

# 1. The origin and early history

The two International Polar Years of 1882–3 and 1932–3, followed by the International Geophysical Year (IGY) of 1957–8, had drawn attention to the advantages of international collaboration in geophysical science and how this could often be turned to practical account of advantage to mankind. It is understandable therefore that biologists were stimulated to think of some similar international effort. In the discussions and events just after IGY, during the years 1959–64, which culminated in the actual inauguration of the IBP, three biologists took a leading part, namely Sir Rudolph Peters, who was President of the International Council for Scientific Unions (ICSU) from 1958–61, G. Montalenti and C. H. Waddington, who were successively Presidents of the International Union of Biological Sciences (IUBS). Sir Rudolph has kindly written a brief statement and Waddington has written a personal account of events as he saw them.

Montalenti has declined to write separately, being satisfied that the subject is well covered by the other contributions. For additional information, however, attention is drawn to his papers (1961 and 1966) in which he looks at the origin and early development of the programme, as well as to the account of an early discussion which is summarised in IUBS (1961). In a lighter vein, many of the elements of IBP are well expressed in Montalenti's Christmas card for 1973, as follows:

## CANTICO DELLA CREATURE

Laudato sie, mi Signore, cum tucte le tue creature,  
 spetialmente messer lo frate sole,  
 lo quale jorna, et allumini per lui;  
 et èllu è bellu e radiante cum grande splendore;  
 de Te, Altissimo, porta significatione.

Laudato si', mi Signore, per sora luna e le stelle;  
 in celo l'ài formate clarite et pretiose et belle.

Laudato si', mi Signore, per frate vento,  
 et per aere et nubilo et sereno et onne tempo,  
 per le quale a le tue creature dai sustentamento.

Laudato si', mi Signore, per sor'acqua,  
 la quale è molto utile, et humele, et pretiosa et casta.

*The Evolution of IBP*

Laudato si', mi Signore, per frate focu,  
 per lo quale ennallumini la nocte,  
 et eliu è beilu, et jucundo, et robustoso et forte.

Laudato si', mi Signore, per sora nostra matre terra  
 la quale ne sustenta e governa,  
 e produce diversi fructi, con coloriti fiori et herba.

Laudato si', mi Signore, per sora nostra morte corporale,  
 da la quale nullu homo vivente po skappare.  
 Guai a quilli ke morrano ne le peccata mortali.  
 Beati quilli che se trovarà ne le tue sanctissime voluntati  
 ka la morte secunda nol farrà male.

Be praised, my Lord, with all your creatures, especially  
 master brother sun, who brings day, and you give us  
 light by him. And he is fair and radiant with a great  
 shining – he draws his meaning, most high, from you.

Be praised, my Lord, for sister moon and the stars,  
 in heaven you have made them clear and precious and lovely.

Be praised, my Lord, for brother wind and for the air,  
 cloudy and fair and in all weathers – by which you give  
 sustenance to your creatures.

Be praised, my Lord for sister water, who is very  
 useful and humble and rare and chaste.

Be praised, my Lord, for brother fire, by whom you  
 illuminate the night, and he is comely and joyful and  
 vigorous and strong.

Be praised, my Lord, for sister our mother earth, who  
 maintains and governs us and puts forth different fruits  
 with coloured flowers and grass.

Be praised, my Lord, for sister our bodily death, from  
 which no living man can escape; woe to those who die in  
 mortal sin; blessed are those whom it will find living  
 by your most holy wishes, for the second death will do  
 them no harm.

*Francesco d'Assisi (1182–1226)*

(Translated by G. Kay)

*The origin and early history*

**Statement**

*by Sir Rudolph Peters*

*20 November 1973*

My first activity as President of the International Council of Scientific Unions, was to act as Chairman of an ICSU Bureau meeting which was held in Gonville and Caius College, Cambridge, in March 1959, when F. J. M. Stratton, who had in effect organised ICSU from his college rooms for a number of years previously, was host. An excellent dinner was given to us by the Master of Caius, Neville Mott.

After this meeting, in the train to London, I fell to discussing with the Past President, Lloyd Berkner, and Guiseppe Montalenti, the possibility of having a project in biology similar to the IGY. At first I thought that this might be in the nucleic acid field; but on consulting A. Todd in Cambridge and L. Beadle, then in Oxford, it seemed to be clear that the nucleic acid field was then fully stretched. Nevertheless, I went on with the general idea and on my way to Italy in the spring of 1959 I discussed it with R. Fraser, Secretary of ICSU at our Office in the Hague. I then went on to Naples to see Montalenti, then President of the International Union of Biological Sciences. He was enthusiastic about the idea, and thought that it was particularly important to make observations upon human populations which were more or less still isolated on islands, in valleys, etc., before civilisation stirred these up beyond genetic recognition. Montalenti always supported this and it became part of the final programme.

The idea of a biological programme spread and was discussed at the Lisbon meeting of the Executive Board, at which I was Chairman and at which Montalenti gave a lecture. The rest is well described by Waddington. The narrow field did not appeal to those from the USSR who rightly wished it to be extended to human welfare. At the same time they were extremely critical for at least a year, though they never actually banged the door shut. The position was so bad at one period that I made up my mind that we should proceed with the project with or without the USSR. Fortunately they decided eventually to take part and it became an important part. I think that IBP would have failed without the help of Waddington. It did accomplish what I had hoped – a synthesis of the isolated research efforts of biologists in many fields and even the setting up of new stations for work.

## *The Evolution of IBP*

### **The Origin**

*C. H. Waddington*

I first came into any close contact with the IBP at the General Assembly of the IUBS held in Amsterdam in July 1961. When I was sent by the Royal Society as one of the British delegates to this Assembly, I knew very little about the IUBS or its work. It was a great surprise to me when towards the end of the meeting I was approached by Paul Weiss on behalf of the Nomination Committee and asked to allow my name to be put forward as the next President of the Union. I was rather reluctant to accept this, but eventually did so. One of the reasons why I was willing to contemplate taking on the job was that my interest had been aroused in the potentialities implied in the proposal to start an International Biological Programme.

The first suggestion that an International Programme on Biology should be launched seems to have originated with Sir Rudolph Peters in the late fifties, when he was President of ICSU. He communicated the idea to G. Montalenti, the then President of IUBS. Montalenti drew up the first at all formal scheme for an IBP, which he presented to the Executive Committee of ICSU at its meeting in Lisbon in 1960. ICSU appointed a preparatory committee, with Montalenti as Convener and containing Engelhardt (USSR), Florkin (Belgium), President of the International Union of Biochemistry, MacIntosh or Lindor Brown (UK), representing the International Union of Physiological Sciences, L. C. Dunn or G. L. Stebbins (USA), the latter being Secretary-General of IUBS, and C. Pantin (UK), zoologist. This preparatory committee, with some co-opted people, held its first meeting in Cambridge, England, in March 1961, and drew up a document about possible subjects to be dealt with by the IBP. They agreed that they would put forward a small number of rather definite projects in fairly restricted areas. Three such areas were proposed: (1) human heredity (Montalenti); (2) plant genetics and breeding (Stebbins); and (3) studies of natural biological communities which are liable to undergo modification or destruction (Baer). The preparatory committee held another meeting in Paris in May 1961; following this, memoranda on the three topics mentioned above were presented to the IUBS Assembly in Amsterdam later that year.

Engelhardt had not been able to attend either the Cambridge meeting in March or the Paris meeting in May of the preparatory committee, but he had been present as a vice-president of ICSU at the Executive Committee meeting of that body in Lisbon. Another Soviet biologist, Kursanov, was a member of the Executive Committee of IUBS and had

*The origin and early history*

been present at its meeting in 1960 at Neuchatel. Academicians Engelhardt and Kursanov submitted to the Amsterdam IUBS Assembly a document in which they pointed out that the ICSU Executive at Lisbon had agreed that the title of the IBP should be 'The biological basis of man's welfare'. They argued strongly that the basic social importance of fundamental biology is in its relation to food production and human health, and that therefore the programme 'should be based on investigations leading to the detection, registration and the most effective and rational utilisation of both well known already used, and new, biological resources of plant and animal origin, with the aim to raise the standard of life of mankind'.

At Amsterdam, a morning was devoted to a symposium by four invited speakers on possible topics for the IBP. I was one of those invited to contribute. From what I had learned in the past, I had formed the impression that up to that time the whole enterprise was extremely dubious. There was talk of organising something on a large worldwide scale, comparable to the IGY, but the people putting forward the ideas seemed to have no firm grasp of how it should be financed or organised. Moreover, each sponsor seemed to think that the programme should essentially be devoted to his own speciality – Montalenti to human genetics, Stebbins to plant genetic resources and Baer to nature conservation. My private opinion was that it would probably be most satisfactory to kill the whole thing before it went any further, if this could still be done. However, it seemed rather likely that matters had already gone too far for this to be practical. If, on the other hand, it were to proceed, I felt that the only possible line would be to formulate a programme around something which was indubitably of major social and economic importance for mankind as a whole.

It seemed pretty obvious that the kind of biology in which I was professionally engaged – genetics, epigenetics and so on – did not need, and was not suitable for, any internationally organised co-operative attack of the kind contemplated in an IBP. I therefore felt I had no personal interests at stake, and could attempt to form a judgement from a relatively neutral point of view as to what was the most important contribution biology could make to man. The most attractive field, I thought, was something to do with the way in which solar energy is processed by the biological world into the formation of complex molecules which man can use, as food or otherwise. I gave my contribution to the Amsterdam symposium along these lines. Kursanov took a very similar line, but he, being a professional student of photosynthesis,

### *The Evolution of IBP*

laid most of his emphasis on studies of different photosynthetic mechanisms. However, he tied these up with the study of general biological productivity. The other speakers certainly did not lay their major emphasis on productivity, but on various other somewhat narrower aspects – for instance, conservation or human heredity.

The Amsterdam Assembly finally passed the resolution that an IBP should be set up and that its main aim should be ‘toward the betterment of mankind’. It mentioned three specific areas for action: conservation, human genetics, and improvements in the use of natural resources. A further meeting of a planning committee was arranged to take place early in 1962.

Finding myself as President of IUBS, which was the main union concerned with IBP, I now felt that I had to take a really serious look at what IBP was supposed to be about. In January 1962 I had ready, and circulated to the rest of the Planning Committee, a document under the title ‘Notes on the selection of topics for an International Biological Programme’. I suggested that there were three main topics that might be seriously considered as a possible central theme: (1) *Human genetics*. The trouble with this is that although it is easy to deal with characters of relatively minor importance, such as variants and blood groups, the really important subjects (e.g. racial differences, differences in intellectual and other abilities) are both politically highly controversial, and also, at present, lack an adequate scientific understanding. (2) *Human population growth*. I argued that this was unsuitable, both because the facts are already fairly well known and are under intense study by many bodies with far greater financial resources than IUBS (e.g. the Ford Foundation), and again because the implications are the subject of political controversy. This left (3) *Man and ecology*, that is to say the way in which man can modify the natural environment so that it produces with maximum efficiency on a long-term basis the kind of products he can use.

This document formed the starting point for the discussions of the next meeting of the Planning Committee, which was held in Morges, Switzerland, in May 1962. Our host at this meeting was Jean Baer, President of the International Union for the Conservation of Nature (IUCN) as well as Chairman of the Division of Zoology of IUBS. We were meeting, therefore, in a rather ‘conservation-orientated’ atmosphere. Moreover, there were several other IUCN members present at the meeting (H. Ellenberg, Max Nicholson, and Ed Graham). I found myself in the position of being almost the sole sponsor of the idea that ecology

*The origin and early history*

should be looked at as a matter of energy through-put and processing. Kursanov, who would have been entirely on my side, was not able to attend and was replaced by Steinberg of Leningrad, a very nice general zoologist who, however, was not very clued up about production ecology. The main man who really knew about this, in one particular field was Sidney Holt of FAO, a fisheries expert, but his main point was that fishery biology was already very largely internationalised and did not really need much more international organisation – and at any rate, if it did, this should fall to the province of FAO and not of IUBS.

The difficulties of that meeting were increased by the fact that Ledyard Stebbins, the General Secretary of IUBS, had circulated a questionnaire, based partly on my memorandum and partly on his own ideas, to a few hundred American biologists. Now, general ecology was certainly not one of the strong points of American biology at this time. Such terrestrial ecologists as there were, were either ‘wildlife and forestry’ oriented, or had particular research projects of their own which took up their full energies (e.g. the Odums). The replies to Stebbins’s questionnaire were, therefore, somewhat discouraging, and to my mind almost totally irrelevant. It was clear to me that if we were to organise an IBP concerned with the energetics of different ecosystems we had got to start almost from scratch in insisting that this was indeed the subject we felt ought to be studied. I had come across much of the same attitude in Britain at meetings of the Royal Society committee dealing with the IBP. The general idea amongst biologists at large seemed to be that ecology dealt with a blow-by-blow account of a day in the life of a cockroach, woodlouse or sparrow; and the notion that it could study such questions as what does the ecosystem do with the incident solar energy, tended to be greeted with blank stares of incomprehension – whatever was one talking about?

The Committee finally came up with a structure for the organisation of IBP which is essentially what has been preserved throughout the programme, namely, a set of seven sections or sub-committees: three on biological productivity in terrestrial communities (of these three, one on general productivity, a second on the metabolic processes – mainly biosynthesis and nitrogen fixation – on which this productivity depends, and a third on the conservation of threatened communities); then a fourth on productivity in fresh water; a fifth on productivity in marine communities; a sixth on human adaptability (physiological and genetic); and a seventh on public relations and training. Stebbins was made Convener of the seventh, but in the event each of the more specific



### *The Evolution of IBP*

sections had to look after its own public relations and training programmes. The seventh section therefore became devoted to more practical problems and was referred to as 'Use and Management'; it aimed to deal with those aspects of applied biology which in one way or another fall just outside the fields of FAO and WHO.

The whole scheme had, of course, still to be sold to the biological world. It was right up the same street as the Russians had suggested, and I suppose took little selling to them, but they hold themselves so much aloof from the rest of the world that it was quite a long time before one knew whether they were playing or not. France and the French-speaking world in general was pretty forthcoming, particularly through the co-operation of F. Bourlière in the field of terrestrial ecology. Sweden with W. Rodhe in freshwater biology, soon followed by Italy with L. Tonolli, also quickly took its place. On the marine side, R. Glover of Edinburgh and several Scandinavians soon picked up the ball.

The really difficult biological communities were those in Britain and particularly those in America, which had very strong traditions of the dominance of physiology, biochemistry and molecular biology. In Britain the dominant medico-physiological establishment, who hold more or less *ex officio* the main official positions in the Royal Society, were sceptical and indifferent, though not definitely antagonistic. The intellectual leaders in molecular biology and genetics were just simply uninterested, but again not really effectively hostile. The only strong British group in the ecological-natural history field was centred round the Nature Conservancy, which had its own sub-committee (the third) in the programme and which was even prepared to think they might in future take productivity more seriously than they had done in the past. Meeting the Royal Society IBP Committee in its early days, I often felt that I was being called upon to invent a new discipline of production ecology off the cuff; but the Royal Society officials – the Executive Secretary, David Martin, and his assistant, Ronald Keay, whose professional fields were science policy – immediately saw the socio-economic importance of IBP and gave it every assistance.

The toughest biological community into which to launch the scheme was that of the United States. The American biological world was not dominated – establishmentwise – by medical physiologists and biochemists to the same extent as the British; but the analytical school of molecular biologists and microbiological geneticists had a far higher status than in Britain and much less hesitation in asserting, in the hearing of government or the academy, that any organism bigger than *E. coli*



*The origin and early history*

serves only to confuse the issue. Moreover, there were schools or personalities in field biology which ranked much higher in the pecking order than any comparable groups in Britain, for instance Dobzhansky and Mayr in evolutionary biology in the field, and even one or two ecologists. Finally, practical ecology in the United States was famous for two ‘achievements’ – if that is the right word – at the opposite ends of the spectrum: the view that there is enough land to justify simply mining it – leading to the dust bowls of the thirties; and by contrast the development of an extremely elaborate wildlife and National Parks system, which (fairly successfully) aims to produce unspoilt wilderness, rivers full of salmon or forests full of deer within reasonable vacationing distance of the main centres of population. The idea of studying the energy balance of ecosystems had therefore got to thread its way between the adherents of the ‘central dogma’, who couldn’t care less but were apprehensive it might take away some of their public funds, and an opposite party whose line was that ‘we *are* field biology, and productivity is not an American problem’.

I had a particularly ‘close huddle’ with the Americans in May 1963. I had taken myself across to Washington, under no particular official auspices, but with some financial help from Edinburgh University – Sir Edward Appleton, the Principal, didn’t quite like giving it to me, but eventually did so. My basic purpose was to interview people in the US science policy establishment in Washington, to find out how their system worked in order to use this knowledge in connection with the Trend Committee’s restructuring of British civil science – but this is another story. I also took the chance to see some of the important US biologists concerned with IBP. T. C. Byerly, of the US Department of Agriculture, and head of the Division of Biology in the National Academy, fixed up an interview at which he and about a dozen others were present. Byerly was already fairly favourable, but he was playing it very cool indeed; and many of the others started pretty sceptical about what these Europeans were up to. I had both to convince them that production ecology could be good science, and to pull out all the policy stops, about the starving world, etc. However, Byerly told me some time afterwards that this was one of the key meetings which began to bring the Americans round to the idea that they should play a part. In the early stages, the most helpful American was Edward Graham, who had attended the Rome meeting as a representative of IUCN. But he was a rather mild person, and also, unfortunately, he died before the programme had got very far. He was succeeded by Stanley Cain, also from the

### *The Evolution of IBP*

wildlife and conservation field. Later the Americans put in charge someone with the reputation of a real thruster, Roger Revelle.

I found the initial hostility in the senior American biologists sufficiently disturbing to write a personal letter to a number of my more influential friends such as Muller, Sonneborn, Sewell Wright, Mayr, Dobzhansky, and Ebert. Ebert's reply (3 June 1963) was typical: '... at the AIBS meeting last August, most ... of the proposals were for warmed-over ecology of the thirties and forties on a worldwide scale ... I think my statement (made to a group made up to a large extent of conventional systematists and ecologists) had excellent shock value. I believe strongly in a well planned international program ... which is truly international in character, and which attempts to use the international aspect of the endeavour as a powerful tool, rather than just a gimmick to keep up with the physicists. You can count on my assistance'.

It was not till about 1966 that the real importance of IBP got across to the Americans. In a Plenary Session of the National Research Council, 13 March 1967 (see *Proc. Nat. Acad. Sci.*, 1968, **60**, 1–50), a leading American ecologist, Frederick E. Smith, gave the introductory speech in which he spoke of IBP as 'lifting a minor subject to a position of major status'. Again, 'The participation of many ecologists in IBP ... will have a profound effect on the orientation of the profession ... This change of orientation has already begun ... Within the last year, the sudden support of IBP by a large number of ecologists, after strong initial resistance, has been surprising. The ferment that took place at Williamstown last October had all the excitement of a "happening".' As things subsequently turned out, these manoeuvres paid off handsomely, for the ecological contribution to IBP which the United States made later on exceeded that of any other country and did much to change the face of environmental science.

The other thing I set about doing, early in 1963, was to find suitable staff, and in particular a suitable senior executive. I clearly couldn't and didn't want to run it myself – I had already spent more time on it than I would have liked. Montalenti also could be no more than a President – he hadn't the spare time, or the knowledge or real understanding of the aims of the productivity approach, to guide it when it began to get down to detail. I was not much impressed with the disinterestedness of most of the ecologists who had been associated with the early stages, nearly all of whom were so closely connected with particular lines of work that they had axes to grind. What we needed, I thought, was someone who had considered experience in ecology, and in