse generator. The model used in this investigation did not yield current amplitudes that were a simple mathematical function of the external circuit resistance. Therefore, empirical calibration charts of pulse current amplitudes vs. external resistance had to be prepared. The charts consisted of 36 amplitude curves, representing all combinations of generator ranges and potentiometers. Since the latter were adjustable, it was possible to induce small parallel shifts of the amplitude curves for fine regulation.

In the selection of the proper generator range and potentiometer, the objective is to find the amplitude curve that matches most closely the peak-power requirement in the entire interval of possible resistor values. If R_b designates the largest resistance in the distribution of values before trimming and R_t the smallest resistance to occur (that is, the target value at which trimming is terminated), the condition for peak power may be written as

$$R_b I_b^2 = R_t I_t^2 \tag{2}$$

The two current amplitudes I_b and I_t at the beginning and at the end of the trimming process can be calculated from the resistor dimensions according to Eq. (1). A suitable generator range and potentiometer combination is one in which the amplitude drops from I_b to I_t while the resistor being trimmed changes from R_b to R_t. In most cases it is also necessary to include an additional series resistor, derived from the calibration charts, to satisfy Eq. (2).

Although established only for the extreme values of the trim interval, the constancy of peak power is also maintained at any value in between. With proper selection of amplitude and series resistance, variations in the peak power have been kept below ±1 percent. The procedure must be applied to each resistor type individually for the dimensions and range of resistor values given. Although it was not specifically stated, the constant-power condi-

FIGURE 6. Resistance decrease of 25- × 250-μm resistors vs. number of pulses at different peak powers. Pulse duration: 5 μs. The initial values of 100 percent refer to the state of resistors after one-hour annealing at 400°C.

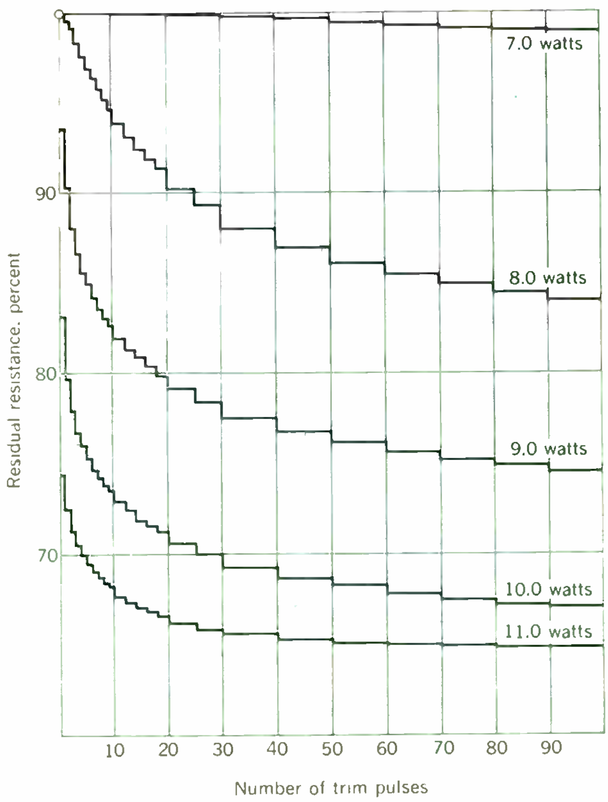
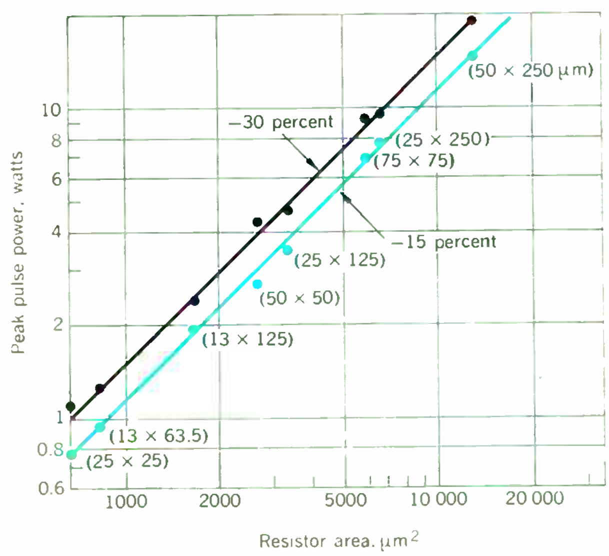


FIGURE 7. Peak powers required to produce 15 and 30 percent decreases in resistors of different size. Constant operating conditions: 100-Hz pulse frequency, 5-μs pulse duration, 100 pulses. The numbers in parentheses are resistor widths and lengths, in micrometers.



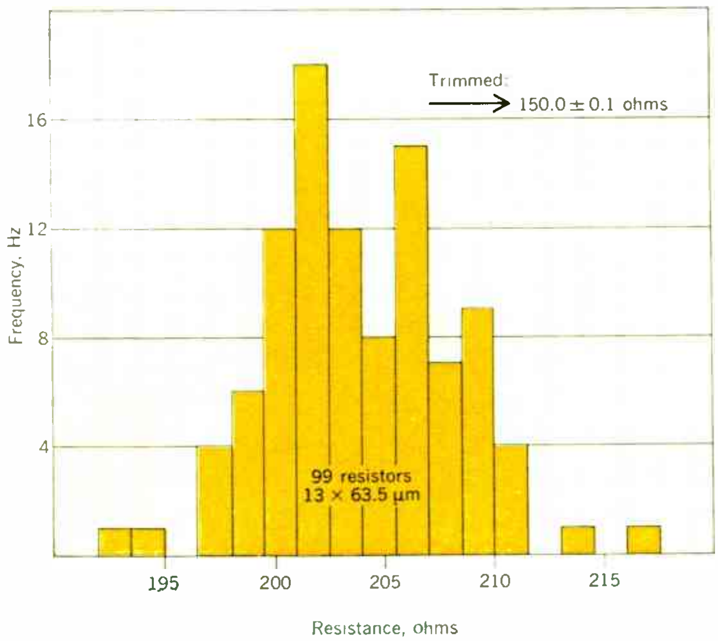
tion was fulfilled in all the experiments described previously to demonstrate the effects of trimming variables.

Results of pulse trimming

Microminature precision resistors. The difficulties in obtaining precision resistors with dimensions of less than 50 μm were discussed extensively in a previous article.[2] The greatest dispersion of values was found for the smallest etched resistors, which in this case were about 13 μm wide. To demonstrate the capability of the pulse trimming process, 100 resistors of 13- by 63.5-μm dimensions in a continuous array on one substrate were measured after having been annealed at 400°C for one hour. The data were grouped into 1.5-ohm intervals, each of which corresponds to 1 percent of the target value of 150 ohms. The histogram of these resistors prior to trimming is shown in Fig. 8. The values are spread over a wide range because the resistor dimensions deviate from the design data. The displacement of the distribution toward values greater than 150 ohms reflects a sheet resistance higher than 30 ohms per square, to facilitate downward trimming. One of the 100 resistors is not included in the distribution because its value was higher than 220 ohms as the result of an irregularity in the etched pattern; the device burned out during trimming. All other resistors were trimmed to the target value within ±0.1 ohm.

Since accuracy rather than trimming speed was the primary objective, a relatively low peak power of 1.32 mW/μm² was applied. Therefore, only 93 resistors reached the target value with less than 100 pulses. The remaining six resistors required a greater number of pulses because their values were at the far end of the histogram and needed decreases of about 30 percent. Exceeding the 100-pulse limit could have been avoided by increasing the peak power to about 1.4 mW/μm². However, the resistors

FIGURE 8. Histogram of 13- × 63.5-μm resistors after annealing. Trimming conditions: pulse frequency, 100 Hz; pulse duration, 5 μs; peak pulse power, 1.32 mW/m².



at the lower end of the distribution would then have approached the target value with a relatively small number of pulses and in decrements so large that the final tolerances would have been greater than ±0.1 ohm.

High-speed trimming of multiple-resistor chips. In practical applications, the overriding consideration is usually the trimming speed, whereas an accuracy of ±0.1 percent is rarely required. Consequently, the peak pulse power should be relatively high to enable trimming of all resistors with 100 pulses, which necessitates widening of the tolerance limits. As an example of automatic operation, trimming of the five-resistor network chip shown in Fig. 9 will be described. The network covers an area of approximately 0.75 × 1.5 mm on a substrate of SiO_2-coated silicon. The resistor dimensions were designed for a film-sheet resistance of 30 ohms per square. After annealing and prior to trimming, the resistor values corresponded to a sheet resistance of 34.5 ohms per square, and thus required an average trim decrease of 15 percent. Prior to trimming, the resistor networks were protected with 1.5 μm of RF-sputtered SiO_2. Access to the aluminum resistor contacts was provided by etched holes and 150-μm-diameter evaporated Cr-Cu-Au lands. Each of the seven external lands was contacted by two of the probes shown in Fig. 1. Trimming was performed on the completely processed substrate wafers prior to dicing.

Before automatic trimming can be attempted, it is necessary to program the process for each resistor type. Although the standardized variables such as maximum pulse number, pulse duration, and repeat frequency are permanently adjusted, other trimming parameters must be determined for each resistor type individually and transferred into the control tape. The latter forms a closed loop consisting of as many information blocks as there are resistors within the network. The stop values and lower limits were chosen 0.5 percent above and below the target values shown in Fig. 9. The pulse number was limited to 100. The current amplitudes were calculated according to the resistor dimensions for a k-factor of 1.4 mW/μm², and the most suitable combinations for the generator range potentiometers and series resistances were chosen from the calibration charts. The trimmer permitted checking these settings on the oscilloscope by substituting the decade resistance box for the resistor to be trimmed and closing the appropriate relays manually.

The precalculated peak powers are not always the optimum values for a particular wafer. Discrepancies may arise because the design dimensions rather than the true resistor lengths and widths are used for the determination of peak power. To make the last fine adjustments, a few resistor chips are trimmed with the control tape and their initial and final values as well as the number of pulses applied are recorded by the typewriter. The printout allows an evaluation of how well the calculated current amplitudes satisfy the exact requirements of the resistor substrates. Final corrections can be made at this point by slightly changing the pulse duration or by adjusting the generator potentiometers. The resulting small parallel shift of the current amplitudes does not affect the constancy of the peak power. This procedure also serves to ensure that the control tape has been correctly prepared and the tape block reader is functioning properly. Thereafter, the resistor networks on the substrate may be trimmed at high speed, printing only rejected resistors.

Although the net trimming time per resistor is at most one second, the time needed to reset the probes between tape cycles contributes a small delay.

Approximately 200 networks of the type shown in Fig. 9 were trimmed. Of these, 88 percent had all five resistors within the preset tolerance limits of ±0.5 percent. Other networks with up to eight resistors have been trimmed in equal or larger quantities, and the yields were consistently between 85 and 95 percent of all chips having every resistor within ±0.5 percent of the target values. Rejects could often be related to irregularities in the etched patterns and occurred mostly near the periphery of the wafer substrate. In these locations, one finds the most extreme deviations in resistor dimensions and therefore the peak power is not as well matched to resistor size and values as in the less peripheral areas of the substrate.

Other film compositions

Pulse-trimming characteristics. Up to this point, pulse trimming has been discussed only for resistor films of Cr with 20 mole % SiO. Since other compositions offer different sheet resistances, their amenability to pulse trimming has also been investigated. To make the results comparable, all films were deposited on SiO_2-coated silicon wafers at 200°C to a thickness of 1000 ± 100 Å. From these samples, 25- by 125-μm resistors with Cu-Cr contacts were etched and subsequently protected with 1.5 μm of SiO_2. Uniform thermal history was assured by heating all wafers at 400°C for one hour. Since different Cr-SiO compositions were used, the resistors had different sheet resistances, and decreases induced by pulse trimming were expressed as percentages of the resistances in the 400°C annealed state.

The ability to be trimmed was evaluated by subjecting resistors to groups of 100 pulses of 5-μs duration at 100 Hz, and recording the total decreases. The results of several resistors were averaged and the procedure repeated at higher peak powers until the resistors burned out. With increasing peak power, greater resistance decreases were obtained as previously illustrated in Fig. 6. From these data, maximum decrease vs. peak power curves were derived for the specific conditions employed. These curves varied depending on the SiO concentration in the films. Their most important characteristic points are listed in Table III.

Since the resistors were subjected to prior heat treatment, a certain threshold power must be exceeded before any resistance change can be induced. The second column in Table III lists the peak powers per unit area that caused 1 percent resistance decreases. The compositions from 10 to 30 mole % SiO have lower threshold values than the others.

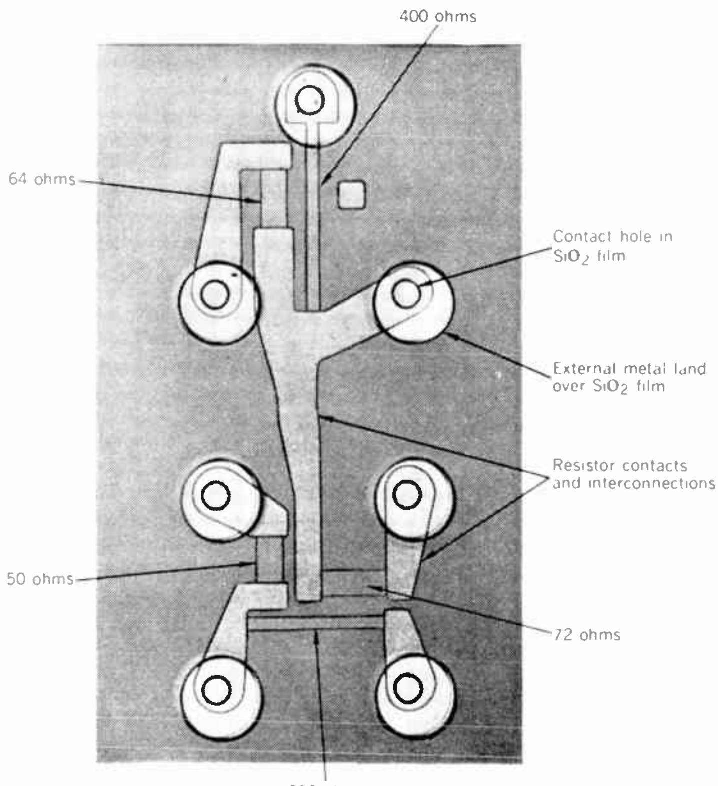
The next characteristic point is the peak power needed for 20 percent downward trimming, a decrease sufficiently large for most applications. The values for films with 20–30 mole % SiO are again lower than the others.

Burnout of resistors occurred with surprising regularity in rather narrow peak power intervals so that the maximum figures in column 4 are truly significant. If these levels were exceeded by 5 to 10 percent, burning out of the resistors was practically certain. Two compositions, 15 and 20 mole % SiO, excel in their resiliency toward overload. This is an important property, because it reduces the risk of damaging those resistors that deviate most strongly from the average values and dimensions. Conversely, the power factors for 20 percent trimming and for the maximum decrease are very close together for film compositions with less than 15 mole % SiO. Therefore, adjusting the peak power and maintaining it constant becomes sufficiently critical to question the practicability of decreases as high as 20 percent for these compositions.

The last column lists the greatest resistance decreases observed after 100 pulses of maximum peak power. Except for pure Cr films, all compositions yield at least 20 percent. From 10 mole % SiO on up, the maximum decrease rises nearly in proportion to the SiO concentration. However, although decreases up to 35 percent in Cr–20 mole % SiO films can be obtained at levels about 20 percent below the permissible maximum, equally large decreases in other compositions require peak powers close to the burnout levels.

Review of Cr-SiO film properties. To assess the usefulness and ranges of application of Cr-SiO films, the properties of interest in the design of resistors are reviewed in Table IV. The different phases associated with the various film compositions are shown to demonstrate a correlation between maximum decrease during trimming and the presence of increasing amounts of Cr_3Si. Also, the lower peak power thresholds found for 10 to 30 mole % SiO films coincide with the region of Cr_3Si formation. The 40 mole % SiO films, which consist only of Cr_3Si, have a greater minimum peak power and an unusually steep decrease vs. peak power curve. It is therefore concluded that the presence of α-Cr in addition to Cr_3Si im-

FIGURE 9. Five-resistor network chip. The resistors are 25 and 50 μm wide. The numbers indicate the corresponding target values.



III. Characteristic trimming data for Cr-SiO films of various compositions*

Composition, mole % SiO	Trim Power Factor k, mW/μm²			Maximum Resistance Decrease, percent
	Minimum	For 20 Percent Decrease	Maximum	
0	1.24	—	1.55	14
5	1.24	1.47	1.55	34
10	1.01	1.68	1.71	23
15	1.01	1.74	2.18	28
20	1.01	1.32	2.18	36
25	1.01	1.35	1.71	41
30	1.01	1.38	1.71	44
40	1.24	1.57	1.71	48

* Operating conditions: 100 pulses; pulse duration, 5 μs; repeat frequency, 100 Hz.

proves the trimming characteristics, and that the formation of the α-Cr/Cr_3Si eutectic is responsible for resistance changes at relatively low peak powers.

The greatest resiliency against overload (see Table III) is also associated with film compositions near the eutectic. Whereas the composition of the eutectic as indicated by Elliott[22] corresponds to an SiO content of 25 mole %,[13] the conspicuously favorable trimming characteristics of Cr–20 mole % SiO films suggest that the eutectic may coincide with the latter concentration since the greatest reactivity would be expected to be associated with the eutectic.

The resistivities in Table IV depend primarily on the concentration of SiO_2 and cover two orders of magnitude. Annealing causes significant resistance decreases, which are proportional to the amount of Cr_3Si formed. The resistance decreases induced by trimming are comparable to or even exceed those obtained by annealing at 600°C.

A property of great interest in the design of resistors is the highest sheet resistance that can be produced. It is determined by the resistivity and the smallest film thickness considered to be reliable. The latter figure is usually assumed to be about 200 Å, and the maximum sheet resistances in Table IV are calculated accordingly. Since annealing is necessary to produce stable resistors, the sheet resistances after heating at 400°C and those after an additional 20 percent downward trimming define the range of useful values.

The reproducibility data in Table IV indicate the degree of sheet resistance control achieved by the pellet flash evaporation technique[1] if all substrates are subjected to a standard annealing process at 400°C for one hour. Again, film compositions in the α-Cr/Cr_3Si eutectic range are most favorable in regard to control of final resistance values, whereas sheet resistances greater than 250 ohms per square are associated with greater deviations. However, this does not exclude their practical use, since sheet resistance corrections can be made by pulse trimming. Cr–50 mole % SiO films could not be evaluated because the pulse generator did not provide sufficiently high current amplitudes at very large external resistances.

The temperature coefficients associated with the film resistivities after various types of stabilization are listed at the bottom of Table IV. Small to moderately large negative values are typical for unannealed films, which tend to be amorphous. Both forms of heat treatment induce atomic ordering processes and thereby shift the TCRs toward positive values. Exceptions to this behavior and the conduction mechanisms involved are discussed elsewhere.[13] For practical purposes it should be noted that extremely small coefficients of less than 100 parts per million occur again in the eutectic composition range from 20 to 30 mole % SiO.

Summary and conclusions

Cr-SiO films are uniquely suitable for pulse trimming because large resistivity decreases can be induced thermally. The sheet resistances and temperature coefficients after trimming are similar to values obtained by annealing at temperatures of at least 600°C. This indicates excellent stability of electrical properties as a result of the trim process. By varying film thickness and SiO concentration, sheet resistances from about 10 to several thousand ohms per square may be obtained. Films with 20 to 30 mole % SiO are preferable because they have temperature co-

IV. Properties of Cr-SiO film resistors as determined by composition and heat treatment

Composition:	In mole % SiO	0	5	10	15	20	25	30	40	50
	In vol %									
	α-Cr	100	91	79	62	47	30	21	—	—
	Cr_3Si	—	—	5	15	23	34	39	51	15
	Cr_5Si_3	—	—	—	—	—	—	—	—	30
	SiO_2	—	9	16	23	30	36	40	49	55
Resistivity, μΩ·cm:	As deposited at 200°C	130	280	440	440	540	820	1160	3600	20 000
	Annealed 1 hour, 400°C	100	260	380	350	370	480	550	1050	6 000
	Annealed 1 hour, 600°C	90	250	320	290	310	340	440	520	2 500
	Trimmed (maximum)	86	170	290	260	240	290	310	540	†
Sheet resistance, Ω/square:	~200-Å film; 1 hour, 400°C	50	130	200	200	200	250	250	500	3 000
	Trimmed down 20 percent	45*	100	150	150	150	200	200	400	2 400
	Reproducibility (untrimmed)	9%	7%	3%	2.5%	2.5%	3%	3.5%	5%	12%
TCR (25-85°C), ppm/°C:	As deposited at 200°C	−60	−650	−250	−120	−120	−150	−160	−320	−750
	Annealed 1 hour, 400°C	+150	−600	−300	−110	−60	−30	+6	+100	+40
	Trimmed down 20 percent	+250*	−400	−650	−450	+10	+40	+90	+250	†

* Trimmed down only 14 percent.
† Films of this composition were not trimmed.

efficients of resistance between 0 and ±100 ppm/°C in the annealed as well as in the trimmed states. The same compositions also permit the closest sheet resistance control in the film deposition and annealing steps. Futhermore, they are the least likely to burn out in the trimming process if the peak pulse power accidentally exceeds the optimum level. It is concluded that these properties are related to the formation of α-Cr/Cr_3Si eutectic, which is the metallic constituent in films of these compositions.

Of particular importance for practical applications is the ability to automate pulse trimming by using multiple probes and controlling the process through a tape reader. Operating economy is ensured by net trimming times of about one second per resistor and established yields of about 90 percent for multiple resistor chips. Resistor tolerances of the order of ±0.1 percent can be achieved routinely regardless of resistor size, whereas etched film resistors with dimensions of 12.7 to 127 μm are normally subject to variation of ±5 to 10 percent because it is not possible to control their lengths and widths accurately. Thus, pulse trimming is an excellent method to fabricate precision resistors that are compatible in size with monolithic circuits.

The competent assistance of T. C. Prizzia in some of the trimming experiments and in the TCR measurements is gratefully acknowledged. Preliminary experiments to determine the required power and frequency ranges for the trimmer system have been conducted by R. L. Hallen and R. A. Holmwood. G. Schmidt contributed substantially in the assembly of the trimmer system.

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Reinhard Glang was born and educated in Germany, where he received the Vordiplom and Diplom degrees in physical chemistry from the University of Greifswald and the doctor of science degree from the Technische Hochschule in Darmstadt. He was employed by Diamond Ordnance Fuze Laboratories in Washington, D.C., in 1958. He joined the IBM Corporation in Kingston, N.Y., in 1960 and transferred to the newly formed Components Division in 1962. His technical activities, mostly related to microelectronic components, resulted in numerous articles on semiconducting oxides, diffusion in germanium, epitaxial growth of silicon, and various thin-film deposition processes. Currently a senior chemist at the East Fishkill facility of IBM, he is working on thin-film and semiconductor-device processes.

Klaus H. Jaeckel was born in 1936 in Berlin, Germany. He joined the Siemens-Schuckertwerke A.G. for a two-year training program in their R&D laboratories, where he worked on power cables and capacitors. After receiving a diploma in electrical engineering from the Ingenieurschule Beuth, Berlin, he worked for Westinghouse Cooper Hewitt, Berlin, on the development, design, and testing of regulated dc power supplies. In 1961 he joined IBM as a customer engineer. Following two years of service in the U.S. Army, in 1965 he joined the IBM Component Development Laboratory, where he worked with a group on a manufacturable thin-film process for microminiature Cr-SiO_2 resistors. He has since been active in thin-film vacuum process control.

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Leon I. Maissel received the B.Sc. degree in physics and chemistry in 1949 and the M.Sc. degree in physics in 1951, both from the University of Cape Town, South Africa, and the Ph.D. degree in physics in 1955 from the Imperial College of Science and Technology, London. In 1956 he joined the Philco Corporation, Philadelphia, Pa., where he worked on metal-to-semiconductor contacts and on thin-film hybrid circuitry based on tantalum. In 1960 he joined IBM in Poughkeepsie, N.Y., where he worked on the study, development, and application of thin films. He is currently manager of basic studies on thin films at the IBM Components Division. He has published more than 30 technical articles and is on the editorial board of several journals. He is a member of the board of directors of the American Vacuum Society.

Scanning the issues

Advance tables of contents
Translated journals
Special publications

Digital data transmission. It is an unusual event when a program of sponsored study is allowed to reach such a level of completeness that an author can present a meaningful and useful paper concerning the subject to his colleagues via the professional literature. In this writer's long experience with a large university-oriented research organization, innumerable instances come to mind where a particularly exciting development was allowed to remain dormant for lack of additional funding, or a well-thought-out research proposal never permitted to come to fruition because the funds were never there in the first place.

Such is not the case with the research efforts of Dr. Phillip A. Bello and company, who, with the sympathetic support of the U.S. Air Force Rome Air Development Center, have published a definitive set of five papers on "Digital Data Transmission Via Troposcatter Channels" in the April issue of the IEEE TRANSACTIONS ON COMMUNICATION TECHNOLOGY. In the words of Martin A. Perry, who writes an introduction to the set, "They complement each other in presenting an exhaustive treatment of the fundamental processes involved . . . ."

A unique and comprehensive discussion of the subject, these papers range from the initial modeling study through a review of the actual systems involved, and are expected to arouse considerable interest everywhere.

In the order of appearance, the titles of the papers are:

"A Troposcatter Channel Model"

"Selection of Multichannel Digital Data Systems for Troposcatter Channels"

"A Class of Efficient High-Speed Digital Modems for Troposcatter Links"

"Error Rates in Diversity FDM-FM Digital Troposcatter Transmission"

"Performance of an Energy Detection FSK Digital Modem for Troposcatter Links"

It would be improper to attempt an individual description of each of these; let it suffice that the following comments were derived from a host of competent reviewers:

"These papers present major contributions to solutions of the problems of digital troposcatter communications, with information which may affect the design of future systems."

"This paper is the first in open literature in which the concepts of modern communications theory are applied to optimum modem design for digital tropo. The analyses and computer simulation procedures, for calculating error performance of multifrequency, multiphase, adaptive bias systems, under conditions of additive noise with flat fading, and with frequency selective fading, are new and will be useful for workers in this field."

"The paper contains a single, unifying treatment of the frequency and time-dependency of a tropo channel."

"The definitions used in the development, if widely and commonly available, will provide a good basis for the advancement of tropo channel understanding."

"A concise compilation of the various modulation techniques, from the standpoint of power-bandwidth limitations."

"The analyses are complete, in particular, the discussion of modulation and detection techniques for tropo communications, which have been given a thorough and systematic treatment." ("Digital Data Transmission Via Troposcatter Channels," *IEEE Trans. on Communication Technology*, April 1969.)

LSI testing. The realization of LSI circuitry with multilevel metalization has brought with it an increase in the difficulty of testing such devices. Such microcircuit complexity seems to place even greater emphasis upon measurement of the fundamental parameters that are involved within the silicon chip. Authors E. S. Schlegel and G. L. Schnable have reviewed the possible failure mechanisms of multilayer LSI circuitry in a carefully written paper on "The Application of Test Structures for the Study of Surface Effects in LSI Circuitry," which appears in the Special Reliability Physics Issue of the April IEEE TRANSACTIONS ON ELECTRON DEVICES.

Relying upon 710 references compiled in two previous papers, the authors describe a set of basic test structures that permit the study of these failure mechanisms, and show how they may be most effectively used. Although the discussion of test structures is limited to those designed to measure surface effects (among the most important failure mechanisms), the techniques described are applicable to both bipolar and MOS structures, and to structures with either single- or multilevel layers of metalization. The effects of variations in materials or processes on the electrical properties of the interface are also given, and the authors provide experimental data on actual tests they have performed.

These techniques are doubly valuable since there is a trend in the industry toward greater use of glass passivation and better control of insulator–silicon interface properties. (E. S. Schlegel and G. L. Schnable, "The Application of Test Structures for the Study of Surface Effects in LSI Circuitry," *IEEE Trans. on Electron Devices*, April 1969.)

Glass lasers come of age. Invented approximately eight years ago, the glass laser is about to divest itself of all laboratorial integuments for the armorial sheathing of commercial application. This is roughly the length of time that it takes to accomplish such a transition, or so states C. Gilbert Young, the author of a most interesting paper entitled "Glass Lasers" that appears in the July PROCEEDINGS OF THE IEEE.

The author concentrates on reviewing three particular doping agents—neodymium, ytterbium, and erbium—and their behavior in assorted glass matrices. Dopant combinations are also considered.

Although some of the glass lasers have been operated continuously, according to the author, they are not at the present time serious contenders in the CW field. Pulsed operation, with discharge times