COLLOQUIUM

Dr. Helmut Zwölfer, Moderator
CIBC Delemont, Switzerland

ZWOLFER Let us begin our colloquium with questions dealing with host specificity. Yesterday Professor Barr raised the question relative to the danger that a phytophage, once established in a new ecosystem, could change its host pattern. This is an important point which has not been discussed in the previous days.

As regards host specificity we should distinguish between the following aspects:

a) Size of host range. This term refers to the number of plant taxa, particularly supra-generic taxa, included in the host spectrum of a phytophagous insect.

b) Type of host pattern. The host plants of a phytophage may be closely related (= phylogenetic host pattern) or they may be taxonomically unrelated (= disruptive host pattern).

c) Variability of the host pattern. Individuals of a given population or local populations may differ with regard to host specificity or host preference. As a rule this intra-population or population variability of host specificity concerns minor differences in the size of the host range. Thus, some of the populations of *Urophora stylata* (F.) (Trypetidae) studied by us, are able to breed on *Cirsium vulgare* and *C. arvense* whilst other population are not capable of breeding on *C. arvense*. In Delemont we found a population of *Cassida rubiginosa* Muell. where some individuals could develop to maturity on leaves of *Centaurea jacea*, whilst other individuals needed *Cirsium* spp., the normal host plant of this Chrysomelid, for their larval development.

d) Phylogenetic stability of the host pattern. If a phytophagous taxon has co-evolved with its host plant taxon, or if it had remained associated with it over geological periods, the host pattern can be called phylogenetically stable. If, on the other hand, there have been major host transfers during the evolution of the taxon, the phylogenetical stability of its host pattern is relatively low. Criteria for the relative stability of host patterns of phytophagous insects can be found by comparative studies of the biology and distribution of the phytophage and its relatives.

Professor Barr, would you, please, elaborate the question you raised yesterday?

BARR There are several concerns. The intial concern yesterday was in the matter of testing, with the procedures or method that is employed in trying to ascertain if the insect that you would like to release is safe for all intents and purposes. Petri dishes or small cages under laboratory conditions usually do not allow an insect to function as it would in nature. Obviously in some cases there may be very intricate behavior patterns associated with mating or oviposition. It may be necessary for some kind of feeding or other factors to take place before an insect is able to oviposit, and under these circumstances you may not be getting a true picture of what is happening in nature. I don't think any problems have arisen as yet, but they may in the future and certainly I think we should take advantage or be aware of the biological variability, and I'm speaking in terms of behavior here and the general ecology of insects. You are all aware of this obviously, but sometimes these pressures that are upon you, prevent you from really conducting your work with this thinking in mind. Now, the points you raise here, obviously are something above and beyond pure procedure and method of testing. This is taking into account the obvious biological nature of not only the insect but the host plants. These are interrelated and certainly have to be taken into consideration. I think we have two points of view here that have to be considered. One is the pure methodology of testing and the other is the overall biology of the organisms involved, but these are interrelated points.

ZWOLFER The first point would be your question whether laboratory methods of screening are conclusive for the behavior of insect populations in the field. Who would like to comment on this?

SCHROEDER Feeding tests are one of several procedures in checking the specificity of an insect. With feeding tests in petri dishes, small cages, and so forth, the insects will be under stress. We often observe in feeding tests a certain mortality on the control plant, let it be 50%. The question is if this 50% of the insects isn't that part of the gene-pool which under field conditions would react otherwise than the remaining 50%. It would be ideal if the stress which is superimposed to the insect in the feeding tests is similar to the stress in the field. If the stress which is superimposed on the insect in the test differs from the stress in the field we may observe later on in the field some feeding on certain plants which had not been attacked in the tests. This, however, didn't occur as far as I know, but it could occur. Otherwise I think in feeding tests insects are forced on plants which they never touch in nature, to which they never get in nature. The results of feeding tests may, therefore, be rather secure.

GERLING I just want to ask Dr. Schroeder how he would overcome this problem of stressing in the laboratory and he would be able to learn how the insects would react without a stress.

SCHROEDER For the time being I don't know. I think there will be stress anyway. If we can't take our tests to the field we have to do our testing on potted plants and there will be some stress. We can only simulate and get as close as possible to natural conditions but we never can get that. Maybe our distinguished colleagues know more about it.

CARESCHE I agree entirely with Dr. Schroeder. I am bound to say that putting insects in testing cages does not necessarily imply that stress is caused. It is a matter of the insect involved. If the insects to be tested are put in cages large enough, the stress is not very important or does not occur at all. When we tested an aphid, we put the aphid both on Chondrilla and on the wild or cultivated plant to be tested in rather large cages. The cage size was 60 cm high 35 cm wide and 40 cm long so they had plenty of room, and the behavior of the aphid on Chondrilla juncea appeared to be quite normal. When we experimented with the midge Cystiphora schmidti we did the test in the insectary room without cages. So, I think that the stress can be avoided to some extent.

CAVERS There is one problem that I wish to mention here. I think the testing for agricultural or crop plants is probably very adequate in most cases but, if you are introducing a species that is going to be effective against a certain weed then the close relatives of that weed, whether they be weeds or simply innocuous plants, are the ones that should be looked at more closely. For example, if you introduce something into North America to control curled dock (Rumex crispus), there are approximately 20 native species of Rumex in the sub-genus Lapathum in North America and each of these has its own organisms which feed on it and are supported by it. If something is introduced from abroad it might be far more damaging or dangerous to one of these species than the target weed species, which is cosmopolitan and which is subject to the control organisms in its own centre of origin. For this reason the close relatives and not necessarily the economic plants should be studied a bit more. By such a study we may anticipate problems which will arise as people become more concerned with wildlife and with species which are outside the immediate interest of agriculture.

DUNN I interpreted Dr. Barr's remark to refer to the unknown stresses that we are putting on insects, rather than known stresses which Dr. Schroeder seemed to think we were talking about. I believe that these insects are so delicate that the minute you put them in a cage, no matter how big, you change their environment and subject them to stress. There is always the possibility of erratic behavior induced by these occult stresses. This is why the CSIRO Chondrilla unit and we are in Europe. We make certain experiments in the field, without cages, so we have just about as natural a condition as you can get. I concur with Dr. Barr, that the minute you put insects in a cage, you really don't know what you are doing. You can get two things from cage tests, positive data and fuzzy data. The positive data you get are when the insects starve and the fuzzy data are when they live for a long time and feed and/or oviposit. It's my feeling that the only thing we really get from a starvation test is if the test insects die on the non-host plants and they live on host (control) plants.

WAPSHERE First of all, I would like to make a comment on what Dr. Schroeder said. He said the testing of cultivated plants was only a small part of the work involved, and scientifically this is true, but in actual practice, one finds that testing sixty-two or more plants involves a lot of time, a lot of labor and a lot money. In fact, you have to make a choice between testing these plants or doing scientific work, and if you have to test these plants anyway, you cannot do the other. The second point is that I agree with Paul Dunn that we are trying to demonstrate whether, in fact, the organisms will attack the cultivated plants under field conditions and this should be our main aim. As close as we can approximate to this, by putting plants in the field, the better. There are, of course, difficulties. For instance, I don't know how you would do a survey in the field to see if the cultivated plants are attacked by the *Chondrilla* root organisms. If you have to dig up many plants of each cultivated plant to prove your point, it becomes easier to do the tests in the laboratory.

WATERHOUSE I would like to support the views that Mr. Dunn advanced. We try, wherever possible, to have the specificity tests carried out in the country of origin of the organism This does have quite a number of advantages, although there are some problems. One of the advantages, for instance, is if you are examining a fungus you don't have to have a top security laboratory in your own country. In most cases I don't think the quarantine security would be good enough to bring in, for example, a powdery mildew or a *Puccinia* and feel quite sure it wouldn't escape into the environment while it was being tested to determine whether or not it was safe. So, for that sort of organism, unless you've got a really top security quarantine, of which there are very few around the world, you are obliged to carry on the trials overseas. I would agree also, that if you can make observations on the economic plants growing in association with the weedy species, and can see that they are not attacked under these conditions, this gives you much better assurance that, over the years, there hasn't been any strain of the organism which does attack the crop species. Also, of course, the crop species has a whole variety of stages of growth and sometimes the older leaves or the leaves with less carbohydrate or more nitrogen or something like that may not just happen to be available when you are doing a test under laboratory conditions.

An occasional disadvantage in testing an economic plant in an overseas country is that there are other organisms in the environment that do some nibbling or chewing. Dr. Harley carried out very similar trials with *Lantana* insects in Hawaii. The economic plants were grown in pots and then taken out into the areas where the *Lantana* insects were present in profusion. The pots were placed so that the foliage and the stems were actually intertwined with the *Lantana* bushes. One of the insects that was being examined was a stem boring beetle, *Plagiohammus*, which produces feeding scars on the *Lantana* twigs. There was, however, a different sort of nibbling on some of the economic plants and this was caused by one of the local grasshoppers. Attack by other polyphagous insects in the environment must be clearly distinguishable. However, there may be some countries where this sort of field testing may not be appropriate just because of such interfering organisms.

DREA I think in some projects you don't have any choice. We worked with *Halogeton* insects in North Africa and many of the plants to be considered were not crops. They were range and forage plants. We were unable to raise many of these plants in the laboratory and they didn't occur naturally in Morocco where the insect existed. As a result we were forced to test material in quarantine. Although it would be ideal to check the insect under field conditions, one can't always do this because of the nature of the crops with which one is often involved.

ZWOLFER When speaking of screening tests we should specify which aspect of host specificity we are investigating. Screening tests may assess:

i) the larval feeding range

ii) the adult feeding range (does not apply to Diptera and Lepidoptera)

iii) the oviposition range

iv) the range of food plants which allow maturation of the ovaries.

The last type of screening test is probably only little or not at all biased by the laboratory techniques used. In many phytophagous insects a specific type of food is required for the development of the ovaries, hence ovarial maturation can be a very useful criterion.

ANDRES Everybody is aware that in confining insects to certain plants we short circuit the variety of processes an insect may require in locating and determining the suitability of a potential host plant. However, the close confinement of insect and plant assures any review committee that indeed the insect has been in contact with the plant. Placing test plants in the field to see if they will be attacked has limited value unless you can observe the plant for extended periods of time and note whether the insect actually came in contact with it or not.

On the question of stressing insects in cage tests, observations on insect mobility will often indicate the degree of stress that may be occurring. If the insect is uncomfortable it will tend to wander. Periodic observations at 2, 4, 8, 16 and 24 hour periods and so on will indicate when the insect begins feeding on the test plant as compared to its normal host. This information is good and allows you to make some compensation for any stress involved. The review committee can take this into consideration during its evaluation. If feeding does occur, then the question of ovarian maturation, oviposition and larval development can be explored. Principal purpose of the cage test is to make certain the insect and test plant are brought together.

HUETTEL It seems to me that most of the effort in any of these screening tests should go into maximizing the response of the insect to its normal host plant. In this way one can be fairly sure that the experimental conditions are favorable for that insect. If 40% of the test insects die before laying eggs on the normal host, then negative oviposition on a plant being screened may be meaningless. If 98% of the insects complete a life cycle on their normal host then there is some basis for saying that normal behavior has been compared with the behavior on a non-host plant. Also, it seems very important to test populations of insects from different biological associations, because there may be intense selection against certain host selection behavior in areas of competition. Any population which may be used as a source population should be tested.

ZWOLFER As concerns the problems of selection we will have to go into more detail.

ENGLERT I want to answer to Dr. Wapshere and Mr. Dunn because I don't agree with them. I think natural condition tests are very nice but it would be very important to have unnatural conditions. In the case of successful biological control we will get rather unnatural conditions because if it really works out you get an explosion of the insect and then you have a situation that would never occur under natural field conditions, so I think it is better to be sure that even under such conditions you will get no damage on desirable plants. Testing abnormally in a small cage with few small plants and a lot of insects in them may simulate this overcrowding in the field.

SCHROEDER Perhaps Mr. Dunn and Dr. Wapshere can tell how they can get reliable data if they are testing the insect without confinement. I am asking how you are going to get quantitative data on feeding or nibbling in the field? "Insects in the field", this is the term Mr. Dunn used and I wonder how he gets quantitative data on it.

WAPSHERE It depends very much on what you're trying to establish. My criteria for introducing an animal or organism into another country to control a weed is that this particular organism would present no economic or social problems. In other words, it would not attack socially important plants. To my mind this means that it should not attack these plants under normal conditions of cultivation or normal field conditions, but I agree that if you can't approximate to this condition and if you feel the difficulties of demonstrating this one too great, all the better to put the organism under a stressful situation to try to force it to feed on a cultivated plant. In the case of most of the Chondrilla insects, they don't attack anything else. They are so highly specific that there is not this problem of differential feeding. However, if you need to quantify this in some way, or if you find you have feeding in the field on other hosts, then certainly bring your insects into the laboratory and see if you can show that it feeds only two times on one plant and 10 times on another.

WATERHOUSE I'd like to come to Mr. Dunn's defence with regard to the insects suddenly finding themselves without food, being in large numbers, and searching around on surrounding plants. This will be a very temporary situation unless the insects are able to live on an alternative plant. I think we have probably seen this with the attack on sesame and with some of Tribulus insects, in California, where there was a temporary attack, but this soon passes. On another point, we are comparing laboratory trials, some involving only five or ten larvae and often not many more than this, for plant species, with a field situation, although the latter admittedly does not ensure always that the crop plant is effectively contacted by the organism under examination. However, if you take care to carry out the crop plant exposures in exactly the same environment where the weedy species is under constant attack by the organism, then there is every opportunity for the organism to attack the crop plant. Often there are multiple contacts and multiple opportunities. There is no one method that is likely to be applicable in all situations but, where possible, field observations and field exposures do add a great deal to information from laboratory observations.

DUNN I would just like to come to my own defense a minute. I'm not a field test purist by any means, but the cage tests only support what we find in the field. As far as quantitative data is concerned in the field experiments we have made, we released a known number of insects on a group of plants without the host plant in the vicinity. The number of insects that stay or leave are our data. The insects left the test in the case I have in mind but remained on those host plant controls which were not close enough to attract the insects from the test plants. This is valid quantitative data in my opinion. As far as oviposition in small cages is concerned, we have never quite figured out how to evaluate it when the insects lay their eggs on the cotton in both the host plant control and the non-host test.

BUCKINGHAM I'm very pessimistic about the importance of field tests in the immediate future. Theoretically they are the ultimate tests, but with the limitations on manpower, money, time, etc. that we have today, I don't think that they are practical for the major part of a testing program. We are, therefore, still limited primarily to laboratory tests. I have a question for Dr. Schroeder concerning these laboratory tests. We say in the example of the tingids attacking sesame in West Africa that the petri dishes had to be rotated every so often to insure that the phototactic beetles would contact the test plant and not spend all the time near the light. Dr. Schroeder how are you or do you take this into account in your laboratory tests? Lloyd evidently does when he observes the behavior of the test insect to see how much time it spends on the host plant in comparison to the cultivated plants, but I think that most researchers set up their experiments and then merely check the amount of feeding.

SCHROEDER Well, I have the feeling that we got away from the starting point of the discussion. I didn't want to make a point for or against laboratory testing. The question was, "what means a laboratory test?", "How much security do we get out of feeding tests in the laboratory?" and my first statement was in this direction. I didn't want to say that we should do only field tests or only laboratory tests. But answering your question, oviposition tests are only meaningful if carried out with insects which need a specific part of the plant in a specific stage of development to oviposit. In this case oviposition tests will be useful. But if the eggs are just laid in a line or in batches on the stem or any leaf surface or on any part of the cage oviposition tests will be meaningless.

GERLING I would just like to add to what I think is the most crucial point here. Every insect or insect group has its own characteristics and one must balance them between the field and laboratory. What worries me in these cases is the manifestation of the stress in someways. For instance the matter of mating, either the insects may not mate or they may mate but not mate properly. In other words, to our eyes it would look like the regular mating process but the sperm wouldn't be in the spermatheca properly, and so on. That may, of course, not only prevent oviposition but cause abnormal ovipoaition. There are insects that feed differently when fertilized or not fertilized. We may perhaps prove that any of these happen because our cage isn't big enough, our ventilation isn't good enough, or something else we don't know about may produce completely different results. This is just one example. What would work for certain insects may not work for others, so I think it is just a matter of balance between the two methods, according to the individual species involved.

BENNETT Where we have a wide range of candidates I think that you must have a lot of flexibility in the way you go about doing tests. Yet, it would appear that we are attempting to establish hard and fast rules of how to go about this. While general principles sound fine it is out of the question that any single set of tests will apply to all candidate insects or other organisms. The other point which Dr. Waterhouse asked me on the side, "Why aren't all these things done?". Well, it becomes a question of finance. In countries where scientists are fortunate enough to have administrators who appreciate the problems involved finance is apparently not the limiting factor to the range of tests. Elsewhere it is usually not a question of convincing the working people that a species is or is not adequately tested but the people higher up, the ones who control the funds.

BALOCH There is one point I think we have not discussed in the host specificity test. There is a danger that, while we are testing insects and some five or ten percent develop in the feeding tests which may not be true under field conditions, we might be discarding insects which are potentially good just because they were forced to feed on the test plant.

ZWOLFER Before discussing this point, I would suggest that we draw some conclusions:

a) Before making any decision, we should thoroughly know our insect, its biology and, if possible, the biology of the related species. This would provide the necessary background information for any evaluation of the phytophage concerned.

b) We should try to develop different types of tests and should not rely on a single method. Carrying out tests in petri dishes as well as in laboratory cages and field cages means spreading the risk of a misinterpretation.

c) The results of our tests should be compared to the field situation and should be interpreted in view of the general background information available.

As concerns our work with thistle insects, we are in a fortunate situation, as we have a background information on the insect fauna of about 60 European and Asiatic thistle species (i.e. members of the Compositae tribe Cynareae). This is a considerable help when we are interpreting the results of our tests. Comparing the results of screening tests with several dozens of thistle insects with the available field observations we can make the following statement: As a rule the "experimental host range" (as obtained by working with confined insects which are restricted in their choice of food) is broader than the actual host range as found under field conditions. The "experimental host range" includes the actual host range and often also field hosts of related phytophagous species. Hence, if the "experimental host range" of the candidate species forms a definite pattern and is restricted to not desirable plants, there is little reason to worry about the safety of the candidate species. If, on the other hand, the "experimental host range" does include desirable plants or does suggest that the candidate species may be capable of a major host transference, then we shall either "throw it out of court", or give it another chance by analyzing its behavior more in detail. The decision what to do with a candidate whose behavior in screening experiments gives reason for doubts about its safety will depend on various aspects. The availability of other and perhaps safer candidate species, the relationship between the possible risks and the possible benefits and other factors will have to be considered.

Relying exclusively on laboratory tests may obviously involve the risk that we reject a perfect biological control agent.

ENGLERT I have a question for you. How do you decide if you have a crop in the potential host range?

ZWOLFER You have to balance the possible risks against the possible benefits of an introduction. E.g., insects associated with Centaurea might be harmful to safflower. As in Europe and Asia we had to stop growing safflower because there are too many pest insects attacking it; the Americans have now a monopoly for this oil crop. Hence, even if there is a slight risk that our candidate might damage safflower, we do not recommend its introduction to North America. When studying the host specificity of a Chaetorellia sp. breeding in yellowstar thistle, we analyzed its oviposition behavior and larval development in much detail, since at first this Trypetid appeared a promising candidate as in our preliminary tests it did not accept safflower. However, hybridization experiments showed that our species is very closely related to a notorious pest of safflower, Chaetorellia carthami Stack. Further tests showed that by certain procedures of conditioning females of our candidate could be induced to oviposit into safflower heads. First-instar larvae of the candidate species did not develop in safflower heads but second-instar larvae could be reared to pupation. We concluded that whilst it was very unlikely that immediately after an introduction to North America the species would transfer to safflower, the possibility did exist that strains adapted to safflower would develop. Hence, we could not recommend the introduction of this Trypetid.

On the other hand, when investigating Urophora sirunaseva Hering, another Trypetid species associated with yellowstar thistle, our results suggested that it was safe as far as safflower was concerned. However, the species might be capable of doing some limited amount of damage on ornamental Centaurea spp. This risk was perfectly tolerable and U. sirunaseva was given the permit for introduction to California.

As mentioned above, in Chaetorellia we found that in screening experiments insects may become conditioned as a result of the experience made during the course of the test. Would somebody like to comment on "habituation and conditioning of phytophages in screening tests"?

GERLING I wish to give an example of habituation which did go through. In this case I am referring to the Mediterranean fruit fly, which is of course a plant pest. When they started working on it in Israel in 1956 it was almost impossible to grow it in the laboratory for two reasons. It wouldn't grow on artificial medium and it wouldn't lay on anything but fruit. In about one and a half years after they started they managed to get the females to oviposit on plastic lemons in which holes were made and on a medium which was made from dried carrots and bran. A year later they got females which would oviposit through a piece of cloth and larvae which would grow on an artificial medium. For the last ten or eleven years, they have been producing about a million flies a week. As a result, they have a new animal because this fly is not as competent in the field as the field flies. My point is that this particular fruit fly was able to switch over to completely different oviposition habits, on cloth rather than fruit. Also, stimuli changed and the feeding habits are completely different, because it will readily grow in the medium that a field field population won't successfully grow on, unless you use tremendous numbers.

ZWOLFER This is highly interesting. However, I think it is mainly selection which is responsible for this adoption of new breeding substrates. Habituation may have played a role just in the initial phase of the process.

According to Professor G. Bush similar changes occurred in North American population of fruit fly species belonging to the genus *Rhagoletis*, when they had come into contact with new host plants. I am inclined to think, if one had tested *Ceratitis* or *Rhagoletis* in the laboratory, one might have found indications for such a capacity to adopt new hosts. In both genera host transfer has occurred quite frequently during their evolution, and now members of these genera are associated with many host plant families.

On the other hand, in most of the Trypetid taxa associated with thistles there occurs a high phylogenetic stability of the host pattern. There exist, moreover, many physiological, biological and morphological adaptations which tie the species closely to specific oviposition and breeding substrata. If adoption of new host species occurred, then these new hosts are relatively closely related to original host. In the genus *Urophora* sensus stricto (Palearctic species) and in the genera belonging to the Trypetid tribe Terelliini no major step of host transfer had occurred during the evolution of these taxa. Their many species are all associated with a related group of Compositae host plants.

Allow me some comments with regard to the stability and evolution of host patterns. It is regrettable that in the literature dealing with the phylogeny of insect taxa the evolution of host patterns is rarely discussed. One of the reasons may be that there are no catalogues of host records of phytophages on a world basis. In this respect our colleagues working with entomophagous insects are in a much more favorable situation. With regard to phytophagous insects we lack a comprehensive documentation on their biology and ecology.

In contrast to museum workers who usually have an ample material of preserved insects but only scanty biological information, we are working with living insects and plants. This fortunate situation should stimulate us to pay attention to problems related to the evolution of host patterns in phytophagous insects, to the formation of host races, etc. Perhaps we can give consideration to this at our next meeting.

WATERHOUSE The general question of whether or not apparently polyphagous insects are really able to live satisfactorily on a range of plants is a field for further detailed investigation. This subject is one that Professor Hal Gordon of the University of California, Berkeley, has been investigating, particularly with the milkweed bug. This bug can be maintained indefinitely on milkweed seeds. It can be fed on a variety of other seeds, but cannot reproduce for more than a generation or two on these other seeds. On peanuts, I think, or perhaps sunflower seeds, the reproductive rate goes down after a generation or two. Often at the end of the F1 generation the organism is no longer able to maintain itself effectively on the alternative source of food: perhaps this is an incipient extension of host range. Each case of apparent conditioning extending the host range really should be carried through to the F2 or, perhaps, later generations in order to be sure that there really has been a modification of habits and that the new strain is able to continue to maintain itself under the new conditions. Quite possibly, even if an insect attacks a new host plant, this may not provide all of the necessary nutritive factors to enable the production of a new strain attacking only that plant.

ZWOLFER Let us change the topic. I think Mr. Huettel made the proposal to discuss the use of inundative releases of insects against weeds. Would you, please, comment on this?

HUETTEL It seems that there is a difference of opinion among the people here as to whether this can be used or not. There is also some question about whether we can control native weeds with native insects. It seems that the inundative method might be useful in this situation. By this method the reproductive capacity of the resident parasite populations might be overpowered and the host plant rapidly killed out. If it is an introduced weed and you're introducing an insect on to it, then by inundative releasing it seems possible that the chance of native parasites switching to this large new population of introduced insects would be increased more than if you inoculate with just a few insects and let the population build up naturally.

ZWOLFER We are vaguely considering such a procedure with *Urophora cardui*, a monophagous Trypetid which mades stem galls on *Cirsium arvense*. We have introduced this species into the Delemont area (where it did not occur). We found that after one to two generations chalcid parasites associated with a second, native *Urophora* sp. passed over to the newly established colonies of *U. cardui*. At present this environmental resistance formed by two native parasite species is the most important mortality factor operating on our experimental field population of *U. cardui* in the Delemont region. Nevertheless, if we could develop a cheap method of mass production of *U. cardui*, it might be worthwhile trying to inundate stands of *C. arvense*. There is little doubt that on the long run we would not be able to maintain high population densities of *U. cardui*, but if we could produce such high densities just for a few generations, that might be useful, particularly at sites where *C. arvense* is already under environmental pressure (e.g. in pastures where it is exposed to competition with grass species). Of course, the question remains open whether one could stabilize the thistles at a low level. Perhaps Dr. Andres could comment of this.

ANDRES We really haven't tried inundative releases, only large inoculative, releases, and then only with *Agasicles*. The alligatorweed host and the beetle are killed back each winter in the more northern areas of the plant's range necessitating annual reinoculations. Where we have transferred large numbers of beetles, we have had no particular parasite problems, although coccinellid predators have had some impact on the population. It is still too early to say whether we will attain satisfactory control in the areas of annual reinoculation.

WATERHOUSE Surely, the possibility of inundative release does exist, but it would be a matter of economics. If it were possible to mass rear the insects cheaply on artificial medium, so that you could get enough for a massive release at almost no cost, this might be practicable. If, on the other hand, you had to grow hundreds of acres of the weed, in order to crop insects from it, under conditions where they wouldn't be parasitized, and these would be sufficient only for an equal number acres when you liberated them, I think that it would come down very rapidly to a matter of economics. It is the same situation in inundative releases of parasites of insect pests.

ZWOLFER It may be a future development, once we have gotten rearing methods on artificial media.

WATERHOUSE If you can rear them artificially in such a way that they are then going to be as effective in attacking the weeds as the natural strains.

SCHROEDER If we are able to rear the insects cheaply as was expressed by Dr. Waterhouse, there are some other important points. First, the insects must be able to suppress the weed quickly. Secondly, it is important that the area which has been cleared of the weed is immediately occupied by other plants in order to prevent the weed from reinvading the area.

SANKARAN I am interested in this subject because as Dr. Waterhouse has said, one could think of breeding these phytophagous insects on artificial diets instead of the method mentioned in my paper. We have a problem with a noctuid which feeds on Striga. The population of this noctuid, Eulocastra argentisparsa, is low in the field and it has been suggested by those who review our reports, in the U.S. Department of Agriculture, that we might develop an artificial diet for this noctuid. I wonder if you could really breed a phytophagous insect, which is supposed to be host specific, on an artificial diet; does it not mean that the insect is not really specific and that it might as well switch over to plants other than the target species.

ZWOLFER I think you may very well raise a highly specialized phytophagous insect, even a specialized entomophagous insect, on artificial medium. This has been proved now in Canada. You may still have a high amount of host specificity, because host specificity is largely a function of orientation stimuli. When working with artificial media, you do not have this orientation component. In the field, insects have to use their orientation system to find the hosts and then many insects respond of course to deterrents or feeding stimulants. An artificial medium would be free of such deterrents being a bland nutrient, such as lettuce.

WAPSHERE Dr. Waterhouse has mentioned the problem of cost, but there is another problem which Dr. Schroeder mentioned in passing which indeed increases the cost. You may be able to suppress some weeds by introducing an insect to them in an inundative way, but then you would have to keep this up every year exactly as if you are using a herbicide. I may mention the case of Xanthium. There would not be a suitable plant to replace the Xanthium in Queensland and as soon as you took away the insects, you would have every reason to believe the plant would return to its former density after a few years. The program of rearing would have to continue for as long as you wished to control the plant.

BENNETT Dr. Wapshere's comment about the lack of competition apparently does apply in many of the aquatic situations. I mentioned the work going on in Florida and the possibility of the inundative releases of the snail, Marisa, for weed control in selected ponds. In these it is partly inoculative and partially inundative. As the populations in most of the areas will be killed out by the cold weather every year, it will require annual releases. If the importance of having areas weed-free is sufficient and the money is available to provide this weed-free situation then there is room for inundative releases each year. Of course, it is becoming a fairly common practice to make releases of parasites or predators to control certain insect pests, and this can be a desirable alternative to the use of pesticides. It certainly doesn't apply in areas where the economic returns from the land is still relatively low.

GERLING Could you not protect the release population against its parasites by using a low dosage of insecticide. In other words, do just the opposite of what you tell the farmer not to do because he ruins his parasite complex. Wouldn't that work?

ZWOLFER This has been done in South Africa, where insecticide treatments killed the parasites attacking Dactylopius, a biological control agent of Opuntia spp. In the case of Urophora cardui I would prefer to destroy the parasite reservoir by mechanically eliminating bull thistle (Cirsium vulgare) which is the host of the native Urophora sp. As the dispersal range of these insects is very small, there is no need to control the parasite reservoir over large areas (which would not be feasible).

BARR I have a couple of points and questions that I would like to make and ask. First of all, in connection with these inundative releases, in the biological control of insects we find what Dr. Wapshere mentioned to be the case. We find that you must repeat this time after time and it becomes a very expensive operation and the economics have to be considered seriously. Secondly, the point was raised concerning the use of native insects against native weeds. We have looked into this on several occasions in Idaho with range land weeds and have found that we cannot get any success whatsoever, even though we know the insects have an immediate effect on the weeds. Even by transporting these insects in fair numbers it is just impossible to achieve any degree of control even in the apparent absence of parasites. It seems that we have done something to the insects by moving them, or have gotten them at the wrong stage of their population build-up. These trials were against rabbit brush, Chrysothamnus spp. and against big sage, Artemesia tridentata.

Also, I would like to ask a question about one other point which was raised involving population densities of the insects. In some cases, we know insects are rare because of effective parasitism against them and these, of course, offer the potential of being effective weed control agents in the absence of parasites. On the other hand, we know there are some insects that are uncommon or rare in their native ranges and that exist at low population densities. The question I wanted to ask is, "Have any of you had any experience at all with insects that are rare, for example, the aphid on Chondrilla? Is it ever possible that these low population density insects, introduced into new habitats, can control weeds? Can they build up to population densities in a new area to the point where they become an effective weed control agent, or do they have a built-in mechanism that keeps them as low population density insects? Under these circumstances are these the kind of insects you should ignore and not contend with?".

ZWOLFER A very interesting question. Who would like to answer it?

SCHROEDER I can give a recent example. Celerio euphorbiae has been introduced into Canada and this species is by no means abundant in Europe. After its establishment in Canada, it appeared that it would not build up, mainly because of predation by ants of the early instar larvae. After some years it looks promising and the populations are high in certain areas.

If an insect is rare in the country of origin, there may be several causes: it can be parasites, it can be lack of host plants, or physical factors that keep the population down. If we find that this population control does not exist in the country of introduction we may look forward to successful control of a given weed by a "rare" insect.

SANKARAN Well, I think Dr. Schroeder has given me some fresh hope with regard to my work on Eulocastra which feeds on Striga. The population of Eulocastra is very low in the field and we are about to have a second look at this situation and to find out whether it is worth pursuing this investigation. Probably, from what Dr. Schroeder has said we should pursue it and find out why the population is low. If we find out why the population is low we may know if this noctuid is introduced into the United States, whether it will fare better or not. As regards Dr. Gerling's suggestion of using insecticides to keep the parasites of phytophagous insects down in the native areas the problem is, what is the guarantee that the insecticides will not kill the parasites that attack phytophagous insects which are pests.

GERLING There is no guarantee. That is all I can say.

ZWOLFER With regard to the rare insects mentioned by Dr. Barr, I would suggest that often otherwise rare species may be quite common in certain parts of their distribution range. Dr. Wapshere mentioned some examples and we find the same situation with certain thistle insects. Tyria jacobaea, an Arctiid species, which has now become an effective control agent of Senecio jacobaea in Canada, is most rare in the Swiss Jura around Delemont, where nevertheless the host plant abounds. Actually, during the last 12 years I have only twice found larvae in the field.

As concerns the build-in mechanisms which may stabilize insect populations at relatively low levels, we suspect that such behavior patterns occur among some of our thistle insects and we have been investigating this problem for several years.

BENNETT I was hoping that we might hear from Dr. Hasan on whether pathogens might be more applicable to the inundative release approach than insects because of the ease in rearing them.

HASAN I think the problem of pathogens is a bit different than insects. Pathogens can be more easily released "en masse" and they will produce a continuing infection on different parts of the plant throughout the season. For example, with Chondrilla and Puccinia chondrillina we can release the fungal spores "en masse" and they will continue multiplying on the plant then we would not have to spray again. Since the fungus remains active throughout the year it more readily continues the infection into the following years.

ZWOLFER Wouldn't it be more effective if you artificially increased the density of spores per surface area.

HASAN Of course it will be more effective but the question of cost is always there. I think by releasing once in a massive scale in a population, we can count on the infection to continue.

WATERHOUSE I wonder whether this isn't al illusory advantage? Perhaps you need a million spores to do the same amount of damage as one insect and it may cost the same to produce a million spores as the one insect. So, really, it may be no easier or cheaper to mass produce the microorganism for inundative release.

LITTLE Speaking as a general practitioner in weed control it would be rather nice if we could look upon fungal spores as a kind of specialized herbicide. Dr. Hasan showed pictures of seedlings which had been inoculated with spores and described them all as having died. Also, he told me privately that the effectiveness of the spores was virtually 100%, i.e., each spore was viable and that you only need a limited number of spores per seedling to achieve a kill. The figure of a million spores per insect under these conditions would seem to be not quite the right ratio and therefore I would like for people who are experts in this field to look into the possibility also of applying suitable fungal spores deliberately to the most vulnerable part of stage of the plant, which is normally the seedling. We know that the most important part of weed control is to prevent competition in the early stages of the crop. Once the crop becomes established it can usually look after itself. When you kill the seedling you prevent competition with the crop as well as the production of weed seeds and eventually reach the desirable possibility of extermination. Combined with such things as minimum cultivation where the soil is not disturbed phytopathological weed control provides a very attractive possibility, particularly when you are dealing with weeds which are exceptionally resistant to herbicides and especially in countries that cannot afford to buy or make herbicides.

WAPSHERE I agree with Dr. Little for Puccinia chondrilla. This is a fungus which could perhaps be used in this herbicial manner, but there is still the problem that we are faced with on Chondrilla. Condrilla in Australia, at the moment, is partly controlled by the spraying of herbicides. If you are going to have a system where you spray fungal spores instead of herbicides your costs are going to be about the same.

BUCKINGHAM I would like to comment that the climate of opinion in the United States right now is such that if you can just replace a chemical herbicide with a fungal spore it will probably be readily accepted. If it merely equals herbicides in cost and effectiveness, it can be sold to the American public. I also think that it is a great benefit and something to shoot for.

LITTLE I agree with you completely. There is another point that I think hasn't been considered, Dr. Wapshere, and that is where the cost may appear to be the same because you have the cost of application and the cost of producing spores there is a unique distinction in cost, in that you can make the spores locally and it doesn't involve the magic word "foreign exchange". If you could make your herbicide which is potent and highly selective in your own country it would be a tremendous advantage over having to import something to do the job, apart from the contaminations and so on which Mr. Buckingham has mentioned.

RAMSAY I'm not so sure that I agree with this recent comment about the introduction of pathogens. I haven't attended very many meetings of our own subcommittee on biological control of weeds, but I do get the impression that because of the terrific reproductive potential of pathogens and the chances for mutation that the Committee is somewhat wary of introducing these pathogens and the few that have been suggested have been tabled or deferred for additional study.

ZWOLFER I'm afraid now we come back to the subject of specificity. Is there somebody who would like to comment on this? I understand that in the United States a large discussion is going on currently. I think Puccinia spores may be considered fairly safe, wouldn't you say so Dr. Hasan.

HASAN Yes, I think from our past experiences and also our present studies it seems that obligate parasites are more or less safe, and especially Puccinia spores. For example, Puccinia chondrillina is a macrocylic, autoecious rust. Now we can never expect this fungus to develop on any graminae. There are people who very often think it may react like the heteroecious rust Puccinia graminis and may switch up to another family, or another plant. I think a mutation, of course, may be there but this most likely would remain within the same species or in the same forms of the same species.

WATERHOUSE One interesting sidelight on the liberation of Puccinia chondrillina in Australia relates to a press release which said that it was an airborne fungus. Immediately there were many many protests from people who feared that they might be allergic to this new rust. Fortunately, it was well-known in Europe and in the eastern United States and fortunately no one in our laboratory reacted to it. However, it is one of the problems encountered when any organism is introduced which could conceivably cause an allergic reaction.

BENNETT One of the intentions of raising the subject of pathogens was to ask whether or not an innoculum of a native fungus is usually already present in adequate numbers but doesn't develop until field conditions become right. In other words, it is a question of whether we can gain anything disseminating a native pathogen or if we have to wait until the weather conditions are right.

DUNN I would like to comment on the use of domestic pathogens. It seems to me that we probably have domestic plant diseases that would suppress weeds if they were gathered and mass produced and then distributed as Mr. Buckingham mentioned. It also seems to me that if this facet of phylopathological weed control was initiated it would bring the industrial scientists into our field thus creating an addition to the whole field of biological control of weeds. I'm sure there are any number of possibilities that are waiting to be discovered and used.

Also, I was wondering if I could get Dr. Huettel's opinion on the use of domestic insects to control some of our weeds. For example, if we consider Johnsongrass (Sorghum halapense) an important weed, by looking through the literature briefly, one can find that quite a few corn insects, some cultivated Sorghum insects and other grass insects are found occasionally on Johnsongrass, but Johnsongrass is not the principal host. I have been thinking that perhaps we could take one of these insects that is an occasional visitor to Johnsongrass and if it has the genetic capability, squeeze it over to Johnsongrass by heavy selective pressure and make that plant the primary host. If this method of developing domestic natural enemies would work we would not be bothered with quarantine procedures and expensive foreign research laboratories. In addition, biological weed control by insects could be expanded enormously in many countries with no risk from introduced exotic species, because only domestic species would be used for this program.

LITTLE May I draw attention to two possibilities which Mr. Dunn has mentioned that he thinks is possibly worth following up. One is Johnsongrass, Sorghum halapense. In Rumania we have seen an Ustilago attacking Johnsongrass with devastating results, i.e., complete suppression of the seeds and stunting of the plant to almost harmless levels. Secondly, another phytopathogen in Rumania which seems to be destructive to another important weed is Erysiphe convolulae against Convolvulus arvensis. When this epiphytotic is at its peak it gives me the impression that if the fungus were introduced earlier in the season you might get a much more devastating effect on an extremely resistant weed. When the weed is attacked, it looks very much like the "powdery mildew" we have seen on Dr. Hasan's Chondrilla plant slides, the plants being stunted and harmless and now flowering.

ANDRES This isn't particularly on fungi but it does apply to both insects and fungi. With the inundative release, you have to keep clear just exactly what your objectives are. In many cases, weeds are problems only in certain stages, in relation to a crop plant, and we don't always have to think in terms of the kind of control that we have been achieving with Hypericum or the cactus. If we can just reduce the competitive advantage of the weed at a particular time or reduce its noxious quality, just for a period of maybe 10, or 20 days, this may be sufficient. Once you have more or less achieved your purpose you can just let the weed go on.

ZWOLFER Let us come to the question of integration of biological weed control with other control methods. Who would comment on this topic?

ROBERTS I would like to hear opinions on what we might do, if anything, to control the insect which allows the weed to be there in the first place. We direct a lot of attention in our biological control programs towards working in foreign countries and looking for insects which we might introduce to control a weed in our own country. But how often, and how much emphasis do we put on the possible biological control of our native insects which allow the weeds to be present in our agricultural situations in the first instance?

My question is prompted by some practical experience I have had in Australian pastures where a complex of root-feeding species in particular, and some foliage-feeding insects, will damage large acreages of pasture thus allowing thistles or barley grass or other undesirable plants to come in. I have seen up to 300 acres of pasture land completely taken over by weeds following insect damage; there was no plant competition, so the thistles or other weeds came in. This happens not only on large areas, but also on a small mosaic scale. I believe that if the grass has been eliminated in numerous small areas, each a few cm in diameter, the thistles will then come in. This is the problem I had in mind. I was wondering whether we could integrate control approaches and pay more attention to those insects that allow the weeds to come in.

BENNETT I think it is a valid point, and one that we would certainly like to consider in our Institute. Many of the weeds that have been discussed today, and over the last couple of days, are introduced weeds and you can work at control both ways. When you can put some pressure on the weeds by introducing a specific natural enemy the grasses might be able to fight back more adequately if agents to control their pests can also be satisfactorily manipulated. This two pronged approach may provide the best solution.

SCHROEDER This doesn't reply to your question but is just another facet. There is an interesting paper by Aichinger, a plant sociologist who studied pastures of different types in Austria. He found that there is a negative selection working on our pastures by the cattle we are grazing on them, because they feed only on the most desirable plants, those which we want to be there. Weeds are getting into those overgrazed pastures so he analyzed the plants in the pasture and found that certain weed species invaded only overgrazed pastures. In these cases the cattle, grazing on the area, brought the weeds in. The root feeding insects you mention and the cattle in overgrazed pastures produce the same effect.

ROBERTS This is a good point and I would like to mention that I am very well aware of the role of grazing pressure as it applies to weed problems in pastures because I have been working in a nine-year-old grazing trial involving different stocking rates with sheep. Although thistle and barley grass problems come in, differentially, as a result of the differential grazing pressures, there is also a weed problem which is definitely associated with the activity of the root-feeding and the foliage-feeding insects.

SCHROEDER Are there any examples showing that overgrazing encourages the population build-up of Costelytra? I would think that this could occur.

ROBERTS The insect Costelytra has been mentioned. This is a scarabaeid pest of pastures in New Zealand. Our work to date with our native Australian scarabaeid beetles and also with a native species of oecophorid caterpillar shows that there is, from the insects' point of view, an optimum grazing pressure for a given set of conditions. This optimum depends on which type of insect one is studying. If one exceeds the particular optimum, then at all other higher grazing pressures the insect populations will be lower.

I understand that some New Zealand workers also have evidence that Costelytra populations can be reduced by increased grazing pressure with sheep. The New Zealand stocking rates were above the levels we studied. In our study we used the normal range of stocking rates which the Australian farmer uses, i.e., eight, twelve, or sixteen sheep per hectare.

CAVERS Is it possible in this case to have a solution to your problem based on plant ecology? If you had a greater variety of species in your pastures, perhaps both clovers and several kinds of grasses, then if any individual species is eliminated is it not possible that another component of the sown mixture could come in and replace it before your weed could become established? Annuals which would establish from seed might be particularly useful.

ROBERTS I agree in principal, but I think weeds are usually more competitive, whether the weed is a flat weed or not, e.g., barley grass. In some parts of Australia barley grass is considered to be quite a valuable grass. But in my area in the pastoral zone on the tablelands of northern New South Wales, barley grass is considered rather bad grass because its seeds get in the eyes of young lambs and cause blindness.

The problem is that barley grass, thistles, other undesirable plants, and relatively poor annual grasses including Vulpia spp. and other annuals come in following insect attack on pastures. My experience to date has been that most of the plants that come in as a result of this attack are undesirable species.

CAVERS Do undesirable species still come in regardless of the composition of your pasture?

ROBERTS Yes, but this is a point I'm not clear on, because this particular experiment started nine years ago. The Phalaris white clover system has been allowed to respond without restraint to the different treatments superimposed and now after nine years we do, in fact, have different plant populations.

ZWOLFER Speaking about integration of biological control and other control measures I am wondering if one couldn't consider integration of herbicide applications with the biological methods. In the European station of the CIBC, we are working on insects attacking Rhamnus and one of the ideas of the Canadians is, first to have a chemical campaign against Rhamnus and then to introduce a population of phytophagous insects to keep short the young shoots and new growth.

FRANK We have a very good example of this in the control of alligatorweed, where the Agasicles beetle is fairly effective in some areas in controlling alligatorweed. The effects, however, are much more drastic when the weeds are first treated with an herbicide at very light rates. The beetles seem to prefer the new growth and are very effective then in keeping it down.

BENNETT Dr. Frank discussed the same point I wished to raise and his example is somewhat similar to cases, possibly Lantana or other shrubby plants, where a single agent will not give adequate control. If it promotes new growth on which another insect feeds you have basically the same process but are using an insect instead of an herbicide to start the process then following it up with another insect.

LITTLE There is quite a semblance between the ideas that Dr. Frank just mentioned and the grazing on non-poisonous weeds in New Zealand. In terms of grazing, particularly with plants that are not poisonous, of course. For example, when given a light dose of 2-4-D some plants such as variegated thistle become palatable to stock and can be grazed out. This converts all this plant material into useful product but, of course, such a procedure is dangerous in the case of ragwort.

ZWOLFER Now Dr. Andres would you like to comment on the topic "How to resolve conflict of interests in the biological control of weeds"?

ANDRES I mentioned the other day that I felt we will be faced more and more with the problem of conflicting interests. Up to the present our conflicts arose only when the weeds had some economic value; now we must also consider the role of the weed in the ecosystem, i.e., is it a source of food for wildlife, etc. In almost every case where a plant has become abundant some organism uses it for food or shelter. Also, if our weed insects feed on closely related plant species that have no direct economic value but are of ecological importance, we must balance the potential gain of introduction with potential loss. How do we do this with non-economic plants? I don't know how to resolve these questions. Perhaps these are questions more for the program administrator and our role should be one of advising on the characteristics, advantages and limitations of the biological control method.

ZWOLFER A part of your problem is the question "Can you predict the type of damage of a special phytophagous biocontrol agent and could we, for instance, in cases where we only needed partial destruction of the weed, use less offensive agents?".

RAMSAY I would just like to amplify slightly what Dr. Andres said. I have here the latest guidelines of our subcommittee on the biological control of weeds and in this protocol, item 6 mentions "Only tests conducted under strict quarantine may be conducted in domestic facilities, where tests can be made, preliminary evaluation studies may be conducted overseas. Initial testing in all cases will emphasize plants of economic, ecologic or aesthetic importance". This committee is made up of both the Department of Agriculture representatives and the Department of the Interior representatives and I think you can see that the ecologic aspects of these introductions have not been ignored.

GERLING I would like to hear another facet of the subject of the conflict of interest. Not the conflict of interest that arises from a change in the habitat of the biocontrol agent but that of different groups in the same country respectively thinking the same plan usable and noxious.

ZWOLFER Actually we have several tough problems, one being that described by Dr. Gerling.

WATERHOUSE I can comment on that in relation to a number of our weeds, but the one that is most relevant, possibly, is that of Chondrilla juncea, the skeleton weed, which over most of its range is not thought kindly of by the primary producer. In southern New South Wales, however, this plant is regarded by many people who are involved in raising sheep and particularly lambs, as being a desirable plant. In this area they don't happen to grow very much wheat and the skeleton weed grows rapidly in late winter and early spring and the rosettes are eaten by the sheep. This is the time that the prices for lambs are highest and if they have early spring lambs which can feed on the skeleton weed rosettes, these lambs bring a premium price. For many years, there was no decision to go ahead with the introduction of skeleton weed organisms, unless we could guarantee they wouldn't spread into this particular region.

SANKARAN I am afraid we cannot generalize about the harmful or beneficial nature of certain species of weedy plants. For instance we have a project for the shipment of beneficial insects to the island of St. Helena. The agricultural officer there feared that if Lantana is eradicated they may have the problem of soil erosion, leaching and so forth. I think it is a matter for the local authorities to decide what they want to control.

ZWOLFER Is there somebody who would like to comment on conflict of interest, as far as different types of agriculture are concerned.

SCHROEDER We could mention here the example of Rosa rubigenosa, a New Zealand project we have been working on at the European Station, CIBC, for several years. This plant was considered to be a pest after the rabbits were successfully controlled. The roses invaded pastures and were spreading so there was a request to study the phytophagous insects in Europe with the hope of finding some useful insects to control Rosa rubiginosa in New Zealand. We made this study, but after some five or seven years, we got a letter from New Zealand telling us to stop. "No more interest", because people found that Rosa rubigenosa is a very good pioneer in areas which they want to re-forest. It collects humus and prevents erosion in these areas. This shows that there are certain historical aspects in such programs and new considerations may arise while one is working on a certain problem which will show that the work in progress is the wrong approach and is no longer useful.

CAVERS As a further elaboration of this idea, consider that many of our common weeds are also found on river banks and flood plain lands, particularly where the water lies for a long time during the year. Now, it is on these very sites that we also get a great deal of pollution, in North America at least, and these weedy species are often the only ones which survive in such areas. If we were to eliminate these plants there is not much else to hold the river banks and you would have more problems with erosion.

ZWOLFER I think it is quite a widespread phenomenon that one of the benefits derived from weeds is soil protection. We have the same problem with goldenrod (Solidago) in the southern part of Europe. Solidago has been introduced from North America, and is very aggressive. It would be a suitable weed for biological control but some of the botanists still think, and they may be right, that goldenrod protects the soil and it is better to have the goldenrod than nothing else. Would someone else like to comment on this protective function of weeds as far as water is concerned?

CARESCHE In the country formerly called Indochina where I spent many years, one plant, Ageratum conyzoides, was considered as a beneficial plant and as a protective cover for the soil against erosion, so it would have been unadvisable to try to eliminate this weed.

FRANK At present, in the United States, we have a conflict of interest with regard to Tamarix on flood plains in the arid areas of the country. Many people would like to see all of the Tamarix eliminated because they believe that much of the transpired water could be saved and used to irrigate crops. Unfortunately, Tamarix is a desirable habitat for doves hunted by sportsmen, and it is also considered important in the management of other wildlife. In addition, we have the honey producers who, in a land not blessed with abundant vegetation, look on Tamarix as a component essential to their livelihood.

GERLING Since I have been working on this plant I would like to comment on that problem. The question is, could a plant be brought under biological control enough to be removed from the pest stages and reduced to the status it occupies in its natural habitat. Tamarix occurs in Israel and is thought to be very beneficial by 95% of the population. It will occupy areas that other plants won't occupy, i.e., barren soils, and will stop sand dunes.

etc. It has a very large number of (maybe 40 or 50) insects which feed specifically on it and none of these insects are in the United States. If one could introduce a complex of insects including some of the parasites; one could possibly reduce the number of, but not eradicate the plant and eliminate some of the main problems. There are not only problems of water loss with this plant but also plugging up of water shed areas by all the plant refuse the water carries along which plugs the drains and causes flooding. If this plant was partially controlled as in Israel and only the riverbanks occupied by Tamarix there would be an equilibrium which may solve both problems.

ZWOLFER There may be another beneficial side effect, because in this way you would enrich the diversity of the ecosystem and you would get additional channels for energy flow.

BENNETT I think the Agasicles introduction is a classic example of an insect that does very well only in an aquatic situation; where the weed is terrestrial the type of growth is unsuitable for the development of the beetle. Therefore, assuming this beetle is really effective on alligatorweed only, in aquatic situations you could end up with some useful plant cover on the bank, having solved the problem on the water. Perhaps some of the delegates from the United States would like to comment further on this.

FRANK Even here we have a conflict of interest because the cattle ranchers, in Louisiana for instance, like to have alligatorweed because it provides forage for cattle. They resisted quite strenously the introduction of the beetle. However, through accident or design it was introduced into this State.

CAVERS We have been working with Barbarea vulgaris, the yellow rocket, which may also be a valuable plant in river bank situations.

The flea beetles which eat this species apparently are eliminated over winter when the plants are completely covered with water; therefore, these plants are much healthier, have very little damage and seem to grow better than the plants which are weeds of agriculture. At present, we are not sure that the beetles are reducing stands of this weed even though they cause extensive damage to the plots. Nevertheless, you do get this great difference, in the flood plain situation where the plant seems to be beneficial, it is virtually unattacked.

ZWOLFER You mentioned the aesthetic values of certain weed species. That may be an aspect of a certain importance, but I am wondering whether it is not also a matter of educating the public. Let us try to convince the public that a diversified ecosystem with different plants and animals is preferable to a nicely looking but sterile plot of goldenrods. As there is now an increasing public interest in ecological problems, I think the public could be educated to accept the idea of biological control of weeds, even if these weeds are of some ornamental value.

BENNETT In the short time remaining I would like clarification from Dr. Andres on the point that he once raised on whether insects have or do not have special adaptations to enter an aquatic environment, and how this affects the probabilities of success using insects for control of submersed weeds.

ANDRES In looking over the literature I was going on the basis of the number of host specific insects that had been taken from submersed plants versus those from emersed plants versus floating plants versus terrestrial plants. It was on the basis of these insect lists from several sources that I came to the conclusion that the chances of using or finding host specific insects on submersed weeds are not that great. But, then again, it really hasn't been looked into that much.

BENNETT I agree, that this merits further study. The other point raised by Dr. Harris was that since some insects are adapted to developing in aquatic plants with a very high water content these are therefore less likely to be problems on terrestrial plants. This leads to a point I raised, that we can use insects that have a broader host spectrum provided we can show that they are tied to an aquatic environment. This point was raised initially in the context of damage to economic crop plants, but the more we consider the possible environmental importance of native aquatic plants the more we should, when possible, stick to host specific insects in preference to those "broader spectrum" ones even though they are tied by their biology to an aquatic environment.

BUCKINGHAM I would like to raise a point that came to mind while listening to Dr. Cavers' talk. Recently a European species of *Longitarsus* which feeds on *Barbarea vulgaris* was found in the United States. This particular species is recorded in the literature as having been found on some crop plants in Europe. If we were planning a program against *B. vulgaris*, we would probably eliminate this species from further testing on the basis of the literature records.

It would be interesting, however, to do feeding tests with this insect and see what decisions would be made. Maybe in this way, we can learn more about the predictability of our screening programs. There are many accidentally introduced non-pest species in every country that could be studied. Also, species which are pests in the country of introduction, but not in their native country, are good subjects for study. Studies such as these would not only help increase our knowledge of decision making, but they would also be excellent ways of training students and bringing them into this field.

SCHROEDER This idea is very interesting and we have been promoting this idea at CIBC for a long time, and we have had some success with our Canadian counterparts in the insect parasite work. In order to learn something along these lines we try to select some, what we call potential pest insects or insects introduced into North America and specifically into Canada from Europe, which didn't become pests. We study the parasite complex and the ecology of these insects in Europe and in Canada. I think we can learn a lot and we are promoting this idea. It would be very useful if we could do something similar with phytophagous insects which have been introduced into a foreign environment and didn't become pests.

ROBERTS I think this is an interesting point that has been raised. I want to make a comment, and then ask a question. Some years ago, I discovered *Plectris aliena*, a scarabaeid, which was the first record in Australia for this insect. This insect was previously described from South Carolina in 1934 and had not been heard of since until it showed up in one of our pastures in Australia.

Of course the question immediately arose, is this scarabaeid going to become a major root-feeding pest in our area? We did a short ecological study on the animal and it was fairly apparent both from the current work in South Carolina and from our own work in Australia that this insect was restricted to very sandy soil. There are not many pastures grown on very sandy soil. Now, my question is to the other workers here. How often is soil type considered in biological control programs? I do not know what the percentage is, but a very high percentage of plant pests spend at least one stage in the soil and this could be, in fact, the most critical stage. We hear a lot about host plants and host plant selection but how often do we look at the soil type? In my example, soil type is critical and I am quite sure the insect, *Plectris aliena*, has no hope of invading our major pastoral regions.

WAPSHERE Indeed we have considered the soil type in the study of the *Chondrilla juncea* root organisms.

C. juncea is a weed of sandy soils and the root feeding organisms which feed on this plant are probably non-functional in heavy clay soils. There is a *Chondrilla* insect which occurs in Persia and southern Russia, *Neomargarodes chondrillae*, a coccid, living on the roots. The female of this particular species has fossorial legs and digs into the ground to feed on the *Chondrilla* roots. In a heavy clay soil, it wouldn't be able to do this.

On the subject of experimental introductions we have a good case where this would be possible if we received the cooperation of the Spanish, French or Italian government. The two Chondrilla insects which occur only in the eastern Mediterranean region could very well be introduced into the western Mediterranean region. If this were done and no cultivated plants were attacked in the western Mediterranean, this would be very good evidence for introduction into Australia.

ZWOLFER The time is up. I wish to thank all the participants of this lively and informative discussion.

END OF FIRST SESSION OF COLLOQUIUM

* * *

UNSTRUCTURED COLLOQUIUM
(Second Half)

Dr. Fred Bennett, Moderator
CIBC Trinidad, West Indies

DREA I was impressed with the photographs of Lantana, and the dense mat it made over the areas. I just wondered, what if you had a very effective insect that would control Lantana in a given area, but this insect also attacked, to some degree, a crop in the area, would it be possible to arrange to pay an indemnity for the damage done to the crop?

ZWOLFER I think that in such a case the decision would be up to the organization who sponsored the research and I know that the Australians, for example, who are very strict, have after a while given authorization to introduce insects which actually did a little harm to economic plants just because their pest problem was so big. There are certainly cases where one has accepted an insect even one which does a minor damage to economic plants, if there is an urgent need to introduce something against a weed.

BENNETT This has not answered the question entirely about payment of indemnity. I don't know of any examples where actual payment has been made. Certainly it would be feasible if the situation warranted it. In the control of weeds with herbicides where the weed problem has been overriding there have at times been side effects which are tolerated on crops in the West Indies. The control of weeds in sugarcane has occasionally resulted in the drift of 2-4-D onto cotton and the sugar producers or the government have paid indemnity to the cotton people. In a lighter vein, some of those who were paid the indemnity made more money than those who finally reaped their crop! Of course this is on a temporary basis and it is a question, when considering the introduction of a phytophagous insect, of whether we can predict that a relatively minor crop now may always remain a relatively minor one. As the pattern of the economy changes, a minor crop may become a more important one. Whether we should evaluate the risk on present day conditions or on future possibilities poses a problem. I raised the point only for discussion, not to arrive at a serious conclusion. Politically, if not on scientific grounds, if this question did come up it might be possible to suggest, under certain circumstances, that if we want to control an insect introduced for the control of the weed we could go back and, as we do for the biological control of insect pests, bring in the parasites of the weed