8.
Discussion Workshops

(i) Use of whole-farm systems in pesticide impact studies
Chairman: Dr T. Lewis

(ii) Methods for interpretation of data from field studies of pesticide impact
Chairman: Dr P. I. Stanley

DISCUSSION WORKSHOP The use of whole farm systems in pesticide impact studies (i)

Chairman: Dr T. Lewis, Institute of Arable Crops Research, Rothamsted
Rapporteur: Dr B.D. Smith, Institute of Arable Crops Research, Long Ashton

T. Lewis: It is my unenviable task to stimulate you to talk for an hour and twenty minutes on the theme of 'the use of whole farm systems in pesticide impact studies'. Listening to the papers, all excellent, one gets the impression that we easily get carried away with detail. To begin, perhaps, we ought to consider the need to do this work at all, remembering that we are discussing whole farm systems. I suggest that for five or ten minutes we talk about the philosophy of such studies and then consider different approaches. There are a lot of pertinent questions which have not yet been addressed and certainly not answered. Thereafter we might consider the layout of experiments and finally, if there is time, the choice of pesticides. It is all too easy to start work on a pesticide only to find that it is no longer used or recommended by the time its environmental impact is appreciated. It is important that to address this point.

Why, then, are we are doing this work at all? It is appropriate to remind ourselves that we are not aiming to produce nature parks or nature reserves on farms. The farmer has to earn a living through food production for the community, and it is in that context that we should consider the effects of pesticides on the enviroment. What environmental aspects are we really interested in? Is it the flora, the fauna, the large fauna, the invertebrate fauna, or aquatic fauna? Are we concerned with only arable or also with grassland and woodland? Is there such a thing as a generalised farm system in which we can look at all these aspects? I would doubt this and, that being so, how do we compare one with the other? How do we compare different sites? So far no speaker seems to have mentioned that. Perhaps it is because we have only one farm system represented from each of the four countries represented by papers at this meeting. Can each country afford more than one? We need also to address the question of whether productive or economically-viable farming is totally compatible with all aspects of environmental preservation. If it isn't, which aspects are we willing to relinquish? I am sure the answer to that will be very subjective. I return to the question posed earlier about compounds. Is it worth doing this type of work at all, other than to develop methodology? If the effects are long-term will the compounds that we are actually focussing on be of current interest by the time we know the answers? Would anyone like to comment on why they think we should be doing this work other than to devise methods?

P. Jepson, Southampton University: Perhaps one of the more important reasons for looking at farming systems, other than creating economic strategies for farmers, is that farming constitutes a very important habitat for wild life as well as an industrial medium for producing crops. We must create a balance between the two. We should seek options that permit us to produce an economic yield and preserve wildlife. Another approach to justifying 'a system' is that it has the advantage, as we have been hearing about with the Boxworth experiment, of looking at more than one year and more than one treatment in more than one field. A whole farm system expands the scale of conventional experiments without necessarily seeking a new strategy for growers. Just determining the effects of the combined treatments on this scale makes it a very important approach.

T. Lewis: It would be interesting hear what farmers think. One doesn't hear of too many who stress the importance of this approach.

N. Sotherton, Game Conservency: I must contradict you there, Chairman. I think farmers are becoming increasingly more aware of their responsibilities, as a result of the great pressure on them as the producers of food and as the end users of pesticides. Farmers are increasingly aware of the need for evaluation of side effects of their farming methods. This is for a number of reasons. They are concerned purely from a public relations point of view. Also, a growing amount of leisure time, plus a growing awareness of, for want of a better word, 'greenness' which exerts increasing pressure on them to be concerned. At the other end of the scale, in terms of game production, there is actually an economic return for those people who are prepared to practice crop production in a more judicious manner. This does lead, as we have shown, to increased production of wild game birds. Somewhere in the middle, perhaps, there are those who are concerned with wild life *per se*. I think we are witnessing the growth of the wildlife movement. There are people who are concerned to encourage and maintain certain species for which no economic value can be calculated on their farm land. For example, there is only an aesthetic value in maintaining a diversity of wild flowers in many situations. I do not agree that farmers are not concerned. I think that they are concerned and that concern is rising for a number of reasons.

T. Lewis: Thank you Dr Sotherton. You speak, I suppose, as one of the few representives of the private sector who is prepared to find ways of funding this sort of investigation. Should such work be funded privately more often or should it be funded largely from the public purse, or both? Public funds are shrinking and I do not see many private concerns that are big enough, or willing enough, to take this work on. The Game Conservancy is an exception, having a vested interest in studying this topic. Who is willing, or should be willing, to pay? The public sector in the UK has done something through Boxworth. Can Dr El Titi or the speaker from Nagele, tell us whether they are funded privately or publicly?

Dr El Titi, Stuttgart: On the Continent we have a different situation to the UK. We have government regulation which more or less forces people to do something towards environmental protection of the soil. This limitation forces the farmer not to use certain compounds. Companies have reacted already with regard to the leachability of different compounds in the soil. There are many points of view which we could consider when we look at the farming system and the value to the farmer of taking care of the invertebrate components. For instance, in the context of soil erosion, we have quite good evidence linking the population density of earthworms and water uptake by the soil to decrease in soil erosion. A major problem is that you cannot evaluate the financial cost of erosion to the farmer, even with annual losses of say 1 or 2 cm of topsoil. Also, over 10 years, we have obtained results showing clearly that increasing numbers of different agents (e.g. carabids and mesostigmatid mites) are furthering the emergence of sugar beets. We don't apply insecticides to sugar beet, to avoid damaging the population of potential antagonists to pests and diseases and of biological agents which are improving soil characteristics. In the long-term the farmer is interested in avoiding pesticide effects, but mainly for economic reasons. Generally, insecticides or pesticides are not the most efficient components in the farming system and reduction in their use will benefit those biological agents of value to the environment. This biological value is often greater than the benefits given by pesticides. There are many farmers associations which are interested in

producing alternative products and they are financing this work themselves.

R. Heitefuss, Gottingen: There are several other organisations which support research in developing methods for integrated control. For example, the German Research Society has, for 5 years, supported a big project, in which several universities participate, developing integrated systems for wheat production. Many of the things we were talking about are included in this inter-disciplinary research project. The main question we must answer is - Does it pay the farmer to switch from conventional insurance spraying to supervised, integrated plant protection? Who will tell the farmer what to do, count the aphids, weeds and things like this? Has this to be done by consultants or the public sector? The answer must be to train the farmer to judge the situation in the field for himself. He must decide whether to spray or not, and whether he has to spray following a set routine. This is the easiest option for the farmer. As scientists, we have to prove that this is economic for him. He is not so much interested in wildlife *per se*, but will have to decide how many predators to preserve and how many weeds are needed to do this. In this he is interested principally in what he can gain for himself; farmland must have optimum revenue. On the other hand we should also consider public opinion, the tax payer, and the consequences of the actions of the European market.

T. Lewis: You have touched on the question of politics. Is the Common Market, by selecting and emphasising the growth of particular crops in particular areas, and therefore reducing the diversity of crops in all areas, encouraging specialisation? Will not the long-term effect of specialisation be to encourage the use of more pesticides in specialised areas, so decreasing wildlife in general?

R. Heitefuss: That may be one result of the common Market policy. This will result also in loss of agriculture in certain areas where we can produce little, e.g. in areas where production is not so high as in England. In these areas soil fertility will decline greatly. What will be done with this land then? Will any new use of this land be supported by public funds? Will it be used for forestry? That is one picture I could envisage. Our policy now is to try, at least in these areas, to keep agriculture at a lower intensity and support the farmer by public funds. The question is not yet fully answered. In other areas, farming intensity will go up as will the difficulties we are running into right now. Look at nitrogen. This was not seen as a problem years ago. Even if we had looked at nitrate leaching in arable fields at that time we would still have nitrate pollution of groundwater and of drinking water now. We know this is toxicologically important and public pressure insists the water has to be clean.

T. Lewis: That is one aspect of public pressure but there is another. The public, the consumers, want a wide range of unblemished produce for a greater part of the year than they used to. They cannot have this without pesticides.

P. Mineau, Canadian Wildlife Service: I think it is too simple to say pesticide or no pesticide, food or no food, happy farmers or farmers that are broke economically. What we should be doing, and encouraging in our research, is looking at things in perspective. In Canada, when we do a wildlife study we try to include not just the one compound we are most concerned about but also possibly another one further along the development curve that we are less concerned about. This might allow us to to say we have looked at this compound and do not like it for specified reasons.

However, the other compound is a good substitute under these particular growing conditions for these particular needs. In most regulatory systems, pesticides are assessed by Ministries or their representatives, who usually focus on only one compound. As results are difficult to interpret, it is very difficult to get an absolute answer. Therefore, I think that more ought to be done by regulatory authorities on the comparability of different products. They (the regulators) obviously should have a very different cost-benefit view to that of industry. In industry, a company will produce only one or two aphicides. In practice, however, there are 8, 9 or 10 available on the market. Thus, for that company to be told that their aphicide is causing wildlife damage is hard to swallow and interferes with profits. In contrast, for the farmer there are a number of alternative aphicides that do not have such severe consequences, and will effect an acceptable level of aphid control. In effect, I am saying that wildlife studies, especially using comparisons of pesticides, should be supported by public funding.

T. Lewis: What happens in Holland?

C. Booij, Plant Protection Institute, Wageningen: In Holland there are different reasons for the study of farming systems compared to those in other countries. The main reasons, at present, arise from the fact that we have two main crops which are potato and sugar beet. With potatoes nematicides are used frequently and cause many problems by leaching into groundwater and into drinking water. One of the reasons the State is promoting this line of research is to stimulate crop rotations and not specialisation. Specialisation only causes more stress on the environment. The continued use of (especially) nematicides in potatoes has severe effects on soil fertility in Holland.

P. Bunyan, ADAS: I think that, in answer to your question about funding, the 1986 Agriculture Act, Section 17, commits the Minister to have due regard to natural beauty etc. in all that he does. I am sure, or as sure as one can be in political activities, that this commits him to funding work on the environmental aspects of agriculture. Most people accept that it is one of the areas which is right for public funding. I used to stand proudly and say 'we did have a voluntary system for the control of pesticides'. Of course, we now have a mandatory system. Under this system we have to give guidance to farmers on the product label and they have to follow it.

Turning to the beginnings of the Boxworth project, I would say that we set it up in order to establish broad principles. Then we could look into whatever came out of it rather more carefully in smaller batches in other places with other limitations.

When you sit with a group of people evaluating pesticides, you are very aware that decisions based on human toxicology are clear cut. You can see where you should draw the line. In contrast, with the environment it is much more difficult. Even so, we now have a law and we have to comply with it, even though the law doesn't actually line up very easily with science. Therefore, I think that what we must hope to get out of environmental impact studies are broad principles. This probably results in using pretty rough justice in order to make meaningful decisions about the permissable use of pesticides and their effects on the environment. One such decision, which we made a few years ago, was to remove carbofenothion seed dressing from England. It killed some geese on the East coast of England and we concluded that the only way to avoid this was to stop the pesticide being used in England. It can still be used in Scotland. That sort of broad decision will, I believe, still need to be

taken even though you cannot defend it scientifically. Perhaps some of our original thoughts about spring use of pyrethroids are now wrong in the light of the results of the Boxworth Project. The Boxworth Project has been reasonably successful in achieving its original objectives. Dr Hardy posed the question - Is a single farm better than a lot of small units? This is something we need to think about. My own view is that we have got as much as we could probably expect out of Boxworth and we now need to go on from there. We need to define exactly where we should go.

P. Jepson, Southampton University: I think we have been talking about two separate things. One is work on farming systems which will reduce pesticide inputs, which has been generated by public wish, perhaps by small pressure minorities. Secondly, we are also talking about the need for fundamental scientific research. In both cases, public funding is appropriate but, perhaps, the need for scientific research gets pushed to one side.

One question, which was raised recently by the Advisory Board to the Research Councils, was the spatial dynamics involved in farmland ecology. This concerns the issue of how different controls in farming systems affect the ability of populations to recover and recolonise. How resilient are these populations on a large scale? Until we can answer questions like this, we cannot develop a scientific rationale to support our conclusions.

T. Lewis: Can we proceed to more detailed points. Most speakers in the paper sessions talked about indicator species. Do indicator species represent the best approach? How does one know that the right indicator species have been chosen? Are we not principally concerned with the breadth of the fauna, its species richness? What is a good indicator species? Is it the most abundant and, therefore, suitable for statistical analysis or is it the rarest and, therefore, the most likely to disappear under pesticide induced stress?. What factors affect the choice of indicator species? For birds, for example, does one choose a certain predator because it is easy to spot?

A. Burn, Cambridge University: One point about taking key individual species for analysis (perhaps you prefer to call them indicator species) rather than dealing with faunal richness, is that the latter approach may overlook interactions. Investigations of a single species particularly one that is easily sampled and analysed, at least gives the opportunity of observing mechanisms of pesticide action and predicting long-term effects.

T. Lewis: How do you know it is a key species until you have done the work?

A. Burn: That is a very good point. You have to do a certain amount of work to decide which are the likely indicator species. For example, he Boxworth project reports, between 1983 and 1986, include the sort of scheme I have outlined. Our choice of 'indicator' species has changed. I do not really like the term indicator species. It is more appropriate to consider indicator groups or functional groups within which there are certain species which are amenable to detailed study and within which it is possible to look at interactions in a detailed way. This produces more possibility for predicting effects rather than simply observing effects and concluding that they are the only consequences.

R. Brown, ICI Agrochemicals: In the context of the way we all do our work on pesticides, indicator species are very important. Most of them seem to be from the class level and include a couple of birds, a couple of mammals,

some insects, some arachnids. There never seem to be any amphibians. The data package that has been built up for registration of the pesticide includes detailed toxicological work for maybe 30 or 40 species and gives an overall indication of where the toxicity hazard lies. Obviously, as pesticides are selective, there are differences between families and between genera. Generally, however, intrinsic toxicity is not very different over a wide range of species. It is probably not important, except in order of magnitude terms. Often, exposure is the important factor that determines the response to a pesticide and the choice of an indicator species for study in the field, thus, requires consideration of likely exposure to the pesticide. There may be many orders of magnitude difference in exposure depending on whether the selected species is mobile during spraying or before the pesticide is adsorbed onto soil particles. Choosing an indicator species is the only way that we can include the exposure factor. We have tried looking at indices of diversity. Technically, there is much argument as to which is the best index to use. According to the mathematicians it is probably the Bergey-Parker Index. We have applied this to a range of species in a 3 yr experiment. The results were totally incomprehensible.

T. Lewis: Couldn't you say the same for indicator species and could anybody prove you wrong?

R. Brown: If you see a list of toxicity values for a range of 30 to 40 animals it gives you a general feeling for what will happen. For example, although carabids don't all respond to a pesticide in exactly the same way, our knowledge of their habits gives some idea of whether something is likely to be important. The indicator species concept is very important. It is the basis of almost all current toxicology work. We have got to have some detailed information which can be compared with what happens in the field. The two approaches go hand in hand.

T. Lewis: I am trying to elucidate the logic behind choosing the species for study. For example, with birds and mammals, does one choose species that are attractive to the public or those that are easy to see or easy to count?

A. Hardy, ADAS: Many field trials examining effects of a particular chemical on vertebrates (not invertebrates) must inevitably reflect the characteristics of the compound. This determines the exposure any species in the system is going to experience. I have a feeling we are beginning to put the cart in front of the horse. The central philosophy is that we are going to look, in the field, at pesticide impact on wildlife. We must at least consider approaching that from the point of view of the compound. The behaviour of the compound, its distribution in the ecosystem and its likely contact with wildlife species then indicates the direction that a study takes and whether an appropriate indicator species can be chosen. As Dr Burn pointed out, the concepts change as you do the work. When you find increasing sensitivity in a particular species you focus on that species. In terms of the use of vertebrate indicator species there is a point that nobody else has mentioned. It can be very useful to focus on a species with a particular conservation interest or a high profile in farming. It can then be used as an example to encourage particular environmental manipulation. For example, in arable fields and field margins, a common small bird, which every farmer will recognise, is the yellowhammer. Using it, for example, in an advisory situation allows emphasis of the fact that what is good for that species is probably of general benefit to the habitat of that species.

T. Coaker, Cambridge University: In deciding about indicator species, we have to try to distinguish whether we are aiming at short-term or long-term results or effects. With the short-term investigation one is concerned, principally, to develop correlative relationships. We could use an indicator species, for example, to suggest that, by reducing it, we may cause a resurgence of a pest. However, those who have done long-term population dynamics studies are always surprised at the key species that may be involved in generating stability. Therefore, the distinction I would try to make is, first of all, to define whether we are concerned with short-term or long-term effects. For long-term effects, the interactions involved are complex and have to be understood and identified. One would, by no means, be able quickly to identify suitable indicator species. When discussing this general problem of wildlife stability, and surely that is what we are aiming at, we must appreciate that it can only be maintained by a complex of interactions which we have very rarely revealed in past detailed population dynamics studies.

T. Lewis: That leads us to another point for debate. What is long and short-term? Do representives from industry who, perhaps, think in terms of insecticide persistence have the same concept of long and short-term as biologists, who may be studying creatures with only one generation a year. How would they decide what was long-term?

R. Stephenson, Shell Research Limited: It is extremely difficult to decide in advance exactly what is long term in relationships between organisms and pesticides. In the early work with pyrethroids, in the mid-70s, we and other companies were involved in doing what was then regarded as long-term field studies. These ran for 3 to 5 years. The current Boxworth study has run over a similar sort of time scale.

T. Lewis: Many years ago I recall Professor, O.W. Richards, saying that you might as well forget any ecological work which was shorter than 5 years!

R. Stephenson: It may well be that, in some situations, much longer research periods are necessary before we can understand the intricate details of particular problems. Also, as studies progress, it becomes increasingly difficult to obtain data from them in a cost-effective manner. One then realises that the point of no return has been reached for the investment required. That is in scientific terms, not just in business terms. It is then time to look for more important and relevant problems that need solving. Someone recently suggested that the Boxworth Project should run for a 100 years. Although I am sure that they would still be collecting information in 100 years time, is that really the way in which to deploy major effort? Is that going to provide us, as a community, with good value for money? Thus, what we try to do in industry is to look at the problems in relation to the lifespan of the product, the extent of its usage and the sort of profits that it is making. We try to look at the problem rationally and make sure the investment is cost-effective.

T. Lewis: That is an industrial point of view. Does any biologist want to add to or detract from it or put a different view? If you have worked at Boxworth, as half a dozen or so of you here have, do you think that there will be diminishing returns now for your steady effort or do you think that the returns will continue to be as useful as they have been hitherto?

A. Burn: That is very difficult to answer, especially as I have a vested interest. With the effects we have seen, using the techniques and

methodology that are available and within the experiment as it has been designed, I suggest that we are not going to get a lot further, even with many more years work. The Boxworth Project is looking at the economic and ecological effects of intensive production. The economic effects are very complex problems which take into account many more parameters than we have considered to-day. These include machinery use costs, manhours and many others. There has been a great deal of variation in the economic output at Boxworth under the different treatments. Sufficient variation for me, at any rate, to feel that there is no obvious trend, but rather a kind of average which balances out over time. In general, we are getting certain effects, but I don't think there would be any advantage in pursuing them. One predictable difference will come if, for example, there is a massive change in cereal prices which might very much favour supervised pesticide use. We don't need to continue the project to predict the economic effects of that sort of thing.

While I don't think, as far as the economics are concerned, that there is any great benefit in continuing, it is a different problem with the ecological aspects. Even though some groups have been seen to be severely affected over the moderately long-term, the biggest questions must come from the mammal and bird studies. For the invertebrate studies, the project has been large enough to show some severe effects. With the bird and mammal studies, however, we need to think seriously about whether we can predict the wider scale implications of the sorts of effects Boxworth has shown. For example, in the small mammal studies there are within-season effects, repeated year after year following some chemical applications, which are swamped by immigration. What is the real importance of such changes? We have to investigate whether these population changes matter but this is not best achieved by continuing the investigation at Boxworth. We have to look at the data from Boxworth more carefully before planning what comes next. It is not necessarily obvious that we should switch to a different farming system and a different cropping regime in order to get more data. What is needed next is to analyse the predictability of these effects observed at Boxworth and that requires a different kind of approach altogether.

T. Lewis: Would you personally recommend a different approach concentrated on one site or one in which the same scientific resources (rather than farm infrastructure) are spread over several sites?

A. Burn: That is two different questions really; two equally valid ways of looking at the same problem. In order to predict the more widespread impact of some of the observed effects we should look, on a smaller scale, at a wide range of different areas. In contrast, detailed examination of some of the interactions requires research on a big scale. Thus, with the Boxworth data concerning re-invasion and recolonisation, can we determine the prospect for recovery in the Insurance area? This will tell us a lot about the long-term effects of the treatment and will tell us more about the general ecology of the species.

P. Greig-Smith, ADAS: The suggestion of continuing the Boxworth project was that it should continue in its present form. It is a feature of Boxworth that it has had a rather rigid experimental design, far more so than any other of the long-term studies that have allowed evolution of farming practices. That has been both a limitation and an advantage to Boxworth. We have learned much from having a long run of years with tight experimental control and would not have gained some of the results without that design. However, this rigidity means that it is not appropriate to carry on doing the same thing, just because it sounds good to carry on

something that has been productive. There can be no doubt that a change, or flexibility, is needed in what we do next. Whether some of the questions we would like to ask can be answered at Boxworth is debatable. Some of the things we would like to do certainly can't be done. The contrast between bird or mammal studies and invertebrate studies means that the question posed a few minutes ago (whether we should be on one site or on lots of sites) is fairly easy to answer. We cannot do all these things on one site. The broader the investigation becomes, the more difficult it is to compromise in terms of scale and interference between different parts of the system.

P. Vickerman, Southampton University: I would like to make two points about time scales, thinking back to the days of my involvement with the North Farm Study. The experience there was that, certainly with respect to invertebrates, you need a minimum time of about 11 years. Many insects declined in certain crops over an 11 year period but, subsequently, the numbers of several species have recovered. The big unknown is the extent to which the different species are cycling, for we have insuffient knowledge about insect cycles. With respect to the Boxworth Project, I disagree slightly with Dr Burn. Within Boxworth it has taken three treatment years to start to reveal evidence of resurgence in cereal aphids. It is only over the last year and a half that we have found any evidence of resurgence in other minor pest species. Again, over the last 2 years a number of unpredictable things have started to appear. Although some observed changes could have been predicted to occur in a relatively short space of time (for example, declines in some carabid beetles), I am finding that certain groups of insects, like fungus feeding insects, are doing much better in the Insurance area than in the other two areas. I still feel very strongly that there is an unpredictable element there and one probably needs to look at a system like that for a period of 7 to 10 years.

R. Cousens, Inst. of Arable Crops Research, Long Ashton: In a project like Boxworth, where there are many different integrated inputs, if you get something unexpected it cannot be explained and there is no way of investigating it. The Boxworth Project has many deficiencies. However, one good outcome is that it has made the people involved realise what the different objectives are. On the weeds side, I think we got very little out of Boxworth in terms of ideas. Developing the methodology has been worthwhile and our general involvement also has been worthwhile. Most weed species have persistent seed banks and, consequently, the time scale of the Boxworth Project is not long enough to allow for this. It is very interesting to hear people here to-day describing short-term effects of plus and minus pesticides on different organisms. There haven't been any interesting effects on plants over even 4 or 5 years. I question the need for a study like Boxworth to show you those things. The results obtained concern only those species which are abundant enough to study with the very low input of manpower that we are prepared to use. Effects on the less abundant species are not seen because they aren't sampled well enough. Going back to indicator species, the species that are highlighted at this meeting as being interesting are those which are (a) abundant enough to be able to sample adequately and (b) grossly affected by the chemicals and not just responding in a minor way.

T. Lewis: May I suggest that we go on to consider experimental layout. First, I would like to hear a few thoughts on replication. Do we use a rotation system or not? What governs the choice? What crops should be used? How do you replicate, for example, for aspect, soil type, and previous treatment on a whole farm system? When all these decisions are

made what is the next stage? Does one accept what replication is feasible and then ask statisticians to optimize the analysis, or does one devise a form of analysis and then plan the field layout around it? I would be interested to know why Dr Booij chose to use principal component analysis in his experiments. Did you do the work first and then wonder how to analyse it, or did you choose principle component analysis as an obvious analytical approach and then plan your experiment?

C. Booij: The amount of effort you have to put into replication is enormous and often is impossible. On the other hand I think the years could be considered as replication in time and then you could use the data to analyse trends.

T. Lewis: But, if you study an organism whose population cycles over several years, how can you use years as replicates?

C. Booij: At least you can compare differences. The differences between systems are consistent between years.

T. Lewis: But which came first, the method of analysis followed by the design or vice versa?

C. Booij: The design, of course. The objective of the farm system project was to optimise and integrate the system, not to test for pesticide effects on predators, although the opportunity was there for that. Having decided on the design we then looked for a good analysis procedure. We found that the techniques we chose were good for the purpose because they facilitate looking for trends or patterns in the data sets.

M. Luff, Newcastle University: I keep being reminded of a statement made by John Jeffers of ITE at a quite different meeting many years ago when dealing with the philosophy of experimentation. He said that traditional experimentation involves measurements of controlled variation. This seems to be the approach which, by and large, we have been considering as the essential one. An alternative which Jeffers put forward, was the analysis of uncontrolled variability. In some situations one can get as much information, or more information, from that approach. This is, essentially, I think what the multi-variate methods are doing. They take a variation that exists, not because we control it but because it is there from other environmental factors, and fit patterns to it. In this way conclusions can be drawn which may be just as valid as those obtained by conventional experimentation. Where, as in the sort of situation you were suggesting, Chairman, we have many variables to consider (aspect, rotation, crop etc.) analysis of naturally-existing variation in those factors may produce a better approach than rigorous experimentation.

J. Perry, Inst. Arable Crops Research, Rothamsted: We should be clear about what we are looking at here. The variates Dr Luff mentioned are not variates, they are factors that were not controlled. They are factors we would generally only be interested in, possibly, in relation to a single variate. For example, population density is a single variate. I think that Dr Booij's approach was that it is very difficult to compare the various treatments that were imposed in terms of their population densities. Therefore, if you remove population density you could look at the phenological profile in the way that the seasonal catch progressed with time and to compare the patterns with the passage of time. You can do this in a multi-variate way, using a similar analysis to that I used with John Bowden in 1982, to compare phenologies of various types of traps. I

do not think we can get a lot of information from experiments that do not have that variation well controlled. It is a very difficult problem. Even designing an experiment in fundamental science (for example investigating movement), measuring different degrees of isolation and different degrees of patches, as mentioned by Dr Jepson earlier, is an extremely difficult problem. It is a very narrow problem that involves a great deal of land at great cost.

Coming back to who funds this research, I think it has got to be funded in the public sector because, by its very nature, it is a very difficult thing to do. I can't see industry being willing to fund such intricate projects, which are necessarily long-term and large scale. I agree very much with Dr Brown about lack of benefit in a species richness approach. I too found it is very open to misinterpretation and argument. Also, it is a very limited form of analysis. The data derived from such studies are useful but fairly general. It should not be anything more than an adjunct to the more specific study. Baseline studies, as Dr Burn and Dr Coaker mentioned, emphasising the importance of deciding which species to study, will provide the sort of information that is usually lacking about variation between different areas of fields. We can possibly use these to decide on the optimum size and shape of our sampling plots, and whether or not to stratify. Stratifying is mentioned very little these days. Is this, perhaps, a forgotten concept? Greig-Smith, who had such an effect in plant ecology with his method for deciding where the various degrees of spatial pattern were in a field, set out exactly what we should do to decide where to put our optimum effort. The use of blocking, and possibly of Latin squares, as has been done by Southampton University and the Game Conservancy, is very important. I don't think we should be under any illusions that using whole farm systems is likely to give clear cut results. It is unlikely to give the sort of results that we are used to in small scale, short-term experiments. We should be looking for a more qualitative and, if you like, a more multi-variate approach, in the true sense of the word.

T. Lewis: Can I move on to the last area that I thought we might consider? That is the choice of pesticides for study. Does one consider those that are already known to have harmful effects, bearing in mind that it might be a long time before answers are available, or should we concentrate on new compounds, just about to come onto the market and not yet identified as harmful? I have not heard much about pesticides other than insecticides. What about fungicides, herbicides and plant growth regulators? Who chooses which environmental contaminants merit investigation? Would it be sensible to have such a thing as a control compound against which to measure all the effects? You might say that DDT was such a control compound, but only subjectively. Would industry like to opt for a control compound? Nobody yet has mentioned anything about the timescale necessary to pick up sub-lethal effects of compounds.

R. Brown: Industry does use control compounds all the time and finds them very useful. Dimethoate is one which is used, for example. It is a very useful 'benchmark' for insecticides as it gives reproducible results. Atrazine is used as a standard for leaching studies. These become standards because they have been studied for a long time and their behaviour and effects are known in detail. Certainly, in general terms, the industry supports the use of standards. It is one way of judging one product against another.

T. Lewis: I am not disputing that but I was wondering whether we had a suitable standard among the various farm system projects that have been

tried in the past or, if we are establishing new projects would it be wise to include a standard, 'benchmark', compound?

R. Brown: Each compound has many unique properties. What is needed is not so much an understanding of the compound, as a definition of the important undesirable side-effects that may occur in an eco-system. Bio-accumulation has been identified as undesirable and we can, therefore, identify bio-accumulating compounds fairly early in their development.

P. Greig-Smith: Regarding whole farm systems in a rather limited way, as simply larger and longer than conventional trial plots, has some dangers. There are other elements that are built in. In the Boxworth Project, for example, we specifically set out to monitor a wide range of wild life and effects and, particularly, to examine the whole package of pesticides. A lot of the remarks that have been made this afternoon really apply just as much to a short-term trial of a single pesticide. One of the big issues concerning whole farm systems is the need to assess the whole package of pesticides. That is another reason why, I would suggest, we don't need to think too firmly about going to classical experimental design as an essential part of whole farm system projects. The Boxworth project was, in many ways, a preliminary sighting exercise to find out what sort of mechanisms one would subsequently want to address using classical experimental design. When you are dealing with an input of something like 18 active ingredients and lots of different farming operations, there is so much uncontrolled variation within each replicate that I doubt whether one could satisfactorily factor it out with any statistical method, and one shouldn't try to do so.

P. Bunyan: Could I just follow that up, Chairman, and pick up something that Dr Coaker said. Dr Coaker asked why we don't do long-term population studies anymore; nobody seems to be interested. I should point out that it has never been done in terms of agricultural land. Moorland or heath has been studied, but that is land which is for all intents and purposes, undisturbed. Agricultural land has always been disturbed and one thing which bothers the public is that agricultural land is more and more disturbed with ploughing or with spraying and so on. One of the problems about Boxworth is that we are tied into a 1978 system and systems have moved on. We took a semi-classical view of the experiment and said that, although we cannot lay it down as we would like to, at least we will keep the system fairly constant. Now we are saying that agricultural pesticide usage has left us behind to some extent. So in a way, I think that what may be important is how the use of agricultural pesticides (a) has an effect on the flora and fauna of farmland and therefore may affect the economics for the farmer, and (b) how it affects those areas of land which are still original with a rich fauna and which the public values. Do we have, in farmland, any sort of reservoir which is interacting with the sort of thing that the Nature Conservancy Council are really interested in? That is where the longer term look is going to be. I do not think you can look long-term at farmland. It does not seem, to me, to be the right answer.

T. Lewis: Can I just come back to this one point; if we perceive environmental problems arising from a broad group of compounds, do we see it as coming most likely from insecticides, fungicides, herbicides or what?

B. Bagnall, Bayer: We shouldn't, perhaps, be considering the pesticide groups as such but rather thinking of the persistence of that molecule or group, choosing some which last for more than a season. In Boxworth, we

have been looking at the things that farmers use *per se*, the market leaders of the day. Perhaps we should look at the impact on the environment from those compounds whose residues persists into the next crop. I suggest that is the sort of thing to choose rather than a new fungicide or a new herbicide. Pesticides are changing a lot now for various reasons, such as the development of resistance. There is no telling what will happen in the next 10 years. Insecticides have stayed surprisingly stable. Who would think that demeton-s-methyl would still be used quite widely 35 years after it was introduced? I don't think many fungicides will be used 35 years after introduction.

T. Lewis: Does anyone from the Continent want to comment on which groups of compounds they have found most interesting?

C. Booij: We in Holland usually concentrate on integrated systems and therefore need to consider a package of pesticides. If your aim is to reduce pesticide use in general or to use safer pesticides, it is very good to compare pesticides. Of course, for industry, studies of packages don't identify which pesticides cause problems. For farmers, however, it is very important to have an integrated package of pesticides for which he knows that the environmental aspects are good.

T. Lewis: I have found this discussion interesting and certainly instructive. Thank you to all the contributors to this excellent workshop.

DISCUSSION WORKSHOP Interpretation of data (ii)

Chairman: Dr P. Stanley, ADAS Slough Laboratory
Rapporteur: Dr P.W. Greig-Smith, ADAS Tolworth Laboratory

I welcome you to the last session of this conference. I have been involved in pesticides and wildlife for a long period, both from an R & D standpoint, through a period when I was formally involved in the assessment of environmental data as part of the registration process in the UK, and now in terms of a laboratory which has a major commitment to environmental studies.

The emphasis in this last session is on the interpretation of data. The only justification for carrying out field assessments of environmental hazards from pesticides is to produce results which can be interpreted in a meaningful sense, when decisions have to be taken on whether the pesticide should be cleared for use, how it should be used, and so on. At the end of the day someone has to take a decision on whether an environmental effect is acceptable or unacceptable. Richard Brown touched on this in the last session. I haven't heard anybody approach the subject of the real interpretation of field data, probably because it is a fairly intractable subject and not one to willingly get involved in. There have been wide-ranging discussions in the last two days, dealing with all aspects of field testing. What I would like to do is to structure the discussion this afternoon to three areas. The first is field trial methodology. There have been a lot of contributions on field trial methods and methodology and there are some outstanding questions, some of which surfaced this afternoon, and I will raise some more. I would then like to go on to the intepretation of trial data from the decision-making view point. Finally we should consider a third area, that the result of field trials are not considered in a vacuum. There is much information available to put alongside the company's trial data. For example in the UK the Advisory Committee on Pesticides has information available from field monitoring. Obviously this is a valuable aid to the decision-making process.

Eco-toxicology is a young discipline and field trial methodology is a rapidly developing science. This is recognised by the registration authorities and there are moves, internationally, to produce for notifiers more guidelines on the acquisition of environmental data. Some of you may be aware of recent moves within the USA where the Environmental Protection Agency have worked, over the last 18 months, to produce guidelines for terrestrial testing. These have been released in the last month. The guidelines are a lengthy document in which the authors have grappled with a number of the aspects which have been discussed here to-day and have produced their answers or guidance. For example, how many field sites should be looked at? What size of field sites and what location? The EPA wishes to have a statistical significance attached to environmental hazard assessment. It is questionable, philosophically, whether this can be done, and I am sure we will debate that. They propose the use of simple statistics based on the binomial theorem, and the entire guidance centres around mortality of vertebrate wildlife. They have said that we can guess quantitatively the chances of finding casualties in the field if they occur. This will vary between species. If it is swans that are dying you will probably find most of them in a field trial. If it is very small birds then the chance of finding them is very much less. You can quantify this in the field. The EPA then asks for an 80% probability of being able to detect this environmental hazard, i.e. mortality. The binomial theory can be used to define the number of sites needed to to give that degree of confidence in the identification of an environmental hazard. Using figures of 20% success in casualty searching and 80% probability of detection

suggests something like 32 sites. Obviously, varying these figures produces different numbers. However, it usually produces a large number of geographically separate sites. The guidance goes on to explain that, at each site, those doing the work carry out a full range of pre-treatment studies on all non-target wildlife populations. This includes casualty searching and environmental measurements of several kinds; blood sampling, esterase measurements and residue measurements. The studies should continue for a full year after treatment. Every attempt must be made to measure effects on populations (not just individuals) of non-target wildlife on the sites.

This is a demanding requirement which is now appearing in the documents from the EPA when pesticides come up for review. It results in requests for trials on 8, 10, 12, 14 or 20 sites with a tremendous investment of resources, millions of dollars needing to be committed. In the UK, and generally in Europe, there has been a major contribution to the development of field trial methodology. The principal approach has been to recommend intensive, well-designed, wildlife trials at a limited number of sites, often only one, on the basis that the proper resources put into design and execution will give meaningful and cost-effective data. There have been attempts, certainly in the UK by the Advisory Committee on Pesticides, to strive to ensure that data requests are justifiable in terms of the need for the data and the ability to interpret them. This contrasts to the statistical approach in the US which demands a large number of sites and considerable resources. It is not certain that the trained staff and facilities are available to carry out such trials to the required quality. Before going on to look at other aspects of field trials, the discussion should consider the approach that should be adopted to the selection of the number and size of sites. I think Drs Perry, Sotherton, Burn, Stephenson and others have all commented on sites and site selection, and I am sure Dr Cousens will have a statistical viewpoint.

N. Sotherton : Selection of numbers and types of sites is a very difficult problem unless you can identify the organisms at risk and define the quality of their sites. I think that for a short term effect I wouldn't look at more than one site. I would adopt the semi-field approach which would mean a single field site with lots of small plot replicates. This enables you to distinguish between the various reasons for changes in animal numbers.

P. Stanley: In a semi-field trial, what size of plots are appropriate? Presumably this would depend on the species you are looking at?

N. Sotherton: Yes, there are many factors involved, such as the species being studied, toxicity of the chemical and whether you need to assess all species. If you have chosen the organism at risk, half the problem is solved. Many of us would accept there is not much problem in assessing short-term effects. One point I was trying to make was that people are already using that type of methodology to deduce duration of effects.

P. Stanley: In terms of short-term hazards to invertebrates you would adopt that approach. Dr Brown, how would you assess short term hazards for vertebrates, for example, in a rodenticide assessment?

R. Brown, ICI: I have been guilty of trying to do a rodenticide impact study using one huge site. The EPA approach is a kind of insurance policy, because lawyers stalk behind the EPA. The way that they have set the requirements of using the binomial approach is not the usual way one would design a field trial. Normally you would look at the variations in the

parameter you were measuring and decide what effect you wanted to measure. There are formulae which will tell you how many replicates you need. They have not taken this approach. They have said - Are we right or are we wrong? How many sites do we need to decide this? They are worried that if they say yes and the answer is no, they may be dragged into court. They must, then, be able to say that the best statistical advice says that they are right. This moves out of scientific arguments and into legal ones. It is obvious that they started off with what they felt they could get away with and realised that it was not possible to do such major studies routinely. They have then devised ways of splitting plots and pairing sites to reach a level where they considered the work was possible. I think that deciding the number of sites is very difficult, depending on the number of skilled people that you can get into the field and the practicalities of that. Generally, it is a case, of 'the more sites the better'.

P. Mineau, Canadian Wildlife Service: I am not going to defend the EPA guidelines but I think you have slightly misrepresented exactly why that figure of 80% comes in. My understanding is that they are saying there is a lot of variation in how a pesticide is used and hence potentially on its impact on the environment. We would like to catch those compounds that cause an impact, however defined, more than 20 percent of the time. It is then a question of how sure you are of detecting if it does occur and setting that at 100 percent in order to come down to a reasonable field number of 7. If they take a more reasonable figure for the certainty of detection, which boils down to the model I tried to present yesterday, then it quickly becomes unreasonable.

By going to one field only you have to make the assumption that the application of the chemical, and the impact, will be the same everywhere the compound is being used. The EPA system was built around the idea of mortality and their guidelines evolved in parallel with the effort to deal with the problem of granular pesticides. With fenitrothion forest spraying, a clearer picture of its impact has emerged only after 10 to 12 studies.

The door is open in the EPA guidelines to accept any level of impact. Basically you set that level of impact and then go out in the field. First, you must be sure that you can find a given level of impact. You have to adjust your plot size, your population number, and so on, in order to be assured that you will detect any impact that occurs if it occurs more than 20% of the time. That is somewhat different to the way that you presented it.

In the case of a chemical with which we have had a lot of experience (fenitrothion used on forests), we are fortunate to have the benefit of 10 or 12 different studies. I must admit that initially things were a lot clearer than they are now. There is a lot of variability among sites in the possible impact of any one compound. We started by thinking that the product was quite acceptable but now, after 10 or 12 trials, there are very serious doubts about that. We have not changed our methodology but have looked at more situations.

P. Edwards, ICI: I do not think that you can generalise about how many sites to study. It depends very much on circumstances. The pattern of exposure may be very variable, as with a rodenticide bait. Concealment is the most important thing which determines hazard from rodenticides. Thus, with different individuals applying the baits, there will be a high degree of variability in concealment. Then, it may be very important to look at a lot of sites, even though you probably cannot look at them in very intense detail. On the other hand, with relatively standard applications, such as

the broadcasting of pellets or sowing of seed, with good measures of exposure (e.g. through esterase levels), it will be more fruitful to get information from a small number of sites in greater detail. Taking that approach, with only a few sites, requires great care in choosing sites that present a worst case situation in terms of the wildlife available. It is a mistake to just put a figure on the number of sites. Maybe in the USA the problem is a bit harder because the area is a lot bigger than the UK and the environments are more varied. For example, the east coast bird populations are different to those in the west. As their problem is bigger, you can't actually say that the EPA are wrong.

P. Stanley: The approach in the UK has never been to rely on wildlife trial data from one site. There has been a process of phased introduction with increasing confidence from the data that were generated in the process. Individual trials have revealed evidence of a scale of hazard which, whilst acceptable, was a matter of some concern. Surveillance exercises during commercial use have been carried out on a fairly wide range of compounds with a programme of observations at a number of sites throughout the country. There are other approaches.

P. Edwards: I support that. In work that I have done with compounds used in arable land, 2 or 3 sites have usually been adequate for a worst case situation. It must be done it intensively with some kind of back-up system. The back-up is important so that the regulators have some confidence. In the UK we have the British Trust for Ornithology's monitoring programme and the Wildlife Incident Investigation Scheme. These approaches cannot be used quite so well in the USA.

B. Bagnall, Bayer: What is missing in the UK is the limited clearance that we used to have and from which we could get impact data. When we talk about the number of sites for an assessment of a field study we are discussing scientific trials. The data obtained from limited clearance was from incidents caused by people who did not bury spillage or who did things not according to the label. Certainly, one needs two or three sites to get an authentic picture, but there is a vacuum that provisional approval isn't going to make up for under the current regime. It can't replace the limited clearance that we had in the past.

P. Stanley: Perhaps you could explain, for the people who are not aware, what limited clearance was.

B. Bagnall: In the stepwise clearance system, you were granted permission in the first year of the new product to use it on, say, 10,000 hectares of cereals for, perhaps, two years. You were asked to monitor the impact of the product on sites used by farmers. The farmer applied the product, he read and interpreted the label and he was either good or he was one of those who spilled half a kilo on the headland. Monitoring this sort of situation gave us a lot of feed-back which was put into the PSPS system as part of the additional data package that went towards the provisional clearance under the old scheme.

Anon. Consultant: During the development of carbamate insecticide as a seed treatment it was necessary to demonstrate safety to birds. We were able to distinguish between the hazard to small birds like sparrows and non-hazards to rooks and pheasants. This was done in a number of countries within Europe so that, while there may have been only 3 or 4 sites in the UK, when you add up all the other countries in Europe the total site numbers were much larger.

R. Brown: As Dr Mineau pointed out, and as we all know, carcase searching is very inefficient. The important thing is to try to quantify exposure rather than go round counting carcases.

P. Mineau: I agree with you except that, in a number of cases, through the removal from the population either by death or emigration, you are already causing a severe bias in the collectability of the different individuals in that population. So, if you rely on sampling for your exposure measurements you will get a biased picture. We have demonstrated this in forestry where you can collect individuals up to a certain level of exposure and then, beyond that, they are very difficult to collect. Many individuals will hide and die in tree trunks and will not be found. Analysis of the exposure of those organisms which you can see may suggest that exposure is within tolerable bounds. In fact, you are missing the peak of the most susceptible individuals.

R. Brown: That is not actually my point. The point is the need to identify the potential exposure with, say, granular rat poisons. It is much more instructive to analyse the distribution of the residues of the rodenticide in space and time in the field in relation to the diet of rodents.

P. Stanley: The assessment of exposure must be one of the weak links in field trial methodology.

P. Bunyan, MAFF: If you tie assessment, quite rightly, to a need to take some sort of decision, then it is very difficult to make the assessment even for birds and mammals. At least you can see them, and consequently the public are very aware of them. Entomological studies pose a much more difficult problem. Potentially, there are big effects on certain insect populations and a need to quantify them and determine how and why they occur. We are doing a lot of work, particularly in cereals, to confirm that effects do occur. As regulators we must ask ourselves at what stage do we consider we should draw the line because there is some effect on insects. It does seem to me that we have not addressed that problem at all.

P. Stanley: When one is assessing pesticide hazard to an insect population, particularly on cereals, it is clear that there will be other insecticides applied as part of the crop protection programme. We are clearing chemicals individually and yet they are being used in programmes and this obviously affects the balance.

N. Sotherton: One of the most exciting things that has struck me over the last two days, has been Dr Kenward's work with telemetry. Perhaps a lot of the problems with vertebrates could be solved very easily by telemetry. Maybe that would reduce the EPA requirement for 20 sites. You could concentrate work with a few highly trained and technical staff who would be able to follow individually marked animals. With the technology that is available, and becoming increasingly more available, you could know exactly what is going on with each individual and have no problems with searching for carcases. I would welcome the view of the regulators and industry on that.

P. Stanley: Telemetry is being pushed very hard by the EPA as one of the methodologies to be used in these field trials. There are people here who have first hand experience of telemetry and its interpretation. Dr. Hardy, how do you see telemetry in field trial methodology?

A. Hardy: It is a very valuable tool as it enables you to follow an animal in a way that you cannot do with other methods. However, there are severe constraints on using the system. It cannot provide all the data that are needed. It is exceedingly technical, time-consuming and demanding in terms of skill and manpower and understanding the technology. Whilst I think it does have an important part to play in looking at predatory species, which are wide-ranging and have a lower density on the ground, there can be problems of misinterpreting the transmitted signals. The problem then is how big an area one ought to include. I really do not see telemetry as the total answer to the problem. It is a tool to be used sensibly to expand our capabilities of following animals for more of the time.

P. Edwards: I would agree with that. It is very difficult to decide what species you are going to look at and, as you said, it is a very specific and time-consuming technique. It comes in, perhaps, as a second stage. If a problem of a particular species at risk is identified then, maybe, that is the time to use telemetry. There may be no other method available to find out what the impact really is and why that species is sensitive. I think, at this state of the art, telemetry is a second line.

A. Cooke, Nature Conservancy Council: As a more general comment on methodology, one of the keys to what we are trying to achieve is the extent of the follow-up surveillance exercise that was mentioned earlier. This leads to differences in the effort that is put into exercises on vertebrates and bees, for which there is a MAFF scheme, compared with field experiments on whether invertebrates are affected. Those who wish to determine any effect of a pesticide on an invertebrate species, or group of species, have got to produce an answer. Nobody else is likely to do the work. We are in a position of having some concern about pesticide, otherwise we would not be asking the questions we are asking, so, in a sense, we are looking for some sort of reassurance. We say that there could be problems so please do these field trials and tell us if any do, in fact, arise. Even if the compound gets a completely clean bill of health at that stage, we move cautiously. I cannot see us ever getting into a situation where we spend half a million pounds on one particular exercise; that is not necessary in the UK context in order to achieve our particular ends. Also, when we do these exercises in the field, we should be looking at average cases as well as at worst case situations.

P. Mineau: There are two big differences between the USA and here. One is the lack in the USA (and Canada) of enforced post-registration surveillance schemes of any type. Secondly, you will find that the severity of the pre-registration screen in the USA is probably directly correlated with the difficulty of pulling a bad chemical off the market once it has been registered. In the UK you are moving in that direction and in another 10 years from now you may be thinking along the same lines.

W. Pflüger, Bayer: Flexibility will not be encouraged if you exaggerate the pre-registration demands on products. We can only simulate certain scenarios, the typical worst conditions. However, most problems, or the most spectacular problems, arise in exceptional conditions which we do not, and cannot simulate. What we simulate, repeated on 10 or 30 sites, is a sample of what operators cause with controlled application. It is better to refine our ability to predict from stepwise testing and then have better surveillance and better methods to estimate the effects.

P. Mineau: I disagree. Two cases that I can cite involved compounds where there was very good evidence that, regardless of when and where they are used, their side effects would have been identified had the pre-registration studies been adequate and more detailed. They were not unusual circumstances. The particular cases that the EPA is now going after are not cases which occur once in a blue moon. They result from the regular use of those compounds.

P. Jepson: Can I respond to the point made earlier, that entomologists should be commenting on what constitutes a significant effect? There is a big danger that we look at the type of data that have been collected on immediate effects, the type of IOBC methods which measure mortality of organisms. The point that I tried to make this morning was that, in order to focus on the organisms that we ought to be concerned about, we should be looking at their capacity to recover. Research is needed to understand the way in which communities recover from pesticide treatments, before we can say what is a significant or non-significant effect. A phrase like 'step-wise testing' fills me with trepidation. People round Europe at the moment are beginning to establish criteria such as percentage mortality and immediate effect as an index of the hazard posed by a compound. These are not relevant criteria. If an organism recovers within a certain time, or has the capacity to recover, at the commercial scale of use, then we need not be as concerned as with products that might reduce the rate of recovery or prevent recovery in the long-term. Boxworth data are just beginning to show that this type of effect may occur with repeated use of a range of products. I am trying to be constructive, but we are at the stage when this research is in its infancy and we need to develop the ideas a little further first.

P. Stanley: There is just one topic to consider before we leave field trial methodology. That is the statistical side of the approach. Dr Cousens had harsh words to say about ecologists and agronomists. Can I ask for his views on the EPA approach to the number of sites, to give some sort of statistical confidence in the return, as opposed to the one site intensive field trial which we have seen in Europe in the past.

R. Cousens: There is no one answer. For every given objective there is a different answer. If you want to get a basic idea of what happens over a lot of sites, then you use a lot of sites. If you want to really understand something fully then you have got to study it in detail. How many sites you do in detail is debatable.

P. Stanley: The EPA proposals were subjected to public consultation and a number of eminent American statisticians advised them on this approach. It has gone through in the regulatory documents unchanged, as being a valid approach to giving statistical confidence in the identification of an environmental hazard.

R. Cousens: I think, as in talking to any statistician, it depends on the way you put your question to them. If they were asked to identify how many sites were needed to categorically show, within a 95% probability, that there is or is not an effect, they probably gave that sort of answer. If a lower confidence had been required then they might have given a different answer.

P. Stanley: How does the approach of looking at a one site wildlife trial without a statistical appraisal, appeal to you?. The data from the pre-imposed treatment can be looked at in terms of whether there is some

sort of statistical effect but you lose your opportunity to look at variation across a number of sites.

R. Cousens: If you are trying to understand something, then you limited to the small number of sites that you can look at with the required precision. If you are trying to show that your chemical is safe to use across a number of different soil types, then you have got to try it on those different soil types. It is certainly ridiculous, when testing a chemical over a large number of sites, to choose them all to be roughly average sites. You are unlikely to reveal the interesting exceptions. Extreme sites where you might expect the pesticide to cause effects must be included. Lack of effects those extremes, shows a lot more than if only average conditions are investigated.

P. Stanley: Surveillance exercises have been done in the UK involving a programme of observations at a number of sites when a chemical has been used on a commercial basis. The farmers concerned have been very careful in their pesticide applications, which is not consistent with what you are saying. It is a 'tame' set of sites, perhaps not showing the extremes that you would wish to see for a proper appraisal.

R. Cousens: As a politically-aware person I would love someone to say that you have got to study 35 sites intensively. That would give a lot more jobs to young scientists. I am certainly not going to say that you should only study a small number; the more the merrier for scientific rigour.

P. Mineau: I agree with your comment on farmers being very careful. There is a way to deal with that. There have been published investigations looking at the pattern and distribution of the emission from sprayers. Farmers have applied standard rates and the variability that resulted have been assessed in relation to the equipment and the operator. The data produced show that you should probably be looking at 1.5 or 1.6 times the normal rate if you want to be in the 30% tail of the normal distribution. Thus, there are ways to weight the initial application rate, as one factor, to try to get a worst case situation.

R. Heitefuss, Göttingen: I would like to make a general comment. The topic of this symposium is 'Methods to assess the environmental effects of pesticides'. It would be not very wise to concentrate only on this topic. All we are talking about here is defensive research; we have to defend a compound. Can we ask the other question and say 'how can we produce or formulate or apply a compound so that these environmental hazards are reduced?'. We know that carbofuran has a high acute toxicity. If uses as a granular formulation it is bound to be toxic to birds, especially if ten to twenty percent remains on the surface. Why don't we try to formulate it as a liquid and then spray it only in narrow rows, just above the cornseed? This would reduce the environmental impact much more effectively than anything else.

P. Stanley: Dr Stephenson's contribution earlier was a particularly elegant example of a change in formulation reducing the hazard to fish.

P. Mineau: If the EPA scheme is going to distract us from this sort of research then that is bad. I agree that this is the direction to go. The reason for using granules in North America was to get a timed release. My understanding is that micro-encapsulation can now do just that. I am not aware that anyone has looked seriously at replacing granular toxics with

micro-encapsulation. It is certainly something that should be seriously considered.

R. Heitefuss: In Germany there was a problem with bird damage from aldicarb. The formulation was changed using a less active form which was incorporated so that not so much was laying on the ground. Much of the problem was then solved.

P. Stanley: That was a very successful development; moving to carbon-based granules and incorporation really did solve that problem. We were also involved in that in the UK. Can we now move on to the problem of reaching a decision on the significance of the results of field trials registration? Registration departments require a framework within which they can make sensible decisions. There was a problem a few years ago with microbiological testing, where there is a plethora of tests and registration authorities were guilty of requesting information which they could not interpret. Mr Greaves and Dr Somerville, among others, were involved in a venture to bring together an expert group which met to consider this problem and produce some recommendations. Would you care to comment on what that achieved?

M. Greaves: The essential concept that was used as our starting point is that, before you can do anything about interpreting data on side effects, you must know what is normal. In the case of the microbiology of the soil we were, perhaps, in an advantageous position compared to entomologists or bird biologists. There was a vast amount of data already available. Professor K. Domsch found thousands of references, going back over many years, all of which were relevant to some degree to the problem. After a long period of very patient work building up a database, he was able to show that there is a spectrum of 'background noise' in microbial populations and microbial physiological functions. Analysis of that and comparison with measured side-effects, (admittedly mostly in laboratory conditions, but with some in the field), allowed some fairly broad guidelines to be established. Essentially, these say that the magnitude and the duration of pesticide-induced side effects should be measured separately and then compared with comparable values from the 'background noise'. This includes the effects on the microflora of ploughing, fertilising, drought, flooding etc., - all 'acceptable' phenomena. The argument is, in simple terms, that if the pesticide-induced effects, are not greater than the magnitude or duration of background variation, they are tolerable. The inclusion of duration of effect in assessment means we are considering recovery. This has been talked about in this meeting but not yet resolved. In terms of microbial functions in soil, 30 days (i.e. 3 average generation times of 10 days) to recovery to full function after natural stress is quite normal and 60 days is not unusual. It is, therefore, quite reasonable to say that persistence of pesticide effects of up to 60 days is tolerable; longer persistence is not. That concept is now being accepted widely by the registration authorities throughout Europe, even though the question it begs to answer is 'what do you do if you get an intolerable effect?'. Do you conclude that the chemical is not acceptable or go to tiered testing, perhaps by going to the field and trying to confirm the laboratory results? Until this meeting I would have said that there was no way you could use field tests, but Dr Grant has convinced me otherwise. He has proved to my satisfaction that appropriate microbial tests are sufficiently robust to use, in very simplistic forms, in the field. I cannot finish without saying what I am sure Dr Stanley is expecting me to say. That is that, in general, I do not think that the methods and tests that are available at present have any value for regulatory purposes. Thus, the question of interpretation is a void one.

However, the approach and concept that Professor Domsch has developed is extremely useful and could be applied to any class of organisms. This should be given serious consideration.

P. Stanley: This principle, which has been very helpful in the registration field, is that by looking at the magnitude of pesticide effects in relation to the magnitude of effects due to ploughing or other agricultural operations, it has been possible to say that the effect of the chemical is not of biological significance.

M. Greaves: I did forget to say that statistics do not enter into this. We are perhaps arrogant in claiming that, as microbiologists, we know that an effect is biologically significant without needing statistical confirmation.

P. Stanley: Staying with the invertebrates, it would be very helpful to have a similar framework within which to judge effects on cereal arthropods, for example. That is, could one look at arthropod populations within a framework of effects of normal management practices, in relation to the observed effects of pesticides or pesticide treatments?

P. Bunyan: This concept may be something that could be fed into Dr Brown's model. What happens if you put ploughing into that model, do you think it will work?

R. Brown: The model is so simplistic that, essentially, any kind of density-independent mortality, such as ploughing or pesticides, is just an additional mortality factor. Ploughing itself reduces soil invertebrate populations ten-fold. Maybe that conforms with Mr Greaves' concept.

P. Bunyan: Making regulations is simplistic. What we really need are some broad guidelines. We have heard a lot of very detailed work on invertebrates these last two days. At the end of the day, though, we needed some principles on which we can make decisions.

P. Jepson: I would like to comment on the ploughing versus pesticide comparison. With pesticides we have options, whereas with ploughing, perhaps, we have fewer options. Therefore, it is still relevant to ask whether one pesticide is more or less toxic than the alternatives that are available. Although in agriculture we are looking at damaged or manipulated habitats, the arable environment in the UK is a very important habitat for wildlife. Put in the hands of people who do not take a biological view, there is a risk of saying 'well, if ploughing kills them all anyway, what is the point of worrying about pesticides?' Can I just take up the point Mr Greaves made about the value of work that has been done on microbes? As we progress between trophic levels, one awkward factor is that the organisms become less localised, less restricted to a lump of soil. That is one obvious reason why we need to have good research and development on recovery rates. They come in from all sorts of places. We can't just look at an initial rate of recovery in a field and treat that as a standard enclosed system. Also, there are problems with the invertebrates because of the taxonomic and ecological diversity of the group. We are beginning to find ecological groupings where we can define the hazard of exposure. We are beginning to move towards a way of deciding what methods can be used for a particular group. It would be foolhardy to come up with a single, simple, solution quickly now. What we should be doing is describing the problems and beginning to tackle them.

P. Stanley: I agree with the reasons you give as to why one can't give a simple decision now. The problem is that the Environmental Panel of the Scientific Sub-Committee is faced with the need to make decisions. There are notifications and one has got to arrive at answers now, not to-morrow.

P. Jepson: To the best of your ability, you must do that with the information that is available at the moment. Clearly all the criteria will improve with time.

A. Burn, Cambridge University: The obvious rider to what Dr Jepson has said is that we are not simply considering whether a pesticide should be registered or not. We have a range of options of use that include different types of pesticide and the use of strategies of supervised control. The question, as far as insects are concerned, is whether we should use supervised control or insurance spraying. We accept that pesticides are there and that they kill insects, but is there any advantage in supervised control rather than insurance spraying? That is what we are discussing, not whether or not to use pesticides.

P. Stanley: I accept the comment. How do people here feel about the problem we are faced with constantly in doing field trials and in registration? In these areas chemicals are dealt with on an individual basis and yet, in practice, they are used in programmes. Is it valid to do short-term field trials to look at the effects of an individual chemical, when that chemical is to be used as part of an overall programme? Is it that one cannot tackle the overall programme in field testing? Is the short-term test on individual chemicals a poor substitute?

N. Sotherton: It is again a matter of defining the question you ask and deciding whether you can or cannot define the organism at risk. First of all, I would very much like to back up Dr Jepson's argument about ploughing versus pesticides. You haven't got one choice that you must accept or reject. You have got a multitude of choices. With examples like the Partridge work, that I have been involved with, you can look at individual cases because you are looking at the effects on a food source. We are lucky, like Mr Greaves, in that it sounds like a relatively simple system. In general, though, if you can define your problem and the question you are trying to ask, then it is the possible to devise a way of working it out. I get very worried when I hear over-simplistic comparisons of pesticide impact with ploughing being advocated.

R. Brown: In this situation, you are looking at the pesticide impact on a species which is demonstrably able, for whatever reason, to cope with the strictures of ploughing. I wouldn't get too worried about that one.

P. Stanley: Can we move on to vertebrates? There the decision-making, at times, is even more difficult. For example, there are two wheat seed treatments in the UK; these are organophosphates used against the wheat bulb fly. One is chlorvenfinphos, which was demonstrated very early on as toxic to pigeons. It is used widely in the UK as a seed treatment and every year there are incidents involving feral pigeons. That is deemed acceptable. The other compound is carbofenothion which, particularly when it was used in Scotland as a replacement for dieldrin, led to the deaths of wintering geese. We happen to have the world population of pink-footed geese wintering in the UK and there are also other important goose species. In a number of incidents a proportion, perhaps 5%, of the world's population died. That was unacceptable and the chemical was withdrawn from use in Scotland to protect the wintering geese. This sort of decision on

what is acceptable and unacceptable is, at best, an informed guess. Dr Cooke has a poster which draws attention to woodpigeon casualties from seed treatments, particularly chlorvenfinphos. I would like his comments on whether he regards the use of chlorvenfinphos, and the attendant pigeon casualties, as acceptable, in a biological sense, both in terms of local and national populations.

A. Cooke: I would agree that acceptability for vertebrate mortalities depends both on the size of the effects (that is, the degree to which the population is affected), how long it takes to recover, and its perceived importance. Part of this is measurable, if we know enough about the basic biology. It is more subjective when we try to define where we set the threshhold. If every woodpigeon in Britian was killed that would be unacceptable even though it is a pest species. The particular incidents that I am describing, I would regard as being acceptable because there are only a few instances a year. The idea of the poster was not to draw attention to the size of something that might be acceptable but to point out that, if one looks in detail at a relatively abundant species, one can find incidents that are not being found by the Wildlife Incidents Investigation Scheme. Between the extremes of one or two incidents a year and everything dying, there are obvious grey areas. What we do within registration is to work with the companies. Dr Stanley mentioned the case of the pink-footed goose and carbofenothion. Here is a species which is rare and is important and relatively small mortalities will be significant. What I am trying to do for the future, is rather than to deal with each problem on an *ad hoc* basis, is to define what is acceptable, and what is obviously unacceptable. Between those two extremes is the grey area and we must discuss how we can try to reduce the boundary of that grey area. So much is based on subjective judgement, which different people see differently.

P. Stanley: Presumably with the geese you have got a very good idea of their population size. You have a lot of information on which to judge the impact of the incidents. With the woodpigeon you haven't got anything like the depth of information necessary to reach a decision.

A. Cooke: With those geese, perhaps 1% of the population were affected. If 1% of our woodpigeons were affected, then very soon it would be a public relations exercise. It would be a policing exercise, rather like mute swans dying after eating fishing leads. That is not a conservation exercise, in that it is not affecting the swan population. It is an issue of animal welfare and public perception. Every year we have three thousand mute swans dying a very nasty death.

P. Stanley: The geese are a good example for modelling. There is very good information on where they breed, their breeding performance and what controls their populations. There are very few vertebrate species for which you have got that depth of information. A number of people were advocating models, but there are others here who may have different thoughts. Should we be trying to develop more models for individual species? Martin Smees (Shell) developed models which are applicable to tawny owls in woodland and one or two other predatory birds. These were quite educational in looking at the possible impact of organo-chlorines on predatory birds, and the effects of taking just a few mature breeding females out of the population, giving a knock-on effect for a number of years. Is this the approach we should be using for vertebrates, to give the confidence to take decisions in the registration field?

A. Cooke: This is one of the effects we certainly should take into account, particularly when we are talking about species of conservation importance. For example, with slow-breeding birds of prey the mortality of adults may be much more important than sub-lethal effects. Thus, DDT had effects on egg shell thickness of the peregrine and yet, when it started, the peregrine population was increasing. Then 10 years later, when the cyclodienes were used as seed dressings, they caused poisoning incidents and the population decreased. You have got to take that sort of thing into account. Obviously, if there was information that a material was in the environment and was causing sub-lethal effects, then we would be very worried if it was affecting those species, even it is wasn't reducing their population.

R. Cousens: As a modeller, I am extremely worried that there is so much interest in the use of modelling in some of these areas. Models are basically no more than using mathematics as a best guess. This is usually the case if you do not validate your models. Its predictions are then useless and, certainly when it comes to any sort of judicial proceedings, you would be in difficulties if your assessments were based on an unvalidated model. Unfortunately, it is not possible to validate some models in terms of whether or not a species can be driven to extinction. I am worried about people seeing modelling as being a wonderful tool that they can all use. It has got a lot of limitations.

P. Jepson: Models do generate testable predictions. They permit us to understand what is going on by exposing areas of weakness. That is a very positive feature. With invertebrate field tests, I foresee that we may be able to go down to a very small scale to examine key parameters, such as aspects of the physical chemistry of the pesticide and the exposure and uptake of particular organisms, that are most likely to influence the risk of a long-term effect. That is one case where models could be used.

R. Cousens: What you are talking about is validating the models and using them as an investigative tool.

P. Jepson: I agree with you and would personally be opposed to anyone using them to justify, at the final stage, whether or not a pesticide is passed on to registration or not. That would be disastrous and naive.

P. Mineau: Dr Cooke is saying that if there are two cereal treatments for a particular use pattern, there is a choice if one is better than the other. Presumably, when third, fourth and fifth products come in, and each is better than the second one, then you will favour it in those areas where you have high woodpigeon mortality. This continuing replacement with better, safer, products as they arise has to be kept in mind. Dr Sotherton says he is concerned about the plough versus pesticide argument. That argument is being made in Canada now with respect to vertebrates and birds in particular. I was in a Court case recently concerning a certain turf insecticide and the arguments were that TV towers and motorways kill birds, just as a pesticide kills. Additionally, it was argued that, if you really care about the birds, you should have have left the area as a native forest and not felled and turfed it. This is a slippery slope. We have to keep track of the eventual benefit of the treatment, not necessarily to the manufacturer who is making that compound, but to society as a whole.

R. Brown: Having got some idea of effects, the benefits of the treatment must be weighed against the risks. Sometimes the benefits are clear but, in many cases, it is difficult to define them precisely in money terms.

Putting the risks into such a scheme is incredibly difficult.

Returning to the issue of models and validation, there are two sorts of models. There are simulation models of, say, plant growth or soil functions, which can be verified. On the other hand, strategic models (e.g. rabies or German Measles epidemiological models) cannot possibly be validated, yet they are extremely valuable when a decision must be made. There is a role for making those decisions in areas where lack of time or experimental ability prevents determination of effects directly.

A. Hardy: If mortality can be demonstrated as an effect of a pesticide, what significance has that in the natural mortality that that species is likely to experience, whether it is in the adult life cycle or generation life cycle? This is certainly one area where population modelling can have value, making maximum use of the biological information available to us in assessment of those mortality factors and in identifying potential significance of that particular pesticide effect. Then we go further in validating it. I can cite one example; following a 20 years' study of woodpigeons over a very large area in Cambridgeshire, we have a very good population model which demonstrates clearly one of the major factors driving mortality in that particular population. Therefore, we can put shooting, for example, into the model as a factor relevant to effects in the field.

P. Stanley: As I am not now involved at first hand in regulatory decisions, I can be rather more provocative in some of these issues. A number of useful points have emerged and, I am sure, a number of you sympathise with those who have to arrive at decisions on a day by day basis, particularly when it comes to invertebrates. This is an area where much time is spent and where some good guidance is needed. I would like to finish by saying that this meeting has one particularly helpful aspect. This is that it will result in papers appearing in print dealing with actual field trials and field trial methodology. Much development of field trial methodology, and much information obtained using this methodology, has been developed by companies. In many cases they did not wish to publish it in the scientific press. Many of the papers that will be published in the monograph are helpful. I hope that the people present, particularly those from the companies, will take the opportunity, whenever possible, to publish some of the very valuable wildlife trials which I have seen, on a confidential basis, through the registration process. That is the only way the process of developing methodology and the approach to hazard assessment will improve across the board.