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PRESESSION SEMINAR ON BLACKBIRD PROBLEMS

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APPENDIX

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Tom Stockdale, Chairman
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STOCKDALE: I'd like to open with a very brief discussion, then call on some selected people with whom I've corresponded earlier, and ask if they will discuss a particular aspect of this total bird problem. I've asked that they keep their comments from three to five minutes.

Trying to break the total problem down into its several facets we came up with six arbitrary subtopics. For want of better terms I've called these: population census techniques, chemical repellants, sonic repellants, wetting agents, lethal control techniques, and damage evaluation. Your particular problem in your area may be slightly different than in other areas, but I think it all boils down to one thing—birds are causing either a nuisance or an economic problem. Our job, no matter what our affiliation, is to try to reduce the nuisance problem or reduce the economic damage caused by bird species in feedlots, blueberries, corn, urban roosts, or whatever.

In extension work we are frequently asked to put a dollar value on a particular problem. We've been asked to do that here in Ohio with the blackbird corn problem. Others of you have related similar experiences in your respective states. Those who have worked primarily with birds in livestock feedlots, have had some interesting work in economics; you've been able to put a dollar value on the amount of damage caused by the birds.

Those working on corn have not been quite so fortunate. The magnitude of the problem is anybody's guess. We know we've got a hell of a lot of birds, we know they're doing damage and causing nuisances, but from there—dollar-wise—who knows? Work is being done all over the nation, needless to say. The Federal government has become even more involved this past year with the establishment of the research facility at Sandusky, here in northern Ohio, under Mel Dyer's direction.

That is as much as I'm going to say to introduce the program. Mel Dyer, Bureau of Sport Fisheries and Wildlife, Sandusky, will initiate the discussion with a consideration of population techniques.

MEL DYER; It's no secret that the best way to get a population size figure is to count birds. This is all right for a few birds such as the whooping crane. However, when we're dealing with other species, particularly the redwinged blackbird, we come far short of counts and it's obvious that we have to make estimates. The problem starts here.

There are many ways of making or computing estimates. I want to bring you up to date on some of the best methods we've found for redwinged blackbirds and related species.

In the future we will see more sophisticated work being done on such animal counts when the more sophisticated equipment being used in Viet Nam for smelling out animal bodies is released. This will only add to the biologists' problems of engineering, however.

The major problem in any of the methods is making sure we allocate our animal populations realistically. That we don't have homogeneous distribution of animals is the main reason we have problems in making these counts. Non-homogeneous distribution functions in two ways: time and space. Truly the redwing is a good example of territoriality and thus, spatial distribution in the spring. After the breeding season the territories breakdown, the population collapses on what is called an epicenter, and we have large concentrations of birds. This complicates trying to count them.

The methods of counting are varied. I mentioned the extremes: one is simply counting with eyeballs and the other extreme is use of ammonia detectors [to sample roost nitrogen] which are used by the U.S. Army. In between, then, we have the more realistic, useable methods.

I personally was involved with a method during thesis work a few years ago in Sand Lake, South Dakota using radar. Radar has not been fully exploited in estimating gregarious population numbers. However, again I must emphasize the problems, mostly technical, of utilizing such equipment.

Other techniques are a bit more theoretical in design and rely a lot on statistical bases. When we have more than a few individuals in a population, say a countable number measuring into the thousands, we must rely on statistical methods. We have to make sure that each animal has the same possibility of being counted. This is where we run into problems. We have sources of bias, and we have to understand them; they take on many forms depending on the species that we're working with. For example, in redwings we have rhythmical behavior throughout the day. This is a relatively new science called circadian rhythm. Unless we understand that the visibility of the bird fluctuates during the day, we are going to be collecting non-useable data.

The two best times to measure blackbirds or other gregarious bird populations is during their time on the breeding grounds or during the winter roosts. To count birds on the breeding grounds give us density per unit area. Besides density per unit area we must know a few other things that feed into life history tables: birth rate and the death rate, male to female ratio. These are big undertakings because birds are spread non-homogeneously over the North American continent. Each study must be taken with the initial thought that it is going to be different from somebody else's published material. The only reason for reading someone else's work is to make sure you're in the same ball park, so that the data can be truly comparable.

The other period of year that's used to estimate within a few million, many million I suppose, the North American blackbird population is the winter roost count. This is fraught with problems. We know what ten animals are, a hundred, a thousand; we may know what ten thousand are (I'm not sure we know a hundred thousand), but beyond that we lose sight of the situation very rapidly. The point is

that these large concentrations create many problems in censusing despite the fact that they're concentrated.

There are many statistical designs for counting numbers of birds; some of the most useful are strip censuses. This is simply a method where you walk through a specific habitat counting the number of birds that are flushed and gathering the type of information necessary to apply this to some empirical mathematical formula. It is quite successful; but again, unless you know the biology of the animal, it can be misrepresentative.

Another method is the capture and recapture method. Petersen in the late 1800's in Norway working with fish populations noticed that a certain proportion of animals previously caught and marked appeared in subsequent captures. He gathered enough empirical data so that he was able to establish a very simple ratio that gave estimates of the total population. Knowing the total population in an area gave him the sought after data of density per unit area, which we are after.

A few years ago Dr. Hewitt, working at Cornell, invested a very ingenious method for capture-recapture research in population enumeration without actually having to have the animal in the hand. This is a remarkable technique in many respects. There are two main advantages: one is that you never have to introduce the "spook" or aversion factor to the trap, since you never have to handle the animal. The second thing that's wrong with trap-retrap data is usually the proportion of returns is so small that you have a tremendous error factor in your statistical representation, and this error factor will beat you—you'll simply have too-wide confidence limits on the data you've collected.

Ollie Hewitt's method provides for exceedingly high trap-retrap method. It goes like this: On some sort of random or planned basis sample sectors are laid out for observation from a car, so that a great distance can be covered in a short time, thus enlarging your sample size. A person runs around the route once, recording all animals into a tape recorder. You're driving down the road and you see a male redwing on some particular tree; you must keep track of where you are, at all times recording all birds. When you repeat this route, you see many birds that you saw the first time. You mark these; you see some new birds. This ratio then predicts or gives an estimate of the population in that area. The reason for me to dwell on this is that you get a high porportion of formerly marked birds—theoretically. The basis for being able to count the animals this way is their territoriality.

I said that I would comment on the reasons why we need to have censuses. We can state briefly that any program that goes on without having any idea of the animal's number is inviting disaster. We are very much aware today of endangered species programs, pollution programs; all these programs are quite interrelated. We're using the same principles for redwing population as are used for endangered species, only we're working at the other end of the scale. Thus we must know what the population is; it's an absolute necessity because there can be no meaningful treatment nor can there be meaningful analysis of the treatment, if you're using data after the fact, so to speak.

There's one more thing that's going to help us in the future and this is the most exciting thing of all. From data such as these, the next place to go is computer simulation. We have a man now who I believe may be interested in computer simulation of this particular problem. The advantage to this is that the computer can create, given

information, more possibilities and more data in a few microseconds than what we can collect in the next fifty years. So I predict that within ten years we'll have this whole system computer programmed. It doesn't mean that the computer is going to control everything; it simply means it's going to give us the different possibilities.

STOCKDALE; Mel has introduced not only the subject but has done a real good job giving you some insight into some of the work he has begun with the new facility at Sandusky. At this time I think we should take a couple of minutes to ask questions or to make other comments on experiences you may have had with regard to census population techniques.

WOULFE: What time of year would these samples be made?

DYER: A spring breeding census runs during May and the first part of June. It must be done during the peak of territoriality in the breeding of birds—the redwings; this is our assumption. This time is when they're easiest to count and this is when the chance for error is least because we're pretty sure we're not miscounting the birds when we go on our census.

QUESTION; Mel, do you pick any particular time of the day to make these counts or is it throughout the day?

DYER: Throughout the day. These fellows got up early in the morning and worked until late at night. Now we know there is daily variation which I mentioned a little bit ago. The next step will be to get our design situated so we can compare times of day. I know there will be differences, but so far all we've done is say that everything is grouped.

SCHWAB; Will you be able to make some estimate on fall population sizes on the basis of a breeding census?

DYER: If we knew how far away the birds were coming from, I'm pretty sure that we'd make the two jibe somewhere. If we knew the migratory distance of the fall populations when they collapse in on these epicenters at the marshes, we'd be in pretty good shape to make these. We need, as I said before, the male-female ratio and the successful offspring. We're a long way from that for a regional study. This has been done locally. There's a lot of North American data, and using the North American data, I've made estimates which are not at all realistic.

SCHWAB: I take it then that you plan to repeat this.

DYER: This is a three year pilot study at the moment. We're going to compare the data in every conceivable way possible, and this comparison will dictate to us next spring's work. We know now we've oversampled; we took a hundred samples per transect per block, and we don't need that many to get the statistics. So this will allow us to put our manpower into more regions that we need.

NEGY: Mel, this census was based on territorial males, wasn't it?

DYER: Right.

NEGY: Is there any correlation between the territorial male and the female population at all?

DYER: Yes there is, and I was out to the University of Washington seeing Gordon Orians, who has been doing a lot of basic ecology in the redwing system. He feels it's very important to understand the polygamy of this species. I noticed a couple years ago that we did not have enough females per males in Ontario; I attributed it to bad data collection, and now I'm wondering if this is what Orians would predict.

Wood County (Ohio) data has been collected for four years by Webb and Meanley of Pautuxent at Laurel, Maryland, and I just simply can't believe how stable these populations are. Their data goes something like this: they come up with a count of n— the first year it was 651, the next year was 634, then 653, then 650. Now this is impossible except for one thing—this is a very stable community and it's being counted very well. If you accept these, then we're on our way; we have good data from one county.

WOULFE; In those instances where you have variations of significant proportions, do you have any other analysis of topography or background habitat?

DYER: Good point. In my estimation I can see two ways in which blackbird populations will fluctuate. One is going to be background habitat which is different while the density per unit area is the same. The other one is that there will be similar background ecology and the density per unit area (actually, the bird per unit area) is the thing which is fluctuating. I'm sure we have both. I'm most interested in the latter; I'm pretty sure this exists in Ontario from other data that I have. In other words my prediction for Ontario is that the population will continue to grow until it reaches the same density that the Ohio populations have. Therefore there will be more birds, more productivity, and so on.

STOCKDALE: The second topic for discussion is that of chemical repellants. I'll call on John DeGrazio of the Denver Research Lab to lead us on.

DeGRAZIO: I'd like to limit my comments to one chemical. It's a repellant; we call it a frightening agent. Our number on it at the Denver Wildlife Research Center is DRC-1327. It's also known as Avitrol; it comes from the Phillips Petroleum Company of Bartlesville, Oklahoma. I want to talk only about 1327, since you might consider it's being used now on a semi-operational basis. And after I finish my brief discussion I'd like to ask Don Harke and others to give us a brief rundown on how 1327 did in tests in Ohio and Michigan.

As some of you know, the problem we're working on in northeastern South Dakota is blackbird damage to field corn. We've been working in an area around Sand Lake National Wildlife Refuge northeast of Aberdeen, South Dakota.

Sand Lake Refuge consists of about 8000 acres of water out of a total 21,000 acres; 4000 acres of it is in marshes. The dark green you see on the slide is cattail and the light colored vegetation is phragmites. This is the habitat that the redwings roost in. The Refuge runs 17 miles north to south and we have marsh on each side of the water, so essentially there are 35 miles of roosting habitat. The birds have roosting "hot spots;" they don't all roost in one spot, but are scattered throughout the 35 miles of marsh shoreline.

Over the years we have tried a variety of techniques to control the problem at Sand Lake. This has consisted of decoy crops, such as early maturing varieties of sorghum, in an attempt to get the birds to feed on these decoy crops before the corn matured. We also tested some so-called blackbird resistant varieties of corn. We've done some work with the contact poison DRC-632 sprayed on the roost and vegetation and also sprayed on the bird. We've done some work with habitat manipulation; we've tried burning sectors of the roost, applying herbicides to the roost and habitat, and also some disking and plowing in one of the years when it was dry. We've tried many different types of baits and baiting sites. We've swabbed millet which matures prior to corn, sprayed that with strychnine, and have just done a tremendous amount of work to try to bring this problem under control. In the final analysis we feel that DRC-1327, or Avitrol, applied to cracked corn bait, and then applied in the field during the damage season, has been the best method we know of, and so we've been pursuing this quite hard in the last year.

In 1964 we selected an eight section study area, took every corn field in these eight sections, which amounted to about 1100 acres of corn, and treated the ears with DRC-1327. We had one treated plot of five ears per two acres throughout the fields. We were able to reduce damage by about 75% with this technique.

Then in 1966 we took the same 8-section area that we had in 1965; again it amounted to a little over 1000 acres of corn, and instead of treating ears with DRC-1327 we applied the chemical to cracked corn bait and spread it in the corn fields on the ground. We tested a variety of methods of bait placement. We did some work with aircraft. However we did bait most of the acreage by hand. This again was time-consuming and was a lot of hard work.

We have enlarged our area to 508 sections, and the Green Grow Fertilizer Company, which has these Highboy tractors that run down the field in standing corn, have mounted electric seeders on top of the Highboys, and are applying baits for the landowner. We've made arrangements with the Phillips Petroleum Company to send the concentrated, packed cracked corn bait to the wheat grower's association and they dilute this bait. In 1966 we used the ratio of one part treated to 30 parts untreated; and in 1967 we used one part treated to 100 parts untreated, and we also had 1 to 100 this year.

This year from our population estimates, the population is down from 1966 and 1967; in fact it is the lowest it has been since we've been working up there. That's all I have to say about 1327. I'll be happy to answer any questions. First perhaps Don Harke would like to give us a few details on the program in Ohio.

HARKE: We had two major study areas in this area—one in Monroe County, Michigan, and one in Coshocton and Muskingum Counties in Ohio. Both these areas have high corn damage histories.

We haven't yet compiled and statistically analyzed our data. Where John and the crew in South Dakota put their Avitrol on with a Highboy spray apparatus or by hand, we used a type of Pawnee aircraft to put the bait out at three to five pounds per acre at 1:99 dilution ratio. We got excellent coverage this way. There were several reasons for choosing the aircraft; corn in this part of the country grows considerably higher than corn elsewhere. A lot of farmers wouldn't go along with the idea of using the Highboy apparatus in their fields. So we went to aircraft. Our study area in Ohio consisted of about 1600 acres. In Monroe County, Michigan, we had about 1000 acres, and it was also treated by the same plane.

The results in Ohio seem to be somewhat better than the results in Michigan. I don't know the definite reason for this, although I have heard people mention that this Lake Erie area has a rapid turnover of birds. As the theory of Avitrol is to "educate" the birds to stay away from corn, this would be pretty difficult in this area. In Coshocton and Muskingum Counties, with our 1600 acres of corn, we had a fairly local population of birds—birds that had bred, nested and raised their young in this area.

We have just concluded running our damage assessment and apparently the damage in the treated fields was extremely low compared with our earlier damage assessment. But damage in the untreated fields doubled; it was at least twice as high in the untreated fields as in the treated fields. Here again I don't know the reason behind this. It's conceivable, of course, that we pushed them out of the treated area into the untreated fields. That's about the gist of our tests here, unless Don Negy and Chris Stottler, who were working in Coschocton, and Bill Shake, the assistant state supervisor who headed up the program in Monroe County, have anything to add.

NEGY: One interesting thing in the Coshocton County area, Don, is that one of the cooperators down there has considerable acreage planted in a resistant corn. He'd never had any trouble with it before, and on treating an adjacent field, he found that he was getting considerable damage in this so-called resistant corn. To me this would indicate that it's not educating the birds against eating corn as much as it is against eating corn in a treated field. Another interesting thing that happened down there is we had one field that had a 35,000 bird roost in the corn field itself. In one treatment we were able to completely drive the birds out of this field.

HARKE: I'd just like to say something about driving birds from one field to the next. Perhaps our study area wasn't large enough, but could be that had we treated a much larger area we could have driven them to Indiana or someplace.

SCHENDEL: Work in pecan groves with crows seems to have proved the very thing that you're mentioning here—that is you deny them an area of some acres of pecans, you will have them worse in an area about a quarter mile away.

STOTLER: It looked like this year's damage would be about what the county average was in the past. But our treated fields came out with less than half of the county average, and it looks like the (untreated) average damage for this year is only going to be 0.5%. My highest damage field was running 2.7%, and all this damage occurred before we were able to get in these and bait.

SCHWAB: Do I understand correctly then that there was no more damage to the untreated fields the year that you did put out the treatment than there was in the preceding year?

HARKE: As far as we can tell right now. This is just going on a county survey that we ran last fall.

SCHWAB: Then you have no assurance whatsoever you were driving birds into these fields.

HARKE: No. Of course, I am not finished. I have more damage checks to make in these untreated fields.

SCHWAB: What about this compound simply killing the bird?

DeGRAZIO: It kills the bird, yes.

SCHWAB: Did you figure that in your extensive treatment that you actually did kill a lot of the blackbirds?

DeGRAZIO: No. One good thing about this chemical is that if you affect 1% of the population that's in that field, then you should get clearance. And really that's about all you affect, and that 1% is killed—they go through their distress reactions. We're not denting the population, by any means, by having them feed on this bait.

SCHWAB: You say 1%, then, of the birds that go into this distress thing will die?

DeGRAZIO: 1% of the birds that go into the field.

SCHWAB: Into the field. How many birds do you figure take this, that don't go into distress reaction? What percent?

DeGRAZIO: Well, the way we have our dosage keyed, every bird that takes a treated kernel should go into distress; it should be effective.

SCHWAB: But they don't, obviously. Do they? I've not been able to duplicate this is California. In some very preliminary tests we found that out of every hundred birds given this compound, 99 of them would die; and the last one would go into this distress thing before he died. And I just wondered if you had anything similar to that? Or was my dose wrong?

DeGRAZIO: No, the complete reverse; 99 out of a hundred that take a treated kernel produce this distress reaction, and 99 out of a hundred die; this is in our lab tests and also in our field observations.

DYER: In the way of testimonial I can give some results that have never been written up or published. It involves two years work in Ontario using the same method of ground baiting that was used in South Dakota. We experienced difficulty in a mobbing reaction of birds rather than flight reaction. I'm pretty sure I'm correct in interpreting the data. I had a thirty acre field that I was treating. We used other control methods plus non-treated controls in the field, and the one that received the greatest amount of damage was the Avitrol field. It was 5 to 7 times the damage; for instance 38 to 40 bushels per acre were removed from the field whereas other fields, even control fields, were down as low as 10 bushels per acre. I believe there is a report concerning magpies and mobbing reaction out now. Bird behavior is a very important aspect of this.

BECK: I have a comment to make. In listening to the three of you discuss your treatments, I think you made an assumption that the treatments were basically the same, both in dilution factors and in other factors. And I don't think they were. This question was raised in my mind—you talked about a 1 to 99 dilution, John, in the Sand Lake area by hand broadcast in many instances. And Bob, you didn't specify, but you did indicate some cage tests. . .

SCHWAB: There was no dilution of this compound when it was first fed.

BECK: All right, under field conditions you're getting a natural dilution, assuming that they pick up at random and they don't choose treated or untreated material; I don't think you can compare the two results. Mel, I wonder if there isn't something different in the actual technique of distribution in your study?

DYER: There are two dilutions, so don't get confused here. First of all there is the dilution theoretically to make it barely effective. That was the original aspect, was it not, to get an ET_{50} (effective treatment); in other words 50% of the birds are affected when you give this particular dosage per kernal. Then you mix this or dilute with untreated grain 1 to 30 (or 1 to 100), so there are two types of dilution in this. I had affected birds same as the Denver personnel had, except I drew birds in. I would have the initial flight response and then, I don't have good counts on this, we'd have more birds in the trees and in the field, the periphery, than we had prior to any reaction. Your point is good; what your point is really is that each treatment area is different.

BECK: Each treatment is different, each dilution factor seems to be different, and each population seems to be different. I don't think we have comparable situations.

GUARINO: Dr. Schwab, what concentration were you giving these birds in the last test? Was it around 3%?

SCHWAB: I don't recall; it was several years ago. I do know this—we would have samples of 50 birds and they would all be intubed with a given concentration, and I think we used five different concentrations and five groups of about 50.

GUARINO: Well, this makes quite a difference in reaction.

SCHWAB: I didn't get this flight response in but 1% of the birds, no matter what concentration we gave them; it either killed them or it didn't. I seldom saw anything resembling this fright. I'm not saying it doesn't happen; I think John's work has shown this without a doubt. I brought the point out only because there was such a different result.

DeGRAZIO: One thing, we treat this bait with 3% and the LD_{50} to redwings is between 2 and 3 mg/kg so they're getting about a triple dose with this 3%, a triple LD_{50}.

FRINGER: Were you working with starlings or redwings?

SCHWAB: Starlings.

FRINGER: Oh, with starlings. That makes about as much sense as .....

SCHWAB: Yes, I know. I just brought this up.... I based the dose on what Balser told me would give the same response in starlings. I should have mentioned this to begin with; I'm sure I led John way off the track here. Have you done similar field trials on starlings and found anything comparable to what I've just said?

DeGRAZIO: Yes. We've had quite a problem with starlings regurgitating a lot of toxicants. We've been quite successful with DRC-1339 which is Starlicide for starlings in feedlots. And we have conducted some tests on starlings in feedlots with 1327 treated bait and we got some response, but not nearly the response that you get from redwings.

COMMENT: I've had the same flocking behavior with Avitrol and starlings.

SCHICK: John, did I understand you to say that with a dilution of 1 to 30 you reduced damage in corn by 50% and a dilution of 1 to 99 was about 70%?

DeGRAZIO: That's right.

SCHICK: What's the reason for this?

DeGRAZIO: There are two possible reasons. One is that we had fewer birds in 1967 than we did in 1966. Another possible reason is that more acreage was treated in 1967 than in 1966. One thing I might mention—this was on a voluntary basis by the farmer. If he wanted his fields treated, then he contacted the wheat grower's association; and not every field in that 500 section area was treated.

FRANCIS: It's an economic problem and the thing that's been bothering me ever since I got connected with these blackbirds is that on a large scale, like the State

of Ohio, for example, how much good are you doing by scaring blackbirds out of one field or out of half a field? Do you have any idea about that? Are any of these experiments on a scale that will tell you whether or not you will cut down the total damage equivalent to the cost of treatment?

DeGRAZIO: I have some theories on this. In South Dakota blackbirds don't have to feed on corn to survive. There is enough alternate food in these areas such as waste grains and weed seeds that blackbirds don't have to feed on corn. We don't know what percentage of the population is frightened out of one corn-field into another. We hope perhaps next year to instrument some birds with radio transmitters and follow these birds to see where they go after they're "kicked" out of a cornfield—do they move completely out of the area, do they go to another cornfield within the same area or more distant, do they switch their diets to waste grains and weed seeds? Every meeting I've been to someone has mentioned the same thing you have, and we'd like to have some very good data on that question; but we don't have it yet.

STOTTLER: I might add that in Coschocton we noticed there was a nearby foxtail field; after we started baiting there was an increase in the number of birds feeding on the foxtail.

BECK: Once again the two areas are not at all comparable. In Sand Lake you're dealing with a population that's pretty much in that area, aren't you? Not so in the Lake Erie region. You could take exactly the same set of data from both areas and I don't think you'd get at all the same conclusions.

DeGRAZIO: That's correct.

DYER: Two things have come out in the literature recently and are quite applicable. It involves two publications-, one by Hamilton in California on radiation of birds from a picenter, and the other one by Schoener from Harvard University on territorial density of birds of North America. If you put these two papers together you are forced to come up with some very interesting aspects. I'd like some more people to come up with thoughts on this. If you take Schoener's paper first, he predicts that the blackbird territory is a fixed area and in this territory the individual will be doing most of its feeding. When these territories collapse and the flock becomes gregarious, what we must find out is if that gregarious group consisting of a hundred will have the same total range in acreage as the hundred originally spread out as individuals. I hypothesize that they will. Therefore though the areas of Sand Lake and Ohio are different, in principle they will still be the same. This predicts that x number of birds can only feed and have to feed in a given area, no larger, no smaller, providing one thing remains constant, and that is the food supply. If the food supply becomes enriched, then Hamilton's paper becomes very important because these birds balance out their daily energy requirements by having to fly out, these flocks start competing. I think we have some data to show that competition becomes very important and these birds do get

further out. When the corn becomes ripe, they collapse right back in on Lake Erie again. These two concepts will have to have a great deal more work. We can predict in the future how far the bird is going to have to go with what particular food he has, and I hope we can judge our effort to spread Avitrol accordingly.

HARKE: Is there any indication that the birds will become habituated to Avitrol? Is it possible that a bird could react and the flock not leave the field more and more often? I guess it's a possibility, but is there any evidence?

DeGRAZIO: As you say, it's a possibility but we don't have any evidence along those lines.

SCHWAB: I think one interesting thing that came out in this controversy of starling versus blackbird was a complete lack of communication. For example, I had no idea that the starling and the blackbird responded differently to Avitrol, except for perhaps in amount of effective dose. Somewhere along the line, communications among people who are working on these compounds must be very lax. And I wonder if we here as professionals couldn't make some kind of gentleman's agreement to pass the word on a little bit. I frankly didn't think it made any difference if it was a starling or a blackbird on this Avitrol, provided the dose was indeed the type that should induce this response. I think the lack of communication was brought out very well in that one rather embarrassing point.

SHAKE: I just want to make one mention about what we did in Michigan. Our results weren't quite as enlightening as Don's were in Coschocton. We did notice that the birds were very susceptible to Avitrol; we'd get one or two reacting birds and we'd move several thousand birds out of a field. But then again there seemed to be a constant influx of birds into these fields, and so I just don't think Avitrol in some places very close to the marsh in Monroe County where we used it was effective.

I must admit that our first application was not a good one; it was quite windy that day and we thought that with cracked corn we would not have to worry about a problem of getting it on—we thought it would come right down and there would be no drift problem. We put it on and found that when the plane was going into the wind, he would put twice as much bait down; and then a little later he wouldn't get enough down. So we got a poor application in the beginning. And we were too late; we should have been out there a week or two ahead of time. But the second two applications went very well. We'll have to look at our final damage assessments to tell whether we did any good. But I know some of the farmers next to the marsh are not going to go along with this again next year because they felt they were wasting their time and money. They got just as much damage this year as last year. Then again some of them think it's just great.

JACKSON; I'd like to reinforce what Bill Shake was saying in Michigan and it certainly goes back to some of the early work John Beck was involved in here in Ohio with Avitrol. We found that in areas close to the Lake Erie marshes Avitrol was a

useless tool. The birds became distressed but the flocks were moving over in such waves that it had no effect on the flock organization. Certainly along Lake Erie we've had huge trouble trying to use Avitrol as an effective tool.

BECK: I kind of think we educated some flocks, but by the time we had educated them, the corn was gone.

HARKE: Last year Hal Stickley and the group from Patuxent were up at Ottawa Refuge, and they painted Avitrol on the ears of corn; and their conclusion after a bunch of statistical analyses was that the Avitrol in this line of work does not work as well as exploders. Exploders do work under certain circumstances, so I wouldn't just throw it out even in this area.

STOCKDALE: I'd like to move directly into a discussion of the use of sonic repellants and call on Bud Boudreau from Jennings Industry, Biosonics Division, Santa Cruz, California.

BOUDREAU: In years past I got some Avitrol and to satisfy my own curiosity I conducted some experiments. I was interested mainly in determining whether recorded distress sounds under the influence of the drug were as effective as the natural distress sounds or the alarm sounds. I found that they weren't. I found that there isn't any comparison between the sounds. In the course of these experiments, however, I was interested in knowing how much of this material it would take to produce a given response. I was using grain sorghum for bait and force feeding the birds, and I found that I got pretty good results from one to two kernels of treated sorghum. The elapsed time before the first reaction appeared and the intensity of the reaction was of course directly related to the species of the bird involved and also to the dosage. After force feeding five kernels to a redwinged blackbird, the bird went through the usual convulsions and salivated, and everything else, and then recovered. I kept the bird another week or so on normal feed and then subjected it to the same treatment again, and it recovered again. I didn't continue this too long, but it might be of interest to you.

In my work with biosonics, which involves the use of bird alarm sounds, we have had some very interesting developments in the past couple years. In the past two years we've had to deal with such things as acorn woodpeckers and scrub jays, California linnets, house finches, waxwings, and incidentally black bear. Some of these problems are really unique. I remember one in California where the scrub jays were harvesting a man's pistachio nuts, and he was the only grower of pistachio nuts in the whole area. The nuts were maturing at the same when the jays were carrying on their usual winter hoarding activity, and they'd just take nuts and drop them in a hole somewhere. It never occurs to a jay that the hole never fills up; he just keeps dropping them in there. What happened was that a jay found another man's irrigation pump that had a vertical hollow shaft motor on it, and he thought that this was a good place for his nuts, and he filled the motor up with nuts. The guy didn't know it, and when he tried to start the motor, he tore it up. It didn't take much of a detective to discover that those were pistachio nuts in there or

where they had come from, because this other fellow was the only grower in the country. So he sued him. The judge threw it out of court, of course. At any rate I suggested to the nut-grower that he take some 1 x 1 2 boards about 10 feet long and nail them into a square, close the top and bottom, bore a hole in the top, and let the jays harvest the nuts and drop them in this hole, put a trap door in the bottom and he could pull the nuts out. He thought that was a good idea, except that the jays like them a little less mature than they are picked for human consumption.

Another interesting problem we got into was acorn woodpeckers in dwarf apples. This is quite unusual. In fact I didn't know acorn woodpeckers even molested human food of any kind, but they do. The damage wasn't so much actually pecking the apples, but it was their sampling technique. They fly into the apple and make two little pecks about 3/4 inch apart. If they don't like the apple, they fly on to another one, but the first apple is already ruined with the two light pecks on it. We were able to reduce the damage by quite a little; at least the guy was able to get a crop off. When we got in there with our sound, he had about a 50% loss at that time. So I felt that if he got any crop it would be an improvement. He did succeed in harvesting 500 boxes from this 10-12 acres.

We have made considerable advances in the technique of analyzing bird sounds. The ideal objective would be to find the sound which would be applicable to all species of birds. From what we know now, it doesn't look like this is going to occur; there's too much variation in them. Having their own alarm sound, one species does not react to another bird's sounds; they're quite species-specific in that respect.

At the present time we have quite a few bird control units in operation in the table-wine grape vineyards in California. This problem has been intensified in the past few years with the invasion of the starlings into the Napa Valley area and the other table-grape growing areas of the state. Ironically enough, as you would expect, they don't take to cheap table grapes; they take to high-priced ones. They don't bother the $80 a ton grapes, they got to get the $350 a ton grapes. One reason for this may be that the higher-priced grapes mature fairly early.

The size of the actual grape berry doesn't make much difference. The starling is able to ingest quite a sizeable berry if he wants to. Most birds have to have a certain sugar content in the grapes before they become interested in them. This doesn't apply to the starling. I've shot hundreds of them in the vineyards with green hard grapes in them. The grapes were so hard and green that it was impossible to get any juice out of them to take a sugar concentration reading. The advance of the starling into this area has really got the grape growers up in arms, and rightfully so.

In a certain area last year there was a total loss from 80 acres of table grapes which were valued at about $350 per ton. The yield on this 80 acres would have been somewhere around 300 tons. This 80 acres was entirely stripped; not a berry on it. Of course there was no bird control at the time in this particular vineyard. Total loss in this vineyard from all bird species and all varieties of grapes was conservatively estimated by a very good authority at $45,000; this was one year's loss. In this same vineyard this year we have 24 automatic sound units in operation. The coverage

on these 24 would ideally be about 550 acres. Of course they are trying to cover 3000 acres with it and we're getting some thin spots. But in general the thing is working out pretty well as far as the starlings are concerned.

Not much has been done with biosonics and blackbirds for the past four or five years. One of the main reasons being that they have been so preoccupied with other species. But in years past we have done quite a little work in repelling blackbirds—redwings and yellowheads principally, some Brewer's blackbirds and the associated cowbirds—in Arizona and California.

As far as keeping them out of crops and off feedlots, it's a pretty difficult job. It's not so bad if you get there in the fall before the birds have established a firm feeding habit, but if you get there in the middle of the winter and the birds have become attached to the feeding area, they are extremely difficult to get out by any method.

In small selected study areas, we've been successful in reducing populations by probably 80 or 90%, but these were not over 5 acre feedlots. We haven't done well in large feedlots (40, 60, and 120 acres). We got shot down in one test near Blyth last spring which was just toward the end of the blackbird season. The feedlot was roughly u-shaped with 40 acres of barley growing in the middle. Blackbirds were nesting in the barley, and, of course, we weren't able to move those blackbirds one foot.

I don't know what I can predict as far as blackbirds and biosonics are concerned; they have quite a repertoire of sounds. We haven't analyzed all their sounds yet, but we find that they generally follow the usual pattern of other bird alarm sounds. We also find that cowbirds will respond when they're associated with blackbirds. But blackbirds by themselves in segregated flocks pay absolutely no attention to their own alarm sounds. So here we have the old situation where it works on some and not on others; that is as far as our present knowledge goes.

STOCKDALE: Bud, you've mentioned that you haven't completed the analysis of various songs. Bud represents an organization that is somewhat unique; he represents an industry that has taken a very keen interest in this. I'd be interested in a brief explanation of some of your laboratory analytical activities, if these aren't classified by Jennings Industries.

BOUDREAU: No, not at all. Actually it's very simple. A sound is picked up by a microphone which converts sound vibrations into electrical voltage. This voltage goes into an amplifier, then back out into a speaker where electrical voltage is converted back into sound vibrations. Between the time when you pick lip sound with a microphone and when you push it out of a speaker, you are dealing with electrical voltage which can be altered; it can be changed, you can do a lot of things with it, and when it comes out of the speaker it can be an entirely different thing. You can't trust your ears to compare bird sounds, but you can't go wrong by comparing pictures. It's possible to take a picture of various sounds with either an oscilliscope or a sonograph, and compare these. Now it's interesting to note that some sounds, alarm sounds in particular, have a very rapid rise time. In other words they start very quickly. We're beginning to find out just what limits this rise time has in order to become an effective alarm sound.

Also the sounds consist of various tones or frequencies; each species has its own frequency. This doesn't mean that there aren't some frequencies that are used by two or more species, but each one is identified by a particular frequency. This appears on the oscilliscope as a sine wave. The sine wave is broken in spots by what we call modulation, and each species has its own modulation pattern.

What I'm trying to do is to correlate and find whether we have any parameters in these sounds that are common to all birds. Of course, in order to do this, it's necessary to analyze quite a few birds to get a statistical base. This is what I do in my spare time. I don't know yet whether we're going to come up with any meaningful information from this long program of analysis or not; I don't think it's going to do any harm. At least it's giving us an insight into the bird's vocal and hearing mechanisms.

In my brief tests at Moody Air Force Base, we found that it was possible to jam blackbirds' communication sounds. You have heard them chirping back and forth; this is purely location sounds letting one bird know where the other one is. If you can jam these sounds, the bird cannot operate in this environment. He moves over into an area in which the sound level allows him to operate. I can't prove it scientifically yet, but I think this is what we did when we diverted streams of blackbirds coming into the roost around the air base with sound. To find out exactly what we were doing is going to take a lot more research than was justified in this particular case, but it is interesting. And this was the only thing I could come up with because there were only certain frequencies of sound that would cause these birds to detour, and they weren't alarm sounds. The bird while it was flying would not respond to the alarm sound, and when you think about it it's very simple why they won't. When you flush a bird with a sound he's in a flight mode, he's flying; and when he's already flying and you subject him to an alarm stimulus, he's not going to drop dead, he's going to keep on flying. He doesn't necessarily deviate. But these sounds that we used at Moody did cause them to deviate by a half mile around the air base. It was an interesting development and one that we all were surprised at. Any questions?

SEUBERT: Would an alarm sound of a flock of starlings in California work on a flock of starlings on the East Coast?

BOUDREAU: I think I can answer that. The alarm sounds of starlings that I recorded in Arizona were quite successful in Labanon, Algiers, and Italy. We have regional dialects, of course, in their territorial mating songs. I don't believe, however, that we have regional dialects developed in birds alarm sounds simply because it's an innate endowment, and I think alarm sounds would work almost anywhere as long as it's the same species.

SHAKE: Are you reproducing the actual distress sounds of birds or are you artificially producing these sounds?

BOUDREAU: These are natural.

SHAKE: I was under the impression that some companies along the same line as you produce artificial sounds, and I was wondering how they compared with the natural.

BOUDREAU: I have been experimenting with synthetic sounds. At first they were comparing quite favorably. Lately, I've not been able to do as well with synthetic sounds as I have with the natural. This is on different species of birds, however, so there is no direct comparison. I wouldn't say there are no possibilities there; but before you can produce a synthetic sound, you've got to know what the natural sound is made of.

DYER: As I remember basic physics, sound falls off as the inverse square from the source. What kind of power do you have to get to? You get fantastically oriented with power output don't you?

BOUDREAU: You're right there; sound intensity decreases by 6 decibels each time the distance is doubled. So you start at four feet from the horn. When you go eight feet you've already dropped 6 db, etc. Our present units have an output power of 60 watts. This gives them an effective range, I would say with starlings, of at least 1200 feet. But there's a limit to what you can economically produce in the way of an amplifier. If we assume that a 60-watt amplifier costs $200, and we want to double that and go to 120-watt amplifier, it does not cost double what the 60-watt costs, it costs more than that—it costs around $550. But in doubling power capacity, we have only picked up 3 db, and so there's an economical limit under which you can build commercial equipment. From that time on it's cheaper to put in more units than it is to try to reach out the power with one unit. This is the state we think we are at now—we think we are at the optimum power output.

SCHICK: Do I get the impression that the alarm sound is more effective than the distress sound?

BOUDREAU: Very definitely.

DYER: As a point of information, Gordon Orians has just published a comparative behavior paper at the University of California, Berkeley, on the redwing, and yellowheaded blackbirds.

BOUDREAU: It's interesting to hear about the effects of Avitrol on attracting birds—those birds coming in the mob. You get the same thing with the distress sound. It is called the positive phonotaxis, and it is quite common with certain species of birds that habitually do mob predators. You can create a response like this with blackbirds very easily. In fact it's downright embarrassing I found out in my early work about ten years ago. I was trying to demonstrate this thing and show how good it worked. The first time I played it, thousands of birds came overhead. But you don't get this so much with the alarm sound, but distress sounds, yes.

GREENLEAF: What do we know about the sounds that really are so high that they are inaudible to man?

BOUDREAU: We know quite a little about them. We know that in general bird's hearing range parallels the human range which means then that most birds can't hear ultrasonic sound. Of course, there are some publications and works indicating owls are more or less able to hear the ultrasonic squeaks of mice; and possibly other species which use echo-ranging techniques might have a little higher sound capabilities than humans. But for the most part we believe most of them can't hear quite as low or as high as humans. This means that if you're going to work with ultrasonic sounds you're going to literally have to burn that bird up with sound energy in order to effectively get him to move.

STOCKDALE: That's why the ultrasonic devices that are commercially available have application within structures—warehouses and places like that.

BOUDREAU: We haven't really worked too much with those, so I couldn't venture an opinion. It's been my experience with them, however, that they were not effective in open areas; I've not worked with them within a structure.

SCHWAB: It's my understanding that the ultrasonic sounds that have been used have a very short effective radius. Do you know anything about that, Bud?

BOUDREAU: It's common knowledge that it's very difficult to project ultrasonic sound any distance. It's not transmitted well through air. The higher the frequency goes, the narrower the beam is that you're projecting; and when you get up in a high enough frequency, you're projecting nothing more or less than a pencil point of sound energy. Of course, this same principle greatly elaborated is our microwave system of communication across the country. Ultrasonic sound is extremely difficult to project in air. It works very nicely in steel, wood, masonry, or some solid medium, and it works much better in water than it does in air. In air, your effective range is very much limited, unless you have a large amplifier, which is economically undesirable.

SCHWAB: The device I heard about had a radius, I think, of 30 feet for use on rooftops.

STOCKDALE: I think Huge Co. has a product on the market at the present time-two of them as a matter of fact: Ultrasons A and Ultrasons E are the product names. I did a little analysis of some of these devices to attempt to correct the bird problem in hangers at our University Airport. We determined very quickly, on the basis of the specifications, that it was economically not feasible to try to use this device under those circumstances.

HARKE: Have you tried other sounds in combination with distress or alarm calls on the recording, such as shotgun blasts or this sort of thing?

BOUDREAU: Yes, I've tried that. For those of you who intend to try it I'll give you a little tip—don't go out to record a shotgun blast in the open air because it sounds very flat, nothing like a shotgun blast. If you want to reproduce a gun blast all the way from a little 22 short to a 16 inch naval gun you do it by the size of the book you use—you take a book, open it up, and bang it together, and that gives you the closest thing to a gun blast in the microphone that you can get. If you take Webster's Unabridged Dictionary and have enough force to do it, you can get a 16 inch naval barrage out of the thing.

Actually as far as effect is concerned, I don't see any behavioral effect from the sounds. There are some interesting physiological effects of recorded bird alarm sounds. For example, if you get electrodes in a bird and then subject this bird to an alarm stimulus, you will, of course, get a rapid increase in the heart rate, possibly in the respiration rate. While you can distinguish between the heart rate and the respiration rate very readily, what you must get into actually is the bird's eighth nerve, the one leading from the ear to the brain. This in itself is not too difficult to do providing you're working on turkeys, but still, all you get there is the information that the bird has received the sound. From there on you get no information on what this sound has done - has it gone on into the brain? Or has it gone directly into one of the ganglia?

STOCKDALE: I'd like to ask Don Harke to briefly describe some of the experiences he has had with the use of wetting agents as a control technique.

HARKE: Until now we have been talking about methods of bird control which are non-lethal with the exception of Avitrol which kills some of the birds after they react and have served the purpose for which the material has been put out. Wetting agents are lethal avicides designed to reduce populations of birds—blackbirds, starlings—within their roost. Wetting agents are classed as surfactants, as are soaps and detergents.

We became interested in wetting agents quite some time ago, in the early 60's when I was at the research center at Gainesville, Florida, as a method of wetting birds down and causing them to die from exposure. We discarded this idea at the time because of possible contamination of water supplies and soil. However, with the advent of biodegradable wetting agents, we have become again interested in this technique.

The principle is simple, but the methodology is giving us a lot of headaches. It amounts to find a wetting agent that meets a host of criteria, finding a roost that also meets a host of criteria, and a proper method of putting the material on the birds.

The criteria concerning the wetting agents themselves are such that they must have soil absorptive abilities to prevent underground water pollution, they must have maximum wetting ability at minimum concentrations, they must be nontoxic to fish—this is most important where most of our Ohio and Michigan birds roost in marshes. Turgitol 1559, which is a Union Carbide product, is the wetting agent that we're currently interested in. We are also interested in some of the sucrose esters which are quite a bit less toxic to fish, but they are very difficult to get into active solution.

Our methods of applying the material have varied. One used a ground spray apparatus which consisted of standpipes put around the perimeter of the roost; water was pumped through a fire tank via an educer valve, which was set in the concentrate of wetting agent, up through the standpipes. Birds were herded through this curtain of wetting agent. Other methods we have tried use heavy aircraft—B26's and C123's—to dump a tremendous volume (900 or so gallons per acre) on the roosting birds. Recently we put a rather highly concentrated material onto roosting birds with a light aircraft (a Drummond AgCat with standard spray guns) where we put out about 25 to 29 gallons per acre. We anticipated that the added volume of water would be obtained through natural rainfall. This to me is a major drawback in this kind of a setup—you must depend on the weather which is usually difficult to predict. First the outside air temperature has to be cold, some mortality has been obtained through temperatures above 50°, but generally the consensus is that below 50° the cooling ability is much greater.

In effect the agent breaks down the oil in a bird's feathers, enabling the water to penetrate and soak the bird to the skin. In cold weather he is thus exposed to the elements without his normal fluffing of the feathers to keep himself warm. Mortality occurs through heart and respiratory system failure.

The three criteria which must be met before we can even attempt to spray a test roost are arbitrary. One is that we have a high population of blackbirds and/or starlings and low population of desirable bird species. Another criteria is that drainage from the roost be either slow or in the direction that is not into rivers which abound with fish or ponds or water supplies which may become polluted. We also require arbitrarily that the roosting material, the vegetation, be of little consequence, because some of these wetting agents are rather phytotoxic.

FRANCIS: I wonder, Don, since the effect is primarily one of killing, what is the rate of evaporation of some of these things? Could you lose enough heat by evaporation to amplify the effect.

HARKE: Frankly, I don't know the answer to that. I do know that the birds will be susceptible to wetting several days after they are sprayed with the material. This is what we were banking on in Tennessee—that we'd have a rain within several days of our application—which didn't come about.

SCHWAB: You said that this was more effective below 50° F. Is this on field trials or laboratory tests?

HARKE: This has been in laboratory tests.

SCHWAB: May I ask at what time these birds were treated in the lab, at what time of the day.

HARKE: Now this is the work that Jim Caslick did at Gainesville, and he has written this up in a report. He used a cold chamber where he could run the temperature to any predetermined level, spray the birds with a certain amount of

wetting agent, followed by water or by wetting agent in solution with so much water, put them into the cold chamber, and time the interval until mortality.

SCHWAB: Again speaking strictly about starlings and their resistance to cold, there seems to be a very great diurnal variation. For example, at 0° C on an immobilized bird with wings spread, which of course enhances the cooling, we found that a starling could tolerate 0° C only about two hours at nine o'clock in the morning. Now the same age starling, same sex, same size given the same treatment at five in the afternoon could tolerate the same temperature up to 14 hours. I wondered how this would affect the results of the use of a wetting agent? Granted it will be more effective than just the non-treated bird…but I brought this out because if this is a repeatable phenomenon, you might want to consider this when you're running some of these tests.

FAULKNER: We ran some tests at 26° F with a 15 knot wind at 9 o'clock at night and had very high mortality on a flock composed of 80% starlings and 20% redwings. At the same time the following night we had 42° F and almost 0 wind velocity; we still obtained mortality. We had excellent mortality for the birds that went to the roost through the wall of water. Here again, as Don suggested, it is getting the birds to go through the wall of water which is difficult.

SCHWAB: First of all, because of the limited time we can't talk completely about lethal control with toxicants; we can't talk about the cost, the ethics. I will say, however, that from what I've seen in California, the machinery that Bud Boudreau has been using has produced some very spectacular results and I'm quite impressed. But that hasn't stopped us from going ahead and looking at the use of toxicants as an auxiliary tool. In the use of toxicants on a bird population for reduction, there are so many things to consider that it's absolutely fantastic—all the ecological interactions that can occur with other avian species, with environmental contamination; it really is an unexplored field. One thing which does deserve some comment is that if you're going to use some toxicant to control a bird population, there are several ways which this can be done on a species specific basis.

One of these ways is through the development of a material which is biologically active only on the target species. We are a long way from this, although I will give credit to the Denver Research Unit for the development of 1339 which, to my way of thinking, is the most species specific material we have—at least in the difference between birds and mammals. This is a step in the right direction. I think we owe them a vote of thanks.

Another way is by the application of the toxicant. I'm speaking only on starlings. We've made a small holding cage of 1 by 1 inch wire—the size of this cage is really non-critical—about 2 x 2 x 4 feet. Into this cage we introduced a handful of starlings (6 to 10) to use as decoys. We placed this station in an agricultural situation, say in a fig orchard, cherry orchard, or a nectarine orchard. In conjunction with it, we put out small wood platforms on which we could place the toxic material. In this case we experimentally used 1339, basically the Starlicide formula that Purina is marketing now.

In one cherry orchard we determined that there were 16 species of birds inhabiting this orchard. We put up a bait station as I've described, and initially used

non-toxic baits just to see what would come in to the station. Much to our surprise and happiness only starlings came in. So we took it one step further, and we put on the toxic material which was exposed to any bird who came to that decoy station and wanted to help themselves. These stations were watched during the hours of daylight, from sunup to sundown. Again and again, we found that we simply sucked the starlings into that bait station where they would accept toxic bait. We did not draw in other species, with one exception, and that is an occasional mockingbird would come up, sit on the bait station, look in at the starlings, and "smirk;" they did not take any toxic bait.

I don't know what to say beyond this. It has proved very successful and I'm confident that we can go into an orchard in the early part of the season and selectively poison the starlings right out of there. Unfortunately other applications, such as feedlots, don't work so neatly. I'm using this as an example of what I mean by a technological species specific application. It's certainly not the only one; but it is encouraging that by using a bird's behavior and his flock reaction, we can indeed deliver species specific toxicants, even though they themselves may have application over a wide variety of avian species.

That is essentially all I want to say. I would like to know from anyone here really what is being done, if anything, on the possibilities of environmental contamination with DRC-1339. Is there any work being done on phytotoxicity, on the effects on fish, on snails?

DeGRAZIO: I don't know of any work that's being done with 1339, but I do know that it'll eventually have to be; and we have just recently at Denver hired a chemist to answer these questions on 1327 (Avitrol). It's going to be necessary for registration of any chemical that's going to be used on a food crop. We hope that this chemist can come up with a microanalytical technique for translocation studies, say through a corn plant, and soil contamination.

SEUBERT: I might contribute one thing. At the Patuxent Center, where they're doing most of the research in the Bureau now with surfactants, we are involved with the problem of environmental contamination. We have looked at the residues of 1347 (1347 is the base of 1339) on rice plants and have found that they are well within the tolerances for most predators on rice. Although FDA has not done too much with things as exotic as this, or with things like surfactants, we worked with the fish lab at Racine, Wisconsin on the fish toxicity. We have had the Wisconsin Alumni Research Foundation look at the phytotoxicity of a variety of materials as well as the residue problem. We have not gone into any of the other vertebrates as yet.

SHAKE: Did you attempt to use other species other than starlings in your decoy traps.

SCHWAB: No.

SHAKE: I was wondering if you put robins in there, would you only attract robins.

SCHWAB: I don't know. There are disadvantages to using a live starling as a decoy; they'd pick up a piece of bait, fly up, and drop it into the cage with the decoys. One other thing we did find out, which is interesting from an animal behavior standpoint, is that these starlings are all fledglings and they decoy better to an adult than they do to another young.

SHAKE: I have another question. I'm sure you're familiar with the large modified crow traps. Do you think this kind of application would work in conjunction with one of those, or do you think it would defeat your purpose by using the crow trap with the use of a toxicant?

SCHWAB: No, I think it would work very well. We've used traps as large as that, as large as three times the standard Australian crow trap, and we really found no difference. The only reason for the very small holding cages is simply ease of handling and ease of setting up.

SHAKE: But what I mean is: do you think you can take birds that would possibly not enter your decoy trap?

SCHWAB: Oh my, yes. We've set this thing up with Australian crow traps, and for every bird we capture in the trap, we figure we can poison at least a hundred. This is only in a couple of trials; I'm sure it would depend on the population. It's infinitely more effective, no question about it.

DUDDERAR: Do you think the selectivity which you obtained could have been to the bait to which the toxicant was applied?

SCHWAB: Possibly, except that when we first tried this we used a wide variety of bait and did get visits with other birds. But here baits were exposed without any decoy. It seems like that not only does the starling decoy entice other starlings in, but he's so darned obnoxious that the other birds stay away. Anthropomorphic as it may be, that's the way it looks.

BOUDREAU: What was the stage of maturity of the cherries when you started this program? And, did you continue it through the entire cherry season?

SCHWAB: At the time the program was started, the birds were actually eating the agricultural crop, whatever it was. We didn't have to trap clear through the season, because in a couple of days it was all over. This is not to say that another flock wouldn't move in, but we had the vast majority of starlings out of there in a couple of days.

QUESTION: What was the bait that you finally used in the cherries?

SCHWAB: It was the same as this Starlicide - the pellet bait.

QUESTION: Did you mix it with any fruit, raisins or anything else?

SCHWAB: No, just the bare pellets. Now, in another case where we did use a different bait, we simply prepared a very concentrated 1339 solution; and instead of putting out the pellets along the baiting tray, we took a sprig of grapes off the vine, dipped it in, pulled it out and threw it up there for bait. We figured with a concentrated enough solution one grape is lethal. There's no soaking; just dip it in and place it up on the tray station.

GUARINO: I have to back up some of the studies you've just mentioned. We've run some similar tests in the Denver area, and we've used crow traps decoyed with starlings and common grackles with bait trays using Starlicide pellets. They were pretty selective to starlings and grackles. We tried to reduce the numbers of an urban roost this way. We had some fair results.

SCHWAB: It would have been interesting to see if the starling alone would have attracted only starlings as has been our experience, and the grackle only attracted grackle.

GUARINO: We used both species because the roost was composed of both starlings and grackles.

SEUBERT: What do the California people know about the rate of degradation of 1339 in the environment?

SCHWAB: Not a damn thing. We've heard rumors that it is broken down by ultraviolet. I don't know. Certainly, we have mixed established concentrations of 1339 and determined what it does to birds at a given time. We've had these solutions setting in a room like this, just open with sunlight, for over two years and then evaluated them; we could find no degradation. Of course, this is inside a bottle. I exposed some to ultraviolet overnight, and I could find no change. Again these are not extensive studies.

STOCKDALE: We've talked about a lot of different techniques, from scaring devices through to lethal control techniques. I think we've all assumed up to this point that we could measure the effectiveness of any one of these devices in a particular way. Those of us working in some of the eastern states may have reached a stage of development that some other states may not have yet reached. Our public and especially our legislators are interested and are asking for absolute figures on the extent of damage being caused by each of these species. The legislators are interested in knowing just how much they are loosing before they pass on more research funds. At the same time we're testing a good many devices, yet how do we determine the effectiveness of a particular device? As a result all of us who research this problem have decided that there is some need to do a damage assessment, to develop some technique whereby we could measure the extent of loss in any crop. For want of a better method, I think each of us has pretty much developed our own method. We've seen in print, at least in the circulation of the Fish and Wildlife Service publication, what I have come to know as the DeGrazio method of determining the extent of loss. We know that Jack Linehan of the Fish and Wildlife

Service has worked extensively with what I have come to know as the Linehan method for damage assessment. Mel Dyer worked up a technique while he was up at Ontario which was a modification of the DeGrazio technique. What I'm getting at is that each of us seems to have developed a technique which seemed to fit the particular condition that we were working with at the time.

There is a real interesting organization of people interested in blackbird research. This is the Northeast-49 Cooperative Research Project composed of representatives from several states who banded together about five years ago. They now do some cooperative research using regional research funds from the Department of Agriculture. Unfortunately Ohio does not belong to the northeastern states; but not being able to stimulate the generation of a similar project in the north central states, we kind of jumped on the coattail of the northeastern states and now have a very active research project. The first objective of our research program through the NE-49 Cooperative Research Project was a need for a damage assessment technique which would have application across state lines.

Some of the people involved are Jack Linehan, stationed at the University of Delaware; Phil Granett, stationed at the experiment station in New Jersey; Don Messersmith, working with the experiment station in Maryland; and myself, working with the Research and Development Center here at Ohio State. Bob Fringer works closely with Phil Granett at New Jersey, and I'd like to ask Bob if he could very briefly describe to the group the approach we're taking.

FRINGER: Tom, listening to you talk about the different systems reminds me of the blackbirds using the "Columbus system" of damage analysis, which is just to find it and land on it. (Laughter) Actually, what we've tried to do is feel our way through in selecting a practical solution or a method of making evaluations of our attempts at damage control.

We picked two fields in each state—Ohio, New Jersey, Delaware, and Maryland—of about a half an acre each. We took a look at each ear on each stalk. These fields were broken down into sixteen different blocks; there were twelve rows to a block and they were forty feet long. We put them all on IBM sheets, broken down according to damage classifications from 0 to minus 5, minus 5 to minus 15, minus 15 to minus 35, and minus 35 to minus 70, and over 70. We picked representative samples throughout the fields while we were making these classifications, so that if we went through and felt that a certain ear was a minus 70 classification, we would pull ears at that time which we felt were minus 70, so we could go back later and really see if they were minus 70. We're in the process of doing that now. This will give us a check on how we're classifying these. We found that in what we called damages over 70+, if we figured out damages by row centimeters, damage is around 41, 43, or 45%. In essence we're trying to determine a way of making a good practical damage survey. These things will all be run through our computers at Rutgers, and I think they will provide a lot of information once we ever get through it.

HARKE: How do you handle mold problems in the corn? If you go by at a certain time and see 5-10% damage on an ear, how do you account for mold that may later develop and really ruin a whole ear?

STOCKDALE: Bob, you may want to take a crack at this, but let me answer you quickly how I'm coping with it right now. I'm running into this problem because I took samples two weeks ago which were put into paper sacks which were put into larger paper sacks and then taken back to Columbus, Ohio. They've been sitting for two weeks in high humidity in the sacks because the corn was still wet, and I'm getting quite a bit of mold and some sprouting. But I've discovered that even in those ears that are quite moldy and well sprouted, you can still differentiate between bird damage and losses that might occur from these other causes. Bob, have you had the same sort of experiences in New Jersey?

FRINGER: Jack has had the same sort of experiences. But we heard about it and we pulled our ears out. I don't think Don is referring to that. I think he's talking about total economic damage when you finish. Really I don't think we've taken that into consideration, and I don't know how you could.

STOCKDALE: To my knowledge it has not been taken into consideration, and further I don't consider this a problem that we as bird biologists need to concern ourselves with. I'm saving a good many ear samples, and I plan to turn these over to our Department of Agronomy at Ohio State University. I think it's their job to determine the economic effects of molding and sprouting that may be a secondary loss which may or may not be directly related to the mechanical damage done by birds.

FRINGER: I think that every year this kind of damage is going to change. If you have rain after you have damage, you're going to get bad molding problems. But if you don't get the rain, then you won't have the problems.

HARKE: Yes, but the idea is to compare from one year to another. You should have the same relative method set up from one year to the next so you get comparable data.

BECK: They are separate but related questions.

STOCKDALE: I think you're right, John, but it goes beyond the work that the bird biologist ought to be doing. I contend that our loss in Ohio is probably greater due to secondary factors that occur than the mechanical damage done directly by birds. We blame a lot more of it on birds than may be true.

BECK: I've been working with Blair Jansen this past year down at Ohio State, and he identified some of these ear rots that came in. I don't really think you can separate the mechanical damage done by birds from the resulting invaders that come as a result of the ear being opened. I think that this is a rather arbitrary thing to say that you can count the mechanical damage, but you can't count the secondary molds and ear rots and this sort of thing. I think you need an agronomist working with you, true, but I think they pretty well know already what the normal amount of ear rot or mold is in a given section of the state. And I think what we need to do is to include that in and count the difference.

STOCKDALE: I may have misled you in my answer earlier. I didn't mean to suggest that we weren't measuring the other or that we weren't considering it. What I meant to say was that in our particular study we are only concerned with the damage that's done directly by the birds, and in the lab we can differentiate between the mechanically damaged kernels and a kernel which has sprouted or which has molded or something like that. But I agree, John, this is an area where we need much more agronomic research. We need the results of their work to add to that which we are determining before we can finally come with that thing that many of us are constantly being hounded for, and that's a dollar loss figure.

BOUDREAU: We have a similar situation in the wine grapes in California. The birds come in, peck the grapes, and then the bees come in after the birds have opened the grapes up. The pickers won't come to pick the grapes because the bees are there. So we've been attributing all this loss, loss of the unpicked grapes and everything, to bird damage.

DYER: How do you plan to account for various year classes? You're going to do this successive years and damage appraisals ought to be compared; I wonder what your plans are for 1968 versus 1969, 1970, and so forth.

STOCKDALE: I don't think we've even gotten to that stage yet, Mel, other than to just think about it. We're still trying to find a method which we all can use, and which will give us some basis for comparison in a single year. From there we'll broaden to subsequent years.

DYER: This is the major problem. First of all, when you're dealing with percentages, percentages of what, from area to area, year to year? I know I never have liked this; statisticians get pretty excited over this type of thing. It occurs to me that you've got all your data here. If you save all your cards from year 1, year 2, and so on, and you keep stratifying them year after year, then you let the computer do the work, and you come up with an actual fancy analysis of variance to show whether there is yearly significance. I would suggest at this time that you simply file all these, collect comparable data next year, and compare the two, then compare the following year and so on, 25 years from now you'll be. . . .

COMMENT: You'll be an old man. (Laughter)

DYER: No, this seems to me to be a logical method. You've got something started there; I'd hate to see it dropped.

FRINGER: Do you think there'd be that much variation between fields, or would you select approximately the same location, the same area?

DYER: The question always comes up whether to resample the same areas or to reallocate the sample points. I don't know that answer; I don't know whether anybody knows. If you speak of 25% up or down from one year to the next, it

doesn't mean anything to anybody. So I'd hope—you're off to such an auspicious start—that you'd catalogue these from year to year, and let IBM do all the work.

FRINGER: It seems like a lot of work. Tom's working all alone, so he's really breaking his neck to get this done. A two-man crew can probably do the work in about six working days. This is everything—putting down on paper, going over and checking them. The work with measuring row centimeters takes a little longer, and I don't even know if that is necessary. It seems to me that somewhere along the way we're doing something relative to the centimeter thing—we're assuming that each kernel is the same size which is wrong right there.

DYER: Regardless of the method you come up with, I'd like to see it done inter-regionally and year after year. That's the important thing. It's important to the corn industry and it's important to the bird studies.

FRINGER: I think this is our ultimate objective. We're trying to develop a technique now, or at least we hope the computer and the statisticians will give us a technique, which will not require the man-hours of time which are being expended this year. Then in subsequent years as a regional project we can, using the technique that results from this study, begin this annual accumulation of data; then have the data to compare with past years, interstate, and so forth. I think this is our real intent behind the whole thing.

STOCKDALE: Folks, we've been at it for nearly four hours, we've heard of several subjects, there are subjects relative to this problem which we have not touched on, and I think it only proper at this time to begin open discussion.

BUSWELL: I'd like to ask a question of Mr. Guarino. You mentioned oral poisoning of starlings in an urban area, is that right?

GUARINO: I mentioned a test where we were trying to reduce an urban roosting population through baiting using decoy bait traps, yes.

BUSWELL: Were you able to get the starlings to eat in the city?

GUARINO: No, we had the trap sites away from the city in the feeding areas that they used during the day.

SCHWAB: I'd like to illustrate part of our communications problem by taking two recent publications from the Denver Wildlife Lab. One of these papers was on the use of Starlicide to control starlings in a turkey feedlot. This paper appeared in the Journal of Poultry Husbandry, I believe, which is very appropriate, because I'm sure these turkey growers should be aware of this, and this is one place to make them aware of it. But how many of the rest of us saw this paper? Now a second paper, which was very excellent, and came out from the Denver Center by Dan Thompson, was on the digestion of starlings; and this appeared in the Journal of Laboratory Animal Care. Again, who of us saw it? If we are going to up the publication rate

of this type of information, we've got to have someplace in which these papers can appear without waiting for a two-year time lag between submission and publication and yet reach a wide audience. I don't know what the answer is, but to me it's a problem, and an important problem. We simply don't know what others are doing.

SEUBERT: Insofar as real research is concerned, many of you have seen these work unit progress reports. We send these to all interested parties, mostly people involved in research or management. The contents of these reports are not considered finite. I think the Denver people make these available to interested parties as well. So as far as the Bureau is concerned, our material is available before publication usually in some form of publication. I think if you ask to be placed on a mailing list, we can solve at least this part of the problem.

SCHWAB: Not all information is released in these bulletins either. I'm not talking just about the Bureau; I'm talking about country people, state people, universities, the whole smear-we don't have an adequate outlet for our stuff, and I think it's really slowing down the progress.

STOCKDALE: I see two problems, Bob. One is outlets for publication. The other is a problem of finding the material once it has been published. I think what we need is an executive secretary through whose office every bit of information on bird control that any of us might run onto might be funneled, subsequently reproduced, and sent out to all of us who are in a special interest area. I don't suggest that anyone appoint me an executive secretary, but I think my organization would be happy to go along with the idea.

SCHWAB: The time lag is what I'm getting at. First the man prepares the article, he sends it in, then waits eighteen months until it's published, then waits another six months until it's listed in the Wildlife Review.

MITTERLING: I find that what he says is quite true, and another thing you have to combat is failure of a good many editorial people to understand what you're saying when you write up a pest bird problem. They are not familiar with it, and perhaps it's being published in a journal where they do not ordinarily deal with problems of bird control. Some of the reviews that you get back from these may make you feel like going in there to fight.

BECK: I'd like to make a comment here in relation to this. I don't know how many of you have attended these meetings in the past, but there has always been a Proceedings which has been rather complete, sometimes almost verbatim. And frankly, there isn't a whole lot of progress from one meeting to the next. Sometimes it seems like in that anything that happened two years ago was reported two years ago. Anything that happened in the interim has pretty well been reported in this conference. I don't think you'll find any major accomplishment in bird control that has not been reported in one of these Proceedings from this conference right here.