NCC PINK BOLLWORM TECHNICAL ADVISORY COMMITTEE MEETING
Thursday, March 19th, 2009, 9:00 a.m.

A meeting of the National Cotton Council Pink Bollworm Technical Advisory Committee was held at the Arizona Cotton Research & Protection Council office located at 3721 East Wier Avenue, Phoenix, AZ 85040-2933 at 9:00 am.
Chairman Bob Staten called the meeting to order at 9:05 am. Chairman Staten requested a roll call. The roll call reflected a quorum:

In attendance:	Bob Staten, Charles Allen, Larry Antilla, Ted Boratynski, Tom Miller, Steven Naranjo, Jim Rudig, Don Parker and Robert Hull.
Absent:	Joe Ellington, Jim Ed Miller, Craig Brown, Steve Birdsall and Bruce Tabashnik.
Also present:	Dr. Richard Zink, Director, CPHST, Phx, AZ and Ft. Collins, CO; Edward Herrera, Texas Boll Weevil Program, El Paso / Trans Pecos, TX; Ernie Miller, Facility Director, PBW Rearing Facility, Phoenix, AZ; Greg Simmons, CPHST, Phoenix, AZ; Michelle Walters, USDA APHIS PPQ CPHST, Phoenix, AZ; Cloti Tate, USDA APHIS PPQ CPHST, Phoenix, AZ; Leighton Liesner, ACRPC SIT Program Coordinator, Phoenix, AZ; Mike Whitlow, ACRPC Program Coordinator, Phoenix, AZ; and ACRPC Secretary Letitia Tamulis, Phoenix, AZ.

Chairman Staten briefly went over agenda items and reviewed what was to be discussed. He advised that time is running out to get any transgenic in the field and stressed the need to have very definitive, very analyzable data on competitiveness on the DS Red pink bollworm strain versus the standard APHIS strain. The objective is to obtain enough solid information to confirm whether or not the insect is either as good as or better than the APHIS strain. The test run last year was more operational. The DS Red did not perform as well, and there was some concern that it was simply the difference in zones. We will need highly analyzable replicated data to clearly illustrate any differences.

Dr. Staten stressed the importance of the decision we make as to whether or not to incorporate DS Red into the facility. Once we go into the facility with it, we cannot rear two insect strains unless someone will maintain a strain somewhere else. It has to be all DS Red.

There are two proposed field trial options that are on the table: the San Joaquin with Jim Rudig and Greg Simmons, and in Arizona with Leighton Liesner and Cloti Tate.

Jim Rudig reported that we are trying to obtain comparative data on the two strains. In San Joaquin Valley, they have 14 sections and looked at different release rates; 75 moths/acre/day (lowest number of moths necessary to effect control); 150 moths/acre/day (more consistent with average release rates used over the past 5-10 years); and 200 moths/acre/day (what we use to start off our releases which gives us the flexibility to be able to add sections). Jim distributed a hand out describing the proposed trial in detail.

Greg advised that the goal of this trail is to conduct it in an area in order to obtain stakeholder acceptance, CDFA testing (so they can have access in using the technology), as well as getting more comparative release data. This is not an operational experiment. In the field lab, California performs 48-hour mating and seven-day mortality. Upon inquiry, it

was clarified that CDFA is looking at practical applications of this moth including how they will use it in their program.

Greg stated that it is also a comparative experiment as they will be comparing the two strains. Greg discussed many specific details that were noted in the hand out. Jim did not agree with the facility rearing the strains and CPHST collecting them in their own collection systems. Greg's position was to control everything possible on this test including rearing and felt that it contributed to continuity. Dr. Staten agreed with Greg. Greg went over further details referring to the hand out regarding releases and shippers and aircraft. Greg discussed every other day shipping a different strain, stating that it would be easier on him rearing and handling the moths here, preserves the ability for CDFA to easily do their quality control testing in their lab, as well as saving costs on shipping without having to deal with extra boxes. Greg reported that with using the alternating box method, they will be operating on a two-week cycle.

Steve Naranjo asked if the goal of this experiment is to take the information gathered to a peer review journal in order to get it published. Dr. Staten advised that it was. Steve Naranjo asked questions concerning replication. There was much discussion regarding how to break down the areas. The possibility or running the identical trial in Arizona was discussed, but subsequently dismissed due to the variation in the environment. There was consensus that key factors would include having big enough blocks of cotton to be able to achieve the goal, having them close enough to where the environment would be similar, yet far enough apart where drift or wind concerns would not be an issue. Upon inquiry, Greg answered that the statistician from Imperial College is an independent source.

Don Parker likes the idea of rearing both of the insects together. Rearing insects can have a big impact on the fitness of the insect. Consistency of personnel, diet, etc. can have a big impact on the fullness of the insect. Charles Allen asked Steve Naranjo if you could replicate based on an area of San Joaquin including collecting very specific trap information and segregate the data that way and consider those replications. Steve Naranjo said that it is definitely a possibility, and the key criteria are keeping them independent of one another. Jim Rudig distributed maps showing the areas proposed and answered questions regarding field size and size of the blocks of cotton. Charles Allen clarified and Steve Naranjo agreed, that the factor you are measuring is the response to traps between these two different strains, and that different places are replicated by geography. Charles stated that it would need to be run by some statisticians, but felt it would work. Steven Naranjo concurred.

Charles was concerned with the frustration we have encountered with dyed moths in not being able to isolate a moth based on the time it was released. We do not know when we pick up a moth that doesn't have color, how long it has been out there. He sees that same concern here. You lack having that longevity in the field. Greg advised that he has some longevity data from field cage studies. Greg offered that he would be able to conduct that again for this experiment.

Charles stated he would like to see a single release study (DS Red & lab moths) one time and then you trap and determine how long you capture those moths comparatively based on the number of moths that were released. Then you may replicate that over time in order to get your replicates. Dr. Staten said that could actually be done as a separate experiment, and it doesn't necessarily have to be done in San Joaquin. It may be more advantageous to do it closer to the lab. Greg stated that he has some of that data already and his intention is to produce a final technical report with everything that he has done. Greg felt that it was a good idea to get some more information on that.

Jim reported that they are planning on releasing on every field this year. Charles advised that they will not be releasing on every field in the Trans Pecos area. Ted Boratynski was concerned regarding the "check," in the competitive measure in the moths when they are competing against each other. Would you need fields where one field has DS Red and one field has APHIS? Greg felt that if we were not flooding the traps with too many, we are out of that issue at least as it relates to recapture. Different factors concerning recapture rates were discussed.

Dr. Staten requested clarification on which insects go on which fields on the map that was distributed. Kern County (5 sections totaling 945 acres) and Tulare will have both strains released on them. You would have three replicates in Kern County. There is probably a 6-10 mile buffer between the two of them. Charles Allen felt that you could replicate by field. Steve Naranjo felt that would give you the most separation. Jim advised that he geocodes in the traps and could go by traps if that is what is decided. Dr. Staten expressed concern that it would be a very large volume of data you would generate. Jim stated that If the group agrees that the only way to do this is if the CPHST lab do all of the insects, from an operational standpoint, his airplane is going to do the same thing every day, but it will put a real strain on the production capabilities. Greg advised there is a regulatory issue too.

Tom Miller did not feel that it would be a good idea to use Arizona as the replicate, and that everything should be replicated in the air. There should be a separate trial somewhere else instead of just looking at the competition between the two strains as a quality control step that should be done. Tom felt that the longevity study was worthwhile, and agreed with Dr. Staten that Phoenix would be a good area for it. Jim advised that he can perform the longevity tests in California. Greg Simmons advised that the permit is only for two counties. Jim advised that he can get twenty miles separation in Kern County. Tom Miller felt that it would increase competitiveness if they are too close together. Charles Allen felt that you would be able to fix that if you looked at the data a different way. Different ways of measuring and looking at the field data as well as minimizing the variability and paired comparisons were discussed by Charles, Jim, Tom, Steve and Dr. Staten.

Dr. Miller asked why do we care about the competitiveness between the APHIS and DS Red strains? Greg answered that we want to be able to provide information if we make a decision to switch strains that we are not buying into a strain that isn't as good as the strain we are using. Steve felt that Ted's idea of having these separate areas is a more meaningful measure of their performance than in an area where you are only going to be releasing one. Dr. Staten stated the problem would be that there would not be a sufficient number of natives to look at to get any kind of measurement, regardless of whether we work in AZ or CA. The problem exists to minimize variability between treatments (paired comparison). If you have separate field releases, we will not have the energy to do a lot of these. We cannot do a large number of replications in the San Joaquin and take care of their operational needs with the number needed to generate. I see no option but to put the same insects in the field at the same time. If you have them there, then all other variables but those two insects are gone. Steve felt that if you released 150 insects per acre, which is a very tiny number, maybe the interaction between the strains is not such an issue. Greg reported that he had a limited set of native performance data from mating stations and he saw that there was no difference between strains in the females in attracting either both native and/or the other strain. Dr. Staten felt that the biggest battle is the elimination of all of the other variables so that when you look at this data set you can say I looked at this against this in as equal as possible a footing.

Greg reiterated that we are trying to eliminate differences by doing everything the same as possible and primarily focusing on recapture rates. Dr. Staten agreed and felt it was the most reliable set of data to acquire over time as compared to mating stations where you

look at so few numbers. Larry agrees with Don Parker. Larry verified that as far as the number of moths put up, one would use an estimate of the total of moths, rather than weight, so you could have equivalent numbers of moths released rather than an equivalent number of grams. Larry advised that the danger is releasing one insect one day and the other insect another day is there will be variations in conditions regarding wind, heat, delays in flights, etc. The only solution to address that would be to release both insects at the same time; however that would lend itself to its own set of issues. However, with this experiment being conducted for such a long period of time, the variables will become less of an issue.

Upon inquiry, Jim answered that the traps are being monitored weekly. Jim stated that if we mix the strains, that would totally eliminate any variability concerning grower treatments or any other concerns, if they are released at the same time. You do make it more difficult to complete the mating study. If we could figure out a way to identify the strains in the cold room, then we can move ahead with that procedure. We already sort them by gender. Steve asked if that was the only limitation to mixing. Greg replied that when they mixed in the 2007 trial, there were fewer insects. To get the numbers right, it is very complicated. You know what the ratio is, and you use that as an index for trap capture, and that is a lot of more complicated. It does keep the release numbers the same. It does entail taking two quality control samples and other factors come into play. It is a lot more work for our staff to make that happen, but we can do that. The bigger issue with the bigger trial is mixing the insects actually into the release machine itself without shipper. You don't want to handle them too much.

Dr. Staten asked if Greg has considered extending the parameter of the test of “equalness“ to start with introducing equal numbers of pupae in each tray into the inclusion system. Greg felt that in doing the design, he came up with what would be easier to control and easier on his staff, but it is a good idea.

Ted suggested doing two flights in one day with each insect. It was subsequently determined that it would not necessarily be feasible due to the cost of the flights, and also it would still be subject to a different time and temperature of the day, and other variables that optimally should try and be avoided. Tom Miller felt that even a one-degree difference when releasing the moth may make a significant difference. Also, California not being able to ship additional boxes comes into play, as well as the bumping issue.

Ernie Miller advised that the empty weight of the old shipper is actually heavier. Jim advised that if they have passengers on the flight, the shipment gets bumped, however, it is a rare event. If we were to use two shippers, that would increase the probability of being bumped. Dr. Staten was concerned that the bigger issue is that California may not be granted the permit in time.

Upon inquiry, Jim reported that the wind is not measured, but the temperature is, and the information gathered is put into a heat unit model in order to make generation predictions. If our trapping drops suddenly, the information is also looked at.

Upon inquiry, Ernie advised that you can not partition a shipper, however he does have a prototype unit that has three separate magazines within the unit. Ernie offered that if we built two more of those shippers, that may solve the problem. Tom Miller was concerned that if you have a release of APHIS in one area and DS Red in another area, there are many factors that can come into play to effect the separate environments such as wind or temperature.

Jim stated that if we were to design the longevity test of the two different strains, we will select probably a four-mile separation between the two release points, as well as large block cotton, preferably the same grower, so that we can at least have a data set in those fields. Charles Allen asked for clarification. Jim reported that it would be two different locations and a one-time release situation. One strain we would apply one day and then the next day we would apply the other strain and then just trap them out. Tom Miller stated that even a one-degree difference would sometimes have a significant effect on the trap catch. Dr. Staten advised that would show only if there is a big difference between the two strains. It would not show small differences. Charles stated that it may be a good addition to what Greg already has. Dr. Staten commented that it would be relatively simple to complete. The test that Jim described would generate a lot of data. Then you have to go back to the questions of whether or not the data will be analyzable, utilizable and publishable. This is such a groundbreaking event, you would want this for the future insect pests of the world.

Jim discussed the APHIS reared moth, even though it's under a 26 million per day strain, as being the bench standard by which we are trying to compare this DS Red. One of the arguments Greg had was that he didn't want his moth to be considered a "pampered" moth because of the conditions that he is rearing it in. Greg saw Ernie's production technique. So has Cloti. In this large-scale operation where we could be releasing over 2,000 acres instead of 1,000 acres in a variety of climate conditions and growing conditions in the San Joaquin Valley, do you really think that's going to significant? Dr. Staten advised that he has a lot of experience with it as well, and offered to get opinions of people involved in the rearing including Ernie and Cloti.

Steve Naranjo asked Greg how much different his rearing system is compared to Ernie Millers. Greg advised that there are a lot of little things that go on, but the biggest issue is the management of a colony is slightly different because of the scale. We also have the issue of the same people. There are a lot of things that are difficult to control unless you use the same personnel with the same protocol. They are issues of scale and personnel. Steve Naranjo verified that it is quality control issues. Greg agreed. Cloti stated that there are not as many larvae being damaged, and there are differences. Ernie views their rearing facility as a "pampered" system, compared to his, even though a lot of the technology employed is the same as his, there is a difference. He notes differences in production between 2 million, 10 million and 26 million. There are some costs when you push them to the high levels. There is also a difference in the personnel. Ernie's rearing system makes more effort for our layers than for the rest of the colony. The density in the rearing containers is less; therefore the return from that rearing unit is a little bit less, resulting in fewer issues with pupation substrates.

Dr. Staten found that the size of the moth depended on what time of the day or what time the cut was being taken out of the enclosure would determine whether it was a large or small moth. There are many many nuances that determine so many different factors which are controlled by human decisions and it could give us variables that I do not want to see here. If we are going to do this, we need to control these variables to the maximum degree possible.

Steve felt that there would be potentially more replications versus fewer replications. How big of a difference is biologically significant, because you can manipulate the statistics based on replication to get whatever answer you want. You want your "confidence band" to be as large as possible. If you do two replications, you are almost certain you will find no difference. However, if you do ten replications, you more than likely might find something, even if it's small. The more replications, the more powerful the test. How big of a difference is biologically meaning in terms of comparing these two strains is the critical question. That will determine how many replications are necessary.

Greg reported that this test is more ambitious than their 2007 test. Greg stated that it comes down to cost and program effectiveness. Steve advised that you have to look at how big of a statistical difference is biologically meaningful, meaning that you could have an insect that is 5% different in whatever you use to compare it, however, if it gets the job done, then it may not matter. You have to determine what is meaningful.

Charles stated that the production itself is introducing a lot of differences between the two different rearing facilities, which is muddying the water already. You're not going to be cut real thin in terms of looking at small differences and come up with those differences because the rearing is different. Greg agreed. Charles stated that if we went to a DS Red moth, it would be subject to the mass-rearing rules. Greg agreed. Charles advised that you would have to write that in when presenting your data, and does not feel that we could deal with that statistically. You would be dealing with a messier data set.

Dr. Staten advised that the most important issue is that the DS Red is an APHIS moth with one construct introduced two times, so the only thing we really want to know is whether or not that introduction changes the insect. It may be there are some other subtle things, but the closer we can control all of the variables, the better off we are.

Members liked the idea of the partitioned shipping container so the release is the same every day in the field and it does reduce the variability. Ernie estimated the cost to be around $1,500. Ernie asked if you wanted to release six consecutive days. Dr. Staten advised that he can not release DS Red on his northern counties, so on the APHIS shipments, Jim is treating the DS Red blocks and continuing to treat the other blocks. Jim said he has to release on the map on those areas that aren't receiving DS Red. The only way he could do it without doing two flights a day, is every other day. Dr. Staten verified that a partition shipper would not help you. Greg advised that not on that issue, but it would help with the mixing issue only.

Greg is concerned about the amount of staff hours that would have to go into rearing more insects. The size is right and the labor fits. The other design that was proposed by John Klaus and then by Bob, is just mixing the pupae in the inclusion boxes and taking what you get. There's no handling. It's pre-mixed. The only issue you have is that the numbers aren't going to be exactly the same, but we can measure that. It just makes the analysis a little more complicated, but over time the inclusion rates will even out. We will have to record what actually went out, track it, index the trap recapture in our analysis, and work it through the data. It will make the analysis more difficult, but it can be done. It's simple because the handling is taken out of the equation and it's actually a nice design.

Upon inquiry, Ernie explained that the shippers are like having three suitcases in one box where each compartment is totally isolated, but when you pull the guillotine, they will fall in the hopper at the same time and be augured out at the same time. Charles mentioned that the only thing you would need to do is maybe some quality control to sample and make sure that the release ratio is equally distributed.

Dr. Staten verified Jim's calendar that was distributed concerning the alternating days and two-week time cycle. The good part of it is as you add finds, you will not be changing the release rates in one place by taking something from here to treat another find; you will just be getting more APHIS moths. Jim said that if he were to get a find in Kern county, he would want to add that find section to the DS Red APHIS strain test because he is dividing it, from an operational standpoint, into two separate routes. You have to factor the flight time route into the equation. Dr. Staten confirmed that the same thing will happen in this test where the bottom section [the one by itself] when you start release over last years

cotton, then by June or July you find out there's no cotton in that field, and then it will be dropped. So, there will be a real chance that you will loose a rep at that point. These are not this year's cotton at the start of the season. These are going to be released over last year's cotton. As time progresses, there will be shifts in this map. At some point in June or July, you will have stabilized this years release areas. Jim advised that is true only if there are no additional natives found outside these sections. Dr. Staten said that they will most likely still get releases the rest of the season. Jim agreed.

In the past, he had gone two weeks beyond the second field generation. He has not caught a native in a section under release at two weeks beyond the second field generation. Jim has the option of dropping that section from release. About that time, around mid-August, Jim is picking up natives in other areas and we want to take the moths and put them in the other areas. Because of the decline in acreage that California has experienced over the last ten years, that hasn't been as critical in that we are looking at release rates. We have gotten stretched a little thin with new natives coming on and that is why we have dropped sections where we have not picked up anything. We are looking at 100,000 acre reduction between 2008 and 2009. Jim estimated 945 acres in Kern county, but it will probably be forty-percent less than that number. It is possible that one of the four sections you see in Tulare county may not have cotton.

Upon inquiry, Jim advised that the traps are located in the same spot for the whole season, on the edge of the cotton field. Trap placement does not change through out the season. Dr. Staten noted that the allocation of conventional APHIS moths to the San Joaquin is way over what he is asking for. In other words, you are asking for around a million moths a day, and they are not all APHIS reared. This is requiring less, so if he has finds, certainly away from this release area, it will be easier for him to make a decision of using the APHIS shipments to do that. That gives Jim some cushion.

Jim estimated that he should have a finalized map of the Kern county map by June 1st, and if there's no cotton in there, he will drop. June 1st is way beyond over-wintering emergence, and there would be no need to be releasing on that after that peak of over-wintering if there's no cotton on there. Jim advised that he will concur with the group.

Steve asked if we try and find a way around the one release a week versus two the following week. Greg noted that if we do any other design other than this design, we can not send two shippers a day. This is the only design that keeps the strains separate when they arrive at the CDFA lab. If it's important to get the quality control data from the CDFA lab, the only way we can accomplish that is to have them separated. Jim inquired on Ernie's partition box. Greg concurred. Dr. Staten advised that this group is not anticipated to make management decisions. If the people who run the test see that they have a better way, they will do what they have to do to manage this test. Dr. Staten wants to be able to tell the Pink Bollworm Action Committee that the test that is going to be run was vetted as well as we possibly could and be able to say that this is probably the best test we can run. There has been a lot of intense planning detail and thought put into this.

Dr. Staten had hoped that we had a mixed shipment over the entire San Joaquin of a million moths per day on every one of those blocks, and that would be ideal and the most simple test. We have seen a lot for the Committee to think about. We also need to evaluate the Arizona test. The Committee broke for lunch.

Don Parker asked that the Committee advise all members concerning the recent job changes that have occurred. Charles Allen has taken another position. Larry Smith will be taking his place (who was also in attendance). Greg Simmons accepted a four-year TDY on LBAM position in California. Cloti Tate will fill the gap.

Dr. Staten advised that we will now review the competitiveness studies in Arizona within the environment within Arizona. Cloti Tate and Leighton Liesner will discuss the proposal. Their thinking has been outside the box and is very original. Should we do both? Should we operate only in Arizona? Dr. Staten yielded the floor.

Leighton distributed a hand out and advised that he and Cloti have made some modifications to their proposal based on some concerns that were raised earlier this morning, and he will incorporate those modifications during his discussion with the group. A couple of questions that motivated these designs in regards to DS Red are what is the point to releasing sterile moths? It is to mitigate an in-field population or prevent a population from moving in. With an over-flooding type situation, you have a smaller native population, and you are releasing sterile moths to do that. This brought forth the idea of using ratios.

One of the moths performing as "established population", and in over-flooding moths, as sterile released insects like the program would do. The second question is in regards to competitiveness. We have seen in program data vast differences in captures in light situations when the numbers of moths are decreased per acre per day. That is the rational behind the structure of our study.

We originally planned for eleven treatments, four replications and forty-four fields (10-20 acres on each field). We discussed doing alternate releases; DS Red one day and APHIS the next day. We also discussed doing single native releases (one flight APHIS and one flight DS Red) on the same day. Several methods were discussed.

We submitted our study to Bob and the statistician reviewed it and advised the best method of approach is what Bob expressed earlier. Cloti and I further refined the study in asking how we can simplify it and get more replications out of it. With some guidance from Ernie and Bob and Larry, we have decided that the best way to achieve our goal would be to have three treatments at 100 moths/acre/day, 250 moths/acre/day and 600 moths/acre/day. Those would be half DS Red and half APHIS moths. Under those conditions, we would be able to do six replications, eighteen fields, and would consume 1.5 million DS Red moths.

In consideration of targeting on a per field basis, we have solved an anticipated problem in the time lost when the pilot flips the switch to release insects, compared to when they are actually coming out of the plane. We have created a buffer for the pilot, so that the pilot will be in full-release mode when he goes over the field.

Cloti feels that it is a good design to show a side-by-side comparison behavior of the two strains as well as providing the capture rate data and longevity. Additionally, it would be convenient as we would conduct it in Maricopa county. Leighton advised that under this structure, the moths would be packaged together under one shipper. Releases would be done in three days. Bob verified that it would be conducted on a group of Bt fields where there is very little activity. Leighton concurred.

Bob inquired whether it would be appropriate for the program to avoid that area and consider these the refugia moths for that area. Leighton concurred and further advised there would be four weeks or releases, and trapping two weeks beyond that. Dr. Staten said that assuming we do not get permit in California, if you had the "full tilt" production, could you do this over a month period of time and go through and see what your data is doing, and have the ability to modify it quickly if you wanted to. You could get a full set of data and evaluate and turn it into a three-tiered test? Leighton advised that it was possible.

Upon inquiry, Leighton explained that he was using the 600 moth release rates because of the limitations of the machine. At 600, which would not be too far off from what we have done in the past with extra moths trying to mitigate a population, the pilot would be making four passes over the field just to satisfy this. Bob clarified that three treatments would be run six times for a total of eighteen fields. Leighton concurred. Ted clarified that eighteen fields would be the total. Dr. Staten commented that it is far more a conventional replicated laboratory trial rather than trying to run in an operational mode.

Upon inquiry, Cloti advised that that they would be able to do mating propensity 48-hours after sorting them by gender. Jim explained how they currently sort them by gender in California. Leighton explained he can supply the sterile release plane and get the traps in from the field and CPHST can go through the traps weekly.

Steve asked what the hypothesis is. Do you expect that when you get higher densities, you are going to get radical changes in behavior of these two strains? Leighton advised that he has seen that indicated in program data in New Mexico and Arizona. In 2007, we had massive number of moths moving into the area during the latter part of the season. The pheromone treatments had run out and his approach was just to smother the fields with sterile insects. Not only did we not have any populations we could find through extensive boll cutting, but we also had very low trap numbers of steriles.

Cloti added that we are looking to see if we can get traps shut down earlier with the higher densities of moths. Leighton stated that would put pressure on the insect. Dr. Staten clarified that one of Leighton and Cloti's hypothesis is that one of these may show up different from the other because it is a high number rather than low numbers. Leighton advised that it might accentuate a difference between the moths.

Jim asked if you would know whether or not it was females out calling from one strain versus the other or males being more responsive in one strain. Dr. Staten stated that you would not know. Cloti offered to put out mating cages and actually go out and look. We can't really get a sense of what the females actual do. Cloti offered to look at it to see which males are responding to females and things of that nature.

Don Parker verified that the different treatment rates are trying to see when you shut down the traps. The purpose of the DS Red would be to make sure you are getting similar performance behavior between the two strains, so that you know what you expect to catch. Steve inquired, and Cloti confirmed that they would be rearing these themselves. Dr. Staten noted that the ultimate question would be where would we put the priority if we had the choice to run both? We don't have the choice to run both? That is the idea.

Don inquired on the trap density. Leighton advised that it was up for discussion. Dr. Staten advised normally a non-Bt field would be trapped at one trap per ten acres and one could assume that would be the minimum trap density. Leighton concurred. Leighton advised that he uses fifteen for the purposes of discussion because in the original document he assumed 10-20 acres.

Leighton advised that originally he was hoping to do this in Pinal county where he had ideal cotton fields spread out around the Coolidge Airport. However, the historical permit in Maricopa County could probably be more easily renewed. Upon inquiry, Leighton advised that there is a minimum half-mile distance between fields. Dr. Staten expressed concern over crossover. You may be able to fly it at a lower elevation, and with multiple passes, that may help to address that issue. Let's focus on what we would get from this statistically.

Don advised that you may not have a cluster of traps at that one location. If you had a field that had adequate isolation from your adjacent fields, you may be able to run a trap line across there to see dispersal out at those different densities, and you may also be able to get some longevity information. Leighton advised there would three releases per week for four weeks for a total of twelve completely mixed releases.

Don inquired if it was documented enough on the movement of insects from the release site. Dr. Staten reported that we did use data that was presented to EPA. The vast majority of movement from the San Joaquin is within the first mile, not very far. But there is movement within three miles that can be measured with reliability. Jim reported that he ran a trial where he released in a quarter-mile and then trapped up from there. Sixty-three percent of the released insects were trapped in the quarter-mile. Approximately twenty-two were trapped within the next adjoining square mile. Approximately twelve to fifteen percent were trapped in the third mile out or beyond. Dr. Staten verified that the airplane was at 500 feet, and that if the airplane released at a lower elevation, it may be quite a bit less. Jim Rudig verified that it was 500 feet, and it was released over non-cotton.

Charles Allen verified his understanding concerning the number of reps, fields, mixing of moths, etc. and inquired how you would design it on a map. Cloti answered that in general he would try and block it, but he wasn't sure how the fields were set up in Maricopa county. Charles asked if you would try and put three fields; one with 100, 250 and 600 in each block. Cloti advised that they would try and select three fields with similar conditions. Charles verified that for the three fields in one replication, and you may also call that a block, to be similar, but maybe somewhat dissimilar and maybe somewhat geographically isolated from the three fields in the next block. Cloti agreed. Steve stated that you would want sufficient independence even between the fields within a rep, but you want something like an equivalent planting date, or an equivalent variety, within a rep. The goal would be to eliminate as much variation as you can. Charles felt that you could completely minimize it, but the danger in that where you may have several treatments under fairly similar conditions. Charles would rather see it blocked.

Dr. Staten asked Cloti and Leighton to point out on the map where their likely location would be. Leighton showed where you would be splitting Maricopa and showed the possibility of locations. Leighton advised that one of the areas mentioned last year as well as this year is 100% Bt. Cloti reiterated that they will try and get fields that are as similar as possible, such as the same grower in order to have the same cultural practices.

Charles asked that with the exception of the high release rate, what do you get from this data that you don't get from the previous one. Cloti verified that you would get the same data. Charles stated that as far as resources and personnel are decisions that Cloti and Leighton would have to make, as well as the locality. Charles felt that you may get to a point where you can do one, and not both. Dr. Staten suspects that may be the case. Dr. Staten feels that it's absolutely critical that we have some options. There is a real good chance that California may not get the permit. Even if this is choice number two, it is very wise to keep it up and running as much as possible.

Greg advised that we have a choice number two. Greg advised that the same experiment we have for Jim with the permit submitted, we talked about having an aircraft. His intention was to repeat the 2007 experiment on a larger scale. Dr. Staten requested that we finish discussing Arizona before the discussion moves on to something else.

Leighton stated that if it is going to be done in Arizona, he would like to see it done prior to the anomalies we see in the fall with trapping. Charles noted some of the similarities of the two studies in Arizona and California.

Greg added that a permit has been applied for with a design that would encompass either experiment. An Arizona permit has been submitted in Cloti's name that was written broadly. He pointed it towards Laveen based on Cloti's recommendation.

Don noted that one of the big differences between the two is that it would test political waters in California. Dr. Staten advised that the replication and where you have it and how it is set up, you can make some choices as to where your plots located. That is probably the biggest scientific difference. Steve agreed as potentially a better resolution of the data.

Charles advised that the California test has more of a bang in it from the standpoint of testing the moth in an operational program and the political social aspects of getting a transgenic moth into California. The Arizona test has more bang in terms of the resolution being a little bit better. If you have to choose one over the other, those would be the components.

Ted asked if there would program releases to satisfy EPA over other fields in the area. Dr. Staten reported that there will be program releases that will satisfy EPA. Under these circumstances, you can probably keep the drop plane at a reasonable distance and this is all Bt. USDA recommends 10 per acre per day. USDA is recommending that we avoid these areas for a certain amount of time. We can probably avoid these areas for a certain period of time under intensive monitoring. We can keep test integrity from other releases here by keeping them in a strict Bt area where we have no hazard, and monitoring to make sure there are no natives found during this time period that would cause us concern from our resistance perspective.

Ted asked how handle the data for grower applications if you have one rep that gets hit and another that doesn't. The grower could use an application of insecticide on one field and not the other. Dr. Staten reported that insecticide use in Arizona has now fallen far below the San Joaquin. Leighton suggested the test be conducted at or just after peak first bloom and the first week following in order to try and eliminate some of the behavioral things we see in the Spring and in the Fall. Upon further inquiry, Leighton reported it is around July 1st. Larry said that it could be late June, early July.

Tom expressed concern over proximity effects, distance between the fields and temperature differences. Leighton reported that one of the options he has available is that program-wise he has one trap per forty acres on all of the Bt cotton. He could pull from some of the program traps to glean some of the information.

Leighton was asked what he thinks the planting will look like in 2009? Will you have a good enough grid to do what you need to do with the program data? Larry advised that it should be similar. He has heard from his growers that it should be about the same acreage, maybe a little more, but that's optimistic. Leighton added that he does not anticipate any non-Bt cotton in that area.

When asked how the day would go, Leighton replied that ideally, we would get the moths packaged in the shipper, and get them to the airport. We would commence release as early as possible on the test fields. Charles asked about the release rates. Leighton specified that the higher rate is the one that has a problem with the number of passes. The intermediate rate would be two passes and the lower rate would be one pass. Dr. Staten verified that the control setting on the plane is the same. Upon inquiry, Leighton advised that they will have to flip the switch on the plane as well. He will have to calibrate the release rates. Even at the intermediate rate, you start running into output problems.

Charles verified that the number may not come out exactly as you have them, but those are the target numbers. The release rate, whatever it is, will be the same each week. Leighton reported that it will be the same machine, same auger, same pilot, etc. Leighton advised that they will have distinguished calibrated settings on the machine, and there are four switch settings. It was clarified that Leighton will fly three days a week. Leighton advised he uses standard measures, and if it's spread out over a distance, one release will start out at one end, and then the next one will be reversed.

Dr. Staten offered that if you are looking to run this at first bloom, you may want to run your high rate at a low elevation of flight in order to see just how much movement we will get before you pick your fields and the distances between them. You can do that with standard APHIS moths. You can set up some very intensive trap grids, and would definitely be worthwhile.

Charles asked if there was a difference in wind and speed during early season in Arizona compared to the time Bob is talking about running the test. Dr. Staten reported that it is one of the lowest wind environments in the country (Maricopa County). Leighton reported that when it is windy, it usually occurs later in the day, and the flights are in the morning. Dr. Staten reported that the biggest battle Arizona has is the heat. Larry advised that their goal is to get the planes out as early as possible. Larry stated that if the test is run in Arizona, it is unfortunate that the permit is not available for Pinal county, as there are ideal blocks of fields in that location.

Greg discussed Oxitec's riddle strain. Dr. Staten advised that we will try and cover other things if we have time, and urged that we stick to the agenda. Dr. Staten welcomed Bob Hull and went over a very brief overview of what the Committee was currently discussing. The goal is for the Technical Committee to vet both proposed trials and advise the Action Committee that we have reviewed these projects scientifically. Upon inquiry, Don advised that we are submitting scientific discussion to the Technical Committee. The TAC can report to the AC that we reviewed the research proposals and offered scientific comment to the researchers for their consideration.

Jim Rudig went over 1x (125), 2x (250) and 4x (500) and advised that if throwing a switch is not an option, doing it in flight would simply it. Dr. Staten then asked if there were any comments by non-Committee individuals.

Dr. Miller advised that in looking at the design, the field size is too small. He felt that in releasing by aircraft, there is a delay in response by the pilot concerning the release activating in the machine which runs about a quarter-mile. There isn't much room for error. Dr. Staten advised that he is planning on leading the field by as much as a field length in order to accommodate that delay. Leighton advised that there are differences from machine to machine as well, in how fast the gate opens and he will measure that and factor it in. Jim agreed with Ernie and stated you are going one mile in thirty seconds, and it's very difficult to target a field. Leighton advised that he would buffer it out on the pilot's AgNav screen. Dr. Miller asked for clarification in the reason for using three densities. Dr. Staten advised that you may get a feel for some interaction that no one has ever expected.

Steve Naranjo stated that a bigger test is always going to be more powerful, and that it would be a contingency analysis, or a ratio of the two strains could be the response variable. Dr. Staten confirmed that it would be a relatively simple statistic, but it would be powerful. Steve concurred. Ted asked how you would know if what you are seeing is real. Dr. Staten advised that is the crux of the entire discussion.

Charles stated that he would like to see a design like that. If the reason for the Arizona study is being a contingency if the California study does not end up materializing, then it might be a good idea to have two designs for Arizona. One should be similar to the California. Greg asked for clarification asking if it not the same thing, and does not see how it increases replications unless you use different lots of moths and separate releases. He felt that we really need to define the experimental unit. Greg advised that if you marked out fifteen fields and said these are our replicates, we can do the same thing in San Joaquin. Dr. Staten advised that you have already done that in the fact that you have three blocks in the San Joaquin that are basically three contiguous data sets and you have eighteen here. Greg advised that the only difference he notes is in the ratios. Unless we experimentally define a block and say we can actually treat this block different, it can not be a different replicate. Greg would argue it's the same moths, same blocks and same flight. Your replicate is actually the release itself and does not see it as increased replication.

Don advised that the difference in what Greg was doing was that he was releasing over that section and they are talking about an isolated field. Greg argued that the tests are the same. Leighton advised that he sees different trends in the data at different rates. Charles advised that the release rate is really not the objective. The goal is whether or not DS Red is responding differently from APHIS. When reviewing the designs, we don't have to really get into things that do not pertain to the objective. The release rate is interesting, but we want to see if the two moths are similar in their response to the pheromone trap. Leighton advised that the response changes as the density of moths changes. Charles felt that it may be addressed in both Arizona and California tests. However, if you are able to run two studies, run one study that looks at the density, and the other test that gives you better resolution. Dr. Staten advised that since you are doing it over a six week period, you may be afforded the luxury of being able to change experimental type as your data comes in. That would be one advantage of working in Arizona.

Greg advised that the number of moths they are asking for, he feels he could supply. On a smaller scale, he could probably handle it. It would be more of a question of staff and resources, but moth-wise, he could do it. He would like to try to include some of the other questions including getting some longevity data out it. That could be the benefit of running both of the studies. You are only talking about 189,000 moths per week, which are the moths that would be tossed on the off-day, so he would have enough moths. Leighton advised that he has not had enough time to re-write the proposal and that it is going to change again if we follow Jim's suggestion, which Leighton feels he should. It won't effect any of the things we are trying to look at if we go 100-200-400. Leighton was calculating about 250,000 moths per week per treatment. Greg advised that if it were a California and Arizona operational trial, we have to delivery California first, but he feels that there would be enough moths to do a smaller trial in Arizona with this number of acres.

Michelle Walters stated that sometimes they are releasing a lot of moths and we don't catch them as we want to in order to do the ratios, but she doesn't know why it is or how to measure it. She reported that they are not responding to the trap, but that doesn't mean that they are even in the field, or that they are shut down by too many females. Wild moths don't do that. Why do we think our sterile moths do that? Michelle stated that if you do the study, you are dealing with reared moths. It would tell you something about how reared moths behave, especially using a lot of replications. As opposed to the Fall, Summer would be a good time to perform that test.

Upon inquiry, Dr. Staten reported that he has not seen data that shows that you are getting trap shut down by excessive number. You get lower and lower percentages of recover, but has never seen it "beat" the numbers in the field. Leighton advised that you are trying to compare a released moth to a wild moth. He is doing something with the released moths

that you would never see in a wild population anyway with the over-flooding. Michelle stated that in Buckeye during the middle of the day she had seen pink bollworm flying around, which means that they do not have any place to land. Charles agreed. Dr. Staten concurred.

Don Parker reported that there is some data showing pheromone interference with trapping. There is biological data supporting that hypothesis. Dr. Staten reported that the pheromone reception in the sterile is not the same as the native. Michelle confirmed that the sterile is more easily confused. Dr. Staten agreed.

Bobby Hull expressed concerning that we are comparing one false moth to another false moth, when really we want to compare to real moths. All of the historical data we have is comparing native moths to the APHIS moth, and so now we are studying the relationship of the dyed moth to the APHIS moth, and in a real population sense, we really want to compared the native moth with the dyed moth. Dr. Staten advised that right now we can't. The more important issue is did we do anything at all by inserting that gene in two locations to the APHIS strain, because they are actually the same moth, with the exception of the existence of two constructs on two separate chromosomes.

The question is whether or not we are we going to change the rearing facility to the new moth. Dr. Staten reported that he does not know of anywhere the test could be run where we would have enough native insects moth left to get a comparison against native insects. There are a few places in Texas where you may find low-level populations. In Mexico, the logistics would knock out any possibility of this trial, and it would be a huge challenge to try and get the test run there instead of running the test in the United States. The Mexicans generally do not want any test run in Mexico that you are not running, or had not run, in the United States. Ted said that the primary reason for running the test would be because there is not enough of a population here to effectively run the test. Dr. Staten felt that you would still run into those stumbling blocks, and also with the logistics, lab support, etc. Dr. Staten speculated that if the first permit would have gone through when it was supposed to, and each subsequent permit had gone through, this insect would have been in that rearing facility ten years ago, or at least five years ago. The permitting has been the biggest obstacle in getting this technology put in place.

Dr. Staten will go through all of the minutes after Tish has completed them. Those of you who are designing experiments and those of you in management have as much benefit as possible out of this.

Tom Miller inquired whether or not Dr. Staten wanted him to take a copy of the description of the intended experiments and it past the editor of the Journal of Economic Entomology. They are pretty quick about turning things around. Don Parker expressed concern regarding our function, which is just to provide input, and feels that should be the decision of the researchers themselves. That would be great to offer them, and may be very helpful to them, if they agree. We do not want to dictate their research. Dr. Miller said that if the researchers are interested, they can get in touch with him. Greg asked if he could list him as a suggested reviewer of the 2007 work. Dr. Miller said that if it is the same thing, that would be fine.

Dr. Staten advised that he is going to table the program release issues for the moment, and asked Cloti to provide a quick summary of other marker work. Cloti advised that he is currently looking for a supplier for strontium and rubidium in order to perform testing on their diet. If he is able to obtain it at a reasonable price, his goal is to make diet with rubidium in it and to perform testing in field cages. Currently, he has looked at the literature and has some concentrations for rubidium that he may use. Upon inquiry, Cloti reported that he

found literature on rubidium that mentions strontium. Charles asked what the objective is in using the markers. Cloti reported that it gives you an additional marker besides the Calico red dye. It would be done in the APHIS lab. Dr. Staten advised that once incorporated in the body of the insect, he would guess that it would have less chance of being lost over time. Dr. Miller concurred. Charles inquired on the cost. Cloti advised that the price for rubidium is $150 for 100 grams, although that is pure. He tried to do the stable isotopes with Carbon 13 and Carbon 12, with mixed results. Cloti suspects that may have been due to the fact that it was not pure sugar cane, but rather some sugar bead beat sugar. Ernie advised that do now have sugar cane. There is no label that identifies it as cane sugar unless you specifically order cane sugar and this year we are 100% cane sugar. Dr. Staten urged Cloti to summarize all of the literature he can find on both rubidium, and everything you can find on strontium. Rubidium is even used as a trace element to measure competitiveness of sterile moths. The data was very compelling that it had no effect on the competitiveness of the moth. If we can find a way to use rubidium, even if it's only used for the last three weeks of production, the time when the issue over marking seems to be most acute is at the end of the season, when you've pulled release, the temperature has fallen, the moth is living a long time. That seems to be when we get dye depletion by age, if that is what has happened to us in the El Paso, TX area and even in the San Joaquin. We shouldn't just look at this as a season-long tool. If the costs are high, we should focus on the end of the season. Dr. Staten urged Cloti to continue working on it. Dr. Staten advised there was a lot of information on strontium in the lab at one point in the group that was looking at atomic absorbtion spectroscaphy to measure trace elements in different populations across the board. Their resolution was at five decimal points passed part per million in strontium. It was incredibly sensitive, so the use rates would have been incredibly low. Don Parker offered to get together with Cloti after the meeting in order to provide him with contact information for a researcher that has recently used it in ovaposition studies.

Dr. Staten moved the discussion toward the requested change in definition of eradication, and distributed a hand out. He also stated that we need to discuss minimum standards for trapping like New Mexico, Texas and eastern Arizona for next year after they declare eradication. We would also like to talk about response and will need to get documents together. We will need that information when we meet with EPA in order to get a natural refuge or refugia relief for the west without pink bollworm. The differences in the labels in Bt between the south and the west are all based on the west being generally infested with Pink Bollworm. Steve Naranjo verified that it is Bollgard II. Dr. Staten agreed. Steve stated that all of them will be stacked genes. Dr. Staten said that there will be different cottons, but they will all be combination toxins. The question to EPA will be are we sufficiently in make up and so on, to be able to use the same label for Heliothis. That is the argument. A group of us have been working with Monsanto and plotted a strategy to get it into review for output. However, it does still require definition and action plans. Don reported that he will probably talk to the EPA about it two-gene products, because the Dow product is actually still under consideration using the data set that was submitted for the Monsanto project. So, they are already considering other projects based on a similar set. We would ask that the treatment be similar. Steve asked why anyone would want to grow Bt cotton after the pink bollworm is eradication. Dr. Staten said that it seems to have a yield advantage no matter what. The other issue is how much seed they have. The way the label is written now, if we were to get an infestation, you could not release steriles over five-percent refugia. They are explicitly exempt, and you would not be able to pheromone treat. You would not be able to do a lot of things with that five-percent refugia. Then you have a grower community that's looking at twenty-percent, which would really affect farming practices across the board. It just makes sense that if there is no pink bollworm, there is not a pink bollworm resistance issue. Let's not use that to drive labels.

Dr. Staten reported that there was a definition of “eradication” put forth at the last Board meeting. Larry distributed a hand out reflecting the exact wording that the Arizona Cotton Research & Protection Council proposed. The top paragraph went through the Action Committee at their last meeting. When it came back to Arizona, the experiences of the Arizona growers, particularly with karnal bunt, makes them very sensitive on how eradication is defined, and how regulations might have unintended consequences to them and their livelihood. Dr. Staten advised that it is an emotional issue. They have forwarded to the Action Committee a request for changing the wording of the first paragraph. It will certainly be a more fluid term at the ground level. The question is will it satisfy the people who are promoting changes in labels and others. It is strange to promote a position leaning towards eliminating regulation, not asking for regulation.

Larry reported that the Arizona Cotton Growers Association has a Pink Bollworm Committee among it’s members. It is apart from the Council. Rick Lavis is their key lobbyist, which lends itself to thinking about law and statute. Basically, they have two approaches. They do not like the idea of zero tolerance. Zeros are difficult to deal with.

More importantly to that group is by statute, Arizona set itself up to start the clock when a region declares that it’s entering eradication. The region has four years to complete eradication by law. That was the key component on the referendum. The growers were concerned about a program going on for an indefinite period of time. We were not able to pass the PBWEP in the past, but by incorporating the four-year time frame, we were able to pass the referendum by eighty-percent. As a result, by the end of 2009, central and eastern Arizona, statutorily, will have completed eradication. If they don’t and there’s substantial activities that involve grower funding, then there would have to be another referendum.

Larry reported that Arizona has an annual bale assessment up to $5/bale, in central Arizona and up to $2/bale in eastern Arizona, in order to run the programs, which includes monitoring, field activities, supervision and management of the program. The actual cost of treating non-Bt cotton i.e. pheromone and/or chemical treatment, everything non-sterile, is covered by another assessment that requires growers who plant non-Bt have to register it and pay $32/acre to the Council to cover the cost. At the end of the forth year, the $32/acre funding source ends. The bale assessment does not.

From a practical standpoint as a program manager, Larry believes that we will be completed by the end of this year. In other words, in 2010, Larry does not expect to have to put any pheromone or insecticide on any non-Bt cotton in central or eastern Arizona. If we can not tell our growers that we have completed eradication in Arizona within the four-year time frame we agreed to, it creates an issue of a loss of trust and confidence. If it turns out that eradication depended on any straggler moth that came in for this year, or next year, or the year after, it may be ten years before we could say that we are eradicated.

Bob Hull stated that at the end of the day we have to loose our mindset and realize that there will be a few moths from time to time. We have to set up a system of cooperation within government and science that will take care of these stragglers moths and also any outbreaks that may occur. We need a funding source to take care of those issues. Certainly, the Arizona growers should be free to plant whatever they want once we achieve functional eradication. We will need sufficient funding to continue trapping, the continuance of scientific advancement, as well as the ability to solve outbreaks that may very well occur. People are going to be upset, but it will pass. We should not be in a hurry.

Dr. Miller advised that the greatest danger in the aftermath of a successful pink bollworm eradication program is complacency. If you are dealing in the post-eradication era, all fields become monitoring sites, no matter what kind. Dr. Miller likes the definition, and stated he

understands taking into account the “fly-ins”. If you switch the complacency of everyone involved to realize that any field in the pink bollworm free area now becomes a huge magnet, it becomes a sensor or detector for fly-ins. It is human nature to be complacent when the problem is gone.

Charles deferred to Mr. Smith (his replacement). Mr. Smith felt that you must have flexibility, and liked the definition.

Don liked the definition as well. If you find something at the level that we had defined it before, you can run into a lot of problems. The zero detection of carcinogen functionally worked at that time, but then over time, the ability to detect something increased so dramatically, that it ended up locking out everything. You can paint yourself into a corner by passing something too stringent. It can defeat the purpose you are trying to achieve.

Steve advised that the insect will never be truly eradicated from North America. It is a cosmopolitan insect. It is in Mexico. It’s still going to be in Mexico in 2009, when Safford and Maricopa may decide to declare eradication. There is no such thing as a zero detectability level. There will always probably be some level it will be detected if we monitor it for the next ten years. By declaring it eradication, you do in some sense create a false sense of security. We can’t be complacent. The monitoring still needs to happen at some level, regardless of which statement you use. The point is you can’t be complacent. Politically, you must have a definition of when you are going to quit the program, in a full-fledge way. The second and third item covers that.

Jim also likes the first paragraph because it is consistent with Fruit Fly eradication definitions. They even came up with a number of males in a geographic area, and simply because you catch one or two males does not mean that you have a problem.

Dr. Staten advised that the weak spot in item number one could be attacked when we go for the 24c because we haven’t defined what it means to not have evidence of a breeding population. Hopefully, we can defer that if we have very strong, well-written contingency plans for what we do when we find even a single moth in an area. Then our argument will be that they really don’t care how we define eradication. You can’t respond to a resistant population as well as we will respond to even a single find. That should eliminate those concerns. The rest of the argument will be amongst those who want to write and publish papers. Dr. Staten is willing to accept this, and the test will be when we go to EPA.

Don stated that EPA and the natural refuge is not as important as upsetting the growers over this definition. Dr. Staten concurred. Don advised that we would be better off not to have natural refuge because of the disagreement with EPA, than to have growers upset and filing a lawsuit because of a definition. Larry reported that natural refuge is extremely important to Arizona growers. That will be the only stacked gene they will have available. It will be pretty easily addressed at the grower level if you incorporate all of the good comments made here. You could use this definition, and if we don’t find anything in central and eastern Arizona by the end of the season, you could call it eradication. The next step would be confirmation of eradication which would take at least one to two years where the same level of monitoring. The Arizona growers are not opposed to high levels of monitoring. We declared eradication of boll weevil in 1991, and have done a lot of boll weevil trapping since then without any complaint at all. They are used to that and do not want re-introductions.

Don asked that if that definition then did not change, because we wanted to satisfy something with EPA, we are stuck with the original definition and that definition then met some straggler moth came through and prevented you from having eradicated an area

within your four year period of time, would you then have a grower problem? Larry stated that if it weren't properly explained, we could. Don reiterated that the scenario could be a little more serious rather than dealing with EPA over that. Larry reported that he felt the growers could be dealt with, and liked the broader definition, which allows some flexibility, knowing that our plan is to be as stringent as possible. Larry felt that the contingency / remedial action plan will probably matter more to EPA. If you have the ability to react and have a plan in place that will allow you to respond should anything come up would satisfy them and felt we could have the best of both worlds. Don concurred.

Greg discussed continued support from USDA for sterile insect budget. As it relates to the definition of reproducing population, we may want to do some research on that issue, so that we do keep tools and budget available to us for the post-eradication phase. That would help our side to keep supporting the rest of the eradication and the post-eradication. What is the evidence of reproducing applications? Some of this is researchable, and that we could maybe help in addressing it specifically to the program.

M/S/P **Don Parker moved to accept the amended definition (item #1) of eradication as proposed by the Arizona Cotton Research & Protection Council. Jim Rudig seconded the motion and the motion passed unopposed.**

[Polled: Bob Hull, Jim Rudig, Steve Naranjo, Don Parker, Charles Allen / Larry Smith, Tom Miller, Ted Boratynski, Bob Staten, Larry Antilla]

Jim Rudig reported that in the San Joaquin they have developed some criteria of what is evidence of a reproducing population. If they catch a lot of males, they will go in and do a boll survey. Obviously if we get infested bolls, we surmise that we have a reproducing population. We also do the heat unit model, so that if we catch one moth on the 15th of July and on the 15th of August we catch three more moths in that field, we know we may have a reproducing population. It is something that can be defined. We have picked up four or five moths in a trap over a few day period and never catch anything else in that field for the rest of the season.

Larry reported that it is similar to boll weevil in post-eradication development. A single weevil does not trigger an overwhelming response, but it does trigger a series of actions. It is only two or more that result in escalating scenarios. A remedial action plan can be included and is not vague. If you go out and cut a few bolls and don't find anything, you surmise it is not reproducing.

Steve inquired about the eradication definition for boll weevil. Larry answered that basically it was the inability to find any reproducing populations. In 1991, we did not catch any weevils at all. In 1994, we caught one near a railroad track out of 2,500 traps per year.

Jim reported that the last weevil he caught was in 1990 and declared it eradicated in 1993. Dr. Staten reported that there is no consistency in the definition of eradication for boll weevil from state to state. Some states did not have declaration criteria.

Dr. Staten moved the discussion to step down monitoring guidelines and emergency response plan. Item #3 refers to monitoring guidelines, which we should define. Charles stated that you can scientifically get a handle on it. As long as we understand that, and consider the risk, that should be the objective.

Dr. Staten reported that they had an elaborate plan which they put before the group during the National Cotton Council meeting. We will have to talk about traps per acre. San

Joaquin is a somewhat unique environment and that with one exception, there has never been a general infestation of larvae there that could be measured as a larval infestation. We should leave the San Joaquin out of this discussion because they already have their standards and their guidelines and they work for them. Dr. Staten would like the guidelines that we are defining to be for those areas as defined by the maps in Phase I, II and III (the Pink Bollworm eradication maps south of the San Joaquin). In other words, we should address the areas where we have eliminated ongoing infestations.

Don asked if there are sufficient differences in the San Joaquin substantial enough to warrant exclusion. Dr. Staten reported that we should use the knowledge we have gained there, but there are several things about the San Joaquin that are cause for concern when considering minimum standards for the rest of the area. Their field size and topography is different. In San Joaquin, you have some areas that rotate fields by the square mile. They would consider a forty-acre field a small field. The average field size in El Paso is approximately twenty acres. There are all of these very wide diverse separated areas.

Dr. Staten feels most comfortable leaving the minimum standards in place for two years after eradication as far as trap density. They were good enough to get us here. Ted verified that there is one trap per ten acres in conventional and one trap per forty acres in non-Bt cotton in Mexico. Dr. Staten felt that every field is a magnet, but the consequences of a positive hit in Bt are not as grave as if you miss one in a conventional field.

Don stated that it is not a good idea to separate San Joaquin when defining a minimum standard. You will always have to explain why that was done and it causes confusion. Why would you keep the same trap densities after you have declared eradication?

A member stated that Dr. Staten reported earlier that San Joaquin never had any reproducing pink bollworm, so why don't you consider them never infested. Jim reported that he collected bolls in 2008 and had adult moths emerge out of those bolls. We did boll surveys in 2002 or 2003, and found larvae in bolls by cracking bolls.

Charles felt that you could work some of this through definitions. You could use the words "not generally" or "not widely" infested or something along those lines in order to make a distinction in San Joaquin. Charles agrees with Don in not drawing distinctions between the areas.

Jim advised that he disagreed in that if he has gone through it and trapped it 1/40 and 1/10 and hasn't found anything, why do you think you need to continue trapping at that level? Jim feels you do not, but you can go to a different level. Jim feels that the trapping density needs to be tied to the control activity, if you find it. Ted clarified that it's detection and monitoring, not control.

Larry stated that if we do not find any larvae in central and eastern Arizona, that area would be declared eradicated. If we went to a significantly lower trapping density, given the fact that we still have Yuma, Mexicali, San Luis and migration, we could set ourselves up for a real problem. Jim advised that it depends on how we define minimum standard. Dr. Staten felt that Larry would have an extremely arduous task convincing his growers that he would have to spend that money, because after all, the minimum standard is "this".

Charles remembers that in the boll weevil discussions, you set up risk zones, so that in the higher risk areas, there is a different minimum standard compared to the lower risk areas. Larry advised that pink bollworm moves a lot differently than boll weevil. Charles felt that if you were not held to minimum standards, and were able to convey that to the growers, you could trap at more dense levels.

Upon inquiry, Jim advised that infested sections were trapped at one to forty, which is the standard Arizona uses in Bt. San Joaquin traps one to sixty, one to eighty, and one to one-hundred, outside of the infested sections. If the group decides that in order to declare eradication, they would trap one to ten non-Bt and one to forty Bt, Jim would consider trapping at one to ten in those sections that they have found moths in before, but would not be able to afford to do one to ten in the entire San Joaquin Valley.

Charles advised that you could set up risk zones. Jim said that he has bio-potential zones that have low risk due to climatology, and forty years of trapping data. Don stated that we have to keep in mind too that while everyone here understands this now, a couple of years from now, you will have some people who don't understand it and will ask why they are paying this for so many traps. Budgetary issues will exist and we need to be honest about what is a comfortable trapping density at the minimum. We know that there is not a lot of data to support it one way or the other, but we need to focus on the minimum.

Larry advised that his concern is that until we are at a point where the entire area has been completed eradicated, he can not afford to back off on the trapping, because he does not have enough money to do a wide spread treatment program. At the end of this year, Larry looses the ability to fund anything other than sterile moth treatments, with the exception of a small contingency fund which can be used to treat small acreage that may be re-infested. If he had a thousand acres that were infested, that would create a huge problem budgetarily. Bob Hull stated that there needs to be a new referendum stating that we are at about an eradication point, or bale assessment fees to justify having a big enough contingency fund in order to deal with these issues.

Jim deals with a large geographic area, as Texas does too. Dr. Staten felt that if we use that kind of terminology, we are looking at the San Joaquin at being the lowest risk for immigration by flight. Jim reviewed a situation in where they caught 151 moths in one location and some more in the other where they suspect that was seed from Israel. Larry advised that what has worked in Arizona for boll weevil, was to use the risk base approach in relation to a certain distance from a population or ongoing eradication program where the population has not reached zero. We used the tier approach in Arizona, a trap per section on a grid, so that it's in cotton but equally spaced with the goal being that an infestation would not exceed a square mile before we were able to detect it. Something like that may apply to pink bollworm.

Don advised that it does help to discuss things that go on in boll weevil while we are talking about pink bollworm post-eradication. If we can use some of their experience, that would be to our advantage. Charles inquired, and Larry answered, that his trapping for boll weevil now is a trap per quarter section in cotton on as much of a grid as possible, beyond fifty miles of the border. Within fifty miles of the border, which is probably not applicable anymore, we are at a trap per forty acres.

Dr. Staten stated that San Joaquin is almost an island of desert. It is a considerable distance from other cotton. If we look at the rest of the areas, there is almost contiguous cotton from Torreon, all the way up through Texas, and from Mexicali all through this area. It's like stepping stones. Biologically, you could certainly define that area as different in geography, and in that no grower has ever had to treat a pink bollworm population in the San Joaquin. Jim agreed. Dr. Staten advised that it has always been handled through regulatory action and the vast majority of it was handled with sterile insects. That defines the San Joaquin separately from the rest of the area.

Upon inquiry, Jim reported that the San Joaquin is 160 miles from any active pink bollworm eradication. Don asked what the biggest outbreak problem was in San Joaquin before it was detected. Did you have a native flight that moved in and got out of control before you realized it was there? Jim answered no. Dr. Staten said that he felt there was. Jim elaborated by saying that in 1990, he had a rather large infestation. There were seven or nine thousand trapped. There was a million acres in the valley, but the field size in question was a little over a hundred. Dr. Staten reported that the year they caught seven thousand was not the same year they got the larvae. Don stated that it is a significant thing to think about, in looking at it as an island. You have had the movement come in. Dr. Staten and Jim concurred that the trap density at that time was one to forty.

Charles stated that since the pink bollworm in more apt to movement, state that beyond one hundred miles of an active pink bollworm population, a minimum standard would be one trap per hundred acres. Then you would have the flexibility within the San Joaquin, to have a higher trap density in your higher risk areas. Dr. Staten said that you could actually say that your minimum trap density is one trap per one-hundred acres of conventional cotton.

Upon inquiry, Dr. Staten answered that if San Joaquin does plant Bt, they are not going to monitor where it is. Charles said to maybe make it 200 miles, in order to draw a distinction between San Joaquin and the other areas. Dr. Staten replied that in places like Arizona, Trans Pecos and New Mexico, we want to see monitoring for the next two years in order to insure that we have reached eradication, and that they do not get into trouble before their resources can get them out of trouble. Particularly in New Mexico, there are no resources. Basically, they are going to have to be bailed out. Fortunately, there is only projected to be about 10,000 acres of cotton next year.

Bob Hull stated that when we don't have the ability to attack a problem freely, quickly and accurately, we have got a problem. Don reported that the Boll Weevil Technical Advisory Committee had a recommendation that there needs to be a reserve pool for use wherever needed, because of the difficulties associated with moving funds across the states. There was supposed to be a program fund in order to deal with outbreaks. Charles advised that the fund was only accessible if the program met the minimum standards. New Mexico will be struggling to get the funds in order the meet the minimum standard.

Ted Boratynski asked about an area where they stockpiled equipment and things of that nature, that they could bring to bear. Don reported that there had been some proposals, but it has not been finalized. Larry advised that a while ago, there was a discussion that occurred regarding various areas having some sort of contingency fund regarding minimum standards in order to be able to tap into that pool. Larry wasn't sure if that stuck or not. Charles advised that it is a concept, but is not complete. Don Parker advised that things should get wrapped up soon. Within two years, we will no longer have a Boll Weevil Action Committee, rather we will have a Boll Weevil Post-Action Committee where everything has to roll into a totally different approach and totally different structure.

Don asked if the Committee is in consensus in the approach that the trap densities are based on the distance from the risk zone? Dr. Staten felt that is our best option. Don stated we should start discussing the actual distances we want to use. Ted asked if we are going to discuss what is going to happen in the areas that have been declared eradicated in the two years after the eradication is declared. Don stated that we should define our distances first from the risk zones. Upon Dr. Staten's request, Leighton put up the map districts on the screen.

Dr. Staten asked that we get some broad concepts of how this should flow and then get a small group write it up and write it out because we are starting to run out of time. Don

concurred. If we look at a map, we know what we want to happen in the New Mexico and Trans Pecos, and even the east side of Arizona. We know that we don't want to change the San Joaquin operates, and we want to give them a minimum standard that they can afford to operate in and let them understand their own risks.

Let's look at Trans Pecos and Juarez. Don asked which specific area was New Mexico told that they could check every other week. Dr. Staten answered the non-Bt cotton. Joe thinks he will be able to go weekly, one to ten and one to forty. Dr. Staten advised him that the highest priority area is south of Las Cruces. As you move away from there, if you do not have resources, go on a two-week schedule with the same trap levels. Joe feels that because he is not going to have the identification crisis, with so many traps coming in so often, he would probably be able to maintain that standard this year. There will only be a relatively small area where he is going to have sterile overloaded traps. Don advised that using a lower density of traps, there is a risk of not being able to detect something. A program being voted out would be a far worse situation. Dr. Staten concurred, and advised that he does not think they are at risk of having a program voted out.

Don advised that there is a referendum coming up in Texas that he does not know if they feel comfortable with yet. Charles advised that he does not feel it should be an issue. They are paying a lot more compared to the guys in New Mexico. Charles pointed out on the map where the cotton in the Trans Pecos primarily is located, as well as the risk zone in New Mexico reporting that a lot of it is Bt cotton, and showed where the low level pink bollworm infestations in Texas are located. Charles reported that there will be some trapping in eastern New Mexico, because no one trapped there for a while.

The other risk in that area other than the highway is Mexico. Dr. Staten advised that there was no discovery of pink bollworm life forms in the area being discussed. In Ojinaga, three moths were caught on the river. Charles stated that there is no cotton on the U.S. side. Dr. Staten concurred, and reported that there is a well-documented migration through the corridor. Along the river, it should get pinsquare malathion treatments again for boll weevil, and is under heavy boll weevil control. They have in the past in all of the rest of these areas used pheromone for any discovery in the Fall. We have not seen a larvae there in a long time.

Charles advised that there are active populations of pink bollworm within one hundred miles of cotton in the Trans Pecos area. If you use the hundred mile criteria, it's where the trap density could drop off with the Trans Pecos. You could do the same thing along the river in El Paso. There's another risk factor there though because that is the border. Charles advised that in boll weevil, they left the trap densities high along the border. No matter what goes on in Mexico, we don't have our eyes on it, so we have high trap density.

Dr. Staten reported that it is still active eradication, so it is the hazard. Charles advised that he is looking beyond that. In 2008, that area would be eradicated. We didn't find any evidence of reproduction in the El Paso / Trans Pecos area. Dr. Staten advised that if you are trying to use boll data as evidence of reproduction, you will fail. You will not detect it with boll data. Charles argued that we just worked that out. If you caught moths and didn't have any evidence that suggested otherwise, you didn't have a reproductive population. Dr. Staten disagreed and said that may be true for boll weevil, but not pink bollworm. Charles stated that the definition we voted upon lends itself to that theory and that was his understanding. Dr. Staten stated that's why he felt uncomfortable about it because in boll weevil, you have better ability to find reproduction. You could have a tiny cluster of pink bollworm in different places, for example, in the San Joaquin they have had it, Jim used more than boll data. He uses evidence of generations as well. In this case, you have really

good evidence that you have over-wintering into the Spring. You have positive captures in the Fall.

Charles stated that he didn't catch anything within season, until last season. Charles reported that they went about eight weeks without catching anything, and felt it was unreasonable to think that you have a reproducing population when you are able to go to the middle of the year without catching anything. Dr. Staten disagreed. Charles stated that it could be argued you don't have a reproducing population right now in the Trans Pecos area, according to this definition. Dr. Staten stated that's why he disagrees.

Don Parker agreed with Charles, stating that by this definition, that is the case. Greg suggested that we may want to do some research on this. Greg advised he understands that the Committee is defining it that way in order to have a resolution to use for the program. Don stated that it would probably be a good idea, but we would need to have something before the research can be done. We have been trying to stress that. Larry advised that one or two adult captures would not meet the generational structure criteria.

Charles feels very strongly that the late season captures were not native moths. The problem is that it's guess. Dr. Staten reported that we have never generated a fraction of data that says that dye is completely gone in any moth that we know was ours under any experimental conditions. Charles reported that Greg Simmons has. Don stated that it was worth pointing out and asked Greg to elaborate. Greg stated that he did find one that was negative for Calico red, but positive for DS Red on a PCR test. We also discovered that the fluorescent microscope has an error rate. We measured error rates on Calico and DS Red fluorescence. There were some moths we couldn't PCR for technical reasons. You can see the Calico red going away. Dr. Staten concurred, but we don't have a body of experimental data where we have been able to demonstrate that and define it under any controlled conditions.

The problem is even if we know that it is possible, we have no way of knowing which one it is, and therefore can not assign a probability statement. If you are very sure they were native, Mexico did not get any non-sterile moths at all in that late season. Dr. Staten does not want Charles to cease sterile release. Charles agreed, and stated that he does feel confident of what he saw. Larry inquired, and Charles advised that they had no generational structure, and they were very thin. Dr. Staten guessed that if you were to do a heat unit graph for that area, the peaks wouldn't match.

Jim advised that concerning the draft document that is a risk-based monitoring and post-eradication scenario discussed earlier, Larry Antilla, Edward Herrera and I circulate what is written up. Dr. Staten advised that as Chairman, he would like to make the appointments. Jim stated that he was volunteering and making a suggestion. Dr. Staten that since he is here with Larry, Dr. Staten will work with Larry to draft something, and get Jim on the phone and get it to him. We will do it in two tiers. We'll do that and do the next tier. Dr. Staten would like to get it to Charles as well because he is now neutral as far as program, which is a nice thing to have him on the Committee. He is no longer a Program Manager. The reason Dr. Staten does not like Jim's idea, is because it is all program management and Dr. Staten wants it to come from the scientists as well. Jim concurred.

Dr. Staten suggested that we look at it, look at the geography, and see how it fits. Make sure it's going to stand up to the test that we think it ought to stand up and then define those distances based on that. Are people going to be comfortable with having one trap per ten and one trap per forty? Are we going to be comfortable in New Mexico telling Joe that he can go to one trap to forty acres across the board? At any rate, the trapping density should be tied to your confidence level of being able to control that native or non-sterile insect that

you find. Jim doesn't see any reason to trap at one to ten, if you can trap at one to fifty, find a moth, and control it with either a one square or nine square mile sterile release. Jim does not "dis-correlate" his trapping densities with his subsequent control activities in any other insect program that he runs including gypsy moth, fruit fly, Japanese beetle, etc. Don advised that when you have a find in boll weevil, there is a minimum standard for what gets done and the area around it where it is tied to the trap density.

Dr. Staten advised that given field sizes and field geography, perhaps we should look at the maps of the area, and ask if we have this kind of density, what is it going to look like, and are we going to find it in any field before we get a large larval infestation. Jim stated that you can do that trap density on a risk based approach. There may be areas that you define as high risk or low risk areas where you assign a particular trap density in order to distinguish. We can change that density and not loose the program.

Larry advised that we can also incorporate Charle's idea, for example in New Mexico at one per forty as the minimum standard. Larry wouldn't go that low in his area until he completed eradication in his region. The only problem is New Mexico does not have resources. Dr. Staten advised that he told the growers there that we have never dealt with an area where we have eliminated an ongoing, full-blown, grower has to extensively spray, before. He doesn't want to find a population there like the population we found in El Paso two years ago on seven acres when the trap adjacent to it wasn't catching. Dr. Staten is concerned about that, and will be very conservative until he finds out that we are doing okay. Dr. Staten wants to stay very conservative with New Mexico and Trans Pecos right now. Dr. Staten said that tells us how we are going to approach trap density, and wanted to move the discussion to the Action Plan. Don concurred with Bob and stated that if you are going to appoint a Committee to get a draft sent out to everybody for comment and start working it from there, we may as well do it all. Dr. Staten suggested that we try and get something in draft to everybody within this coming week. Larry concurred. Dr. Staten said he thinks we may be able to work out a draft in one day. Larry stated that April 1st would be better.

Don felt that there is something we should clear up before the Draft Committee gets together so that we know how to start approaching things. Since we are going with the risk zones, he verified that Dr. Staten's intention is to keep San Joaquin in the whole picture. Dr. Staten agreed, stating that Jim's standard will be right where he wants it to be. Don stated that it does help with regulators.

Ted asked how long you have the sterile insect will be one of your tools. Dr. Staten requested everyone look at the template carefully and make sure that this is what we want to be working with. Don reported that earlier we had comments about the nine square mile block, and because traps double in the nine square mile, there were some heavy comments. Dr. Staten advised we can back up on the trap data. Dr. Staten discussed the nine square mile block when he met with Joe and Edward. Dr. Staten wanted to make sure that we were all on the same page in how releases would be handled, because the plane was flying out of Texas. Joe was very comfortable with the nine square mile block.

Greg asked if we should put release rates into the document. Dr. Staten said that we probably won't. Steve suggested that another source for information may be the pink bollworm resistance to Bt remedial action plan. Dr. Staten reported that is where they got the information for the nine square mile concept. Upon inquiry by Ted regarding the location of the epicenter, Dr. Staten reported that you could never really define the true epicenter.

A member inquired whether or not they should go farther in order to define larval finds, or is that the second layer. Dr. Staten felt we should look at evidence of reproduction. Bob Hull

stated that alerting the community is a good idea. You have PCA's and farmers out in the field. With the bloom survey, even if you just ask people to look for the rosetta blooms, it still can be pretty effective. Dr. Staten noted that the early historical cases in San Joaquin were found by nursery inspectors who stopped to see what cotton looked like, which is not unusual in the detection of exotics. Most frequently, many surveys were set up, and it's the household call-in, or the Berkley taxonomist that finds the pest. Awareness is very important. The good news is that we will have a very aware population. Dr. Staten commented and Jim concurred, that probably many of the PCA's in San Joaquin have never seen a rosetta bloom.

Don asked if there should be a sterile release on any cotton field, with the capture at the epicenter, and when comparing some of the boll weevil information, it was when you had a capture, you increased your trapping, and if there was another capture, then you had treatment around there. Larry agreed and noted that your epicenter would expand around that. Don stated that if you have a capture on a field, is that enough to justify that expense when it may have been a fly in. Jim advised that you don't have a choice. You have to err on the aggressive side and put out traps because the risk is too high.

Dr. Staten advised that the technology for control in boll weevil is to kill it. Jim stated that he has had different criteria depending on where he caught the moth. If it is his hot zone, he immediately throws another trap in that field and starts sterile release on that field the next day. If it's in our least-concerned bio-potential zone, we increase the trap density and sit on it and see what happens. In some cases, nothing ever happens. Charles stated that he doesn't have a bio-potential zone in Texas. Greg said why not tie it back to the reproduction.

Don stated that if you are talking about a minimum standard document, is the minimum response that is acceptable going to be a sterile release on any cotton field within nine square miles of that epicenter. Jim asked if you don't set a precedent in doing that where you are not going to treat, in terms of where APHIS might go in the future in terms of rearing and availability of steriles, and where that might go from there. Dr. Staten does not want anyone to draw parallels too much between boll weevil and pink bollworm. We are using soft technology with pink bollworm, and not killing it. Don agreed.

Charles noted that there is a very small window of time where you have the ability to address it before that insect is in there laying eggs. Don agreed. Charles asked the location of the densest cotton producing area in 2009, and how many acres of cotton would be within a nine square mile block? Dr. Staten answered it would be in El Paso. Charles stated that we are dense, but we are strung out in a line. You are really only talking about three miles and maybe a couple thousand acres at the most, maybe fifteen hundred acres. Ted stated you have to consider the other side of the river as well. Jim stated that his area varies by county. In some counties, his average acres per square mile runs around 200-225, and can drop down to 160-170. Ted asked what happens when you get a native caught in Blythe where there is no conventional cotton. Dr. Staten said that he would not release sterile insects over Bt cotton. Dr. Staten advised that it was geared to the Mesilla Valley and Trans Pecos area, and it was our first cut and a very short period. Larry advised that Charles makes a good point in that the whole concept of eradication of pink bollworm, there is a lot of pressure of new resources all over. Larry wants to see the sterile moth facility in place for a long time because that is our bread and butter in control. Dr. Staten advised that everyone was adamant that we put other technologies in for control in a response. Dr. Staten said that we could make the exclusion of higher elevations in Mexico if we want to. Charles advised that maybe it is a lower bio-potential zone. Dr. Staten wants to make sure that we maintain sterile insects as our first line or reaction for everyone. Charles agreed. Dr. Staten advised that it is the most mobile. You don't have to worry about labor

contracts. That's all that really fit for Joe. The good news in a place like New Mexico is we would be able to release the next day if we have to. Don agreed, and stated that the sooner we can get this put together, the better. Dr. Staten requested that everyone from the Committee watch for an e-mail from Tish Tamulis. When we get this done, she will send it out to everyone on the Committee. We will need comments back quickly.

The discussion moved to regulatory issues, and referred to the document Greg distributed. Dr. Staten said that the reality is that the regulatory issue has fallen to program managers. Dr. Staten reported that USDA APHIS has agreed to absorb the cost of going after a general release plant permit for DS Red. It will take more than a year. We should have a conference call, and we will have to get it to Joe. He has two draft letters. One refers to each program manager sending in the request for the permit in order to release for DS Red. The other is to do it as a group where it would be a document that each program manager would sign. When you release your sterile insects, you are now releasing it under a permit that was applied for by the person who receives the sterile insects. This keeps it in that arena. It is incredibly complex paperwork and there are a lot of things that make it very difficult. You will be authorizing Bob Rose to do it at the expense of APHIS. Don stated that each program manager would be liable for meeting all of the requirements of the permit. Dr. Staten advised that is true with any permit. The bad news is that sterile insects are excluding from labeling as pesticides. Dr. Staten suggested that we have a program manager call and get all of the information to you guys and do it by conference call. Larry said that he has not seen the document, and asked if he would have to discuss any liability issues with the Attorney General for example. Dr. Staten advised that he believes it is just like any other bio-control release permit.

Dr. Staten said that the other issue they want to pursue is going forward to the organic grower associations and trying to turn that around. Don hopes that they would talk to the Council before initiating anything like that, because he has been in communication with the BRS administration on the improper solicitation of information. Rebecca was doing an environmental assessment, which does not require input from the organic association.

Upon inquiry, Don answered that when the organic growers were asked about their position, they said they really didn't have one, and subsequently, they advised that genetically engineered insects did not fit the model of organic. It was not compatible. Jim advised that as a condition of the permit in San Joaquin, it will probably be on the permit that they do not release on organic cotton. Upon inquiry, Don answered that you can not have transgenic insects and you can not have transgenic plants, so transgenic in itself pretty much knocks it out of organic. You could see where a program manager who is signing a liability document could knock some organic farmer out of 800 acres of an organic crop because a program moth flew over there and got him disqualified.

Don stated that he did have a request in with BRS today trying to get some information. We have made a formal request this morning on the status of permits and have not heard back yet.

M/S/P **Steve Naranjo moved to adjourn the meeting. Ted Boratynski seconded the motion and the motion passed unopposed.**