PRIMARY GOALS FOR 1991

1. Establish an experimental study area with T-males and C-males and other control-control areas with unimplanted males.

2. Carry out focal males watches on T- and C- males in spring (see grant proposal).

3. Collect transmitter data on home ranges of T-males and C-males at various stages of reproduction (see grant proposal).

4. Collect DNA samples from adults and nestlings in both study areas (see grant proposal).

5. Observe behavior of T-males and controls toward fledglings and measure relative fledgling survival.

6. Continue to monitor demographics of population including rates of return by sex and age classes, site fidelity, population size, etc. As part of this it is especially important to compare the rate of return of males whose implants were left in at the end of the breeding season in 1990 and to quantify their molt state. Also as part of this we wish to document the changing composition of the population as the spring progresses, e.g., how does sex-age structure vary with date in early spring?

7. Determine whether and when the dominance relations between the sexes change during the spring. How about between the races.

8. Compare testosterone levels in T-males, C-males, and, especially, control-control males caught off the study area. Collect blood to determine whether corticosteroid binding proteins are higher in T-males than in controls.

9. Can we distinguish between when young and old males become territorial. We know that the age classes differ in body mass and in testosterone. Can we tell whether old adults become territorial (i.e., start to display, I guess) earlier than young adults. Does treatment affect the answer?

SECONDARY GOALS FOR 1991

9. Quantify effect of male treatment on begging calls of young in nests of T- and C-pairs. Try to verify experimentally the effects of the implants on male responsiveness to begging calls. (Mess around with the idea that females are feeding in response to male song rates?) Make another valiant attempt to measure load size?

10. Determine effect of T-implants on incubation by male solitary vireos.

11. Perfect methods for measuring sperm, coccidial oocysts, and white blood cells.

12. Quantify habitat in order to determine what attracts juncos and determines their density.

GOALS FOR APRIL

1. Implant scheme

A basic warning. We do not want our manipulations of the birds to interfere with our measurements of paternity. It is quite important to minimize the length of time birds remain in traps and/or how long they are kept off territory. We do not want neighbors inseminating females whose mates are sitting in traps! We need to employ sound judgment *every* time we handle a bird and keep complete notes about possible interference with the birds' lives.

An important consequence of this need prevent experimenter-induced EPFs is to MINIMIZE THE USE OF BAIT, especially after the first sign that females are ready to lay eggs. This too will call for sound judgement. We need to catch the birds, we don't want to alter their behavior with our bait.

Variables to consider in determining an implant scheme include (1) how we should structure the experimental study area in terms of the spatial juxtaposition of T- and C-males, (2) how to treat males that were also treated in 1990, and (3) which areas to set aside as control-control areas without compromising sample sizes in the experimental portions of the study area.

(1) Spatial juxtaposition of T- and C-males

Ideally, in order to test experimentally whether "a novel phenotype would spread," we would have areas of just controls, areas of just T-males, and areas where the treatments were evenly distributed, so that the treatments could "compete" against one another. Then we could quantify how C-males compare to one another, how T-males compare to one another, and how the T-males compare with C-males. Even more ideally, you would proceed the way a modeler would and assess the effect of varying the proportions of T- and C-males.

The reality is that our samples are not that large, *and* we have to treat some of the males before we know where they will be settling to breed, *and* we need to take a structured approach to the treatment of returning males. Consequently, we have relatively little control over the treatment groups of neighbors. This is really a serious problem, and one that we have given a lot of attention to, but it is not one we have conquered. When we assign treatment at random, as we have always done, blocking by age, sub-portion of the study area, and order of capture, we always end up with little clusters that are biased toward one treatment or the other, but are not pure colonies of either one. I suspect that will be the result this year too; does anyone see any way around this?

Thus for young males, i.e., first-year birds that have never had territories before, we need to assign treatment at random for each portion of the study area (Hotel, WVS between WPR and the Hotel, WVS between the station and WPR, WPR, The Station, Jungle Trail). We can proceed as follows: for each site, or as we have done the last two years, for each capture location, you toss a coin ten times (heads are T, tails are C) before you begin, in order to determine the treatment that should be assigned according to order of capture. Then each time you catch a young male at a particular site, e.g., on the station, you check to see what treatment is due to the next station-caught young male, and you do it. After you have treated 10 birds, you toss the coin again.

Adult males that were *not* treated in 1990 should be treated like young males. That is, within sub-areas of the population, they should be treated at random. Chances are you will encounter very few of these because site fidelity is so high that almost all old adults you meet will have been there the year before.

(2) Returning males that were treated in 1990

For adult males that bred on the study area last year, I think we should simply give them the opposite treatment from the one they received in 1990. In all years except 1990, that is how we have treated returning males. Most males are not in the breeding population for more than two years, so this does the trick. For the relatively few males that are there three years, they simply rotate.

In 1990, I was worried that the change in treatments between years might have affected our measures of mate fidelity. The reasoning was, how could you ask whether being mated to a T-male in one year affected mate fidelity in year 2, when the question was confounded by the fact that the male's treatment in year 2 was different than it had been in year 1? Therefore for one year, 1989 to 1990, we maintained treatments between years. We observed no difference in mate fidelity, as compared to earlier years, so the mate fidelity question appears to be dead (females do not use a male's treatment to determine whether to be mate-faithful). Consequently, I think that the independence of data points between years is the more important consideration and that we should return to the practice of alternating treatments between years.

There is one thing that is different about 1991, and that is the fact that we did not remove implants in 1990. Therefore, returning T-males could be in worse shape than returning C-males, e.g., if they molted incompletely. Returning control males do not seem to pose any problem, except one of symmetry if you treat them differently than you treat returning T-males.

My guess is that any returning T-males will either be dead, obviously in bad shape, so that we can simply decide in advance to ignore them, or OK (e.g., because their implants actually fell out in late summer so they molted just fine). Therefore, when you catch a male that was treated in 1990, my reaction is to say treat him anyway and give him the opposite treatment in 1991 to the one he received in 1990. I would be glad of any input on this problem.

Upon reading this, PR suggested that we could just catch and remove all returning males. That would be hell on demographics, but the fact that we left the implants in last year is already hell on demographics, so we do not have a great deal to lose.

(3) Relative size of experimental and control-control areas.

The answer to the question of how much area to devote to the experimental study area depends upon the number of people we can employ in the field, the density of breeders this year, and whether the chipmunks are up or down in abundance. If we have lots of people, we can afford to go farther afield to look for control-control nests. If we have lots of breeders, then we don't need such a large experimental study area. If chipmunks are down, then we can get a sufficient number of blood samples from nestlings on the core experimental area, as well as sufficient information on the behavior of males toward fledglings, etc.

Summing up all these considerations, it seems to me that the experimental study area should be at least as large as last year, possibly larger. Ideas?

2. Sampling procedures and processing birds in early spring

I am sure that we are agreed the we should implant the birds as simultaneously as possible, which requires that we rotate regularly through the various sub-areas of the study area. It is not a good idea to treat all the birds on WPR and then move to the Hotel, etc.

Also, we want to avoid over-investing in areas early in the year that may dry up later, e.g., Jungle Trail which becomes almost unworkable because, at least in some years, the birds disappear and the nests are so difficult to find.

As much as possible, we want to space our nets evenly along the trails and use traditional net and trap sites, so we can compare samples across years. It is important to keep a record of net hours and trap hours, so we will have some index of the effort expended. This might be easier if we made up some record keeping forms in advance.

We also need to standardize the data we gather on birds, both in terms of the variables we measure and the way we make the measurements. As always, we need to measure wing, tail, tarsus, mass, fat class, covert molt, eye color, presence or absence of a brood patch, and cloacal protuberance (diameter with calipers or a drill bit measurer and height on the anterior side of the clo pro with a mm ruler). (Have I forgotten anything? Should we modify our data sheets in some way to give people reminders about what to measure?) As always, decisions need to be made regarding each bird's sex (M, F) and age (N, J, A,Y, A,O). Ray and Zig and Lise should make conscious efforts to confirm that their methods are identical, e.g. measure each others birds, calibrate an object of know mass against one another's pesola balances, fat class the same bird independently, etc. Kevin should point out when he is given conflicting instructions, since that will help us recognize where we are differing among ourselves.

Each time we handle a bird, we need to note whether we have done anything to harm it. If you think you have done something that makes it unlikely that the bird will be caught again, e.g., broken its heal, you must indicate this on the banding sheet at the time of capture. These decisions are very much harder to make later.

If we go out to catch a particular bird "on purpose," e.g., because we needed to bleed it or to know its mass at nest-leaving, or whatever, then we must make a note to that effect. This is so we can eliminate such birds from the sample when we analyze the data base later for seasonally varying population structure. This is because birds we set out to catch on purpose cannot be treated as caught at random.

Please follow Zig's guidelines about putting new captures on separate banding sheets than recaptures. This hugely simplifies the task of reporting our activities to the banding office and makes it easier to enter the data into the computer. It is also

It is also important to record all encounters with birds on the banding sheets, even if the encounter is recorded in some other way. That is, if a bird is bled, then that fact is noted on a bleeding sheet, but that does not relieve one from the responsibility of recording the encounter on a banding sheet.

Sightings are one of the most difficult categories to deal with. Some birds may be sighted in early spring and never seen again; others are seen on a near daily basis and we grow weary of recording the fact. Sightings should be recorded on banding sheets that contain only other sightings, not captures. Sightings in early spring should have some statement regarding how certain the observer was about the identity of the bird seen, e.g., "perfect view of bands in excellent light," or "certain of left foot, but the red of the right foot could have been orange." We still need a way to deal with how to record the presence of birds we see continually, e.g., because it lives near the lab building, without overloading our data base.

3. Blood sampling, 1991

We have been sampling seasonal profiles for some years now, so our minimum objective here is rather modest. Each year we need to confirm that the implants are working. That means catching 15 of each treatment group under conditions that give reliable estimates of T (no lures, quick catch, etc.). I think that this is simply a must.

There are several questions that would be nice to address, if we could find someone interested in the question and willing to catch the birds. We know very little about *natural levels of variation in T* among males of the same age and stage of reproduction. I don't know of anyone who has attempted to document natural variation in T in the field, much less relate it to variation in behavior or correlates of fitness. Are some males high secretors of T and other more modest secretors? What is the shape of the frequency distribution? Do returning nestlings resemble their fathers in their levels of T?

It would be nice to sample control males at two stages, when their females are *known* to be fertile and when they probably are not, because they are incubating or tending nestlings. This work is best done toward the beginning of the season because the birds will still enter traps (but take care with bait).

To do this right, we would work off the study area, because it is possible that associating with T-implanted males influences T in controls. This is what Wingfield found in song sparrows, and it is something that we have never checked out. So it would be good if we got some samples from off the study area as well as on.

4. Dominance relations between the sexes

This is something that really interests me and if we could figure out some cheap and easy way to gather the data I think it would be worth a quick note. Someone could then follow up with a longer project.

People could observe at nets as they are waiting to catch birds for implant. You could try both scan sampling (write down every encounter you see as best you can) and focal animal sampling (select a female and watch her until she is involved in some kind of an aggressive encounter and note the time elapsed). Many focal animal samples may end in the female's being chased off in flight, and you will have no idea who the chaser was. Others may end in her displacing another junco, note his ID. etc. If each of us gave this 30-45 min. per day for four weeks, we might be surprised at the results.

Side projects of interest here are relations between northerns and carolinas. It seems possible to be that encounters between the races might decrease before their departure rather than increase, but the reverse may be just as possible. There are interesting speciation questions here that might be addressed, e.g., at the end of the pleistocene was it the larger individuals that remained near their natal sites to breed, and the smaller ones that had to disperse prior to breeding?