Summary of Meeting to Discuss the Planned Simulation Studies of Alternative Stock Assessment Approaches for the Little Colorado River Population of Humpback Chub

Venue:

Conference Call – April 26, 2005, 3:00 -5:00 p.m.

Attending:

Lew Coggins - GCMRC
Barbara Ralston - GCMRC
Bill Persons – AZGFD
Rich Valdez – Interested Party
Mark Brouder – FWS
Glenn Knowles – FWS
Dave Otis – Iowa Co-op

Purpose:

1) To brief interested parties on the background, methods, and objectives of the planned simulations to investigate the performance of alternate humpback chub stock assessment options.

2) To solicit suggestions and possible modifications to the study objectives from interested parties.

Agenda:

3:00 pm	Introductions and review of study motivation, background, and meeting materials	Coggins
3:15 pm	Overview of planned simulations	Otis
3:45 pm	Discussion of objectives	All
4:45 pm	Wrap up and recount any study modifications	Coggins
5:00 pm	Meeting Ends	

Summary:

The participants generally expressed that they had a much better understanding of this study as a result of the meeting. Much discussion centered on: 1) the scope of the simulation effort relative to both the population that was to be modeled and the types of models and assessments to be compared; 2) the utility of the effort relative to providing guidance and clarification to managers on uncertainties associated with stock assessment results; 3) how parameters would be incorporated to best simulate the important characteristics of the LCR population such as

movement and skip spawning, 4) how parameters would be incorporated to best simulate alternative sampling designs, 5) recommendations for modifications to study objectives.

Revised Study Objectives:

1) Estimate the magnitude of bias in abundance estimates produced by each of the 3 ASMR models, Jolly-Seber models, closed models, and Robust Design models, when there is: i) error is assigning age at first capture, ii) various types of heterogeneity in capture probability not related to age, iii) annual variation in adult mortality rate, iv) various assumptions in movement patterns between the Little Colorado River (LCR) and mainstem Colorado River. Each of these scenarios will be evaluated under various designs that involve multiple sampling occasions in the LCR only or in both the LCR and mainstem Colorado River. .

2) Compare the bias and precision of abundance estimates and the power to detect population trends, from ASMR, Jolly-Seber, closed and Robust Design models when sampling is conducted every year, every other year, every third year.

3) Compare statistical precision of population growth rate estimates from Pradel (1996) models to growth rate estimates derived from population abundance estimates, in the context of the listing status decision process.

4) Advise on the potential utility of model selection procedures for ASMR models.

5) Use simulation results to make recommendations on the design of future HBC stock assessment programs in Grand Canyon.

6) Explicitly evaluate concurrent sampling approaches during Spring months compared to Fall months incorporating differences in proportions of fish inhabiting either the LCR or the LCR Inflow reach of the Colorado River, and differences in sampling efficiencies in each of those locations.

7) If possible, compare the bias and precision of recruitment estimates and the power to detect recruitment trends among various models, sampling designs, and assumption violations as specified in 1).

8) Discuss all findings, particularly the statistical power and robustness of trend, adult abundance, and recruitment assessments, in the context of utility to assessing humpback chub recovery goals.

Bolded objectives were identified during the course of the meeting.

Timing of Next Meeting:

A subsequent meeting of this group and interested parties within Region 6 of the USFWS will be scheduled prior to the August AMWG meeting in order to brief participants on study progress and possibly refine simulation structure and input parameters.

Minutes:

Lew Coggins stated the overall purpose of the meeting is to brief on the objectives and overall tact of the simulation study that Dave Otis has been contracted to provide us. Also, to solicit suggestions from everyone relative to those objectives and the overall study to make sure any kind of items that has been missed that anyone would like to see addressed and for us to look at to see if they can be addressed and, therefore, modify the overall objectives before Dave launches off on this project.

Lew Coggins gave some background on why the simulations are being conducted. Over the last few years there has been a lot of discussion about the adequacy of the current chub monitoring in Grand Canyon, particularly for assessing Fish and Wildlife Service (FWS) recovery goals. Several years ago, about 1 ½ years ago, Adaptive Management Program commissioned a review of Humpback Chub (HBC) stock assessment programs, specifically for the Grand Canyon, but also this independent panel looked at stock assessments programs in the upper basin. In the March 2004 meeting of the Adaptive Management Work Group (AMWG), the results of that panel's findings were provided by Dave Otis, who was on that panel, to the AMWG and their final report was made available to that panel. Also at that meeting, a letter to the chairman of the AMWG was available from both Regional supervisor from Region 6 and Region 2 FWS and the Director of the Arizona Game and Fish Department (AZGFD) was also available. A copy of that letter is available upon request. Basically, the AMWG passed a motion at that meeting to fund concurrent abundance estimates for the LCR population of HBC. The next thing that happened was a minority report was sent to the Secretary from some of the members of the AMWG that recommended against that recommendation. Subsequent to that, the Secretary asked the DOI AMWG members for recommendations, essentially what the Secretary should do since she was confronted with competing recommendations. He also has a copy of the minority report if anyone needs one, it will be available. The AMWG members then drafted this letter, which he sent to everyone, suggesting or recommending rather than initially funding the concurrent abundance estimates that these simulations be conducted and that filtered down to him to make sure that it happened. He (Lew) has been in contact with Dave Otis and, after much discussion, he has developed this simulation overview, which everyone has copies of, and one thing he (Lew) points out is on the final page of the simulation overview is a timeline for this project. At this point, the timeline suggests a first progress report in July 05 and what that is meant to coincide is the summer AMWG meeting, which he (Lew) now understands is going to be in August, not July, so the hope is that Dave will be able to provide a progress report to the AMWG at their summer meeting and that there will be subsequent progress and final reports in December 05. That is the overall timeline and what got us here.

The next thing Lew hopes to do was to have Dave Otis essentially walk through his simulation overview as well as get into some specifics of the objectives and how to achieve them. Then we will have a discussion among everyone to look at the objectives and what Dave has provided and talk about how they do or did not meet some of the groups expectations.

Dave Otis stated he became involved in this about 1 ½ years ago. He started out by talking about the simulation approach in general in a sequential way. When the committee wrote the report that Lew mentioned, one of their recommendations was to do some simulation. It really wasn't

in the context of this concurrent sampling issue. That was discussed at that meeting in California, but we (the committee) were really more focused at that point of the conversation about ASMR models and closed models vs open models and that issue as opposed to the concurrent sampling issue. So we had proposed that some simulation be done to see and dig into the operating characteristics, so to speak, of the ASMR model and to validate it, if you will, or whatever term you want to use. That is a fairly standard approach that statisticians often times use to try to look at things like bias, precision, and variance and those kinds of things statisticians worry about in a situation where we know what the right answer is. Of course, that is the big advantage of doing all the simulation modeling if you know what the right answer is so you can calculate things like bias and in the field, of course, we never know what the right answer is. The other part of that was not only to evaluate bias precision, but to evaluate how that model and other models broke down when some of these assumptions that you all are concerned about may be violated so that gets into this business about heterogeneity in capture probabilities and what happens if fish move from place to another and become basically invisible to the capture and those kinds of issues. What kinds of trouble we get into if we use one method or another and which is the best one that might give us the best insurance, so to speak, against those kinds of breakdowns. That is kind of why we had suggested simulation in the first place.

Then the concurrent sampling issue came up and the notion of doing the field survey and try to compare the estimates that resulted from two field surveys to, and in some sense validate ASMR model or try to decide the best way to go. His thought about that has been that is not necessarily a bad thing to do, but to him, that you would do that within the context of knowing something about the statistical properties of these techniques, in a perfect world admittedly. The downside is that these models are based on assumptions, they are not real like the field is, and we all have to recognize that. But it places results that you get from the field can help place those results in context so they are more interpretable in terms of what do these things really mean, given what we know about how these different models work. Again, simulations seemed like a valuable sort of tool in this context as well as the initial one about evaluation of the ASMR model. This gave us a way to directly compare the strengths and weaknesses of all these potential competing models because we can basically can simulate data under a given set of assumptions and we can feed that data into several different models and estimators and see what happens and do this over and over again and try to get a picture about how it performs on the average and how much variation there is in that performance. We can look at breakdowns and assumptions on a level playing field; we can also play games with looking at different sampling designs. There are issues like aside of all the capture probabilities issues, how many passes should we make and what do we gain if we go down 3 times instead of 4 times, etc., we will be able to look how those basic sampling dimensions effect the precision and the power to detect trend over annual timeframes so we can simulate this over multiple years, whatever makes sense to you guys in terms of what those timeframes would be. I am sure the recovery goals are relevant in that context and are really probably the most powerful tool to do that sort of thing.

Admittedly, as I said before, these models are based on assumptions and so how good these models and how good the simulation is obviously depends on what kind of parameter values we are able to feed the models and, fortunately, you guys know a lot about the system and are able to translate that into reasonable specifications, so to speak, about what values some of these things we worry about might take on, how much do capture probabilities vary, how much

movement might be among these fish, what are the different population size ranges we want to look at, all those kinds of things that normally have to have a number assigned to them. We are informed about what those things might be so that is obviously a huge advantage over some place that does not have a very rich data history that can be mined to give us those kinds of things as opposed to having start from scratch. That is sort of the philosophy I guess of the simulation approach toward the general. As he said before, no one pretends that it is real, because it is not and that is a downside. On the other side of the coin, in terms of a single sort of year of concurrent sampling, statisticians would react and say "well, that's ok, but that is only sort of a single realization of a very complex and variable process and what can we learn from a single realization." Again, we can learn some things but there are a lot of things we can't learn about a sample size of one, so to speak, in terms of that kind of comparison. But together, I think with simulation again put in context in a single realization of unfortunately what is going to be a very complex process is very informative. Of course, in the real world we don't know what the right answer is, so there is this context of bias and that is what we can really get out of the simulation.

So, a few words about the assumptions that I am making with my limited knowledge about the canyon and the fish is that it will build basically two subpopulations, for the lack of a better term, or maybe it is a single population that occupies two different places, however we want to frame that, but there is the issue of the LCR and the issue of the mainstem near the confluence and, of course, that relates back to this movement back and forth between the two, but we can build populations in both of those places and assign them different capture probabilities, which I understand in probably appropriate since again we know you guys are not as successful catching them in the mainstem as you are in the LCR, we can attach to them some movement rates and that sort of thing and allow there to exchange between these two places based on the guidance that you guys have in biology. We can have different capture probability structures in those two places, we can make different assumptions on how fish react to capture in those two places, perhaps depending to what gear types are used in different places, maybe it makes sense to not assume the fish are not going to react in one place the same they will react in another place. Again, to build that population up in the two places and then go sampling as you all plan to do with concurrent sampling, with multiple passes presumably through each subpopulation and again sort of allowing things to be going on during that sampling, most notably I suppose movement, and then under all these various scenarios, sort of subject that data to any one of a number of different ways of handling the data. Which might range from using the outline that Lew referred to from the Secretary to look at closed population estimators, perhaps independently in the two places, do a closed population in the mainstem and a closed population in the LCR, and add them together. There are other ways to doing that, you could sort of pull all the data together and treat it as one big single concurrent sample, if you will. There is interest then, of course, if we only sample in the LCR and use only data captured from the LCR either in a closed population or in an ASMR model, there is the ability to then do the sort of validation we talked about in the original Kitchell Report, which was ok, lets see if the ASMR model does anything different than the Jolly Seber model does. I guess which it probably doesn't, and then also look at the possibility of using what is sometimes called the Robust Design, which I think is also mentioned in the Secretary's letter and is mentioned in our report as sort of a way to combine the best of both worlds for those models and is really appropriate for a situation you all

might get into were you have multiple passes in one or more of these places, you already have this kind data actually, you have multiple passes within a year and you have multiple years.

Again, there is no reason not to try to utilize that information among years and treat year independently and, in a perfect world, there is no reason if you can do it not to pool all the data in a given year because pooling usually loses information, and try to squeeze the most out of each individual run down the river that you can, that sort of notion that the Robust model uses and see how that might work. There are still lots that he (Dave) has to learn. He doesn't necessarily know what the statistical objectives are of the whole project in general. That goes back to the recovery goals, in terms of how precise we want these estimates to be, he knows that the issue of power have been mentioned and what we would have to do to be able to detect such and such a decline and such and such an increase, which always begs the question, well how much of an increase or decrease we want to detect and all those nitty gritty statistical details that we still need to work out and obviously get feedback from you all on. That is sort of my thumbnail sketch that maybe helps a little bit more than did the original 3-4 pager, because I am trying to work my way through this as well and trying to make sure at the end of the day it accomplishes what you guys need and it is certainly worthwhile trying to decide that before launching off.

Lew Coggins opened up the discussion relative to what is outlined in this document and what Dave just talked about and how that may be different than what some of you had in mind. Maybe that is one way to phrase the question in order to get some questions and kick this off a bit.

Rich Valdez thanked Dave for providing that overview and for what he provided over the phone was very helpful. Dave laid it out very well and this addressed some of the major issues with regard to precision and some of the questions regarding the statistical aspects. I think the fundamental question that brought this about was how many HBC are there in the Grand Canyon population and he thinks another thing we need to keep in mind, as we go through this exercise, is to try to see if once we get beyond the evaluation of the precision and the sensitivity of the estimates, is it in fact possible to come with some kind of a real estimate, if you want to call it that. He asked Dave what his take was on that. Is it going to be primarily statistical exercise or can we come back to the managers with some kind of real numbers?

Dave asked if he meant to come back to the managers at what stage, after the simulation or after we do concurrent sampling has been done for while?

Rich answered after the simulation.

Dave stated that one way to think about that you could start by laying out the optimum case, perhaps, and work back from that. He thinks, for example, that if it turns out that we decided what would happen if we made four passes (arbitrarily picked numbers) down each of these places in a year, which is probably exceeding the limits, the first question would be "how good is that?" under all the potential of the worse case or best case scenarios, the absolute maximum sort of effort case, you can be assured as much as we ever are as field ecologists that you would likely get pretty close to the answer that is dependable statistically and that will satisfy what you

need to do to manage the species. As you back away from that, here is what you give up and here is how much more uncertainty you are left to grapple with, so to speak, in knowing how many fish there are and what are the trade offs. I don't know if that is the sort of thing, Rich, that you think will resonant with the managers or decision makers. What do we do, will we be able to make an informed decision if we do this, our expectations is we can get this out of it, if we only do this we get this much less. We never know the truth, but I don't think anybody expects us to know the truth, we just have to provide information to them so they can make the most informed decision there are about much uncertainty is acceptable.

Rich responded that he agreed and thinks that is a realistic answer and thinks it is important that the people understand. Managers always want a number, but Dave is right, with that comes a certain tradeoff and understanding that with that number comes a certain amount of precision and confidence and they are either accepted or like Dave said, make a 4th pass if they have the money to do it. He asked Dave if he would populate the models with prior data with real data that has been collected. How is he going to handle the simulation?

Dave said initially he thought they would parameterize the models based upon previous analyses of real data. So again, if we are going to try to simulate the probability of a capture of an age structure, or of such and such age old fish that pass through the river or through the reach, or whatever, we have a reasonable guess from the past analyses what that might be, is it 10 %, 20%, or 50%. These models are going to have a fair number of those kinds of parameters in them that are going to be required to build sort of make believe populations, but again, as he said, before we could make pretty good guesses what is realistic and what is not and we know the differences and things about the mainstem vs the LCR. I guess that is the way he envisions using the past analyses is to extract not raw data per se, but through parameter estimates that come out of those things to be able to parameterize the simulations

Mark Brouder added onto Rich's first question from the service perspective and what he has heard from the short time he has been in the group is there is strong interest on the part of everyone that we want to know when can we get an accurate population estimate of the Grand Canyon population of the HBC and start the clock toward trying to meet some of those recovery goals, which is where you were kind of leading in the first place in regard to numbers, right now we have estimates of the LCR HBC with concern that there may be a significant component of the mainstem that goes unaccounted for during spring and fall estimates in the LCR. Will the stimulation modeling get us to the point where we know if there are unaccounted for fish in the mainstem? Mark had some other comments that were impossible for the transcriber to understand because he was speaking too low.

Dave responded he had a couple of thoughts. We are going to have to make some assumptions about and play out some scenarios about how many fish might there really be in the mainstem while we are doing this concurrent sampling and how much movement might there really be between the LCR and the mainstem, I guess that is the skip spawning issue, all those sorts of things that can screw us up and give us only an estimate of part of the population. Again, under the assumption of so many fish in the mainstem and whether they move or not, we can make some informed guess about how much we might be underestimated the total population if, for example, we only sample in the LCR. Of course, that average is going to be different depending

on assumptions about how many fish are in the mainstem and whether they move or not. So ultimately you can think about that as a circular argument. If there is a bunch of fish in the mainstem and they don't move and we sample in the LCR, of course we are going to underestimate things. But there is no way, if that is the case, you are going to get to that problem unless you sample in the mainstem. In order to be perhaps more confident or the most confident about it in the long term relative to getting this clock started, the only way you are going to know that is to actually go out and do the simultaneous concurrent sampling.

Lew added that we need to think about the broader structure of the HBC in the Grand Canyon, the distributional structure and, in his mind, none of the work these simulations are going to address are going to, for instance, try to account for fish that all of our marking data suggest don't frequent the area of the Little Colorado or the LCR inflow area. Like Randy's Rock or Pumpkin Springs. Relative to an overall Grand Canyon estimate of HBC, I think we are just talking about the LCR aggregation in terms of how he thought of these simulations. That gets into some areas the recovery goals that he doesn't understand real well in terms of do we consider fish as contributing to the recovery goals that we really don't have any information to suggest that they contribute to a spawning population of fish or contributing to the recruitment dynamics of fish, of chubs, in the canyon. This is part of Mark's question. The other part is that we can formulate some simulations in terms of movement dynamics of the LCR population of fish, some of which occupy the mainstem and the Little Colorado River, that includes a component of the population that never moves out of the mainstem Colorado. We can easily simulate that as a potential population migration structure. It will become clear that if we have a monitoring program that only includes sampling in the Little Colorado, those fish that never move into the LCR and, in fact, likely don't contribute to the recruitment dynamics are invisible to that monitoring program. We would also simulate a monitoring program that includes sampling in both areas that will likely have the ability to monitor those fish. That also begs the question of whether or not it is important to count towards recovery those fish that likely never contribute to the recruitment dynamics. However, that is a question for the FWS and will not be answered through these simulations. Ultimately relative to having some understanding of what the overall population estimate is, if it turns out that when we simulate a sampling program that basically looks like past sampling that has occurred in the canyon and if the simulations uncover that we are likely to have underestimated or have negative bias in our estimates by some amount, it is that information that we would potentially use and that managers would potentially consider relative to the current abundance estimates that we have. How they may be biased in the context of an overall abundance estimate. Additionally, this exercise is not meant to reanalyze the data that we have in some fashion that gives us a less biased estimate than our models currently provide. It is meant to look at different sampling options including the ones we currently utilize and given certain population characteristics in the form of differences in capture probabilities by location, movement dynamics, and then to look at how badly our current estimates or those from alternative sampling designs and models may be biased.

Rich stated that helped at lot and he was hoping Lew would bring forth some of that explanation. He gathered we should have enough information regarding the movement characteristics of that group of fish to and from the mainstem, to have a pretty good idea of the numbers of fish that come in and if there is skip spawning and so on going on. With regard to the first issue brought up, he addressed as when the recovery goals were drafted, the idea was that the what was

considered to be the population or recovery in the Grand Canyon, that population was considered to be those fish that are centered around the Little Colorado River and reproduce in the Little Colorado River because that was the existing condition at the time, and still is, of the recovery goals were developed. Lew was right about that. Assessment of the recovery goals will not include fish in Middle Granite Gorge or Pumpkin Springs or any of those other populations, 30-mile or any place else in the canyon, it is only going to include this group of fish that are tied to the LCR. There may be, and he doesn't know how large it is, a component of fish that remain in the mainstem at the mouth of the LCR and don't enter into that population in which case you are probably right, they won't enter into overall contribution to recruitment and reproduction. He thinks that at this point in time, the universe of what we are defining as the population that is going to be examined or is in this exercise is probably pretty much what will be assessed in the recovery goals. It is those fish that contribute to reproduction in the LCR. The reason that was done in the recovery goals was that the anticipation was that if something changes in Grand Canyon, for example the temperature control device, and if there were to be mainstem reproduction, then it would change down the road the need for the way we monitor that population. He just threw that in for now as a thought for everyone to keep in mind, he was not suggesting we change the protocol or design at this time, just kind of a thought for the future possibilities.

Lew said one could also make the argument that even if these other downstream aggregations currently are not contributing to recruitment or if they never contribute to recruitment because of changes in the mainstem that need to happen for that to occur never occur, they do also potentially serve as reservoirs of founders in the event of a drastic collapse of our reproducing population. So he didn't mean to suggest they were useless, but he was glad the recovery goals were clarified and how they look at those aggregations.

Rich agreed that we don't know what might happen in the future but for right now we need to focus on those fish that are reproducing in the LCR.

Rich asked Lew if he was finding evidence of skip spawning and does he thinks they have a fairly good handle of the movement of mainstem fish to and from the LCR in order to at least portray some realistic characterization of what those numbers of individuals might be.

Lew responded that what we can tell from the analyses that we have done thus far is that the vulnerability or capture probability to fish as function of age is really dynamic over a course of a year. You look at this dynamic of, for instance looking at some of the oldest fish in the population, the largest fish in the population, and you look at how their capture probability changes over a course of a year. You see time periods where they are much vulnerable to capture, particularly in the spring, right when you expect them to be based on most successful sampling occurring in the Little Colorado and then it falls off in other months of the year. And so, the problem is that there are two ways to interpret that pattern, you can either interpret that given most of the sampling is in the Little Colorado, as these fish move in and out of the Little Colorado or you can interpret that as these fish remain in the Little Colorado and, for whatever reason, they become much less vulnerable to sampling, so a behavioral issue, perhaps. We do not have the data to resolve those two competing hypotheses. As far as evidence of skip spawning, all you really end up with if you think about the way the data have been collected is

you see for example a fish over the course of say 12+ years. Now that fish may have been captured 10 times and sometimes its capture is in the mainstem and sometimes its capture is in the Little Colorado, so you can definitely say this fish has moved between these two places, but when it wasn't captured in the Little Colorado in a particular year, you don't know it wasn't captured because it simply because sampling is a stochastic process of whether a fish gets captured or not, or whether it wasn't captured because it didn't come into the Little Colorado. He thinks that relative to capture probabilities, we can make parameterize the sampling process in a fairly informed way. But relative to the movement, we are basically going to have to specify a range of movement probabilities that operate on various proportions of the populations, a range of proportions and look at how the estimates associated with these various sampling regimes and various models then become biased because of that range. Without additional information that can be gleamed through telemetry or, for instance the remote PIT-tag antennae that Bill has just got done messing with and has been pioneering for the last 2 years, it is those type of data that are going to allow us to get estimates on rates of skip spawning and so far. In other words, we can't simulate or model our way out of this. All we can do is look at what kind of biases may be evident in a simulation under some various movement rates and proportions of the population that may exhibit skip spawning.

Rich asked Dave if that age specific capture probability is capured in your Objective 1.

Dave said yes, well, for example the open ASMR model and Jolly Sever model can be stratified by age to account for those by different age and different capture probabilities. He was thinking of things more that are akin to the kinds of issues the closed population models try to account for, like this behavioral response, that kind of thing that Lew referred for whatever reason some gear might cause subsequent changes in capture probability, so after you are captured once, there is no way you are going to get captured again, you change your behavior for whatever reason. That is a different issue. And maybe there is heterogeneity, he doesn't know, maybe there is heterogeneity caused by the gear. These are things that may violate that sort of critical assumption that everybody has the same capture probability that may be operating in addition to age and the behavioral one is potentially the most serious and maybe it is the least likely, he doesn't know. You can get in some real trouble if you have some serious response on the part of the fish or the animal to that initial capture. He doesn't know if we have any evidence of that, we probably don't have very good data from adequate passes in a given year to get a peek at that.

Lew said Rich is the person who has tried to utilize those models the most and his read on that work is because of a lot of those passes basically occurred over a long enough period of time, that you started running into other types of assumption violation with close populations models like closure because to have to deal with movement, relative to sampling in the mainstem, and you also have to worry about, over a period of time, some demographic closure problems with mortality and recruitment. As he recalled, Rich's best model was the "T" model that looked at heterogeneity in capture over sampling occasions, but that result seems to be prevalent to work being done in the upper basin and is likely due to sparse data sets for the most part because getting a good handle on that behavior requires a lot of recaptures and those models are data hungry to be able to estimate some of those other ways that we can be introducing bias as a result of behavior and time.

Rich said he thought that was an accurate assessment. It is a problematic issue in the mainstem because the fish tends to occur in quite dramatic clumps and at specific times of the year and aggregate in large eddy complexes. You have large numbers of them if you drop a net in the right place, you catch a lot of them, but if you put the net over 30-40 ft and put it in the wrong direction, you won't get too many.

Dave said he thought it was that kind of phenomena that could potentially cause problems with closed models. We have groups of animals that maybe on an individual by individual basis have similar capture probability, but we have different groups of animals that in effect have very different capture problems. It doesn't have anything to do with age. That is the kind of phenomena he was thinking of, but needs guidance on what sort of realistic kinds of things Rich just said to know how to model that kind of thing.

Dave asked when this concurrent sampling is envisioned, would you think of it as you were making multiple passes down in both places, that those passes would be occurring roughly simultaneously or alternatively or what? How might that stack up against what sort of movement rates—how we model movement rates. He asked if anyone could give him some feel for how to think about that in practical terms, could this be viewed simultaneously to speak or would that be totally naïve.

Lew responded there has never really been a fleshed out proposal, but he thinks the way we have all envisioned it is that sampling would occur in the Little Colorado and in the mainstem at the same time. Based on past efforts, we spend about 12 days in the Little Colorado in order to sample the Little Colorado and we spend about 12 days to sample that LCR inflow in the mainstem and these distances are large enough, you are not necessarily sampling every area of the LCR inflow all at the same time nor are you sampling every area of the Little Colorado all at the same time. Presumably you start at one of the LCR inflow reach in the mainstem which is miles away from the confluence and over the course of 12 days, about ½ way though you are sampling real close to the confluence and towards the end of the 12 days you are perhaps downstream of the Little Colorado and so there does exist opportunity for there to be movement within the events.

Rich said we have never come up with a formalized proposal for a concurrent estimate, but the other consideration to try to minimize that effect was to conduct a concurrent estimate at a time when there was not as much movement in and out of the LCR that is outside of the spawning period. The tradeoff of course there is you may have lower capture efficiencies in the LCR and the question becomes how much do you sacrifice there for that part of the estimate. So there are tradeoffs, no matter which way you approach it.

Bill Persons said that is where some simulation modeling might be real helpful in how we design a concurrent estimate, do we do it when most of the fish are in the LCR when we think we have a higher capture probability, do we do it when most of the fish are in the mainstem when we think we have lower capture probability. It sounds like you can run some capture modeling that might give you some guidance there, which he thinks would be pretty useful.

Dave said Bill was suggesting a mix of a fall parameterized scenario and a spring one.

Bill Persons said it was always tough in the spring because the fish don't have calendars and we schedule our work for Apr 7-19 in the hopes that the majority of the spawners will be in the LCR but they don't always cooperate. He doesn't know any way around that, that is just the noise we have to live with. That is a tough one, he was hoping someone from FWS upper basin would participate here and maybe give us a little more guidance on what an acceptable population estimate might be for them, but that is probably a different call.

Rich said Dave characterized quite well when he asked that first question, and that is you do the best you can with the available data and you understand the physical parameters of that effort that hopefully reflect the population and then you recognize that there is a certain amount of precision and managers have to make the decision as to whether that is acceptable or not.

Lew told Dave that he thinks that basically when we look at the range of movement probabilities and look at differences in what proportion of the population occurs in one these strata or the other, we are basically looking at the alternatives of sampling in a time period of low movement fall vs higher movement spring and differences of which strata the majority of the population occupies.

Dave said and perhaps higher or lower, whichever, average capture probabilities as well. He was trying to get an image or feel about how to think about that concurrent sampling.

Rich asked Dave if he was talking about making some year to year comparisons using pradel's lambda value.

Dave said he hadn't thought much about that, so far just sort of focusing on Capital N(?) and probably that is sort of an issue that would be helpful to get some feedback from the group, because to be honest he doesn't know from the services standpoint or whomever—in the big scheme of things he was thinking—he didn't know whether to put it on an even priority with Capital N or to think of it as more of a secondary thing or what. He doesn't think it is that big of deal to throw some of those pradel models into the mix or some of the Jolly Seber things that can be reparameterized in terms of rate of change of Cap N, that kind of stuff. He knows there was some very specific recovery goals in terms of how many fish and that sort of thing, but he don't know if there are similar goals in terms of trends over a specific time periods----

Bill said there is a specific recovery goal of not having a downward trend.

Lew said this was one of the items in his original list of objectives that he hoped we could look at and a big part of why it was included it was because it was one of the recommendations from the panel. In part, he sees it as another metric that managers might consider if they thought it was a reliable way to look of trend and it could also be compared in terms of robustness with abundance estimates. In his mind it was value added thing to get someone like Dave to do this because it is another way to assess trend that in the literature is portrayed as more robust and more reliable than abundance estimates.

Rich said he thought that was true, we are interested in both the point estimators as well as a trend that is probably over less than a 10 year period. Along with that too, something he thinks would be helpful to the managers would be an understanding of the risks that are involved in power if we call trend a non-significant decline and it is. I think that some kinds of things we need to understand is what would it take to be reconciled through sample size or whatever, what would it take to minimize that risk.

Dave said he agreed, especially if it is another goal that has to be achieved, so to speak, of no negative trend, no significant negative trend. Then of course that begs of statistical questions, one which he just mentioned, do we think of that trend in a sort of statistical terms or do we think of it more in biological terms. That gets into the power issue, is it any deviance away from significant at all.

Rich said it should be a full power issue, a full power trend analysis.

Dave agreed. It is similar to the Capital N business, the decision makers should understand what the relative cost is of messing around with and trying to decide if it worse to miss a trend that is that is there or declare a trend that isn't there. Of course, in this situation, that is a pretty big decision.

Lew said that thus far he hadn't heard any specific recommendations on modifying objectives and asked if he was missing something.

Rich said that one is the one that Bill Persons brought up and he would concur with that and that is to at least look at the two population segments and sampling those in the spring and fall under different assumptions of totals numbers present and the associated performance.

Persons said he could see that in #5 and we may come up with other specific questions that we might want to have answered once we see what you can do with this simulation model.

Dave responsed that as Lew said, we can come with a combination of parameters in terms of proportional residence and movement that portray fall sampling as opposed to spring sampling.

Rich said there is another aspect and that he thinks we are talking about adults, but the recovery goals call for estimates of both adults and juveniles. Lets look at juveniles to try to get some ideas for recruitment. The ASMR model approached that in a more comprehensive manner. He asked Lew if he had idea as to whether we need to address recruitment in this respect or does he think the ASMR pretty much covers that.

Lew said that the ASMR and the age-structured Jolly can deal with that relative to once you define the criteria age at which a fish is called an adult and also recognizing the problems with assigning age is a function of length, which is in the first objective. Right now we basically don't plan on trying to deal with fish that were too small to PIT-tag so we are dealing with basically 2-year old fish that are usually through their first year based on the growth curves, and about half of them are large enough to PIT-tag at the end of their second year. So the way we have dealt in the ASMR is that we are estimating an abundance of 2-year old fish and estimating

a vulnerability parameter that is reflecting that not all that age class is vulnerable to tagging. Certainly and with regard to closed estimators, we are in about the same boat. We can certainly stratify the abundance by size, the estimate by size, at 200 millimeters, which is what we have settled on kind of universally, and we can do that as well. He hadn't talked specifically with Dave about this, so we can get his reaction, in achieving this #1 objective, because of the way we assigned age to fish, we have to basically grow these in our population as well and assign them age and that doesn't seem to him a difficult thing to do and that allows us to stratify closed population estimates by size as well as look at issues associated with mis-aging fish because of variability in the growth function. That is how he had conceived this and he asked Dave if this is what he was thinking.

Dave answered yes and to be honest, he had thought a little bit about trend and hadn't thought at all about recruitment. You can get estimates certainly out of the open model directly and indirectly, but any way, in comparison to closed models, he doesn't know that there would be extra to learn about that. It is just a matter of—he thinks what we learn about the operating characteristics assuming that these are all adults, is that going to be much different than what we would learn by parameterizing a little bit differently if they were juveniles?

Lew said with regard to closed models, he doesn't think so. But Jolly and ASMR deal with recruitment estimates in different ways. That is probably the biggest difference between the two models. He had in mind, particularly when we want to look at change detection under a population that is either growing or shrinking or has some pulses of recruitment, that we can in particular utilize the ASMR model but also the Jolly Sever to look at detecting changes in recruitment. He has actually done some of it with the ASMR model, what magnitude of change of recruitment is likely to be visible. But of course, that is assuming some of the other assumption violations we are going to look at.

Rich asked if Lew is using something more kin to a robust design

Lew said in a sense we kind of do have a robust design now, but he wasn't trying to examine the detection of a recruitment signal with a closed population model, it was the ASMR model. What amount of recruitment above a kind of a baseline would it take before we said a big spike in age 2 and 3 abundance. That is something that he and Dave can talk about some more, but is that a specific objective that Rich would see as far trying to characterize just what Lew said, what magnitude of recruitment signal are we likely to detect under various sampling designs.

Rich said he thought that was an important parameter.

Lew said then the question is does Dave think we can do it.

Dave said he needs to think about that. He is not quite sure of what Lew was trying to say.

Lew said basically what he did was he simulated the data set relative to capture probabilities in the most recent years since we are basically doing closed populations in the Little Colorado and with those capture probabilities in hand, he then assumed that the overall recruitment had increased by an order of 25%, 50%, 75%, or 100% for a 2-year block of time and he sampled

that simulated population and saw that in a Monte Carlo setting, it was unlikely we would see a significant spike in abundance of 2-year old fish unless we at least a 50% increase in recruitment and a 100% is when it really showed up well.

Rich said the reason he brought it up was related to the recovery goals and said Dave would have to decide if it could done and again to focus in on the recovery goals.

Dave asked if the recruitment goals were in terms of trend, absolute, or-- he can't remember what that parameter is.

Rich said it would be an average annual recruitment that would equal or exceed average annual mortality, very generally population stability or growth.

Lew said as he understood it, the upper basin program is based on a closed estimate type of analysis, at least currently, and recruitment strength is inferred only indirectly. That you basically make the assumption that if the adult population falls, then recruitment was less than adult mortality, right?

Rich said yes, but with HBC we are having difficulty coming up with that parameter, obviously, because the fish are difficult to capture and we have small recapture rate.

Dave said that part of his hesitation initially is that population trend is probably the easiest thing to estimate, perhaps as we mentioned earlier, at least the most robust, then comes the population size and then comes recruitment. In his experience, trying to estimate recruitment rates, they are just pathological sometimes, they are just very difficult to estimate. He doesn't know about the ASMR model, but he does know it about the Jolly Sever model. To be honest, the right parameters are just often not very good and it could sort of muddy the water, so to speak, in terms of trying to make decisions about which way to go. But those tradeoffs, again, should be known. We can certainly say things about making recommendations about decisions on how these models perform with respect bias or whichever one makes no sense, we are just sort of driving the bus here, and so secondary issue, here is what you can expect in terms of recruitment but not base decisions on sampling designs and things on how well we are estimating recruitment.

Lew said we could need huge changes in recruitment to have a good chance of detection, which is another way to look at it..

Rich said that some treaties about this subject would be appropriate for this exercise and he would leave that up to Lew and Dave to decide about how much they would want to go into actual analysis, certainly this information should be provided to the managers.

Bill Persons said it was a good idea and to let them know there are different estimators that you can get a trend estimate, he doesn't want to call it easy, but it is probably the easier of those three, the abundance, which is still tough, and recruitment, which may not be possible. As long as they understand that.

Dave said that expectation may be more realistic.

Glenn asked to the extent that they could, he asked that they could comment on that relative to the recovery goals in their report, that would be really helpful to the managers too. He doesn't know if that was the goal necessarily to this project, or how easy it would be for us to do, but it would be really helpful if we could do that.

Lew asked for verification of what Glenn was asking – for an assessment of the robustness of those three estimators relative to reliability about making decisions about recovery- the robustness of them?

Glen said he just meant specifically what the recovery goals say are good estimators to use, not necessarily good estimators to use, but the language in recovery goals to speak to what they are saying in regards to what these simulations tell us about those estimators.

Dave said to make recommendations in the context in being able to evaluate things relative to those recovery goals, specifically consider recovery goals—put in context of what the recovery goals are.

Lew asked if there are any other suggestions/discussions. So the ones he got are explicitly are: looking at spring vs fall designs; recruitment trend detection; overall robustness of trend estimates; and kind of a relative comparison of trend estimators, adult abundance estimators, recruitment estimators, in the context of the recovery goals

Lew asked if anyone would like to discuss anything else about their studies.

Rich asked Dave how they are going to do this-- You are going to start cranking away on this? He asked if we will see anything before July or are there scheduled periodic conference calls or what?

Dave said he would defer to GCMRC folks. He and Lew had discussed getting together as soon as school is out and he gets loosened up a bit, to discuss some nitty gritty details and start plunking away. He defers to Lew.

Lew said it was to be determined, but he had in mind that at this point, with the exception of him and Dave getting together early next month, they would turn Dave lose and see what he came up with for the AMWG meeting as far as a progress report. That is what he had in mind.

Rich asked if a conference all at the end of June or first of July after Dave had a chance to look at some of this would be appropriate as kind of to touch base on how you are doing and if you have specific questions perhaps we can assist at time.

Dave said that was fine with him. Maybe we can agree to that but leave it a little bit lose, maybe for a 2-3 weeks period, sort of if we are going to finish something up at a certain time. Maybe revisit that in a month or two, certainly before the AMWG meeting.

Lew said we are going to have our annual stock assessment meeting the later part of July, we could also get some other involvement in conference call and you could even consider coming out here then, but if nothing else, we would have Carl here as well as Steve Martell and our other fish cooperators. Stock assessment will be the topic of the week. We will all have our pencils about as sharp as they get right during that week.

Rich suggested however we accommodate that, we have the opportunity to meet. This has been very helpful for him and however, that is done, on a conference call or physically at the Stock assessment workshop, however that works.

Lew said we would make it happen, we will figure out the right time over the next month and keep everyone informed.

Rich said at that point in time we will hopefully be able to bring the Region 6 folks in from the service and apprise them of what is going on.

Lew said that would be great. He will send a meeting summary to each of the participants and rely on Mark to forward the summary to region 6 staff.

Mark agreed.

Rich asked Mark to forward that at least to Bob Muth and Tom Czapla.

Conference call ended.