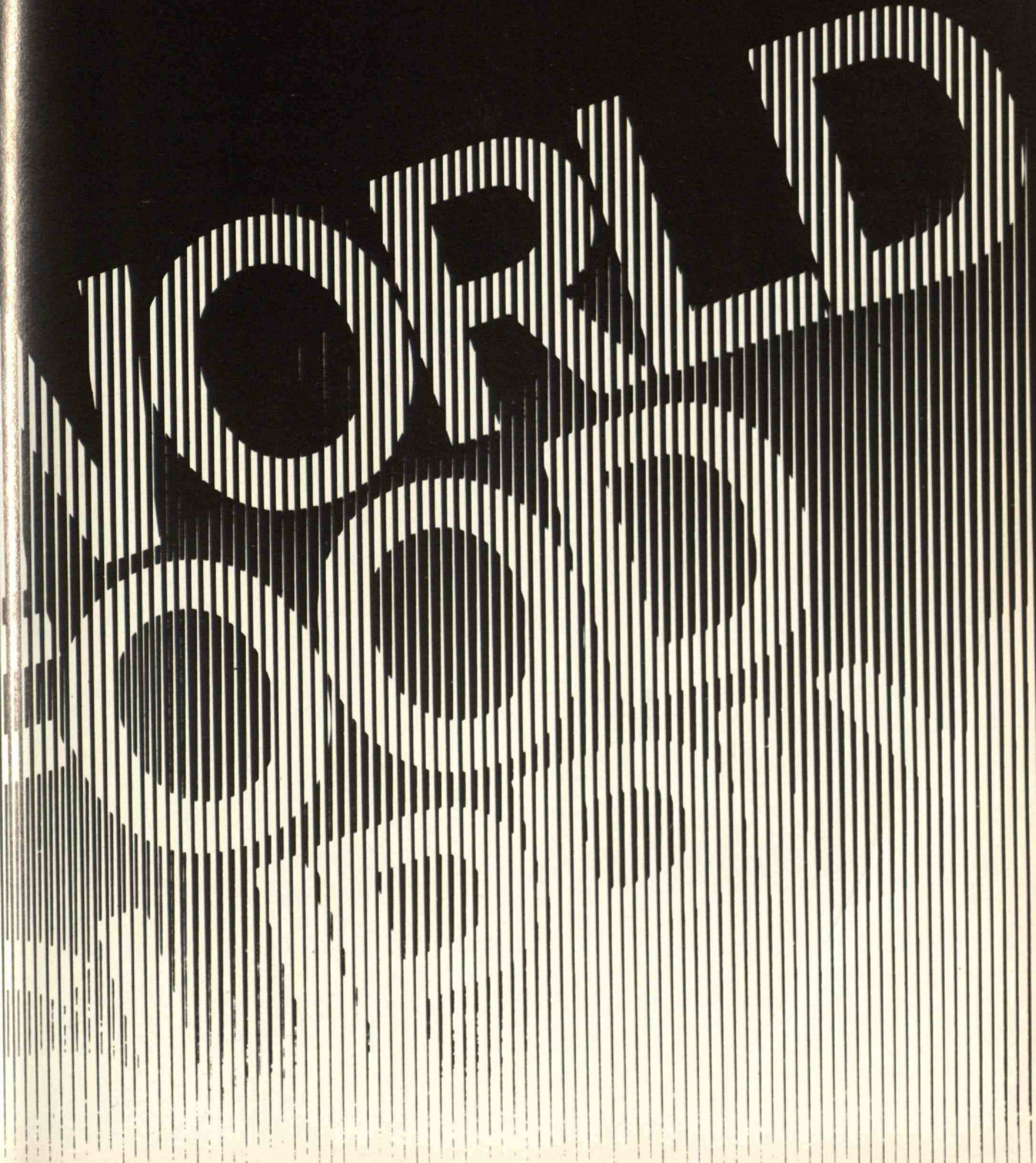
December, 1974 Price, $1.75

# Technology Review

Edited at the Massachusetts Institute of Technology



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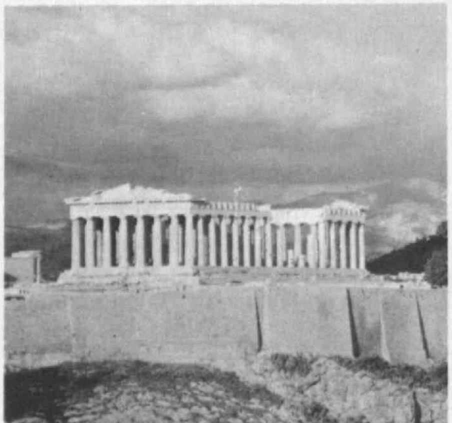

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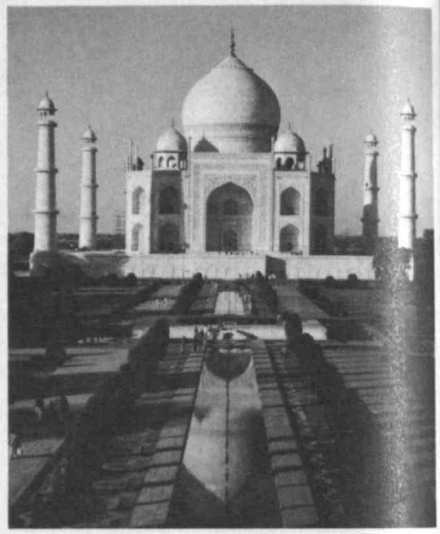

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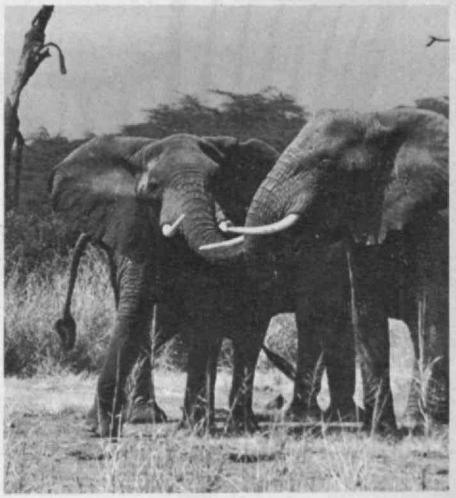

pink flamingos at LAKE NAKURU; multitudes of lion, zebra, wildebeest and other plains game in the MASAI-MARA RESERVE and the famed SERENGETI PLAINS; the great permanent concentrations of wildlife in the NGORONGORO CRATER; tree-climbing lions along the shores of LAKE MANYARA in the Rift Valley, photographing rhino and other big game against the majestic snow-covered background of Mt. Kilimanjaro in the AMBOSELI RESERVE; and the vast and fascinating wilderness of TSAVO NATIONAL PARK, renowned for its elephant and lion and for the unusual desert phenomenon of the Mzima Springs. There is also a stay in NAIROBI, the most fascinating city in East Africa, as well as features such as a visit to a MASAI MANYATTA to see tribal dancing and the tribal way of life. Total cost is $1799 from New York. Optional visits are available to the VICTORIA FALLS, to UGANDA, and to ETHIOPIA. Departures in January, February, March, May, June, July, August, September, October, November and December 1974 (extra air fare for departures in June, July and August).

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## Articles



## Departments

# Letters

## What Priority for the Breeder?

In "The Breeder Reactor in the U.S.: A New Economic Analysis" (*July/August, pp. 26-36*) Irvin C. Bupp and Jean-Claude Derian fail to note important considerations which, in my opinion, considerably weaken the authors' negative assessment of the incentives for early development of breeder reactors:

—The cost of the nuclear steam supply system, the area in which most of the light water reactor (LWR) and breeder cost differences should be concentrated, is only about one-sixth of total plant cost. Thus the $125/kwe. cost penalty allowed by the authors (which also happens to be about one-sixth of total plant cost) *really* represents a 100 per cent cost differential between concepts—a not-so-preposterous obstacle to be overcome by the breeder. At the very least there are no grounds for implying that the land, turbine plant, switchyard, etc., for the breeder will be more expensive than corresponding LWR items.

—In contrast to their diverse thermal reactor programs of the past, almost all other major industrialized nations (Russia, Germany, Japan, France, Great Britain, etc.) have also decided (and unless knowledgeably contradicted, one would assume independently and with some degree of reasoned evaluation) that development of the liquid-metal-cooled fast breeder reactor (LMFBR) deserves top priority. Indeed, from all reports, the currently operating French demonstration plant, Phenix, has established a cost-effectiveness benchmark which already gives reasonable assurance of successful commercial prospects for the breeder. Bupp and Derian offer no rebuttal to this favorable foreign opinion and experience.

—No mention is made of alternate breeder concepts, especially the gas-cooled fast breeder (GCFR), which has a primary system design substantially different from that of the LMFBR, and therefore offers a much different approach to achieving capital cost equity with LWR's. The molten salt breeder (MSBR) offers still another fundamentally different route to competitiveness.

—Important synergistic effects enter in, which reduce the costs of a mixed economy of thermal and fast reactors. In particular, the breeder can easily produce (as excess fissile product in its radial blanket) $^{233}U$, a premium fuel for all thermal reactors. General Atomic has shown, for example, that by this stratagem a system composed of one GCFR and three high-temperature gas-cooled reactors (HTGR) can be self-sustaining *without* recourse to uranium enrichment! Even without exercising this particular option, the breeder will help relieve a projected severe excess demand on enrichment capacity which will otherwise escalate future LWR fuel cycle costs.

Important technological considerations have been omitted from the analysis: LMFBR's operate at a primary system pressure of only 100 p.s.i. or so, while PWR reactors operate at around 2,200 p.s.i. Thus the LMFBR requires *much* thinner pressure vessels and piping—a factor which offers prospects for cheaper, rather than more expensive, plant costs in the long run. Attractive trade-offs can also be cited for the other breeder concepts: GCFR and MSBR. Indeed, all the breeder concepts have a significantly higher thermal efficiency than LWR's-hence lower waste heat disposal costs and environmental impact. Nowhere in the article is any analysis presented showing why the breeder *must inherently* be more expensive than a LWR. Novelty and "learning-curve" effects can be cited against any new system.

Michael J. Driscoll
Cambridge, Mass.

*The writer is Associate Professor of Nuclear Engineering at M.I.T. Drs. Bupp and Derian respond:*

Mr. Driscoll's comments focus mainly on the problem of the capital cost differential between present light water reactors (L.W.R.s) and future breeder reactors. Thus he provides us with the opportunity to discuss a point that, for space and editorial reasons, we did not develop in our article.

The issue is: On the basis of domestic construction experience with LWR technology, how confident can we be about the future costs of the breeder, a similar but essentially new technology? A related question is: How can foreign experience be taken into account?

The first point to be stressed is that for the past ten years in the United States there has been a systematic discrepancy, averaging a factor of two, between expected and actual costs (in constant dollars) of L.W.R.s There is *no* evidence that this discrepancy has begun to narrow. Indeed, contrary to experience with most industrial products, we have not as yet observed a "learning-by-doing" phenomenon in the nuclear reactor business. It is roughly true that L.W.R.s have been increasing in cost and still continue to do so today at the average rate of $30/kw./yr. in constant 1973 dollars.

In a separate paper ("Trends in Light Water Reactor Capital Costs in the U.S.," a report from the Center for Policy Alternatives, M.I.T., November, 1974) we and M. P. Donsimoni and R. Treitel have reviewed the capital cost experience of L.W.R.s in some detail and proposed an interpretation of it. The cost of a nuclear plant today has little to do with the cost of assembling different pieces of equipment and raw materials on a given site; it is rather highly dependent on the process by which these machines are licensed. The cost of the identical reactors on different sites is likely to vary widely according to licensing difficulties and the local intensity of nuclear opposition. The real question, therefore, with respect to the future cost of a new and similar product like the breeder is the response of the licensing process to the safety issues which may be raised. The cost history of L.W.R.s in the U.S. suggests that engineering estimates may be only a very weak guide, at best, to predicting the eventual capital costs of breeders. Ultimately, it is the perception that the public will have of this new technology and, as a consequence, the design criteria established by the licensing procedure that will determine the cost of a commercial breeder reactor. The questions and objections already raised by environmentalists during the recent debate on the environmental impact statement made by the Atomic Energy Committee

(*continued on p. 80*)

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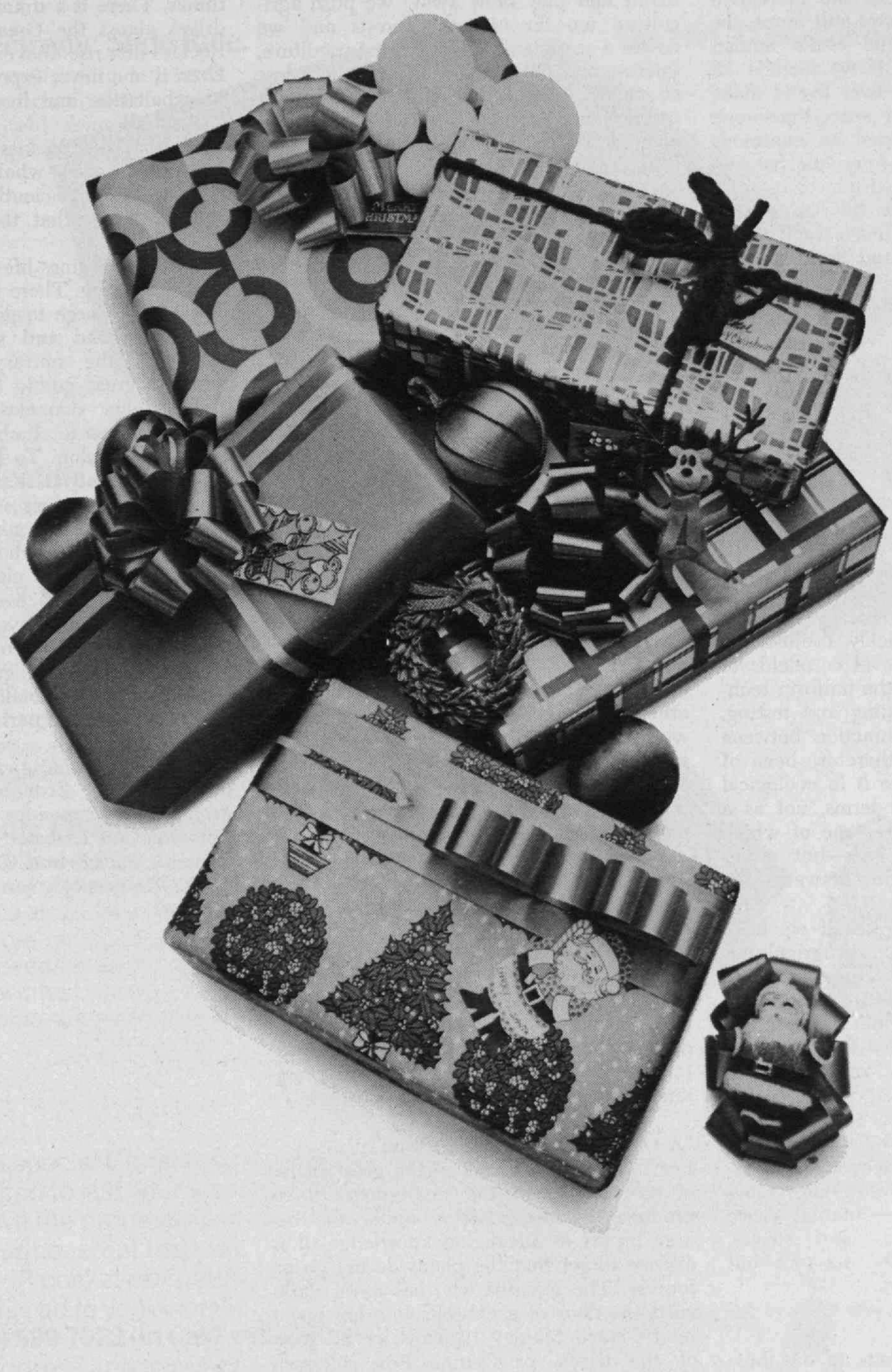

# Plains of Science, Summits of Passion

Technology/Society
by
Kenneth E. Boulding

The 10 billion neurons of the individual human nervous system, and still more, the $3 \times 10^{19}$ neurons of the whole human race (about $77 \times 10^{19}$ if we include all human beings who have ever lived) make a very large habitat in space-time—one that has already developed an enormous complexity of mental species but has yet realized only a small proportion of its total potential. One thinks of this as a vast ecosystem populated by images and ideas, perceptions and beliefs, and one perceives science as a small, but very productive sub-ecosystem within this vast habitat. This scientific ecosystem is rather like the agriculture in the Middle West and the Great Plains, surrounded by a vast expanse of the meadows of ordinary experience, the lush forests of religion and art, and the wild glaciers and peaks of ecstacy and agony, mysticism and power, sainthood and devilry.

I happen to live in a marginal ecosystem, where the Great Plains meet the Rocky Mountains and cactus blooms under the ponderosa pine. I have also lived most of my life on the uneasy margin between science and religion. Prickly cactuses of faith also bloom in the level cornfields of economics, cultivated by the uniform technologies of scientific planting and testing. The often conflicting interaction between science and religion has therefore been of great interest to me: I see it in ecological rather than in dialectical terms, not as a battle between two armies—one of which must win and the other lose—but rather like the wavering margin between the cornfield and the forest.

In the last century and a half we have seen an enormous expansion of agriculture, and the forest and the prairie everywhere have retreated before the relentless advance of the field. This is not unrelated to the similar advance in science, which is a kind of mental agriculture, and of government, which is political agriculture. Science raises periodic tables, testable equations, and mechanical and evolutionary models and routs out witchcraft and astrology, alchemy and old wives' tales. Government grows—we hope—internal peace and controlled economies and strives, somewhat less successfully, to rout out crime, strife, and depression.

### Science as Monoculture

Nevertheless there are limits to our husbandry in the field, in the laboratory, and in the legislature. We plow up the Great Plains and they blow away; we push agriculture too far into the forests and we create a precarious ecosystem. Agriculture, science, and government all result in a loss of species: An Iowa cornfield has far fewer species than the prairie which it supplanted.

Science is a world monoculture. The mandala of the periodic table appears in chemistry lecture rooms in Peking, Moscow, Rome, Tokyo, Hobart, and Singapore. There is no such thing as Communist chemistry, Catholic chemistry, or Hindu chemistry, white chemistry or black chemistry. Even economics is practiced somewhat furtively in the mathematics departments of socialist universities and Darwinian biology in the laboratories of Catholic universities.

Government likewise tends to create cultural uniformity, at least enough to ensure that everybody pays taxes. Only the nation, the religious sect, and the hippie cult stand between us and world monoculture.

There is something a little frightening in this. If one ecosystem goes wrong in a world of many ecosystems, the others do not; in a world of many isolated cultures, one can collapse, like the Mayan, and the others are quite unaffected. But if the world becomes a single ecosystem with a single culture, then if anything goes wrong, everything goes wrong. The Irish potato famine of the 1840s stands as a solemn record of the dangers of monoculture.

But as great as was the Irish catastrophe, it was retrievable because it was local. There comes a point as catastrophe moves toward universality where it becomes irretrievable. In a period of time over which the generalized Murphy's Law holds (if anything can go wrong, it eventually will), there is clearly an optimum degree of diversity from the point of view of maximizing the possibilities of continued long-range evolution.

### "A Dynamic Dance of the Mind"

For those who live out on the great plains of science, where the rich square fields produce increasing yields under the benign inputs of advancing knowledge, it is easy to forget that the plains do not go on forever. The scientist who has never darkened the door of a church, who has never read Gerard Manley Hopkins, or St. John of the Cross, or George Fox, or even Tennyson's "In Memoriam," may be living in a more restricted ecosystem than he thinks. There is a dramatic moment as one drives across the Great Plains where the Rockies first rise above the endless horizon. Even if one never experiences this moment of exhaltation and lives in the middle of Kansas all one's life, it may be nice to know that the Rockies are there. Even if one spends one's whole life raising good, solid, sustaining, scientific wheat, it may be good to know that the fields end somewhere.

At the margins, life can be difficult as well as exciting. There is a constant tension between the urge to go off into the plains and raise solid and nourishing scientific wheat and the contrary urge to disappear into the great gothic forests of the mind and indulge shamelessly in prayer and praise, or even to climb to the icy summits of mystical union. To have a foot in each world can lead to a very uncomfortable straddle, but it does surely lead to a dynamic dance of the mind which is seldom enjoyed by those whose feet are solidly planted in the rich plains. These margins are a good place to live for those who are agile enough to survive in them, and it is necessary for some people to live in them if we are to see the great habitats of the human mind as a totality and not as a set of totally unrelated parts.

*Kenneth E. Boulding, former President of the American Economic Association, is Professor of Economics at the University of Colorado and Director of the Program on General, Social, and Economic Dynamics at the University's Institute of Behavioral Science.*

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# Science Comes to Medicine—Slowly

Washington Report
by
Victor Cohn

In Washington, everything is political. This includes disease.

When Richard Nixon planned a 1972 campaign role for his daughters, he told his man Haldeman to have them go out to the "Middle America type of people" and "do the breast cancer thing." He wanted them to speak of his role in pushing a federal anti-cancer crusade, (once he was forced into it by Senator Kennedy and others).

When Mrs. Betty Ford had breast cancer surgery at the Bethesda Naval Medical Center on September 28, and the extent of the cancer's spread showed that her chance for long survival was uncertain, the question on political lips was, "Will this change the President's decision to run in 1976?" (He soon said it would not.)

When, within weeks, the wife of Mr. Ford's vice president-designate also developed breast cancer, one of the first reactions was a degree of sympathy for the increasingly beleaguered Nelson Rockefeller. Now he was not just Rockefeller, the political warrier, millionaire dynast and greatest of gift-givers. He was also Happy's loving husband whose main thought was her future.

Because Mrs. Ford and Mrs. Rockefeller and their husbands were all political persons at the very top level of American awareness, these women's breast cancers—like Nixon's phlebitis and Johnson's heart and Eisenhower's gut—were important news.

Furthermore, anything pertaining to this disease now became news. As it happened, there *were* in these same weeks some important new things to report. The entire country thus got a series of remarkable lessons in breast cancer prevalence, alternative kinds of breast cancer surgery and, in the process, science and even the ethics of science.

**Still Waiting for the Wedding**

There were not yet final answers. Much about breast cancer remains controversial or unsettled among medical practitioners. Much of the reason for this unsettled state—it became clear in those weeks of September and October—was medicine's long-standing failure to apply some of the simplest tests of science to this most common of women's malignancies and perhaps most feared of all cancers.

The breast cancer story has implications for many kinds of science and applied science.

In 1891, when Dr. William Halsted first removed a woman's breast at Johns Hopkins University, by what would soon become known as the "Halsted radical" operation, there had not yet been any real wedding between surgery and science. Each surgeon mainly did what he thought best.

If enough patients survived the very onslaught of the knife, the surgeon might write up his results. If the results seemed good enough, other surgeons might copy him. Not until the early years of the century did doctors like the Mayo brothers begin to look back systematically and assess their results, then guide their future operations by their past outcomes.

And not until the 1920s did more advanced biometrics begin to take hold in even the leading medical centers, with statisticians beginning to apply more sophisticated tests than just looking backward and counting.

One of the new biostatisticians' conclusions was that looking back—in statistical language, doing a retrospective study—is often unreliable. For many purposes, selecting a proper study population, giving alternate treatments on a randomized basis, *then* looking at the results—in short, making a prospective study—is far superior.

**Starting 80 Years Too Late**

Back to the breast. In the United States alone, 90,000 women develop such cancers and have such surgery every year. One woman in 15 can expect to get the disease sometime.

Yet not until three years ago was a group of academic surgeons headed by Dr. Bernard Fisher at the University of Pittsburgh able to begin what they and National Cancer Institute statisticians considered a properly designed prospective study of the true efficacy of Dr. Halsted's 1891 operation, compared with a simpler and gentler procedure.

Why so long a wait? Surgeons, says Dr. Fisher, are conservative types. Some operate all their lives in the way *they* were trained.

Not until the year 1971 could Dr. Fisher find surgeons at 34 centers willing to concede that they did not *know* which kinds of breast operations were better, therefore willing to compare the highly mutilating Halsted radical (which removes the breast, underlying chest muscles and the nearby armpit's complex of lymph nodes, the common site of the first spread of the disease) with two other methods—a so-called "simple" mastectomy (removing only the breast and no other structures) and a simple mastectomy followed by post-operative radiation.

Between 1971 and 1974 Dr. Fisher and his colleagues (in somewhat over-simple language) found that the results of 1,684 operations seem to show—there can be no certainty without longer follow-up—that:

—For cancers still limited to the breast alone, the simpler operation with or without radiation has the same results as the Halsted.

—For cancers with spread to the lymph nodes, the simpler operation with radiation has the same results as the Halsted.

On Saturday, September 28, three days before Dr. Fisher was to present these findings at the National Institutes of Health, just across the avenue from the Naval Medical Center, Mrs. Ford's surgeons, though knowing of Dr. Fisher's results, chose to perform on Mrs. Ford (whose cancer, remember, had indeed spread) a full Halsted radical mastectomy A few weeks later Memorial Sloan-Kettering surgeons chose to perform on Mrs. Rockefeller (whose cancer, so far as could be determined, had not spread) a "modified" radical, which leaves some of the muscles.

In both cases, the surgeons said that *their* and *others'* past results showed superior survival rates for the more extensive surgery. Dr. Fisher did not directly comment, except to say it was too bad that the more scientific study had not started years earlier, so the answers would now be more certain.

One wonders how many other surgical procedures are equally uncertain, and how many future patients might benefit from more science in surgery.

Ironically, Dr. Fisher—the man who felt so strongly about investigating the possibility of a gentler breast operation—during most of the time of his study used a consent form which did not inform each patient that:

—*Most* surgeons, though not the academic group, still believed that more radical surgery was almost always advised;

—*Her* own treatment would be chosen randomly, from a number list spewed up by a computer.

Dr. Fisher strongly (and probably correctly) maintained that he had taken great