serve to determine which explanation is true.
40. There are presently eight species of rockfish (*Sebastes* spp. and *Sebastolobus* spp.) which have been reported from the Aleutian Islands, only two of which (*Sebastes ciliatus* Tilesius and *S. polyspinus*) are abundant nearshore. *Sebastes ciliatus*, by its prevalence and higher abundance in today's communities (*18*), is probably the species occurring in the midden remains.
41. K. W. Kenyon, *J. Mammal.* **46**, 103 (1965); T. H. Scheffer and C. C. Sperry, *ibid.* **12**, 214 (1931); F. Wilke, *J. Wildl. Manage.* **21**, 241 (1957).
42. P. K. Dayton, in *Proceedings of the Colloquium on Conservation Problems in Antarctica*, B. C. Parker, Ed. (Allen, Lawrence, Kan., 1972).
43. R. I. Black, *Quat. Res. (N.Y.)* **4**, 264 (1974).
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46. L. Stejneger, *Am. Nat.* **21**, 12 (1887).
47. D. P. Domning, *Syst. Zool.* **25**, 352 (1976).
48. Nearshore community structure at Adak Island in the Andreanof Islands is similar to that at Amchitka Island in many respects (*12, 13*). Sea otters are near carrying capacity at Adak (K. B. Schneider, personal communication).
49. J. A. Estes, in *Environment of Amchitka Island, Alaska*, M. L. Merritt and R. G. Fuller, Eds. (TID-26712, Energy Research and Development Administration, Oak Ridge, Tenn., 1977), p. 511.
50. The black oyster catcher (*Haematopus bachmani*), which also preys on limpets, is common in the Rat Islands but absent from the Near Islands. The presence of oyster catchers in the Rat Islands complements the effect of sea otter predation on limpets to some unknown extent.
51. C. A. Simenstad, unpublished data.
52. K. W. Kenyon and J. G. King, "Aerial survey of sea otters, other marine mammals and birds, Alaska Peninsula and Aleutian Islands, 19 April to 9 May 1965," Bureau of Sport Fisheries and Wildlife Report, on file at the Fish and Wildlife Service, Department of Interior, Anchorage (1965); J. A. Estes, unpublished data. Population estimates of seals are uncertain because seals are readily observable only when hauled out, and their hauling out behavior is poorly understood.
53. We thank R. Desautels for access to unpublished data and specimen material from 49 Rat 31; E. J. Dixon and the University of Alaska museum for providing additional material from 49 Rat 31; and R. Burgner, P. Dayton, D. Eggers, C. Fowler, C. Harris, P. Martin, R. Nakatani, R. Paine, J. Palmisano, and C. E. Ray for criticizing earlier drafts of the manuscript. J. McMahon and S. Nancy Steinfort assisted with laboratory analysis. The Aleutian Islands National Wildlife Refuge, and particularly the R.V. *Aleutian Tern*, provided essential logistic support on Attu during the 1976 field season. This work was supported as a research project of the National Fish and Wildlife Laboratory and by U.S. Fish and Wildlife Service, contract 14-16-0008-2043, to the University of Washington. Contribution No. 482, College of Fisheries, University of Washington, Seattle 98195.

NEWS AND COMMENT

Guillemin and Schally: The Three-Lap Race to Stockholm

This is the second of three articles on the history of the pursuit of the brain's hormones by Roger Guillemin and Andrew Schally. Last week's article described how the two scientists had spent 7 fruitless years in search of the putative hormone known as CRF and a further 6 years in quest of TRF. To decide whether to continue supporting research in the field, the National Institutes of Health convened a conference in Tucson, Arizona, in January 1969. Three weeks before the conference began, averting an otherwise almost certain cutoff of funds, Guillemin obtained a result of critical significance.

The discovery made by Guillemin's team on the eve of the January 1969 conference in Tucson was a small step forward in one sense, a major advance in another. After processing some 270,000 sheep hypothalami they had obtained a 1-milligram sample of thyrotropin-releasing factor (TRF), the hormone with which the brain directs the pituitary's control of the thyroid gland. Their sample was pure enough to allow two conclusions to be drawn. First, the sheep TRF molecule consisted of three amino acids, glutamate, histidine, and proline—the same trio that Schally had found in 1966 in his preparation of pig TRF.

Schally had had chemists at the pharmaceutical house of Merck Sharp & Dohme synthesize the six possible combinations in which the three amino acids could be arranged. (He declined to share the samples with Guillemin on the grounds, says Guillemin, that "the FDA did not allow such transfers across state lines.") But all six tripeptides were biologically inert. Schally had therefore concluded that the biologically active part of the hormone must reside in the other two thirds of the molecule, with which he could make no headway.

The second conclusion which Guillemin was able to draw was that the other two thirds didn't exist—it was just an impurity, the three amino acids being essentially the whole of the molecule.

But now came a hard decision. The three amino acids were evidently not joined together in any simple way or Schally would have solved the structure with one of his synthetic tripeptides in 1966. If the new composition were announced at the Tucson conference, the prize of deciphering the structure would be up for grabs by any chemist in the world, with the Guillemin team having only a 3-week start.

A Photo Finish Race for TRF

Guillemin took the gamble and announced the composition. In the event, his start was more than abolished. Schally, who had temporarily abandoned the TRF problem, instantly perceived how close his rival was to the coup of being first with a chemical structure for a brain hormone. At the conference site he joined forces with an eminent structural chemist, Karl Folkers of the University of Texas at Austin, and arranged for the synthetic tripeptides to be transferred—across several state boundaries—to Folkers' laboratory. Guillemin, also in a call made from the conference, asked Hoffman–La Roche to synthesize the six tripeptides which Schally would not share.

From January through the fall of 1969 there ensued a furious race to solve the structure of TRF. The finish was so close and confused that to this day both teams claim priority, although on the Schally side with some internal difference of emphasis. Schally seems content to concede a draw, having written that the credit for solving the TRF project "had to be shared with Burgus and Guillemin, who elucidated the structure of ovine TRH* about the same time." Folkers, on the other hand, says flatly that "We were working totally independently of Guillemin and his team and we got it before they did."

The TRF molecule did not respond to the established chemical tests for identifying the ends of peptides, so evidently nature had blocked the ends in some

*The two teams naturally have different nomenclatures for the hypothalamic hormones or factors. Schally now calls them hormones, which indeed they are; Guillemin prefers the term factor to distinguish them from all the other hormones. The term factor was first used by Saffran and Schally in the name CRF. There are different versions as to who coined the term. In their respective contributions to *Pioneers in Endocrinology*, vol. 2, a forthcoming volume of memoirs edited by Joseph Meites, Saffran says that "we" coined the word CRF, Schally that "I" did, and Guillemin that credit belongs to R. A. Cleghorn, another member of their department.

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way. Knowing that some natural peptides have acetyl groups at one end, the Guillemin team acetylated the six tripeptides (which Hoffman–La Roche had synthesized in record time) and tested each for biological activity. Roger Burgus recalls as the most exciting moment of his scientific life standing beside the radiation counter that was set to record results of assays with the six acetylated tripeptides and seeing that just one of the six had biological activity.

The active tripeptide was that with the sequence Glu-His-Pro. The Guillemin team on 15 April dispatched to *Science* a paper describing this first step of the last lap in their 7-year search for TRF. The paper was rejected, Guillemin has written,† "on the comments of a referee who said (or words to this effect) that '. . . these hypothalamic releasing factors were not much else than a lasting fancy of Guillemin's vivid imagination.' "‡

Several more steps remained before the structure was solved. The Guillemin team learned that it was not an acetyl group that blocked the end of their biologically active TRF molecule but something else that just happened to occur during the acetylation process—the bending round of the glutamate unit to form an internal ring, known to chemists as pyroglutamate. The other end of TRF was also blocked, perhaps by an amide group. In a paper submitted on 30 June 1969, the Guillemin team discussed for the first time the substance known as pyroGlu-His-Pro-amide, only to conclude on the basis of infrared spectroscopy that the real TRF was something slightly different.

The team comprising Schally, Folkers, and Cyril Bowers of the Tulane Medical School, joined the race scarcely a month later. In a paper dated 8 August they suggested that they had synthesized TRF. Although they didn't know what the structure was, one of the substances in their reaction mixture was pyroGlu-His-Pro-amide. On 22 September they submitted a second paper stating for the first time that this was indeed the structure of pig TRF.

By this time the Guillemin team was realizing that they had been over fussy in the interpretation of their infrared data. In a paper submitted on 29 October they announced on the basis of mass spectroscopy that sheep TRF was indeed pyroGlu-His-Pro-amide. Guillemin and Burgus argue that the Schally-Folkers-Bowers paper of 22 September is not definitive because of the ambiguous nature of the method of identification, which depended on the behavior of natural and synthetic TRF in chromatographic systems. "Our evidence was the ultimate proof," says Guillemin. Folkers states that the chromatographic identification was conclusive, and Bowers adds that their data on biological activity helped clinch the case.

The patent on TRF is held neither by Schally nor Guillemin but by Folkers and a colleague, Franz Enzmann.

To the narrow question of who was first with the structure of TRF, the an-

†Memoir contributed to *Pioneers in Neuroendocrinology*, vol. 2, edited by Joseph Meites, to be published by Plenum Press, New York, in press.

‡A search of *Science*'s archives indicates that the manuscript was rejected, but the referee's report to which he is evidently referring makes no mention of Guillemin's vivid imagination.

Briefing

Mottur Resigns from OTA Job

Ellis Mottur, the long-term aide to Senator Edward M. Kennedy (D–Mass.) who was influential in helping to establish Congress' Office of Technology Assessment (OTA), has resigned from his OTA job and is no longer part of the Kennedy staff. The move came at the end of a series of events in which Mottur's considerable power was reduced.

Kennedy helped to get legislation passed that capped some 10 years of debate on Congress' need for more technical advice. As a result, Kennedy became chairman of the OTA's governing Congressional Board of Directors in alternating sessions of Congress. Hence, Mottur, as Kennedy's most senior staff aide at OTA, had considerable influence, a fact that was widely noted and sometimes resented around OTA. In fact, last year, when a Republican board member resigned, protesting that Kennedy was trying to use the supposedly nonpartisan office for his own political gain, the charges were widely taken to be aimed partly at Mottur. After this incident, Kennedy appointed another staffer as his official liaison, and his board of directors went out of its way to pick a prominent Republican, Russell T. Peterson, who was head of the Council of Environmental Quality and formerly governor of Delaware, as the office's new director.

After taking office, however, Peterson made two moves which substantially reduced Mottur's influence. The first was a one-line change in the rules, giving authority to the director instead of the board, to hire and fire all OTA employees. This small change thus ended the practice of having staffers answerable to their Congressional sponsors instead of to the office itself. The change was described by staffers as part of Peterson's move to "depoliticize" OTA.

A second move was a reorganization of the office announced in early April, which would have programs such as the one Mottur runs there, which is on the health and impact of national R & D, reporting through a layer of division directors instead of to Peterson himself. There will be three division directors, Peterson announced; another OTA staffer was promoted to one of the slots, and the other two will be filled from the outside. Mottur handed in his resignation on the day the change was announced.

Mottur could not be reached for comment despite several attempts to reach him before press time. His letter of resignation to Peterson gave "personal renewal and career progression" as the reason he was moving on. Associates say he is enthusiastic about a new job he will undertake, but he could not be reached for information on what it will be.

Security Agency's Role in DES Confirmed

The Senate Intelligence Committee has released a report of its investigation of charges that the National Security Agency (NSA) tried to influence development of a public encryption system for its own secret purposes and that it harassed university researchers working at the forefront of cryptology and their government sponsors.

The unclassifed summary of the committee's classified report leaves unanswered many of the original questions about NSA's role (*Science* 29 July 1977). But the report also calls for clarification of "vague and ambiguous" federal regulations that apply to the general field of

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swer is the Schally-Folkers-Bowers team, by a margin of 5 weeks in a 7-year race. In a wider sense, perhaps the Guillemin team scores more points. Schally's team came tantilizingly close to the structure in 1966 but was clearly on the wrong track until the Guillemin team brought them back again in January 1969. Without the powerful last minute help of Folkers and his laboratory, Schally's team would probably not have done as well as they did. The Guillemin team, relying principally on their own resources, kept the initiative from January onward in fitting together the successive pieces of the chemical puzzle. Their 30 June paper, though wrong, was within a hairsbreadth of stealing victory before Schally, Folkers, and Bowers had even started. Yet Schally by a whisker turned his impending defeat into a technical victory. The result of the 7-year race was in substantial measure a draw, but one so complexly attained that each team could interpret it as a win.

Discovery of the structure of TRF was a milestone in all sorts of ways. It proved to a world that still needed convincing that the postulated hypothalamic hormones (or factors) really existed. It laid a firm basis for a whole new branch of endocrinology. It assured continued funding for work which the National Institutes of Health would otherwise almost certainly have suspended. And it vindicated not only Geoffrey Harris's theory but Roger Guillemin and Andrew Schally and the long years they had spent in the wilderness. After the discovery of TRF, "No one could laugh at Guillemin and Schally any more," says a fellow physiologist.

The Two Team Error with LRF

With TRF solved, both teams turned with new energy to the next lap of the race, the isolation of LRF. Situated at the apex of the body's hierarchy of reproductive hormones, luteinizing hormone–releasing factor is a substance of considerably greater medical interest than TRF, and its isolation would be a proportionately greater coup. Proof of LRF's existence was first found in 1960 by S. M. McCann in America and Geoffrey Harris in England, the two teams working independently of each other. Guillemin, then based in Paris, started the search for LRF shortly thereafter. After 4 years' work he abandoned it because the best available physiological test for LRF was in his view too erratic to serve as the basis for an isolation program.

By 1969, however, more reliable tests had been developed. It took comparatively little time for both teams to get hot on the trail of LRF. By the end of 1970 each team had reported that LRF is a nonapeptide, a chain of nine amino acid units. The conclusion was not only wrong but put them in jeopardy of being scooped by a rival team. Cy Bowers and Karl Folkers, Schally's collaborators on TRF, had not been invited by him to join the search for LRF, so decided to set out on their own. After processing some 500,000 hypothalamic fragments they had not got as far as Guillemin and Schally but one thing they did know was that LRF contained the amino acid tryptophan. When peptides are broken down by acid, the conventional first stage in analysis, tryptophan is de-

Briefing

cryptology. It has received a mixed reception among scientists.

The most unsettling part of the report is that which confirms that NSA had a hand in developing the new data encryption standard, or DES, a public encryption system developed by the IBM corporation and approved by the National Bureau of Standards for commercial use in the United States and for export. Some scientists believe that the DES could have been made more secure at little extra cost by a number of methods, among them making longer the 56-bit key which defines the code. They suspected that the NSA wanted DES to be less than fully secure so that it could break into the encrypted traffic once the standard was in widespread commercial use.

The report confirms what had previously only been alleged: that NSA had a hand in IBM's development of the DES, and among other things, that "NSA convinced IBM that a reduced key size was sufficient." It also says that NSA "indirectly assisted in the development of S-box structures"—a feature of the scheme which scientists had said looked suspiciously vulnerable to someone who wanted to break onto the code. However, the report gives no reason why NSA played this role, or what its motives were.

The report says that the "overwhelming majority of scientists" and the NSA find the DES "adequate for at least a 5–10 year time span for the unclassified data for which it will be used." But Martin Hellman, one of the most prominent critics of the DES, objects that the government is doing a disservice to commercial customers by not proposing a code system that will be secure for any longer period.

Hellman adds that the report does not answer the key issue, which is, he says, whether NSA can crack the DES scheme.

On the question of harassment of academic scientists, including Hellman, who have developed new approaches to encryption that could lead to codes more secure than the DES, the report exonerates the NSA from any wrongdoing. "There has been no direct or indirect government harassment of scientists working in the field of computer security" it says. It claims that a letter, written by J. A. Meyer to the Institute for Electrical and Electronics Engineers (IEEE) which was interpreted by scientists as a form of harassment, was written by Meyer as an individual and "was not prompted by any NSA official." Press reports had identified Meyer as an NSA employee, but the Senate Report declined to confirm this.

The committee also looked into press reports that NSA had pressured the National Science Foundation (NSF) to withhold funds for research in public cryptology. NSF supports Hellman and other researchers who, as a by-product of basic work in problems in mathematical complexity, stumbled on a new approach to cryptology which could have wide application in electronic banking, communications, and perhaps even in verification of the comprehensive test ban treaty. The report says NSA had not pressured NSF in this fashion, but suggests that the two agencies clarify the "ambiguity and uncertainty" in their relationship.

After the report's release, NSF Director Richard C. Atkinson was quite blunt about not wanting NSA to harass his agency on the cryptology issue.

Atkinson says that the new developments promised by this research—such as virtually complete communications security—are too valuable to forego. "If we don't do the research other countries will get farther along with it. The long term disadvantage of not doing this research on our own in this is monumental."

Deborah Shapley

Together again—Guillemin and Schally at a Swedish prize-giving ceremony last year.

stroyed. Both Guillemin and Schally had somehow overlooked this problem. (Schally denies the rumor that he knew but held back his knowledge of tryptophan when publishing that LRF was a nine-amino acid peptide.) If Folkers and Bowers had been quicker to combine their results with those of Guillemin and Schally they might have been first to arrive at the true composition of LRF.

Though Guillemin and Schally appeared to be neck and neck at the end of 1970, the Guillemin team had suffered a crippling misfortune. With the techniques of the time, peptide chemists needed about 100 millionths of a gram of material to solve the structure of a peptide such as LRF. To obtain LRF, Roger Burgus, the Guillemin team chemist, had gone back to the side fractions of material collected during the course of purifying TRF. First estimates of LRF suggested he had recovered from 100 to 150 micrograms, just enough for structure determination. It was the worst setback of all, says Burgus, when closer measurement showed he only possessed 40 micrograms of LRF; "If I had had 100 micrograms I would have beaten him, I think we would have had the structure in a few months." Knowing it would set him back several months, Burgus started to work out new methods of microanalysis that would enable him to deal with a sample so small.

Meanwhile Schally's team was also facing the problem of quantity but from an altogether happier angle. From his side fractions of pig hypothalami Schally had isolated 800 micrograms of LRF. He considered that too little for analysis but turned it over to his chemists to do what they could with while he set out to isolate more LRF from fresh brain tissue. Schally's chemists at the time were two visiting Japanese, Yoshihiko Baba and Hisayuki Matsuo. Both, according to Schally, worked "desperately hard" on the sample he had given them, Baba with conventional techniques and Matsuo with a method he had developed himself. Matsuo's method, based on making the last amino acid in the chain radioactive and then chopping it off for identification, turned out to be the key. It was also Matsuo who reminded Schally to check for tryptophan.

Schally's Most Joyous Moment

By March 1971, Schally was reasonably sure that he had the structure. But he felt he needed to do further tests. Meanwhile the next scientific meeting of the specialty was due to take place in New York in May. Knowing four other teams were in the race, those of Guillemin, Folkers, McCann, and Harris, Schally feared one of them might announce first. He sent his physiologist, Akira Arimura, to the conference with instructions to declare the structure of LRF if and only if one of the other groups did so. None did, but Arimura himself gave a paper reporting the results of experiments with synthetic LRF, making plain that he knew what the structure was. He was barraged by complaints from the audience when, choosing to obey Schally rather than scientific tradition, he declined to share his knowledge.

The specialty's next big meeting took place in San Francisco a few weeks later. It was here that Schally chose to show his hand. The program had Guillemin's group speaking first. Schally's moment is best described in his own words:† ". . . the large Imperial Ballroom of the San Francisco Hilton . . . was very crowded. I listened with tension and suspense during the immediately preceding lecture and discussions, but Guillemin's group did not report the isolation and structure of ovine LH-RH [LRF]. When my time to speak came, I rose to my feet and presented our paper on isolation, structure and synthesis of porcine LH-RH. It was one of the most joyous moments of my life. . . ."

By a clear margin Schally had beaten Guillemin before an audience of their peers. "That must have been perhaps the worst thing that ever happened to Guillemin," says Bowers. However disappointed he may have been, Guillemin by all accounts bore the defeat with dignity. As chairman of the session, which he happened to be, he rose to the occasion by congratulating Schally warmly on his work.

Schally had been right to fear that Guillemin might be first. Within 2 months of the San Francisco conference Burgus had teased the structure of sheep LRF out of his minute 40-microgram sample. Knowledge of Schally's result certainly helped in coming up with a structure so promptly, for both pig and sheep LRF turned out to be the identical, ten-amino acid chain, with the same pyroglutamate beginning as TRF:

pyroGlu-His-Trp-Ser-Tyr-Gly-Leu-
Arg-Pro-GlyNH_2.

Discovery of the structure of LRF "raised our victory by 100 miles, it blasted him out of the water," says Schally. It also made Schally a player in a larger game. LRF is a substance of considerable potential interest to physicians and pharmaceutical houses. By synthesizing chemical analogs of LRF—LRF molecules with one or more amino acid units changed—it may be possible to produce a once-a-month contraceptive with minimal side effects. Since LRF also supports the reproductive function in men, there are equal hopes of producing a male contraceptive. Control of fertility through LRF could be important in animal husbandry. LRF also seems to exert a direct effect on the brain itself—quite apart from its action on the pituitary—in influencing mating behavior, a finding which may lead to an aphrodisiac of therapeutic significance. Such projects, though yet to materialize, indicate the range of practical possibilities created by the discovery of the structure of LRF.

The Pig Blood Booby Trap

LRF was the obvious target once TRF had been isolated, but after LRF, the object of the next lap of the race was not so evident. In a sense the third milestone was reached before either team knew what race was being run. The milestone concerned the means through which the brain directs the pituitary to produce growth hormone, the substance which in adolescence controls the body's growth and development. By analogy with CRF, TRF, and LRF, it was generally assumed that the hypothalamus controls growth hormone production with a growth hormone–releasing factor (GRF). Pursuit of GRF was a particular concern of Schally's in 1968, when Guillemin was making strides with TRF. As Schally puts it, "Because of our deep involvement [with GRF], I almost missed solving the TRH problem." Schally returned to GRF and in 1971 he published its structure, a chain of ten amino acids.

Schally had had the misfortune of falling, as Guillemin did not scruple to put it, into a booby trap. Revelation of his discomfiture came about when he asked chemists at Merck Sharp & Dohme to prepare synthetic GRF according to his structure. The Merck chemists obliged, but let him read first in print a discovery of their own: Schally's GRF structure closely resembled part of the beta chain of pig hemoglobin. Evidently blood in the pig hypothalami had been degraded by enzymes in the tissues, despite special precautions taken against this very possibility. The segment cleaved from the hemoglobin chain happened to give a false positive result in the GRF assay, and it was this segment that Schally had sequenced. It was "an incorrect claim," Schally concedes. "Guillemin attacked us viciously but he failed to point out that we published an honest finding and we corrected our own mistake. We were just too keen, I was just obsessed by it."

McCann Proved Right After All

The real GRF, though still considered to exist, has not yet been isolated. But there turned out to be another side to the story. McCann, who had been first to divine the existence of LRF, and his colleague L. Krulich announced in 1968 that the hypothalamus can specifically *inhibit* the pituitary's release of growth hormone, presumably through the agency of a growth hormone–inhibiting factor (GIF).

Both Schally and Guillemin were skeptical of the hypothesis; neither thought it worth committing his resources to a search for McCann's putative GIF. There the matter rested until the fall of 1971 when Wylie Vale and Paul Brazeau, two physiologists in the Guillemin group, started to look again for GRF. But the results seemed reminiscent of the McCann and Krulich data. Despite some skepticism they and Burgus went ahead with an attempt to isolate an inhibitory factor.

Whether because the assay and chemical techniques had been so well perfected by that time, the search for GIF was quite unlike the years of painstaking labor spent in pursuit of the earlier factors. Plentiful amounts of GIF were found in the old side fractions left over from the TRF and LRF chases. Within a few months, the substance was identified, isolated, and sequenced. The molecule is a Q-shaped chain of 14 amino acid units. Unlike the other factors, GIF has been given a new name—somatostatin—which indicates its function in staying the growth of the body.

Just as discovery of LRF made Schally's team in particular known in wider circles, so somatostatin helped put the Guillemin team on the map for physicians and other physiologists. By one of nature's curious economies, somatostatin turns out to be made not only in the hypothalamus but also in the pancreas. Since its function there is to control the two blood sugar regulating hormones, insulin and glucagon, there are prospects of using somatostatin to treat diabetics. One of the first practical fruits of the new technique of gene splicing may be that of programming bacteria to produce somatostatin in pharmaceutical quantities. A DNA sequence constituting a gene for somatostatin has already been synthesized and is ready for cloning. Schally delayed till 1976 the solving of the structure of pig somatostatin. "We were," he has written, "somewhat disappointed that the structure of porcine somatostatin was identical with the ovine hormone"—NICHOLAS WADE

Next week: The nature of the competition

NATO Science Committee: Redefining Mutual Security

Brussels. The North Atlantic Treaty Organization's science committee is the alliance's principal effort at nonmilitary cooperation. As such, the committee has been cast rather in the role of odd man in at NATO. However, at a mid-April conference in Brussels commemorating its 20th anniversary, the committee could claim not only to have survived, but to have served as one of the sturdier props of an Atlantic scientific community. Despite a relatively static budget—about $10 million a year—and the inroads of inflation, the NATO civil science program has weathered the recession in science support better than most other transatlantic scientific activities.

The general theme of the conference was the impact of science on society. Papers devoted to developments in particular scientific disciplines and to discussions of broader policy implications of science and technology formed the agenda for the 2½-day conference. The meeting also served as old home week for a number of notables present at the creation of the science committee and active in it since then. From the United States, for example, came physicist Isador I. Rabi, a founding father and influential U.S. member of the committee over its two decades, and James R. Killian, Jr., who was President Eisenhower's science adviser when the science committee was launched at a summit conference in 1958.

The science committee has operated in comparative obscurity, and part of the motivation for the meeting was obviously to call attention to the committee's good works and to make a bid for better support and a broader role.

Participants included a relatively rich mix of scientists, politicians, and diplomats for such a meeting. The U.S. delegation, for example, was led by Frank Press, the President's science adviser and director of the Office of Science and Technology Policy, who in London and Brussels was seeing his counterparts for the first time on their home ground.

As a forum on the current state of science and society, the conference offered an interesting summary of perceptions of

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