PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY B BIOLOGICAL SCIENCES

General Discussion

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General discussion

E. B. Worthington (*Colin Godmans, Furners Green, Uckfield, E. Sussex*)

On the subject of communications and meetings Dr Worthington outlined the international arrangements during the decade of the I.B.P. Early technical meetings were concerned with methodology and planning research programmes. About the middle of the programme a number of regional symposia were arranged so that the maximum number of field workers could meet each other and discuss progress without undue expense – those by P.F. in Latin America, Africa and the Far East were particularly successful, and also those arranged by H.A. Towards the end of the programme to aid in the synthesis process, some bigger international symposia for the presentation of results were organized as well as a large number of small workshops and editorial meetings; the latter are still continuing. It is particularly noteworthy how rapidly the methodology of production ecology has changed during the decade, particularly with the introduction of systems analysis and mathematical modelling. As a result some of the early Handbooks, sound as they were, have been somewhat outdated, and perhaps we should have done more to have them revised. The principle which S.C.I.B.P. adopted of not attempting to standardize methods but to encourage their evolution and improvement certainly proved sound.

In efforts to establish uniformity in the use of quantities, units and symbols, the SI system, though a useful beginning, was quite inadequate for biological data. An intersectional committee appointed at the first general assembly failed to get far with the project and it was not until the synthesis phase that a small working group convened by E. D. Le Cren dealt with this subject thoroughly: their report has been widely circulated through the agency of U.N.E.S.C.O. as well as I.B.P. and in its revised form appears as an appendix in the introductory volume to the international synthesis series published by the Cambridge University Press.

Professor G. E. Fogg (*The Marine Science Laboratories, Menai Bridge, Anglesey, Gwynedd*)

I should first of all like to emphasize a point that has already been made by several speakers, that is, the great value of the I.B.P. handbooks on methods. As a university teacher, I find that I am constantly referring both undergraduate and postgraduate students to those on primary productivity and the study of benthos. My other point is that I wonder whether I.B.P. has not suffered somewhat from too rigid vertical division into sections. There are several instances where horizontal links might have been valuable. One, collaboration between P.P. and P.M. in study of nitrogen fixation in the sea, I have already mentioned. Another was referred to by Professor Pirie; U.M. might have received help from P.F. in investigation of water hyacinth and other aquatic weeds as sources of leaf protein. A third topic germane to the I.B.P. theme but falling between two sections is that of the role of bacteria in energy flow in planktonic communities. At an I.B.P. symposium on primary productivity at Pallanza in 1965, which was attended by both freshwater and marine biologists, no fewer than four papers indicated that this might be appreciable but neither P.F. nor P.M. followed the matter up. The studies on beach ecosystems by the Aberdeen group have, in fact, shown the great importance of bacteria in converting dissolved organic matter into cell material available for invertebrate food.

Probably bacteria are of equal importance in some freshwater and marine planktonic communities.

C. H. Waddington F.R.S. (*Department of Genetics, University of Edinburgh*)

I should like to make some general remarks about the I.B.P., taking up the two questions put to us by the Chairman of this series of meetings, Professor Clapham. 'Did the I.B.P. fulfil the expectations of those who began it?', he asked, and again 'Was the preliminary planning adequate?' I feel I can comment on the I.B.P. and offer my personal replies to those two questions from a point which is somewhat outside it; although I have been in pretty close contact with the I.B.P. throughout its existence, I have never been wholly within it. Perhaps I was closest to it at its beginning, or rather in the period just before it was born, and I seem to be getting close to it again now, as it reaches its termination and transmutation into other activities. But I have never worked for an I.B.P. project, since they are not the kind of biology in which I have specialized.

I first came in contact with the I.B.P. when I suddenly, and to my surprise, found myself more or less shanghaied into becoming the President of I.U.B.S. This was at the General Assembly at Amsterdam in 1961. At that meeting it had been fairly well, but perhaps not completely, decided that I.U.B.S. would launch something like an I.B.P., but it was still very doubtful what its subject should be. The first major task that fell on my plate as President of I.U.B.S. was to organize meetings at which this question could be further discussed, a basic field decided on and the first steps put in hand to bring it into reality, if possible with the help of other like-minded organizations.

At this stage only very few biologists were involved. They were essentially the Executive Committee of I.U.B.S., with representatives of what were then only three divisions, Botany, Zoology, General Biology, with a few extra people who had been brought in at still earlier stages of the proposal. Ideas for the topic of an I.B.P. ranged from well-defined single themes, such as human genetics with particular reference to primitive communities whose isolation was being broken down by modern means of transport, or the plant genetics of species useful to man, to much wider subjects. It was always envisaged that an I.B.P. should be on some subject which indubitably contributed to the welfare of mankind. Eventually, as you know, we settled on the very general theme of biological productivity, which is the basis from which the human species derives the whole of its food, and many other materials, such as wool, cotton and so on; and we coupled this with a related topic which was christened 'human adaptability'. From the very beginning it was hoped that these two general themes could be dealt with in a way which would bring out the links between them: that man in all the different parts of the world is actually a member of the general productive ecosystem, in the midst of which he is placed.

I think it is necessary to emphasize the extent to which the initial formulation of the plans for the I.B.P. were not only very theoretical, but very unorthodox theory at that. Very few indeed of the original group of biologists who agreed to try to launch the I.B.P. were themselves ecologists, or concerned with human genetics in a way which linked it with ecology. Even when the first contacts had been made with larger groups of biologists in these two fields, the basic outlook proposed for the I.B.P. was not quite what they would have expected. Very few people indeed took productivity as the central theme in ecology, or approached human biology with an ecological outlook. The main group of ecologists who saw their subject from this standpoint were those concerned with marine ecology and fisheries. Circumstances had already compelled the organization of very active and quite successful international activities in this field, and although many marine ecologists were very helpful in reinforcing I.B.P.'s argument that productivity is a sensible standpoint from which to approach the study of ecology, they did not feel in any great need of such assistance as the I.B.P. might be able to offer them in their own field. Some of the few ecologists particularly those concerned with terrestrial freshwater communities, who had been treating these from the point of view of productivity had successfully carved out adequate work niches for themselves, and had not much time to spare to spread the doctrine more widely in an international effort. However, there were some, and fortunately a fairly rapidly increasing number, who could see the benefits of international cooperation and expansion of effort. The early days of the I.B.P. were by no means easy, and it was not greeted with open arms even by the whole of the small and scattered band of ecologists and human biologists who then existed.

For these reasons the first and primary aim of the people who actually established the I.B.P. as an official organization, and persuaded I.C.S.U. to set up a Special Committee for the subject, was very general, though whether you would call it modest or not is perhaps a matter of definition. The prime aim was to bring it about that ecology, considered in terms of energy flow and production, and human biology, considered as something in which man is linked to the rest of the biosphere, should be recognized as major and substantial parts of biology. In my opinion the I.B.P. has been very considerably successful in achieving this. For instance, in I.U.B.S. ecological sciences now have the status of a Division, of equivalent rank with Zoology, Botany, Functional and Analytical Biology and Microbiology. Professor Clapham quoted John Kendrew as saying that when the I.B.P. started, ecology was a word known only to a few obscure specialists. This may be something of an exaggeration, but I think one could agree with the other authority he mentioned, an American biologist who said that the existence of I.B.P. had converted ecology from an obscure and neglected branch of biology into a major branch.

I think our main failure in this connection has been due to the collapse of what was to have been the seventh section of the I.B.P. In the initial plans this was not to be 'use and management' (U.M.) as it later became; it was to have been training and public relations. We well recognize that there were far too few trained ecologists in the world to deal with all the tasks we should have liked the I.B.P. to tackle. In particular there was a great lack of ecologists in the developing countries, who we felt had the greatest need for further understanding of their natural ecosystems, as a basis for developing their productivity in ways useful to their human populations. We had hoped that the I.B.P. could mount a large effort of training young ecologists, particularly from developing countries, either by a system of Fellowships or by conducting schools within certain countries or regions. We knew also that this would be impossible unless very considerable funds could be found, and that this fund-raising would only be successful if the message of the I.B.P. and its aims and potential uses could be made widely known, both to the general public and to governmental and official bodies. Unfortunately, for a variety of reasons, Section 7, which we hoped would tackle both these interlocking tasks, failed to get off the ground. It has been one of the most disappointing features of the I.B.P. that there are still far too few trained ecologists, particularly in the developing countries who need them most.

What I have said should have made it clear that the initial planning could not in the nature of the case ever have been adequate. There was, at the beginning, insufficient participation of

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knowledgeable and experienced ecologists to make plans in great detail. It was always envisaged that the discipline of production ecology would grow hand-in-hand with the entry of more people and more diverse interests into the activities of the I.B.P. The plans for the I.B.P. projects had to grow gradually rather than being laid down rigidly from the beginning.

One of the most definite parts of the early planning was the organization of meetings for the discussion of methodology. As has been pointed out by others at this meeting, it would have been undesirable to standardize the methodology too strictly. The subject was advancing too rapidly. The fact that some of the early handbooks already seem almost quaintly old-fashioned, should not in my opinion be counted on the negative side against I.B.P.: they are, rather, evidence of the success I.B.P. has had in stimulating the growth of ecology. If the I.B.P. had not existed the old methods would probably not have gone out of date, in fact they might not even have reached the stage of being definitely formulated. Of course, the I.B.P. can claim no credit for the fact that now computers exist and can be used to assist formulation of ecological models, but I doubt whether many such models would in fact have been made if the I.B.P. had not been operating to provide the data to keep the computers busy.

One must remember also that a number of plans made in the beginning, on some of which quite a large amount of preliminary work was put in, eventually failed to materialize, often for political reasons. For instance, in the early days there was a great deal of work, including several international planning symposia, on a projected study of several species of large herbivores other than those commonly used as domestic animals. One of the main centres of this study was to have been in Uganda, where an effective substitute for the cow is badly needed. However, the political events in that unfortunate country forced the abandonment of this project. Again, in the C.T. section a great deal of preliminary work was done in connection with a site for both conservation and terrestrial studies at Azrak in Jordan. Again political events rendered this impossible. On the other hand it was the relative flexibility and generality of the early plans which made possible some of the fortunate development which did occur, such as the cooperation with Japan as well as with local biologists in the study of Malaysian rain forests, and cooperation with various national or regional bodies in much of the H.A. programme.

For all these reasons my own answer to Professor Clapham's question 'Was the early planning adequate?', would, I think be, 'No, thank goodness'.

I wonder how much more precise the planning will be for I.B.P.s successors – M.A.B., S.C.O.P.E. and U.N.E.P.; and how in a few years time we shall answer the question about them: 'Did they underplan or overplan, or have about the right degree of flexibility?'

E. B. Worthington

E. B. Worthington, as an international observer, expressed appreciation for the meeting as a contribution to the overall assessment of the value of the I.B.P. to the future of science and of human welfare. Some countries, including the U.S.S.R., had already held similar meetings and others planned to do so. He drew attention to the very important part taken by British scientists in the early planning stages of the programme, and also in the last phase of international synthesis, although in the operational phase the U.K. activities might have seemed to be somewhat eclipsed by more massive programmes such as those of the U.S.A. and U.S.S.R.

Much had been said of the achievements of the I.B.P., and some criticisms expressed where

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there had been failures or disappointments. As lessons for the future Dr Worthington referred to four points:

(i) Developing countries had in a number of cases made good plans for their own contributions, but with few exceptions these never got off the ground, except for joint projects with developed countries. The reason was clear – that nearly all science in developing countries is organized by government, and the funding for a non-government programme could not be secured. He hoped that this problem would be solved by the inter-governmental M.A.B.

(ii) Integration between the sections had often been disappointing for there were so many disciplines involved each talking their own scientific language. In particular the integration between plant and animal ecology and the human sciences, and even between aquatic and terrestrial ecology had not been adequately achieved – although there had been certain successful examples.

(iii) Few international data-banks had been established although some national centres had taken over international functions in the storage, retrieval and processing of data. The lack of data-banks to provide a continuing service had been a disappointment; but it might be as well in the long run, because the evolution of methodology was reshaping the form in which they could best be established.

(iv) Public relations and training aspects of the programme, originally intended as a separate section but in the event taken over by the specialized sections, had been inadequate throughout, although in this again there had been noteworthy exceptions.

R. S. Clymo (*Botany Department, Westfield College, London NW3 7ST*)

Other speakers have pointed out that the coordination of work (in the P.T. and P.F. sections at least) was loose and by consent. One consequence is that during the synthesis phase of the I.B.P. there has been inevitable pressure to combine estimates which have widely varying precision and accuracy. If the random error (precision) of components is known – as it often is – then the overall precision can be calculated in the usual way by weighting standard errors in proportion to their partial differentials. But it is rarer for there to be any estimate of systematic errors (accuracy) such as may be got for example by using independent methods.

If estimates of productivity made by complex combinations of measurements differ by an order of magnitude we may perhaps believe there is a real difference. It would be rash though to conclude anything if they differ by as little as a factor of two, even if the difference is significant in the statistical sense, because such tests are of the random error component alone.

In general, we should be more critical of such syntheses than we would be of the components.

P. J. Newbould (*New University of Ulster, Coleraine, N. Ireland*)

The most intractable component in primary production measurements is the estimation of root production. This was foreseen at the outset but was never fully tackled or solved. It now represents a major source of error in all the primary production measurements of the P.T. section and invalidates a number of between-site and between-biome comparisons. The reason for this is that the I.B.P. depended on suggestions coming forward from people and had no machinery for commissioning laborious, tedious, unrewarding research which no one volunteered to do.

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M. W. Holdgate (*Institute of Terrestrial Ecology, Natural Environment Research Council*)

This meeting has been about the United Kingdom contribution to the I.B.P. In our concluding discussion I hope we will not, however, be narrowly national. It is legitimate to ask 'did the I.B.P. benefit biological science in Britain?', but also to ask 'did what the U.K. did in the I.B.P. benefit biological science in the world?'

In these remarks, I shall address myself to three questions which are not very different from those posed by Professor Clapham at the start of the meeting:

(i) Did the I.B.P. lead to positive advances in science that would not have happened otherwise?

(ii) Did the I.B.P. divert resources that would otherwise have been spent better?

(iii) Did the I.B.P. materially improve communication among scientists, or between scientists and others, and so confer indirect benefits?

The answers are unlikely to be the same across the whole heterogeneous field of the I.B.P., but generalization is unavoidable in so short a time. It is also worth seeking some general lessons about the design of international research programmes and data exchange.

The I.B.P. was about biological productivity and human welfare. Three sections – P.T., P.F. and P.M. – focused on the productivity of sites. In all – and P.T. shows this most clearly – two problems arose at the programme design stage. First, sampling. The programmes gathered a great deal of data about individual sites – data we may call 'within-site'. But the I.B.P. sought generalities and these can come only from fitting those sites together – that is, through establishing 'between-site' comparisons. In I.B.P., certainly in P.T. and I suspect in P.F. too, we did *not* begin by surveying and quantifying the field of variation between sites, and from this selecting detailed locations for intensive within-site study. The result is that we do not quite know where our sites 'fit', and wide generalizations about the productivity of ecosystems are not easy to make from the data – although Dr Burges, Dr Satchell and Dr Heal did show that in broad terms the results, when compared, do make apparent sense.

The second problem is linked. In a few examples, there was an attempt to structure the within-site study by adopting a systems approach, and drawing up a descriptive model. This was done for the Moor House and Meathop Wood main P.T. sites and (to some extent) for Loch Leven. There is no doubt that because this modelling approach was adopted widely in the Tundra group, that group was particularly successful in comparative assessments. Even here, to quote an evaluation by Dr Heal, we underestimated our ability to collect data and overestimated our ability to use those data. Because there was inadequate project design, and because the methods of data synthesis were not thought about at the planning stage, we collected information that was unused and unusable in the final synthesis. The quantitative and systems approach to ecology was, however, new when I.B.P. began and the rapid development we have seen in this approach owes a good deal to the stimulus the I.B.P. provided. Rather than criticize I.B.P. with hindsight, we should ask whether more attention has been given to design and to synthesis in the planning of M.A.B. – and if not, press these lessons of the I.B.P.

Those site studies that did have a model generally structured it about a simple question – 'what is productivity at this site – and what major components must we measure to determine it?' The most stimulating actual research approach, however, has often been from another angle: 'what *controls* production at this site?' The I.B.P. asked this question in a more general sense in the P.P. section, and it may be no coincidence that a degree of precision and universality seems to have been attained within the relatively advanced disciplines of physiology that the more diffuse ecology lacked. In fact, the advances in our understanding of the carbon cycle and of nitrogen fixation seem to have been success stories in the I.B.P.: did we link these experimental studies closely enough to the ecosystem studies?

The I.B.P. always seemed to me to have a somewhat tenuous bridge between the basic ecology and plant and animal physiology of P.T., P.F., P.P. and P.M. and the 'human welfare' component of H.A., U.M. and C.T. (for the conservation section of the programme surely belongs in the 'human welfare' section). Perhaps this was partly because the field productivity studies concentrated on semi-natural or natural systems and rejected sites with a large element of 'human impact'. In contrast, the projects on nutrition, on novel food resources, and on biological control were concerned with channelling energy to man – and with enhancing that flow – and were particularly concerned with intensively managed systems. Even so, the lack of a linkage is surprising, and unfortunate.

I think I can now answer my first question. I suggest that the I.B.P. in Britain *did* stimulate scientific advances that would not have happened otherwise. I feel most confident about saying this for P.T., where the long-standing ecosystem approach of freshwater and marine scientists began to be applied to land systems, catalysing developments in systems and quantitative ecology. Undoubtedly – to take question (ii) – this was at a cost of diversion of resources from other applications. We just cannot tell whether quantitative ecology would have advanced anyway. We might, perhaps, have made more progress if the interflow between ecologists working in different media had been greater.

My own view – as a somewhat arrogant generalization – is that the United Kingdom did not need the I.B.P., but that it was beneficial on balance. But we must not be parochial. Several British projects directly stimulated biological research overseas. The Lake George project, the Pasoh Forest site, the Indian beaches study and a number of H.A. programmes are examples. Our programme, putting together existing and new U.K. research and communicating it widely, may also have helped scientists in the developing countries. The existence of the I.B.P., in which scientists of those countries could participate, may well have helped them win resources to develop biological research. Governments sometimes respond to a bid for national participation in a world scheme while remaining deaf to pleas for research funds from individuals. Was the I.B.P. valuable in this way? As a communications exercise I am sure I.B.P. was valuable – and indeed laid foundations on which we built at Stockholm – for by 1972 many Governments in developing countries had recognized that the environment was a natural resource whose productivity was something to cherish, not a fantasy of rich, developed countries pursuing conservation as a tool wherewith to retard the development of the Third World. The I.B.P. cannot claim sole credit for this evolution of thinking, but I believe it helped.

May I suggest indeed that a weakness of the I.B.P. was that it involved developing countries too little, and that the causes of this were partly historical, partly financial (S.C.I.B.P. did not have the funds to underwrite major projects in the Third World), but particularly that the programme appeared too academic and too geared to the 'productivity' and too little to the 'human welfare' aspect. The great problem of the Third World today is how to develop the resources of its countries in an ecologically prudent fashion and to raise the standard of living of its people. The I.B.P. had much to say about this. It could have said more. For in I.B.P. we concentrated on energy flow in samples of natural or semi-natural ecosystems. To solve today's problems we need to be able to compare alternative systems – natural vegetation, semi-natural pasture under various herbivores, managed woodland, or intensive agriculture using conventional or unconventional crops – in the same area, and to follow the energy flow through to man. Even in Britain we need to use our indigenous biological resources of soil, pasture and arable land more effectively. The uplands cover over 25 % of our islands and produce under 5 % of our food. Dr Heal and Dr Perkins displayed the basic data about the functioning of the upland system as it has developed over several millennia. Professor Pirie and Dr Blaxter suggested ways of using such systems better. Dr Durnin raised the question of what energy intake man really needs. All this is of immense practical significance and it does seem ridiculous that ecologists, agriculturalists and nutritionists are not collaborating in this field – should not the British National Committee on Problems of the Environment take note? If we were planning the I.B.P. today, can we doubt that this would have been a dominant theme?

Some final questions. First, having said all this, *does* biology lend itself to international programmes? In the I.G.Y., our exemplar, it was evidently necessary to make synoptic measurements of magnetic flux or upper atmospheric ionization, on a worldwide scale. But biological systems may not need such a tightly coordinated approach – except, perhaps to some problems of widespread pollution, where it is instructive to note that ways of harmonizing methods, or at least inter-calibrating them, remains a very live issue. Can we pursue a less structured programme, concentrating on the communications component, especially with the Third World, and on joint projects in developing countries, if we want to do something like this again?

Finally, was not the I.B.P. too diffuse and all embracing? Should we not, in future, discriminate more carefully between programmes needing to be world-wide both in planning and operation and others where planning and operation can be national, but with international collaboration to harmonize design and interchange results? Is it not a dissipation of effort to elevate to global status, with the consequent superstructure of international committees commuting round the world in jumbo-jets, work that is national and regional in character and whose results can be disseminated adequately through normal scientific channels? Should we not, in future, be much tougher and only include in a global operational programme projects designed for that programme (using our communications links to exchange information about other national research, relevant as background?).

Last of all, the results. These are of three types. The I.B.P. methodological handbooks have already proved their worth. The programme has produced a mass of scientific papers. The third component, now awaited, is the series of synthesis volumes. We clearly allowed too little time and too small a share of the resources for the synthesis phase. But time now presses, for the information we have is dating. As a semi-serious question to section P.T.: how long can a synthesis volume remain bogged down before it begins to decompose?

Sir Cyril Clarke, F.R.S. (*Royal College of Physicians*, 11 *St Andrew's Place*, *London NW*1 4*LE*)

Many of the papers yesterday and today concern multidisciplinary projects and it occurred to me that it might be interesting to raise the problem of possible difficulties in the organization of such investigations, particularly where they involve people of different nationalities (as well as different disciplines) working together. On reading through the papers which were sent to me there is only a little about this (though there has been more in the discussion this afternoon), and it may be that on the whole nearly everything went smoothly. If this is so it would be well worth setting out in some detail the ways in which this was achieved and the pitfalls that were successfully avoided. Pitfalls are certainly not always avoided outside the I.B.P., for I read recently in *Nature* (8 May) that, at sea and in the U.S.A., scientists can dislike each other very acutely. This particularly interested me because in Professor Crisp's paper he emphasized that, long before the I.B.P., international cooperation in marine biology and physical oceanography was well established. On the other hand, when you leave the sea and get into fresh water, all may be not so well, Professor Le Cren's paper had several criticisms to make of the conception, planning and organization of the I.B.P.

I wrote to Geoffrey Harrison about the matter and he thought: 'Perhaps the most fundamental difficulty was the fact that in multidisciplinary work each investigator typically has his own specific objectives which are not necessarily those most appropriate for the interdisciplinary objectives and it is hard to ask people to subordinate their own requirements for the general good. Of course, in this respect scientists are no different from anyone else.'

I would add that when multidisciplinary investigations involve studies on man, the patients (if I may call them that) also need much thought, and my guess is that investigating human problems in New Guinea would be much more difficult than biological approaches to the control of aphids.

Harrison's overall view was that at least the H.A. section of the I.B.P., which had multinational involvement in New Guinea, Israel, Ethiopia and India, did not suffer unduly. With regard to the patients, he went on: 'Interdisciplinary work in human biology usually requires that the different types of observation be made on the same subjects, and unless the different investigators happen to be "in the field" at the same time there are innumerable problems in attempting to organize this.' On the whole, the overlap in the H.A. work was quite good, even in New Guinea where different investigators were going out over a five-year period, but the proportion of individuals on whom we have every measurement is small. It is very easy to be critical of this from a distance, but when one gets on the spot and faces subjects who are not necessarily all that enthusiastic, and usually have better things to do anyway, one comes to be much more sympathetic. I think, for example, that those who were totally immersed in water for the nutritional studies in New Guinea were somewhat reluctant later to get in the heat tolerance bed and who can blame them?

Actually, in simple societies where there is not an excessive amount of movement you can often find the same subjects years later. Last summer I chased up hill and down dale some women who had been involved in the physiological work some three or four years earlier and managed to find about 80% of them, but I was only interested in completing a reproductive history questionnaire. Had I had some complex instrumentation I am sure the ascertainment would have dropped quite dramatically.

Another way in which there may be difficulty was pointed out by Dr Ratcliffe this morning in his paper on conservation of terrestrial communities. You remember he said that there was not always close integration between the various contributing bodies or individuals. There was, for instance, little if any connection between the Conservancy's Nature Conservation Review and the parallel survey and selection of key sites for nature conservation in Northern Ireland. There were also criticisms of the I.B.P. projects by Professor Pirie this afternoon.

All I want to do therefore is to raise the matter of possible difficulties, both between individuals in a project and between various bodies who are collaborating. It may well be that the I.B.P. problems were very minor, in which case full marks to the organizers.

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