FIFRA SCIENTIFIC ADVISORY PANEL (SAP)

OPEN MEETING

INTERPRETATION OF THE ECOLOGICAL SIGNIFICANCE OF ATRAZINE STREAMWATER CONCENTRATIONS USING A STATISTICALLY DESIGNED MONITORING PROGRAM

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MR. DOWNING: I'd like to call the meeting to order. And note that we've gotten a couple of handouts. One comes from Bill Effland, and it's a reference for you to consider. And, then, also, the other handout we just gave that has some photographs on it is an additional, supplemental information for you to consider from Syngenta. So, with that, I think we are about ready to begin. I'll turn it over to our chair, Dr. Heeringa.

DR. HEERINGA: Thank you ver much, Jim, and welcome back everyone to the third day of our meeting of the FIFRA Science Advisory Panel on the topic of the interpretation of the ecological significance of Atrazine stream water concentrations using a statistically designed monitoring program.

I think we have a very full schedule today. As I indicated at the end of yesterday's

portion of this meeting today. And the panel, itself, may be involved in a writing session to write up their review comments tomorrow morning.

But, we'll try to aim to finish the public meeting today, and just to, sort of, give everybody a sense of where I'd like to head, we'd like to use, at least, this morning to finish up the charge questions, in response to the presentations and the material that we've had provided to us on the study design for the monitoring study. And, then, to reserve the afternoon for the discussion of the final topic, including presentations and the two charge questions related to where do we go from here with all of this.

In the interest of time, I think everyone's had a chance to introduce themselves twice, so I'll pass up on that. Again, I don't think we need to do that, and I'll turn, right away, to Dr. Doyle or Dr. Irene to, sort of, open things up for the EPA.

DR. BRADY: This is Don Brady, and I thought it'd be appropriate in the interest of completing the one discussion where there was an open question to ask Dr. Erickson to define the question that he was going to provide an answer for later, and then, to do so, hopefully, quickly, so we don't disturb your agenda.

DR. HEERINGA: Yeah, and pardon me for getting your last name wrong.

DR. BRADY: That's okay.

DR. HEERINGA: I switched from Doyle to Brady. I don't know why I did that. Must be some old friend.

DR. BRADY: It's all the same to us.

DR. ERICKSON: Okay, thank you. This is Russell Erickson. There was a question yesterday about positioning of the LOC to equalize false positives and false negatives, and why that was done. And that I wasn't prepared to answer yesterday. But, a question noted that LOC's are, typically, based on more sensitive species, when based directly on species sensitivity, and not the median sensitivity of the species, and why was the, basically, the median approach used with mesocosm microcosms. Should first note that, even with species, it's not, necessarily, the most sensitive. And, also, for any one particular species, it's not the most sensitive test, uh, it's not the most sensitive test for that species, but rather, the median value for that species is, generally, used.

So, it's not, strictly, going to the sensitive end of it. And, so, the question is to whether to treat mesocosm microcosms like a sensitivity

distribution for species, or as a more analogous to the spread of results within a species. And, especially, given the fact that mesocosm microcosms already contain a spread of species sensitivities. Another factor in this is that, another way to state this is, the overlap that I showed on the mesocosm microcosms scores, how much does that reflect a difference in sensitivity of different systems, that we think is relevant to the field; and how much might represent variation or even test error, that we don't necessarily want to completely go to the low end for this, uh, low end of sensitivity.

And related to this, although, I would take some exceptions to Syngenta's analysis regarding the false positives and negatives, there, admittedly, should be some rate at which tests do produce false negatives statistically in any large collection of tests. And there should be some care in just, simply, identifying the most sensitive mesocosm. Another aspect of this is that the false positives and false negatives, also, include any modeling error.

And it was noted yesterday that the model results do not sort out exactly with the mesocosm microcosm test, even within a category. For example, the most sensitive category rated three with the model

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is not corresponding to the lowest rated three concentration of the mesocosm microcosm. And for these reasons, there was a decision, not made by me, but a decision made earlier to take the approach of equalizing the false positives and false negatives, though that was the rationale behind it. Although, I would just, then, finally, add that, you know, this is a decision that, you know, could be revisited, and subject to, you know, feedback from the panel.

DR. HEERINGA: Thank you very much, Dr. Erickson. Yes, Bob Gilliom.

DR. GILLIOM: I know you're trying to move on, but I asked that question, and I just want to make an observation, maybe, for the moment, and then leave it for the time being, is that, one of the things, in sifting through all of this, which effects almost everything we've talked about, is the sensitivity of the whole final decision making process you do to the decisions starting with the Brock scores, and then proceeding to the LOC, which you just described.

And then, how to relate the LOC, in terms of the Steinhardt deviation back to concentrations and all that. And part of the whole discussion's been about model error, model error or

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uncertainty or reliability in filling in the time domains, which was the main reason it was used to supplement the mesocosm results as a correlate.

So, the one thing I would say that seems relevant to that whole decision thing is, whether it's revisited or not, at some point, where that LOC should be with the mesocosm scores and the Brock scores, is it seems like, the mesocosm results that are, actually, at the time domain, that you have of interest, in terms of moving averages, should, in effect, trump model results. You know, in other words, you have empirical data at exactly the time domain that you want, and you have a collection of studies, why would you, then, instead of using the empirical data from the lab studies, you know, go to the model and fill in. So, it's a big issue, maybe, but I want to raise it, because it's something that's come up in discussions from time to time.

DR. HEERINGA: Sure.

DR. ERICKSON: Just to clarify that, I would say that the arguments I presented, really, weren't, necessarily, just pertaining to the model. It was arguments pertaining, if, even if we went directly to the mesocosm microcosm in that overlap region, and so, the, this was a, sort of, a pre-model decision to

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strike the median, even in that, based on the empirical data. And so, the, for, I mean, just to clarify that. This wasn't, strictly, the model trumping the results. It was a decision about the results themselves. Again, that could be argued, as far as the appropriateness.

DR. HEERINGA: Okay, at this point, thank you very much for that additional clarification and the discussion. I think that's helpful. Dr. Effland.

DR. EFFLAND: I wonder, will there be an opportunity to ask one or two clarifying questions before we go to the charge questions, or should we do that - -

DR. HEERINGA: I'm about to go to the charge questions, so if you have a clarifying question, please ask it.

DR. EFFLAND: Okay, I'd like to, I guess the question is addressed to Dr. Olsen. You showed a correlation plot between the HUC warp score and the sub-watershed warp score. And I'm curious how, it looks like a very strong positive correlation, but I'm curious how the calculation of the sub-watershed warp scores was conducted, I guess, is my first question?

DR. OLSEN: The sub warp scores, actually, were calculated by Syngenta. They, actually, did delineate the sub-watershed, and went through the

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same process, presumably, the same process they used with the HUC 10 to get the use rates down to the sub-watershed, and then apply the work model, the same way they did for the HUC 10. But, you know, if it, you know, they may, whether they'll need to clarify any more than that. It was done exactly the same process, though.

DR. THURMAN: This is Nelson Thurman. Is, from what we looked at, in doing a preliminary look at it, it looks like, basically, you end up with the same use data because you can't get that, you know, because of scale refinement. But, what you saw variation differences in some of the other site factors that result in those differences.

DR. EFFLAND: That helps, because the warp score is driven so much by the use component. I mean, those other, those other variables are a, fairly, small contribution to the overall prediction. And that helps that, basically, that was the, that's why that correlation is so strong. And I wonder, when you start zooming in to those scales, I think you're use data may be somewhat different from what it would be at HUC level.

DR. THURMAN: Stay tuned this afternoon. I think this is the type of discussion that will come

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up again this afternoon.
DR. HEERINGA: And I think that, thank you, Dr. Effland. Additional questions?
DR. EFFLAND: Just one more, and then we'll move on. The, if I understand right, when you calculate the use data, it was pounds of AI, or pounds of Atrazine. I don't know if it was AI, or whatever, but per area, and the area was the harvested acres. Why wasn't it the, because, Atrazine is, typically, applied at the beginning of the season, so I think it would be the planted acres, rather than the, I guess, I don't understand why harvested acres was the - -
DR. THURMAN: The explanation that Dr. Harbourt gave to me is that, when you take a look at the acreage, and started planted acres, because if you have frost damage or something, you can end up replanting the acres, so those would still, you know, so you could end up with some acres being double counted. But the harvested acres are, if, you know, just the field. I mean, and to be honest with you, I think, you may have, it may have had some subtle changes, but I'm not sure how much of a difference you would really see in most cases there.
DR. EFFLAND: Okay, 'cause that leads me to question, again, that use number, because of the,

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just because you're planting corn doesn't, necessarily, mean you're going to apply Atrazine. You know, it depends on conditions. Four dollar a bushel corn, you know, you're going to use as many inputs as you need, but if the price is low, maybe you'll, you know, I know a few producers that don't, necessarily, put nitrogen on every year, even though they know that nitrogen boosts their yields, just to try to save money. So, I just want to make that clarification.
DR. HEERINGA: Is that all, now, okay. Let's, as a group, then, if there are other items that require clarification as we go along, I think we've covered a number of them, but certainly, they can be brought out. But, let's proceed with charge question number six, and I'll ask Dr. Brady to read that into the record.
DR. BRADY: Thank you.
DR. HEERINGA: I should remember that. I mean, we've got a distinguished graduate of the University of Michigan who plays football with the same last name.
DR. HEERINGA: I don't think we ever had a quarterback named Doyle, and so, I don't know why. That's fine - - Plus I'm sort of what?
DR. BRADY: Let's go to the charge

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question.
DR. HEERINGA: The reason he's sitting there is he's from Ohio, you know.
DR. BRADY: I knew that would start a- -
DR. HEERINGA: Grind us out till you smell it.
DR. BRADY: Charge question six, this is Don Brady. The monitoring program used a tool, warp, designed to assess the vulnerability of watersheds in stream segments to, one, identify watersheds within the corn sorghum growing region that are likely to be most vulnerable to Atrazine exposure; and two, select sampling sites within the watersheds that are likely to be more susceptible to Atrazine runoff.
Please comment on the use of warp predictions for hydrologic units, HUC's ten and eleven, to restrict the survey design to those HUC's in the upper 20th percentile. And then, one, to stratify by warp predictions between 80th and the 95th percentiles, and above the 95th percentile; and two, to select HUC's with probability proportional to higher Atrazine use rates. Comment on the use of survey design population estimation approach for estimating the number and percent of HUC's that may have LOC exceedances.

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DR. HEERINGA: Thank you very much. Our lead discussant is Dr. LaPoint, and will supplement his discussion, the associate discussant, with contributions from some of the statisticians, too. But, Dr. LaPoint, if you would be willing to take the lead here.
DR. LAPOINT: Thank you very much. This is Tom LaPoint. Yeah, I'll look for input from my colleagues on this, because, in answering the question, and doing a lot of work reading the background, and listening to the discussions tomorrow, from the white paper, several criteria were used to make the final sub-watershed monitoring location decisions, and the justification for the use of the criteria appear sound to me.
These are all listed in the white paper, but basically, the consideration of drainage areas to be no more than half the HUC unit size, unless that was 50 square miles, and so forth, and so on. I won't spend time here going through the criteria, because everyone's read them, and they were in there. But, the emphasis, almost independently of the size, the emphasis on the more susceptible HUC's is appropriate to me, and focusing on the ones that were in the 20th, upper 20th percentile. And the reason for that is

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that, in a survey, no matter what size or, ultimately, the discussions about the nature of the size of the watersheds and the use thereof, but if the focus is on the ones that are expected to be a problem, and little or no problem shows up here, then the rest of the sites will, very likely be of a smaller magnitude of problem, and very likely be okay.

As to the further stratification above the 95th percentile, what I can say to this that, hopefully, has some value, stratification's always good, especially in a first survey. Because, what it does is, it provides information on the upper bounds of Atrazine concentrations, the expected hot spots and consistent problem areas, perhaps. For future monitoring plans, however, the plan to link warp to the newer GIS-based stream segments may lessen the need to stratify the uppermost.

That is to say, the above 95 percent of situations where high Atrazine concentrations exist. The reason I say that is, it would be better, and my presumption here, and we talked a little about this yesterday in the discussions, is, depending upon the resources available, and the, also, how far and how long the monitoring program goes on, the selection of further sites, if, and as, needed, could be taken off

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the upper portions of the cumulative distribution of sites ranked by the CASM scores in the slide we saw yesterday presented by Dr. Olsen, because I don't think, at this point, given the information that's already existing, about the distributions of Atrazine in some of these, and the nature of those more at-risk or mor vulnerable watersheds due to Atrazine exposure, I don't think the focus has to be on the, necessarily, the upper 5 percent of that, or the 95th percent and beyond.

So, that would be my response to the first part of that. And, in terms of representation, how representative these was, I was very impressed by the similarities presented in the Sielken and Valdez-Flores report, which is part of the readings that we have, or we've had to go through in matching the distribution of the twenty sample warp scores from the 80th to the 95th percentile of the 5800 sites closely matching the population distribution of the warp scores from the 80, 95th of the, in the 95th percentile. They, pretty much, closely match that population distribution of warp scores, and that, to me, on that reading, again, given my lack of absolute background in this, seemed appropriate, seemed good, it seemed logical. Now, if another, in terms of the

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characterizing the, kind of the distribution of these sites, and to the sense of how vulnerable they are, and how often one might expect to see the Atrazine concentrations in here, perhaps, because they have a good set of additional data, the 34 characteristics that were mentioned in the Sielken Valdez report, seemed like a canonical correlation, or some independent assessment of the geochemical variables, and how they respond, how they influence each other that, say, how they correlate with each other is really the correct thing here, to correlate with the Atrazine concentrations, might be an independent way of analyzing some of these to see how those characteristics influence Atrazine runoff, not runoff, but concentrations.

On part two, the survey design population estimates were estimating a number percentage of HUC's that have LOC exceedances. That seems to be a discussion in progress. And my best answer is that, what we've talked about yesterday, and what you had mentioned, Dr. Thurman, is that, you know, it seems here that to best describe how it, to better determine that, I like the exceedance distribution that Dr. Olsen presented yesterday. And to go any farther with that, to be able to do that, it seems like it's

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going to have to be an updated Atrazine with a GIS-based approach to be able to better identify stream catchment characteristics and land use. And that means, whether we like it or not, is more data.

Otherwise, we're, the answer I came up with is the data now represent the best available, and would have to be used. But it does seem, from the discussions yesterday, that there's a lot more information coming down the line on this, in terms of better, not better use, but GIS mapping that can help explain some of that. So, it's kind of a weak answer. Like I said, I'm hoping that with some expertise from the others on the panel, we can fill this in, be more help to EPA, so that's my comments.

DR. HEERINGA: Thank you, Dr. LaPoint. Our second in associate discussant is Bill Effland.

DR. EFFLAND: Bill Effland here, and I would agree with what Dr. LaPoint has just discussed. And I would also say that, statistically, I'm probably more dangerous than, actually, helpful, but I do believe in talking with statisticians very early in the design of something, so I think you're, I think that's a good point. But I will make a couple of comments.

One, I think that the availability, the current availability of what, I believe, is more

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accurate and precise GIS data is something that, and Tom mentioned that, something that should be considered, as far as looking at that data, in light of what you had a few years ago. Because, you do want to use the best available data.

And, again, there's still going to be some questions about the compatibility of that. And I just want to bring that up, because, I just want to point out. I just handed out one page, this is an article by Gotway and Young, in 2002, talking about combining incompatible spatial data. And it's really just a overview of various issues and problems related to doing that. And, so, I bring this up as a point of reference, that, I think, and working with Tony and other folks, I think it's a valuable thing to consider, as far as how to put some of those pieces of data together. And this paper discusses some very explicit examples of that. So, I think that's something to look at.

The other point that I'd make is that, the work model, in my opinion, in looking at the equation, the regression equation is primarily driven by that use data. And we've already had a little bit of discussion about that, but I think that that use data is, if you're going to use the work scores, the

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use data, having the most accurate use data that you can is critical to being able to apply that model. And so, I would, you know, caution that if, if you have uncertainties, if there's a lot of uncertainty about your use data, then your work scores, and, certainly, related to your work scores is going to go up quite a bit.

And then, I really can't comment on the population estimation component of this question that Dr. Olsen went over yesterday. I'd hope that some of the statisticians on the panel could help with that, 'cause I, although, I work with the National Resources Inventory, understanding how the statistics and all of that works is still, I still have trouble grappling with some of those things. So, I'll defer to my colleagues for that.

DR. HEERINGA: Our next associate discussant is Jim Fairchild.

DR. FAIRCHILD: Yes, this is Fairchild. And, from the data provided, it looks like that the warp model effectively reduced the, approximately, 6,000 HUC's down to the pool of, approximately, 1100 units, exhibiting the 80th percentile of warp scores. And the document indicated to me that there were parallel comparisons to other approaches for

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identification of some of the highly vulnerable areas Atrazine exposures, including the Minnesota Erodibility Index data from NRCS soil ratings, some flow up accumulation models from the NLCD, and actual surface water datasets for Atrazine. And all of these showed that, seemed to support the predictions of warp that, indeed, they were selecting the most vulnerable watersheds. So, there is a high level of redundancy in potential approaches.

The decision to stratify between the 80 and 95th and greater than 95th percentile warp scores appears to me to be based on the best professional judgement. But this is made in order to ensure that an adequate number of highly, vulnerable sites were selected for the model, given the amount of resources that are being put into this study.

In regards to the survey design, again, I'm not a statistician, but the documents presented indicate that a survey design method across the two vulnerability strata used appropriate weighting procedures to ensure that the design was spatially balanced and not unduly biased. The statistical methods that have been used, have been peer reviewed, and subject of a three peer review publication, and has been in use by EPA and other entities for over five

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years, but not being a statistician, I really can't comment further on the appropriateness of the survey design method.

DR. HEERINGA: Thank you very much, and our last associate discussant is Dr. Novak.

DR. NOVAK: This is Jeff Novak. Over the past two days, you've probably heard from some of my questions that I'm not a modeler. I ask basic questions. I challenge people with their assumptions, but, yeah, I, also, try to acquire knowledge, but I'm also trying to understand their frame of reference, and where they're coming from. Unfortunately, like my other three colleagues here, I am not a statistician. I do not have the knowledge base nor the wisdom to impart Mr. Olsen with any earth-shattering or lightning new components for his research. However, one of the questions is very dear to my heart, because I have done pesticide monitoring work. And I have made assumptions about the studies before I start. And the one question asks for, to select HUC's with probability proportional to higher Atrazine use rates. Well, what I'd like to do is, I'd like to impart some wisdom to the group here by reading my opinion on the answer to that. It'll take a few minutes, here, but Mr. Olsen, or Dr. Olsen, I'm sorry, I can't give you any more wisdom for your

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design, but I hope to leave you with a little bit of a warning here.

Selection of HUC's with probability proportional to higher Atrazine use rates is a simple first step to label HUC's that may have surface water Atrazine issues. This approach sounds appropriate on a large, national scale. This will allow the utility of other web-based data pools to be merged or overlaid onto watershed vulnerability maps. This is where I was saying that, I understand from your perspectives, that if you're working at your desk and working on a large, multi-state basis, you need information that would be available on the GIS data. Ground-truthing is very hard in this situation. However, on a watershed scale, selections simply based on Atrazine use per watershed area, agricultural land use area, or as we're learning today, it might even be on the harvestable acreage. And I, again, I still don't believe the USGS explanation for that, that was provided last night. But I think it would lead to spurious conclusions selecting watersheds based, simply, on Atrazine use.

Now, let me tell you why I can make that conclusion. I was involved in a pesticide monitoring study on a coastal plain watershed that had an extensive history of agricultural production,

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everything from making Mount Olive cucumbers to tomatoes to corn and soybeans. The area receives approximately 45 inches of rainfall per year. It's loaded with massive sandy soils that have low soil carbon contents and low herbicide Kd values. You may consider that these conditions are extremely conducive to offsite pesticide movement. So, you're going into the study with some pre-conceived notion that, boom, there's a lot of pesticide use, not only a lot of pesticide use, but a multi-varied pesticide use. And the soil conditions slope K value, I guess, as you work with, are telling you red flags, there may be runoff here.

Okay, it was our objective to determine if pesticides were occurring in water sources, but also determine if the farmers were applying best management practices to minimize the movement into the streams. When the study commenced, we made a few assumptions, based upon this information. We ended up gathering thirteen grab sa-, or excuse me, fourteen stream locations. We monitored them for two years on a weekly basis. We ran over 2200 water samples, first screening with immuno-assay and extracted approximately 220 positive detects.

After two years of monitoring, GC

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analysis revealed that a minimal amount, four percent, had the parent pesticide. That's important. We did measure the degradates, but we knew they weren't important. It was the parent that we were concerned with. We, also, by comparing to the literature, found less than one percent of those parent detects had anything over the how. So, basically, the water was in pretty good shape.

We concluded from this study that the surface waters did not contain massive amounts of pesticides. Farmer applicators were successfully using BMP to minimize offsite movement. So, ultimately, we were wrong in our assumption of expecting massive amount of pesticides, based upon making prior assumptions. So, again, not to belabor this point, but a person needs to be careful when they're selecting or pre-selection going into a study. The question that I would like to leave the EPA panel here to slug though is, can your initial assumptions about higher pesticide usage also, then, lead you to spurious conclusions. Thank you.

DR. HEERINGA: Thank you very much, Dr. Novak. At this point, I'd like to open it up to other members of the panel, and I'd like to go to Dr. Young first and then Dr. Ellsworth.

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DR. YOUNG: First of all, I'd like to congratulate EPA. I've been on several review and advisory panels for EPA, and, honestly, a few years ago, you'd never see a probabilistic sampling in water. So, the fact you tried to target toward the most vulnerable, and have probabilistic sampling, and all that gives you is terrific. Congratulations, guys. So, I think this is really a move forward, because there's some problems with that, or some, not problems, some challenges with it, but it's a move in the right direction. Now, I didn't really have this in my prepared remarks, but on this last deal, the weighting toward higher Atrazine use in the context of probabilistic sampling, if that's not the right thing to do, you still can get unbiased estimates of these proportions. It may not be as efficient as what you'd like, but you, it does it no harm no foul, as long as you're doing probabilistic sampling. That's one of the great, the great benefits. Now, Steve, if I'm wrong on that, you correct me. That's right, right? Okay. Okay, I'm just, I, you know, so, I mean - -

DR. HEERINGA: I dare not.

DR. YOUNG: That's one of the things, ha. Well, I mean, that's, yeah, I mean, and that, that's what you get when you use statistics properly. So, way

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to go guys, way to go in thinking about the design. So, that's, I, too, wonder whether you really should weight additionally, do the weighting by Atrazine use. Especially, after you've already done the strata, and whether you really are getting anything more, given all of the uncertainties associated with that. But, you know, given that you're doing the probabilistic sampling, you're kind of saving yourself, even if you've messed up in that regard, okay.

So, that part, I feel pretty good about. The problem I see is, kind of, reflected in that second comment, or the challenge. It's not a problem as much as a challenge for estimating the number and percent of HUC's that may have LOC exceedances. That's not really what you're trying to estimate here. It's not what you did estimate. You're estimating the percent of HUC's in those strata that have at least one-, you know, that have some kind of LOC exceedance.

So, it's not of all the HUC's, not of all the 9500, but of only this sub group. And so, to be able, when you're targeting your sampling, to be able to generalize to the proper population, and keep everybody clear on exactly what you're generalizing to, is really important. And it, and I mentioned that yesterday when we looked at that distribution function.

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In fact, I'm not sure that that distribution function, if you put out for common consumption, is going to be interpreted correctly.

And, so, I think, some creative approaches to developing the statistics that will accurately display the picture that you've developed, because the percent say 9 percent, but it's 9 percent of the 11,000; not of the-, 1100, not of the whole group. And so, it's a lot better picture than the 9 percent would show. And so, how to do that in the correct way is really important. So, I just, I, overall, I'm very happy. It is a complex sampling design. Fortunately, I, it's not my first time I've seen it, so I've had some time to kind of catch up, and you know, I know, kind of, how it works. It's innovative, and yet, it still brings us, it allows for that geographical spread, and at the same time, gives us a probabilistic base upon which we can generalize to the population. And so, I feel really good about it. And I commend you for using it. Thank you.

DR. HEERINGA: Thank you, Dr. Young. Dr. Ellsworth.

DR. ELLSWORTH: Yeah, just, Dr. Ellsworth, Tim Ellsworth, Doctor, whatever. The point is, not used to saying that. I have a couple comments

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on this that I wanted to make, too. First of all, it seems like there's an issue that keeps coming up from Bill and others here, the idea of scale. And the total area, it seems like, that you would classify as potentially, or stream length that you would classify as exceeding an LOC.

That total area would increase as you got finer spatial resolution. Now, if you called whole United S-, or the Mississippi Basin one watershed, one, and looked at a HUC that scale, you'd say everything's fine. As you get finer and finer resolution, you're going to classify more and more area as, perhaps, exceeding an LOC. And the other thing, so, for step one, I would, the first part of the question up here, I would agree with you.

I like what you did. I think you did the best you could. You had warp. You used that to help identify the most vulnerable, everyone else has said that. The second part, I think, is very problematic to me. And I think that was pointed out by Tony yesterday. There's not a good correlation between SSI and the warp value. You're restricted to this very small set range of factors. So, if you wanted to try to develop a regression relationship between physical factors and SSI, because of the study design, you're

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really limited in doing that. And, uh, so, you know, the best way now, with the new data, would be to try to use the smallest spatial scale where we have good resolution, and develop regression between your index to go on, anyway, that's all I have. That's all.

DR. HEERINGA: Dr. Randolph.

DR. RANDOLPH: Yes. I'd like to follow up on that. I continue to be very concerned about the land cover land use data. As I understand it, the data that were used were from the 1992 LULC database. Those, that database was developed from imagery from the mid- to late 1980's, which means the information is now twenty to twenty-five years old. I realize that in, say, central Illinois, in high productivity agricultural areas, that once a cornfield, it's, probably, always a cornfield, or at least, in tillage. However, other parts, other watersheds in the Midwest, and I'm from Indiana, so I'm familiar with southern Indiana, southern Illinois, parts of southern Ohio. In those less productive areas, land use change is very common.

We get agricultural abandonment. We get shifts between tillage and non-tillage or low tillage. We get shifts between tillage and pasture. And, of course, in all agricultural areas, we know that

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there's, generally, a trend in land use change from agriculture to residential suburban and other kinds of, including some commercial uses.

So, my question is, given the level of detail and the attention that's been given to other parts of this study, why use twenty-five year old land use data. I mean, it just doesn't make any sense. The follow up question is, given that you've got forty watersheds, it strikes me as entirely feasible to secure the land site TM or ATM data from 19-, uh, 2004, 2005 actually do supervised classifications for each of the watersheds, develop your own accurate up-to-date land cover maps, and use that information in warp, in the analysis, in looking at the actual sample sites within the watershed. There's a lot better way to do this.

DR. HEERINGA: Dr. Randolph, in that latter, for the forty sites, you're suggesting, possibly, updating that information, repeating the warp analysis, and looking at how that would correspond to the original sites?

DR. RANDOLPH: I'm a little reluctant to suggest that - -

DR. HEERINGA: Okay.

DR. RANDOLPH: Because that's a huge

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amount of work. And, I'm just saying, maybe a way to do it would be, pick a water-, Indiana eleven comes to mind. Pick a watershed where there's some question. Perhaps, go back and do a supervised classification. Compare that current land use information with the '92 database. I think there's a way to, perhaps, examine the validity of the data, or in some cases, if it's really me-, if it's really off, at least to know that. And that would give you, I think, an indication of just how serious, or, perhaps, not serious as a site. Parts of the Midwest, I don't think land use changes all that rapidly. But I know, for a fact, that in parts it does.

DR. HEERINGA: Dr. Young and then Dr. Portier.

DR. YOUNG: I think that there are two different things going on here. And I think it's important to keep them separate. One is, the sampling design that allows you to estimate the proportion of watersheds that have, at, some occurrence of an LOC exceedance. And that's what this was done. And then, the natural ques-, follow up, and you can do everything like this.

And I visited with Steve at lunch the other day, just to kind of go over this, because his

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area is sampling. And, you can make some misclassifications. You can do, and you do lose some efficiency, but you still can get out those estimates of those proportions. Now, but then, the question comes up, the naturally, is, well, there are some exceedances, so what are the characteristics, and how do those relate. And then you begin the modeling phase. And, and this wasn't, really, designed for a modeling phase, which is, what I brought up at the end of the day yesterday.

The question's now changing. And when the question changes, this question of changes support or scale become important. The how to do regressions properly becomes important. Whether we have the right land use cover becomes important. But that's, kind of, to, in my view, the next step. And, so, it's a, it's, kind of, trying to answer the next question, as opposed to the purp-, does that seem, and that's all I'm try-, I would encourage our panel to, kind of, keep straight is, is whether we're just doing a samp-, uh, a design phased estimate of proportion of wha-, something, that something needs to be clearly defined. Or, are we trying to model relationships for which these data are, really, not set up to do, and they're, are all of these issues do have to be answered before we can go to that

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next step.

DR. HEERINGA: Thank you, Dr. Young. Dr. Portier, then Dr. Randolph.

DR. PORTIER: Actually, Dr. Young just said everything I was going to say. So, I'll just leave my notes here, and - -

DR. HEERINGA: Dr. Randolph, please.

DR. RANDOLPH: Let me just respond, I completely agree. I think there's, sort of, the na-, as you say, there's the natural progression, and perhaps, in discussing question six, I think several of us have then moved, at least in our minds, to question seven. So, I, also, agree with the comment that maybe it's a step ahead.

DR. HEERINGA: As we approach the end of this, you know, there will be some convergence of some questions here. And I think some of these issues are coming forward. That's fine, because we can have that as background as we move on to ten and eleven. Just a, Steve Heeringa, here. I'll ask just a few questions.

Dr. Young is absolutely right in her assessment. One of the things that, as I read through all of this material, in trying to, this statistic, the proportion of watersheds, however we define them, stream segments, sub HUC's, HUC's that exceed the LOC.

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That's really, sort of, a one-time statistic. And then we move on to, sort of, looking at, you know, how do we identify. So, it sort of sets a threshold. So, in the end, that's really not the primary target of this program. Ultimately, you know, it's an intermediate statistic. We'd look at it. I mean, clearly, if it were 50 percent, it would raise a different concern than, you know, an issue of 11 percent or 9 percent. Again, as Dr. Young, 11 percent of, maybe, 20 percent. So, I think it's probably well within the realm that all of us would anticipate. And so, I think that it's valuable to focus on it, and Dr. Young's right. I mean, the design is good to provide, at least, an unbiased, or a nearly unbiased estimate of that. And Dr. Olsen has worked out with his methodology variance estimates, and we saw fairly large confidence intervals. The, I think the critical thing here, too, is to think about what is your population of inference. And, you know, just in the strictest terms. We really are sampling stream segments. So, the inference is to stream segments that would have been eligible here. And, again, we have to extrapolate, does a stream segment, is that sufficiently representative of the sub HUC or the HUC itself. I think those are issues we can deal with. But, we just have to be cognizant of that.

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Because, you know, we, sort of, talked about this as an estimate for HUC's, but it's really an estimate for bridges within stream segments. And, so, technically, and that's fine, and we have to realize that, and, but I think, as we think about it, we have to realize, is there anything about bridges within stream segments that wouldn't, necessarily, and we all, I think, particular the ecologists here, would rec-, you know, immediately, go, there are many factors, but this is the way we have to do it.

So, with regard to additional comments on question number six, the, I think the stratification based on the warp, we had an initial problem, and you set out a fairly systematic and informative design. I mean, the design was scientifically informative, and that is, we didn't so over-skew this design that it would only answer one question or one set of assumptions. It provided a broad base of data, somewhat restricted. We only looked at the upper 20 percentile of the warp scores. But, nevertheless, I think it's representative and systematically so. And, in a way that can inform the next step. And I think that's what's important here. I'm not sure, you know, in hindsight, whether I would have added the additional probability proportionate to size within the strata. I

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think Dr. Olsen, looking at this data, might have revisited that decision, too. But, initially, you know, you're thinking about a model in which the warp score is proportional to the impact. If you think that variance of the LOC is proportional to the SSI, then this would be an optimal design. It doesn't appear to quite hold that way, but it, as Dr. Young points out, it's not optimal, but it's appropriate, and leads to correct inferences when we incorporate standard error. So, I guess, that's comments that I have on that particular question. Do, any other comments? Can I turn to Dr. Olsen, Thurman, Corbin?

DR. OLSEN: This is Tony Olsen. I, first off, I appreciate everybody's comments. In terms of the statistical part of it, I agree with what's been said. As always, you know, once you get done with the study, if you go back and you look at it, you might actually do it slightly differently. But you know more, so it's not a fair comparison. But, I really appreciate that. I think it was Linda that made the comment about the inferences, well, then, actually, Steve did it, too, making inference to the right po-, the correct population. And our inferences that we, explicitly, did were only to the 1172. And it is very clear, 'cause we've had this happen in other cases,

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that somebody will end up taking that, and they take it out of the context. And they're going to interpret that with respect to the United States. And so, you, that's something that's very clear, and it's extremely difficult to, actually, label the figure or an answer, and not have them do that. So, that's something that we do have to pay attention to.

On, in terms of the statistics, I guess, I'd, sort of, I'll leave it there. The other, 'cause I think a lot of the other comments have to do with, sort of, the prediction problem, and we're going to come up with that later.

One comment, I guess, about the National Land Cover Dataset, of, that this is, the National, the NLCD for 1992, since I was involved in that, or that data in, coming involved because of a program I've been involved with. The imagery, I think, is from '90, '92. It's not, it is from that time period. They didn't get the, they didn't get the entire classification done until late 1900's, or late, you know, like 1999. It took them a long time to do it. So, it is, it is that 1992, but it still is ten to fifteen years out of date. So, but at the time, it was the only national one that was available.

I guess one comment, when you talk, and

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this sort of goes down the other path. When we're talking about doing, sort of, a classification for individual sub-watersheds, is there any particular reason why one wouldn't want to look at aerial photos and, as an option?

DR. HEERINGA: Dr. Randolph, if you could use your mike.

DR. RANDOLPH: J. C. Randolph, sorry. Sure, you could. I just think TM imagery would be easier, and you have the advantage that you could develop three or four or more classes in a supervised classification, and then apply it. It's a more effective approach, I think.

DR. OLSEN: Thank you.

DR. HEERINGA: All set? Thanks.

DR. PORTIER: What kind of imagery were you asking, he was talking about land set TM imagery, and you were talking about, I'm sorry, you were talking about what kind of imagery? I'm just getting my notes down, so.

DR. OLSEN: Oh, just, as opposed to using imagery, using aerial photos.

DR. HEERINGA: Thank you very much. Yeah, Bob Gilliom.

DR. GILLIOM: Bob Gilliom. One, just one

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final comment. I agree totally on the background discussion that happened on statistics from Dr. Young and so forth. And just want to note that in refining the statement of what's being the outcome of the survey, that the scales are really a critical issue to keep people square on. We talk about HUC's, and I, every time we slip into the discussion, I, you know, I hear the HUC's, when it's really these small sub-watersheds that were sampled. And everything's got to be referenced back to what that tells us at that scale. 'Cause every part of this, including the concentration duration curves are scale dependent.

DR. HEERINGA: Dr. Grue.

DR. GRUE: Just a quick comment on interpretation. The statement, and I realize why you're using it, but at least, the exceedance in at least one sub-watershed, that's just problematic. And I don't know the, what, how to address that, but if that were to be viewed by folks outside of this discussion, you could interpret it a couple of ways. And one interpretation would be that, in some situations, there were exceedances in more than one sub-watershed. And, I don't have the answer to that, but I think it's something that the Agency needs to think about.

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DR. HEERINGA: I think those are important points, in terms of public presentation and consumption of this data. Because I think the people sitting around this table, after two and a half days, have a little better understanding of what this is, but, you know, the two or three word, two or three sentence paragraph, that presents this in any other format, so be carefully considered to make sure the, survey population and the statements about inference are coincident.

Okay, if there are no other comments on this, again, I think we'll have a chance to revisit some of these issues. But, Dr. Brady or maybe Mark or Nelson, if you would want to read this into the record, question number seven?

DR. BRADY: This is Don Brady. I can read it in. Question number seven, once the vulnerable HUC 10 and 11 watersheds were selected for monitoring, specific monitoring sites were selected within each watershed, using criteria that were designed to maximize the potential for selecting the streams most vulnerable to Atrazine exposure. However, with only a single point monitored per watershed, estimates of within HUC variability for detections of Atrazine could not be calculated. The resulting population estimates

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reflect variability across watersheds, but not within the monitored watersheds. Please comment on this approach, and identify and discuss any alternative approaches to extend the results of the monitoring sites.

DR. HEERINGA: Our lead discussant on this question is Jim Fairchild.

DR. FAIRCHILD: Yes, Jim Fairchild. Well, the study team arrived at the decision to select a single sampling site within each HUC, based, primarily, on logistical and cost reasons. But a standard protocol was used to locate the actual sample site at the lowest end of the selected stream segment, even though, in some case, it was adjusted, based on the decision tree. In this case, this minimizes bias across sampling units, and assures that the location will include the resulting Atrazine inputs from all possible locations from tributaries located upstream from the sampling point. This is prudent, since it samples the largest drainage area possible, and minimizes the uncertainty associated with sudden changes in cropping patterns or crop rotations. This ensures that the main objective, which was to identify the watersheds that are most vulnerable, is accomplished. But it is a given, as stated in the

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question, that you have data from only one location. However, the data that you do have can be implied in many ways. Because, you already have data regarding the exposure duration pattern, in addition to discharge data and other factors within the warp model.

And to follow up on some of the previous discussion, the first thing that could be done as to further ground truth actually cropping patterns from aerial photography or satellite imaging that might be available. This would allow you to fine tune that dataset, in terms of actual exposure potential. And the quality of the digital elevation sets and remote sensing data is constantly improving. Therefore, post HUC modeling can be used by examining Atrazine concentrations from the watershed in a hydro-geomorphic context, in consideration of the slope, or the soil type, slope, timing, intensity of the rainfall could be used to model concentrations in the root zone, as well as the edge of field, has already been done in some of the presentations showing the combination of the basic warp stream concentration combined with some PRZM exercises. In examining the relationships between peak flow events and Atrazine concentrations, in relation to these other post HUC data, may allow exploratory data mining to place each HUC into a

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classification that can be examined using cluster analysis or principal components analysis techniques previously discussed. And, to some degree, this is a similar approach taking to place the Missouri sites into their own separate class. Once similar land classes are clustered or differentiated, then they could be compared to long-term study sites, such as the Heidelberg datasets, or any of the numerous loca-, datasets located at, for example, as USDA ARS stations located across the Midwest. For example, one of the sites in this study, the Young's Creek lies within the good water creek ARS watershed, where Dr. Lerch is doing some of his research. Many of the sites will lie near NAWQA sites, for example, the Wolf Creek, Iowa site lies within the East Iowa NAWQA basin. And I do know that they have study-specific studies specifically targeted in these different watersheds and data available. USGS gauging stations may also present opportunities, giving access to long-term data regarding rainfall stage and discharge relationships. So, in each of these instances, there is a wealth of data that have been collected that you could examine and compare to the site's specific data, where you only have one point in a specific sub-watershed. But, ultimately, you can use this to generate hypothesis

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from the observations you have at individual sub-watersheds. And use these observations to form hypotheses, leading to future collaborative studies on Atrazine, in addition to other substances of concern, such as nitrate. And I think that the high quality of the data that you have, and the level of interest, is going to create these collaborative opportunities to work with other entities, and try to cluster these specific watersheds into similar sites, and use those to make inferences based on other outstanding data.

DR. HEERINGA: Thank you very much, Jim. Our first associate discussant is Tim Ellsworth, Dr. Olsen.

DR. ELLSWORTH: Tim Ellsworth. Okay, the, I agree along the same lines with what Jim's saying here, the idea of getting better data, the corn acreage. I know from the satellite, you can identify that. And it seems to me like, perhaps, within local regions, crop protection regions, you might, actually, come up with a better estimate of what's the average Atrazine application on that land. Anyway, that seems doable to me. The 86 site years that you have, and then there's 10 more coming, I guess, it looks like this year, so you've got about 96 site years site years of data and all the associated environmental factors,

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like use rates, flow accumulation on corn acres, the Sirsco dataset that you're using now, with the increased estimate of, you know, restricted layers, conductivity, you've got a wealth of information that, kind of, at this, to come in now, with this SSI index that you're using to develop a regression for that. And I'm trying to take it to where you would go next. That could be used to develop this relationship between these variables in an SSI index similar to what was done with warp, and perhaps, let you move on to another level. But I think it's real important what Bob was just saying earlier, is that, it's really, at this scale, now, because of the design. It's no longer the HUC 10, 11. You're really constricted, now, to be working more at this spatial scale because of the design.

DR. HEERINGA: Our next discussant is Dr. Gay, Paige?

DR. GAY: Paige Gay, University of Georgia. I think, pretty much, all of my comments have been discussed during question six and seven. The use of the improved NHC plus data seems very appropriate to me, and along with that, perhaps, tweaking the criteria used to get the smaller selection definitely needs to be done. I, also, agree about PRZM estimates,

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estimating field of edge loadings could be quite beneficial in conjunction with this, if they could be tied together in some method. And that's about all I have different.

DR. HEERINGA: Thank you very much, Dr. Gay. And finally, associate discussant is Dr. LaPoint.

DR. HEERINGA: All right, thank you. Very simply, I concur with everything. It's a matter of scaled assessment. For regional, or among watershed risk assessment, it would be best to have some inter-watershed estimates, no question. But, given the reality of limited resources and the way this study was designed, I think the approach described more watersheds as valid and quite appropriate. It may be, as we've already said here, or heard here, I should say, that more within HUC variance could be accounted for by continued GIS models, linking them to either Atrazine or some of the stream characteristics. Thanks.

DR. HEERINGA: Additional comments, Dr. Young.

DR. YOUNG: Again, I think it all goes to the goal of sampling. If your goal is to estimate the proportion of watershed that has some, some occurrence of exceedance somewhere in that watershed, however, you

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want, however that needs to be said, then you have, you get your smallest standard airs, you get your most precision by looking at as many watersheds, and not sampling within, doing replicate samples within a watershed. Now, so, again, I think it's a matter of the question beginning to change a little bit, for the original purpose of estimating the proportion of watersheds, then, this was the correct approach to use. And you would not want to do it now. As the question changes, that may lead to a different design.

DR. HEERINGA: Dr. Lerch.

DR. LERCH: Bob Lerch. I just want to reiterate what Jim Fairchild was saying about, you know, ARS does have, at least in some of these watersheds, some data we would be happy to share. I don't want to preempt, but I will give a little bit of a preview of what that looks like in my response in question ten. But something that's related to this, for me, at least, is the ability to draw statistical inferences from sub-watershed to the bigger HUC 10. And it would seem to me that, given the statistical design that exists, you should be able to draw that inference. And I will, also, address that in my example, and show that I do believe you can use the segments that exceeded the LOC to infer something about

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the whole HUC 10.

DR. HEERINGA: Thank you, Dr. Lerch. Steve he-, oh. Dr. Randolph.

DR. RANDOLPH: This, I think your comments about the within watershed analysis are appropriate. I have a question, and I've just been thinking about this, and would, simply, like to ask a question, and hear from others. If there's an Atrazine application and a given rate, let's say two kilometers away from the sampling point, and you contrast that with the same Atrazine application a hundred meters away from the sampling point, same weather conditions, same cropping practices, same soils, all I'm saying is, let's extend the distance. How much influence does that have on the chemograph?

DR. HEERINGA: It's a problem, I think that's a question a lot of us would like to have the answer to.

DR. FAIRCHILD: Jim Fairchild. I think in that particular example, it's going to be dependent upon the hydrologic factors associated with that particular sub-watershed. And that, you know, if one has good flow discharge data, and, you know, reasonable estimates of the topography of that area, that you probably could, pretty, effectively, model the effect

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of distance and, as I said previously, I think that ARS probably has some of that data across their ARS laboratories, which are, basically, located probably in every state that's been sampled as part of this larger eleven state effort.

DR. HEERINGA: Dr. Grue.

DR. GRUE: Just to make a comment that, that, actually, was the basis, this discussion is, actually, the basis for my questions related to the position of sampling site within the system. And to what extent was EPA and Syngenta using position as an integrator for what might be occurring throughout the watershed that was being sampled. And it gets at the questions that were just based. If, in fact, the sampling sites were at the low end, and that's why I was asking, you know, maybe a plot of the distribution of position relative to selected stream length or length of the system within the HUC, that might provide us some basis as to, at least, making the assumption that, if it's, in fact, low in the system, then maybe that's an integration. It doesn't, necessarily, address the issue of what those peaks might be at individual sites along the system, but at least, using the low end of the system and integrator.

DR. HEERINGA: Steve Heeringa. I'd like

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to reinforce Dr. Young's comments, and those of others, too. I think, in this situation. You have resources to choose forty sampling sites, and your objective was to obtain the chemograph for an extended sampling period. Clearly, if we could expand sample sizes infinitely, you know, interesting questions like within HUC variability could be addressed. But, the purpose there would be to, simply, measure components of variance, sort of, between HUC or between segment within HUC. And those are interesting scientific questions, but for your objective at hand, at this point, that's secondary. And I think, we, also, have to be careful, too. People often recommend replication, but we need power to really, components of variance are very tricky, and require large, large sample sizes to measure accurately. So, simply, going through the mechanics of a design that tries to do that, and spends enormous resources on that replication, simply to get at that inter-HUC variability, it might be better, as many of the scientists have suggested is to, to use these direct measures in a very efficient forty sample size. I'm thinking about location, and that decision was right, to come back at the intra-HUC variability using modeling approaches that are, essentially, established through relationships that have, can now be

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fitted with these data, so. Turn to, Tony, Nelson, or Mark, is this clear, with regard to this particular, it's a pretty direct question, and I think that's some good and extended response on the one, give to them. Okay, Dr. Brady, if you would be willing to read question eight into the record, please.

DR. BRADY: Certainly, Don Brady, question eight. Three monitoring sites in Nebraska experienced low or no flow conditions that precluded sampling. While Hampton, et al, suggest that these sites with intermittent or low flow are already stressed by other factors, Meyer, et al, indicate that such aquatic communities are rich in diversity. The Agency has generated statistics for these three sites as a separate stratum. However, the meaning of these separate population estimates is uncertain. Please comment on whether the Agency should consider the low flow sites, and or intermittent streams, as part of the population estimates, or treat them separately. Please comment on whether the aquatic systems and exposure conditions of the existing microcosm and mesocosm studies adequately represent these low flow and or intermittent stream communities. If not, how would EPA determine an LOC for low flow conditions?

DR. HEERINGA: Our first discussant, lead

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discussant is Dr. Gay, Paige.

DR. GAY: Thank you, Paige Gay, University of Georgia. I would, first, like to acknowledge the effort it took to undergo this rigorous monitoring schedule, and commend EPA, Syngenta, and the contractors. The effort and cooperation needed to do this, and just the diligence of everyone to do this on a four-day regime is just, really, incredible. In my view, from being a field technician and laboratory analyst for a long time, it's just a really big undertaking, and they really did a very good job. In terms of the low flow and intermittent streams, in my world, in South Georgia, this is a common occurrence. I, certainly, think that the data represents an important part of this study. The interpolation of concentrations across the gaps for samples were not available, certainly, can produce grossly exaggerated values, particularly, when you have these very high peaks of concentration occurring, preceding a gap in the data. In addition, I would suspect, and based on studies of the intermittent streams that we've done, even in a four-day sampling regime, I'm not sure that you wouldn't miss some intermittent flow events that might curve due to rain driven effects. And that was a concern of mine as well. The flow measurements, I

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know, we got an additional piece of information regarding this this morning were a concern of mine. Because the auto sampler data, everything is so dependent on that, and it would have seemed to me that taking a flow measurement when the sites were visited, perhaps, using some kind of probe to give an initial idea of what the flow is anyway if you weren't able to capture that using your estimated regime here. And why these sites were not ranked among the lowest flow, and yet, had these gaps in data, is really not clear to me if it's the erroneous estimates of the flow, or if it's due to the Lamott sampler not being able to, actually, acquire a sample. And my thought was that, if you say, I believe, it was the Nebraska 5 site, if you had an ISCO sampler out there anyway, and typically, ours are in the streambeds, and you know, the nominal distance from the actual streambed itself, that if you have some flow, you should be able to capture some kind of composite sample. However, it seems that the automated samplers here were strictly used to collect rain-driven events, and could've been used on a more scheduled sampling regime in these low flow areas to try to capture some of this flow that, evidently, was lost, but seems to be there. We often do weekly composite samples and refrigerated ISCO's, and often capture some

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of that flow. The aquatic systems and exposure conditions of the existing microcosm and mesocosm studies would, seem to me, not to, adequately, represent these stream flow conditions. As we've discussed many times, and Syngenta has stressed, that these ecosystems are likely stressed already at the time of exposure. So, the reaction to recovery from exposure to the Atrazine concentration certainly would be altered, if the organisms were stressed or the fauna flora. And in the microcosm mesocosm studies in the handout, I'm not sure that I was able to discern anything that really addressed stressed systems. And to me, it would seem that the LOC wouldn't really apply here, because it would be probable, in my mind, that lower level of concern should be applied because they are already stressed. So, lower concentrations in durations would cause similar effects to the LOC of four. The flow measurements, again, I haven't had time to look at this. If you guys want to comment on that and the accuracy, I would appreciate anything that you might could add to that. I found that that was a huge gap in looking at these in trying to suggest a method for determining LOC when you have to have a 365 day interpolated dataset. I just don't see that that's feasible, based on, especially, the huge gaps in, I

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believe that was any five that really had the big gaps. You might be able to find a better method for interpolation for the other ones that had smaller gaps, but to me, I would like to see further sampling, perhaps, finding a way to better capture low flow, using the auto sampler, which can get closer to the streambed or some other method. In trying to look at the data before trying to address the LOC question, I just don't think they had, the true concentration duration profiles have been identified here.

DR. HEERINGA: Thank you, Dr. Gay. Our second associate discussant is Bill Effland.

DR. EFFLAND: Bill Effland here, and I guess I'm, it's surprising to me in looking at the flow data that was provided in the slides that some of these sites that dried up, actually, have continuous, some pretty high flow rates. And I think, Bob Lerch mentioned that yesterday, that it's, it seems counterintuitive that there seems to be quite a lot of flow yet, they still dried up, or the one that did dry up in, and I have a hard time reading it. The one in Nebraska, I think it was either, I think it's 04, that dried up early in the season. I guess, one of my initial reactions was, what else is going on in this area, in this sub-watershed. And, a couple of thoughts

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came to mind yesterday evening, and one was, to look at aerial photography and see what the kinds of practices are, or the kinds of production and crop management practices in that area, and see if there's water storage. I think Dr. Heeringa mentioned yesterday, maybe there's some water storage going on, or possibly, maybe irrigation. I don't know what the, it just seems counter-intuitive that you've got, you have flow in the hydrograph, but then it dries up. And so, I think, one other comment I'll make that, actually, I asked Dr. Harbourt about earlier is, I think it's really hard for you all to be able to interpret some of this information without ever visiting some of these sites. I'm not saying that you visit all forty of the sites, but, especially, the ones where you have questions about, my experience is, if you don't go to the field, and take a look at what's going on, and maybe do some, you know, on the ground truthing, some truly on the ground truthing, sometimes you miss some simple things that you wouldn't have. So, I guess, that would be one of my recommendations, is to consider it. In some of these areas where you have some questions, and it could even be something as modern, technologically as a virtual tour. There are ways to, you know, there are ways to do that if you want. But, at least, something

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so you get a better idea of what's going on in that particular sub-watershed. Because, looking at these things through reports, and even looking at photographs and things of the sites, you don't always get the, you don't have the same interpretation. So, I just make that one comment. And I always like to encourage my soil science colleagues to get out into the field again, 'cause it helps to bring them back to our roots. So, I make those comments.

DR. EFFLAND: I make those comments, and then, as far as the question about whether the existing microcosm mesocosm studies adequately represent a low flow, I just wrote, no. I don't think they do. And, this was last night while I was sitting at the dinner table at 9:30, so, you have to take it partly, is because of that time of the day. And, I guess, I also, in looking at, I'm also a fisherman, and by hobby, and looking at conditions where there is low flow or no flow, those systems are stressed.

I mean, they're under some stresses related, you know, the organisms under stress is related to things besides maybe inputs of contaminants. So, I guess, I'm somewhat questioning the reality of why you would treat a low-flow, a no-flow site exactly like you would ones that have, at least, some minimal flow. And

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so, I just, it seems, if you would try to explain that, it just, it seems hard for me to grasp it, so, just, those are my comments.

DR. HEERINGA: Dr. Ellsworth, I wonder, I want to get through the associate discussants, and then - -

(WHEREUPON, the speaker was speaking without a microphone.)

DR. HEERINGA: Oh, okay, if you could hold it, we'll come back to that, that would be great. Our next associate discussant is Dr. Lerch, Bob Lerch.

DR. LERCH: Bob Lerch. I'll just answer each of these in order, although, I do have a slightly different take on the relevance of the microcosm mesocosm studies. I think the answer to the first one is, they should be included in the larger population set if the typical planning window was included, in terms of flow regime and in some samples collected. And, I think, in some of the examples yesterday, for, at least, two of three, I think that was the case. And then, also, your own criteria of 25th to 75th percentile flows, if those are met, I don't see any good reason to throw them out. I think the only one that's problematic is Nebraska 4 and, yeah, something's going on there, because you don't get an event followed

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by an immediate return to base flow without some alteration going on in that system.

The, my tact on this, or my viewpoint on the relevance of micro and mesocosm studies here would be that, I think, at least, the statics tests are, actually, probably, pretty relevant. What we're talking about, between flow regimes are series of disconnected pools that are a whole lot like a static system. So, I think, that, actually, it's some of, not all of the studies that are, I think, there were 77 or whatever included, but those that were static, I think, are relevant. I'm not sure if I have any other comment about that.

And so, therefore, I think you could use, in most instances, the normal approach to determining a LOC, one issue that was brought up is, what if the last concent-, or the last sample collected is high concentration. Well, let's assume that during these low flow periods you're going to get a series of disconnected pools that, probably, have a similar concentration to that last sample. Because, if, by definition, there's no base flow coming in to dilute it, the only thing that's going on is evaporation, which would, actually, increase the concentration between flow. So, I think you can use your last sample

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to extrapolate forward. And, therefore, I think that, well, and then, I guess at the other extreme there, if your sample was below the limited detection, then you could extrapolate or interpolate with zero. So, I think that these could, and should, be included in the larger set, other than maybe the Nebraska 04 where there may be some human alteration going there that would justify throwing that side out.

DR. HEERINGA: Thank you, Dr. Lerch, and Dr. Novak, Jeff.

DR. NOVAK: Yes, this is Dr. Novak, again. I have answers to three questions here. I acquired some knowledge and wisdom earlier in my career by serving as a post doc at the Savannah River Ecology Lab. And in that time period, I had opportunities to speak to many stream water ecologists who talked to me about nutrient spiraling in streams, and how important that was for organisms. Basing some of my answers on that past experience and wisdom from those folks, I've come up with three answers to the questions that have been posed to the Scientific Advisory Panel. The first answer is that, yes, the EPA should treat Atrazine detection in low flow intermittent streams as a separate case, and not mix this data with their entire data pool, which is argumentative now. The ecosystem

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created under low flow conditions is not comparable to conditions in other stream ecosystems with flowing water. Low flow will cause nutrients to concentrate along with temperature increases. Flowing water disperses or shifts many of the chemical and physical life-sustaining variables needed by water critters. Additionally, the spiraling concept of nutrient flows in streams predicts that flowing water delivers processed food sources, and notice I emphasize process here, to critter available to them. If those processed food sources decline, stress will occur to those critters. The stressful conditions will propel shifts in the consumer producer populations, which was brought up over the last few days in some of the models, resulting in upheavals of this bio-mass carbon production, which, I believe, is one of the core measurements for the statisticians. These changes in environmental population characteristics will undoubtedly change bioenergetic parameters, which is one of the buzzwords I heard yesterday, thereby, making comparisons with previous macrocosm studies conducted under higher flow conditions as unrealistic. Retooling of bioenergy equations may be needed to reprocess them, retest them, and post-validate them.

The second question concerned about the

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Nebraska sites, it's good that the EPA has generated statistics for the sites in Nebraska, and treats the system in different strata. Perhaps, this should be a trend in your overall research design that intermittent flow streams will be examined. Heavens, we have plenty of dry streams and almost dry rivers in South Carolina right now. Florence, in particular, as of December, '07, is about 12 inches of rain short, when we normally have 45 inches of rain annually. In the big picture, it remains to be determined how will standing water systems be correctly studied using the CASM model, which is referenced to flowing water.

My last answer, here, I believe, the EPA has the data in hand, which was handed out to us yesterday. Not to belittle the fact, but I think those LOC values are obtainable from reviewing the data, but let me go back to one of the questions that I proposed about two days ago for the EPA panelists. Wouldn't this be a great opportunity to verify results from the CASM Atrazine model? Now, again, seeing Stephanie's face the other day, depending upon funds, I know that funds are hard to come by. The team could create a macrocosm with low flow to static flow, inoculate with critters, gather information on changes in their bioenergetic parameters, carry through the experiment

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at different exposure durations, which I heard for the last two days, was one of the entrenched tools that the modelers were using, and then examine changes in the population. So, I'm learning some of the words from them and trying to make suggestions to get validation. Again, this provides the opportunity to create some ground truthing data that should convince skeptical SAP panel members, that the model will provide trustable results. Thank you.

DR. HEERINGA: Thank you, Dr. Novak. Dr. Ellsworth, Tim Ellsworth.

DR. ELLSWORTH: Tim Ellsworth, I didn't mean to interrupt there. I was just thinking there was something that have saved some conversation. I had a discussion with Dr. Harbourt after this came up, and asked him, kind of, what was going on there, and there was, you know, maybe it'd be better just to, I don't know if that's appropriate to have him say something, but it's like what Bill was saying. Talk to the person that was out there, and he's kind of got some info on it.

DR. HEERINGA: Dr. Brady, is that okay with you?

DR. BRADY: Yes, I think that's fine.

DR. HEERINGA: Dr. Harbourt, would you be

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willing to come to the public mike here over Dr. Frankenberry and. Maybe, just for the record, restate the question that led to the conversation.

DR. ELLSWORTH: I just asked him why did it not have grab samples available, although it looked like there was flow in what was going on there.

DR. HEERINGA: Simple enough, thanks.

DR. HARBOURT: Sure, Chris Harbourt, here. The Nebraska, the three Nebraska sites, first of all, had a flow condition there that made it very difficult to measure flow, first of all. And, we provided EPA the fifteen minute stage and flow data that we calculated sometime in the summer to help them with their White Paper. We really provided that without the necessary clarification and background information to use it, perhaps, in the best fashion. Especially, at the three Nebraska sites, we saw flow hydrographs yesterday, oh, thank you, in that slide packet that was sent around this morning. I guess, I can just go through some of these with you, if you want to pull them out. It's probably the best way to go through this.

I'm on the first page, I'm just showing, again, some slides, the type of equipment we used, either pressure transducers or ultrasonic sensors. The

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bottom slide, two, is a description of how we calculated flow from the river depth. Dr. Heeringa, they want to put it up on the screen, so it'll just be a second here.

DR. HEERINGA: That would be fine. I'm still trying to, I have it, and I've lost it, so. I don't think I've got it here. These slides here, Dr. Harbourt, are illustrative in general, not of the Nebraska sites, isn't that what you're saying?

DR. HARBOURT: Correct, yes, and it talked a little bit about the Nebraska sites.

DR. HEERINGA: While we're waiting, I mean, if, Nebraska, if you know Platte River Basin, and, often, very wide channels with very, very shallow flows. I mean, literally, in a matter of an inch of water, across Grand Island, it's 100 meters wide and an inch of water flowing, now, is that, are you thinking that might be part of it.

DR. HARBOURT: Very much, it's a, it's different from the vast majority of the rest of the Midwest, especially, in this study. Just to carry through these slides, slide two, this is Bill's slide. If you just click one button at a time, we'll see it. That's an indicator of the ultrasonic sensor, how we'd locate it on the bridge. What it does is, it, there's

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a cross-section. The blue line represents the water surface, and the green line's the channel bottom. Shoot an ultrasonic pulse down, and it measures that change in river stage. And a pressure transducer, also, coming in from the bank, measures the depth up. We, then, measure the channel gradient for slope to determine the energy grading of the flow. We integrate that with Manning's equation to come up with the flow. Yeah, go ahead to the next one. So, just, in summary, we're measuring a river depth. We're surveying a channel cross-section. We're surveying the channel's slope. We're applying a Manning's equation. And we're using that to come up with an estimate of flow. Now, within that flow estimate, there's uncertainty. And, you know, I'm sure as USGS can talk to this, we've, we are overestimating low flow, and I can go into that in some detail. And that's, particularly, evident at these streams that go dry or are very, very low in depth. We're not verifying the flow with a flow meter or anything like that. We're, also, not checking for back water. And that's a condition where the small streams that we're monitoring, and these are all very, very small headwater type streams. As they enter larger conveyances, sometimes those are deeper, perhaps, and they cause backflow and the velocity

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profile changes. So, we're not accounting for that. And that could lead to overestimates of flow, particularly, during the storm vents. And we're not coming up with a rating curve, and USGS can talk to this. I mean, they've spent years and years developing these rating curves, where they measure flows at different stages to develop that relationship between depth and flow. And, so, really, what we're saying here is that, although, we're providing flow, it's really there as a contextual measure to take a look when we're viewing things, in terms of a residue in time chemograph, what was occurring in the field? Was there rain? Did that drive some type of runoff? Is that sample tied to runoff, or is it just something that occurred at random throughout the year? Next slide.

And here's an example of the Nebraska 7 site, and we saw this earlier in one of the other presentations. Highlighted in the orange circle, the pressure transducer is sitting on the bottom of a dry creek bed. We went back to the data record, and this is the same data that was provided to the EPA in the, over the summer in August. And for that day, we had a range where the sensor was predicting a flow of somewhere between .7 and 4.2 cubic feet per second.

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And largely, that's due, and the sensor has a physical dimension. It's, literally, an inch off the bottom. As soon as the water drops below the level of the sensor, even though it's dry, it still thinks there's an inch of water, because it's a calculated flow, there appears to be flow moving by. So, estimates like this, especially at these dry down sites, it's very important that we tie that back to the observations that the field folks made every four days. And they made observations of river depth and, also, condition, whether or not they could collect the sample. Next slide.

This is Nebraska 5. We had an issue here, and, really, at all these Nebraska sites. They're very active channels. They're moving, and I use the word aggrading here, which means, they're actually filling in with sediment. There's sand that's coming to the channel. The channel bed is changing. So, in one of the figures yesterday, it appeared that stage increased halfway through the year, and we had this flow where you had nice hydrographs early on. And then, all of a sudden, it's this elevated level, and there was conjecture that, perhaps, it was an upstream release. It's more likely the streambed moved enough, and the channel shifted, and we're not correcting for

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that in our flow calculations. Nebraska 5 and the others all have that. And then, just, Dr. Gay spoke, touched on this earlier on talking about the depth. The Lamott samplers, according to our protocol, they're a four inch device, and I showed one of those on Tuesday. We require that they be at least an inch off of the bottom to not stir up the sediments and things laying right there. So, really, we had five inch minimum depth in order to collect the sample. And it's true that an auto sampler, it's just, I mean, the intake's the dimension of your finger, so it can lay right on the bottom and could collect a sample at a lower stage. And that's why, in some cases, we weren't able to collect a grab sample, but the auto sampler was successful on the same day. And that explains that a little bit. Next slide.

We also, these low flow samples, we documented them. They're in the reports, both in summary, on a site by site basis in text form, and then, there's a summary table, detailing each of the ones that we were not able to collect at each site. If we just go to the next, here's an example of the table. And then highlighted in the red block, and this is showing our success, our number of grab samples that we were, actually, able to obtain. Just to highlight, we

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were on the site every time. We never missed a trip to the field. So, every four days, we were there, without fail across the years and across all sites. This table indicates the number of samples we were, actually, able to collect. In the red box, if you look into 2005 and 2006 for these three sites in question, and we're talking about 10, 15, you know, in the case of Nebraska 7, only three samples that we were able to pull with our equipment across 37 to 40 attempts there, where we were physically there attempting to take a sample. I mean, it's our job to bring back a bottle of water. Now, if we can't, we're kind of upset. So, it's unfortunate this happened, but it really highlights to us that something's different with these three site, absolutely, as a group.

DR. HEERINGA: Thank you very much, Dr. Harbourt. Any questions, Dr. Gay, do you have a question?

DR. GAY: I think I just have a comment here, that, now that I'm really looking at the streambeds and thinking about the inch of water, I'm wondering if you could somehow incorporate, like, an H flume or something in one of these streams to concentrate the flows that you do have, in order to get a handle of the concentrations, particularly, since

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there are such high concentrations when you do have some intermittent flow that would capture it in conjunction with using an ISCO on a regular sampling basis, so someone doesn't have to be there every day.

DR. HARBOURT: No, it's a good idea, and really, what the suggestion is, is some type of stream modification, where you're putting in a small retaining structure to form a small pool that you can sample from. And that's something that, you know, the, we didn't look into. We were concerned that it's altering the environment. We're, really, just coming there to sample. We didn't know this condition was going to happen, obviously, when we instrumented, but it's a good idea, if we could get a buyoff from Fish and Wildlife, that we weren't changing the ecosystem, we'd be happy to, kind of, install that type of technology, if further monitoring was required.

DR. HEERINGA: Dr. Randolph.

DR. RANDOLPH: I'm not a hydrologist, so this may be, there may be an obvious answer to the question, but why didn't you put flow meters in each of these sampling sites?

DR. HARBOURT: That's a good question. Current flow monitoring technologies has some kind of test. There's either under-bridge-mounted devices,

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that are very hard to accurately calibrate, and, typically, you know, like the USGS owns one or two of them, and they move them around the country. The other types of flow meters are, you locate them on the bottom of the stream, and they use a sound pulse or some kind of radio frequency emission, and it bounces off of particles in the stream. And they're reliable sometimes. In other times, a lot of these streams are flowing too clean and too clear to pick something up. So, they're unreliable. We've tried to install some of them, and we've had marginal success, especially at low flows, which is the question here, of course. All of the technology is stressed for low flows.

DR. HEERINGA: Thank you very much, Dr. Harbourt for that clarification, and Dr. Ellsworth for bringing it to our attention. Dr. LaPoint?

DR. LAPOINT: Thank you. This is Tom LaPoint. Low flow sites and intermittent streams, here's a perfect component where the biota needs to be linked to the response of the streams in this case. We can maintain it, or establish a distribution of flow, and that distribution can go from zero flow up to full flow for the year, and that's how we define intermittent streams, from ephemeral to full flowing. And the response in the types of organisms that live in

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there, then, in terms of the stress that stems from being in an ephemeral, in an intermittent situation, if the, to provide something positive, here, if there's a framework to put this in to consider these streams, I think I would refer you to Michael Houston's work, where he has a series of papers. And, also, this has been carried on to pesticide work and published by others, and where you look at the degree of stress and the degree of duration, or the length of duration, the duration, and use those two axes, then. Because, as I mentioned, here's where, and this feeds into the second part of this, or the second question on this, is, one cannot look at the stream flow and the degree of intermittency and understand stress to the biota, without understanding and linking the biota to the nature of the stream here. And that gets back to the micro mesocosm test as well. A lot of times we think that the result of a mesocosm test is some kind of, being glib now, I suppose, some kind of multi-metric fathead minnow. Well, it's - -

DR. LAPOINT: It's actually a, you know, there are multiple types of organisms that are responding here, from phytoplankton to periphyton to microphytes then to the fish we've talked about. So, if the degree of match, if you wish, from the micro

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mesocosm, the experimental ecosystem tests, to the consequences for Atrazine exposure under these stressful conditions, then you have to look, and there's no other way to do it than to do this post hoc analysis, because the data are, actually, already here. We have an extensive database for all those mesocosm tests that went on. The ones that are going to come back for us is Meyer, say, if you look at that, the rich diversity is, largely, the very small organisms that have the potential for rapid recolonization, rapid development. They're probably hyporheic, which means they're living in the substrate as, at least as propagules, and once the water comes back in, they can come up in a very short time, literally, hours to a few days, and you can have a complete population again. So, that ties to how long the stream has been dry, and you can build a distribution, a nice distribution of those for how long, match that back, and maybe the distribution, then, on degree of how, the extended periods, for which those streams are dry, can be a stress index. And you might want to just consider those that are flowing, I mean, from that, you can see how many are in what component, and consider the upper 90 percent, or upper 50 percent, or something like that, but at least, you could do it that way. And

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then, if they remain less intermittent, they have low flow for more of the year, then the match will be to the higher or-, higher level organisms in some of the experimental ecosystems. So, there's going to have to be, to answer this, in my opinion, it's going to have to look at, very carefully, the distribution of flows, and then relate that back to the nature of the types of organisms that responded in some of the tests, some of the experimental ecosystem tests. And I think that, honestly, will provide an answer there, so, thank you.

DR. HEERINGA: Yes, Bob Gilliom.

DR. GILLIOM: Maybe prelude to the discussion, I agree with about the application of the meso microcosm studies to the intermittent systems, which, I think, they do have applicability if applied carefully, is it seems like the first question in this topic is how and do, and I, and you've partially answered it here, intermittent streams fit into the priority of resource protection from both the scientific and policy point of view. So, if headwater intermittent streams are ecologically a key part of the system that are affected by Atrazine and other stressors, they've got to be dealt with. And then, the question's, how? If, on the other hand, somebody decides they're not, then some of these issues go away.

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I have a feeling that the answer will probably be, is they're important because of their role in the whole, overall ecosystem, and they represent a lot of stream miles in the country. But, they're probably going to take some different adjustments to the approach. And I think some of the ideas that were just brought out are some of the possibilities.

DR. HEERINGA: Good comment. Dr. Young, and then, maybe Dr. Portier, and I'll think we'll move - -

DR. GAY: Yes, just to follow up on that idea. Because of the way that the design is established, and because the intermittent and low flow conditions do exist, I think you want to leave them in, if at all possible. The reason you might not be able to leave them in is, whether or not you can determine whether the LOC's been exceeded or not. You know, that would be a case where you just couldn't get data, but as far as estimating this population proportion, you need them there. Because they do represent a whole class of streams, and you, and if you throw them out, you throw out all streams of that kind, basically, and all watershed that would've been selected by that criteria, and you begin to bias your results. So, for that reason, I understand all the reasons why they're

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problematic, but an effort to keep them in would, I think, be of value.

DR. HEERINGA: Dr. Grue?

DR. GRUE: Yeah, I'd just like to follow up. I agree with the comments associated with the intermittent streams, and I think critical to that, that Dr. Young just mentioned, and also, Dr. LaPoint is, are these actually ecologically part of the system, or are the low flow conditions being created by some land use practice that is atypical in the area. If these are ecologically driven, then they should be considered and considered a-, we'll have to make some decision in terms of how you're going to relate those conditions and the biota associated with those conditions back to the microcosm mesocosm tests, which are, for the most part, static tests. There aren't that many in there that are flowing tests. The other point that I wanted to make, in terms of stress here, if, in fact, these are ecologically driven systems, then, as Dr. LaPoint indicated, the species that are going to be pre-, present have, in part, adapted to these conditions. And they are geared to respond. So, I think, we have to be careful how we use the terms stress, because it sounds, you could look at that and say, well, you're taking species that has adapted to a

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flowing condition, and then you're putting it in an intermittent condition. Yes, you've added stress, because those species, or at least the portions of that population have been adapted to a flowing condition. If, in fact, these systems are, essentially, intermittent, then the biota associated with those conditions are there, because they've adapted to those situations. And as such, I guess my point is for the base ecological decisions, in terms of whether these are really ecological systems, part, and then I agree with Dr. Young's assessment, they need to be included. How the Agency, then, decides to determine an LOC for those is going to be the critical step.

DR. HEERINGA: Thank you, Dr. Grue. Dr. Young.

DR. YOUNG: Just as a follow up, I would say that you'd want them in, whether it's ecologically driven or man driven. Only because, this won't be the only man driven consequence out there, but you know, you just got one of them by chance. And so, still, that just goes into the whole mess of what you have to deal with when you're looking at a nationwide type approach. So, I'd leave it in, no matter what.

DR. HEERINGA: Dr. Portier, Ken.

DR. PORTIER: It's interesting to follow

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the discussion. One of the benefits of being a permanent panel member, as we sit through a lot of these discussions, and so we see, kind of, different viewpoints, a broader viewpoint, which is nice. And this discussion, and especially, the discussion of low flow stuff, kind of, brings me back to our last meeting, which was, what, about a month and a half ago, where we were looking at Atrazine and amphibians. And so, while Dr. LaPoint, kind of, went down in the web, and looked at smaller things that respond quicker, I'm thinking that, for some of these systems, maybe the species of interest of something like a frog, right, something further up, that integrates a very complex ecosystem or intermittent wet ecosystem, so we might be looking at tadpoles and frogs instead of diatoms and something else further down in the system. So, I just, kind of, lay that in there as a link to our previous discussion on the panel.

DR. HEERINGA: Thank you, Dr. Portier. Dr. Young's comments, and maybe, just to be clear for the discussion, and I think, you're really talking about representation of these watersheds. And so, in terms of, I, sort of, have the same view. At least, it's important to, sort of, keep them in, because they were, you defined a population which has some

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misclassification, you defined a data collection protocol, which, unfortunately, in some of these situations that you encounter, is inapplicable. So it would be like the machinery, a piece of machinery breaking down or going out of calibration in any sort of, so, in some ways, these sites are out of protocol, because you set up a measurement protocol under the scientific design, and you can't capture it. So, I think the distinction is, you want to leave them in, because they're representative of the population, but in some ways, in terms of handling them for inference, you have a problem, because you, they're, sort of, essentially, out of protocol. So, I don't think you want to exclude them, necessarily, unless you make a deliberate decision to, sort of, change your criteria on site selection to make sure that you have water in these systems year round. So, I, and I think that extends to the comments that a number of the other scientists have made, with regard to thinking about them, somewhat, uniquely as a class of systems. I mean, there's a whole gradation here that we're looking at, and you've already, sort of, separated stagnant waters from flowing waters, but this is, sort of, something that's, again, to one extreme of that, of those continuums, so. Yes, Jim Fairchild.

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DR. FAIRCHILD: Jim Fairchild, regarding protocol, I wanted to ask Dr. Harbourt that, you know, typically, you are collecting at the bridge, but, you know, for some physical reason, possibly the design of the bridge, if it had a concrete base, and, actually, was forming a dam with an a, an unstable channel, you know, you could accumulate debris there until you get the next strong storm event. But, on those states where there was no water or samples, was there any attempt made, or was there a protocol to go a thousand meter upstream or downstream, to be sure that you could get a sample, so that you didn't have to deal with missing data, which is always a problem with statistics.

DR. HEERINGA: Turn to Nelson, do you want to, I know this isn't your purview, but, um.

DR. THURMAN: Yeah, honestly, that wasn't a condition we had anticipated, and, really, there was no protocol to say that if there's nothing there, to move elsewhere. I think the intent was to try to, a, sample the same places for consistency, and b, because this wasn't the only site they visited in day. You needed to do it fairly quickly. So, I think it was more of an access thing, but that, you know, honestly, that wasn't specifically written into the protocol.

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DR. FAIRCHILD: This is Fairchild, again. Well, just, for the record, I think that, you know, the whole issue of amphibians wetlands headwater streams is a critical issue that many states and the federal government are facing. And there's no doubt, ecologically, that these ephemeral systems aren't critical ecosystem components. And so, in future studies, I'd hope we'd learn from this that, you know, you need to consider low flow conditions and getting, at least, some representation, if it's within reason. These types of ecosystems are important. Fits outside what someone, you know, I hear the term flowing and stagnant, and it reminds me of swamp and wetland. You know, there's a certain connotation that comes with these things, but they are aquatic systems, and they are regulated. So, let's learn from this.

DR. HEERINGA: Dr. Brady, I see Dr. Harbourt standing up, and he's either interested in starting his break, or he has something to offer. Are you willing to - -

DR. BRADY: Sure.

DR. HEERINGA: Dr. Harbourt, please come to the mike, though.

DR. HARBOURT: Dr. Harbourt, here. Just quickly to address the thought of moving up off of the

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bridge. And one of our concerns that the samplers were there by themselves, and there's the safety concern about them getting out and away from the vehicles, away from the roads, and going out to investigate these sites. There's also an issue of, you know, private property, and we were in public right-of-ways where we were, and we really weren't allowed to trespass and go hunting around for potential samples away from the bridge.

DR. HEERINGA: Logical. Sure, Jim Fairchild.

DR. FAIRCHILD: Fairchild. That, you know, being from USGS, that always is a problem, but, and we, literally, cannot access private lands without the written permission. But, for a study of this scope, it does not take that much time to go out there and talk with the landowners, explaining them the purpose of the study, how the data is going to be used, and you know, that should be part of the future sampling protocol, in my opinion.

DR. HEERINGA: Jeff Novak.

DR. NOVAK: This is Jeff Novak with a suggestion for access to farms. We found in the ARS that the closest, or the fastest way to get access to landowners is to work through the County Extension

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Service. Once you make a request through the County Extension Agent, they can go among the owners along the stream system, find cooperators, make written or verbal agreements, and then you have access to that stream channel. The Extension Service per county was paramount for us to be able to get access to all these small stream channels. Thank you.

DR. HEERINGA: Good advice, thank you very much. Well, at this point, what I'd like to do is, I'd like to call a break. We're just a little bit short of 10:30, so we've been very productive, I believe, this morning. I appreciate everybody's input, comments. Let's take a break until fifteen minutes of 11:00 and then, we'll reconvene, and pick up with the charge question number nine. Dr. Randolph.

DR. RANDOLPH: J. C. Randolph. Just a question about producing results. We're having some trouble with a printer next door this morning, and I just hope that we could get a functioning printer.

DR. HEERINGA: Okay.

DR. DOWNING: It's back and working now.

DR. HEERINGA: Good, thank you. That may be the toughest request of the day.

(WHEREUPON, there was a break.)

DR. HEERINGA: Okay, welcome back

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everybody to the second half of our third morning session of FIFRA Science Advisory Panel meeting on the topic of the interpretation of the ecological significance of Atrazine stream water concentrations using a statistically designed monitoring program. Jim's informed me, we're working on the temperature fluctuation. It's going to be a, heat is going to be a little intermittent here. It's either high or we can decide to let it drop a little bit, but they don't know how far it'll drop, so.

DR. HEERINGA: We'll, we're going to talk faster, so, that. Before we turn to charge question nine, Dr. Grue had one clarifying comment, and then, I think, Dr. Brady indicated that the EPA scientific staff had a few questions for us, just to make sure that we were clear, in terms of presentation on this, so, Chris.

DR. GRUE: Yeah, I just wanted to clarify my comments about these apparent intermittent sites. And the feeling, I wasn't suggesting that they be excluded. I agree completely with Drs. Young and Heeringa about that. But I think, it's important for us to know what's really driving those systems. And I would argue that, right now, I don't think we really have a clear understanding. And, as such, it really

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seems to me that we need to move towards some additional monitoring on those sites to really get an understanding of what's going on.

DR. HEERINGA: Thank you, Dr. Grue. And Dr. Brady, I think that you had some follow up questions, then?

DR. BRADY: Yeah, we had, just, two follow up questions. First, I'm going to ask Dr. Olsen to pose, and that is on the question of how the Agency should consider these low flow sites. We're just trying to see, in our own minds, and be clear about, what the advice of the panel is.

DR. OLSEN: Tony Olsen. I'll state it as my understanding of what the panel is recommending, and then you can clarify whether my understanding is correct. My understanding is, is that the low flow sites should be included in all the population estimation, but that they should, at this point, still be kept separate as a separate category, in terms of, let's say, the LOC things. And is that the understanding of the panel?

DR. YOUNG: Wait, how can you do both those things?

DR. OLSEN: Well, no, I mean, they're included by what they're doing, and, but it, basically,

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the ta-, the estimation table that I ended up presenting that had the different LOC categories, okay. One of those categories was, ended up being the three low flow sites, and then, what you're saying is, is that category should still stay there, that we shouldn't change the way the analysis was done. That's all I'm saying.

DR. YOUNG: Right, but then, when you actually did the population distribution curve, didn't you include those in that curve?

DR. OLSEN: They were also included in that curve.

DR. YOUNG: Okay, yeah, that's fine, though, yeah.

DR. OLSEN: So, that was, just wanted to make sure that we're absolutely certain on that.

DR. HEERINGA: Dr. Grue.

DR. GRUE: My point with that would be, I'm just not convinced that there's sufficient data on the Atrazine exposures for these sites to, really, do an assessment. And that's what I was suggesting, as far as additional monitoring. Really try-, suggesting that the Agency, in collaboration with Syngenta, get the data necessary, really get a good understanding, because they, depending on whether it's real or not,

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the interpolation of those data between sampling pulses, it, you've already shown, has a big impact on how you determine the concentrations that are relative to the LOC. And that's, simply, what I'm saying. I'm not convinced the data are there to do it, but I do agree, that the sites, themselves, should be included in the analysis. Whether or not you can do an assessment relative to the LOC at this point, I think is, to me, is questionable.

DR. HEERINGA: I think, in terms of, Steve Heeringa, in terms of population characterization, I think what Linda and Chris are saying is that they should be reflected in there as representative of a certain number of sites, but for that particular category, within the larger population, I think the LOC status is indeterminate at this point. And it might be determined if, you know, but, again, in all of the issues we have, I think it's, sort of, like a don't know response in a larger sample, Tony, and, but clearly, it's representation, in terms of the population, given the definition of that population and measures used to establish is representative. Second question, Dr. Brady.

DR. BRADY: Yes, thank you. This is Don Brady again, and I guess, this question goes to the

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second part, and just, trying to be clear in our minds what the advice is on whether the existing microcosm and mesocosm studies do adequately represent these low flow sites, just in terms of, is there possible to get some agreement or some clearer statement of that for us. Thank you.

DR. HEERINGA: I think, see, Dr. LaPoint, and, now, did you hear the question with regard to the appropriateness of the meso and microcosm studies for this that Dr. Brady just read? Would you read it again, 'cause I'm not sure that Dr. Lerch has also heard it.

DR. BRADY: Yeah, we, sure, we were trying to get clarity on what the advice of the panel was on that second question about how well the studies represent these low flow sites, just trying to be clear in our mind what we're hearing, here.

DR. HEERINGA: Dr. LaPoint.

DR. LAPOINT: Yes, in my opinion, now, not speaking at all for the panel, of course, I think they can be representative, and here, it's a bit of a deviation from what we've been talking about, because the micro mesocosm tests that deal with the smaller organisms, which by and large have a more rapid response time, also tend to be those that are more

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sensitive, they're the ones that, in static systems, would most likely reflect the, at least, the Atrazine, potential for Atrazine responses in these static systems. So, for low flow, or the intermittent ones, where they pool up, okay, that would be appropriate. For low flow systems, I think, either looking at the flowing macrofied systems would still be a situation that could be followed.

And so, and for the ones that are intermediate, that aren't flowing, that flow sometimes, that, in my opinion, now again, would have to be something based on a distribution system of how often they're flowing, how often they're stagnant, and, somehow, it would have to be a judgment call, to tell you the truth, to pick some percentage of those that are flowing and use those, and then, that are stagnant and how often the water stays in there in the pool. 'Cause that determines what, how flashy it is will depend on how much of a stress, and I don't disagree with that. I think that flashiness can be a stress for organisms that are ex-, they're used to, or adapted to, if you wish, continuous flow conditions. But, if it's an intermittent flow stream, then there, actually, are organisms that are quite well adapted to that, and that needs to be taken into account, then.

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I mean, the last part of it, I recognize, is not direct help, probably. It's more of a problem, and I hate always presenting a problem, but there are, there is a framework that this can be put into, and I would refer, I'll be happy to give some of the references on this, where it's been used before, looking at adversity axes, how strong the stress is. And on the other axis is how long that stress happens to be, or the duration of that stress. And on the basis of those two axes, the theoretical background, anyway, has led to a good understanding of what kinds of groups of species are adapted for those kinds of situations, where you have a moderate stress for a moderate period of time, actually, according to that framework, is where you might find higher diversity. And that fits with what Meyer was talking about for these systems, all right. If it's a high stress, either dryness or high Atrazine, you find that there's a set of organisms that may do well under those situations, but they can be allocated to those kinds of situations. So, I think it would be differentiated. But, that's my opinion.

DR. HEERINGA: Other panel members, Dr. LaPoint, I think, has made a fairly clear statement on that. Would that represent our, well, Bob Gilliom and

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then Jim Fairchild then.

DR. GILLIOM: Sometimes you think you're kind of understanding where the whole flow is, and then it goes a little bit different. A simple prelude to what I think would be a good way to go, eventually, could also be to, similar to, in effect, use the existing microcosm studies as they are, at the time interval that fits the lengths of flow periods and wet periods in the intermittent streams. Meaning, you might be going right to the twenty day mesocosm results, and have an LOC that's defined a little differently than comes directly out of the model, because the model, in these circumstances, isn't really designed to hook together intermittent conditions. So, it's seems like we're back to that thing of, at that point, why would you use the model, and you might as well default to the empirical data for that type of time scale that comes from the mesocosm studies, which are what it all started with anyway. So, it's, maybe the prelude, at a very simple screening level to what would be a more detailed analysis of the nuts and bolts of those individual mesocosm studies that might best apply to the species. So, it's kind of a variation on the other comment.

DR. HEERINGA: Jim Fairchild.

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DR. FAIRCHILD: Fairchild, I agree with Gilliom's comments, and I think - -

DR. HEERINGA: Hold the mike up a little closer, if you would, Bob.

DR. GILLIOM: Since you agree.

DR. FAIRCHILD: Right. I agree with Bob's comments, and, in terms of the fact that that data is probably out there. It may not be captured in the Brock mesocosm microcosm studies, but that data is out there in the literature. I'm not sure that it's going to fit your needs if you're really tied to this CASM model. But I agree with Dr. Gilliom that, why use the data in a model when you can go to the literature, get good data, and, actually, put it in the species sensitivity distribution, and then, compare that to your probabilities of exposures. There's a very direct way to do that.

DR. HEERINGA: Dr. Brady, with regard to the two questions, you think they have them addressed?

DR. BRADY: I would check with my panel members. I, is that, okay, thank you.

DR. HEERINGA: Okay, thank you very much. That's very useful to clarify that. At this point, I'd like to move on to the charge question nine, which is the final question related to the second component of

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our review, the study design. Obviously, all of this is integrated, but the way we've broken it up, so, Dr. Brady, if you would read that into the record.

DR. BRADY: The monitoring study sampled for Atrazine concentrations at four day intervals to characterize the Atrazine chemograph in these low water, Midwestern streams. The CASM Atrazine model used the chemographs with a stairstep interpolation between samples dates to relate Atrazine exposures in the streams to microcosm mesocosm studies in order to determine whether the exposures triggered LOC thresholds What other approaches for interpolation should be considered, given the concentration duration endpoint? How frequently must sampling occur to appropriately capture the magnitude and durations of exposure associated with Atrazine? Sensitivity analysis of CASM Atrazine model inputs suggest that some uncertainty bound on model results is appropriate. The Agency used a 2X multiplication factor from the model's sensitivity analysis to estimate uncertainty in model output. The sample frequency analysis indicates that there is uncertainty associated with monitoring data that may not be accounted for by the model uncertainty factor of 2X. Given the importance of sample frequency in interpolation, please comment on

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whether consideration should be given to placing additional uncertainty bounds on monitoring data to account for uncertainty in the ability of the sampling strategy to capture the magnitude and duration of Atrazine exposures. Please provide any suggestions for how to proceed with this approach.

DR. HEERINGA: Okay, and our lead discussant on this somewhat complex question is Dr. Young.

DR. YOUNG: Good, the mike was off, okay. Okay, on the first one, I think it's very unlikely you caught any of the peaks of Atrazine, given the flash nature of it, and the fact it could peak, and you're going out every four days, and it could occur over a ten or thirty minute period. So, I don't think you captured them, and the question is, how important is that. And I'll let the ecologists on the panel address that issue. Given that, I think that the stairstep approach is a reasonable approach to use. It does make a big difference, especially with those low and intermittent flow streams. And I just, I don't have any great insights into other methods to be used.

With respect to the air in this last part, the 2X, the question is really predicated on assuming that you have the right model. So,

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conditional on the model, then, what kind of error. And there's probably, you know, George Box said, all models are wrong, some are useful. I think this is a useful model, but it's certainly wrong in some aspects. And so, some of the variability has to be associated with just the incorrect model period.

Then you have the variability associated with the inputs into the model, and as a focus of this question goes on the Atrazine in particular. I would, I think some of the techniques that you've used so far to try to do some what if's, and look at various sampling approaches, and accompanied with some simulations to get measures of bounds associated with that would be useful. I think if you really want to capture this, you're going to have to use more auto samplers, so that you can really tighten up those, the timing, so that you can really capture more of the Atrazine that's occurring out there. So, and, I guess, that would be my greatest recommendation, would be that you try to incorporate a lot more auto samplers in there to get more information.

DR. HEERINGA: Our first associate discussant is Dr. Chue, Michael.

DR. CHUE: Michael Chue. Yes, I agree. The selection of interpolation method is very, very

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important. A good interpolation method can help us to get better, a better estimation of what are missing for this time period. But, I really want to talk about something different, of course, about the interpolation method. Let's look at some special case. For example, during four days, 'cause we are looking at some very small watersheds, during four days, let's assume, and there is a, we, that something happened. We have some very short duration r-, storm event, and also, we have some applications, Atrazine applications. Okay, then, we may have a very high peak, high peak concentration during these four days. Now, upon the end of this four day time period, the flow may return to base flow. And then the concentration may, also, very small. That means, we can select a different interpolation method. For example, we can use stair-, stairstep, or linear interpolation methods, but I don't think we can find a good way to recover that peak. It's almost impossible without any additional information about a storm event and applications. So, here, I really want to emphasize one point. And that is, in addition to select of, select a better interpolation method, we also need to take into account some very important factors. For example, storm event and also, application timing. In this way, of course, actually, about At-, Atrazine app

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concentrations, we may not know exactly what are going to happen during this four days. But, we do know something might happen during as unusual or as stable conditions. I'm talking about, for example, during the four day, we may, we know, we may have some information about rainfall. We may have some in, also, we may have some information about application. Of course, if we don't exact application information, but during the intensive Atrazine application period. I think we may, if possible, we can add more, we can get more samples. This is a, actually, this is the second question about sampling frequency. About this one, actually, yesterday we also talked about, something about this. I asked why we selected a four day time interval. Basically, if we select a four day time interval, probably, most of samples will be in the dry time period of samples, 'cause, we may miss some very high peaks. So, I think the determination of the sampling frequency also should consider the major factors, for example, storm events, and Atrazine applications. So, in this case, I really want to suggest to some, instead of constant sampling time interval, I really want to suggest so some variable sampling time interval. That means, during and after a rainfall event, and also, after applications, we may increase the samples. In

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that way, we may be able to get some peaks, you know. That's my major comments. Thank you.

DR. HEERINGA: So, you're clearly recommending some type of adaptive sampling scheme, and I think it's, you know, after this first certain major study, there's a lot of information to inform that, and I think, that's a good suggestion. Jim Fairchild is the next associate discussant in this.

DR. FAIRCHILD: I'm sorry, could I ask.

DR. HEERINGA: You want to defer, okay, we can come back. Dr. Gay, Paige.

DR. GAY: Thank you, Paige Gay, University of Georgia. I think my comments, pretty much, are along the same lines. I had an appreciation for the stairstep interpolation, except where there were higher concentrations and rainfall events. And I thought a more rigorous sampling regime closer to the applications times might be warranted, realizing that that does include more manpower, and monetary resources. But that, certainly, might be beneficial on some small scale to fill in some of these gaps. And I do recognize that missing total peak might not be as obvious as it might seem that that could occur, because it, as noted, usually, you, probably, would catch some portion of the peak. You know, even if it's on the

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very tail ends, that you would have some indication that some kind of event had occurred during that four day period, and how you could use the precipitation data, perhaps, to, in conjunction with an identifier, that there was some kind of change in the concentration to make an estimate, and that's just way beyond what I have the expertise to really think about, but identifying places in the chemographs where, based on the rainfall, where there seems to be something missing in the four day regime might be something to pursue. In looking at the linear interpolations based on PRZM, I became really unsure that the, out of 80 chemographs, 66 yielded higher concentrations than the absolute site measurements. And these ranged from 1 to 1,320 percent higher, although, none of these estimates caused there to be an increase in the LOC. I was not sure that this didn't just introduce more uncertainty by trying to estimate fill in gaps using this method, than, that's just a, really, big range for me and introduces more error that you're not going to sufficiently capture. I, suggestions for interpolations, which I gleaned in the manuscript, were previewing conditional simulation time series interpolations and trying to discuss this is way beyond any kind of scope that I could get into, so I'm just going to defer on that. The Crawford data,

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I thought, was interesting, and certainly was worth pursuing, in order to try to estimate some kind of uncertainty factor, particularly, since one of the sampling regimes did kind of mimic what we had here. And, I think that the, with the exception of samples collected during the initial post-application period, I think the W46 might have had a higher rate of those, maybe. But, it was a pretty good fit, and I think it gives a really good thought that you could use that, and you know, with pursuit to get an estimation factor of the uncertainty, based on the sampling regime. Because, certainly, when you start applying this to other areas, you're just not going to be able to go out there every for days. It's just not going to happen.

DR. HEERINGA: Thank you, Dr. Gay. Chris Grue, Dr. Grue.

DR. GRUE: Chris Grue speaking. There's a significant tradeoff here between, you know, sampling frequency and logistics and expense. And that's been, that's been already discussed. And, I think, I'd argue that this study is unusual in its sampling frequency. You just don't get frequencies this often. So, to suggest that we're going to sample more frequently, probably, is not realistic. And, I make that point, maybe, based on some other, some other thoughts looking

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at the data. It may require some understanding beyond that I have, but, certainly, maybe, in the existing, in the existing data. For example, the auto sampler data, you know, how well that compares to the data that were collected on those sites. I think you can give us some insights. In the overall assessment, if I'm interpreting the data correctly, it didn't make any difference on the SSI's relative to the LOC's. So, that's some, that's some comfort. Yet, you're still left with, you know, some uncertainty, and I think the point made that, and we've discussed this, that the frequency of peaks, and the magnitude of peaks, are very important in deciding whether or not the ultimate SSI exceeds the LOC. So, what we don't want to do is miss peaks, or miss peaks frequently enough that it alters our interpretation of the position of the SSI relative to the LOC.

One thing that I think might be helpful, and is, you know, in looking at precipitation events, themselves, relative to the grab samples and the auto samplers, and others, do auto samplers accurately capture that. It may be that, as you move forward with your monitoring effort, just to develop some additional comfort level with this, that the auto sampler effort be increased to, again, confirm the, at least, initial

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results, that even if we miss some peaks, the magnitude of the effect on the position of the SSI relative to the LOC is, doesn't, really, appear to have a big impact.

Related to the stairstep and the linear, those seem to be the two approaches that could be taken. I'm certainly comfortable with the stairstep. It's certainly more conservative, or potentially more conservative than the linear. And, the idea of, you know, from a regulatory perspective, to build a certain amount of conservatism in the modeling or in the results, the process is certainly, is certainly realistic. And I'm going to, I know Dr. Ellsworth has some comments related to interpolation, and I'm looking forward to having him chime in at the appropriate time on this.

The other thing I just want to mention, with respect to the interpolation is, I think the approaches are appropriate, except where you're missing data. And we've talked about this, relative to the intermittent sites. I don't think, just say, well, we recommend stairstep, but then, stairstep across the board. I think stairstep where you feel you have adequate data to apply any type of interpolation strategy.

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That kind of leads in, then, to another point, and that's the 2X. I'm really left unsure as to why 2X. I don't know if that was a-, to me, I couldn't, really, looking at the information, get a good feel, as far as 2X. In the introductory material relative to the development and application of the model, and it was a question I brought up earlier, in terms of position of the SSI relative to the LOC, and the, what appeared to be, now, some, not necessarily real, but somewhat modified chemographs, where those sat, relative to the LOC and the suggestion that, well, we can apply a multiplication factor here in terms of safety, but I'm not really clear on why 2X. Why not something else, what's, what is 2X. And, in fact, given that, at least it's been suggested, that there's a number of cons-, there's a certain amount of conservatism that's been applied to each one of the different steps. How does that come into coming up with the 2X. And, so that's, essentially, what I would like to see some additional clarification as to the basis for the 2X.

DR. HEERINGA: Mark or Nelson, I guess, the question as I understand Chris' comment or question is that 2X has been presented, was there sort of a quantitative argument, based on a set of data or

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results that led you to that factor, is it just that 2 is better than 1, and less than 3? Well, that 3X is used in a number of other situations, but, so.

DR. ERICKSON: This is Russell Erickson. As Mark talked about in his talk yesterday, the 2X is the rough uncertainty factor that we carried over from the, the CASM model because of the uncertainty established with the toxicity value selection. And so, that it, basically, went out, based on the sensitivity analysis regarding random selection of the EC 50's going out to standard deviations for several of the chemographs that went out about a factor of two, and what it really means is that, if you, in application, if we're within a factor, then, within that factor of two, of the, of saying the LOC is exceeded that the LOC, actually, might be reached. Again, depending upon the nature of the chemograph, which and it isn't refined for that yet. But the, that the LOC, actually, might be reached at those sites for, again, the two standard deviation limit on the possibilities of the toxicity data. But, Mark's work was, actually, saying, and this question is, actually, addressing, not so much that 2X factor, which I just explained, but the additional uncertainty that might need to be opposed on the analysis because of the uncertainty in the exposure

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information. So, the 2X is, sort of, a starting point, as far as saying, if you're within a factor of two model uncertainty relative to the toxicity data, means you're, at least, in a gray area of where you're getting to where the LO-, an LOC might be exceeded. But, this actual charge question is more oriented, not that we can't discuss both, but this charge question is more oriented toward the, how you do the additional uncertainty related to the exposure.

DR. CORBIN: And this is Mark Corbin, and I would just add, do you do that for the monitoring data relative to what Russ just described, maybe yes, maybe no. And that's what we're looking for feedback on, is that appropriate. But the 2X factor is separate. That's based on those environmental conditions and the toxicity data that Russ described on Monday, Tuesday, seems like Monday, sorry.

DR. HEERINGA: Dr. Grue, go ahead, no, please, I think you're on the right track, here.

DR. GRUE: So, are we looking at two 2X factors, or are we looking at one 2X factor, and it's applied at this point?

DR. CORBIN: This is Mark Corbin. I think we're not sure, is what we're asking. I think for the model sensitivity analysis that Russ described,

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that was, sort of, a rough estimate. It's not - -

DR. ERICKSON: It is, too, and the one we're applying, we're only applying it once.

DR. CORBIN: Right, and we're only - -

DR. ERICKSON: Okay, well, I, well, we're starting with one 2X factor that started with the CASM analysis, as I, sensitivity analysis, as I explained it, okay. So, that's, and that's certainly subject to discussion, as far as whether that's an appropriate factor, but that, that 2X factor, if you nominally take your exposure data from any field site, and are within that uncertainty factor, you, at least, have to take that, that site is at least subject to some concern, that the LOC might be evaluated within the bounds of that uncertainty. And so, we're really talking about one 2X factor, but then, an additional question about whether the uncertainties involved, like the uncertainties involved in the interpolation, the data interpolation should involve additional uncertainties to be imposed on the LOC evaluations to account for the uncertainty on the exposure side. The 2X is more on the effects side, where there's a starting point, should there be additional uncertainties expressed because of these exposure site issues, and those aren't necessarily 2X. I mean, nothing specific has been

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proposed. So, the 2X in this charge question is just referring back to the effects uncertainty factor. And the question here, more, is, should it be larger than 2X to account for the, some of the uncertainties that Mark talked about in the, on the exposure site. I hope that helps.

DR. HEERINGA: Dr. Grue.

DR. GRUE: Yeah, Chris Grue. So, my understanding is that you're proposing a 2X based on the exposure data, and the variability in the expo-, that, the results of the microcosm mesocosm data,

DR. ERICKSON: Yeah, the effects data.

DR. GRUE: The effects data, yeah, thank you. And so, the question to us is, should we be imposing some additional safety factor beyond that because of uncertainty associated with exposures in the field. Is that, that's correct?

DR. ERICKSON: Yes, that's correct.

DR. GRUE: So, with respect to that, I don't know if other committee members are left with this, but I have, I'm having a hard time realist-, you know, kind of balancing the uncertainty with the conservatism that's been built into the process. And I don't know if it's possible to do this, but it's almost like, you know, a mass balance equation in the sense

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that, you know, at what points do we feel we're on the conservative side, and to what extent do we think we might be conservative versus where our uncertainty is, and how much correction factor do we need. And I don't know, because the, I think this is a-, at least for me, this is a difficult part of this question to answer, because don't know what's been built into the system a priori. I mean, if there's certain un-, discomfort associated with the uncertainties associated with exposure, then it may be appropriate to add on an additional safety factor here. But, I'm trying to look at the entire process, and just from comments that have been made, certainly from the registrant side, and I think it was made several times, in terms of, well, this is an extremely conservative approach, how do we, and I can understand that there may be a different interpretation from the regulatory side, but what is the sense in terms of from the regulator in terms of what aspects of the process introduce conservatism, what aspects of the process, to what extent, then, do we need to add additional, an additional safety factor.

DR. ERICKSON: Well, would you like me to comment from the effects side, as far as where I think it is conservative, or do you want to just leave this as a comment and not a question?

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DR. HEERINGA: I'll leave that up to Dr. Grue, at this point.
DR. GRUE: Well, I think, my point was, when I got to this section in the document, some additional explanation in terms of what the 2X was, what the basis for the 2X was, were we looking at additional, request for additional safety factor, whatever, would be helpful. But, also, some sense of the conservatism that's already been built into the process.
DR. HEERINGA: I think, Dr. Erickson, if we could leave it at that for the time, and we'll get the other comments. And then, before we leave the question, if there's, if this has sharpened up and we need to revisit it, we will. The next, are you satisfied, Jim, or did you have anything that you want to add in this, Jim Fairchild?
DR. FAIRCHILD: I do, and first of all, I'd like to apologize. You didn't catch me asleep. You, actually, caught me thinking.
DR. FAIRCHILD: To answer that, that two stage question that, it appears that sampling every four days is adequate, based on the concentration duration profiles that have been observed. And at the frequency, as long as it's four days or less, appears

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more than adequate, based on the needs and the outputs of the CASM model. However, I might say that if, outside the CASM model, if one were sampling for truly empirical risk assessment purposes, then event based sampling might be developed using auto samplers programmed at prescribed intervals that acknowledges local patterns of planning Atrazine application and rainfall patterns, much as Dr. Chue said. This probably could explore-, be explored using the data at hand on a site specific basis. And it may produce data just of good quality with fewer samples and less overall expense.
In regards to the uncertainty questions, I think, there are a couple approaches to this. One is to continue to run the CASM model under various scenarios beyond the chemographs and uncertainty levels, in effect, levels currently found in your studies to find out what results that leads to. The problem with that is, this is going to force you to select an SSI percent deviation that the EPA or the Agency's uncomfortable with. But, ultimately, I would expect that you would compare the outputs under different uncertainty scenarios, and go back to the original microcosm and mesocosm data, and compare that against the endpoints used by Brock, in addition to

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other endpoints that are associated with these studies. And ultimately, 'cause at that point, and again, I'm a very practical person, you are able to judge the model output with what ecological significance might be. And that should tell you if the 2X certainly, uncertainty factor is tolerable. Again, ultimately, though, it's, kind of, what is the Agency going to accept as a trigger of a level of concern.
DR. HEERINGA: Thank you, Dr. Fairchild. Dr. Portier is our final associate discussant, and then if we have other comments, why.
DR. PORTIER: And you have caught me writing, as I was trying to get my thoughts together on this. I wanted to pick up on what Dr. Grue said, and kind of, think about the issue of whether the stairstep method is truly conservative, because, I think, we have a feeling that it's conservative. So, I was trying to take it through the whole process of passing it through the CASM model, through the SSI index, to the LOC decision rule. So, compared to what we assume the true concentration duration profile is, the stairstep method tends to result in lower magnitudes of longer durations. And we've seen that when we look at events that have been caught with auto sampler. We tend to see a sharper peak, and a quicker return, and the

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stairstep method, kind of, flattens that out. When we look at the effects scores from the micro mesocosm effects assessments, we can see that concentration seems to have more of an effect than duration. And I say that, because the slope of that decision line is not very sharp, right. I mean if duration had a real effect, that slope would be short, and in fact, I keep looking at that chart and drawing different lines, and I almost feel like a better line is straight across, and forget duration, and just look at concentration. So, I have a feeling that concentration, kind of, should be a more important factor than duration in this effects score. The CASM model LOC method, the combined
method, links the magnitude in duration in the chemograph through the CASM model, and then, with SSI averaging to the LOC.
So, looking at the SSI, I spent some time this morning trying to understand what the SSI really does. And it's a, if you take one specie, and you assume that the baseline is 100, and then you look at, well, what happens if I reduce it by 10 percent, 20 percent, 30 percent, what is that SSI response going to look like. It's really a convex curve, kind of. So, that the effect is not that big on small changes, and it's gets kind of big further along. So, if the effect

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of peaks has a real impact on biomass, but the SSI, and you're missing that big effect on biomass, and then the SSI is only, is not giving strong weights to the lower end, and it's giving weights to the upper end, we're kind of combining, it seems to me, you're combining anti-conservativeness here, right. You're kind of being more liberal. And I'm not exactly sure about this. So, I'm afraid that the effect of missing the maximum concentration can propagate through the CASM model and the SSI index, to result in an underestimation of effect. So, that's kind of one side.

The other side is, well, you do this whole calibration process. And the calibration process takes into account that stairstep issue. So, it could be that the LOC you get is adjusted for all of this. And because of the complexity of this whole thing, I really, now, the whole, this whole discussion really comes back down to how critical it is to really do a great job on the sensitivity analysis. Probably an order of magnitude more than what you've done to this point to really understand how these effects propagate through. I just, I can argue it both ways. I tend to feel that it's, it tends to underestimate effect. And I see, the audience, you say, no, it's the other way

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around, but.

DR. HEERINGA: Jim Fairchild.

DR. FAIRCHILD: Well, I think those are, that's a very good point, and, you know, it kind of strikes me in coming to this panel that we see the presentation of the CASM model. We see what this, and this projected SSI deviations are. And we're having trouble saying, you know, what is the significance of that. Well, why didn't the Agency ask them to go to the second level, and, actually, look at the consumer or a higher level using the model, which is exactly what it was created for.

DR. HEERINGA: Dan Schlenk.

DR. SCHLENK: Yeah, I just wanted, Dan Schlenk, just wanted to follow up on that. I thought that was a really good question as well as. I think, anytime you assume that you're targeting the only population of organism in a system, I think you have uncertainty automatically. And there's some other documentation that was sent from National Marine Fisheries that indicated that, particularly, some of Mike Lightey's work, where you saw a mixture effects with other, or you know, phosphates and some other compounds that may target the consumer, as opposed to primary producer that, again, I know, this is, we're

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focusing only on compound, but I think you have to have some uncertainty in that evaluation as well. So, to me, the twofold, I think, is a good starting place, if not, you know, and again, I know this is trying to deal with exposure and not effects, but, I think with the effects, and the fact that, you know, you may not be getting the right target, per se, as far as what you're doing your risk assessment for.

DR. HEERINGA: Dr. Young.

DR. YOUNG: I'd like to build on what a lot of people have said here, and also, to try to get back and drill down to the question of separating this extra variability out. One of the ways that you could begin to do that, based on the model, and I'm going to repeat one more time. You need to build in uncertainty associated with your model, which you haven't done yet. But given that, you do have information on storm events. You have information on a lot of things that you could do some type of a time series and estimate and kind of do some prediction of peaks. So, you could go back, and you could, to, as, get this, this assessment of variation, you could go back and create, using various methods, and I wouldn't stick with one, I'd try several different ones, to fill in that information. And, actually, see how high the peaks you

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would, to be, and especially, I think, to get the tops of the peaks, not just the middle levels or the end of it. And then, see what effect that has. And so, create several different scenarios, that would be consistent with the data, that would capture the peaks, and see what effect that has, and how much variability is associated with the LOC in doing that. And then, you could, could begin to assess whether or not you need to build in. And I would say, yes, you need to build in that, in addition to the 2X, because that is an additional source of variation.

DR. HEERINGA: Dr. Ellsworth, did you have something to add on there.

DR. ELLSWORTH: Tim Ellsworth, here. This is kind of an area dear to me. I like this topic, so, humor me here. The auto samplers provided this continuous sipping during a six or eight hour period, and that has a great impact on the peak estimation, like has been said. I actually went to the literature, and I found a paper for several different catchments where they had seven to fifteen minute sampling intervals for Atrazine. And, you know, I did, my estimate on that, conservatively, the peak over, compared to an eight-hour composite, the peak was about 150 percent greater than what an eight-hour composite

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would estimate during a storm event. So, I mean, that's from other studies, but I'm just saying, that's a point of concern. So, even though you found that the peak, the auto sampler data showed you had a higher peak, than you got with the grab samples, the true peak is fairly, is bigger than that, considerably.

And another thing, that Michael mentioned, the auto samplers, it says in the Syngenta report, caught 149 of 178 flow events, okay. So, that was with the auto samplers. They missed some, too, somehow. I don't know. But the grab samples, obviously, missed more than that, and we would have that information from the depth recorded, you know, idea, so you could get an idea. There's some uncertainty there, and how many of those chemographs had events missed. And I think the thing that was brought up earlier, here, is, you know, you targeted which areas to sample in the U.S., based on probability. You probably want to do something like that when it comes to Atrazine time series. You want to concentrate on, like Michael said, during the events, during that two month window following application, you know, where you're going to get the most for your money.

The other thing that was mentioned by

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Dr. Young and others is this idea of auto correlation. The composite samples give you some indication at, you know, ten of these locations of that auto correlation. They're going to have a greater degree of auto correlation. There's some ways to, you know, you can, at least, bracket what the auto correlation would be for a point sample, given what you've got with the auto sample data. And I think it would be important to go back in and try to quantify for, and it's going to be flow event specific. It's going to be watershed area specific. The characteristics of that watershed, what's the slope, et cetera. I think it's going to be, those factors are going to come into play, but you could look at an estimate of auto correlation, and then you could come in on your graph sample time series, and start to put some uncertainty. You can predict what the concentration would have been with the most important part of that is, what is the uncertainty in between a four day grab sample that's in there. You know, you can come up with a pretty good estimate of what that uncertainty is to propagate through your uncertainty analysis.

And then, the ideas of looking at secondary auxiliary flow data to help you improve that estimation or minimize that uncertainty any way you can

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would be useful.

Finally, I'm, actually, there's two more things here. One of them is something I discussed with Dr. Erickson. And that is, what is the proper time weighting that should be used here, in terms of a daily sample. CASM has an average daily concentration that it uses. The actual concentration during a day fluctuates greatly, you know, consider a CASM model that had an hourly time step. Okay, if you put in a certain daily fluctuation and concentrations into that kind of model, and got a, and used it to calculate an average daily SSI for that day, what would be the equivalent concentration you'd want to put into this daily time step CASM that would create that same SSI. And it's not clear to me that it's simply the average concentration for a given day. You've got a non-linear model here. There's some upscaling that's definitely going on when you feed in that type of a measurement. So, and what you may find from that analysis is that, really, you can get away with, maybe, a two day composite. Maybe what you want is a composite sample. And maybe it's a two or a four day composite. Maybe those short range fluctuations are going to give you something similar to what you would get feeding in. You know, you might, actually, end up saving yourself,

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in terms of sampling costs doing that.

The last thing here is on the uncertainty analysis. I've got several references. One of them, specifically, I think, applies really close to home here. It's a forest landscape ecology model looking at four scenes due to global warming uncertainty. So, they've got similar bioenergetics equations. They've got similar uncertainty in the input variables like we've got here for physical chemical properties. And they've got uncertainty in the model parameters between the different species. And, so, what they're doing is, I mean, they've got methods fast is one of them, that looks at, even though there are dependencies between the input variables and the model parameters, you can come up with good estimates on what are the sources of uncertainty in the model output. And you can bracket those and come up with good, you know, confidence on what that range of uncertainty would be. So, that's it, anyway.

DR. HEERINGA: Thank you very much, Dr. Ellsworth. Yes, Bob Gilliom.

DR. GILLIOM: This topic opens up a lot of things that you could have a lot of detailed discussion on, and I'll try to put some comments in my input to the discussants on this. The one issue I just

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wanted to return to for a second that, I think, was brought up by Chris, and it's been alluded to by others, is the importance of considering each source of uncertainty and bias, intentional bias for conservatism. In the whole scheme of the approach, starting with the Brock scores in the LOC. And without saying which decision should be made, it needs to be really, clearly, spelled out where there is, and is not, a decision that effects a uncertainty factor, or a conscious choice about conservatism. And I return, just for example, to the LOC decision, because, when you look at the LOC at four, and you look at the sensitivity to short-term events, it was relatively low. I'm going from the Syngenta presentation. Those upper level choices on LOC exceedance at four weren't that affected by using the PRZM fill in method, which produced high peaks. On the other hand, if you looked at the same issue down at the lower levels of LO-, if you had lowered LOC to one or two, there was systematic effect on those values with some of the very high peaks. So, there's an interactive sensitivity going on in all the choices that starts at choice one. And then, if that's all bought off on, and you're going ahead, fine. State that degree of choice clearly, but then acknowledge, and it may change how you do the

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later uncertainty with exposure, acknowledge that, then, you're building off that, and now you need to deal with it on those terms. And that may change which duration exposure is really what you need to quantify the uncertainty end. And that's, it's a big topic, but broad advice.

DR. HEERINGA: Additional comments. I wanted to, my role as chair, Dan Schlenk had mentioned a communication he received, which he forwarded to the DFO, and the DFO will distribute it to the panel from, it appears, Tony Hawks of the National, is it National Marine Fishery Service. There was a short email message, which is part of the public docket, and then, an PDF of a report produced by the National Marine Fishery Service, which, I think, critiques the application of the CASM model. In addition, Syngenta has provided a scientific response to that critique in the National Marine Fishery Service report. I don't believe that I'm going to bring that into this discussion at this point, but I wanted to make everybody aware that those two documents are available in the public docket and will be made available to the panel fully, too. So, we're all aware of that in terms of that, so, okay.

Additional comments or contributions on

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charge question number nine. I'm going to turn to, this is probably the, at least to this point, one of the more difficult, the safety factor, or uncertainty factor questions in these panel proceedings are always the one that lead to the most ambiguous responses. And, but I think that there's some evidence that the panel, even with their extensive review, here, is struggling with some issues of this, but I'll turn to Dr. Brady and then to the scientific team here, to see if there are specific points of clarification.

DR. BRADY: Yeah, I would just ask down the table to the EPA scientists, is there any additional questions or - -

Okay, plenty of food for thought was the comment.

DR. HEERINGA: Okay, what I'd like to recommend, then, is that we're a quarter of twelve. Let's take an hour and fifteen minutes for lunch, and reconvene at 1:00 p.m., at which time, I believe, that we're going to have another presentation that Nelson is going to do, setting us up for our consideration of how might this all be applied. And that results in our review of charge questions ten and eleven. So, we'll see everybody back at 1:00 p.m. then.

(WHEREUPON, there was a break for lunch.)

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DR. HEERINGA: Welcome back everybody to the, I think, I'm confident the final afternoon session of our formal three-day meeting of the FIFRA Science Advisory Panel on the topic of the interpretation of the ecological significance of Atrazine stream water concentrations using a statistically designed monitoring program.

I think that we are at the point where we are going to begin to address the final of the three topics that were part of this meeting. Again, it's integrated, but it's been broken down into three sets of presentations and charge questions. And, I guess, Dr. Brady, if Nelson Thurman is ready to go, well, why don't you go ahead, Nelson.

DR. THURMAN: Okay, so, without further introduction, this is the last part, and this is, kind, this is very much a work in progress. In fact, there's work on my desktop back in the office that I haven't even been able to address yet. So, I'm going to try to give you an update, just kind of a reminder, we're looking at the third part of the IRAD was to identify those attributes that we use to identify where high Atrazine exposures are likely occur, particularly, in relation to exceeding the LOC.

There's been a lot of comments that

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have, kind of, started down those lines, and some, I'm looking forward to hearing more of those. I'm just going to give you some of the preliminary ideas. It's not an all exhausted list of factors. In fact, I think, as you look at them, you're going to probably recognize advice I have as being a former field soil scientist in some of the initial things I've looked at. And that doesn't mean those are all we're looking at, but it's, at least, a start. And this is one where we very much look forward to your input on this and recommendations, 'cause I think this is, we're just starting on this. We've got some ideas, but we look forward to more input. When we look at this, there's actually two parts to this question, and I think we, there's been a lot of discussions already zooming in this. We know there's an index monitoring site within that HUC 10. And we know that, at least, two sites that index monitoring site exceeded the LOC. So, one of the questions that we have is, can we identify other stream segments, or that, are there streams of the water bodies that may, also, be exceeding that.

One of the conditions of that IRAD was that, if they did, if Syngenta's monitoring did find sites that exceeded the LOC in at least two years, then

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they needed to go follow up and do some additional monitoring in that area, look for additional sites, look for sources, and there's a lot of characterization work that's gone into that. That's not part of this discussion because they're still working on that part, but that is additional information that we feel will be coming into inform us on that.

The second part of the question is, are there characteristics of these watersheds that can help us identify other watersheds within the population vulnerable watersheds. So, that's the, within the target population, and we've talked a little bit about that, those implications. I think a lot of this discussion has already occurred. I'm just going to recap here. Tony noted earlier, it's feasible in a monitoring study at a single targeted site. And, so, we do know that there are two sites in Missouri that exceeded those LOC's. And, I think, they're, actually, in three years of studying.

The next issue, and it's one important, it's important both for targeting the mitigation actions that have to be done in those watersheds, but also, identifying other stream set, segments may be similar for future monitoring studies and future efforts is, how well can we identify other streams that

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may, that may, similarly, exceed that LOC. And I'm going to start the, confine it, first, to talk about within those particular watersheds, but then, we're going to expand that beyond.

You know, the first idea, there's three possible initial approaches you could take a look at. The first one is, okay, we had a target monitoring site within that HUC 10. Let's assume that all the waters, all the streams within that HUC 10 exceed the LOC. Now, there are obvious problems with that because, and, because the selection criteria automatically excluded some types of streams, and targeted others, so that, you would, basically, be lumping in streams that were excluded because of the initial criteria. And excluded for reasons where we didn't think it'd be likely that they'd have elevated levels of Atrazine.

The second approach is to assume that all the stream segments within that water, that HUC 10 watershed, that met the initial site selection criteria would, also, be exceeding in the absence of any information. A third approach is to have more monitoring at each of those stream segments. And then, kind of, another approach that goes beyon-, starts to extend beyond just these initial sites, but in other areas, is take advantage of other, improved geospatial

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information.

Take advantage of the site specific information that Syngenta was collecting in these watersheds. Take advantage of new, things like the new NHD, National Hydrography Dataset, more detailed land use information and more up-to-date land use information, and see if we can better, better predict additional streams that might exceed that. So, I'm going to try to, I think, first of all, I'm going to, kind of, use this example to illustrate that, these approaches.

This is the Missouri O2 watershed. The sa-, the stream segment five was the stream that was, was the segment that was sampled where the index location was. Obviously, if you took the first approach, say, all right, that index site represents all the streams, the entire streams, you can see the little here, this shows where your larger stream segments are in this. The same watershed in the upper left here, you see a lot more detail, the stream segments, they were color coded based on flow accumulation. The, well, it's, allegedly, orange, whatever color that is right there, that's the 92 NLCD row crop area. So, you can get an idea that, it is an area that has a fairly intensive amount of row crop

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there for each unit, so it's not one that's just, you know, the stream segments didn't just, necessarily, hit the one isolated spot within the watershed in that regard.

So, the second approach would say, all right, let's just look at the stream segments that met the criteria. And if you look in the right-hand side, those stream segments shown in red are the ones that met the criteria. And so, that approach, basically, said, you know, they are similar in conditions to what we had here, and, therefore, you know, those would similarly meet the LOC in the absence of any further monitoring that is the likely approach to take.

A few issues on that, you know, one of them is, is that you're making the assumption that those landscape characteristics that impacted this stream are uniformly present in these other segments. And, having spent time in the field, that's probably not going to be true. You're also silent on what's happening upstream here, or even, how far downstream those exposures may continue. So, that's, to us, that may not be, that's the most satisfactory approach to do that. There is another issue that Tony appr-, talked, mentioned in the looking at these site selection of HUC's. They ended up at the fifth stream

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segment here. That means one, two, three, four, for whatever reason, they couldn't find a sampling spot. Now, if the reason they couldn't find a sampling was purely based on, there were no bridges, here, then we might still be able to assume that there are similarities.

But, if it turns out that when they went out there, corn is no longer grown in these areas, or something else along those lines, then there may be a reason why these would be different. And so, you see, there's a lot of, you know, issues related to this approach that we're not really satisfied with, and we'd like to see if there's a different way to approach that. We haven't had a chance to start looking at the detailed site information that has been collected for these areas. That's on our, you know, huge laundry list, which has grown a lot bigger after this week. But, I think, those are thing that we do need to take a look at. And, you know, I just want to say, I appreciate that, because it, these are things that we do need to pull together.

And there's a couple of things I want to point, you know, point out. In 2003, county level soils data was not available for a large part of this, the Atrazine use area. So, there was no way you could

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go that site, you know, get the more detailed county level soils data, at least, not, at least, it wasn't electronically available. There are still a few counties where that's not available, and in talking with NRCS later in that, sometime in the next year, they expect all of them to be rolled out. We've already been talking to them about, you know, the other challenge, and I'm going to mention it now, because it's, comes up in why you only see certain things in this presentation, is, right now, you've got to process that county level data county by county. And it takes a while to download it and get it processed spatially. So, we've been, also, been talking about NRCS about, you know, ways of doing this in multi, you know, pulling together counties covering, at the least, these areas of interest, but, ideally, the entire country, because there's a lot of value in that. And I think, there's, NRCS people realize that, and, I think, we will eventually get there. But that's one of the issues.

The other thing, in 2006, a revi-, an improved version of the National Hydrography Dataset, the NHD plus was released. Honestly, if this version had been available back in 2003, I suspect we would have been going this way, rather than the watershed

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approach we took. And I notice there's some nods in the back from the Syngenta folks. I think we've all recognized the value of this and the importance of this. And I think there is value in this dataset now, in terms of taking the information we've gleaned from this study, taking the characteristics that we can get in more detail, and seeing what it tells us, using the NHD Plus as a vehicle for predicting sites that may similarly have elevated levels of Atrazine. So, this is one dataset that we're really going to be taking advantage of. And I will point out, even with the improved version, you start looking at it, you know, in detail, there are still some areas that need improvement in the improved version. But it's a lot better than the earlier version that we had. So, it's, you know, as one of my colleagues who works in GIS tells me, you know, that sometimes the best thing to do in GIS is wait, and it gets better.

DR. THURMAN: And I think, honestly, we've been, because of some of the issues we've had, we've been pushing some things that have led to some improvements and changes along the line.

So, in taking a look at the second part of that question is, all right, we know that from that population of forty watersheds that were sampled, we

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can say, for instance, you know, nine percent with the confidence bound of this vulnerable set of watersheds, and as you folks have pointed out, that's what we need to make clear, the target population was not the entire Atrazine use area. It was this set of watersheds that are identified as vulnerable, based on the warp values. We can say about nine percent of those vulnerable watersheds had streams with Atrazine levels exceeding the LOC multiple years.

The other question we, we've, we had put in the original charge, which is, you know, no small feat in itself is, can we say where? Looking at that larger population, are there characteristics we'd be able to pull out of the study, and as it's been noted before, the study wasn't specifically designed to say where, but there are things that, in terms of gathering information, that we'd like to see what we can pull out. See if we can have a better idea of identifying areas where there's a higher likelihood for the exceedances to occur.

I'm going to start with warp, because that's what we use to identify that level of high vulnerable areas. The darkest blue and that you see here represents the highest vulnerable watersheds. The sites that were sampled are shown in red and orange.

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One of the things that we are looking at is that, once again, we're representing, these represent this highest vulnerable, this darkest blue layer you see here. One of the things we want to take a look at, in terms of going beyond is, first, let's focus on the forty watersheds themselves.

What similarities do we see in the watersheds that have exceeded the LOC, and what differences do we see between those and the other sites that didn't? So, there's a lot of, I think there was a report that Syngenta has recently submitted that, other than taking a look at it, a very quick glance through it, we haven't had a chance to look in detail. But they've looked at thirty-four various variables. We're looking at a number of factors, and we suspect there's more that can be looked at and hoping for some feedback there. But, are there things that we can extrapolate to the larger pool of vulnerable watersheds that may help us, based on what we learn from the comparison of these forty that may help us identify other areas with similar characteristics.

I think Tony Olsen mentioned this earlier, and I just want to emphasize. We didn't, really, and this, we're not, this is not a knock on warp. We did not expect warp to have, the warp to have

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a correlation with those SSI values. You know, for one thing, we're only looking at the upper end of that distributional warp values. And we don't have SSI values for the, for everything else. So, you know, you're looking at a small snapshot.

So, we don't, necessarily, expect that, didn't expect that to occur, but, you know, it's one of those things, that, well, we have it, let's just check. You know, you might, you know, let's check the obvious first before we move on. The two Missouri sites, just to let you know, these are the two multiple years of the Missouri '01 site, and multiple years for the Missouri '02 site. That dash line there is your four percent SSI. And you see there on either end of the scale of warp values. And this happens to be the warp estimates on the sub-watershed area, which, in effect, I think, as Dr. Effland's pointed out, it's still driven largely by use. But, you know, this, you can see, if these two sites had both been on this end, that might tell us something. The fact that they're on either end suggests that there are other factors come into play.

Before I leave warp, I just want to, I want to put this up, and Bob Gilliom may cringe at the way we did this. And it's one of these things that,

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it's a gut feeling I've had that I'd like to be able to test in a little bit more detail. In this particular instance, we zeroed out the use part to see what would happen when you looked at the other factors. You could've used the uniform use intensity and, pretty much, gotten the same value, but we wanted to take a look at where would the other factors, the other factors included warp, where would they have ended up if use had been uniform in this area. And what you see is a slight shift southward in this area. And you see, there's the two Missouri sites.

And one thing to point out, if we had stopped at use only, and selected based on the highest use intensity, those two Missouri sites would not have been in there. And this is my, you know, one of my impressions that, I think, can be tested, but we haven't done this yet. I think warp may be giving us two sets of vulnerable watersheds.

One, because use is such a large factor, in one set of vulnerable watersheds, the use is so high that overrides any of the vulnerability factors, and so that you end up having a high-, those included in the high tier, based on use alone in this. And the other vulnerability factors, the soils, the weather, and such, may not be as, that high in and of themselves.

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The second one is where, you still have high use, but it's not nearly as, it's not as high as your highest areas, if that makes sense. It's a little lower a tier of use, but your site factors are so vulnerable that they, that that becomes a driving factor and pulls that up. And that's one of the things that, I think, I want to look at evaluating here, because that may be one factor explaining these two Missouri sites in relation to the other. So, that's one thing we're going to look at on the warp.

I think there's been another number of caveats and precautions made on this. In fact, the use is one of the major factors, and you know, honestly, use is one of the factors where the uncertainty is got a high, there's a high amount of uncertainty there, and just because of the nature of the way the data's collected, what's available. So, as we look at that, we realize that's one area of uncertainty.

The other part that we note here is, what we've been doing, so far, is looking at a spatial component of vulnerability. There's a temporal component of vulnerability to this, too. And that's related to the use variability from year to year. So, that's something that we know that needs to be taken into account ultimately. And one of the reasons why,

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when we're defining that vulnerable population, we need to be specific with what that, actually, represents.

So, there are a number of other factors that we have started to look at, and once again, this is going to show my field soil science bias. Because, the first thing I want to look at is the soil related factors that's going to be mapped in the field. And, there may be things that go beyond that, but to me, that's a good way of taking a look at some things. The soil maps represent an integration of landscape factors in the field, so, I like to start there, and it may not be the place where we, ultimately, end up. These are just a few of the things that were looked at.

There are some things that crawl up the management factors that are easy to get to now, and there's some things that are not. Tile drainage is something that we really haven't found a good dataset that identifies tile drainage, and some of the people I've talked to, it doesn't really exist out there. In fact, you probably have farmers who don't know where there tile drains are, so that is, we're probably looking more at something that's going to be an indicator of tile drainage, or the indicator of the likelihood of tile drainage.

Dr. Effland mentioned your B slash D, or

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A slash C, those slash hydrologic soil groups, which may be a place to start. Dr. Novak mentioned some BMP practices. That's something that we know has an impact, and we know that, as we look at those forty sites, we should be able to get more, you know, site specific information on that, but in terms of taking a look at a broader perspective, there's still a lot we need to try to find out there. So, anything, I mean, I think we've, these are areas that, if you want to look at research, and look at some areas that might be helpful, these are things that any suggestions, advice would be greatly appreciated in that, because we know that's, that's a factor, but how do you incorporate that management factor in this is something that's a challenge. Yes.

DR. HEERINGA: Yes, go ahead.

DR. NOVAK: May I speak?

DR. HEERINGA: Sure.

DR. NOVAK: Okay, this is Jeff Novak. There's people at the County Extension level that get paid to give you that information, so, it's their job. I mean, that's available for you, depending upon how deep you want to resource that information.

DR. THURMAN: Okay, well, that's something, and you know, I see, you know, mostly, with

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a national approach, it's a challenge, but when we start looking at site specific, then, obviously, that's a source that we'll take a look at.

I'm just going to show you a few things I've done so far. I do want to point out, you know, Syngenta suggested that the Missouri 1 and Missouri 2 sites are unique, because of the clay pan, shallow clay pan, the slopes. These are things that can be tested. And so, there are ways to test that. And that's one of the things we're looking at. We think they, potentially, represent some conditions where that, a shallow restrictive layer could result in Atrazine exposures greater than LOC.

And I think part of that is because, it's not just the magnitude, but it's a duration. And I think there are some things that would lend to doing that. And so, that is something that we're taking a look at. In fact, I think the next slide kind of fleshed out, a working hypothesis to try to test along those lines. And you know, the, I think, you saw on Tuesday a figure that illustrated where you have the shallow subsurface layers. You know, in effect, what happens is, your soil storage capacity is reduced because there's only a shallow topsoil layer that can absorb the water, so you get more runoff. Well, you

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might get runoff quicker than you would in other soils that did not have that restrictive layer.

So, that may exaggerate your, the magnitude. At the same time, and I think, you saw some illustrations, where you might have toe slope contributions, some of the subsurface flow over that restrictive layer could be making contributions that would spread things out a little bit.

We've taken a look at the flow data, Missouri, the sites, and this is an illustration from Missouri '01. You take a look, you can see there was three significant flow events. I didn't plot the precipitation data here, because I need to use a different program. Excel tilts whenever you try to add more in there.

But, this is just, kind of, give you an illustration. One thing you see on this is the chemograph itself, the exposures, and what's missing here is, there's a point around 180 micrograms per liter up here, but you see this tail stretches out well beyond the spike and flow. So, you know, this suggests that something's happening that, after the flow event, something's continuing to happen there. This, also, suggests that when you start looking at, can we use, you know, the flow information to fill in, these are

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some of the challenges we need to take a look at, in terms of can we use that. But these are some of the things we looked at that suggest that this might be occurring. We need to look closer at that.

I think it was something that Dr. Effland had pointed out. An argillic horizon is probably not your best indication of a restrictive subsurface layer. As a field scientist, an argillic is basically, an indication of clay movement down through the profile, and they can occur in sandy soils as well as clay soils.

USDA does have a criteria that they use in evaluating soils, an interpretation called soil restrictive layers. And these are nearly continuous layers. It could be physical, chemical, or any number of things, you know, for instance, a permafrost, which we don't expect to find in the corn belt, anyway. But, that would restrict, not just movement, but it could restrict air, roots. So, what I want to point out here is that there is a criteria for identifying these. These criteria are, also, within the county soil, they're related to the map, soil mapping series. These are things that, we can use the county, this Argo data to pull up.

What I point here is, here is a, some

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classes of restrictive layers, and what I've done is asterisk the ones that, based on my experience and knowledge of the ones that are going to be affecting water movement, restrictive drainage layers. So, these are the ones that we would take a look at using the county soil layer. You know, one thing I do want to say is that the assumption is that all the, each of the county soil layers has carried all of these interpretive criteria in them. I think, Dr. Harbourt was talking about not seeing the argillic horizon everywhere. And as I pointed out, pulled up some of the county layers, I noticed in some of those county datasets, the diagnostic layer, such as the argillic horizon, weren't filled in. So, that may be a possibility that we do need to take a look at as we go through, and, the same thing with the restrictive layers. But it does show some promise and, you know, from a field perspective, I like to look at there first.

This, let me try to give you a context, this is the Missouri '01 site. The monitoring station is up in here. This is your Missouri '02 site, your monitoring station is here. The background colors represent the various major land resource areas. This light blue you see here, that's the portion of the

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central clay pan, major MLRA that occurs in Missouri. There is also a portion that is found in Southern Illinois, and I'll refer to that in a little bit.

What I've done is, I've just downloaded those counties that include MLRA 113. So, the counties outside of that, I have, I just, what you see is missing it because I haven't looked at those counties. And the first thing I did is, all right, just show me the depth to the top of your restrictive layer. And just as initial cut, you know, the red, here, are those where your depths in the top of the restrictive layer begins, is less than fifty centimeters, and roughly twenty inches below the surface. And I looked at those as kind of intermediate, and those that were great, yellow, greater, the restrictive layer begins greater than seventy-five centimeters below the surface. What you see in this MLRA is, there's a preponderance of soils that have as shallow depth the restrictive layer. And the interesting thing is, when you start getting off that MLRA, there still are some areas with restrictive layers, but they tend to be deeper. So, this is something that we suggest that this may be something we need to look at.

There are, there is a, you know, a higher density of soils with restrictive layers

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upstream from the monitoring sites in Missouri 2. There's not as much, I mean, there are some, but it's not as mu-, there's not as much in Missouri 1, but, just something that we've taken a look at. The other part is, okay, there's depth, but is there, what kinds of restrictive layers are there. As it turns out, within these, within this clay pan MLRA, those restrictive layers are classified as having a broad textural change, which means you have a fairly, permeable surface layer, and then you hit this clay rich, the clay pan, subsurface layer. So, that's how that's being identified. And you can see, once again, within that MLRA, and within the sites, it's the abrupt textural changes is what we're seeing there.

The blue, as you see there, are restrictive layers related to bedrock. There is some oranges in there that are described as other dense materials. I think I actually lumped those together, and I believe, we documented them in a, in the White Paper. But you, again, say, well, there is something there. I mean, it looks like a certain type of restrictive layer that is occurring shallow. I, also, point out, there are some up in this MLRA. So, there are some other areas where we're starting to see this. So, one of the questions we had is, what are we, what

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are we finding in the other thirty-eight sites? And so, once again, what I've done is, I've only processed the county soils layers for those counties that included the watersheds over sampled. So, anything around them, if you see nothing, it's probably because it didn't process the county. It just takes a while to do that. Some of this you see stretch out because it, probably, only includes a portion of that county.

What it, basically, did, the screen here is, let's look at the soil restrictive layers within fifty centimeters of the surface, and what kinds occur within fifty centimeters of the surface. The reds that you see here, that's your soils that have brought texture changes. What you see right here, this is south of this particular watershed. This is where the other portion of the central clay pan MLRA occurs. So, there is some suggestion, at least in this county, that you, also, see those shallow restrictive layers, the abrupt texture change occurring there.

The other thing is, and I know you've got to strain your eyes to see this. You don't see that shallow, abrupt texture change occurring in any of the other thirty-eight watersheds. To caveat that, the soil, the county soils data was not available for all the counties in this Tennessee site, and it wasn't

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available for that county in, for that site in Minnesota. So, at this point, we weren't able to compare those.

The Indian 11 site, there is, what you see is green, which is a fragipan. So, there is a fragipan that's be-, that's tend to occur in this area. We see fragipan in some of these other areas, too. So, there are some other restrictive layers that are, occur there. But that at least gives us something that we can take a look at.

We took a look at hydrologic soil groups C and D. In the original, both in terms of, whenever the warp model developers were looking at soil parameters, they had STATSGO, which will give you a percent of these hydrologic soil groups within the STATSGO mapping units. With the county data, you can, actually, say, map out the separate units and show me where they occur. What I'm showing here, once again, the clay pan MLRA is outlined in, kind of, a green here. And there's the Missouri 2 and Missouri 1 sites. The pink are hydrologic soil group C soils. The dark are the D soils. And then, there's a red, which are C slash D soils, and they're actually mapped as C slash D. That covers just about that entire, the entire MLRA consists of those type of soils.

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I think Dr. Effland pointed out that a lot of, some of, you know, when you see, like, B slash D, A slash C, the hydrologic soil groups that are slash, a lot of times, that's an indicator of soils that, if they're used for agriculture, then there's tile drainage that gives you that better, the hydrologic soil group. Particularly, in your central clay pan area, the soils are dominantly hydrologic soil group D. Even when you start looking around where the monitoring site was in this Missouri 1, they're either dominantly hydrologic soil group D, or C slash D in that area. So, there is something there that, you know, may be looking, focusing on particularly hydrologic soil group D soils may be something that we could take a look at.

And so taking a step back, and you see some of this, you know, right here, all I'm mapping is the C and D or B slash D. B slash D or C slash D, in this particular case, or D. And so, so, just taking a look a this, the D soils are dominant in these two sites. These are the three sites in southeastern Nebraska that we've discussed with the low flow, potentially, intermittent streams, dominantly D soils, compared to the site in Iowa and the other sites in Nebraska, which are predominantly in B soils. You see

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some areas up in here that are also C and D soils. Once again, what you're starting to see in here is the soils that are reflective of the upper part of that central clay pan MLRA in southern Illinois.

Something to look at, we need to take a closer look at these, you know, what's going on in, up in here. My look is that a lot of those were actually B/D, C/D soils, and you can't really distinguish a color too well with the slide, particularly, going from the slide to the computer to the projector. But, those are more of your C/D, B/D soils, which would be suggestive of tile drainage in those areas.

So, just based on preliminary, the preliminary, and I'll admit, most of it is mapping it on the GIS and eyeballing it and seeing what we see in this. The drainage respective layer, particular by depth and by type, hydrologic soil groups. A potential tile drainage identifier, and, you know, I was looking at the B slash D, C slash D as one approach. If there's any better approaches, we'll be glad to go in that.

One of the things we, I think, we need to look at is, I'm looking at total watershed area. There may be some value of looking at the currents of these conditions under crop land, specifically under

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crop land, which may be more likely to impact your ru-, the runoff. So, that is something to consider. That is, primarily, what I was looking at are things that are mappable. There are some other physical properties we could look at, hydrologic, saturated hydrologic conductivity is probably a fairly good indicator worth looking at. And there are non-soil factors, I will admit, that we should be looking at, but this is just where I started. So, so, that's one of the reasons why I'm looking forward to the feedback from the panel, because I'm hoping for a lot more information, and I don't mind that laundry list, by the way. Don's not here. He might mind it, but I don't, I don't mind it.

To wrap up and summarize, and kind of give you a description of what we're thinking about, in terms of evaluating, what are the potential conditions, and then where do we go from there.

This happens to be describing what we're looking at for counties, using the soils data. It is any of the spatial data we would take a look at, is first we need to relate it to the watersheds, but I think, in particular, we recognize that, and the panel has already mentioned this a number of times, you got to look at sub-watershed, and, basically, look at the spatial variation at a smaller scale, particularly,

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around those, you know, the sub-watersheds are feeding into the sample points. So, we would be zooming in on a closer level in that regard.

The idea is, first, compare those forty monitor watersheds. Look for differences that distinguishes the categories that we've had. For instance, distinguish the two Missouri sites, condition to distinguish those three Nebraska sites and the comment recommendation of determining whether or not that's an ecological condition or human impact condition, I think, is going to be very important for us to take a look at. Look at the sub-watershed differences.

Look for things that are similarity between the sites within the categories that are, separate them from the remaining sites. And then, take that, and extrapolate it to the larger pool of watersheds, looking first at the most vulnerable areas. Are there other conditions that occur in that high vulnerability center. I pointed out, there's another, there's another area of central clay pan area in southern Illinois that, at least, at first look, there are some similar soil conditions. And we need to take a look at the slope and some of the other conditions to see whether that suggests that's another area similar

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to what we're seeing in Missouri 1 and 2.

And, you know, one thing to take a look at is that, these are conditions, these are vulnerability conditions that may occur outside that, that initial 1172 watersheds that are in the high vulnerability condition, and then, you know, the question we're asking is, well, what if you do. We identify, what if we identify those in areas outside, you know. We need to figure out what kind of approach do you take there, and it may mean going back and taking look at use in those areas to see how close it is.

One of the things we've talked about, and we want to do is to provide the warp model developers with this information that's been collected from here, because I think that it's viable input for them to go back and feed back into their model, and I've talked to Bob Gilliom, and I know there, there's already been thinking about some li-, things that we've been looking at. And so, I think there's some, still some potential for that. There's some potential for using NHD Plus and this datasets to zoom in on that. But at this point, I'm going to quit talking about what we've been thinking about, and I hope to get some feedback from the panel on suggestions on where to go

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and things to consider at that.
DR. HEERINGA: I think this will be a relatively open discussion. So, rather than have a separate period of clarification, questions, why don't we just go directly into the panel's responses, and, again, if you do have a question of clarification that you want to pose before you give you your response, feel free to raise that, but at this point in time, I'd like to, maybe, ask Dr. Irene to read question ten into the record for us.
DR. IRENE: Sure. Stephanie Irene. Question number ten, while the monitoring study was based on a watershed vulnerability assessment, the ultimate value is in identifying water bodies where Atrazine concentrations exceed the LOC. One approach is to use the updated version of the National Hydrography Database in HD plus, and apply the criteria used to select monitoring locations to identify streams that appear to have the potential to exceed the LOC. Please comment on the strengths and weaknesses of the Agency's proposed approach for identifying streams within watersheds that exceed the LOC. In what ways can the preliminary approach be improved? Please recommend alternative approaches, if any, that may be suited to apply the watershed based assessments to

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stream.
DR. HEERINGA: Thank you, Dr. Irene. Bob Lerch is our lead discussant on this, and I think he has some presentation materials to go along with that that have been brought up, too, so Bob.
DR. LERCH: Thank you. Yeah, I want to start out my comments, and first, before I'm in any way critical, I want to say that Mr. Thurman's presentation, particularly, the last part of it, really moves in the direction of a lot of my suggestions under this one. But, I, also, have, because my ARS unit works in this watershed, I have, what you might consider, insider information.
DR. LERCH: And because it was one of the two that exceeded the LOC, and it was included in the White Paper, I felt I might have a little bit of liberty to use this as an example of how the existing criteria for choosing the monitoring sites would not be appropriate for looking at LOC reaches that exceed the LOC. So, I've put up here, this is the same example that was in the earlier presentation by Mr. Thurman of the Long Branch HUC 10.
And it shows the segments that would have been chosen or were chosen by the monitoring site selection. And so, I'm addressing the specific aspect

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of the question of would this criteria be appropriate for choosing reaches that exceed the LOC. And I've put up the monitoring site selection criteria up there for people to look at. I will, also, preface this part of it saying the percent flow accumulation under urban land or under crop land, I don't, I'm not a hydrologist.
I don't have a real keen feel for how that might have affected site selection. From a soil scientist standpoint, I would have, probably, chosen percent of urban land area, or percent of row crop area, but that's my simple mind at work. So, what I have here is a couple of questions that relate to this example.
In a case where an LOC is exceeded in a sub-watershed, do the proposed criteria appropriately pick other stream segments in that same HUC 10 that would likely exceed the LOC? And then, is it reasonable to infer that if a monitor sub-watershed exceeds the LOC, so does the entire HUC 10? That question I wrote before I talked to Linda Young. I'm not a hundred percent sure, statistically, whether that's absolutely correct or not. But I will say that, this example illustrates the ability that maybe you could do that. If you could go to the next slide for

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me, please, Nelson.
Okay, just a little bit of background. There's the land use from the 19-, or no, I should say from the 2004 dataset that just gives you an idea of the flavor of the distribution of land uses in the watershed, and then, the various sub-watersheds. Good Water Creek more well known in this group as MO '02, is a site we've been monitoring for 35 years. And then, there are two sub-watersheds, the red stars in there, the monitoring sites, that we have monitored within this watershed. So, we've got four sites total. One is in the upper part of the Long Branch. It's about 65 square miles. The other's further down Good Water Creek in Youngs Creek at about 70 square miles. And then, at the USGS gage, it's kind of hard to see on this graph, all the way at the bottom of the watershed. So, that just gives you a flavor that overall, 60 percent of the watersheds cropped, and it tends to be that that cropping intensity increases as you go upstream, little bit of exception at the upper part of Youngs Creek on that. Next slide, please.
DR. LERCH: And so, here's some of the data, and I also have the Good Water Creek data that was sent to me this morning, and so, I'll let you know what that looked like. But this is 2005 Atrazine data,

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and I'll also preface this by saying, I did not do any interpolation on this. I just took our data and averaged it for those appropriate periods. So, this is not as rigorously computed as you all would have done in order to determine whether the LOC is triggered or not. But the upper Long Branch site there exceeded the rolling average criteria in every one of those 14, 30, 60, 90 day. And I know that you're going to, I don't remember the designation, maybe, it's MO '05 is going to include that, and I think you can see, that was a good choice.

Then, moving onto Youngs Creek, we exceeded the rolling average on 60 day and 90 days, but not at 14 and 30, but the levels weren't trivial. And then, at the outlet to the watershed, or very near it, it exceeded, again, across all four criteria. So, obviously, at the bigger scale, it was quite integrative of the whole watershed, and also, infers that the entire watershed would have exceeded, or potentially, would have exceeded the LOC, based on that data.

Oh, and just to, since I didn't get in it to graphic, the Good Water Creek data, for that same period of time, 14 day was 7.4 parts per billion, 30 day was 8.4, 60 day was 16.4, and the 90 day was 12.6.

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And I'll also say, our data was very, very close to that that was shown in Mr. Thurman's presentation, and that's always good to know. So, Good Water Creek exceeded a rolling average only for 90 day. So, that just gives you an example of the type of data that you can get in a real world example of what this monitoring data shows as you scale up. And I, also, recognize it's only a single example, and for a single year, but I thought that it might be instructive for people to see that.

So, in my mind, if you can go to the next slide, just in conclusion, then, it would seem to me, then, the existing monitoring selection criteria would not be adequate for determining these three reaches within the HUC 10. And, at least in this example, the HUC 10 scale monitoring supports the conclusion that if a monitored reach in the AMP exceeds an LOC, so does an entire HUC 10, that might be a little strong, and I don't know if it's supported statistically, and I'll defer to the statisticians, but it would appear to me that you could draw those kinds of inferences, based on the statistical design that you have for the AMP.

So, I'll stop with that part of it. I'm done with that, and just go on to a few other comments.

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Let me scroll down here. Uh, let's see, a couple of specific things, I think there was, also, something in there about the use of warp, potentially in combination with NHD Plus for identifying reaches within watersheds. I don't think that'd be a good use of warp because of the reliance on herbicide data or usage data of unknown accuracy. I, also, think because warp is really intended to be an average annual, kind of, long term Atrazine concentration predictor, it's not going to do a good job in smaller stream reaches. I think that might be pushing its applicability, and Bob Gilliom could help me address whether I'm correct about that.

I think Mr. Thurman really hit the nail on the head, in terms of where to go, in terms of looking at other watersheds or reaches within HUC 10's with respect to the soils data, and utilizing SSURGO certainly looks to me to be the, and again, I'm biased as a soil scientist, so I tend to think about SSURGO as a very valuable tool here for identifying, in the example given by Mr. Thurman, restrictive layers. I think, really, it moves in the right direction, and is supportive of or consistent with research that I've done in conclusions that I've drawn over the years.

There are, also, some approaches out

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there. I think the original one is an index model that was developed by NRCS WINPS2, pesticide screening tool.

It has some limitations, but there are attempts, and now that SSURGO's on board, to improve risk assessment index modeling, based on SSURGO for pesticide contamination, and I'd be happy to offer up some specifics on that. But it offers an approach to integrate all that SSURGO information and bring in some of the quantitative things that Mr. Thurman referred to, such as Ksat a depth to that restrictive layer, and then what is the Ksat, and be more quantitative, perhaps, than just, hydrologic groups. Which I think is actually a good, maybe, first tier screening tool, but then, as you dig deeper, maybe, get more quantitative, and SSURGO gives us the capability of doing that.

Other useful approaches, if you have long term hydrologic data, and I would say, at least, ten years, another thing to look at, potentially, is a runoff propensity index, which was something that Paul Blanchard and I proposed in a paper in 2000. It's, essentially, just a log of the ratio of the 90th to 10th percentile of flow, which tells you something about how flashy that system is, and if you have good long term information, that's a useful indicator that

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might be, also, in that higher tier like soil hydrologic group in terms of distinguishing, at least as a first cut, large watersheds that may be likely to exceed the LOC.

The NHD Plus dataset, also, seems to have a lot of merit, and I'm, perhaps, combining soil information and Atrazine use intensity information, and I do want to make a comment on the use intensity data, as well. With the hydrologic information, because now with NHD Plus, you can have the ability to characterize flow regimes that might also lead to LOC exceedances. How, exactly, you feel about combining all those things, I don't know. I would go with the risk assessment approach, based on SSURGO because I understand how to do that. I don't have a specific recommendation on how you'd integrate flow, soil, and the use intensity. But those with a hydrologic background might be able to see a way forward with that.

I would concur, also, that the MO '01 and '02 sites represent unique conditions, or at least, such that one could justify that it's a separate strata, or that there's something unusual about that. And then, looking for those unusual restrictive layers in other watersheds makes a whole lot of sense to me.

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My last comment goes to the herbicide usage data. I think, particularly, if the intent is to look at specific stream reaches within HUC 10, we really have to have better herbicide usage data that reflects changes year to year, as well as, watershed specific information. Farmer surveys can be one way to get that information, but it's awkward and it's slow. Another way to go about this is herbicide sales data, and you might consider requesting that of the registrant. The state of Iowa has a program in which they use sales information, and they have a defined procedure that could be followed to convert sales data to herbicide usage data. I believe they're the only state I know of that is doing that, but there is an example out there to follow, if, in fact, sales data were available.

Yeah, and I guess, I would just say, in relationship to that, it, and this is the point I think that was made earlier, not necessarily today, but maybe it was on Tuesday, is that, because these sub-watersheds are what were monitored, we don't really have a good handle on exactly what the herbicide usage was in the monitored sub-watersheds. And that kind of goes along with the fact that we need better, more specific herbicide data. And that's all I've got for

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that.

DR. HEERINGA: Thank you very much, Dr. Lerch. Next associate discussant is Dr. Gay, Paige Gay.

DR. GAY: Paige Gay, University of Georgia. I think that Dr. Lerch has pretty much covered my comments in much more depth than I possibly could. It's nice to have someone familiar with that area to give us an expert view of that. I did, also, note that herbicide usage, if we're going to look at sub-watershed levels, would be pertinent to try to get a better handle on that.

The NHD Plus database certainly will allow so many more parameters that I felt were kind of missing in the assessment that we've made concerning soils and hydrology to be incorporated into the assessment when trying to make some comparisons between sub-watersheds and determine likenesses or not, dissimilarities. So, I'm just not real sure how you would link that together, either, if you could incorporate some of those hydrologic and soil differences into your criteria, in between your, for selecting your final sites that are most vulnerable. I just, really, wasn't real sure how that would happen.

I guess the major weakness that I saw

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was that it's just time, money, and manpower. That's just too much of, data available to manipulate, but it's wonderful.

DR. HEERINGA: Thank you, Dr. Gay. Bob Gilliom.

DR. GILLIOM: The result of, kind of, restatement of the problem out of the stratified design was to estimate the frequency of occurrence of HUC's that had one or more sub-watersheds that exceeded the LOC. My understanding of the future need to go ahead is that restated goals to estimate the number of stream miles that will have exceedances of LOC, I think. We can address that in a moment. If that's a goal, then my idea for how to get there is a number of steps that tie together some of what's been talked about.

The first step, I think, would be to get to the, move to the NHD Plus, like's already been discussed. And then, just drop, from an analytical point of view, the linkage or constraint of going to the HUC 10. Each of those little basins is now it's own little guy out there on his own, and he's going to be in the NHD world with all these other basins. And they all have their own characteristics and everything. And then, in that framework, update all the possible causal factors that have already been discussed that

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you can get to improve the scale of information, SSURGO, more current use data, all those things, and there are, there's new land use data coming out this year. There's a lot of readily available, some of them easy marks, to go at, and then some that become more difficult on a more basin level.

And then, with that in hand, I would prefer to see, for this step, the LOC restated in terms of concentrations for the moving averages, so they can be more transparently related to cause effect regression analysis. You could do it with the Steinhart similarity index, and that's your independent variable, but I'd prefer to see it go back to, what was derived the model anyway, I believe, were these specific numbers for certain moving averages, that match the same SI values, basically.

At that point, I think, you want to expand the target area to the corn belt, and pick up all the other monitoring sites you can at different scales. Because, what we're aiming for is to, basically, refit a new, make a new work model that's designed for the region, and doesn't get thrown off by all the other regions around it, 'cause you're targeting the corn belt system, and tune in that fit, so you can give a more accurate probability type

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assessment of concentration conditions in this region, without having to deal with all the rest of the country, like was done in the main version of warp that we've been talking about.

I think, by doing that, and fitting the model, specifically, to the target that you want, either the specific moving average, or the end score, if you want, you'll get a lot better idea and a, basically, a probabilistic answer of what's the probability, given these conditions, that any particular stream reach is going to go over whatever LOC you define. And I think that will work pretty well, and, actually, when I see the correlations just presented in a preliminary fashion of looking at the small sub-sheds, compared to the bigger watersheds that were in warp, originally, you know, that plot we've looked at, that's, actually, well within the uncertainty bounds of that model. It's got kind of a plus or minus order of magnitude concentration thing. I mean, these, all those were less than a factor of ten off.

And within this upper range defined of the 80 upper 20th percentile, it's not surprising at all to have those kind of outliers. The air distribution in this regression model's log normally

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distributed, but it's fit, it's a model fit to the, you know, a transform of Atrazine concentration.

So, you expect to see those high outliers. Now, the reasons for them can come into play with things like improving underlying causative factors, like this certain soil condition or drainage or whatever. So, I think if that process was done, it will enable comparing a couple different approaches going directly to SSI, and, also, going directly to the moving averages, and see how those work out with their comparability in producing level of concern estimates, and it won't take that long to get there to find out whether that's a productive approach or not.

And it would be a region specific, retuned, regression analysis, which is all warp is. It's just a step-wise regression model. So, there's no, it's not like CASM, which has a lot of stuff in it. It's just a step-wise regression model. So, that'd be my suggestion.

(WHEREUPON, there was a brief recess.)

DR. HEERINGA: Thank you, very well organized. Doctor Schlenk.

DR. IRENE: Doctor Heeringa

DR. HEERINGA: Oh yeah?

DR. IRENE: Stephanie Irene. Can I just

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ask a clarification --

DR. HEERINGA: Sure.

DR. IRENE: question? Bob, you mentioned originally about stream miles. Is your approach, I'm not sure that stream miles are where we would be going and I'd like to ask the A Team if they have any comments on that.

But is your approach applicable just to watersheds? Because I think we would be ultimately regulating our watershed basis.

DR. GILLIOM: I'm certain I've seen in the white paper the stream mile objective. And it was talked about.

But that's neither here nor there.

I'm totally talking on a watershed basis. In fact I'm still a little confused over the prioritization of the segments in the sub-watersheds because you'll have a segment colored red for priority but there's upstream watershed that's colored light blue. I don't know what that means.

Everything I talk about would be watershed based. And it can be translated into either whole watersheds, segment watersheds or stream miles.

DR. IRENE: Yes. When I was listening to your discussion I kind of assumed that it could be

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applicable to just watersheds, but when you started out saying stream miles you kind of threw me there for a minute.

DR. GILLIOM: Well I think it tends to be an interesting, a useful way to summarize the extent of the resource in different categories. Because if you say you have so many watersheds, well you have to say exactly what size category are watersheds and they're all nested.

It gets really messy and this whole problem is scale dependent with a lot of confusion.

So I'd rather hear about stream miles in a certain order stream category or something and think about the resource that way, even though all the analysis of cause and effect is watershed based.

DR. IRENE: Thank you for the clarification.

DR. GILLIOM: Yeah.

DR. HEERINGA: I think that's useful, that quantification, that step and it's one of the pieces that a number of us, you know, I think we all realize we're dealing with units of varying size, that we assume varying degrees of homogeneity within those systems.

And this is one way of sort of

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quantifying the total extent of sort of community exposure of community potential I guess.

Doctor Schlenk.

DR. SCHLENK: Dan Schlenk, UCR. Just to let the panel know, in discussions with Doctor Lerch we kind of came up with well, he came up with pretty much most of anything that I was going to add.

The only thing I would add in sort of support is that from what I could understand of the database when going to the website and looking at the advantages, it looks like a no-brainer, I mean this is amazing stuff that you can use, particularly with the low flow and the limited flow type of area.

So I would definitely with agree with the utilization of that in site selection as well.

DR. HEERINGA: Doctor Lerch, a question. You showed measurement data from a watershed in Missouri. You had four monitoring points. I presume you have a number of these monitoring locations or is it just unique that you were here or is there maybe 20 or 40 others?

DR. LERCH: I'm glad you asked that question. It just so happened that we had been working in Goodwater Creek, the research unit I'm a part of for 35 years.

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A project called the Conservation Effects Assessment Project came along about three years ago that is essentially a cost/benefit analysis of the conservation portion of the Farm Bill. And as part of that we scaled up our monitoring to the entire Salt River Basin which includes the bulk of the Major Land Resource Area 113, so we have I believe 13, not 40 sites that we're monitoring.

DR. HEERINGA: It's very useful because personally had not seen any data from the larger HUC that would suggest that sort of relative degree of homogeneity. And that may be unique but again across 13 other locations it would give certainly some sense of just, you know, what sort of penalty are we paying when we scale from a set of observations at a bridge to a much larger growing area. Yes?

DR. LERCH: Yeah, Bob Lerch. And that's why I wanted to show that, was to illustrate the fact that at least for this watershed and for that year's worth of data, that, you know, in hindsight I recognize.

But part of the reason we did a multi scale was we wondered what the scale dependence of the atrazine concentrations would be, and I thought it would be instructive to share that.

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DR. HEERINGA: Bill, did you have some

DR. EFFLAND: Yes, Bill Effland. I don't' know how I ended up sitting next to Bob, I guess it was just by random chance, but the project that he talked about, the Conservation Effects Assessment Project is something that I've been working on for the last year, basically since January.

It's older than, it's older than me, it's a few years older than I've been working on it, it's about three years.

And but we were talking at lunch and I asked him, I said are you working on one of what we call the SEAT watersheds? And he said, oh yeah, I'm working on this one in Missouri and I said, oh, that's pretty interesting because I'm working on one in Puerto Rico which is very lucky for me.

But the in some ways anyways but the point is that because of the enhancement of funding to the conservation in the Farm Bill, Congress has asked us to, you know, justify if you're spending this much money on conservation practices, what are we seeing in return?

And so that's one of the originations of CEAP and CEAP is actually three components, it's a national modeling assessment. And the major agencies

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within USCA are ARS, NRCS and CSREES, extension, former extension now combined with the Cooperative State Research.

And then we also have a lot of other partners. We have EPA working with us in some watersheds. We have USGS working with us. We have NOAA. The particular watershed that I'm working in in Puerto Rico, we have NOAA because we're linked directly to the coast, which is something unique about the Hovis Bay Watershed Project.

We actually have almost 40, we have 37 I believe, 37 different projects around the country. Not all of them are involved in pesticide analyses, but quite a few of them are. On the one that I'm working on that we just started in Puerto Rico, we're looking at both groundwater transport and surface water transport of atrazine. We have some, actually have some data from USGS monitoring wells that have been in stalled and where ARS is installing more.

So there's a lot of opportunities there. But at the same time there's also some cautions because for us to do these studies there's concerns about regulatory effects, you know, the regulatory impact.

And so I just bring that up, but there's a lot of data sets. And then within ARS what we call

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their benchmark watershed studies that have been going on for decades, tremendous data sets.

And if you can align some of that data or some of those sampling locations with some of the things you're doing, I think there's some real added value there. And it's something to consider. A lot of background information, I mean some more detailed information.

And we're, you know, in some cases we're doing some very intensive surveys of producer information and those kinds of things. And that's where it starts to get the confidentiality is, you know, very critical.

So we've got the national assessments which are essentially a very large modeling effort. And the we have the watershed studies.

And then there's another component that you may be interested in. And that's the literature reviews that we've completed for a variety of different things. And these are all available on the web through the National Agricultural Library.

But if just type in CEAP, C-E-A-P, I'm pretty sure it'll direct you to the NRCS website and then from there you can drill down and move around and find some other ARS connects through there and the

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CSRES and USGS and some other groups.

And, you know, that's just a real tremendous resource and I was glad when I talked to Bob at lunch and he, oh yeah, I'm working on one of the watersheds and I was, oh, that's another one that I and I've visited some of these watersheds, not all of them, but I think it's something to consider in helping with addressing some of the questions that you have.

And to see this kind of data it's just, for me it's very exciting to see that linkage there.

DR. HEERINGA: Thank you very much, Doctor Effland. Doctor Young?

DR. YOUNG: This has been a really interesting discussion and follows along with some of the lines that I, I've been thinking about.

There are a few things that I just want to build on.

I think it's first very important to decide what you're going to use as your basic unit. Whether it's going to be one of these sub-watersheds or a total watershed or what, because you've got to set your scale.

And then there are immense amounts of data available and all kinds of GIS coverages. And they are usually on a whole host of different scales.

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So the first challenge is to get everything on that same scale in a statistically valid way.

So consider the change of support issue and to get that under control and then it's, in listening to Bob talk earlier, I'm not quite sure what your response variable would be. I agree with a regression type approach.

Whether you want to just model the probability of exceeding the LOC or estimate the LOC or estimate the SSI or what it is you're going to try to estimate, that needs to be sorted out. I would tend not to go with just above or below because you may want to change that above or below, you know, what that cutoff is.

And then but begin to get a model where you, based on your current data, you can estimate the outcome for a particular unit, whatever that unit is.

Then you could begin to do some ground truthing, some targeted sampling to validate the model to see whether under the conditions that you anticipate, you really are getting an excess amount of atrazine or not and begin. And of course you'd have to do it in more than one place, a one shot deal, you know.

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But nonetheless you could begin to focus, do focus sampling to do model validation where needed, or maybe supplement it with some of the data that are already being collected at the state.

But how to great care has to be taken I think is just assembling the data, to be sure that you have a common support and that, then what regression does actually makes sense, that you decide to do makes sense.

DR. HEERINGA: Thank you very much, Doctor Young. At this point I guess oh, Doctor Chu, Michael.

DR. CHU: I think using NHD Plus is really great because from this database we can get a lot of useful information about the hydrology, you know, the stream flow network, the stream network, flow directions and accumulations, all useful information.

But I'm just curious, I'm just wondering if you have any plan to do watershed scale hydrological modeling, because I have this question because it seems to me we have all the information or data for hydrological modeling.

For example GS soil data and land use data. Also you're talking a lot about hydrological soil groups, hydrological soil groups. That means that

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actually we can use a very simple method. For example a method to do hydrological modeling. In this way we will have all the information about silt runoff distribution for each sub-basin.

Also, we also can generate hydrographs for all sub-basins.

So I'm just curious about if you have this kind of a plan. Thank you.

DR. HEERINGA: Doctor Chu, so I can follow this too. The hydrologic modeling gives you the flows and you would have to marry that to the atrazine applications presumably and weather events, is that

DR. CHU: Yeah, Michael Chu. If we have the flow information, for example, silt runoff for each sub-basin, distribution for each sub-basin and also the flow at an outlet of each sub-basin. In this way it will be very helpful for us to do the water quality analysis, assessment, yeah.

DR. HEERINGA: Nelson, do you want to --

DR. THURMAN: Well although it's not specifically related to atrazine, but yeah, we've been looking at approaches for hydrologic modeling, watershed scale modeling. You know, a lot of our pesticides assessments are based on screening, screening level, exposure estimates and a lot of it is

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using a PRZM which is, as you know is a field scale model and then emptying it into a water body model by exams.

So we have been looking at, particularly as the data has progressed in terms of what we can do in terms of water scale modeling.

It's separate but, you know, it's kind of separate but after, at some point all the data started, all the information starts merging together. But it's on our list of things.

DR. HEERINGA: Okay, at this oh, Doctor Grue.

DR. GRUE: I think I would just ask a question, back on slide 184, what's the N there, what's the basis for that graphic? Yeah, on the printouts.

DR. THURMAN: The N is the number of sample years. So I think that N is 86.

DR. GRUE: Okay, thank you.

DR. HEERINGA: Yes, Doctor Olsen.

DR. OLSEN: Yeah, I've got a couple of comments actually, mainly generated by Bob's comment earlier and other people talking about NHD Plus.

I use NHD Plus a lot, not necessarily the full accumulation part, but for other parts of the survey design things. NHD Plus is great, but it's not

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perfect.

One of the things that you will see, and I know this happens along the Kansas/Iowa border and there are certainly other parts of the country where it happens as well, is you'll see different densities of stream networks in two basic quad maps side by side. And it could be at least half the density or even more.

So when you end up looking at these things you've got to be really careful, you've got to look, you've got to be aware that that occurs.

But I do agree it's a great enhancement.

I guess one of the questions is, is in some sense going down to using NHD Plus and sort of using the, let's call them the sub-sheds with each segment that are identified in there, you really have changed the scale down from HUC 10. And you only have one stream segment within that.

And if you were going to monitor at that level you'd probably monitor at the outflow at the downstream end of that. And the assumption you would be making, if you wanted to convert it to stream miles, would be that that entire segment is above the level of occurrence. That's probably what, the sort of assumptions you might make.

You could make the assumption to measure

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the upstream portion, but that's probably less likely to make that decision.

So essentially in some sense you've simplified the problem of going from the HUC 10 to the sub-shed but you haven't gotten away from it entirely.

But it does simplify greatly.

The one Linda made a comment about sort of choosing the sort of the scale that you want to deal with. The segments in NHD in some sense were at the discretion of the mappers because you can see that in some of the stream segments up there. They're broken, you know, there's not an intervening stream, intervening stream intersection coming in. But the segment's broken into two different pieces.

It could be because they're in two different quad maps and that's the only reason they're broken there.

So there's, there's some things like that that goes on. And they are, the watersheds there are very different, can be quite different in size just because of the mapping density differences, let alone different parts of the country. Although the corn belt is a little more similar.

So you're not going to be, you're still going to have some variation in these watershed sizes.

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The other part is, is in thinking about doing, pulling together the data for this, you certainly haven't gotten away from the issue of many of these sub-sheds have water flowing into them. And there are very few of these that are not, they're not headwater streams.

And so when you're going through and you do the modeling, you know, do you want to accumulate data? Certainly you want to do it for the sub-shed itself, but you may end up also wanting to accumulate everything for the entire watershed above that as well, which means that you're going to have varying sizes of watersheds. And we may end up having to control for that in some way.

And I don't have a solution to that, but you have to do that in some sort of a way.

In terms of accumulating the data for this, let's say like the soils data, the natural way of doing it is, you know, creating a summary measure for the entire watershed or the sub-shed, so the percent seedy soils for example.

Is there any utility in terms of accumulating that data in terms of buffer strips around the stream? So, does it make a difference whether you've got 100% CD in a 50 meter or 10 meter buffer

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around the stream versus the entire sub-shed? And I, you know, I'm not a soils person at all, but I know in some of the things that we've done in sort of the landscape modeling, buffering sort of the riparian zone seems to help in some of the modeling efforts.

I guess we've struggled with the idea of the atrazine use data and if you wanted to put atrazine use data into this, we certainly have gone below a scale of most of the atrazine use data because it was county level data and that sort of stuff. But that's not good on the entire area, you're not going to change that too much.

So the importance of dealing with sort of a, you know, the misaligned data modifiable MOP problem is going to become very important. And there's going to be still a lot of uncertainty in the use data there. And that's a nontrivial modeling exercise to get down to that.

One of the things in terms of thinking about sort of building a model like this and Bob mentioned using something maybe other than the SSI deviation as sort of let's say the response variable in the rolling average value or something, one advantage of doing the rolling average is, potentially is, is are there sufficiently other sites from other monitoring

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programs where constructing a rolling average is feasible and you could use a broader subset of data to do that?

I don't know whether that data exists well enough to do that or not.

Obviously there was a hundred and some based upon what GS did for the work modeling, because that then gives you a broader set of data than just the 40 watershed that we've got. Because if we have to, if we're basing it on the SSI deviation, at the moment you've got 40 watershed, they're at the upper end, if you include some other watersheds the question then is, do you have enough data to actually run a CASM model to get an SI deviation in some other area without doing additional monitoring.

And I don't know the answer to that. But that certainly is one of the things that would come up.

But actually that was a great, you know, sort of a great sort of sequence of ideas. And that's it.

DR. HEERINGA: Thank you very much, Doctor Olsen. Doctor Robert Gilliom.

DR. GILLIOM: Well I wasn't sure how you want to address any of do you want to get into any of

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those issues or just leave it on the table? I'm open either way and we can deal with it later.

DR. HEERINGA: Well, you know, I think it's worth getting into it if you feel there's additional I mean if it's just an academic discussion at this point as opposed to, you know, sort of, I think the ideas have been brought forward and I think given Doctor Olsen's response there's clearly an understanding of where the issue llie.

And if you felt we could offer some more clarification

DR. GILLIOM: Well let me mention a couple things that fit into I think the earlier question that Doctor Irene asked too, which is kind of the segments and scale in watershed and things because it's kind of important to how you frame it.

I mean what the nature of the watershed based regression model is, is that it treats each point as having a watershed in its complete entirety upstream. And it's estimating what you would see at the bottom of the watershed.

So when you're in the first headwater watershed, how you assign that to steam miles is kind of a, kind of a rule making call. How are you going to do it to everything up to a certain point, okay? Then

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as you move downstream, a subsequent downstream point has its new big watershed. And what normally would be done in this kind of marching down the network case is that that intervening segment between your upper watershed and the next watershed point, would get assigned the predicted concentration for the downstream point.

So you can do it whether you do it by watersheds or stream miles, the physical structure is there to make some choices.

On the spatial issues of improving things like treatment of land near the stream, the buffer idea, all that, that's one of the critical things that several people have mentioned about testing with the improved data, like the SERGO data. And I'm not sure where the answer will be. I really don't.

On the pesticide use data front, I think some of the ideas that came out and were talked about in the Syngenta presentation about using, at least going to current annual data, the crop reporting districts and attributing it would be a better thing than what we have now to go to the regional prediction mode.

For what you do in the model development mode, which is more critical to model building and

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actually have more accurate data for the actual basins you're calibrating to, then you have these cost/benefit choices to make about how important it is to get actual data for that basin rather than use the more national remote control data.

And that's a decision based upon kind of an uncertainty analysis and cost/benefit of effort in the study.

So that whole sequence of decisions will take some time to work through. There's no question about it.

And I think the scale question is really important to this problem. And in Bob's example that showed the nested scales, you know, you show that you can get these little basins that have their characteristics and they're more variable within that HUC. And then downstream as the concentration duration changes, you actually get more critical conditions because the curve smooths out.

And we're probably going to find that there's these intermediate scales that have the most frequent issues with LOC because of the concentration duration curve. And then as you get way downstream it smooths out, so it's not an issue. You go way upstream it's so peaked that it's not. And it's probably going

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to be some mixture of these intermediate scales.

So with that said, there's more to talk about, but I wanted to respond to a few of them anyway.

DR. HEERINGA: Thank you. Jeff Novak and then Doctor Ellsworth too.

DR. NOVAK: This is Jeff Novak again. This is just a question before I provide some advice. But to Doctor Olsen and Thurman, are you planning on acquiring the soil survey maps along the stream channels of your HUC's?

DR. THURMAN: That's and by that you mean the county soils?

DR. NOVAK: Well yeah, yes or no, are you planning

DR. THURMAN: Yes.

DR. NOVAK: on overlaying the soils map where the impaired streams are?

DR. THURMAN: Yes.

DR. NOVAK: Okay, that's a valuable piece of information. Now you mentioned that you're a soil scientist, you've spent much time in the field. I'm sure you've walked streams, you know soils that are mapped as fluvaquents.

Okay, so you know what I mean, these are entisols that are mucky along the stream channel.

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They're so wet that they don't farm, but yet they're wide enough that some, in some counties they're mapped as a recognized soil map unit. They don't give them a name, they call them fluvaquents.

This is potentially good information for you to get in either an area size, the extent of the buffer, these areas contain vegetation. Most times they're not farmed, they're not disturbed.

So if you had that overlay and if you had an area that had these wetland areas, riparian buffers, those are the natural ones. So that would be like your first attempt to catch even an area using the soils map. You'd have to work with Mr. Thurman on that and I'm sure he has the background from how he's smiling there.

I know I've spent many times trying to keep the gnats and the snakes away from me, and down in my area it's the gators.

The second question I have is that in terms of the buffers themselves that are going to be manmade, that's a little bit more of a challenge that you probably will have to either get from aerial maps or from some of the ground truthing. And I know this is only one of the six or four questions that you asked, but that's why I wanted to know if you were

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going to utilize the soils information.

Thank you.

DR. HEERINGA: Doctor Ellsworth, Tim

DR. ELLSWORTH: Tim Ellsworth. Real quick, two points that kind of are coming up here is there's, so there's these different scales, there's physical processes going on, kind of physical scales and then there's these management scales.

One of them that's coming to mind is rainfall. You know, summer rainfall, the spatial distributions are highly variable. You have, you know, and you've got that interacting with where the corn acres are in one of these little sub-watersheds. You go down too small, you know, and you're going to increase your variability instead of actually increasing the power of the regression, you're going to lose it.

I mean there's like an optimal spatial scale there and there's management scales in terms of what the information that's coming in is. All right, so just the fact that there is some kind of

DR. HEERINGA: It's a good point. At this point in time what I'd like to do is I'd like to oh, Doctor Olsen.

DR. OLSEN: Tony Olsen. Just one brief

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comment, you know, to Tom's question.

In terms of the buffer zones I mean you brought up the point of actually finding out what the real buffers are that people are installing under best management practices and other things. And that would be valuable to do.

I was also thinking about doing something fairly generic of basically just saying, hey look, what's within, you know, 30 meters of, you know, of the stream, just sort of doing the buffer just as a rough guess as to what sort of soils are nearby the streams.

If soils nearby the streams matter a lot versus soils further away from the streams then because one of the things I saw up there on the map of the Missouri sites where we had all the red sites showing up, what I wanted to have, Nelson immediately put up there and I hadn't really thought about this until then, is I wanted to see the stream network because I thought I saw it.

You know, so it seems to me that having, keeping track of things around some zone around the streams may be useful. Not to exclude the other ones as the entire thing, but as something that might actually have a better chance.

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Thanks.

DR. HEERINGA: Doctor Novak.

DR. NOVAK: This is Jeff Novak again. Yes, that's a very good approximation for there.

But a person needs to be aware that not all streams are going to have buffers next to them. So you're again, in your hit and miss operation.

Now Nelson, you spoke, you have a laundry list. Well I've got another thing for you here.

If you look at these maps, aerial maps, from the color of the vegetation you can tell if there's forest up to the stream channel, you can tell if there is a pasture up to the stream channel. Now, depending upon what state you're in and what watershed and on what parent material you're in, the farmers will try to plough all the way up to that stream channel.

But because of the setbacks, they're required to have in some areas up to a 50 feet buffer, most times that's going to be in a grass water, or grass type vegetation. Some farmers don't like trees.

So Nelson, I know you don't want to hear this, but it's probably going to be where you might have to do this by maps and looking at each individual stream channel.

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We all need paychecks, thank you.
DR. HEERINGA: I think we all need a break too. At least I do.
What I'd like to propose is I think this has been a very healthy discussion. We've made good progress.
Let's take fifteen minutes and in a little over fifteen minutes come back here at five minutes of three and we will return to the Charge Question 11, which is the final question. And we'll do a wrap up and I expect we will be done by 5:00 or before.
So again, say five minutes of three, let's be back.
(WHEREUPON, there was a recess).
DR. HEERINGA: Okay, welcome back everyone to the final half of the afternoon for I think our final day of formal face to face meeting of the Science Advisory Panel.
Before we move on to Charge Question 11 I think there are a few additional comments that people wanted to get in on Charge Question 10.
Doctor Lapoint and Doctor Grue.
DR. GRUE: I held off on making this comment simply because I wanted to check with Tom to

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see if I was in part on target, and I may still be off target and Tom doesn't accept any responsibility for that but, or shouldn't accept any responsibility for that.
But, you know, looking at the information we were provided by Syngenta, I think, I can't remember if it was early this morning or yesterday in terms of the influences of peak water, the peaks and atrazine concentration relative to the SSI, the comments this afternoon about the ability now with the new data, the soil data, and as Doctor Chu suggested, the hydrographic information.
I was just toying with the idea, is it possible to incorporate that information to, with precipitation data to actually predict what those peaks would be for sites that have not been sampled? In other words, can you model that?
You have peak data now, both from auto samplers and from graph samples. Realizing the biases that Doctor Ellsworth has pointed out in terms of, well, you might be a little bit low on the, or some percent low on the, on the peaks.
But is it possible actually to model that? Is the information spatially, of sufficient spatial resolution that, you know, as you move forward

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out of this, these 40 watersheds, is it possible based on the enhanced soil data, the other factors that go into the vulnerability index, I'll just call it that, is it poss and the information that we already have on peaks and the drivers for that, is it actually to develop a predicted capability to assess what those exposures would be, to be able to run that through CASM and actually assess or make some predictions in terms of what the effects might be?
DR. THURMAN: The simple answer is yes. It is possible. It depends on how much there is going to be a degree of uncertainty involved in that and the ground truthing.
I mean honestly our risk assessment now, we do that at a screening level, but it is something and generally at a field scale, but as I've mentioned we've been looking at watershed scale approaches.
So, yes it is. And it's something that we hope that we can improve upon the methods as we go along.
The value of atrazine as the wealth of monitoring data you have for comparisons, so you know, it is something that can be done and a direction that could be taken.
DR. HEERINGA: Doctor Lapoint, do you

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want to add something?
DR. LAPOINT: Well Doctor Grue said it all. I mean we've talked a little bit about that, but I really think because he and I were thinking about this after we got this handout.
And I think it would be quite good for looking at, again the kinds of watersheds, or I guess as we've been defining them now as sub-watersheds, and looking at rainfall data. And it could be even ground truth with some of the data you've already collected. You know, because we know what's happened in the past. And it might be a good way of screening some of the more, some of the additional areas that haven't been looked at.
DR. HEERINGA: Thank you very much. At this point I guess I would like to move on to Charge Question 11 and ask Doctor Irene to read it into the record, please.
DR. IRENE: Stephanie Irene. In order to identify areas beyond the 40 sites where higher atrazine exposures are likely to occur, the Agency must determine whether the watersheds that exceeded the LOC in multiple years are randomly distributed within the 1,172 vulnerable watersheds or represent a unique subset of conditions.

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If the latter and the conditions can be identified, monitoring could be focused only in watersheds where those conditions exist.

The Agency has proposed evaluating more parameters and other sub-watershed soil and hydrologic properties to determine the extent to which the monitoring results can be used to identify other water bodies exceeding the LOC.

To what extent can WARP be used to identify other watersheds of concern? Give the influence of atrazine use and vulnerability and exposure, please comment on whether the extrapolation should be limited to the original 1,172 watersheds or include a broader use area.

Please comment on the soil and hydrology parameters the Agency is evaluating for extrapolation to vulnerable watersheds. What additional soil and hydrologic parameters should the Agency consider? What additional approaches to the identification of watersheds that may have atrazine levels that exceed the LOC should the Agency consider?

DR. HEERINGA: The first part of that we've had some discussion of already with Doctor Gilliom's presentation, but Doctor Effland, let's hear your initial overview response.

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DR. EFFLAND: Okay, thank you. Bill Effland here. The interesting thing about the WARP model, and I'm going to actually, I'm going to blend my response between a couple of questions here, I think because we're getting to where there's a little more integration and consensus.

To me the WARP models are regression models, so the data that went into the model helps to define the parameters that are used as far as the coefficients of the, for the different variables.

And in a former earlier career, I did some regression modeling of another variable. And one of the concerns that the statistician that talked to me about my work was that, you know, how transportable is this? As far as the coefficients go, how can it be applied in other locations?

Because the data that I put in was for a very limited geographic region. And with the WARP model of course it was for the entire United States.

And so I have some concern about just using the WARP model with the current coefficients, but bringing in new data, especially new use data since that seems to be an important component.

So I guess I would be cautious about using that particular model. Maybe if the model

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developers, if USGS and folks are willing to, willing to go through the analysis with the newer sources of data and generate new coefficients for their regression equations, then maybe it would be more applicable.

And the statisticians on the panel here can probably clarify that again. I'm mostly dangerous with statistics in most situations.

A couple of things that I want to bring up that were brought up in Bob Lerch's presentation, but I think also apply here, one of the things is you keep talking about vulnerability, and I've never really seen an explicit definition of what vulnerability is.

And so I wonder, and I think it would be worthwhile to explicitly state the vulnerability because in thinking about the fate and transport of atrazine, we can have vulnerability related to runoff or we can also move atrazine through subsurface flow, either in a tile drain system or through groundwater flow. Atrazine has been documented to move in the groundwater in tile.

And so are you talking about runoff vulnerability or are you talking about subsurface flow vulnerability, or are you talking about both?

And so I think that there's a need to clarify that term so that it's explicitly defined. And

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I've kind of struggled with that back and forth in looking at a lot of the material here, so I bring that up.

And then I think maybe one of the ways to look at that is, and I think I heard this from the Syngenta presentation, they've conducted PRZM runs for all of the sites that you've been working with. And so you have some, you have some model information on runoff, so you can get an idea.

And one of the reasons I bring that up is it also points to some questions I have about the chemographs. You can have a, you look at your precipitation data and you can have a precipitation event, but you may not necessarily have a runoff of event associated with that precipitation, depending upon intensity and duration of the precip, timing of the precipitation and then your landscape characteristics, slope and other things. And then maybe there are some best management practices or some other things going on in that particular and I'm going to focus just on the sub-watershed scale because I think that's where you're really looking.

And so you have to look but I think it's worthwhile to go back and look at those PRZM runs as far as, you know, looking at that versus also

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looking at the precipitation that you have and helping to determine whether with some of your sites, did you have a runoff event and that's what contributed to the atrazine that you saw.

Or maybe, you know, maybe you actually had application of atrazine, you didn't have a runoff event even though you had some precipitation, so you didn't see any concentrations in the stream.

And so that's something and the reason that that came to my mind was I was looking at the collection of chemographs, and in some of your chemographs it's pretty clear that you get a runoff, you've got a runoff, you've got precipitation, a runoff event and you're getting a detection. And in others you're not or you're getting somewhat of a it's a fairly short delay but it seems to be somewhat of a delay.

And then also if you look at some of the other chemographs you get that and I think that question came up earlier, you get a late season peak, and it's only a few of them. But it, you know, brings to mind, you know, what's unique about that particular site that with the majority of the sites you get your, it looks like you get precipitation events and you get runoff associated with those and then you get a

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detection of atrazine in the stream.

But then in some cases, you know, several months later, it looks like normally July, August and I think the one that I, just the one that I picked out in random, sometime in mid-August you get a peak of atrazine there.

So what is different about that particular site? And I think that also leads to another comment that I'd make is, have you thought about looking at these watersheds and seeing what characteristics are similar versus what characteristics are different as you have a lot of site information, you have slope and you have a lot of information about the cropping distribution, the cropping information.

So I think that you can tease some more, I think there's some more information that can be pulled out of the PRZM runs that have already been conducted. And maybe you're going to need to do some additional PRZM modeling. And then also looking at the, looking at those chemographs and doing some more analysis of those.

And I haven't heard a lot of discussion about I mean the chemographs seem to be fed into the CASM model and but I think there is also some valuable information as far as site conditions and sub-watershed

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vulnerability if you will.

So that's something that I would suggest.

A lot of the comments that I'd written down earlier have been addressed from question number 10 about applying the more recent GIS data, scale effects, data resolution, and I didn't realize, and I should clarify, the paper that I distributed this morning, the Gotway and Young paper, I didn't realize that Doctor Young was the second author and I'd like to recognize her.

And I've been looking at that paper for, well, for several months now on and off just trying to understand what they did. And I know Doctor Gotway from another meeting and I, you know, it's, I think it's exciting, you've got an opportunity to talk with one of the authors, it's usually not so easy to do that.

So I would again, you know, encourage you to consider that, looking at the change of scale of support as they call it in that publication.

Another thing that came to my mind is how to get a handle on restrictive layers and there is, you know, as much as I can defend the National Cooperative Soil Survey, I can also say that it's

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produced by humans and we know that humans have their variability, and so there are some missing data elements or some things that are more important.

I will caution you, for example, that in some of the data there are some regional biases. For example, where I went to school in Iowa my graduate advisor was the person who set up the hydrologic soil groups for the data for the state of Iowa. And one of the comments he made to me was, we're not going to have a whole lot of these things in our B or C soils, you know.

And so he basically, you know, through his influence has influenced the interpretations of some of that.

So just to caution you on that.

But the point about the restrictive layer is that, I would like to make is that I think that if there is missing information in the database, there's also other ways to get to the same point, to get to the same endpoint. And the soil classification that Jeff Novak mentioned earlier is one way to do that. If you want to look at, if you understand the system, there are ways and I'm sure that Nelson can and he's probably thought about this already, but I just, I bring that comment in, there are ways to tease

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that information out of other components of that database.

And it is, it's an extremely valuable database and it's going through constant revision. If you pulled SERGO data off the web, one of the things you want to make sure you do is register that you're a user of that data, because if they change that data set you'll get an email that says, hey, we've updated that. They won't tell you what they changed but they may have filled in a field, they may have filled in a field that you're interested in. And you'll go back to, ah, here's finally, you know, information on depth to the restrictive layer or whatever.

So it is a very valuable source of information and it is being used in a lot more ways than the original people that started that program ever realized, ever imagined.

Just a couple more comments.

The question about tile drainage and trying to come up with a GIS coverage for tile drainage, and I'm not positive of this but I'm pretty sure that Dave James at the National Soil Tilth Lab, with one of the other scientists there at the Tilth Lab in Ames, published a paper a year or so ago, and I can't put my hands directly on the reference, but I

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remember seeing either a poster or an abstract or something about looking at this was for the Mississippi watershed, but since you're working, you know, at least for this particular area you're working in the midwest, so I think some of that information is available. It may not be as readily available as some of the things that USGS posts on the web as far as their geospatial data and that USDA also posts and other groups.

But I think there are some potential sources for that, and so that might be one other way to look at it.

And I'm a little, I'm not quite convinced about the using the dual hydrologic soil groups. I would like to see some more coincidence to that.

But I will say that even if you go out and I think somebody made this comment if you go out and try to find out where tile drains are in a field, in some cases the people that farm that field cannot really help you identify them. And in other cases they know very well where they are because they've updated them, they've been maintaining and they've replaced them and they've put in newer ones.

So, I think there are some potential

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sources for that and that's something to consider.

And then one last comment, and this actually opens up a whole new, it opens up a whole new laundry list for you, Mr. Thurman. And I apologize for this but since Doctor Brady is not here, we'll go ahead, and I'll go ahead and start down this path.

One of the really exciting that's going on in the geospatial realm these days, and you folks have picked up on it, and actually I learned something with the NHD Plus because I use the NHD data a lot, but I didn't know there was an NHD Plus. And so I learned something from this encounter.

And that use of the digital elevation model data, okay, and as Tony pointed out, the DEM data for the United States, the USGS has done an excellent job of giving us the best available data, but it is at different spatial resolutions. In some cases it's 30 meter data, in some cases it's 10 meter data and in some cases it's actually finer resolution data than that, related to a new technology called LIDAR and some of that data is available. And that's an extremely fine scale, but for some of the sub-watershed modeling I think it's very useful.

But there are some other things that can come out of the terrain data and terrain analysis

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procedures. And you've looked at some of those with flow accumulation. The top model that's used in the Dunn Overland Flor Information, that is also used in the terrain model.

And one of the thoughts I had was just to there's some simple kind of metrics you can have as far as if you're looking at a watershed or a sub-watershed, what's the difference between the maximum elevation and the elevation at your outflow, at your lowest point where you're looking at? So it gives you some idea of within that sub-watershed or watershed, what's your range in topography basically? What's your range in elevation?

And there's a lot of other variables. There are some excellent books, there's a book on terrain analysis, principles and practices, there's several publications out and I'd be glad to share any of those with you.

But I think if you're going to zoom in, why not take advantage of that digital elevation data and some of things that maybe that can do for you?

And just as an aside to what Doctor Chu mentioned, if you're going to do hydrologic modeling, you know, we've got models like, well you have basins which is built on SWAT, the soil water assessment tool,

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and that's basically linking another modeling system that we have, the erosion productivity impact calculator or now it's called environmental policy integrated climate ethic model, which is an old model that I've had.

But all those are runoff models that are curve number driven, but they're linked into, now into a geospatial context and so there's value there and I think this digital elevation data is something that is also a very valuable additional source for you.

And I guess that's, with that I'll, I don't have any additional comments.

DR. HEERINGA: I used to think the military had the ownership of the acronyms but I think the soil scientists have got them beat.

Okay, thank you very much, Doctor Effland. Jim Fairchild.

DR. FAIRCHILD: Fairchild. The question really was asked basically how good a job did the model use to predict watersheds that exceeded the level of concern for atrazine exposure and duration?

And I think, you know, I'm fairly confident with the used of the WARP model in identifying vulnerable watersheds, but it is clear that there are several outliers that weren't expected to

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exceed the level of concern and that is the Missouri sites particularly, and I found that very interesting.

And even though there was predicted that there was possibly some sub-basin variation in atrazine concentrations, it turns out that in the Missouri case there was not, that basically it was reflective of the entire watershed.

So where to go next? At this point, you know, I think that realizing that the Agency is resource limited, I would hope that they would stay within the original set of 1,172 watersheds.

I'd devote some effort to actually putting more effort into examining the Missouri watersheds as well as the Nebraska watersheds that weren't necessarily expected to trigger the LOC.

But I still have a concern. I think that the Missouri situation may be a little bit atypical, that I still have this question of how representative is the selected sub-watershed and stream segment? How reflective is that of the hydrologic unit in its entirety?

And I think that we've heard several ideas tossed out additional data, techniques that might be used to possibly explore how representative they are in terms of representing the entire HUC. But, you

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know, you've got this given data set with this begging questions and I hope that you wouldn't redirect too much of your effort outside of the original effort.

I guess there's a couple of concerns that I still have and this is kind of more of a general comment, and one of those is we still have this uncertainty in terms of the timing of rainfall in relation to atrazine application.

And this is such a critical factor, and I know it's very, very difficult to obtain. But, you know, given the relative interests of corn growers and some of the other groups who are here, I would hope that Syngenta and the Agency might move ahead with some cooperative opportunities to try to get a little bit better data on the timing of precipitation and atrazine application at the field level.

And I'll leave that at that.

And then the other is basically the quality of data in terms of corn harvest. And earlier it was mentioned using remote sensing data, possibly aerial photography to go back post HUC and try to fine tune the estimates of corn and corn harvest related to atrazine application.

And I think that there might be more of that data out there than the Agency is aware of. I

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know you're very, very busy but I would encourage you to explore that.

And then the last thing that I want to mention is that I hear a lot of talk about best management practices. And, you know, there has been questions about how effective they are and that's the whole basis of ARSC program. And there is a lot of data that says that they are effective in many cases, properly designed according to soils, the slop, you know, on a sub-watershed or watershed basis.

However we've got a changing market condition with corn ethanol and the price of corn. And it's my concern that a lot of these CRP lands are going to come out and go back into production. I don't know how the use of the 1992 land cover data, how well that reflects current CRP. But I suspect that we're going to see a lot of marginal land come back into production.

And I know that you all are aware of that but it really does make me concerned about atrazine exposures and possibly people not necessarily taking full advantage of BMP's just because they're concerned that it could cost them on the return side.

So, that's all I've got to say.

DR. HEERINGA: It's a good point. I

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guess $3.80 diesel fuel is the other factor that works the opposite way.

Doctor Lerch.

DR. LERCH: This is Bob Lerch. I've just a couple of comments, some of which just reiterates things I've said in the previous one, so I won't be too lengthy here.

I suppose I'll slightly differ from my colleague, Mr. Fairchild, in a sense that because you could use GIS tools to evaluate HUC 10 watersheds outside of the 1,172 for things like the restrictive soil layers and computation of something like say the runoff propensity index, which is just a ratio of the 19th percentile flow to the 10th percentile or the presence of hydrologic soil groups C and D.

If you didn't identify the watersheds outside the original 1,172 it would give you confidence that that was in fact a good selection, but it's an opportunity to be sure that that's not the case. If it is then those could be included for future monitoring, because I think that could be a GIS based exercise.

In terms of the second one, what additional parameters, et cetera, the only I'd have there is the application of potentially of risk assessment index models based on SERGO data. I think

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the other thing is improved herbicide usage data to get down to the scale in annual variation that I think is going to be needed.

And then the last one, this is Bob Gilliom's suggestion of essentially rerunning the step wise regression for WARP to tool it for the specific need of identifying watersheds or stream reaches that exceed the LOC.

DR. HEERINGA: Thank you very much, Bob. Doctor Portier, Ken.

DR. PORTIER: I don't think there's a lot to be said. I'll pick up on the issue that Doctor Fairchild mentioned and I though of it myself, that the temporal change in use may actually be a key discriminater of vulnerable watersheds, rather than use or intensity.

So the original WARP uses a crop intensity score which may be only part of it, but actually a change in intensity may be a bigger, bigger issue. A watershed that's under a lot of stress, either typically with increased crop acreage is going to be one that's disturbed and should have more.

I guess I disagree on the limiting to the 1,172 for the issues that I mentioned yesterday in the sense that the 1,172 are kind of defined on the

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data that we ran in the current WARP. And if you use new data it may be a broader or a smaller area, depending on what the new usage data says.

So some of the factors in WARP won't change but some of the factors will. And it'll change the area.

And then the final thing is well I guess we can say now that we all believe that there's some aspect of climate change going on right now, so issues like potential drought or drought probabilities and/or excess rain probability issues need to be factored into this as well.

So I'm kind of, maybe my comments are more on kind of thinking not so much statically of what's happening today, but what we expect to happen as things go out and think of vulnerability in that aspect.

DR. HEERINGA: Doctor Young.

DR. YOUNG: The only thing I would add right now is the fact, the way that the selection of the points within the watersheds were derived. They cannot be considered representative of the full watershed, because there was a restriction to only those that were more vulnerable.

And so I think a lot of care has to be

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taken in trying to generalize in any way to the watershed from the sampling points.

DR. HEERINGA: Comments from other panel members on this final question? Doctor Ellsworth.

DR. ELLSWORTH: Tim Ellsworth. One very brief one that ties in with what Jim was saying.

You know, we've been on some plots harvesting corn stover, you know, and there's 500 gallons of ethanol sitting out there just in the stover after you get a 200 bushel corn crop off. And they've got 12 million acres in Illinois of that stuff sitting out there.

It's coming in the next several years and what happens when that starts to get harvested in terms of runoff and just the idea is management is also a factor.

DR. HEERINGA: Those are good points about farming practice changes which are obviously very rapid. Yes, Doctor Novak.

DR. NOVAK: Well, I can comment directly on that removal of residue since I'm part of the REAP team within the USDA-ARS.

Currently we have no money for that research. We have some locations that are doing it on a dime, nickel and dime budget. But generally what

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they're finding is that after the third year of residue removal, that's 100% residue removal, the fertility of the soil goes to zero. So your system crashes.

So that's a short term effect but it's available nonetheless. And it's something maybe to consider.

Now how you get numbers on that, I'm sure they're going to be available by how much residue is being removed per county.

But I saw a questions up here that I'd like to address about additional soil comments or comments on additional soil properties that might not have been addressed.

And what I'm noticing in my areas of research, and we've looked at some of the chemographs here, is that the antecedent soil moisture conditions prior to the application of the pesticides, is very important or when that rain comes down, is it absorbed by the soil because it's so dry?

This goes back to Doctor Portier's point that if the watershed is under drought, you're not going to get much runoff unless it's really an intense storm.

But generally what I'd like to suggest is, can you get access to the states' climate data, the

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drought offices to do the overleafs for the drought potentials for the state?

And then if you can I believe they have the individual weather stations within the state that report the soil moisture conditions at that time. So you may have it within a county or an area. The soil moisture conditions then would give you an idea of the potential for runoff.

This effect on climate change and residue removal and bioenergy is the really hot area in ARS right now.

But thank you for listening.

DR. HEERINGA: Thank you for that addition. I think again it's a very, very important notion to be thinking about the changes, particularly in corn cropping and other biomass products.

Additional comments?

I wanted to see if we could get a little clarification on, we have different views on the extension of this, and I don't think we're necessarily disagreeing, we have this sample population which was defined for the purpose of the monitoring study.

And I think that a number of panelists have suggested that, really at this point we're stepping beyond sort of basic data collection

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potentially to data accumulation, assimilation and modeling, and therefore the price, the cost of going beyond that original group of 1,172 is not large. And I think we're not seeing a lot of advantage in staying with the 1,172.

I think going back to an earlier question, the notion of heavily sampling that lower 80th percentile, a fairly costly sampling to try to detect a few false negatives, again you have to put a lot of resource to really quantify false negative rates. And I'm not sure that that would have been advisable because we could have said we represented it, but with such low precision that it wouldn't have been.

But I think here, is it the sense of the panel then, and I'll let other people respond to this, that with regard to focusing on this 1,172, 1,172 was set as a survey population for the sampling reference and I think that was a useful activity, it's given us a reference, but with regard to any subsequent modeling and sort of assessment of vulnerability there really is, well, there's a marginal cost of assembling the data, but it's not the same marginal cost of expanding the sampling into these lower.

Anybody who would like to comment or react to that?

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Doctor Young.

DR. YOUNG: I agree with that. The thing that is going to basically it seems, if I have the consensus correct, and I may not, but it's my impression that a number are suggesting extensions using some type of regression approach.

And so once you do that it's important that you have the full range of the explanatory variables represented, that you're actually going to want to eventually draw inference to.

So you have a foundation, but that's going to cover all scenarios. And so for expanding the model to cover the region, and for validating the model you'll want to pick places that have particular combinations of explanatory variables in an effort to do some validation.

So it would be more targeted field work as opposed to a sample survey, but it's, the purpose would be very different.

DR. HEERINGA: At this point I think, Doctor Irene and the scientific team, whether we have answered this question?

DR. IRENE: I will defer to the people who know the answer.

DR. THURMAN: I think we've gotten great

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feedback on that. I think there's a lot of things for us to work with and wrestle with and take into account.

And so it's been very helpful for us.

DR. HEERINGA: And just for the record too, there are a number of potential sources that Bill Effland mentioned, some references that he might not have had at his fingertips, but if they are recognized you will see them in the report. And that would apply to any of the discussants here too.

Okay, at this point what I would like to do is, before we close, I'd like to go around to see if there are any additional general comments that the panelists would like to make.

I think we've been fairly free within the context of the charge questions to do that, but I'll just give everybody one more chance.

And let me actually begin right here rather than let Chris think a little while longer and also Paige too, so we can

DR. PORTIER: Are you indicating I think with, I speak without thinking?

DR. HEERINGA: No, I'm indicating that you're a thoughtful person and that you

DR. PORTIER: I've already prepared. The only thing that comes to mind that we haven't talked

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about, and I thought about it this morning is the sensitivity of the process to the SSI.

The SSI is probably one of a dozen kinds of similar, similarity indices that you could use to measure the community structure and change in community structure.

And we haven't really talked about how sensitive your index is. I don't think it would be, but I suspect there's a number of ecologists that are going to want you to prove that to them.

So you may actually have to give a little bit of not a lot of a little bit of thought to that.

DR. HEERINGA: Doctor Schlenk, Dan.

DR. SCHLENK: I have nothing to add.

DR. HEERINGA: The corn expert is coming up.

DR. HANDWERGER: As a physician/scientist my interests are so far removed from the subject of this meeting that I've chosen to heed the words of Abraham Lincoln who said, it is better to remain silent ands be thought a fool, than to speak out and remove all doubt.

So, but I found this meeting to be very interesting. I think I was able to follow about half

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of it.

I was raised at NIH next door to the Nobel Laureate, Julie Axelrod who once said that if an experiment required statistics it probably wasn't a very good experiment.

And I actually believe that.

DR. HEERINGA: Doctor Isom, okay Doctor Young.

DR. YOUNG: I just, you know, I feel like as a part of the panel that we always emphasize what can be improved and what could be done differently and things to think about.

But I do think that both Syngenta and EPA are to be congratulated on a Herculean effort for data collection in a considered and careful way, and for preparation to this point.

There are many challenges left, but I think you have a really good start on finding some important answers.

DR. HEERINGA: Doctor Chu.

DR. CHU: I think this is really a good opportunity to learn many, many things during this three, actually three day meeting.

I just want to mention one thing. In recent years I did a lot of hydrological modeling

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studies. I just looked at the stream studies and it seems pretty much similar to ours, the small creeks, the small streams.

So we did an analysis for example about the collection of the stream flow and processed the stream flow data, development of reading curves and also we developed some special computer software to process the stream flow data.

So if this kind of information will be helpful for this project, I would be very happy to provide any details.

DR. HEERINGA: Thank you, that's appreciated and I remember having seen some of your presentations in a meeting a year and a half or two years ago on some of your stream hydrology work and it would be beneficial, so --

DR. CHU: Thank you.

DR. HEERINGA: thank you. Doctor Effland.

DR. EFFLAND: Actually I don't have any additional comments, I think I've probably said enough. And after hearing that quote I'm sure that I have.

DR. LERCH: I do have one other scenario to throw out that, in terms of another definition or another setting in which vulnerability for contaminant

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transport and herbicide transports exists.
And it's in karst topography. Losing streams or conduit flow through sinkholes in another extraordinarily vulnerable setting in which surface water becomes groundwater and that groundwater often becomes surface water yet again.
And there are some areas, southern Illinois, southeastern Missouri, parts of Arkansas, et cetera, that do have significant row cropping over karst and I think that's an especially vulnerable exception to the general rule that's out there.
It would also be a challenge from an ecosystem standpoint because you're now and I think I got the lingo here you're dealing with producers, you're dealing with anthropods and isopods and that sort of thing in very sensitive karst ecosystems that would be impacted by these contaminants.
And that would be an extraordinary challenge for any model. But I just throw that out as a potential scenario that does exist out there that has not been addressed.
DR. HEERINGA: Doctor Ellsworth.
DR. ELLSWORTH: Yeah, I'm just going to echo Bill. I think that goes for me.
DR. HEERINGA: Bob Gilliom.

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DR. GILLIOM: I feel like I should be taking a hint from you guys and just I'm being really careful here.
I will second the kudos for the huge effort that went into the field work from you guys and so forth. That, the whole effort to pull off that monitoring was major.
One question I have that's more of a framing question I was interested in still getting from the Agency relates to the question I'd asked earlier that you cleared up on the toxicity issues.
And the question I had asked before was whether or not the animal part of the toxicity issue had been addressed for the degradates before then moving right away on to just the plants and just the parent compound. The answer was, yes, it had been addressed in the sense that even for the animals, not just the plants, those degradates weren't significantly adding to the toxicity.
So that answers that.
And so the only, the only other question I have in terms of framing the panel's responses is, what's the Agency's position on what happens if new information comes? And I'm not talking about the amphibian thing specifically, but if new information

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comes out of studies that are ongoing with, you know, fish reproduction or the whole your Agency is going through on the endocrine disruptors and what's the process for how that gets added on as new information to what's going on with this process that's aimed at a plants only exposure assessment?
DR. HEERINGA: Doctor Irene, if you choose to.
DR. IRENE: Yeah. Well, you know, if new in there are several to this.
If new information comes out in the literature that literature would be evaluated to meet certain criteria and then if it met the base criteria for using it in a risk assessment, we would consider that information and, you know, I mean we can always make an addendum I believe tot he risk assessment, because science is an ongoing, you know, field that just, you know, keeps developing.
On the other hand, if there are some significant findings on particular chemicals the registrant is required to send in the 6A2 data. So we can get new information via many sources. And that would cause us to reevaluate to do, you know, another evaluation of what we had already done to see if there would be any significant change in the risk assessment

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that we had already done based on the new information.
Did I get that right, J. C.? Anything else you want to add?
Yeah, yeah, I mean that's, it's sort of, you know, it's an ongoing process so I think that as science is an ongoing and developing and a work in progress.
So does that satisfy you?
Okay, thank you.
DR. HEERINGA: Jim Fairchild.
DR. FAIRCHILD: I too would like to thank the panel and the Agency and all the speakers for the presentations and exchange this week. It always strikes me that I always learn far more than I contribute when I come to these types of meetings and it's just wonderful to go through this type of exchange.
And I guess I would have one last question or thing that I would like to see the Agency follow up with and that is to extend the CASM model to the second, to the consumer level and try to determine by distribution of what a 5% and 10% deviation of SSI actually means at higher trophic level.
DR. HEERINGA: Thank you very much. Doctor

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DR. RANDOLPH: Well I also very much appreciate the enormous effort put in by both the Syngenta staff and the EPA staff. And again I thank you for some very excellent presentations.

One thing that I've thought about and this is something I don't think will be feasible or helpful on the short term, but it might be on the longer term, and it's from the suggestion that Doctor Lerch made a bit earlier involving the registration of individual use of herbicide sales.

I've heard several comments that said the use data are not very good. I suspect they're actually pretty awful, just intuitively, the level of aggregation, the quality of reporting, I just think that's probably not a very good data set.

But, the longer term question is, if EPA were to implement a registration system, and particularly with some followup that not only did you buy the product, but how and when did you use it, so that you have actual use data.

It occurs to me that if we have this meeting a few years down the pike, those data might be really useful. So it's just something I've been thinking about.

I don't know the cost of implementing a

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program like that relative to the benefits from it. It might cost more to do than it would be worth, but I think it's something to think about.

DR. HEERINGA: Doctor Novak.

DR. NOVAK: Yes, this is Jeff Novak. I'm going to coin Bill Effland, I think I've spoken enough and my professional biases and ignorance has at times come out.

But I'd like to conclude with two personal comments.

First thanking the EPA for inviting three soil scientists to interface with them, to exchange ideas on what we know and what we don't know,

and how our specialty and knowledge base can assist them with accomplishing their goals.

So I thank you very much, it's nice to be recognized.

On a personal issue I'd like to give this as another thank you because it allows me to use this as a professional accomplishment to serve as a role model for my children. They're continually asking me, what am I doing besides studying dirt? And now that I can say or, and pig something but now that I have this under my wing I thank you. It's good, it helps my kids understand what I do.

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DR. HEERINGA: Do your children have any expertise in toxicology or things of that

DR. NOVAK: No, but they want to be Buckeyes.

DR. HEERINGA: Okay, they won't qualify for assessment.

Okay, Doctor Lapoint.

DR. LAPOINT: Again, the amount of data that's been generated by EPA and Syngenta and contractors in support of registration for atrazine is astounding. And it, to me the value of this has been seeing the work that's conducted, the research that's been conducted and the model development using the experimental ecosystem data. I think it's excellent.

And I especially want to congratulate the work on the bioenergetics modeling because I think given the mode of action of atrazine, the bioenergetics is really one working with, yes, the sensitive organisms, the plants, but then it also builds in the community aspect because if the primary producers are affected,, that translates up to the consumers.

So it's been excellent.

And I agree with Jeff, Doctor Navak, that I've certainly learned a lot and it's been excellent.

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Thank you.

DR. HEERINGA: Thank you, Doctor Lapoint. Doctor Grue.

DR. GRUE: I'll just second the comments that have been made by others. You always learn more than what you potentially contribute. And again, as I did in my opening remarks, acknowledge EPA and Syngenta

for the tremendous amount of effort they put into the preparation for this SAP.

DR. HEERINGA: Doctor Gay.

DR. GAY: Paige Gay. I would like to thank you for extending an invitation to me to be here for this week.

I have learned so much from each and every one of you and I know that I will use it wisely in the future.

And I hope that my comments in the monitoring section of this study have a little bit of weight anyway, and I would certainly like to see a better characterization of the low intermittent flow sites.

DR. HEERINGA: At this point I think I'll turn to the EPA scientific team that's been here for three days with us.

Doctor Irene.

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DR. IRENE: Yeah, I'd just like to start off and then I'll let anybody else from the team speak who wishes to.

Unfortunately Doctor Novak took my line away from me. I was going to start by saying we had an awful lot of dirt people at this meeting and I've never been but I've learned so much from you so I think it's absolutely great.

I actually think I want to express a heartfelt thanks to the panel members. It's been a long three days, but you've just been terrific. And I think the input and the ideas that you've given to us to pursue are really fabulous. I know I've learned a lot. I'm sure the rest of the team has learned a lot also.

And the other thing is that if we really took to heart everything that you asked us to pursue we certainly would have job security for quite a long time. So I think we all appreciate that too.

But truly, thank you very, very much. It's been a wonderful, wonderful meeting, it's one of the best I've been at and I've been at many.

And you've been great, you have helped us out a lot and I thank you and I wish you a safe trip home and I wish you a very happy holiday also.

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And I also would like to ask any other members on the team or Bill Jordan Don, would you like to say anything?

DR.. BRADY: I would just like to add my thanks and it was, as you've all said, some really good discussion and exchange of ideas that went on here.

Thanks very much.

DR. IRENE: Nelson, Russ, Mark, have I said it all? Thank you again.

DR. HEERINGA: Well, we'd like to thank you. And again I think it's been said around the table form people directly, but I want to express our appreciation to each of you.

We can look at this and recognize just the volume of work that's behind every draft, behind every table is just a tremendous amount of work and thought that has to go into that.

And so the quality of the presentations, the materials that were given to us in advance, the clarity of what we've seen here. If you've been involved in this process for a number of years, this really has come up to speed now and I think that that's great.

And I want to also thank the representatives from Syngenta who presented data and

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their responsiveness on some of the questions that obviously were sort of indirectly being raised through the EPA staff who was interpreting their data, it has been a very I think open and responsive session on that.

So again, thank you to all of the members of the public and the audience who've attended. I've seen many of the same faces for three days and I can only imagine, I guess it's 24 hours in the same chair, but I hope for you as well as for us that there has been some learning experience in this process.

At this point before we wrap up, I'd like to turn to Jim Downing, the Designated Federal Official for the meetings for some final administrative notes.

MR. DOWNING: Yes, thank you. I just wanted to mention that documents that have been presented today throughout the meeting will be actually added to the docket for the meeting. And so within a few days that will be available. Some documents that came in on Tuesday are already up.

So you've be able to view all of those materials shortly, okay?

And of course the docket will contain, you know, everything pertaining to the meeting, all the

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materials, the background materials as well as what's been considered here in the last three days in addition to the final report with in 90 days of today.

So it'll all be there for everyone to see.

I just also want to express my appreciation for everyone's participation. I think it's been great.

I would especially commend this panel for charging into quite a volume of material to digest in a relatively short period of time, and to be able to make some great assessments and recommendations to the program.

And I think as you've heard, we at EPA have really appreciated everything that you've shared with us. And I think it's going to be extremely valuable as we go on.

And, you know, the interesting thing about all of this is we are not at the end, it's not over because we've got more to come.

So anyway, so I guess with that yes, Stephanie?

DR. IRENE: I just want to apologize to Syngenta. I am so sorry for not acknowledging your part. I'm exhausted, okay?

So please forgive me, but I do want to thank you for your presentations, for your clarifications and all the work that you did also, and we look forward to continued work together.

And I do apologize for that.

MR. DOWNING: Sure, no problem.

So I guess with that unless there is something else that I have forgotten, we will consider the meetings adjourned.

DR. HEERINGA: Okay, we're adjourned.

Panel members, everybody, safe travels if you're traveling today or tomorrow.

Panel members, if we could just meet briefly in the breakout room to discuss the preparation of our written summary of the minutes of the meeting.

(WHEREUPON, the meeting was adjourned at 3:55 p.m.).

CAPTION

The foregoing matter was taken on the date, and at the time and place set out on the Title page hereof.

It was requested that the matter be taken by the reporter and that the same be reduced to typewritten form.

Further, as relates to depositions, it was agreed by and between counsel and the parties that the reading and signing of the transcript, be and the same is hereby waived.

CERTIFICATE OF REPORTER

COMMONWEALTH OF VIRGINIA

AT LARGE:

I do hereby certify that the witness in the foregoing transcript was taken on the date, and at the time and place set out on the Title page hereof by me after first being duly sworn to testify the truth, the whole truth, and nothing but the truth; and that the said matter was recorded stenographically and mechanically by me and then reduced to typewritten form under my direction, and constitutes a true record of the transcript as taken, all to the best of my skill and ability.

I further certify that the inspection, reading and signing of said deposition were waived by counsel for the respective parties and by the witness.

I certify that I am not a relative or employee of either counsel, and that I am in no way interested financially, directly or indirectly, in this action.

MARK REIF, COURT REPORTER / NOTARY

SUBMITTED ON DECEMBER 06. 2007

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