The document then had to be filed with the NIH by 15 November 1976. In Thomas's case, his MUA was not filed until December 1977, more than a year later. His laboratory was not certified until 9 May 1977, when it was certified as P2. He was doing P2 experiments continuously up until December 1977 (he states that he stopped his P3 work in July 1976, when the NIH rules were issued). He did not state on his grant renewal applications to the NIH that his research involved recombinant DNA, as he should have done. And he told the Harvard Medical School biohazards committee that his grant renewal had been held up, although it hadn't been, so that the committee assumed he wasn't doing recombinant DNA work, when in fact he was.

The basic reason for the mess created by Thomas and the biohazards committee was Thomas's insistence that the committee certify his lab as a P3 lab. Since the engineers on their first inspection were uncertain if it even rated P2, the committee was reluctant to follow Thomas's wishes. But it also seems to have been pathetically unable to say no. Thomas explicitly rejected the idea of having his lab certified as P2 instead; he wanted P3 or nothing, meaning P3. Because of this deadlock, the laboratory remained uncertified and the MUA didn't get filed. (The laboratory was certified as P2 in May on behalf of one of Thomas's associates; the MUA was filed in December 1977 for incidental reasons.)

Meanwhile Thomas continued to do P2 experiments. According to the Bloch report, the biohazards committee was unaware of this fact; Thomas had told the biohazards committee that his NIH grant renewal had been held up, hoping to pressure the committee into certifying his laboratory as P3; members therefore assumed that he didn't have the money to do any recombinant DNA work. Apparently no member of the committee ever thought to ask him whether or not he was doing recombinant DNA. The committee thus contributed to the problems created by Thomas.

Both the Harvard Medical School and UCSF incidents have come to light at a time when Congress is trying to frame a law governing gene splicing research. Congress's principal interest lies not in the safety rules as such—it is happy to leave that task to the scientific judgment of the NIH committee—but in how the rules should be enforced. Since local biohazards committees are the NIH's chosen method of enforcement, their performance is of some relevance. The Harvard and UCSF incidents, however, do not seem to point to any obvious lesson. They are almost certainly exceptions rather than the general rule. In both instances a human enough misjudgment or confusion on the part of the researcher went uncorrected for some time by the local biohazards committee. No public hazard resulted. A few such errors are only to be expected during the institution of a new system. On the other hand, it is perhaps remarkable that the errors had to occur at UCSF and the Harvard Medical School.

FAOB comes of age

Anthony W. Linnane

There are three regional federations of biochemical societies, FEBS, PAABS and the most recently formed, The Federation of Asian and Oceanian Biochemists (FAOB).

FEBS (Federation of European Biochemical Societies) – the oldest of the three groupings, is clearly a great success and constitutes a focal point around which many of the scientific activities of the European biochemical community are organised. The European biochemical interest groups are drawn from long-established regional societies localised within a comparatively small geographical area. In the case of PAABS (Pan-American Association of Biochemical Societies) and FAOB the total populations in the areas are large, but many of the scientific communities are small, impoverished and geographically widely separated. Nonetheless, the success of FEBS stimulated interest in the possibility of establishing good regional communication in areas outside Europe. To this end, FAOB was founded in August, 1972, largely through the initiatives of officers of the Australian, Indian and Japanese biochemical societies. As funds were limited the first Executive of the Federation (consisting of E. C. Webb, Australia; N. R. Moudgal, India; and T. Murachi, Japan) undertook to begin a newsletter for circulation in the region and the sponsorship of regional symposia.

Despite severe problems of geographical distance between members, by the end of 1975, four small symposia had been organised, three in India and one in Japan. Largely due to the vigour of the Federation's officers, several new biochemical societies were formed in the area and the membership had grown to eight interest groups by the end of 1975.

Following such a good start, the new executive (A.W. Linnane, Australia, President; T. Murachi, Japan, Secretary; Serene Vimokesant, Thailand, Treasurer; K. Imahori, Japan, President-Elect) decided that more ambitious activities might be undertaken and that an attempt should be made to sponsor a large meeting to act as a further catalyst for the formation of new societies and to bring the biochemists of Asia and Oceania together in a large Federation meeting. Accordingly, the first FAOB was planned for October 1977 to be held in Nagoya, with some trepidation in view of the daunting financial problems. Kunio Yagi of Nagoya accepted the Presidency of the Congress and the task of bringing it to fruition. A scientific programme with some relevance to the region was planned and included two large symposia on the 'Biochemical Aspects of Nutrition' and 'The Structure and Function of Biomembranes' together with sessions devoted to short communications and posters.

An excellent programme, drawing on biochemists both from within the region and without, was organised. About 500 scientists from the region attended, including representatives from all 14 interest groups which now comprise the Federation (Australia, Bangladesh, Hawaii, Hong Kong, India, Indonesia, Japan, South Korea, New Zealand, Pakistan, Philippines, Singapore, Taiwan and Thai-

The Opening Ceremony of the FAOB 1st Congress Centre – K. Yagi. From left to right in the back row, A. W. Linnane (the President of FAOB), A. A. Bayev (the President of IUB), B. Maruo (the President of the Japanese Biochemical Society), G. Semenza (the delegate from FEBS), W. J. Whelan (the delegate from PAABS).

land). In addition to the regional symposium speakers invited guests from Europe and the Americas were active participants, along with several members of the IUB Executive Committee, which chose to meet in Nagoya, coincident with the Congress. The Japanese Biochemical Society also strongly supported the Congress and on the conclusion of the Nagoya meeting the two organisations joined together in Tokyo for the presentation of poster sessions.

Remembering that air fares for visiting scientists from the region to Japan ranged from a minimum of $300 up to about $1500 per attendant, the financial aspects of the organisation of the Congress were worrying. However, due to the generosity and strenuous efforts of Kunio Yagi and his colleagues, the financial problems of the Congress were overcome. It would be difficult to place too much emphasis on the value and importance of the Nagoya meeting in the development of the Federation and on the unique contribution of Professor Yagi.

The Council of FAOB is now confident that following recent activities, the Federation is firmly established and has a potentially bright future. Travel costs are a substantial burden to the organisation and its members, but hopefully decreasing air fares and the enthusiastic reponse of member organisations will make a significant contribution to the long term viability of the Federation.

FAOB plans to hold a Congress every three years and the next is planned for 1980 in Bangalore, India on the occasion of the 50th Anniversary of the Indian Society of Biological Chemists.

By the time this report appears a two-day meeting will have been held on April 20–22, 1978 in Singapore, with the theme 'Practical Applications of Biochemistry to the Economies of Developing Countries'. (The Secretary General of FAOB is Dr. M.G. Smith, Department of Biochemistry, University of Otago, P.O. Box 56, Dunedin, New Zealand, from whom details of FAOB activities may be obtained).

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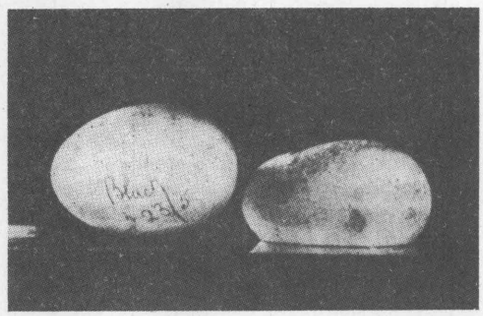
From the Murky Past

Reinhold Benesch

Avian metabolism

It was the discovery of Keilin and Mann that sulphanilamide is a specific inhibitor of carbonic anhydrase which prompted me to deprive eggs of their shells *in vivo*. Since it had been suggested earlier that this enzyme played a role in the formation of the calcium carbonate of the avian egg shell, I put two and two together and administered the drug to some rented hens in the garden of a friend of mine. The



(Reproduced with permission from Nature 1944, 153, 138)

startling result was the monstrosity on the right side of the photograph (reproduced here from *Nature*) with a normal egg by its side for comparison! Much hilarity was created by this accomplishment, both inside and outside the scientific community and the following poem published in Punch on 11 October 1944 was the most artistic example:

Science Moves On

Happy the scientific gent
Who ranging darkly and aloof
Brings years of calm experiment
To a triumphant proof
And happy he I sing today
Whose soaring brain has wrought a spell
Whereby our future hens will lay
Their eggs without a shell
A shell-less egg. The term has grown
Familiar in our war-time fare,
Though up till then 'twas all unknown
As far as I'm aware
Compact of useful vitamins
Men ship it from a far-off land
But dried, compressed and shoved in tins,
Or, as they put it, canned.

But he with his ingenious mind
Has scorned all artificial aid;
This is no dehydrated kind
But whole, and newly laid;
One trifling point I don't see yet:
Will he who has so nobly toiled
Explain exactly how to get
At the inside, when boiled?

And you, O hen, when first you gaze
Aghast on this unwanted sight
Forbear to squawl your wild amaze,
Believe me, you're all right.
And when again that broody fit
Compels a long retirement, then
Unless I'm wrong, you ought to sit
More comfortably, hen.

Dum-Dum (Major John Kendall)

(Reproduced by permission of Punch)

Other repercussions of this discovery will be related in due course.

From dogfish to threshing machines

During my first year at Leeds University I was desperately trying to adapt the handicap of a Central European classical education to the rigors of a Yorkshire one in the natural sciences. One of the prescribed hurdles was a systematic course in General Zoology and this, like all science courses, had to be passed not only with a written, but also with a practical examination. I will never forget the final, where one of the assigned tasks was the dissection of the third cranial nerve of the dogfish. In this unattractive remnant of evolution these nerves are firmly embedded in brittle, inelastic cartilage, so that when I tried to dissect out the required nerve fiber more and more bits of it were propelled into the surrounding atmosphere by my errant scalpel. At this point Professor Spaul, in