IN THE DISTRICT COURT OF MONTGOMERY COUNTY, TEXAS
284TH JUDICIAL DISTRICT

In re: The Commitment of
Billy Johnson, et al. 00-02-01034-CV

THE DEPOSITION OF DENNIS DOREN, Ph.D.
Madison, Wisconsin
February 13, 2001
9:15 a.m.

The Petitioner was represented by its attorney, CHRISTOPHER L. THETFORD, Special Prosecution Unit, Civil Division.

The Respondents were represented by their attorney, GREG BAL, State Public Defender.

Also present: Donna Van Bogaert, Videographer; Lynn Maskel, M.D.; Carole DeMarco, Ph.D.

Prepared for Attorney _____

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I N D E X

E X H I B I T S

EXHIBITS, continued

THE DEPOSITION OF DENNIS DOREN, Ph.D. taken at 9:15 a.m. on February 13, 2001, at the OFFICES OF THE WISCONSIN DEPARTMENT OF JUSTICE, 123 West Washington Avenue, Madison, Wisconsin, before Karen Blair, a Notary Public in and for the State of Wisconsin, pursuant to the Rules of Civil Procedure.

VIDEOGRAPHER: My name is Donna Van Bogaert of Van Bogaert and Associates, Incorporated, 5910 Lexington Street, McFarland, Wisconsin. This is the videotaped deposition of Dr. Dennis Doren taken on February 13th, 2001, at the Department of Justice located at 123 West Washington Avenue in the City of Madison, County of Dane, State of Wisconsin, commencing at approximately 9:17 in the forenoon, regarding the commitment of Billy Johnson, et al. in the District Court of Montgomery County, Texas. The case number is 00-02-01034-CV. The deposition of Dr. Dennis Doren is being taken on behalf of the State of Texas pursuant to Notice.

Would you state your appearances, please?

MR. THETFORD: My name is Chris Thetford. I'm here from the Special Prosecution Unit representing the State of Texas.

MR. BAL: I'm Greg Bal. I'm representing the respondents in this case. I'm with the Public Defender's office, State of Iowa.

DR. MASKEL: Lynn Maskel, M.D. I'm an expert witness retained by the defense.

MS. MS. DeMARCO: Carol DeMarco, psychologist.

VIDEOGRAPHER: Would you swear in the witness, please.

DENNIS DOREN, Ph. D.,
having first been duly sworn, was examined and testified as follows:

EXAMINATION

BY MR. THETFORD:

Q. Dr. Doren, for the sake of the jury would you state your full name, please?

A. My name is Dennis Doren.

Q. Great.

Greg, before we get started I would like to have admitted as Exhibit number 1 a copy of the notice in this case so that we'll have record that the depositions is being taken in all of the cases that are currently pending.

MR. BAL: No objection.

BY MR. THETFORD:

Q. Dr. Doren, I would just like to go through some preliminary things before we get started. First of all let me ask you, have you had your deposition taken in the past?

A. Yes.

Q. So you understand basically what it means to have your deposition taken. And by that I mean you understand that the answers that you give today are just the same as if -- would be just the same as if you were in court?

A. Yes.

Q. In fact, we've come today to take your deposition in case you were not available to come to Texas to testify down the road at a hearing in any of these cases regarding the actuarial instruments which might be used to predict or to predict probabilities of recidivism in any of their cases. Do you understand that?

A. Yes I do.

Q. And you're comfortable doing that this morning?

A. Yes.

Q. Great. Just a couple of things I like to tell

people, and I know you've done this before so I'm sure this is repetitious. If you and I speak at the same time this nice lady to your right will have a very difficult time taking down what we say so I will work with you if you will work with me, and let's try not to speak together at the same time; okay?

A. Thank you.

Q. The second is that it's very polite in common communication for us to nod our heads "yes" or "no" in conversation and if you do that and I look at you and say, "Is that yes," I'm not being rude it's just that I want the record to reflect that you said "yes" or you said "no"; okay?

A. I understand.

Q. All right. You've told us your name is Dennis Doren. We're here in morning on February the 13th of 2001 in Madison, Wisconsin; is that correct?

A. That's correct.

Q. And do you reside here in Madison?

A. Yes I do.

Q. Do you work here in Madison?

A. Yes I do.

Q. Can you tell the Judge what kind of work you do?
A. I'm a psychologist and administrator at a State of Wisconsin forensic hospital. The name of the hospital is Mendota, M-E-N-D-O-T-A Mental Health Institute. I -- and at this point, actually, there's a process of transition so I'm actually associated with a different State of Wisconsin institution, as well, called Sand Ridge Secure Treatment Center. That is actually located in Mauston, but my office is still here in Madison.

I'm employed on a part-time basis administering over the assessment team for people who are doing the pre-commitment and post-commitment evaluations under Wisconsin's Chapter 980 which is called the Sexually Violent Persons Act. It is in some important ways, in my view, similar to a Texas law that we're here about today.

Q. Can you tell the Judge how long you have been working doing pre-commitment and post-commitment evaluations for the State of Wisconsin under their Sexually Violent Predator statute?
A. In Wisconsin it's called sexually violent persons, not predator, just to be clear. I was

actually involved in the first case that went to trial during the summer of 1994. Wisconsin's law passed in June of 1994. And I've been involved ever since, and been involved virtually solely doing that work in my state employment since February of 1997. Previous to that I was also doing other administrative duties.

To finish answering your earlier question I'm also in private practice as a psychologist.

Q. Before we get to your private practice duties so that the Judge understands, what you say pre-commitment do you mean doing evaluations on individuals to determine whether or not they are candidates for civil commitment? Is that what you mean by pre-commitment?

A. In a manner of speaking, yes. It's -- I do them or supervise other people doing them pre-commitment at two different stages. Mostly at the stage where there has been a different prior evaluation in the Department of corrections -- I don't work for the Department of corrections here -- and there has been a referral for a commitment and a prosecutor has filed a petition and a probable cause hearing has been held with probable cause being found.

It's that point where I or somebody that I supervise typically comes into the picture.

The people that I supervise, including myself, then are the team who do all of the State of Wisconsin evaluations, from that point on, that are either state- or court-appointed, how everyone wants to define that.

Q. What about post-commitment evaluations? What are those?

A. Those are after people have been committed to either our in-patient facility or to an out-patient setting. They are entitled to a -- let me abbreviate and say an annual review. It actually starts at six months and goes annual thereafter. They're also entitled to evaluations when they petition the Court for a less restrictive environment or for discharge.

For the annual reviews I now supervise all of the people doing those reexaminations and when somebody that I -- when the department for which I am employed gets the Court-ordered responsibility for a reexamination based on a petition, then I also supervise that work.

Q. So that would be the equivalent in Texas of the review that somebody has to determine whether or

not they should remain committed or not; is that?

A. It is comparable in concept, yes.

Q. To that? Okay. You started a minute ago to tell us about your private practice work because you're also actively involved in private practice. Can you tell the Court what it is that you do in your private practice?

A. At this point in my private practice I am involved almost solely with work related to sex offender civil commitment around the country and not in Wisconsin. Since I supervise the people doing the work in Wisconsin I really can't, then, do the private work in Wisconsin, with rare exception.

But I do instruction, training. I do court testimony. I do evaluations when hired by whomever, and I do testimony for those evaluations. And I have some -- I have a contract for consultations, as well as occasional other people call me for consultation work.

I do that in, at this point, a number of states such that out of the fifteen states with sex offender civil commitment laws -- currently

active sex offender civil commitment laws I have been in some way involved in -- I think now with Texas it will be twelve of those fifteen.

Q. And this is the first work that you've done for us in Texas; is that correct?

A. That's correct.

Q. To make things clear for --

A. For anyone in Texas.

Q. For anyone in Texas. To make things clear for the Judge from the very beginning, the prosecution side of the equation in this case, my office, the special prosecution unit, has retained you as a consultant and as an expert in this case; is that correct?

A. Yes.

Q. And you have agreed to do that, and we have agreed to compensate you for your time in working with us as a consultant and an expert; is that correct?

A. Yes.

Q. And can you tell the Court how much we are paying you per hour to do that consulting work?

A. $200 an hour.

Q. We've talked about your professional experience. I want to talk about your

educational background which gives you the abilities to have your professional experience. Tell the Judge about your educational background starting with your college degree and then moving up from there.

A. I received a bachelor's in psychology from the State University of New York at Buffalo in 1975. I received a master's in psychology from Bucknell University in 1978. I received a Ph.D., a doctorate in philosophy in clinical psychology from Florida State University in 1983. That was with a subspecialty in crime and delinquency studies. So --

Q. Once you finished your education were you licensed to practice psychology in any states?

A. I've been licensed in Wisconsin as a psychologist since February of 1984.

Q. And do you have permits to practice psychology in any other states?

A. I have a permit to practice psychology in the State of Iowa and a permit to practice psychology in the State of Washington.

Q. You were kind enough before we started to bring your CV, one of which you call your generic CV, the other one you call your CV specifically

devoted to sex offender civil commitment evaluations including Chapter 980, Wisconsin's Sexually Violent Predator Act evaluations; is that correct?

A. Except that it's persons act, but yes, that's correct. We can go by predator. I won't keep correcting you.

Q. That's all right. In Wisconsin it's persons. I'll try to remember that. I'm going to show you what's been marked as Exhibits 2 and 3. Are those the CV's that you provided us with this morning, the first one being your generic one the second one being the one that's specific to the work you do in this area?

A. Yes, these appear to be complete copies of both of those different documents updated as of February 10th, just three days ago.

MR. THETFORD: Greg, I believe you've had a chance to look at those. Do you have any objections to the admission of Exhibits 2 and 3.

MR. BAL: No objections.

MR. THETFORD: Great. If you will just hand those to the court reporter then, unless you need to look at them again -- I doubt you do, but they'll be there if you do.

Q. I don't have a lot more questions about your experience other than to ask you, you indicated that you do do some training in this area of work; is that correct?
A. Yes.
Q. Have you trained in many other states besides Wisconsin?
A. A number of them, yes.
Q. Have you ever been to Texas for training?
A. Not to the evaluators or any other people directly involved in the implementation of the Texas law. I have given training at a conference that was in San Antonio.
Q. Which states have you given training to evaluators? Can you remember off the top of your head?
A. Arizona -- I'll go alphabetical here; it's easiest for me to remember -- Arizona, Florida. The people in Illinois were in a training that was in southern Wisconsin but they came up for it. It was coordinated training. Kansas, Missouri, North Dakota, and Wisconsin.
Q. I also had some notes that I found from some materials that I was reviewing that you're in the process of writing a book in this area; is

that correct?

A. I've actually basically completed the writing of it. There are just the final fixing up the references, things like that. It's scheduled to be submitted to the publisher March 1st.

Q. And you've already secured a publisher for that book?

A. Well I have a contract for that, yes.

Q. And can you tell the Court what the subject matter for that book is going to be?

A. The title of the book is, Sex Offender Civil Commitment Evaluations, a Manual. There are nine chapters. I review, initially, the different laws relevant -- the components relevant to clinicians, and then look at diagnostic issues related to the civil commitment evaluation for sex offenders, and then go into risk assessment issues for a number of chapters and end up with a -- well I have a chapter, then, about report writing and court testimony and then end up with an ethics chapter.

Q. Is the material in the book based upon the materials that you rely on in your every-day practice?

A. Oh, yes.
Q. I'm going to ask you now, have you had a chance, prior to beginning your deposition today, to read the Texas statute regarding the civil commitment of what we call sexually violent predators?
A. I have read through it and then spent some time concentrating on the earlier portions of it as part of that work I did in writing that first book chapter.
Q. So you've included, in your book, information about the Texas commitment statute, as well.
A. Included that in particular in comparison to other state laws relevant to the clinical issues for commitment.
Q. So in reading the Texas statute do you feel as though today you're familiar with the evaluation components in the statute?
A. I believe I am, yes.
Q. Would you agree with me that those evaluation components are not completely different than the evaluation components in the many other states that have similar laws.
A. They actually overlap significantly. The standard for the requisite mental condition uses

a different term than any other statute, "behavioral abnormality" versus the more common "mental abnormality" or the occasional "mental disorder" that other statutes use, but the statutory definition for mental (sic) abnormality is virtually identical to other states' definition of mental abnormality. The risk level -- the risk threshold terminology for commitment of "likely" is common to nine other statutes besides Texas. Then there are three other states with "more likely than not" and two states with "substantial probability," but Texas uses the most common term of just simply "likely."

Q. I want to take you back in time if we can, and sort of go through your mind for the Judge and get you to describe the process that you went through when you began doing this work, when you began doing evaluations to determine whether or not a person should be recommended for commitment, starting with what was the first step that you took in making that sort of decision?

A. You're talking about back in 1994 when I began this?

Q. Yes.

A. While I had experience working in the general assessment of sex offenders, it was more about their treatment needs. I was doing treatment with them and so my perspective about my own knowledge in this area was that I was too limited, and yet I was given the first -- the assignment to do that case. And so what I did is, I went to basically the professional libraries, published articles, books, things along those lines, that described -- and what I concentrated on were things that described characteristics of sex offenders that had been found through scientific research to either be statistically related to sexual re-offending or statistically clearly not related to sexual re-offending.

And I compiled the information in a -- actually various different forms, but basically concentrating on the -- both the consistency of findings across studies, as well as the degree of statistical relationship each of these characteristics showed with sexual recidivism, and in effect composed a set of empirically based guidelines or risk factors to structure

the evaluation work relative to risk.

The diagnostic process, being the other major clinical prong for commitment in Wisconsin, did not need anywhere near that same kind of updating. I simply read some articles to make sure that I still knew the right kinds of stuff, but I had already been doing diagnostic work with sex offenders at that point.

Q. I want to focus today, since the respondents in this case have filed a motion seeking to exclude expert testimony on the basis of the fact that actuarial instruments have been used in Texas to determine probabilities of recidivism. I want to focus on that area and get your opinions in that regard.

When we start doing that the first question that I want to ask you is, as a psychologist do you think it's ethical to use actuarial assessments as part of the basis for your opinion as to whether or not the defendant poses a danger to sexually recidivate in the future?

A. Just to correct a word in terms of my understanding, there -- I would not use the term

"defendant," but "respondent."

Q. Respondent.

A. But with the understanding you meant respondent, then I would consider it highly ethical and -- well, let me just say highly ethical under most circumstances. There are circumstances where I do not believe that the existing actuarial instrumentation applies, simply because the science, the research has not studied the relationship and therefore it would be an improper leap of faith to apply the instruments in those cases.

Q. Which groups would it be improper to apply to?

A. The most clear category are female sex offenders. There has been literally no work with any of the major actuarial risk assessment instruments relevant to sex offender recidivism using female subjects of any age.

Another category that seems rather clear are the particularly young juvenile offenders. As they're approaching 16 or 17 there is some suggestive research, in fact one piece of research going down to age 15, where it shows that the instruments still seem to work but it's -- there aren't many pieces of that, those

pieces of research, so I would be inclined to use the instruments but with a thinking of them more as general guide posts than as something that's more clear in its interpretation.

Q. Do you think it's acceptable to use these actuarial assessments when applied to adult inmates who have been incarcerated?

A. Adult male inmates who have been incarcerated for a sexual offenses is by far the most common type of subject for the pieces of research and therefore under most circumstances that would be true. There are still exceptions within that group.

Q. And of course you would be willing to look at different exceptions within that group like you just said there were exceptions within that group.

A. In applying any instrument one should always be aware, in my opinion, of the appropriate application and the inappropriate application. So that is always the first question, not a later question to look at is whether the instrument really applies.

Q. You keep using, and I keep using, the word "instrument" and not "test." I know in the

field of psychology there are things called psychological tests. I'll represent to you that last week I was at Texas A&M University at the request of counsel for the respondents taking a deposition of Dr. Leslie Morey, who is the creator/developer of the PAI. For the Judge's information, in your opinion is the PAI a psychological test or is it more of an instrument like the actuarial assessments that we're here talking about today?

A. My understanding of it is that it's a psychological test.

Q. Would I be correct in saying that the PAI is a test like the MMPI is a test?

A. It has similar attributes as well as similar research design underlying it, so I would say yes.

Q. Can you describe for the Court, as best you can, what the difference is between a psychological test and an actuarial assessment of the variety that we're talking about today?

A. I can answer that question, but as a caveat to my answer I think it's of importance for me to say that the current -- most recent, I should say, written work of which I'm aware leaves a

very broad definition for what a psychological test or educational test may be, and that in the application to specific instruments there can be debate.

With that as a caveat, my working understanding, in brief, of a psychological test is -- and these are not the words of the definition, but that it measures something of a psychological nature, does it in a systematic way with a interpretation that's research based. The implication is that it is used by psychologists. It is particularly likely to be a psychological test if it needs training of a psychological or psychiatric nature -- I'm using psychological in a generic sense -- needs training of a psychological nature for its proper use, both in terms of potentially it's administration, but certainly in it's interpretation.

An actuarial instrument does not have any of those characteristics I just described except that it is systematic and has a fixed interpretation. Actuarial instruments are used by insurance agents, for instance. It is simply a systematic way of assigning a numeric process

to fixed pieces of information, types of information, that comes up with a statistical outcome of meaning. It does not necessarily have any psychological interpretation to it. It does not necessarily have -- need any psychological training. It may need training in its use, but not psychological in nature.

Q. Let me --

A. The basic difference, if I were going to put all that into a nutshell, is an actuarial instrument is for assessing a statistical property, in this case risk, whereas psychological test is more for the purpose of assessing a psychological construct.

Q. So for example, if I represent to you that Dr. Morey told us that his PAI was an instrument that a psychologist could use to focus in on or rule out a given psychological diagnosis would that make sense to you as a psychological construct that the PAI was designed to look at?

A. Yes. And the diagnosis is clearly psychological -- in the broad sense, a psychological concept.

Q. Whereas the instruments that we're here talking about today, the actuarial assessments, are more

analogous to things like actuarial assessments that life insurance companies do; is that correct?

A. That is correct.

Q. When I say that, I mean a life insurance company, as I understand their business, goes through a process of making statistical predictions about people based upon certain factors as to when -- statistically how long they'll live. Is that as you understand what they do with their actuarial assessments?

A. To a point I'll agree with you. I would change one concept that you talked about.

Q. Which is?

A. Statistical prediction. Actually, the insurance agents really are not concerned about which individual will do -- will live a certain period or not; they are more interested in the group information of -- therefore they are assessing a degree of risk for a group of individuals who share certain characteristics. That is the typical actuarial process. It's not of a prediction nature.

Q. So if an insurance agent came to me and I was applying for life insurance, they would compile

the information that they would need to know about me that they could put into their table to put me with a similar group of people which would allow them to make probability predictions; is that correct?

A. And therefore figure out your premium, yes.

Q. And figure out my premium. In that sense it's much like what car insurance companies do -- auto insurance companies do, as well; do they not?

A. That's my understanding. I'm not an insurance agent, but that's my understanding.

Q. So it's not that they predict one individual person's risk, but rather one person with these characteristics' group risk; is that correct?

A. I might word it a bit differently.

Q. Please do.

A. They look at the individual's characteristics. So it's still in that sense an assessment of the individual. But the interpretation of the specific characteristics of the individual is purely based on group information of risk.

Q. You indicated a minute ago that there's much disagreement in your profession about whether or not the actuarial assessments are a

psychological test or are merely an actuarial assessment; is that correct.

A. You used a modifier of "much." I would agree that there is disagreement; I'm not sure that "much" is the right word. The real bottom line of that debate is that -- to my knowledge is that it is of not great importance about what label we put to the instruments or whether we call them tests or not, but whether or not -- the real bottom line underneath that debate is, do they meet standards for use?

Q. For example, I will represent to you that I have a JD degree and that's my advanced degree. I don't have a master's degree or a Ph.D. in psychology. As such I would not be qualified to administered the PAI; would I?

A. Probably not.

Q. If, however, I was trained in performing and scoring the MnSOST-R or the STATIC-99, two instruments that we're here to talk about today, could I be trained to do those despite my lack of psychological training?

A. Yes. In fact, the MnSOST Revised in particular, MnSOST is M-N-S-O-S-T revised or dash R, we say MnSOST-R, the MnSOST Revised was designed

initially specifically for use by case workers in prison setting who may or may not have much of any psychological training.

Q. As we sit here today in February of 2001, in different states, different states have taken different positions in regard to the evidentiary admissibility of these actuarial assessments; is that correct?

A. Well, different states have different standards, if that's what you're asking me.

Q. That's what I mean, yes.

A. Yeah, there are the Daubert standard, D-A-U-B-E-R-T, the Fry standard, and the relevancy standard.

Q. I want to ask you some questions about the position that ATSA is taking in regard to the actuarial assessments. The first question I'll ask you is, can you tell the Court what ATSA stands for?

A. A-T-S-A stands for the Association for the Treatment of Sexual Abusers. It is an international organization largely comprised of psychologists and secondarily social workers and a small set of other people, including psychiatrists.

Q. And do they meet on a regular basis?
A. They have an annual conference. They have just finished their nineteenth annual conference this past November. They also have a journal that comes out, a peer review journal that comes out quarterly, I believe.
Q. Is ATSA in the process of developing an opinion regarding the use of actuarial assessments?
A. They are in process of finalizing on what they refer to as a policy.
Q. Can you describe for the Court what it means for ATSA to come up with a policy?
A. What they did is, they commissioned a group of select members, I don't know who they all were, a year and a half ago who were given the task to develop a policy -- what I would call a position paper, an explanation of their term of policy -- concerning the US form of sex offender civil commitment laws. And after approximately a year and a half of work that committee submitted their work to -- their proposed policy to the board.

That submission occurred this past November at the annual conference, they have an annual business meeting, and then the board has

distributed that draft policy to, to my knowledge, all of its members -- so I was told; that's how I came to see it -- and asked for feedback. And in answer to requests for information I was told that the board will be reviewing that feedback in March, though I was also told they actually don't expect to finalize their decision about what they're going to make as the organization's policy probably until May. This is all of 2001.

Q. Before I show you what's been marked as Exhibit number 1 (sic), which is a draft of that policy statement, I'm going to ask you these questions. Were you on that board instrumental in drafting this policy?

A. I was not on the committee at all. I am not on the ATSA board. I am a member of ATSA. My sole input to this draft policy involved -- occurred at two stages. One was during October when the policy was -- I should say draft policy was being finalized by the policy board. I first became aware of it when one member of that committee contacted me and showed me a copy of that and asked for some feedback about it. So I had a little bit of early input. And then as a

member of ATSA, just like all the other members, I was invited to give a response, which I did.

Q. I'm going to show you now what's marked as Exhibit number 7, which you brought with you today, which is a drafted of the proposed ATSA policy regarding the civil commitment of sexually violent predators; is that correct?

A. That's correct.

MR. THETFORD: Are there any objections to the admission of Exhibit number 7, I believe it is, Greg?

MR. BAL: No, no objection to the Court considering it as long as the Court understands that this is -- this policy has not been adopted at this time.

MR. THETFORD: Correct. We'll stipulate that it hasn't and that Dr. Doren has explained when he expects that the board will.

Q. I want to walk through that policy, if we can, and specifically look at the parts that talk about the use of actuarial instruments in predicting risk of recidivism. Does ATSA's proposed policy statement say anything about the use of actuarial assessments?

A. Yes.

Q. Can you describe for the Court what that proposed policy does say about the use of actuarial assessments?
A. On the second page of this draft policy is a category entitled, Risk Assessment and there is a general statement about -- that ATSA recommends -- I'm paraphrasing -- that ATSA recommends that the risk assessment process be based on, quote, the best available scientific knowledge, including the use of current validated risk assessment instruments, ends of quote. And then a little bit later in that the statement is made here, the evaluator must use a set of actuarial instruments derived through scientific methods, and then it goes on about other things.
Q. It doesn't specifically mention any particular actuarial assessments; does it?
A. Any specific instrument, no, it does not.
Q. Just encourages the use of them, those that have been validated as being scientific; correct?
A. The proposal does more than encourages, it talks about the policy being a "must use." Which, as I have actually stated earlier in my testimony, it overstates my perspective. I think there are

situations where that would not be true and not be accurate.

Q. And you've told us which situations those would be.

A. Yes. Unless one interprets the "must" by emphasizing the "validated." I was talking about certain situation the instruments would not apply because they're not validated for those situations. Then there is a disagreement.

Q. Let me ask you this, since we've talked about the ATSA proposed policy statement. Do you have an opinion as to whether or not actuarial risk assessments are accepted in your field of work?

A. Yes I do.

Q. Can you tell the Court what your opinion is in that area?

A. That they are of widespread use and generally accepted.

Q. Do you think that these instruments have demonstrated sufficient research to justify their use?

A. Yes. But I should qualify both that answer and my previous one by saying, along with other information. But as part of a process the answer for both would be yes.

Q. And let me ask you about some instruments in particularly as to whether or not you have an opinion about whether or not they have demonstrated sufficient research to justify their use, the first being the STATIC-99. Do you think that that instrument has demonstrated sufficient research to justify its use?

A. Yes, under certain circumstances.

Q. Which circumstances would it not be -- has it not demonstrated sufficient research to justify its use?

A. As I was mentioning earlier, particularly about women, there've been no studies of the STATIC-99 with female sex offenders. There has been one study that comes to mind with people who have -- may never have been incarcerated but are in community treatment programs who are convicted sex offenders. That seemed to apply there, but that's only one study so if I were applying it to someone who's never been incarcerated that might be a problem.

And to juveniles there's been a small study, actually in Texas, interestingly, out of the Texas Youth Commission applying the STATIC-99 to people who were sex offender -- an

adjudicated sex offender as a juvenile. Again, it showed some support there, but it's only one study and the numbers were small. So I would be more reluctant to talk about it being fully valid with those other select kind of groups.

Q. So once again, we're talking about women and juveniles.

A. Yes.

Q. In terms of the MnSOST-R do you have an opinion as to whether or not it has demonstrated sufficient research to justify its use?

A. Yes, but to a lesser degree than the STATIC-99.

Q. Tell the Judge why you say that.

A. Simply fewer pieces of research. The support appears to be in the same direction, approximately the same degree in the studies that have been found -- studies that have been done. In fact, in some sense the MnSOST Revised, depending on which statistic one looks at, may show superiority over the STATIC-99 with other statistics would show inferiority. So it's not -- it depends which statistic one looks at. But the reason for my statement is that there have simply been fewer studies of the MnSOST Revised compared to STATIC-99.

Q. What about the PCL-R? Have an opinion about that assessment tool or do you call that a test?
A. That is a psychological test and I have lots of opinions. I need for you to be more specific with your question.
Q. Do you have an opinion as to whether or not it has demonstrated sufficient research to be accepted in this field?
A. Oh, absolutely. Robert Hare, H-A-R-E, the person who developed the instrument -- the test, it's a psychological test -- has reported that there are over five hundred studies of that -- of the PCL-R, the Psychopathy Checklist, Revised. It was -- it was initially developed in 1981. The revised form was developed in 1990 with a manual in 1991. So it has been around for ten years. It has been studied in many countries on multiple continents. The consistency of results, including with females, for instance, is quite robust. There is a different -- there are two different forms. Instead of the PCL-R there are two different forms for potential use with juveniles and that shows there are far fewer pieces of research, and that they are still showing a robust set of

findings, however.

Q. How do you, as a professional in this area, make a decision as to whether or not these instruments are accepted in your field?

A. There are a number of different ways. One of those -- and I would not consider it sufficient, but indicative -- is the frequency of use. There can be reasons why something is frequently used and ultimately found to be -- to go into disfavor. But if something's not frequently used within at least a set of people who are most likely to be using it, then it's hard to view it as generally accepted. So I see it in that sense as a necessary but not sufficient condition.

Q. Right.

A. In addition, I would look at the organizations who are most relevant to that area. In this case I would considerate ATSA to be one such organization, and its proposed policy statement relative to the SVP laws, the sex offender civil commitment laws, would show that same type of widespread or general acceptance.

A third way in which I would look for the general acceptance is in the materials that make

it into professional presentations at conferences and into professional publications. There are -- have been, at this point, quite a few presentations on these instruments collectively and individually, both in terms of training sessions, but more importantly to me in terms of research about the instruments.

Most importantly to me, relative to the research, however, is that when something becomes generally accepted in the field you can tell that -- you can tell that by that the research starts to move beyond the issue of, Is this okay to use or not? and starts going to the details of when it is of best use and with what ways should we be modifying it? So it's a second step of research. That has already begun to occur, even with these instruments that are only a few year old. And so it suggests to me that the issue of general acceptance in the field or widespread acceptance in the field is not a -- so much of a question in my mind as where the field is now going beyond that.

Q. There's often questions raised by the respondents in these cases about how you define what the field is, what the professional field

is. And I'm sure you've heard that in other cases. Do you have an opinion about that, what the relevant professional field is that the Court should look at to determine whether or not these instruments have been accepted by that field?

A. I do have an opinion and at the same time I acknowledge that the exact parameters of defining that may not be clear.

Q. Okay. What is your opinion?

A. The general way in which I describe the relevant field is the set of people who are inclusive of the following: People who are involved with sex offender civil commitment work, community notification work, community registration work, when there are different gradations for risk to be determined; the people who are involved in sex offender treatment; and the people involved with the research related to any of those kind of issues, including risk assessment.

I don't go beyond that in my opinion for the following reason. I don't include, for instance, all psychologists or even all forensic psychologists. I don't include all psychiatrists or all forensic psychiatrists.

And I can include, for instance, some people who are other than that, social workers who would fit into one of those categories.

The reason that I don't go beyond that is maybe best explained by metaphor. If we were to try to determine whether a new instrument or a -- an instrument, new or otherwise, is generally accepted by people who do brain surgery I don't think that we would get meaningful responses by asking all physicians. People who work with the general practice may not have a clue about whether or not this instrument is generally accepted, nor should we expect them to. I might not even specify it be all surgeons. It may be something that is quite specific to neurological type surgery and not to eye surgery or something else. And so I would really want to know from the people who have reason to have knowledge about it, about whether or not it's generally accepted.

The reason to get more specific about why I would not even include all forensic psychologists -- I consider myself a forensic psychologist -- is because I know, for instance, in the field of forensic psychology there's a

sub area having to do with child custody work. I don't know much of anything about child custody work and I would not consider myself an expert in that area. Likewise, just because someone's an expert in child custody would not suggest too me that they necessarily know anything about sex offender assessment and I would -- certainly risk assessment. So I would think that one has to be more specific if one's going to get a meaningful response from some type of, shall we say, polling of the field.

Q. Have you done any unofficial surveys regarding the different risk assessments and which states are using which risk assessments?

A. I've done two informal surveys, the last one being in July of 1999. These are specifically in the states that were then active sex offender civil commitment states and what I did, basically, was find out the relative frequency of use of any variety of instruments, psychological tests or otherwise, that evaluators were either state- or court-appointed, were using in those assessments.

I've also since that time not done any

formal surveying, but as I was testifying to earlier I've had direct contacts with a lot of those same people on a more individual basis doing trainings and court testimony and consultations, et cetera, around the country. So I also have direct experience from either what I've read or what I've been told by those people about what they use.

(Exhibit 10 was marked for identification and a copy is attached hereto.)

BY MR. THETFORD:

Q. Dr. Doren, number 10 has been marked. Is this the compilation of the informal survey that you did regarding the different states which have commitment laws and the different actuarial assessment instruments that the evaluators are using in those states?

A. This was the compilation that I summarized -- a summary from the July, 1999 survey. It was not -- just to clarify your question actually, it was not just psychological instruments -- I should say actuarial instruments, it included psychological tests in the survey; it included physiological measures, as well.

MR. THETFORD: Do you have any objections

to the admission of that exhibit, Greg?

MR. BAL: I would like to ask a couple of foundation questions prior to admissibility, if I will.

MR. THETFORD: Sure.

EXAMINATION

BY MR. BAL:

Q. Dr. Doren, this survey was based on telephone calls?

A. E-mail consultation actually. None of it was through telephone; it was all through e-mail.

Q. And these calls were to evaluators in states? Department of Corrections? Who did you contact in each state?

A. I had one person in each of thirteen states at that time, not all fifteen that currently exist. One contact person per each state. That person was someone who had contact with the state- or court-appointed evaluators in that state. I did not have direct contact with all of the evaluators in each state. I relied on each contact person or each liaison person, per state, what I refer to as a state representative, for that e-mail consultation

network. I relied on that person to forward the information to me.

Q. And these people -- (Unintelligible).

COURT REPORTER: Would you say that again? Your hand --

BY MR. BAL:

Q. The people that you contacted, were any of them with the Department of Corrections for each state?

A. Yes. I'm just figuring out which states.

I believe at the time there were three such people. In California, in Minnesota, and in South Carolina.

Q. So this survey doesn't necessarily represent the Department of Corrections for each and every state you have listed and whether or not that Department of Corrections has adopted this particular tool.

A. It is not representative, to my knowledge, of any single department in any single state. That was not its design.

Q. And this is not an exhaustive survey of, say, various clinicians in each state; correct?

A. It was quite specifically just to survey, even in the informal way, the process of people who

were either state- or court-appointed, not people who may be hired by the defense -- who may or may not be the same people. I did not have access to that set of people, who may have been different. Some people will work for anybody who hires them so I'm saying that may have overlapped a set of people, but there are clearly a set of people who have worked for the defense only. I did not survey those people, even informally.

Q. But it's not an exhaustive survey of even people who may work for the state; correct?

A. Probably not. What it was, was a -- at that point was simply the information from one contact person. How that person gathered the information varied from state to state and I do not know all the details.

MR. BAL: No objections.

COUTINUED EXAMINATION

BY MR. THETFORD:

Q. Dr. Doren, based upon this informal survey that you've done what have you learned regarding the use of the RRASOR in states that have commitment laws?

A. At the time I did the assessment there were thirteen states with active sex offender civil commitment laws and the survey that I did, just to make it clear what I was getting at, was per instrument or test or physiological measure, the person getting back to me told me that that instrument was used by virtually none of the people; some of the people, meaning a minority; most of the people, meaning a majority; or virtually all. The RRASOR was used by at least most of the people in all thirteen states.

Q. What about the MnSOST-R?

A. At that point in time it was used by at least most of the people in ten of the thirteen states. We're going back, now, a year and a half, but that's what it was.

Q. And the STATIC-99?

A. STATIC-99 had only come into existence and come to the states six months previously. At that point of the survey it was used by at least most of the evaluators in five of the states where the state representative to this consultation network for six other states said that they anticipated using it but they wanted to learn more about it first.

Q. What about the PCL-R?
A. The PCL-R was used as part of the assessment by at least most of the evaluators in ten of the thirteen states. An eleventh, that being Missouri, stated that they had full plans of using it when their forensic evaluators -- which is a title they have there -- were trained in the instrument.
Q. Have you done evaluations for civil commitment in other states other than Wisconsin?
A. Yes.
Q. Let's start with Wisconsin. In Wisconsin, if you were going to do an evaluation which actuarial assessments would you utilize, assuming it was a man who was incarcerated?
A. An adult man who was incarcerated who wasn't of certain other kind of characteristics, so the most typical type of cases, then the standard set of actuarial instruments I use as part of my assessment are the RRASOR, the STATIC-99 and the MnSOST Revised. And then I also use the PCL-R, but that's not an actuarial assessment.
Q. And did the staff of psychologists which you supervise use those same instruments?
A. In the pre-commitment process, that is true to a

person. They use all of those. In the post-commitment reexamination process, until recently I believe they were using just the STATIC-99 and the MnSOST Revised, and we are basically having discussions about whether the RRASOR should be added.

Q. Have you done evaluations in Iowa?

A. Yes.

Q. In Iowa which actuarial assessments have you used?

A. The same: The STATIC-99, the RRASOR and the MnSOST Revised.

Q. So wherever you do the evaluations you, personally, use the same set of evaluations?

A. Given that the same type of characteristics of the individual in terms of the adult male incarcerated without special characteristics, then the answer is yes. I don't change depending on where I am.

Q. What other states have you done evaluations in besides Iowa and Wisconsin?

A. Arizona, California, Florida, Illinois, Iowa. I haven't done work in Missouri, but I'm about to get sent a case, from what I've been told. But I guess that doesn't count. Washington and

Wisconsin.

Q. And in all of those cases where it was adult males who were incarcerated you've used the set of instruments that you've described for the Court?

A. In the current set, yes. If you're going back far enough then there were times before even the STATIC-99 existed that I was doing work, so of course I didn't use that. And in Wisconsin I was doing work before any of the instruments so I used them more individually. In addition, in the past I used an instrument called the MnSOST, not the revised but an earlier instrument, and if one goes back far enough I used the VRAG, V-R-A-G, before even the RRASOR exist.

Q. And the VRAG is an assessment instrument for risk assessment in general; is it not?

A. For interpersonal violence. It is not specific to sex offenders and it's not specific to sexual vial.

Q. Can you tell the Court which states you've testified in, in sexual commitment cases?

A. Yes. I'm going to remind myself by looking at my CV. Where I'm looking is on pages eleven through thirteen of Exhibit 3. I have testified

in probable cause hearings for civil commitments of sex offenders in Iowa and Washington. This is besides Wisconsin. I have done depositions either for a -- as a pretrial or pre-commitment hearing process, or like today's where it was submitted as part of a motion hearing, in Illinois, Florida, Washington, and Iowa, and Arizona. I have done -- until today they were all Fry hearings. I was thinking -- which is what the standard was. Fry hearings in Florida, New Jersey, Missouri, Iowa. And I have done testimony in final commitment hearings in Illinois, Washington, Iowa, Florida and I'm scheduled for one in Arizona next week.

Q. Tell the Court, as best you can recall, any instances where you have provided testimony in Fry hearings -- which is the closest analogy I can come up to with our Daubert hearing that's coming up -- in which states has the Court allowed the admission of the actuarial assessments?

A. I don't know that I know the bottom line in all cases because I don't know that the courts have always ruled. I have not followed up in that way. What I am aware of is that in those -- the

Fry hearings in Florida and -- which is mostly where the Fry hearings have been occurring -- and in Iowa, which has just recently been doing those, all of those that I've been involved with were specific to the admissibility of actuarial instruments testimony and in the hearings in which I have testified there were two hearings in Florida where the Judge or Judges -- in one case it was a multiple-judge panel -- ruled against the admissibility and all of the others, at least if they have ruled, to date have ruled for the admissibility. Those are in totaling maybe ten Fry hearings. Ten, twelve, I don't know, the number would be in there, where the courts apparently have admitted such evidence.

Q. If I represent to you that I am aware of one trial judge in Iowa who has excluded them and one judge in Florida who has excluded them would that be your knowledge?

A. I'm aware of one in Iowa and two in Florida. Two hearings. One was a multiple-judge panel. Those are the three of which I am aware, country-wide, that the Courts have ruled against the admissibility of the actuarial instrument evidence in the later commitment hearing. In

all other cases -- again, if they have ruled; I've not always fouled up to find out if that has yet occurred -- they have allowed admissibility of the evidence.

Q. I want to take the Judge back historically before the development of some of these instruments. And can you describe for him what a psychologist such as yourself, a forensic psychologist, would have done if assigned the task under the Texas statute in rendering an opinion on an evaluation, as the statute requires, without actuarial assessments? What would you have done?

A. What I would have done and what I would still do -- what I did do and what I would still do is look at a list of risk factors that have been shown by research to be related to sexual re-offending. I would try to make sure that that list was well grounded in the research findings, and I would use that basically to structure my clinical judgment.

So I would look at the -- this list of risk factors and then look at the individual's life relevant to each of these characteristics and see which have these applied and to what

degree, so it's application as well as intensity, and then in a judgment call process weigh these in whatever way I think is appropriate and make an assessment of whether or not this appears to be likely, however I understand that term.

Q. And now that the actuarial assessments are available to you, can you describe for the Judge the process that you would go through in addition to what you just described?

A. I would actually start, and do actually start, with the instruments results -- again, assuming that they apply to the case. If they don't, I go back to exactly this risk factor list. That is the fall-back position. The --.

But I start with the actuarial instruments and use, again, the individual's information to score the relevant characteristics, the risk factors that are included in the instruments. The only difference are the instruments then attach numbers to these and you add a number -- them up and those numbers have some statistical meaning. So it's like an additional step to the list of risk factors.

I use that process, then, to ground myself

in that zero to 100 percent possibility of somebody's risk, to find the general range in which the actuarial instruments would suggest this person falls, knowing that these percentages have error around them. That's why I talk about it's a range, it's not a number.

The research of which I'm aware, however, it would indicate to me that these instruments are not comprehensive in what they look at relevant to -- I should say related to those -- the complete set of characteristics that I or we of the science have reason to believe are potentially meaningful in the assessment of the sex offender recidivism risk and so I need to look beyond the instruments.

That does not mean I look at all characteristics, and it does not mean I discount the instruments. I stay grounded. But there's research that indicates that certain kind of characteristics such as participation and completion of a treatment program of relevance can lower somebody's risk and so the whole category would move down then. On the other hand, the intensity of someone's illegal sexual interest, for instance, pedophilia to use the

formal term, could actually increase the degree of risk. I say intensity, not simply the existence. Those kind of things I look at.

I have to take into consideration other kinds of clinical considerations that -- such as, is the person telling me that he's going do it again or is the person, for instance, under such severe physical health problems that he's not likely to live very much longer. I mean, those obvious considerations must be viewed as part of the overall risk assessment, as well.

Q. If I understand your answer correctly, that while you now have at your use actuarial risk assessments you do not consider them the be-all and end-all of the job you were assigned to do.

A. That -- emphatically that's correct. They are a useful tool, but they are not the bottom line. And that's, again, the -- conceptually as well as statistically. It's because we know that they don't draw from all the information that may be of potential relevance. They just organize a certain part of the information.

Q. So you're not suggesting that civil commitment programs should get to the point where they simply generate a result on an STATIC-99 or

MnSOST-R, however it's scored, that that's the ultimate decision that's made regarding commitment. That's not what you're advocating; is it?

A. With the current set of actual instruments that's absolutely not what I'm advocating. And to my knowledge there's no one in the country doing sex offender civil commitment evaluations who does a purely actuarial approach that you're describing. There's always a clinical additional process to that as well as, in my opinion, there should be.

Q. There is literature that's been published which supports the use of actuarial assessments that way; has there not been?

A. Yes.

Q. Dr. Hanson published an article in 1998; did he not?

A. He published a few things in 1998 but I think I know which one you're referring to, but yes, he did.

Q. Which one am I referring?

A. The, What Do We Know About Sex Offender Risk Assessment article in the journal, Psychology, Public Policy, and the Law.

Q. Are you familiar with that article?
A. Yes I am.
Q. And do you agree with his conclusions in that article?
A. Virtually to -- every one, yes.
Q. Now, you started off kindly enough for us describing when you first started the process that you would go through, and now with the development of these actuarial assessments the process that you go through. Has there been research done which shows the accuracy of doing purely clinical assessment, which was what you were doing at first I believe, compared to clinical assessment with the use of actuarial instruments in terms of your accuracy?
A. The answer to your question is yes, but the way in which you're using the terms are different from the way that I would use them.
Q. Give me the terms you would use?
A. There are -- probably the easiest way to think of it is that there are four different types of methods of risk assessment, not just the two that you were just describing. One is the -- generally referred to as the unaided clinical judgment. That's where the clinician is doing a

risk assessment based on whatever he or she thinks is important to the case, but without any a priori list of risk factors, certainly, it may or may not be research based at all.

The second form is more of a research guided, structured approach. That's what I was describing as the fall back position and what I used to use, not the unaided clinical judgment. The that's where you have a list of risk factors, but exactly how to combine them in an a priori way, but exactly how you combine them and exactly what different combinations means, is unknown.

A third way that we've mentioned already is purely actuarial. That's where you just use one or more actuarial instruments and whatever their bottom line is, is the bottom line. I don't recommend that, for the reasons we described.

And the fourth is the more common approach for me, as well as around the country, in sex offender civil commitment evaluations. What's referred to as clinically adjusted actuarial approach. So you start with the actuarial then you make clinical adjustments.

Q. Have are there been studies that compare purely clinical evaluations to those utilizing actuarial assessments, as well?
A. Yes.
Q. And can you tell the Judge which approach shows more accuracy, purely clinical judgments or clinical judgments with the addition of actuarial assessment?
A. If we're talking about accuracy in terms of predictive accuracy then the relationship of all four of these is that the clinical -- purely clinical shows the lease degree of accuracy. It seems to be better than chance, but not by much. That's the unaided. Then there's reason to believe that there's validity, some degree of accuracy, an improvement, by using the list of risk factors. It's at least as accurate as the unaided clinical, and probably better. The purely actuarial is then at least as good, if not better than, the list of risk factors. And there are at this point, of which I'm aware, eight pieces of research that look at the clinically adjusted actuarial process and interestingly enough it appears to be at least as good, if not better than, purely actuarial.

With the instruments we have. So that each step seems to be at least as good, if not better.

Q. So it gets better as you move along the hierarchy there?

A. At least as good, if not better.

(Exhibits 11 and 12 were marked for identification and copies are attached hereto.)

BY MR. THETFORD:

Q. I'll show you Exhibit 5, which you brought with you today, which is a bibliography that the first set of articles on the bibliography include articles regarding comparisons of clinical assessments versus the different forms that you've chronicled for the Judge; is that correct?

A. On the first page are five comparisons of actual actuarial assessments compared to the clinical risk assessment, and then going onto page 2 are the eight citations I was -- made mention of looking specifically at the clinically adjusted process versus the purely actuarial process.

MR. THETFORD: Greg, I would ask that those exhibits be admitted. If you have any objections I would like to hear what they are.

MR. BAL: No objections.

BY MR. THETFORD:

Q. Dr. Doren, I'll ask you about the bibliography that you prepared as Exhibit number 5. Would that be an aid to the Court in looking at the publications that have been made in all of the different areas of your testimony today that we're going to be talking about?

A. Well, it would be an aid. It's not inclusive of all publications, but it is specific to a number of the issues that we're talking about, as well as various presentations, dissertations, not just publications. For instance, concerning the STATIC-99, research related to that starts on page six and goes through page eight, and research on the MnSOST Revised starts on page eight and goes through page -- the top of Page ten.

Q. Great. I want to ask you about a study that I read recently by, if I can pronounce the name, and I'll spell it for the court reporter, Nicholiachuk. Are you familiar with that author? N-I-C-H-O-L-I-A-C-H-U-K?

A. Terry Nicholiachuk, yes.

Q. Which focused on the issue of showing or demonstrating the use of actuarials doing a good

job of limiting predictions of violence. When I say limiting predictions of violence meaning, we don't want to predict that someone is going to sexually recidivate violently when they're not. So the purpose of the study, as I understood it, was to show which ones do the best job at limiting the group of people that we say statistically would fall into a group who would commit repeated acts of sexual violence; is that correct?

A. If I understand -- that was a long question, but if I understand correctly you're referring to the Nicholiachuk, Templeman, and Gu, G-U, study in which they first looked at a set of seven hundred and forty one sex offenders being released from the -- or who were released, I should say, from a psychiatric center in the Canadian prison system and they demonstrated initially the utility or accuracy of the RRASOR in assessing the likelihood for risk -- for sexual re-conviction, most specifically, in that study, over a period of ten years.

In addition, however, they also had had clinicians use a more unaided clinical judgment process to assess those same sexual offenders

when leaving prison and found that the RRASOR labeled as high risk -- in their definition, which those familiar with the instrument know is a score of four or higher -- the RRASOR was identifying about 9.8 percent, if I remember the statistic correctly, of the people leaving as high risk for sexual recidivism, which was almost on target to what the final result was, whereas the clinicians had the categories of low, medium, or high risk in their assessment and they had labeled ultimately over 60 percent of the sex offenders leaving prison as high risk. And they were clearly significantly overestimating the risk of these individuals.

Q. So the result of that study then shows that clinical judgment is actually more subjective than actuarial assessment?

A. Oh, that's clearly true. But subjective in a predictable direction that clinicians tend to see more risk than the actuarial instruments would indicate to exist. And the instruments tend to be more accurate.

Q. Oftentimes in what you've testified, and in Fry hearings I'm sure, and what I expect to hear in our Daubert hearing next month, is that the

respondents will argue that these actuarial assessments don't meet the requirements of the APA. Are you familiar with those arguments?

A. The American Psychological Association?

Q. Correct.

A. Because there's also the American Psychiatric --

Q. Right. The Psychological Association.

A. The American Psychological Association, there is a set -- they've been updated, most recently being 1999 -- of what are referred to as the educational and psychological standards -- sychological test standards. I am somewhat familiar with that, yes.

Q. And that goes back to the area that we talked about early on this morning regarding whether or not these assessments are tests or are not psychological tests; does it not?

A. That's where that is written, that definition that I think most people would agree is rather broad and vague.

Q. Do you think that inter-rater reliability is important with these instruments?

A. Absolutely.

Q. Describe for the Judge what we mean when we say inter-rater reliability?

A. The concept is simply that there's consistency across different people rating the same case. Conceptually it-s we can't be measuring anything useful if we can't be measuring it consistently enough.

Q. So for example, there are currently seven people in this room right now. Only the Judge can you see you because you're being photographed, but there are seven people in here and if all seven of us scored the same case we could come up with some ideas on that case regarding inter-rater reliability. I know that that's a small sample. I know it wouldn't be necessarily scientifically valid, but that's essentially what you're talking about; am I not right?

A. The concept would be of that type and then for us to be able to show consistency across a number of cases that we would each be rating, yes.

Q. Out of curiosity, the STATIC-99 and the MnSOST-R and the PCL-R -- strike that. The STATIC-99 and the MnSOST-R, rather, they're instruments that the person who's being evaluated could see prior to the evaluator actually filling them out on that person.

A. Sure.
Q. And it wouldn't change the score.
A. Both of those things are correct. The information on those instruments is available on the internet so it would actually surprise me if there aren't people who are incarcerated who have become familiar with them out of their own need to know. And the scoring of the instrument is based -- on both those instruments, with the exception of two or three items on the MnSOST Revised, are all historical in nature and so the person at that point in time, at the time of which he or she is being assessed -- let me say he, because I said I wouldn't use it with women. At the time he's being assessed, he couldn't change anything. It would be whatever his records were demonstrating.
Q. Dr. Epperson described that for us last week in the layman's terms of, either it happened or it didn't happen.
A. Conceptually, that's correct. There can be a bit more ambiguity than that, but conceptually, that's correct.
Q. Let's start with the STATIC-99. I've got Exhibits 8 and 9 marked and I'm going to get you

to hand those two to the court reporter and get her to mark those next in sequence.

(Exhibits 13 and 14 were marked for identification and copies are attached hereto.)

BY MR. THETFORD:

Q. Those are the materials that we have with us, essentially, on the STATIC-99, as I understand it. The first exhibit is the scoring sheet, the second is the coding instructions and worksheet. The next is the rules for scoring it, and what's the last one? Number 14?

A. It is basically my form of combination of Exhibits 8 and 9. It's overlapping information.

Q. Okay. Exhibits 8 and 9 I'll represent to you are blank copies of the STATIC-99 scoring sheet and coding instructions that are used by the State of Texas and the Texas Department of Criminal Justice. Are the questions that are asked on the scoring sheet identical to the questions asked on the STATIC-99 as developed by Dr. Hanson?

A. I'm sorry, I missed the question. Are they identical to --

Q. -- the questions that Dr. Hanson developed on the STATIC-99?

A. In paraphrasing, yes. I mean, it's not exactly the same words in, but paraphrasing yes.
Q. For the sake of the Judge so that he understands this process, for you to grade or to score someone on the STATIC-99 could you do that without meeting them?
A. Yes. If the records are sufficient. And it's designed to be done that way.
Q. It's designed if you have enough information in the records that you get, you can score that?
A. That's correct.
Q. And it's because it asks biographical data; is that correct?
A. Historical information about the individual, most of which is related to his legal infraction history.
Q. And the coding instructions then -- and the other Exhibit, number 13, which is sort of, I guess, a manual for scoring the STATIC-99 -- is that how you would characterize 13?
A. It certainly overlaps into the issue of manuals. Different people define "manual" in different ways which would mean it should be more inclusive than this document. This is the set of coding rules. That, to me, is part of

the essence of a manual.

Q. And when it's all said and done you come with up with a final score; is that correct?

A. If you have all the information; that's correct.

Q. And what does that final number tell you?

A. The number is interpretable within the context of research findings for groups of sex offenders with characteristics that came up with that same score and is associated with a percentage or, for the STATIC-99, three different percentages, depending on a five-year ten-year or fifteen-year follow up period, of the re-conviction likelihood for new sexual offending.

So for instance, if a person has a score of six on the STATIC-99 one would look up what that is associated with and if one found it was appropriate to look at a fifteen-year follow up figure -- in other words the person's life expectancy was at least that long, things like that, then I would look up and find that the score of six is associated with a 52 percent re-conviction likelihood for -- on average from other research and then the proper interpretation would acknowledge, as well, that there's error on both sides of that.

(Exhibit 15 was marked for identification and a copy is attached hereto.)

BY MR. THETFORD:

Q. Number 15, if I understand it correctly, Dr. Doren, you have broken down, with the RRASOR the STATIC-99 and the MnSOST-R, what the different scores tell us in terms of percentage predictions of recidivism as defined under each of those instruments for groups of people with those scores, along with the -- what's the last phrase? The confidence interval? Or is that like the risk of error? Am I referring to two different things when I say confidence interval or risk of error?

A. There are different kinds of error. Confidence interval addresses one of those. Real quickly, the idea of a confidence interval is what we kept hearing about in October and November for the presidential election. The Gallup polls, for instance, talked about Governor Bush, at that time, having certain percentage of people who said they would vote for him versus at that time Vice-president Gore having a different percentage and then there was a statement, give or take 3 percent or give or take 4 percent.

That's a confidence interval. It has to do with the sampling process.

Q. So when you say on the STATIC-99 a person that scores a six within fifteen years has a 52 percent -- it falls within a group of people that within fifteen years 52 percent of them will recidivate -- in Dr. Hanson's definition on the STATIC-99 tell the Judge what recidivate means.

A. Specifically for the STATIC-99 it's re-conviction.

Q. So will be re-convicted for a sexual offense or re-convicted for any offense?

A. STATIC-99 is re-convicted for a sexual offense.

Q. 52 percent of them. And then what is the confidence interval? Plus or minus what?

A. What I have on this sheet of paper for that is plus or minus 8.6 percent. I would emphasize that these are estimates, and in fact I updated these, but I didn't put it into this format, for a presentation I did at the ATSA conference this past November. The numbers move slightly based on more information, but not by a lot. It gives an idea. Again, it's a ballpark kind of process to give you an idea of the degree of range I

have.
Q. So it could put you down at 44 percent or it could put you up at 60 percent?
A. In general that's the correct idea, yes.
Q. That's the way it would work?
A. So that would be that kind of range on the instrument.
Q. Where does somebody go to get a STATIC-99 coding sheet and instruction form and what I called the manual? Where would I go to get that?
A. Easiest place is on a certain web site where Dr. Hanson has input, but it's actually the Canadian Solicitor General web site. Dr. Hanson is a research psychologist working in that office for the Solicitor General and this is where he puts results from his research.
Q. So that's available to anyone with access to the internet, then?
A. That's correct.
(Exhibits 16, 17 and 18 were marked for identification and copies are attached hereto; a recess was taken from 10:45 to 11:00 a.m.)
BY MR. THETFORD:
Q. Dr. Doren, before we went to break, the court reporter was kind enough to mark the materials

regarding the MnSOST-R which are in front of you. The first one is the general instructions. The second one, I don't know what it is; you brought that with you. And the third one is an article written by you and Dr. Epperson; is that correct? Are you the author on that?

A. No.

Q. Dr. Epperson?

A. I did not author that.

Q. Dr. Epperson and Dr. Kaul, the co-developers of the MnSOST-R, regarding their final report on the development of that. I'll represent to you that last week, last Thursday I guess it was, we took the deposition of Dr. Epperson in Ames regarding his development of the MnSOST-R. We've talked about the MnSOST-R quite a bit. Let me ask you this question first. Where would a person go to get the MnSOST-R?

A. Bulk of the information is on either of two different web sights. Dr. Epperson maintains a web site through the Iowa State University where all of this information is. And the Minnesota Department of Corrections basically has a connection to that same information.

Q. And so you can pull that down off of the web?
A. Yes.
Q. Much like the STATIC-99?
A. Yes.
Q. And Exhibit 16 has the instructions for scoring the sixteen different items on the MnSOST-R; is that correct?
A. That's correct.
Q. There was some controversy last week and many questions were asked about it, I want to see if we can just clear it up once and for all. Item number one asks -- the question, I believe, is, does a person have two or more sex convictions; is that correct?
A. Sex or sex-related, yes. Charges or convictions -- well that -- actually just convictions for that item; that's correct.
Q. Convictions. And in Texas our statute is written that nobody is eligible for commitment unless they have had two sex convictions. Do you remember reading that in the Texas statute?
A. Yes.
Q. So in Texas, everyone who is scored on the MnSOST-R who is going through the civil commitment process is automatically going to be

plus two; isn't that right?
A. On that item.
Q. On that one item?
A. Yes.
Q. I just asked you that to try to cut off some questions that we had last week about interpretations of item number one.

The next exhibit is a chart that you brought with you and you're referring to the top part, I think you said; is that correct?
A. Yes. It's a -- these are just two slides and I'm mostly -- I brought this just because of the top slide. There are different formats for the interpretation of the MnSOST Revised scores and this slide from one of Dr. Epperson's presentations is -- has all the relevant information in one place.
Q. Okay. And what information is on that slide?
A. It is a set of different bar graphs with percentages, and on the bottom part of each one are -- in the four different categories are different categories of MnSOST Revised scores. So there's three and below, four to seven, eight and above -- which is really eight to twelve -- and thirteen and above. And then with each

category there are three bar graphs, not just one, that goes up to a certain -- each one going up to a percentage. And on the right side of that slide are designations of three different what are referred to as base lines, 35 percent, 21 percent, and 15 percent. And without going into all the statistics about it, depending on what base line one is starting with in one's set of samples or sample, that will determine the interpretation for the risk category of the MnSOST Revised scores. That's what this represents. So it gives you all of that kind of information in one place.

Q. On the previous exhibit that you looked at when we talked about the STATIC-99 and the plus or minus eight at the fifteen-year-out level in terms of that group recidivating sexually, do you have similar numbers for the MnSOST-R? I think it's Exhibit number 15.

A. These are --

Q. Exhibit 15.

A. Oh, the confidence intervals?

Q. The confidence intervals.

A. Yes. On Exhibit 15 I also have for each of those score categories the approximation of a

confidence interval.

Q. And what do the confidence intervals work out to be, approximately, with the M MnSOST-R?

A. They vary from a plus or minus 5 percent up to a plus or minus 14 percent. The variation is based quite significantly -- statistically is based on the fewer number of people in some categories versus others across studies. The higher risk categories have fewer people in it and so the confidence in those are wider. If we think of it again in terms of the Gallup poll, the process of sampling a small number of people leaves a wide error possibility and the more people they sample the more it will narrow it down. That's the same process in the computation of these.

Q. Dr. Epperson told us last week under oath that the rate of error was plus or minus two points. Is that a different statistical index than the confidence interval?

A. Yes. He was referring to the -- something that is statistically called a standard error of measurement. That is not about the interpretation of a score but about the variability among raters in scoring people. So

there are different types of errors. One is in the consistency which different raters will score the same person, and that would be approximately plus or minus two points. The actual statistic is just slightly higher, as demonstrated on Exhibit 15. It's also written there.

A different type of error is in this sampling process. If we had a lot of people with a certain set of scores we can get an awfully good approximation of what that means on average, but if we only have a small number of people we're not anywhere near certain. That's the confidence interval. So one type of error is basically surrounding a score, and the other type of error is surrounding the interpretation of that score.

Q. And in the confidence intervals, the where they show the most -- I don't know what the words are, but the percentages are the greatest, that's because the numbers of people who score at those high scores are the smallest? That's the smallest groups?

A. That's by far a contributor to the wider interval, if that's what you're talking about,

is a smaller number of people fall into those categories. There is another contributor, but by far that's the major one.

Q. And so we can also know that logically those people that fall into the category of the highest scorers are going to be the people most likely to recidivate; isn't that correct?

A. That's what the instruments would indicate and the research that supports the instruments would indicate, yes.

Q. So if it was with the high group, what's the confidence interval rating?

A. The largest group -- the largest confidence interval for the MnSOST Revised is approximately plus or minus 14 percent.

Q. So if we were --

A. Which is relatively wide.

Q. It is a wide confidence interval, but if it was for a group that we were predicting and saying people with those characteristics that fall into that group have an 88 percent probability of re-offending, with a confidence interval of plus or minus 14 you could go 14 above or 14 below 88; isn't that correct?

A. That would be the concept.

Q. Okay. So 88's perhaps a bad example to choose because it puts us over a hundred so let's try again and say 86. If we said that people had an 86 percent probability of re-offending if you had those characteristics and you fell within that group, if it's plus or minus 14 that could mean that 100 percent of those people had a probability of re-offending or '72 percent had a probability of re-offending; isn't that correct?

A. That would be the process of looking at the confidence interval plus or minus for the interpretation of the score. And most technically what we're talking about is a 95 percent confidence interval, meaning that no matter where we sampled people of similar types -- so adult male incarcerated, et cetera, that 95 percent of the time people with those scores would show as a group the risk between the 72 and 100 percent range.

Q. So what we're talking about is somewhere between three-quarters and a full 100 percent probability of re-offending.

A. Using the statistics you were just describing, yes.

Q. Correct. How does Dr. Epperson define

recidivism?

A. The instrument was developed -- then MnSOST Revised, and therefore Dr. Epperson, was developed using six-year re-arrest for a new hands-on or physical contact sexual offense.

Q. And it's re-arrest; it's not re-conviction?

A. That's correct. It includes re-conviction, but there'd be some people who were re-arrested who may not have been re-convicted.

Q. Does the fact that he uses re-arrest and not just re-conviction trouble you with using the MnSOST-R?

A. It does not trouble me. It is something that I need to take into consideration in my interpretation.

(Exhibits 19, 20 and 21 were marked for identification and copies are attached hereto.)

BY MR. THETFORD:

Q. Dr. Doren, she's been nice enough to mark, I think, three more exhibits all related to the RRASOR, which I think you've talked about the RRASOR was developed by Dr. Hanson; that's correct?

A. That's correct.

MR. THETFORD: Let me ask you, did you

have any objection to the admission of the things on the MnSOST-R?

MR. BAL: No, no objections.

MR. THETFORD: Okay. We'll move on, then.

Q. On the RRASOR, that's one of the instruments that you use in your battery of instruments that are available to you in your group; is that correct?

A. That's correct.

Q. In doing evaluations.

A. Yes. As well as what I teach, yes.

Q. As well as what you teach them to use and as well as what you teach at trainings.

A. That's correct.

Q. That sheet that you developed showing confidence intervals, what does it tell us about the RRASOR? The chart that you had there?

A. From Exhibit 15 the -- there are confidence intervals that can be computed for each of the different score interpretations for the RRASOR, as well.

Q. For example, on a high score on the RRASOR what are we looking at on a confidence interval?

A. The highest score on the RRASOR for which there

are data is a score of five and that confidence interval is a plus or minus 12.1 percent, approximately -- I should probably leave off the decimals; it almost sounds too exact -- compared to the ten year re-conviction risk figure of 73 percent.

Q. And it was plus or minus twelve points?

A. Twelve, yes.

Q. So 73 was your probability number with re-conviction within ten years; is that correct?

A. That's the number associated on average for a score of five based on the work by Dr. Hanson.

Q. So people that score five on the RRASOR, then, would go somewhere between 85 and 61 percent?

A. 95 percent of the time that would be the expected sampling result.

Q. And is the RRASOR also a tool that you do not have to actually interview the person to complete?

A. That's correct. In fact, the RRASOR is included within the STATIC-99 so some of those same items are -- all of the items on the RRASOR are included within the STATIC.

Q. And where would one go to get a RRASOR?

A. Dr. Hanson's web site, the Solicitor General web

site of Canada.

Q. Great. The other items there that have just been admitted, are they guides to scoring the RRASOR and also an article in support of the RRASOR? Is that what those exhibits are?

A. I'm not sure how to answer your question so let me just describe what they are.

Q. Please.

A. Exhibit 19 is basically a two-page summary that I've put together based on the information that's in the web site. So it's sort of the shorthand way to score the instruments. There is no formal score sheet. Being only four items, there doesn't need to be a formal score sheet.

Exhibit 20 is the written up description by Dr. Hanson that's available on the solicitor general web site describing the development and research supporting the RRASOR. A 29 page document.

And then Exhibit 21 is the February, 192- -- excuse me, February 24, 1999 set of coding rules for the RRASOR.

Q. And all of those came from the internet except for the one that you put together yourself?

A. That's correct.

MR. THETFORD: Greg, do you have any objections to the admission of any of those?

MR. BAL: No.

BY MR. THETFORD:

Q. All right. And if I understood you correctly the RRASOR now has been incorporated into the STATIC-99?

A. It has, yes.

Q. Do you still perform both?

A. Yes.

Q. Can you tell me why?

A. Yes.

Q. Please do.

A. There are both theoretical and research reasons for that so my answer is rather lengthy. Let me start with the research reasons rather than the theoretical.

In Dr. Hanson's developmental work on the STATIC-99 he used four samples, three of which were clearly long-term samples and one was far shorter term. The three longer term samples, when you average across all of their follow up periods, averaged a follow up period of about 16.6 years for a total sample of -- I'm

approximating the number here -- of about eight hundred seventy, eight hundred ninety, something like that. That fourth sample had only a four year follow up on average, with about three hundred subjects, three hundred ten, something like that.

In the development of the STATIC-99 what Dr. Hanson did was compare its incremental value, it's predictive accuracy, in other words, to the RRASOR for each of these samples. Within each of these samples it was -- the STATIC-99 was not better than the RRASOR. But it showed a trend. When he averaged across all four samples then he found that the STATIC-99 was statistically better than the RRASOR. But it was not true in any given sample, just across all four. And that's the basis for his statement that the STATIC-99 should be replacing -- one of the two bases -- should be replacing the RRASOR.

In a generic, potentially short term assessment of risk -- in fact, particularly short term assessment of risk I would not disagree with that statement. In fact, the STATIC-99 clearly does show superiority in the

short run. In other research.

On the other hand, in the specific type of work that I do and that I teach about, has to do with the civil commitment process for sex offenders. In all of the laws that I reviewed, all fifteen, I did not find any time periods specified and it appears to be, at least in my interpretation and the way in which I've done work in all of my cases to date, it appears to be that it is relevant to lifetime re-offense risk of a sexual nature defined.

So what I did was look at whether or not the STATIC-99 had improvement over the RRASOR in the long run. And what I did -- Karl Hanson gave me -- Karl's with a K -- gave me these data and I deleted -- actually, he gave me the data with the deleted short term sample. The four-year follow up. So I had the samples of the three longer term and I looked at the relative effectiveness of the STATIC-99 and the RRASOR. First of all, one was not better than the other. So that effect -- that improvement was in the short run only. Which has been replcated. The STATIC is very good in the short run, short run meaning five years or less.

The second thing I did within that process, however, was look at within the definition of high risk how the two instruments interacted or did not. And without going into all the statistics about it, what I found was that if I defined high risk in an arbitrary way, but in keeping with at least some state laws, of approximately a 50 percent kind of rate, knowing that we're measuring re-conviction and that may be an underestimation, but as an arbitrary cut off, what I found was that when the RRASOR showed a fifty plus percent degree of risk, it did not matter what the STATIC-99 score was. And when I scored the STATIC-99's degree of risk at fifty plus, it did not matter what the RRASOR was. The degree of accuracy for each instrument independently was the same, no matter what the other instrument was. Suggests independence.

A different piece of research. That's one. Independence of course means that we need to be looking at both factors. A different piece of research was done by Dr. Caton, C-A-T-O-N, Roberts and myself where we used a highly select group of Wisconsin sex offenders. People who had already been detained post

probable cause. So they were already well selected into somebody thinking they were high risk. Not all these people ended up being committed, but they were clearly selected in that way. And we looked at the RRASOR, the STATIC-99, the MnSOST, the MnSOST Revised, the VRAG, diagnostic issues, victim categories in terms of age, gender, and looked at -- and statistically looked at patterns. And what we found was that the RRASOR tended to be correlated with sexual diagnoses, tended to be correlated with having child victims, whereas the STATIC-99, as well as other instruments -- including PCL-R, for instance. I forgot to mention -- tended to be correlated with personality disorders, with having adult victims or adolescent victims, and these two did not relate to one another in terms of -- I need to correct that. What they related to didn't overlap. So again, which would indicate independence.

A third piece of research that is supportive to a different dimensional process was not a comparison of the RRASOR and the STATIC directly, but it overlapped, was a

dissertation done by Rebecca with two C's Dempster, D-E-M-P-S-T-E-R, that was done under Steven Hart's tutelage, who is apparently, maybe, giving testimony in this same hearing. In that study, a small numbers of sex offender totaling 95 so it's more suggestive than truly indicative, found that the RRASOR was independent -- I'm shortening the story a lot -- the RRASOR was independent for measures of general violence and yet statistically useful in assessing sexual violence.

Then there's other related research and theoretical work -- which I'm going to summarize rather than go through the whole story. My answer is long enough -- that indicates that there are at least two dimensions, pathways, by which somebody becomes a sexual recidivist once they've been convicted of a sexual offense.

One of those is that they're turned on to something sexually that's illegal. Children, for instance. The other dimension is the person who is a generally violent anti-social kind of person, a person who takes what he wants when he wants it. Is not necessarily turned on to something that's illegal but I will, for

instance, rape people because he just feels like it at the moment. It's part of his general interpersonal disregard for their rights, violent tendencies. Somebody can be both of these, but there is reason to believe that they are independent dimensions. If you're high on one it doesn't mean that you're low risk if you're low risk on the other dimension. If you're high on both, of course, that's a real bad sign.

The metaphor about these two dimensions that I use is if I'm going to get a checkup from a physician I want the physician to check more than just the risk factors related to my heart: Cholesterol level, blood pressure, family history, et cetera. I need to have some assessment, as well, of my lungs, of my brain functioning, neurological system, et cetera. But if I have high risk in some area to my health, there's a high risk to my health. It doesn't matter if the other areas are lower risk. So if I have a malignant brain tumor it doesn't matter that the risk factors on my heart are low. High risk in any single dimension matters.

So I have to assess -- a good assessment is for each dimension. Which brings me back to the -- putting all that together from the other research, if I know that the RRASOR and the STATIC are independent and I know the RRASOR tends to measure this dimension while the STATIC tends to measure in this dimension and I need to assess both dimensions, then I use both instruments.

Q. You get a more accurate picture that way, if you need to measure both.

A. Yes, that's correct.

Q. Both ways. Can you tell the Judge what these actuarial assessments actually measure that help you in determining risk assessment?

A. I'm not sure I understand your question.

Q. We've gone, now, through the three major ones or what I would call the three major actuarials, the RRASOR, the MnSOST-R and the STATIC-99 and you come up with these numbers when you come down to. For the sake of clarity for the Judge, what do those numbers tell you and what do they help you measure.

A. The numbers are associated with a degree of risk for certain kind of sexual re-offending, sexual

re-conviction, sexual re-arrest, general sexual offending or physical contact sexual offending. They help -- in terms of how they help me is not to give me a number, but to tell me ultimately that relative range on that zero to 100 percent possibility so that I get grounded in that level of the science.

And as I said before, I don't stop there. But if I already have somebody who's, let's say, showing a range that's somewhere -- let's, just to take an extreme, between 6 and 16 percent, which in my opinion isn't even close to whatever "likely" means, then I already know that I need to have some really significant sign of increased risk or this person's not going to meet criteria. I don't have to figure out a number, but it grounded me that I needed to know something particularly standing out or this person does not meet.

Likewise, just again to take extreme, if I were going to have someone who was up at the 75 to 100 range, in my opinion that's clearly beyond whatever "likely" means and I'm going to need to have something that would significantly pull me down. I will look for those in both

directions, but I need to find something to pull me down to say that that component of the commitment criteria is not met.

Q. Even though you're an employee, at least part-time, of the State of Wisconsin doing this work have you ever found that a person was not likely to re-offend?

A. Oh, yeah. You have the numbers on that, in fact, in --

Q. Your CV?

A. -- the sexual offender work part of my CV. The -- you have the total number of the assessments that I have done, as well as the proportion in each of many different categories, both in Wisconsin and outside, where I recommended commitment and therefore the others I did not.

Q. When you said that these numbers would help you, grounding you in terms of getting a picture of probability of recidivism, do they help you determine whether or not any specific individual will re-offend? For example --

A. They help in that process. Any applied science is the use of group data to an individual. Again, in the medical metaphor, when the doctor

is checking out my heart, doctor will look at cholesterol level. In order to interpret a two forty or whatever my number is, the doctor has to refer to some group data that did not include me. That gets applied to my situation. In the case of using the actuarial instruments, the process is the same, except that I wouldn't even just stop there, then I look at other things that are individual -- specific to the individual. But the -- the actuarial gives group information related to the category of characteristics this individual shows.

VIDEOGRAPHER: Excuse me I need to go off the record to change.

(A recess was taken from 11:28 to 11:30 a.m.)

BY MR. THETFORD:

Q. Dr. Doren, before we changed tapes you were using a medical analogy and I want to ask you a few more questions about that so it's clear for the Judge, but I want to use myself for an example. Say that I suffer from high cholesterol and that my number is two twenty. If they grouped me together into a group of men with cholesterols of two twenty at my age,

there's probably a medical chart that would tell you what my probability of risk of having a heart attack is. That doesn't necessarily mean that I will have a heart attack, though; does it?

A. No. It's a difference between assessment of risk and making a prediction.

Q. And with the actuarial instruments that you're talking about, all this is doing is making assessment of risk; isn't that correct?

A. That's all I'm using it for; that's correct.

Q. Your CV contains the list of articles which you have published; does it?

A. The general CV is the inclusive list. The sex offender work addendum to that lists the publications I've had specific to that work or the articles that are either in press, meaning accepted for publication but not yet published. It lists my book that will soon be published, and it lists two or three articles that have been submited for publication --

Q. I want to --

A. -- that are currently under review.

Q. I want to focus in on the Daubert standard, if I can, and make sure that we hit those areas that

the Judge will be looking at in the hearing in the coming weeks. We've talked about accepted within the relevant scientific community. You've spoken to that. I want to talk about the issue of, has it been published in peer review journals or has it been peer reviewed.

Dr. Epperson was very frank last week and said that the information on the MnSOST-R had not been published specifically in a peer reviewed journal but that he thought that it had been peer reviewed, and he gave examples of how he thought that the MnSOST-R had been peer reviewed. Do you have any opinions about the STATIC-99, the RRASOR, and the MnSOST-R as to whether or not these instruments have been peer reviewed?

A. Yes I do.

Q. And can you tell the Court whether or not -- or simply tell the Court what your opinions are in that regard?

A. In different formats they have all been peer reviewed.

Q. Such as?

A. Well, the easiest one in terms of that people don't debate the issue at all, that I'm aware

of, is for the STATIC-99. There was a publication by -- written by doctors Hanson and Thornton, T-H-O-R-N-T-O-N, published in a peer reviewed professional journal, Law and Human Behavior, published February one year ago. And that describes the development of the STATIC-99. It includes in its description information about the RRASOR and a different instrument we're not talking about today, Abbreviated SACJ Minimum.

There are other ways in which peer review occurs, however, besides publications in peer review journals. One of those is the process of a dissertation. A dissertation for a doctorate degree invariably, for it to pass, is a process of having a panel of the professors, quite typically at least at an accredited university as having at least one professor from outside of the deputy serving on a panel that the person needs to defend the dissertation to. Both in terms of the acceptance and the idea to do the work, and then the acceptance of the final product. I would pose to anyone that that's probably a far more intense process than a peer review to a journal. A peer review in a journal

involves an editor sending the manuscript to two or three selected individuals, typically knowledgeable in the field, for whom those people then review the manuscript, make some recommendation and the editor decides to publish or not. So there are more people involved, and it involves an oral defense.

Excuse me a moment.

There's another type of peer review having to do with professional presentations, both in terms of the process of having things accepted a presentation, going through the conference committee's approval process and then the reaction of the audience and particularly whether they will adopt the findings or not adopt the findings.

The initial conference committee may be a small number of people. The adoption of the results, of course, is potentially much larger. And --

Oh. One other type of peer review are the few times when a piece of research is submitted for consideration for an award. So for instance, that organization we referred to earlier, ATSA, A-T-S-A, is -- has two awards,

two student paper awards per year and the board or it's designee -- frankly I don't know exactly how they do it -- determines which of submitted papers they will give the honorary award to, student paper award.

If we're looking at that complete -- so again, it's a review process. If we're looking at that complete set, then Rebecca Dempster's work -- did work as part of her dissertation on the RRASOR and that same paper was submitted to ATSA and won the 1999 -- one of the two student paper awards from that organization. And on her committee were people like Steven Hart so it's -- who, again, may be testifying in this. So it wasn't -- it's at Simon Frasier University, certainly an accredited university. It wasn't some fly-by-night process, by any means.

For the MnSOST Revised --

Oh. I should add for the RRASOR, this year's ATSA student paper award recipient was Calvin Langton, L-A-N-G-T-O-N, and his work involved the RRASOR, the MnSOST Revised, the STATIC-99, as well as some other instruments. And clearly the ATSA board thought that that was

a paper -- research paper worth the award. That has since been submitted for publication and is currently under review.

That is listed, by the way, as a reference on page 3 under the RRASOR as well as under the other instruments -- of Exhibit 5.

Q. Five. So if I understand your perspective then, in order for something to be peer reviewed it doesn't necessarily mean that it has to be published in a peer reviewed journal.

A. That would be my interpretation. I mean, I understand that in a Daubert perspective that is purely up to the Court and I'm not trying to usurp that. I would have no right to do so. In my opinion, peer review can include, does include, the peer review journals but can include any process of peer review and acceptance.

Q. And these instruments that we've spent so much time talking about this morning, the MnSOST-R the RRASOR and the STATIC-99, these aren't just instruments that somebody's just thrown out there that people within the relevant scientific community haven't had a chance to look at and to criticize or to support?

A. That would be my opinion, that they've -- it's been more than that, yes.

Q. The judge is also going to be concerned about the error rate on the instruments as one of the Daubert criteria. Do you have an opinion about the error rates of these three different actuarial assessments?

A. Yes.

Q. Let's start with the STATIC-99. Do you know what the error rate on the STATIC-99 is?

A. Well, there are actually different types of errors and so it's not just one error rate. It would be an error, no pun intended, to talk about a single error rate. There is -- I've already mentioned two in my testimony. One is the degree to which raters are not consistent in scoring the same cases. That inter-rater reliability. Let me just call it rater consistency issues.

A second type of error we've been referring to is the confidence interval. That's based on an averaging effect across groups of people. Different groups of people, like in the Gallup polls, are going to come up slightly different even when you ask them the same

question. That's a sampling process where we're trying to approximate the collective populataion, like in the Gallup poll, how the vote will really go, we sample. And that sampling process has error involved in it. The main point to me about those kinds of errors is that they are -- we can approximate them. They are, in essence, knowable.

Q. And the sheet that you prepared that has been admitted as an exhibit shows the approximation of the error rates and they are -- they're available for the Court to consider.

A. On that one exhibit, Exhibit 15, I have estimations of the confidence intervals. For the MnSOST Revised I have one -- I have listed there the same thing you mentioned Dr. Epperson talked about of that degree of reliability across -- consistency across raters. I don't have the relevant statistic on Exhibit 15 for the RRAZOR or the STATIC, but from work I did this past -- in preparation for a presentation this past November I do know those numbers, as well. Again, these are all approximations.

Q. Can you give us those approximations on the inter-rater reliability rates of error on the

STATIC-99 and the RRASOR?

A. Yes. For the RRASOR the standard error of measurement is going to be approximately a half point, and for the STATIC-99 it will be approximately one point or slightly under. And so what that means is that in the RRASOR you would expect relatively good consistency in terms of exactness of findings. For the STATIC-99 it would not be unusual to find a one point difference. That would still be within the consistency rate.

Q. I'm going to ask you about some other experts that have been designated in this case. You know that Doug -- Dr. Doug Epperson has been designated from Iowa State. I've mentioned that to you before. Are you familiar with Doug Epperson's work?

A. His work on the MnSOST Revised. I'm not aware of any of his other work -- except for one piece of work that he and I wrote together.

Q. Do you know Dr. Amy Phenix in California?

A. I know her, yes.

Q. And are you familiar with her work in regard to the STATIC-99 and the RRAZOR and working with Dr. Hanson?

A. I'm at wear of her work on the coding rules of both instruments.
Q. Sitting at the table to your right is Dr. Lynn Maskel who's been designated as an expert by the respondents in this case. Do you know Dr. Maskel?
A. Certainly.
Q. Have you and she testified in cases on opposite sides from each other in the past?
A. Many times.
Q. And she is actually a medical doctor and is a psychiatrist here in Madison; is that correct?
A. Yes.
Q. She has numerous criticisms of the use of the actuarial instruments; does she not?
A. I think that's fair to say.
Q. And you have heard her criticisms in court before; is that correct?
A. Yes, I think on either two or three occasions. It's common that I have testified first and then not heard her testimony, but there've been two or three occasions when I've heard her testimony.
Q. And you're going to have the opportunity later today to hear her testimony, should you decide

to stay and hear her testimony in this case, but I want to ask you a couple of questions.

Dr. Maskel testifies in court and has testified to me in a deposition previously that these instruments are not accepted within the scientific community. You've told us that you think they are. Can you describe for the Judge why it is that Dr. Maskel would think that they were not and why you would think they were? And as a corollary question, does that have to do with how you're defining "scientific community"?

A. To answer the second part first, I think that's clearly true. That it's related to how far one expands the definition of the relevant scientific community or the field.

In addition, I'm trying to recall what I have heard in Dr. Maskel's testimony previously in regards to that issue. My understanding, if I remember correctly, is that she has made reference to a group, the American Association for Psychiatry and the Law, if I remember the words correctly. AAPL is what I know it as. A different organization. And her summary -- I'm not a member of that organization so I don't have direct knowledge of it. If I remember

correctly, her summary is that there are at least people within that organization who are very clearly against the use of these instruments.

Compared to my -- I don't know if I can say more about her testimony. Compared to my perspective on it, I think I've said earlier one of the main points is that one can expand that definition of "field" too widely. Just because someone has an opinion doesn't mean they have a knowledgeable opinion. I think that if people don't -- haven't studied the field, their having an opinion really doesn't matter very much -- or shouldn't.

And yes, there are some people who have studied the field who disagree with the use of the instruments. There are some. I am by no means saying that it is a unanimous perspective among the people that I have defined to be, in my sense, part of the field. But generally accepted or widespread acceptance, in my understanding of those terms -- again I'm not a judge or jury, a judge or an attorney -- in my understanding of those terms does not require unanimity just a widespread, I believe is the

word in the Daubert ruling, use or acceptance of the, in this case, the instruments.

Q. You've mentioned Dr. Hart from British Columbia. Do you know Dr. Hart?

A. Yes I do.

Q. He has been designated as an expert for the respondents who will be critical of the actuarial assessments. Are you familiar with his opinions which are critical of the actuarial assessments?

A. I am aware of them in some detail, yes.

Q. Can you summarize for the Court what you perceive to be as Dr. Hart's criticism of the use of actuarial assessments?

A. Just as a basis for understanding my knowledge, I have not heard Dr. Hart testify. What I have is -- what I am aware of are both transcripts of his testimony and copies of various slide presentations, what he has presented, as well as had conversations with him both through e-mail and in person about these kinds of issues. And some of those conversations have gone back literally some years.

Within the context of that information, his -- as I understand, his criticism is that

there are -- basically two points to it. One is that the methodology as a concept is fine but that the instruments have not been tested well enough to this point.

Q. How do you respond to that?

A. In two ways. First of all, I don't know that Dr. Hart is aware of basically what's in Exhibit 5, the volume of research that exists. I have made it a study to try to find out all of that, and where Dr. Hart does a lot of very good work I don't know that he spends his time in the same way I spend my time. That's not a criticism of him, that's just a difference. And so the issue of what is good enough or sufficient enough may be a value judgment, as well. There is no perfect outcome for reliability, consistency in raters. There is no perfect outcome for demonstration of validity or accuracy. It's always a matter of degree: Is it sufficient for use? And in -- to whatever extent Dr. Hart is aware of that research, for him it is not sufficient. What I am aware of indicates for me it is, particularly within a context, and that context is what the alternatives are. The research is very

consistent, in my reading of it, that these instruments work. That clinical judgments of certain types on top of them even improve the accuracy. Without them we'd fall back to the list of risk factors. Which is better than unaided clinical judgment, but not consistently as good as the use of the actuarial instruments with or without additional clinical judgments.

So I consider my role to be as accurate as possible in an assessment and to give technical information to the Court rather than just come up with an opinion in some more magical way. And so to me it's more ethical for me to be using things that have demonstrated the highest degree of accuracy that we have and that that is, in effect, the definition of "good enough": It's the highest degree of accuracy that we have.

Q. What's his other criticism?

A. That -- this one I find a little bit more strange. That information that is known from the past can't be applied to the present. He doesn't quite word it that way. He words it in terms of the difference between postdictive studies and predictive studies. Predictive is,

I start now with people being released now, I score them up on however I want to do that, I make predictions and then wait some number of years.

Q. Follow them for a number of years after that.

A. Follow them for a numbers of years and see what happens.

Q. See if your predictions are accurate.

A. And see to what extent predictions are accurate.

Postdictive is the same kind of thing, except we're gonna take it and put it into the past. So we take information of people who were released, let's say, ten years ago. Only the information that was known at the time that they were released. Score them up in whatever way we're going to make the predictions based just on that information, and then look to see what happened. It's the same model, but it's put into the past in total.

He makes a major difference out of these two designs for research. And he points -- he makes the statement that there is no predictive research. That's actually not true; there has been. But most of the research is postdictive.

Q. Dr. Epperson told us last week that he had

conducted a post-predictive study on the MnSOST-R and did it in the way you just described very well for the jury and for the Judge about going back and taking out any information from the Department of Correction files which would indicate whether or not the person was re-arrested or re-convicted and just put the basic data in the folder that the person would need to score the MnSOST-R to come up with a number so that they could compare it with what they knew about that group. Is that what you're talking about? That kind of study?

A. Yes. And it's very important in the postdictive process that anything that would indicate what the final outcome was is taken out of that original review of records.

Q. Right. And Dr. Hart says that's not good enough.

A. Dr. Hart says -- the best I understand him, is that the process of using old information like that, you don't know how well it applies to the current and therefore that gap is a problem.

The reason it -- conceptually, on the one hand, it sounds good. But the problem is that it is a concept, as I understand it, that

undercuts all science. Because there is no current meaning, for instance, to use my old metaphor, of a cholesterol two forty. We have to look at, what did it mean for other people basically from the past. Even if it was predictive, even if the study started in what was a now and moved forward, by the time that study is done it has become history. And so when we apply the information we got to the now, we're applying old information. That is true for any applied science. Period. There's no such thing otherwise. So it's a strange argument coming from a scientist -- he's a researcher. He's a professor -- to say that we can't use the past to talk about the assessment of the current, because that's the only thing we can use. But that's my understanding of his argument.

Q. Do you know Dr. Randy Otto in Florida?

A. Yes.

Q. Do you know what his criticisms are of the actuarial based assessments?

A. I know from transcripts of what I have read from his testimony.

Q. Can you summarize those --

A. And from one article and -- that he's written, as well as from e-mail interactions with him.
Q. Can you summarize his criticisms for the Court?
A. I believe so. I believe he has a main one, and then maybe some derivatives. The main one is not about the actuarial process. He seems to very clearly acknowledge that there's research indicating that the actuarial process, if you have a reasonable way of -- tools for doing that, is the right method. But his issue is with the specific instruments that are currently available, and he basically says that they have not demonstrated inter-rater reliability, that consistency across raters, and not demonstrated validity. So that they don't meet the standards for use. The standards that he's referring to are that set of 1999 American Psychological Association standards for educational psychological tests I mentioned earlier.

But he concentrates largely on inter-rater reliability and validity. I have already been aware of him testifying that no studies of inter-rater reliability on these instruments exist when I already knew that they did. So clearly he was, in that sense, ignorant of some

of the relevant pieces of research.

That, to me, is the main criticism that he's put out: That they've not, in a sense, been researched well enough yet and therefore don't meet standards. They clearly have been researched beyond what he was at least aware of at the time what I had communicated with him.

Q. Do you know Dr. Terrence Campbell?

A. I've heard him testify on two or three occasions. I have also read an article he had published in the year 2000.

Q. He criticizes use of the actuarials, as well; does he not?

A. He criticizes every attempt at assessment of risk. Including use of the actuarials.

Q. Does he essentially argue that you cannot assess risk?

A. He says we cannot assess risk sufficiently, and should not be doing so in a courtroom. Period. In any which way whatsoever.

Q. What's his solution?

A. That we shouldn't be -- we, as psychologists and psychiatrists, shouldn't be in the courtroom doing this work.

Q. And so is it a criticism of the civil commitment

laws in general, as well?

A. I don't know that I know his opinion about the laws. What I am aware of from his testimony is, he has issues with the implementation of them, particularly related to the assessment of risk.

Q. Do you know Dr. Woodworth at the University of Iowa?

A. I met him in one Fry hearing in Iowa. Did not know him previously.

Q. And he's critical of the statistical analysis used in these actuarials; is that correct?

A. I am aware of the answer to your question to the extent I heard one -- him testify on one occasion and he was clearly critical of the developmental process of the MnSOST Revised. He was less critical of the RRASOR and the STATIC. It had to do with sample sizes. The sample sizes in the development of the RRASOR and the STATIC were larger, significantly, than for the Minnesota Revised.

He also had issues with the degree of error that was implied through the process though he was also testifying that he could not give exact perspectives on that, given that he didn't have direct access to the data.

He also acknowledged, during testimony, that if there were sufficient replication of any of these instruments in terms of what they were assessing and their consistency in assessing it, that it would diminish in his criticisms. He did not appear to be aware of at least some of the research in Exhibit 5, though I do not know that for a fact.

Q. It says specifically in their expert designation that Dr. Woodworth will also testify that the margins of error are not within the acceptable ranges for science and that the state's experts, including but not limited to Dr. Doren's testimony regarding statistics, is flawed and not statistically accurate. Have you ever heard Dr. Woodworth criticize your testimony regarding statistics and that your testimony is statistically inaccurate?

A. I did not hear him mention my name or seem to refer to me during that one time he was testifying. He did listen to my testimony afterwards. I have no idea what his reaction was.

Q. Do you think perhaps it has to do with the article you published regarding recidivism basic

base rates?
A. I would only be supposing. I do not know.
Q. That's the most writing, publication, that you've done on a statistical basis; is it not?
A. That may be the publication relevant to sexual re-offense assessment, risk assessment that involves the most statistical work. That may be.
(Exhibits 22, 23 and 24 were marked for identification and copies are attached hereto.)
BY MR. THETFORD:
Q. These were articles that you brought with you, Dr. Doren, I just want to put in the record, as long as counsel doesn't have any objections. Exhibit number 22 is an article by Hanson and Thornton, Improving Risk Assessments for Sex Offenders: A Comparison of Three Actuarial Scales. I think you've spoken about that article this morning; is that right?
A. Yes. This was one I was giving as example of a peer review journal publication related to the STATIC-99.
Q. The second has to do with recidivism and rapists and the article is entitled, Assessment of Risk for Criminal Recidivism Among Rapists: A

Comparison of Four Different Measures. That has to do with recidivism rates and predictions of risk among rapists; does it not?

A. Yes. And it was -- it's a summary from research in Sweden that used -- basically validated the use of the RRASOR with that set of people. The authors' names -- I will let you take a look at it -- is pronounced Sjostedt and Langstrom.

Q. Very good. I wasn't even going to try.

The next one is Exhibit number 24. It's, Predicting Relapse: A Meta-Analysis of Sexual Offender Recidivism Studies, by Dr. Karl Hanson and Monique T. Bussiere, B-U-S-S-I-E-R-E. This article has to do with the meta-analysis that Dr. Hanson put together; does it not?

A. One of the meta-analyses, yes.

Q. And you've relied upon that article to some extent in your testimony today; have you not?

A. The information is relevant to things I was talking about, yes.

Q. The next is a article -- and we'll get you to get the court reporter to mark that for me.

(Exhibit 25 was marked for identification and a copy is attached hereto.)

(Next page, please.)

BY MR. THETFORD:

Q. -- that you authored for the Sex Offender Law Report having to do with Evidentiary Issues, Actuarial Scales, and Sexual Offender Civil Commitments; does it not?

A. Yes.

Q. Does this article contain your opinions on those issues?

A. Relative to five issues that are brought up in evidentiary hearings relative to the admissibility of actuarial instruments.

Q. Issues similar to the ones that are going to be brought up in this hearing?

A. Some of them we have talked about; some of them we have not.

MR. THETFORD: Do you have any objections to the admission of any of those articles, Greg?

MR. BAL: No objections.

MR. THETFORD: Great.

Q. Next, Exhibit 6 you provided to us, which is called Psychopathy and Recidivism: A review. That has to do with the PCL-R; does it not?

A. Yes. This was a review article, quite lengthy, published in 1998 of the relationship, in effect, between the PCL-R and recidivism of

various types, including but not just sexual recidivism.

MR. THETFORD: Greg, do you have any objections to the admission of that article?

MR. BAL: No objection.

BY MR. THETFORD:

Q. And last but not least is an article that you authored called, Recidivism Base Rates: Predictions of Sex Offender Recidivism and the Sexual Predator Commitment Laws, that you wrote in the Behavioral Sciences and the Law journal in 1998. I'm going to ask you some questions about that. I'm certainly not a statistician or a mathematician. I've read that article a number of times and this is where I come out with it. That article attempts to argue for a position as to what base rates of recidivism are for rapists and for pedophiles; is that correct?

A. I would change the latter term to extra-familial child molesters. Not all of them are pedophiles. Pedophilia is a diagnostic category versus the real category of a child molester. "Extra-familial" meaning not just purely incestuous.

Q. And you make an argument that the base rates

have been underestimated for each of those groups; do you not?

A. That there are people who will talk about the rates in ways that are underestimates when you are looking within the context of lifetime re-offense rates. To me, the whole issue --

Q. Which is how you would look at it. You'd look at it as lifetime re-offense rates.

A. Within the context of sex offender civil commitment laws, yes. Now there are various other circumstances where that would not be the appropriate context, it would be something, for instance, about the first year on probation may be of -- may matter to somebody, in which case the numbers are very different from lifetime re-offense rates. It was a contextual analysis.

Q. And when you reviewed the Texas law, did you see anything in the Texas statute which limited the amount of time that the trier of fact is limited to in determining whether or not a person is likely to recidivate sexually?

A. I did not find anything in the Texas law or any of the other laws that are analogous to this, basically of the sex offender civil commitment type, that specified a time factor relative to

the risk being assessed.

Q. Tell the Judge what your conclusion -- and the court, what your conclusion is in terms of rapists, what their lifetime re-offense rate is, statistically?

A. I will answer the question. I wish to give the caveat ahead of time that, again, there is some degree of estimation and therefore error so I do not wish the number to be considered like, This is the number. I do not have that exactness.

In this article what I was doing was basically supporting the finding -- ultimately what happened, more accurately, was, I supported the finding that a lifetime sexual re-offense rate for rapists of about 39 or higher percent was a reasonable estimation.

Q. Over their lifetime.

A. Over their lifetime.

Q. Would your estimate of re-offense rate over the lifetime put you in the majority of people within your relevant community or outside your relevant community?

A. I know of four, including myself, people who have made -- or groups of people who have made estimations. I don't know that I can speak

beyond that. I know that this article has been very frequently cited. I don't know -- and I doubt that it's always been in a positive way. But there are lots of evaluators in the civil commitment context around the country who have cited this.

But putting all of them aside, because I don't have any other direct knowledge, the other people of which I am aware who have made estimations of lifetime re-offense rates for rapists and for extra-familial child molesters include Karl Hanson, that we've mentioned before; David Thornton, who was involved in the development of the STATIC-99; and two people by the last names of Janus, J-A-N-U-S, and Meehl, M-E-E-H-L.

The bottom-line perspective Karl Hanson puts out is for that group of people, rapists and extra-familial child molesters, that their re-offense -- sexual re-offense likelihood, not specifically getting caught, not specifically getting prosecuted or convicted, but the re-offense would be somewhere between 30 and 45 percent as a group.

Janus and Meehl, in a publication in 1997

when they were doing some other analysis, needed to estimate a lifetime re-offense rate for their analysis and they ended up estimating somewhere between 20 to 45 percent.

My estimation would include all of those as between basically 39 and 52. Let me just round it off to 40 to 50 because again, these are estimations.

The other piece of information comes from Dr. David Thornton who has some very long-term follow up data from British corrections with a sixteen to nineteen year follow up, depending on where the data came from. He has actual re-conviction rates so it's not a statistical extrapolation, it's not a maneuvering of data, it's actual hard body count, how many of these people were re-convicted. When you look at the complete set of sex offenders released from British corrections -- I should say UK corrections, so British and the English and Wales, from 1979, and in the sixteen to nineteen year follow up if you look at just the rapists and extra-familial child molesters together, their average is just over 30 percent actually re-convicted.

When I look at that, then I have to think that the Janus and Meehl 20 to 45 percent -- their 20 to 30 is underestimation. And that Dr. Hanson's 30 to 45 would suggest that the 30 means that re-conviction rate is the same thing as re-offense rate. That's highly debatable, and I would not agree with the statement. In any case the re-offense rate, even if we give that as a statement, the re-offense rate overall, then, across all of us, would be approximated between 30 and 50 percent. So am I in the ballpark? I think I'm in the ballpark and I'm on the higher end of it.

Q. What about extra-familial child molesters? What do you estimate as the lifetime recidivism rate for that group?

A. In what I was just describing I was including those people. In this article I had them at approximately 52 percent. I would note that in the analysis I did, that I was finding that the extra-familial child molesters had higher re-offense rates than the rapists, over a long period of time. In the data from Dr. Thornton, actually rapists showed a higher re-conviction rate than did the extra-familial child

molesters. So it may be that Dr. Hanson is correct ultimately that their long time re-offense rates are not different between those two types of offender.

Q. If I can summarize your testimony this morning, Dr. Doren, for the Court, from what you've said I would gather the following. It's your opinion that the actuarial assessments that we have talked about have reached a level of scientific accuracy such that they should be admissible in court.

A. In my opinion that's correct.

Q. You also agree that they should not be used alone, in and of themselves. That they should also be supplemented with clinical judgment from trained clinical professionals based upon their training, practice, education and experience; isn't that right?

A. Very clearly correct within the context of these types of assessments.

Q. With these types of assessments. It's also your opinion that these actuarial assessments have undergone peer review in numerous sorts of ways which you have recounted for the Judge.

A. Yes.

Q. And it's also your opinion that each of these actuarial assessments has a known error rate which we have talked about today during your deposition.
A. Multiple error rates, yes.
Q. And that none of those error rates are such that someone who works in this field of science would hesitate to use the instruments based on those error rates.
A. I believe if they're familiar with the estimates of those error that that would be correct; they would still be using the instruments and just taking those errors into consideration.
Q. And when push comes to shove the use of these actuarial assessments when supplemented with clinical, empirically derived assessments are the best predictor that we have for assessing risk of sexual recidivism available to us in February of 2001; is that right?
A. If I understood your question correctly, what you're asking me is that the use of the actuarial instruments is part of the process that is the most accurate process we know. I would agree with that, yes.
MR. THETFORD: And at this point I will

pass Dr. Doren for questions.

MR. BAL: Why don't we take a lunch break at this point and we'll do the cross-examination after lunch.

(A luncheon recess was taken from 12:12 to 1:40 p.m.; Exhibit A was marked for identification and a copy is attached hereto.)

EXAMINATION

BY MR. BAL:

Q. Dr. Doren, I've done, I think, a number of depositions of you before, either deposition or hearings so --

A. We've done one of each, I think.

Q. At least. At least, seems like. Let me just go over some of the same questions that you were asked on direct before I get to any specific questions I may have. According to your employment, right now you are a half-time employee of the State of Wisconsin?

A. That's officially how I'm listed, yes. I sometimes work -- in fact, I frequently work a few more hours than that, but effectively yes.

Q. I'm a state employee also so I understand that. But you only get paid for half time, though;

that's correct?

A. Actually, I get paid for the hours that I work.

Q. And then the rest of your work is outside of the employment for Wisconsin.

A. That's correct.

Q. Approximately what percent of the work that you do is involved with SVP evaluations?

A. I would first need to clarify your question. When you say SVP, the actual doing of evaluations?

Q. Yes, actually evaluating respondents.

A. For the State or for -- in private or both?

Q. Well, I guess you can break it down either way.

A. It's difficult for me to assess that. I'll tell you why. The -- up until this summer I was actively still taking cases in my state employment to do the assessment along with the people that I was supervising also taking cases, and then this past summer I was given additional supervisory responsibilities, basically not just the pre-commitment but the post-commitment reevaluation process. And basically part of the bargain in doing that was that I would theoretically have enough staff to take on the evaluation duties that I had myself. So I've at

this point pulled back in doing cases. So for the State now it is the rare case that I'm still doing. Like, I have one this week that I need to complete, but it's -- it's the relatively rare case.

In my private practice I would say -- it's hard for me to approximate. I'll guess about half or more of what I do is direct evaluation work.

Q. And in that evaluation work do you use the actuarial -- actuarial instruments we're talking about today?

A. Typically. When they apply, yes.

Q. And you also give instructional training on the use of these actuarial instruments?

A. Yes. I have, in various places, various times, yes.

Q. Do you get paid for that instructional training, as well?

A. Certainly. Either on state time if it's within Wisconsin's employment, or in private practice.

Q. You had a survey earlier of points of contact in different states in which you kind of summarized which states were using which instruments. The points of contact, were those people that you

knew from having -- doing training?

A. You're referring to the July, 1999 survey about the frequency of use of the instruments?

Q. Yes. I think the second page of that was a hand tally sheet.

A. Right. And you're asking about the people who served as state liaisons my, contact people?

Q. Yes. How did you know about them? Was that through training that you provided on actuarials?

A. No. In no case was that true. I knew them through other means. There were some -- just to be -- to complete the answer, there were two who later hired me to do training, but that's not how I met them.

Q. You got your master's in Florida, I believe?

A. My master's was in Pennsylvania. At Bucknell University.

Q. And what was the subject matter of your master's?

A. It was a master's in psychology, if that's what you're asking.

Q. Did you have a thesis or dissertation?

A. Yes, master's thesis.

Q. What was the emphasis of that thesis?

A. A test having to do with study habits of individuals.
Q. And you used statistical methods to complete your thesis.
A. Oh, I must have, but frankly I don't have any recollection of what I did. But that's typically what has to occur in order for it to be passed.
Q. Do you have a degree in statistics?
A. Degree in statistics, no. I have a minor during my graduate training at Florida State.
Q. And what was the subject matter of your Ph.D. dissertation?
A. The Application of the MMPI-Based Criminal Classification System in a Forensic Hospital Setting.
Q. Seems somewhat relevant to what you're doing now?
A. I have used that information about the MMPI-Based Criminal Classification System a handful of Wisconsin sex offender commitment cases, but the application work to a forensic hospital setting I've not used directly. It overlaps in concept.
Q. I believe you testified earlier that the MMPI is

similar in many ways to the PAI?
A. In terms of its generally looking at personality characteristics and in the way in which it was developed.
Q. Now, you earlier talked about a proposal that the ATSA committee is looking at regarding the adoption of actuarial instruments.
A. Yes.
Q. Did they also have a provision in there regarding the use of MMPI?
A. There is a statement in that same section -- I could either quote it if you wish me to look at it or just paraphrase, that no psychological test -- and then in parentheses, for example, and I think they do mentioned the MMPI -- should be used without demonstrated empirical relationship -- statistical relationship with sexual recidivism. I think that's a fair paraphrase.
Q. Based on your experience with the MMPI is there an empirical relationship with sexual recidivism?
A. By my experience you're referring to what I read in other people's research?
Q. Just your experience, knowledge, training, yes?

A. In the Hanson and Bussiere meta-analysis published in 1998 -- of which one of these exhibits is that article. Maybe I should find you the number. Exhibit 24 -- there is a finding that two scales on the MMPI show some degree of statistical relationship with sexual recidivism for people who were previously convicted of sexual offending. The other scales, if studied enough -- either have not been studied enough or don't show that relationship. The relationship for those two scales is of a nature, however, that makes it difficult to use in that there's no clear threshold of how much is enough.

Q. And do you have the same kind of problems when trying to use the PAI to predict sexual recidivism?

A. I know of no research that indicates that any scale or the overall instrument in any way of the PAI is related specifically to sexually -- sexual recidivism in previously convicted sex offenders.

Q. When you started working with sexual recidivism back in 1994, I believe you stated on direct that you looked at the different characteristics

of recidivists?
A. From professional research, yes.
Q. Was one of the pieces of research the meta-analysis done by Karl Hanson?
A. That did not yet exist. No, that was completed in 1996, was published in 1998, but I was composing this initial list of relevant and irrelevant, as two separate lists, of risk factors in the summer of '94.
Q. And when you decided what's relevant and what is irrelevant, was that based on the correlation between recidivism and these factors?
A. Typically, and not always. Most of the research back then looked at correlation statistics, but that was not always the case. And so what I was mostly looking at was what correlated statistically and what specifically did not correlate so I knew what not to look at.
Q. What type of factors did you list which did not correlate with recidivism? I'm going to talk about recidivism today. I think you understand that we're talking about sexual recidivism.
A. Certainly. I'm trying to recall. It's been a while since I've looked at those lists. Diagnosis of what's traditionally called mental

illness. Things like schizophrenia -- and let me abbreviate and call it manic depressive illness. Called bi-polar. Those diagnoses did not seem to correlate.

MR. THETFORD: Let's go off the record a second.

(A recess was taken from 1:49 to 1:52 p.m.; Sharon Patrick and Jack Schairer entered the deposition room.)

THE WITNESS: I'm afraid I lost my train of thought. I was in the middle of answering something.

BY MR. BAL:

Q. I believe that I had asked you about factors that you had identified --

A. Oh.

Q. -- back in 1994 --

A. Yes.

Q. -- which indicated a person would not recidivate?

A. I mentioned about a traditional mental illness diagnosis did not appear to correlate. There were mixed reviews of various things, but I -- I'll name those if you wish, but that's not directly what you're asking me. I'm trying to

think of some that just did not get supported.
Q. Well, that's okay. I think we can move on.
A. I'm afraid I can't think of any more.
Q. I was more interested in whether or not the factors were the same as those identified by Hanson in the meta-analysis.
A. I noted when the meta-analysis came out -- which for me was 1996 when I first got a copy of it. I noted that there was an overlap, but it was not the same set. For instance, in my list I remember specifically that I had found seven studies that looked at the relationship of alcohol abuse with sexual recidivism and had found five that were supportive and two that were not. So a mixed result. And that in Drs. Hanson and Bussiere, B-U-S-S-I-E-R-E, meta-analysis, their correlation overall for the studies that they were looking at found no significant relationship and it's -- they're different measures. Because I was just looking at number of studies in terms of a consistency of result, where he basically compiled a single statistic by putting it all together, but it suggested that the seven that I looked at maybe had an over-emphasis in seeing a relationship

that statistically wasn't there.

I had, I think, only one or two studies that were suggestive of a relationship between history of child sexual abuse by the person we're now labeling as the offender with sexual recidivism, and that also showed no relationship in the meta-analysis.

On the other hand, of those things that Dr. Hanson were -- Hanson and Bussiere found to be related to sexual recidivism, if I had found them in my summary I had also found them to have a significant relationship. So some of the things that I thought may have been related did not end up getting supported in the meta-analysis, but of those things the meta-analysis did support I did not have a contrary finding.

Q. Did the met an analysis even attempt to look at factors which would indicate a person is not likely to recidivate?

A. The statistical answer to your question is yes, but if you look through -- well, in the process of doing the meta-analysis Dr. Hanson used a correlational process, which is basically a statistical -- varying in the same way. As one

moves the other moves. And -- in some predictable fashion. In that sense, as something is moving away from recidivism the lack of recidivism tends to follow. So in that sense the answer is yes.

He did not use the information in a predictive way to say, well, this much of this attribute should be -- will be a recidivist and this much is only of people who won't be recidivist. That was not what he did. So it was -- the answer statistically is yes, but it doesn't mean that he came up with, Here are a list of things that mean someone won't be a recidivist. That's not the way it worked.

Q. Then based on this meta-analysis he developed an instrument which could be used, an instrument which had a little bit more of a scientific basis called the RRASOR?

A. The results of the meta-analysis were the fundamental source of information that helped him select seven items to be studied as an instrument. He studied those with seven or eight, at one point, different samples, found that four of them pretty much did the work of all seven, and those four became the RRASOR. It

wasn't that the meta-analysis was the basis for the RRASOR, but it served as the basis for choosing the items to look at that eventually became the RRASOR.

Q. And the four items on the RRASOR are the exact items that are also on the STATIC-99?

A. Those four items are in both instruments, yes.

Q. Now, earlier you drew a comparison between actuarial instruments used by insurance companies, for example life insurance, and the actuarial instruments we're talking about today. Do you remember that?

A. In defining what actuarial process looks like.

Q. Now when you talk about, for example, automobile insurance; you don't get the same rate every town in the country; correct?

A. My understanding that that's correct. You don't.

Q. And the reason is because insurance companies look at different regions, even different cities, and they evaluate if you get the same results for each geographical region.

A. That's my understanding is that they do look at the regional differences and do find that there are regional differences that matter.

Q. You were present in Ft. Dodge where Dr. Woodworth testified a few weeks ago.
A. I believe that's where I heard him testify that one occasion, yes.
Q. And you recall Professor Woodworth also saying that geographical differences are important and should be looked at when developing these actuarial tools.
A. I believe he did make such a statement.
Q. Specifically, you recall him talking about the RRASOR and STATIC-99 as being instruments which should not be used in the United States because the samples were from, I believe, England, Wales and Canada?
A. I remember him making such a statement, though I believe it's inaccurate. But he did make such a statement.
Q. That was my question. You recall him making that statement?
A. Yes I do.
Q. And you don't agree with that statement?
A. No I do not. The original developmental samples were both from Canada and the UK. But there's since been replication of both instruments in the US.

Q. Has there been --
A. I need to correct. For the RRASOR it did include a California sample.
Q. Has there been replication of these instruments in the State of Texas?
A. There was recently -- and I mentioned earlier -- a small study on the STATIC-99 application to people who had been adjudicated as juveniles to have done a sexual offense and were now, at the time of the study, 18 or 19 years old. Sample size was only forty nine so it was statistically suggestive rather than truly demonstrative of its validity, but it was consistent with the validity of that instrument. That was in Texas. That was the Texas Youth Commission.
Q. Didn't you itself earlier that you have an ethical problem with applying these instruments to females and young juveniles?
A. Females, certainly. And I said with young juveniles, that's correct. And this study was not specifically young juveniles. It was a study that I was mentioning that would suggest that there may be some applicability of these instruments to the older juvenile population, but still the amount of research for that group

is smaller. You were asking the question about these instruments being directly tested in Texas, however, and I was answering that question for you.

Q. And have there been studies replicating findings that the RRASOR, STATIC-99, and MnSOST-R on adult offenders, male offenders, in the state of Texas?

A. Not that I'm aware of.

Q. None of the actuarials we're talking about today made a distinction based on a subject's race; did they?

A. Are you talking about within the instrument or within the development of the instrument?

Q. I'm talking about factors that are considered. They didn't break down factors based a person's race; did they?

A. In the instrument, itself, that's correct. In the development of the instrument at some point in the process -- and I would have to define for you that point -- the issue of minority versus majority race was considered someplace in that process and not found to be needed, within the samples that were studied, to be on the instrument itself. That issue is not on any of

those instruments.

Q. You were talking about this issue was considered in the process. In the process for which instrument? For the RRASOR?

A. I said in some step in the process. So yes for the RRASOR and STATIC in the following way. As I mentioned earlier, the RRASOR ultimately were four items chosen out of seven. Those seven were chosen out of the set of characteristics from the Hanson and Bussiere meta-analysis that were most highly correlated with sexual recidivism and also easily, or at least relatively easily, obtained information.

Within the study of the meta-analysis, itself, was the issue of minority versus majority race. And was found not to correlate with sexual recidivism. In that sense that issue was looked at in the development of the RRASOR, and therefore also of the STATIC which was the next step. It was not directly part of the RRASOR or STATIC studies. I don't wish to mislead at all. It was part of something that got considered in, ultimately, the selection of items.

Q. But in the -- from the meta-analysis the factor

of race was not selected to be a factor in the RRASOR and ultimately the STATIC-99?

A. That's correct. And just to fill in an answer I couldn't remember before, one of those things that didn't correlate in my original study of things was race. You helped remind me.

Q. Now, the three actuarial instruments we're looking at today do have a cut-off for age and what scores you get based on your age; correct?

A. Age of the person at the time of assessment is an item on each of those three instruments.

Q. And the highest age any of the actuarials go to is age 31; correct?

A. The MnSOST Revised, of these three, the cut-off is the person's 31st birthday.

Q. And none of these instruments break down age in any greater detail other than over the age of 31?

A. On all three instruments there's a specific age, either 31st birthday or, for the other two instruments, the 25th birthday and the person is scored either as lower risk by being older than that or as higher risk for being younger than that.

Q. And none of these instruments give you a

breakdown, say, if a person is 40 or 45?
A. That's correct.
Q. Or 50 or 55?
A. That's correct again.
Q. A person could be 60 or 65?
A. Anything above that threshold is all treated the same.
Q. They're all treated the same.
A. On that instruments.
Q. And none of these instruments look at whether a person has good or bad community support if they were released; true?
A. The instruments we're talking about, that's true. That item used to be on a different instrument, the Minnesota Sex Offender Screening Tool, on one of twenty-one items, but otherwise is not on any of the current actuarial instruments that are commonly used.
Q. And none of the instruments has a factor for a person's religion; correct?
A. That's correct. That's another one of those that did not correlate that -- you're reminding me as we go.
Q. Now, the proposed policy that ATSA is considering adopting, at this point is just a

proposal; correct?
A. It is a draft proposal that came from a committee that was sent to the board and now they've gotten feedback about it.
Q. And it has not been adopted at this point?
A. It is not an official statement. I did contact a person by the name of Dr. Arthur Gordon, G-O-R-D-O-N --
Q. My question was, at this point it's not officially adopted; correct?
A. It is not officially adopted, and it was also viewed by him as their current stance.
Q. In doing evaluations do you generally use the DSM-IV?
A. Yes. We're talking sex offender civil commitment evaluations? Yes.
Q. We're still talking about sex offender --
A. We won't keep qualifying that, I'll just understand that.
Q. And DSM-IV is in wide and general use by evaluators for doing sex offender commitments?
A. That is my opinion, yes.
Q. Now DSM-IV wasn't developed by psychologists; correct?
A. That's correct. It was developed by the

American Psychiatric Association.
Q. And are you aware that there are forensic psychiatrists around the country who also do evaluation for sex offender commitment?
A. Yes. There are a number, though the number is far smaller than psychologists who actually do assessments. But there are some states where psychiatrists are commonly involved and there are states where there are no psychiatrists involved. There are always psychologists involved in any of these states.
Q. Are you aware that in the State of Texas that only psychiatrists can do sex offender evaluations?
MR. THETFORD: Objection, form. You don't have to answer that question.
BY MR. BAL:
Q. Do you know what the Texas statute says about whether psychologists or psychiatrists can do sex offender evaluations?
MR. THETFORD: Objection, form. Don't answer that question.
BY MR. BAL:
Q. Do you know whether the Texas courts will accept an evaluation by a psychologist for sex offender

commitment?

MR. THETFORD: Objection, form. Don't answer that question.

BY MR. BAL:

Q. Do you have any knowledge whether you can go in Texas and do a sex offender evaluation?

A. There are a number of considerations that I have not looked into. Before I can do any work in a state I need to make sure that there is -- it is appropriate under that state law that I do practice psychology in that state within my current licensure. Given that I am not licensed in all of these states, that would be the first thing I would need to look at before anything about the details of the law, itself. I've not looked into that for Texas. I've not been asked to do a case there.

Q. So you have not reviewed Texas statute with regard to who can or cannot do sex offender evaluations?

A. I don't recall looking at that detail. I am sure I've read it, because I read through the statute, but I don't recall that detail.

Q. Now, earlier you talked about the PCL-R and that was developed by Dr. Hare, H-A-R-E; correct?

A. Correct.
Q. And using that essentially make a determination of whether a person is or is not a psychopath; correct?
A. It can be used in that way. A different way to describe it is, it looks at the degree of psychopathy. But either way, that's correct.
Q. Now, psychopathy has not been accepted as a diagnosis under the DSM-IV; correct?
A. It is not a diagnosis in the diagnostic manual as a separate category. It is described as an associated feature within the category of anti-social personality disorder.
Q. How old is the STATIC-99?
A. By its title it came into existence basically in January of 1999. The research was actually done in 1998.
Q. How about the MnSOST-R? Do you recall when that came out?
A. The fall of 1998 is when it became available. It's first presentation at a national -- actually, international conference.
Q. At this point you no longer use the MnSOST-R; correct?
A. I do use the MnSOST-R.

Q. Don't use the MnSOST. I'm sorry.
A. I do use the MnSOST Revised. I do not any longer use the MnSOST.
Q. Is that because the MnSOST-R is more accurate than the MnSOST was?
A. That's part of a reason. That is an accurate statement. I had been using both instruments for a while because there had been more research on the original form than on the Revised. There is now sufficient, in my view, research on the Revised that I don't need to bolster my confidence in the interpretation of the revised form by using the original form in addition. The statistics for the Revised stand by themselves.
Q. The MnSOST-R was developed for the purpose of replacing the MnSOST?
A. That was the intention according to Dr. Epperson who's the main researcher. It was to be, shall we say, the new and improved variety.
Q. Do you know approximately how many people you recommended for commitment using the old version of the MnSOST-R?
A. Of the --
Q. -- MnSOST.

A. MnSOST? First of all, I didn't -- that I recommended for commitment?
Q. Yes.
A. First of all it would help me to answer the question if I look at Exhibit 3. I'm looking at pages eight and nine where I have a list of the number of evaluations that I've done. What I don't have here are the time periods. The -- I would have to make a gross approximation to answer your question. The MnSOST became available to me during the early spring of 1997, late spring 1997, and I was using it with other instruments including, starting in the fall of 1998, with the MnSOST Revised, through this past early fall, I think it was. I'm not sure exactly when that was. So through -- let's say early fall of 2000. So I used the MnSOST for approximately three and a half years, sometimes with the MnSOST Revised as well as time period earlier. I've been doing these assessments from June of 1994 through the present, which is a period of, let's approximate, six and a half years. So if I just approximated maybe about half of those that I've done. Whether I recommended for or against, the numbers can be

found on page eight and nine of Exhibit 3.
Q. And the scores that you got on the MnSOST, those were different in some cases than the scores on the MnSOST-R?
A. Well, the scores would always be different because they were in a different range. But if you mean -- you're asking me did they show a different degree of risk?
Q. Yes.
A. There were times that was true, yes. And that spoke to the reason why I was using both instruments. That at that point in time did I not feel certain enough about either one of them to stand alone, and a mixed picture told me I had a problem in the interpretation.
Q. Did you ever go back to cases in which you used only the MnSOST to see if perhaps the score may be different if you applied the MnSOST-R to the same data?
A. The risk category is what you mean by score?
Q. Yes. Have you ever gone back and done that?
A. Probably as part of a research project that I did with Dr. Roberts in Wisconsin. I probably, in effect, did do that follow up process of scoring, for research purposes, some people on

the Minnesota Revised instrument who had at the time of the original assessment that instrument had not been part of the assessment, just the MnSOST. But I have no idea what the results are offhand. I've never looked at it that way.

Q. I don't think you talked earlier about this research project that you have done with Dr. Roberts, and that some of your opinions are based on this research that you have done with Dr. Roberts. Do you have the results of that -- any type of documented form?

A. I gave a presentation, both the 1998 and 1999 ATSA conferences, where those data -- summaries of those data were presented. I don't have that with me.

Q. Has that research or the findings of the research been published?

A. Not at this time. There are plans for that, but not at this time.

Q. But it was completed in 1997?

A. No. The work was -- the initial form of it, with some sample size that later was expanded, was completed just before the ATSA conference, 1998. That would have been, I think, September so I finished it about in October -- August,

rather, of 1998. And then we expanded the sample size, we being Dr. Roberts and myself, and presented further findings in 1999. Again, that work would have been pretty much right up to the conference date so fall of 1999.

Q. Are you planning on submitting that work for publication in the future?

A. Yes, but actually as part of a bigger project, not as a stand alone article. It's designed to be probably one of three segments of a larger article.

Q. You use the MnSOST-R to do your evaluation in the State of Wisconsin; correct?

A. Under circumstances of which I consider appropriate application, yes.

Q. Are there differences in the drop-out rate in treatment programs between programs in the State of Minnesota and programs in the State of Wisconsin?

A. Yes.

(At this time Jack Schairer left the deposition room.)

BY MR. BAL:

Q. And how do you compensate for those differences when you use the MnSOST-R N State of Wisconsin?

A. There are two items in which that matters in the MnSOST Revised, items number fourteen having to do with chemical abuse treatment, and item fifteen having to do with sex offender treatment.

Q. Let me clarify. I was referring only to sex offender treatment, so if you can limit your answer to that?

A. Okay. It actually applies both, but I'll limit it in the way you wish. Concerning sex offender treatment, the basic answer -- and I'll go into detail if you wish. The basic answer is if the person has completed the program here then the person gets credit for it. If the person has participated to any degree then the person doesn't get scored less than a zero without there being a very significant reason for scoring him in a higher risk direction. The only situation in which I would make a habit -- in this point, when talking about a Wisconsin case, of a person -- scoring the person in a higher risk direction than zero -- which is average. Baseline -- would be if the person actively did something that got himself terminated from treatment very early in the

process. Without that -- and not just a simple conduct report. I mean, it has to be some, shall we say, illegal behavior that was just handled within the system.

The reason I go to that extreme is because the differences in the completion rates in the two places vary significantly and so I would need to have a very extreme reason for scoring -- to penalize somebody, in a sense, to score a higher risk direction, or else I'll just give the person a base rate score, a base line score -- unless he actually completed the program. Then I give him full credit.

Q. Do you know what Dr. Epperson's recommendation is?

A. He has had two different recommendations. One of those recommendations is what I just described. The other recommendation is to score it as a zero base line throughout. Those are the two of which I'm aware.

Q. Do you know whether a newer version of the STATIC-99 is being developed.

A. I expect what you're referring to is the Risk Management 2000, the RM-2000. I do not know whether or not that's actually an update on the

STATIC-99 or what has been more described to me as an update on the SACJ Minimum, the instrument that is used in the UK, but nowhere else. Structured Actuarial Clinical Judgment hyphen Minimum Scale. That is -- the SACJ Minimum is one of two scales, the RRASOR being the other, that went into the development of the STATIC. But it is -- so the RM-2000 in that sense overlaps a next step from the STATIC, but it's not directly the next step from the STATIC.

Q. Is Dr. Hanson the person who's developing this new test.

A. My understanding is that Drs. Hanson and Thornton are both involved. I do not know if anyone else is.

Q. So the same two people who developed the STATIC-99.

A. And the RRASOR; that's correct.

Q. Well, Thornton wasn't involved in the RRASOR, though?

A. Right. But I meant it overlapped the developers of the RRASOR. You're correct.

Q. You talked earlier about some states in which courts have excluded these actuarial instruments. Are you aware of a recent ruling

in Missouri in which actuarial instruments were excluded?

A. I am not aware of any such ruling in Missouri.

Q. You spoke earlier about using both a clinical and an actuarial approach when doing evaluations, versus a clinical by itself or actuarial by itself.

A. Clinically adjusted actuarial? Yes.

Q. You used the term, I believe, intensity of interest --

A. Yes.

Q. -- when talking about an item.

A. Yes.

Q. Would you please summarize that for me, please?

A. What I was referring to was some research that looked at -- actually specific to the RRASOR, whether or not someone's sexual interest towards children would suggest greater risk than what the RRASOR was already assessing. And the research found the answer to be both yes and no. It is "no" in terms of if it's simply a question of is somebody pedophilic or not, that adds no predictive information to the RRASOR. But that same research showed that for people who -- the way in which the research design was

done, for people who had an inability or unwillingness to suppress their interests when in a physiologically testing situation, that the process of suppressing versus not was incrementally predictive beyond the RRASOR. It is that that I'm referring to as the intensity of the pedophilic interest.

Q. When you talk about unwillingness to control, is that not a subjective evaluation?

A. What I'm referring to is quite specifically that they did not end up demonstrating control. The interpretation of that is, either they were unable or unwilling or both. I can't tell you which it was for any given subject. The researchers couldn't tell you that. What they knew is that the person did not end up controlling.

Q. In other words, they committed another offense?

A. No. What I mean by that is that when they were in the situation of a penile plethysmograph, p-L-E-T-H-Y-S-M-O-G-R-A-P-H, testing situation and they were told -- after they showed that they had pedophilic interests, and then they were again brought, in effect -- or continued the testing, they were asked to -- or told, I

should say, now to suppress their reaction. There were some people who did and some people who did not. Those people who did not, showed higher recidivism rates than those who did, even compared to what the RRASOR would have suggested. It was additional information that was useful. Not the simple fact of showing sexual deviance.

Q. And you're talking about people who were already confined and have undergone the penile plethysmograph?

A. That's how that research was done; that's correct.

Q. And I guess I was referring to the clinical setting. How does a clinician determine whether a subject is or is not able to control?

A. That is more problematic. The penile plethysmograph can be done, but it is not typically something that happens, even in sex offender civil commitment evaluations, even if systematically offered.

Q. And if it's not done then you do have a margin of subjectivity in a clinician's decision.

A. There is a margin of subjectivity in that item, as well as other items. That is part of the way

business is done at this current time. It's a question of how much subjectivity there is.

Q. Now, there were studies done on the purely clinical method; correct?

A. What I referred to as unaided, yes.

Q. And the findings were that essentially the same as chance or slightly better than chance?

A. On average they were better than chance, but not by much. And what also mattered is, there was a lot of variability across studies. Some of them showed nothing better than chance, some of them showed a significant better -- significantly better than chance. On average they were better, but not by much.

Q. Let me back up just a second back to the -- you were talking about some data about penile plethysmograph. Which studies are you referring to or which research? Can you identify that?

A. The one that I'm referring to is listed for you in document number five, Exhibit number 5. It is on page 2. The Hanson -- excuse me, Haynes, H-A-Y-N-E-S Yates, Nicholiachuk Gu, G-U, and Bolton study.

Q. When you do the clinically adjusted evaluation do you first use the actuarials?

A. Yes.
Q. Is that the first step?
A. Again, assuming applicability, yes.
Q. And then based on the results of the actuarials do you then adjust one way or the other depending on what you find in the clinical setting or from additional information?
A. My looking for things to -- whether or not there should be clinical adjustments, and to what degree, does not depend on the results of the actuarials. I will do that no matter what the results of the actuarials are. In terms of looking at -- do I look at things? Always. Do I always make adjustments? No. In some cases there is no reason to be making an adjustment after looking for reasons to be making adjustments in either direction.
Q. You used the term earlier, I believe, that -- when you used the actuarials that somehow grounds your final opinion or --
A. It grounds me before I look on further.
Q. And what I was, I guess, asking was what you meant by grounding. I mean, does that set the parameters, essentially, of which way you can vary or which way you can adjust the results of

the actuarials?

A. It does -- no, grounding doesn't have anything to do with which way I can adjust. I can still adjust in any direction -- either direction or not at all. By grounding I just mean as a starting place. On that zero to 100 percent possibility of somebody's risk it's going to start me out in that -- whatever range seems to be applicable to the individual. And so it will, in a sense -- how would I -- I'm trying to think of a good metaphor, here.

I can't think of a good metaphor so I guess I'll just continue describing. It will anchor me, is another phrase. It will, in a sense, keep me tethered to an area so I don't stray too far in making adjustments in either direction.

So if I have someone where the actuarials are, including the confidence interval, still showing low risk, then I'm not going to find something that, you know, well this attribute just stands out to me so I'm going to say he's up here despite all that, without something awfully extreme. I'm -- as a characteristic. I'm too tethered to this. I'm too anchored to

that area. And the same would apply in the other direction or in the middle going towards either extreme.

Q. So in addition to doing an actuarial assessment you also factor in your own clinical experience?

A. And training and whatever -- yes. All that does become part of it, including what not to adjust for.

Q. Now, when you're doing a purely clinical assessment it tends to overestimate the risk; correct?

A. That is the research suggestion, and I would tend to think that to be true, that people who are doing the unaided clinical process tend to overestimate risk.

Q. And is there research to indicate whether a person who uses a clinical in addition to actuarial tends to lean towards the direction of overestimating risk versus underestimating risk?

A. I'm aware of eight pieces of research that have looked at the types of instruments we're talking about today or either of two others. Six specific to the instruments we're talking about today and then two other studies, one using the SACJ-Minimum and one using the VRAG, that then

had a clinical adjustment process. So my answer to your question is just out of those eight studies.

Out of those eight studies what they've -- and those are listed, again, on Exhibit 5, the very bottom of page one into page two. In the -- in those studies what they found was that with certain types of adjustments the accuracy increased. And that was not true for all types of pieces of information. So a clinical adjustment can actually decrease the accuracy of an actuarial instrument if it's an improper adjustment.

And those eight pieces of research also consistently demonstrated, across all eight, that proper adjustments increase the accuracy. It was not specifically to one direction or the other, it was in the direction of accuracy.

Q. But improper adjustments could decrease the accuracy?

A. Yes they can.

Q. And Exhibit number 5, this is a list that you compiled?

A. Yes it is. I would point out that I got help in the original formation of it from various

people, but ultimately it's my list.

Q. And these are various, I guess, papers or presentations which both support actuarials as well as come out against actuarials; correct?

A. Yes, it includes all that. Presentations, publications, dissertations. These were -- I have them divided per instrument and then when looking at whether they're supportive or not. So far for the instruments that we're talking about, all the studies of inter-rater reliability are supportive so I don't have a category of not supportive of the inter-rater reliability because for the RRASOR, for the STATIC-99, and for the MnSOST Revised there are no such non-supportive studies.

For the validity, for the demonstration that they're measuring what they're purported to be measuring, then for each of the instruments there's at least one study that did not support. But the vast majority, virtually all of the studies for each of the instruments do support, and I have those listed in the proper categories in that paper.

Q. There's a study by Barbaree, Seto, et al. which, for example, comes out non-support of the

MnSOST-R.

A. It did not support the validity of the MnSOST-R for a four-year re-arrest for sexual offending study.

Q. But Barbaree also comes out in support of the STATIC-99?

A. STATIC-99, the RRASOR, and other instruments.

Q. So at this point there is a array of opinions regarding validity of some of these actuarial instruments. There are differences in opinion.

A. Well, if you're asking me is it absolutely uniform in the field that everyone agrees, as I testified before, no. And I doubt that there will ever be uniformity about almost anything in psychology.

On the other hand, if you're asking is there consistency for a vast majority, then in looking at a list like this it's clearly a vast majority would say there's support for validity of the instruments we're talking about.

Q. If we're going to look at the vast majority of these items that you have listed in Exhibit number 5, the vast majority have not been published. Would you agree with that?

A. The vast majority have not been published.

There are a significant number that have been submitted for publication and the vast majority are, at least at one time, presentations that may also now be submitted. Conference presentation.

Q. My question was whether you would agree that the vast majority have not been published.

A. At this point in time a vast majority have not been published.

Q. And some of these are poster presentations. I believe there were some poster presentations at the ATSA conference in San Diego of last year?

A. Yes. Yes to both parts of that.

Q. And a poster presentation, is that essentially where people stand in front of a booth or a display and give handouts to people of their research?

A. Yes. They got to did that because the committee said okay to their submission for doing it and -- the conference committee, I mean -- and that's basically what they do. It's not a presentation in a formal sense except on a board with handouts.

Q. When the committee accepts something for a poster presentation or even for a more formal

presentation, is there a process by which the underlying data is reviewed?

A. Typically not the underlying data. The concept of what will be presented needs to have been submitted in an abstract form, and the committee reviews the abstracts that are submitted. The detail that goes into an abstract probably varies a lot. I've never been on one of those committees, however. I don't know the details in that regard.

Q. Does your list include an article by Karl Hanson titled, Will They Do It Again, Predicting Sex Offense Recidivism, which was published in June of the year 2000 in Current Directions in Psychological Science?

A. No, it doesn't include that article because that's not an article that includes specific research that had not been available elsewhere that would demonstrate a test of inter-rater reliability or validity. That's more of a summary article and a thought piece. I don't have those in that -- in Exhibit 5.

Q. And it's not one of the exhibits that you introduced on direct; correct?

A. No it's not. I am familiar with that article.

It's not something I happened to bring with.
Q. Now, you did introduce several articles by Dr. Hanson.
A. There are some in there, yes.
Q. Would you agree that he is one of the leading researchers, if not the leading researcher, in the field of sex offense recidivism?
A. He's one of them.
Q. Along with Dr. Epperson?
A. He's another one.
Q. In fact, between those two they account for the three actuarial instruments we're talking about today; correct?
A. To at least have had a part in them if not the sole -- well, if not the sole part. Dr. Hanson for the RRASOR. That would be correct.
Q. And the STATIC-99 Dr. Hanson developed in conjunction with Dr. Thornton?
A. That's correct.
Q. But he developed the RRASOR by himself?
A. That's my understanding, and that's certainly the way it's advertised.
Q. Would you agree that he probably knows a little something about the RRASOR?
A. He probably knows a great deal.

Q. Now isn't it true that Dr. Hanson recommends that you do not use both the RRASOR and STATIC-99?
A. Yes, as I mentioned in earlier testimony, as well as one of his main reasons why.
Q. But you do not agree with his conclusion?
A. No. For the main reason that I stated earlier that I went into some detail about. Not within the context of this type of assessment. In a different context I would simply agree with him. In a shorter term follow up context I would agree with him.
Q. Now, you also did your own analysis of some of the underlying data for the STATIC-99; correct?
A. Yes, that Dr. Hanson was gracious enough to let me use.
Q. And did you show Dr. Hanson the results of your analysis?
A. Yes I did.
Q. Did you publish the results of that analysis?
A. It's been submitted for publication; it's currently under review.
Q. Now, you offered Dr. Hanson a chance to put his name on that publication; correct?
A. Yes I did.

Q. Is his name going to be on the publication if it's published?
A. No. He decided did he not want it on it.
Q. Now, earlier you were comparing, I believe, four different types of assessment methods going from purely clinical to the clinically adjusted. Also had in there research guided clinical and purely actuarial.
A. That's all correct.
Q. Now, the purely clinical is the least accurate method; correct?
A. The research would lead me to believe so, yes.
Q. And in your opinion?
A. And in my opinion. Both in terms of the number of people assessed as high risk as well as the general predictive process of differentiating who will versus who will not recidivate.
Q. And the clinically adjusted is, I believe, in your words, is at least as good if not better than any of the three previous methods?
A. Based on the research that I've summarized in Exhibit 5; that's correct. As well as other research, I should say, talking about certain things like the research guided approach that's not talked about much in Exhibit 5.

Q. Inter-rater reliability is an important factor?
A. Yes it is.
Q. And it's one of the -- one of the causes which would introduce error into the calculation?
A. Yes. It could. The concept of different raters not scoring in a reasonably consistent way over the same cases would be a problem. And to some extent there will always be a non-perfect inter-rater reliability coefficient where except for trivial things there's going to be some degree of variation, but we would want to approach 100 percent as close as we can. Consistency.
Q. The only thing 100 percent is death and taxes though; right?
Let me refer you to Exhibit number 15.
A. Okay.
Q. Now, you list various confidence intervals for the RRASOR, STATIC-99, and for the MnSOST-R you actually have the standard error of measurement. Let's talk about the RRASOR. What is the source of -- source of the figures that you have listed?
A. The ten-year risk figures are directly from Dr. Hanson's work on the RRASOR, the

developmental research. I did not adjust those in any way there, or add any other samples. The 95 percent confidence interval figures were therefore also based purely on the developmental research adding -- that are on Exhibit 15, based on just the developmental research samples, the nearly twenty-five hundred people for the RRASOR that went into its development. These set of confidence intervals do not include any samples that anybody else looked at since. To the extent that I, for the ATSA presentation I did this past November, then did look at other samples and I know that these numbers will vary a little bit because of the increased numbers of people per category. They actually don't change all that much with the numbers that I had.

Q. You talked about increased number of people in each category. This is new data that has come out?

A. What I mean by it is, this is from what Dr. Hanson did. And then when I took some of the studies from last exhibit we just had -- 15, I think it was -- no, I've got 15. Whatever it was. When I looked at some of those pieces of research from Barbaree, Seto study for instance,

when I looked at adding Rebecca Dempster's data, when I added data to the original data that Dr. Hanson did, then the confidence intervals have different numbers to their basis and so they move a little bit. But actually, they didn't move a lot.

Q. For the RRASOR, when you have a risk figure of 73.1, what that actually means is, it's somewhere between 61 and 85 approximately; correct?

A. If you took into consideration the 95 percent confidence interval that would be the correct interpretation, is that 95 percent of the time a group of people with that score will show, in that case, the re-conviction of a new sexual offense within ten years. Somewhere in that range.

Q. Somewhere in that range. But that does not account for the inter-rater variability; does it?

A. That's correct. It does not. The confidence interval doesn't change in size, but it will move.

Q. The entire interval will move up or down?

A. The entire interval will move towards the

average, whichever direction it happens to be.
Q. And when you have inter-rater reliability that's going to introduce a margin of error.
A. Not in the size of the confidence interval, but in the --
Q. Right. The direction.
A. -- direct interpretation; that's correct. It is again measurable or accountable. One can estimate it. But yes, it is an error that should be looked at.
Q. But this particular Exhibit, number 15, none of these three groups that you have here takes the inter-rater reliability error into account; correct?
A. That's correct. It does not. I was simply listing what an associated confidence interval would be with the risk figures that Dr. Hanson had initially listed.
Q. Let's go to item number B and that's the STATIC-99. Did you get those figures from Dr. Hanson, as well?
A. The fifteen year risk figures, yes. Dr. Hanson and Dr. Thornton, but basically off Dr. Hanson's web site, yes.
Q. The MnSOST-R figures, what period of time are

those figures based on? The STATIC-99 is fifteen years, the RRASOR is ten years. How many years are we talking about for the MnSOST-R?

A. For the confidence intervals here it was for the six year re-arrest measure of sexual recidivism, specifically for physical contact sex offenses.

Q. And the risk of recidivism does vary depending on what time period you're looking at; correct?

A. In general you're looking at the same group of people over time, yes. There will be an increase over time for some number of years out from the release from incarceration for the same group. It will be ever expanding up to a point of which no one really knows when it finally ends.

Q. But these three groups that you have here, we're actually comparing apples to oranges to pears because we're looking at different time periods; right?

A. If we're talking about how to make sense out them, one against the other versus a third, then one does need to take into consideration that they're different time periods and different measures, re-conviction, re-arrest.

Q. For the MnSOST-R, where did you get the percentages? Are those from Epperson's research?

A. I'm trying to recall. I know I had gotten some figures from him, but I also did my own computations. I'm thinking these are my computations from his data, compiled by -- I'm not sure now. I'm not sure if this is -- if this is -- are these that are listed from his computations based on his developmental research or if they're my computations based on his developmental and some of the cross validation research. I don't recall that. Again, it's -- the set that I actually use is a more comprehensive set that I presented at ATSA, but this was a summary I was giving just as an example. I've forgotten exactly where these numbers come from. It's one of those two.

(A recess was taken from 2:50 to 3:07 p.m.)

BY MR. BAL:

Q. Dr. Doren, I'm going to talk about recidivism base lines; okay? And would you agree that there is some disagreement about what should or should not be the base line for sex offender

recidivism?

A. Well, there's always a context to even answering that question. Are you referring to lifetime re-offense estimates or are you talking about something else?

Q. Well, we could start off with lifetime re-offense.

A. Then I believe that the difference is as I described it during -- difference in perspectives is as a described in my direct testimony, ranging from Janus and Meehl's 20 to 45 percent to my being on the other end of that of 40 to 50, with David Thornton's research data suggesting that a 30 percent re-conviction rate within sixteen to nineteen years is probably a bottom line as a minimum.

In terms of shorter time periods -- and so there's some disagreement and some degree of agreement. I'm not sure how to answer the question with a yes no. In a shorter time period or measuring purely by re-conviction, then it gets -- the degree of differences narrow. It becomes clearer and clearer as we get to shorter time periods and more just restricting to a re-conviction measure, for

instance.
Q. On Exhibit 17 you have a graph for the MnSOST-R?
A. Yes.
Q. And three different categories on the graph based on 15 percent base line, 21 percent and 35 percent base line.
A. That's correct.
Q. Now Dr. Epperson, he recommends a 35 percent base line for the MnSOST-R; correct?
A. That's mostly true. I think -- I'm not positive. I think that he qualifies that statement by specifying that the 35 percent rate would be more of his estimate of getting closer to a lifetime re-offense while acknowledging that his scale is looking at a six year re-arrest. At the same time, I believe he also acknowledges that the 21 percent base line is probably closer, and rather close to a reasonable six year re-arrest rate.
Q. Well the MnSOST-R is based on a six year time period; correct?
A. The research underlying it is six year re-arrest; that's correct.
Q. And you use 21 percent base line in the State of Wisconsin; correct?

A. That is what I recommend. That is what I use, and that is because it is a reasonable approximation of the reasonable base rate for six year re-arrest in various samples.
Q. Do you know if every state that is using the MnSOST-R uses 21 percent base rate?
A. I don't know that I know that. I can tell you what I have trained. I cannot tell you what they actually do and I have not been in all states that do these assessments.
Q. And if there are differences in the base lines that are being used, that could also introduce a margin of error; correct?
A. In the interpretation there would be differences based on these different base rates and in that sense there would be error across raters on the same case, in theory, in other words the same score, in the interpretation of those scores.
Q. Generally speaking the lower the base rate, the lower the score for risk of recidivism; correct?
A. The lower the recidivism risk associated with that score, yes. That statistically will always be the case. To a point. Those have to go together that way.
Q. Now Exhibit 17, was that taken from

Dr. Epperson's presentation?
A. Yes -- one of his presentations, yes. Though I believe this slide is also in the -- I'm virtually certain that the same bar graph is in his web site information.
Q. And do you generally agree with Dr. Epperson's conclusions in the second slide on Exhibit 17?
A. Generally.
Q. The actuarials that we've been talking about primarily look at static factors; correct?
A. "Static" meaning historical, not able to change, yes. Vast majority of them or all of them, and depending which instrument.
Q. Although the MnSOST-R does contain a dynamic factor, that being whether the person has completed sex offender treatment?
A. As well -- that's true, and the dynamic factor of the completion or participation in chemical abuse treatment.
Q. And I guess if a person was a particular age and they go over a cut off score, potentially that could also change their score?
A. Yes. If they go from younger than that threshold to over that threshold it would go from a higher to a lower degree of risk, or at

least a higher or lower score which, if it changes risk categories, would be a lower degree of risk.

Q. Other than the two types of treatment we talked about and age, once a person gets a score on these actuarial instruments, that score is set in stone, essentially.

A. Are we talking just the MnSOST Revised or are we talking any of these instruments?

Q. Just a general statements about any of these instruments.

A. The concept you're describing, that except for a few items, at most, on any given scale that once someone shows a certain degree of risk that they cannot decrease that on the instrument, that concept is correct. There are -- on the STATIC-99 there's one other exception having to do with a significant marital-type relationship of at least two years that the person may not have had previously. That length of relationship, or maybe the term is stability of relationship, would lower risk as well, but outside of the occasional item along those lines your statement is absolutely correct.

Q. It's a little bit difficult to have a long term

stable relationship if you're confined in treatment, though.

A. If you're confined then it automatically doesn't count on this instrument. On the STATIC-99 -- during the time you're confined.

Q. The STATIC-99, did that look at both charges and convictions in the development sample?

A. For one item, absolutely. For four items, it's just conviction; and for other items, it's -- I think three other items it's inclusive of convictions and charges and reasonable allegations. The thresholds and type of material varies depending on the item.

Q. When you say reasonable allegations, that's something that could vary from one evaluator to another?

A. Yes, that's correct.

Q. So that is another -- I shouldn't say another. Is that a factor that contributes to the error associated with inter-rater reliability?

A. That's exactly correct that the issue is whether or not raters actually do score that differently and to what degree and that's built into the issue of inter-rater reliability.

Q. But a person could get points -- points being

bad for the respondent -- because an evaluator looks at what they were charged with, not necessarily what they were convicted of; correct?

A. That's correct.

Q. And that is also true for the STATIC-99 because you can get points based on charges, not necessarily convictions.

A. I thought we were talking the STATIC-99. You were talking about the RRASOR before?

Q. MnSOST-R. I apologize.

A. I'm sorry, what was your question then?

Q. MnSOST-R also gives points based on charges, not necessarily --

A. Will consider the information related to charges offenses even if the person was not later convicted, as long as the person was not specifically acquitted. There is that caveat. And yes, then under those circumstances that information does go into the scoring system of some of the items, I believe it's seven of the items out of the sixteen on the MnSOST Revised.

Q. Let me go to the STATIC-99. I believe you testified earlier about recidivate equaling re-conviction. I'm not sure exactly what that

statement was. That for STATIC-99 recidivism was defined essentially in terms of re-conviction?

A. That the percentages attached -- what I was referring to is that the percentages attached to the scores, the total scores on the STATIC-99, are described by the researchers, the developers, as measures of re- -- the percentage of likelihood for re-conviction during the relevant time periods.

Q. And re-conviction depends on the different jurisdictions from which the samples were taken?

A. To some extent there's going to be jurisdictional difference, at least in theory, in what crimes get charged. And on the other hand, I'm not certain. It may be true, but I am not at all certain that the process, once someone gets charged, of getting convicted of something varies from jurisdiction to jurisdiction. I don't know that's true. The charging process, I've heard enough to believe that that's true.

Q. How about the definition of what constitutes a sex offense for the purpose of recidivism? Could that also vary from jurisdiction to

jurisdiction?

A. The answer to your question is yes. And at the same time the issue in the scoring system, as I understand it, for any of these instruments is to not specifically be looking at the name of a charge, whether it's sexual or otherwise, but to be looking at the underlying behavior from which that charge stemmed. Then, yes, there would still be gradations, but it would not -- in terms of degree of which it was sexual, but it would not be necessarily jurisdictionally based differences.

Q. When you talk about looking at the underlying behavior, that's based on interpretation of a record; correct?

A. Ultimately that's true. The issue in concept is that people working in this area are obviously familiar, as presuming everyone in this room, that there is a process of plea bargaining or even of deliberately charging something with one aspect of the offense and not necessarily the other because it's simply easier to prove: An assault versus a sexual assault or attempted sexual assault, just go for the assault attempt. So there can be difference as long

those lines. But when you look at the underlying behavior it can be quite clear that it was sexual in its attempt -- or intent, should say.

Q. But the interpretation of that could lead to inter-rater variability; correct?

A. Absolutely correct that it is an issue of inter-rater reliability, of consistency across raters. That if there is a significant difference in those kind of interpretations we would expect wide variability in the scoring system and that, in theory, should have shown up in one of the inter-rater reliability studies. One or more.

Q. Talking about inter-rater reliability studies for the STATIC-99 and RRASOR?

A. For those, as well as the MnSOST-R. For any of them.

Q. And have there been studies done on the inter-rater reliability of the STATIC-99?

A. Yes. Three. They're in Exhibit number 5, again.

Q. Okay. Could you please point those out?

A. On page six in the middle of the page there are three studies under Roman numeral I where it

says, Concerning Inter-rater Reliability. Those are of direct tests of inter-rater reliability. I point out that all validity tests are also tests of inter-rater reliability. You can't show something is measuring something appropriately if you can't measure it consistently. That's a statistical property. So if you demonstrate it is measuring what we think it is, then we've also demonstrated it's measuring it consistently enough.

Q. And that has not been published; correct?

A. What has not been published.

Q. The research you were just referring to.

A. Those three studies, the Barbaree, et al. study has been submitted for publication. It is the Calvin Langdon dissertation that he defended, and it is the ATSA student paper award winner. The other two studies are just presentations at this point in time.

Q. And do you know how inter-rater reliability was measured in each of these three studies you refer to on page six?

A. The first two I immediately do know, and I'm trying to remember the third. I believe I remember all three.

Q. Let me ask you a question about the third article listed there. The author is -- first author is Wong. Talks about inter-rater reliability of violence risk scale.
A. Yes.
Q. Is that anywhere related to the three actuarial instruments we're talking about today?
A. The violence risk scale sex offender version is the STATIC-99 plus twenty items of a dynamic nature measured before treatment and after treatment. And you look at changed of those twenty items from the pre- and post-treatment process. The STATIC-99 serves as the historical set of information that gets included in that instrument.
Q. So that is a study of the inter-rater reliability of STATIC-99, in addition to these other instruments?
A. No. It is a study -- the inter-rater reliability study was specifically of the STATIC-99 portion. Period. But it was done within the context of the testing of those instruments, as well.
Q. Now the study by Barbaree, the first item that's listed there, indicates there was little

support -- I'm sorry, a non-significant trend for the MnSOST-R.

A. As I mentioned earlier, yes.

Q. Is that the reason you listed that under the non-support category for the MnSOST-R?

A. The non-support for predictive validity. That is the one study listed there on page nine; That's correct.

Q. All right. Let's go back to Exhibits 15 and 17. In 15 I believe you testified earlier that the higher the score, the greater the margin of error around that score?

A. The wider the confidence interval, which is a measure of error, yes.

Q. And the reason for that is because there was an insufficient sample associated with that score?

A. I would not consider it accurate to call it an insufficient sample. What is accurate is, one of the reasons for a wider confidence interval is that the sample size of -- the number of people having those scores are far smaller in any given sample compared to the number of people having the lower risk scores. With smaller number of people going into the statistic, we have less confidence that it's a

narrow range. It becomes a wider range.
Q. Under MnSOST-R on Exhibit 15, the standard error of measurement is listed as two point three five?
A. Yes.
Q. Now is that two point three five in terms of a percentage or is that in terms of the score you would get on the MnSOST-R?
A. The latter. It's the score you get on the MnSOST-R.
Q. So, for example, if someone gets a score of eight, that would essentially mean -- I'm going to round down to two instead of two point three five -- it could be six to ten; is that right?
A. That would be one way to look at that statistic; that's correct.
Q. Is that an inaccurate way to look at that statistic?
A. There are caveats to that process, but it's not inaccurate. They're just qualifiers. One qualifier, for instance, is that the most proper interpretation, even within a range, is still in the middle of the range. So if someone's score -- if you score up someone at an eight you can say that because of the standard error of

measurement, that inter-rater reliability measure, that other trained people are going to score this person, vast majority of the time, within a range of six to ten. That's the proper interpretation. But in my interpretation of an eight, it's still most appropriate to interpret the eight. All of those things are true.

Just to be clear about this error, if I had a score of -- on somebody of plus fifteen and I gave that give or take plus two, all of that's still in the same high risk range. And so this type of error becomes inconsequential in that case. In the situation you're describing, of a plus eight give or take two, then we're changing risk categories. That would make a difference in the confidence I would have behind the interpretation. It does not change the interpretation of what an eight means, but it changes my confidence that that is the number for this fellow -- the degree of risk, I should say.

Q. For the MnSOST-R, the category associated with the score of thirteen and above, what is the highest range in that category and how high does that go?

A. The scale goes up to plus thirty-one. To my knowledge, if I remember correctly, the highest score I've ever seen is a plus twenty-four.
Q. How about the sample that Epperson used? Do you have an idea of how the distribution goes for that category? Thirteen and above?
A. It's -- the highest score in the developmental sample was a plus seventeen.
Q. So essentially the scores for samples who score anywhere between thirteen and seventeen are being averaged to come up with this percentage? Would that be correct?
A. That's effectively accurate, yes.
Q. So the person who gets thirteen -- the percentage associated with the score of thirteen, perhaps, may be higher because you're also averaging people whose scores are much higher, who is risks are much higher?
A. That certainly was part of what Dr. Woodworth had been testifying about when I did hear him, that that was an issue. And on the one hand, technically, I don't have issue with that interpretation. At the same time -- I mean, technically that is an accurate statement, that it could be true.

And ultimately I would also point out the numbers are small so percentages are not that exact when you have small numbers of people. They move around. The real issue to me is when we're talking about in that level, is that even if one said that the thirteen should go into the lower category of the eight to twelve, that's still a very high risk category so ultimately in terms of relative to my understanding of state commitment thresholds, such as "likely" in Texas, that it's still going to be clearly above that level. We could debate -- and I don't know if it's worth doing -- the exact percentage. We don't know exact percentage in that sense. There's always those errors. But is it still clearly above threshold? My answer would be yes.

Q. But it's difficult for compute exact percentage for the sample in thirteen because there just is not a large enough sample; correct?

A. I would pose that with anything the exact percentage is problematic. Because of --

Q. Or more exact percentage?

A. The more exact. The smaller the number of people involved the more percentages can move

around by any one or two people and so yes, there is more chance for movement -- again, which is why the confidence interval is wider. It's the same -- we're talking the same thing in different terms.

Q. Now, when Dr. Woodworth testified he talked about certain statistical means by which you could compensation for a smaller sample. Do you recall his testimony on that?

A. I did. I do remember that, yes.

Q. And when you were questioned I believe you indicated you were familiar with the general principles of, for example, the basien analysis?

A. The general concept of -- actually you asked me a compounded question there, if I remember, of basien, jack knife or --

Q. Boot strap.

A. Boot strap. And my answer was, I have a vague conceptual understanding of the first two and some knowledge of the third.

Q. You don't have any disagreement with Dr. Woodworth that those are statistically valid means for compensating for a small sample size?

A. I don't have any basis for disagreement. It is not the type of statistics with which I am

particularly familiar.
Q. Now, Dr. Hanson recommends that you have a sample size of one thousand when you're developing these instruments; correct?
A. He has written that statement.
Q. And the RRASOR and STATIC-99 sample size is greater than a thousand for those; correct?
A. The developmental samples were, and replications have gone way beyond that, yes.
Q. The developmental sample for the MnSOST-R was two hundred fifty six --
A. Correct.
Q. -- is that correct? Now there was a cross validation done for the MnSOST-R; correct?
A. That is correct. If you're talking about the one that Dr. Epperson did, yes. I mean, there have been more than that, but yes.
Q. It's the one that Dr. Epperson did.
A. Yes.
Q. And the cross validation was done on a sample size of less than one hundred?
A. The initial process, yes. But there were more people added to it such that it became, I believe, close to -- some number around two hundred. I don't remember the number.

Q. Do you know if the State of California has accepted use of the MnSOST-R in predicting sex offender recidivism?
A. I'm presuming by your question when you say the State of California you're asking about whether the evaluators use that, the State- or court-appointed ones?
Q. Yes.
A. And the answer is no, they do not use it. If you're asking what the courts would accept, I have no idea.
Q. I was referring to the evaluators.
A. Okay. They are not at this point in time nor have they been, to my knowledge, using the MnSOST Revised in California.
Q. When you do evaluations -- and we're talking about evaluations for sex offender recidivism -- have you ever recommended that -- or concluded that a particular respondent is not likely to recidivate?
A. The terms you're using are difficult for me to respond to. When you're saying "is not likely," there have been various times when I have said the person does not meet criteria for commitment and quite specifically means that the person's

risk is below my understanding of the legal threshold. I can think of one case offhand where my testimony, versus my report, went into more detail where I remember saying specifically that this person was very likely to be -- paraphrasing, very likely to be violent, but I had little reason to believe there was much likelihood for sexual violence. But I don't believe I used the words that you were just using.

Q. Well, we don't really have an actuarial instrument or test which look at factors that could reduce a person's risk; correct? And I guess I'm talking about dynamic variables.

A. Of the three instruments we're talking about, the RRASOR and the STATIC clearly do not have that. The MnSOST Revised, there are items on there that score in the negative direction which means in the lower-than-average degree of recidivism risk direction, including two items that are related to treatment, that with treatment completion lower the person's assessed risk. Of other instruments there are -- such as the one we mentioned in one item, Dr. Wong's work with others, the Violence Risk Scale for

Sex Offenders, most of that scale is of dynamic nature. Twenty out of the thirty items. But that's not currently used within the sex offender civil commitment area.

The point you're making, however, is that ultimately that's one good reason why we need to look beyond the instruments currently.

Q. In fact, Dr. Hanson in his article, Will They Do It Again? Predicting Sex Offense Recidivism, he urges people to develop dynamic variables, actually start looking at things which may lead to a conclusion that this person is not likely to re-offend.

A. The concept is called a protective factor versus a risk factor, and he does say that, and I agree with that, though I would expand on what he was saying: That there's just as many reasons to believe that the dynamics factors that represent risk, not just protection. It would go in both directions.

Q. Let me show you what I've marked as respondents' Exhibit A. Is that an article that you have reviewed which is written by Dr. Hanson?

A. Yes, with some underlines that are not mine, but yes, I have read this.

MR. BAL: Respondent offers Exhibit A.

MR. THETFORD: Do you have a clean copy?

MR. BAL: I do not. That's actually the only copy I've ever gotten.

MR. THETFORD: I have absolutely no problems with admitting the article, I would just prefer that it not be an underlined copy. So if we can -- what I we can -- what I will agree to, Greg, is this: We'll agree to send this with her if you will mail a clean copy to her to substitute for the underlined copy.

MR. BAL: We'll do that. Thanks.

MR. THETFORD: Is that okay?

MR. BAL: Yes. No, that's fine. We'll do that.

BY MR. BAL:

Q. Now, you testified earlier about your opinion regarding what is the relevant scientific community, and interrelated in that is a concept of peer review. When we're talking about peer review I guess the term, "peer" -- is that referring to the relevant scientific community?

A. I would think it would be, yes. That people who would serve as the peer reviewers for journals, for instance, would be people who would have had

reason to be selected by the editors of those journals as knowledgeable in this area, therefore they're people who had reason, for whatever reason it's been, to have knowledge in the area. So I think it would be highly likely that peer reviewers would be within the same field, yes.

Q. And we're talking about journals -- for example Psychology Today, if that were to accept a publication on sex offender recidivism, then you wouldn't have any problem with the publication of that journal?

A. If you're asking me would I call that a peer review journal, the answer is no. It's simply a magazine. If you're asking me would it mean that I would automatically discredit the article, no, people can use a variety of places to publish things. I'm not sure what else you're asking me beyond those two possibilities.

Q. Well, there are journals which deal solely with forensic psychology or forensic psychiatry; correct?

A. Yes there are.

Q. There are also journals which deal in the general subject matter of psychology; and are

not?

A. Yes there are.

Q. Limited just to the forensics; correct?

A. Absolutely correct.

Q. And if one of the larger journals which is not limited just to forensics accepts an article for publication and then that article is commented upon by psychologists in general, would that be a relevant scientific community?

A. The community involves people, not a journal. So I need to know, in answer to your question, are you talking about the peer reviewers or the editor of a journal, or anyone who reads the journal? I would have different answers, depending.

Q. Well, let's talk about the people who review the application, the editors and the reviewers who make the decision to accept a journal (sic).

A. If we're talking about the editors, the editor of a -- of a journal, but more importantly if we're talking about the people to whom that editor has sent the article or manuscript for review, then it's very likely those people would have reason to have knowledge in this field and that's why they were selected for review. And

therefore the peer reviewers are very likely within the field.

If we're talking about the editor of a journal I would say, not necessarily the case. Because editors of journals, as you're accurately pointing out, can be dealing with journal material in a whole more general area and their personal specialties may be, in my analogy earlier, child custody, not dealing with sex offenders. So I would not say that all editors of journals would necessarily be within the same field of specialty. And certainly not all readers. I can read journals for a certain article when nothing else in the journal has any meaning to me.

Q. Now, the methodology -- the methodology underlying these actuarial instruments has to do with research, design, and statistics to a certain extent; correct?

A. To a certain extent, absolutely.

Q. A statistician can look at the methodology that's been used to develop these actuarials and provide feedback on whether it's valid statistics or not; correct?

A. Certainly a statistician can do that and provide

an opinion of -- to whatever degree the person thinks it's valid or not.

Q. And if the community of statisticians reviewed these various actuarial instruments and provided feedback, that would be a relevant scientific community; correct?

A. I believe they were already included in the description I had, so the answer would be yes. The description I had already stated was that people have reason to have knowledge in the field, in -- specific to this assessment issue. And so what you were just describing were statisticians who would be studying this area enough to be able to state something meaningfully from their own knowledge base. That would not include all statisticians; that would include some.

Q. Another class would be psychiatrists who would have an opinion on the use of these actuarials?

A. My statement about psychiatrists would be the same as psychologists. There would be some who would clearly be in the field and some who would clearly not.

Q. Earlier you mentioned that dissertation is a form of peer review.

A. In my opinion. It's gone through the process of actually, in a sense, superior review versus peer, at the time. But if we're talking "peer" in terms of a professional review process, the process of defending a dissertation quite typically involves an oral presentation to a committee of four or five, depending on the institution, where the person has to defend their work.

Q. If a person presents a dissertation and it's accepted and that person gets a Ph.D., that doesn't automatically mean that that subject of the dissertation is generally accepted in that relevant community, despite a fact of a dissertation being accepted.

A. I'm sorry, the what's not been accepted?

Q. Just the fact that a dissertation's been accepted does not automatically mean that it has become generally accepted in the scientific community; correct?

A. Oh, of course not. That would be true with any peer review. Just by something having been peer reviewed does not mean that it's generally accepted in the field. It just means that some set of people who are in that peer category and

presumed knowledgeable have accepted that piece of work for a specific purpose.

Q. Now Dr. Maskel, you're familiar with her work in the field of risk assessment?

A. I'm familiar with what she's testified about and I'm -- occasionally have been familiar with what she has assessed concerning an individual respondent. Otherwise, I am not familiar with work that she has done.

Q. Do you consider Dr. Maskel to be part of the relevant scientific community?

A. Yes.

Q. How about Amy Phenix? Would you consider her to be part of the relevant scientific community?

A. Yes.

Q. Doctor Steven Hart?

A. Yes.

Q. Doctor Randy Otto?

A. Yes.

Q. Doctor --

A. Of recent vintage. Approximately the past year, yes.

Q. Dr. Terrence Campbell?

A. Terrence Campbell? My hesitation is because his stated knowledge, both in his article in the

year 2000 and in his testimony, is frankly so incredibly flawed that it's hard for me to believe that he really has knowledge in the field. In concept, he's a member of the field. In terms of whether or not he's really studied the research, I have reason to suspect he has not. So I don't know the answer to that question.

Q. You don't agree with his conclusions regarding these actuarial tools; correct?

A. No. He has made various statements that are statistically just plainly inaccurate that are the fundamental statements that underlie his arguments so the arguments fall apart very quickly.

Q. But he is a psychologist; correct?

A. That's my understanding.

Q. And he is involved in the discussion of whether these actuarial instruments should or should not be used.

A. In the definition that I described earlier, of a person who has reason to have knowledge in this area, then he would fit that definition. And I was raising the question about him in terms of, what he has testified about and what he has

written about is so full of flaw, so full of error, that I have to wonder about that.

Q. But he is involved in the discussion; whether you agree with him or not; correct?

A. In that sense, that's correct.

Q. And you heard the testimony of Professor Woodworth --

A. I did on one --

Q. -- the statistician.

A. On one occasion, yes I did.

Q. And would you agree that he is also part of the relevant scientific community?

A. Yes I would. He has made it a point to learn some of the details about the instrumentation and about the process of risk assessment.

Q. In fact, when you were on the stand I believe you stated that you really didn't have that many areas of disagreement with Professor Woodworth.

A. That's correct. I found him to be pretty solidly based in statistical science. We had one disagreement and it's, in a sense, a minor point.

Q. In the State of Minnesota there is a cut off score on the MnSOST-R of thirteen and above which is considered presumptive for commitment?

A. That's, in effect, how they use it, yes. When they're doing an assessment within the Department of Corrections to screen for referral, people who have a score of thirteen or higher are basically automatically referred.
Q. And was Dr. Epperson involved in the development of those standards using MnSOST-R?
A. I don't know that I know that. I have presumed so, but I really don't know that. In fact, given that it's a policy issue, in effect, he may not have been involved. I don't know that.
Q. You testified earlier about an actual rate of re-offense. That the actual rate of re-offense may be higher than what the developers of these actuarial instruments used.
A. The comparison that you're talking about, I believe, is the comparison of actual sexual re-offending rates versus actual sexual re-conviction rates, and yes, there is reason to believe those may not be the same thing.
Q. When you're talking about re-offense versus re-conviction, you're talking about re-arrest?
A. No, re-arrest would be a third category and re-imprisonment would be a fourth. Basically re-imprisonment, re-conviction and re-arrest,

are all attempts at assessing the reality if we could really watch them all twenty-four hours a day just from a distance to see what they did, kind of re-offense rate. We, as a science, don't get to know about all of the re-offending and so we have ways of using other measurements to approximate that. Re-conviction is the most common. Re-arrest is also somewhat common, and re-imprisonment is actually relatively rare but is used.

Q. When you were talking about the actual rate of re-offense -- let's talk about re-offense -- you're not taking into account any possible rate of false convictions; are you?

A. When I'm talking about re-arrest, I'm talking about -- excuse me, re-offense, the concept I'm using is the reality of what really happens. So it's not about there being a -- someone falsely prosecuted or correctly prosecuted. All the prosecution process is separate from whether or not someone really did something or really didn't. The re-offense rates are an attempt to get at what really happens.

Q. But when you're talking about what really happened you talk in terms of adjusting the base

rate upwards; correct?

A. It is usually thought of that way as compared to re-conviction rates; that's correct.

Q. I mean, that's the rationale in one of your papers, at least, I believe the one with Epperson, in which you argue that the base rate should actually be higher.

A. I believe the publication you're talking about is my 1998 article, not with Dr. Epperson, but yes, I do talk about that re-conviction rates appear to under estimate re-offense rates. And so there would be an estimation that would go up higher.

Q. But the base line, whatever it ends up being, the base rate would not account for false conviction rate; would it?

A. If we're talking about re-offense rates -- and again, we're not talking about the process of getting caught or prosecuted or convicted, truly or falsely. That issue only comes into consideration when we're talking about re-conviction rates or re-arrest rates -- then that issue comes into play about whether someone accurately did something or not for which they were convicted, though there's one caveat each

to that. When we're counting, in research, the re-conviction rate the issue ultimately is not really, Did the person do the specific offense for which he was re-convicted? The issue is really, Did everyone who was re-convicted do another offense, that one or a different one? Because either way then they are a re-offender, accurately counted.

Q. The base line, for example in the MnSOST-R, is that based on re-conviction rate or re-arrest rate?

A. Re-arrest rate.

Q. How about for the STATIC-99? Is that re-conviction or re-arrest?

A. Re-conviction.

Q. And for the -- that STATIC-99 base rate, when you're looking at convictions you did not account for incidents in which the person was wrongfully convicted; did it?

A. There would be no way of knowing to what degree that was accurately true or not; whether someone was accurate or inaccurately convicted. Likewise, it would not take into consideration those people who were accounted as non-recidivists who actually did re-offend.

Both errors are not, in that sense, accounted for. That's what happens when you are approximating re-offense with some other thing such as re-conviction. You're adding some movement, some error in both directions. We would measure their true re-offense rate if we could really know it. If we knew how to do it.

Q. Did you read that in Today's paper, by the way? I forget what it's called in Madison. It was in my hotel. I don't know if you had a chance to read that or not.

A. I have not read today's paper so whatever you're referring to, I have no idea.

Q. Well, there was an article about a man in Virginia who was released after eight years in prison because DNA proved that he didn't commit a sexual offense.

A. And there are such cases. It proved he didn't do that offense. And I'm not saying he did any other. I'm saying it proved he didn't do that offense.

Q. Are you familiar with proportional hazards regression? It's a statistical term.

A. I've heard of the concept. I can't say I'm familiar with it.

Q. Have you ever attempted to replicate another person's research? By replicate I mean duplicate the results by using the same data?
A. In a sense that's what I did with Dr. Hanson's work, to a point, on those four studies that I mentioned, and then deleted one of those four.
Q. Another way to replicate someone else's research would be just do your own study; correct?
A. That's correct. In other words, with your own sample. That's correct.
Q. Now replication, would you agree, is a -- well, important way in the process for gaining acceptance in the scientific community?
A. Certainly.
Q. Are you aware of any studies which have attempted to replicate the STATIC-99 using independent sample? And I mean independent from used by Hanson and Thornton.
A. Yes. Again, the list of what I am aware of is in that same exhibit we've been describing -- number 5, I believe. Yes. All of those that are listed there under either the Inter-rater Reliability or -- in terms of replication of predictive validity, all those that are listed there under that category.

Q. And are you aware of any that have been published?
A. I'm looking at Exhibit 5, page six and seven. One has been submitted for publication, one is unpublished, one is the original work so that's not a replication. Original work by Hanson and Thornton. No. They're presentations or submitted for publication -- besides the original work by Hanson and Thornton.
Q. And what are the years listed for when those studies were done?
A. The year 2000.
Q. These are all fairly recent developments; correct?
A. Yes they are, which helps explain why they've been just at presentation level so far -- or submitted for publication. Publication often takes about two years so --
Q. So some of them are in the process of being accepted for publication?
A. Hopefully accepted. At least being reviewed currently, yes, and potentially accepted.
Q. Well, let me ask a few questions about inter-rater reliability. When you do an evaluation do you initial -- you do an initial

assessment based on a paper file?
A. That's where I start.
Q. Is that your final evaluation?
A. In some cases it can end up that way, but that is not the design. The design would be to include at least offering, if not obtaining, an interview; at least attempting to get interviews of other individuals with knowledge of the person that I'm assessing -- under most circumstances, not invariably; and of consulting with another person on whatever issues there remain in the case. In some cases the person turns down the interview, I do not get any collateral interviewing, though I can always manage to get a professional consultation.
Q. Now, you have in the past changed your opinion on an assessment, based on additional information which you may get in the case; correct?
A. That has occurred.
Q. And you've done that --
A. I changed opinion, if we're talking about going particularly from a position of, "I cannot offer a position" to something more clear. That's the most typical case in which that has occurred.

Q. Have you changed from "I cannot offer an opinion" to "this person is at a risk for recidivating"?
A. Yes. A risk that appears to be beyond the threshold as I know it in the law.
Q. And in the past have you done that on several occasions in the same case? Several, I mean more than two.
A. Have I changed my opinion in that regard? No. I've only changed in my bottom line opinion on one -- in one direction at one time. I have filed multiple addendum reports based on the -- what I consider ethical responsibility when either of the attorneys send me information that I supposedly didn't already have, or I come upon additional information of my own doing. I consider myself to have an ethical responsibility to informal parties so that there are no surprises from me in a future hearing. And so I file addendum reports quite regularly when I have additional information beyond what I originally assessed. But in terms of changing my opinion as the bottom line, does the person meet criterion or not in my opinion, I have never gone back and forth. I have gone from the

position of saying "does meet" to "I cannot tell," and I have gone from the position of "I cannot tell" to "does meet." Those are in different cases. I can only think of those two, frankly. One each.

Q. On the MnSOST-R --

A. I'll change that. Two and one. From a "does meet" to a "cannot tell," twice. I'm sorry. Go ahead.

Q. So these addendums that you may give out in a case may change the scores on the various actuarials?

A. Well, in theory that's true. In none of those cases that I was just describing was the change based on any actuarial information. It was based on clinical adjustment information.

MR. BAL: All right, I think that's all I have.

FURTHER EXAMINATION

BY MR. THETFORD:

Q. Dr. Doren, I just want to redirect you for just a second.

VIDEOGRAPHER: Excuse me. Can we go off the record for just a second?

(A recess was taken from 4:06 to 4:16 p.m.)

BY MR. THETFORD:

Q. Dr. Doren, I want to pose a hypothetical for you and see if you can work your way through it. Assume for me that the respondents in this case argue that the MnSOST-R and the STATIC-99 and the RRASOR are not valid to be used in Texas. And the reason that they're not valid to be used in Texas is that the STATIC-99 and the RRASOR are based upon population groups from Canada and the United Kingdom and the MnSOST-R is based upon a population group from the State of Minnesota, and that those population groups are different than the population of incarcerated male sex offenders in the State of Texas. How would you respond to that?

A. I would basically disagree. To a point. The issue to me is the amount of research done in general, in terms of replication work, and whether differences between that set of people who have been studied collectively are of relevance compared to the person or people in Texas. I'm going to talk about the individual, rather than Texas, as a single entity because

there are differences, of course, among people within Texas. So the respondent -- no matter where the respondent is from, the issue is not whether there are differences. There are always differences. Every individual ultimately can be defined in a unique way. The issue is whether or not the differences -- whether there's reason to believe that the differences are related to what the instruments are assessing.

Mentioned earlier, for instance, was the issue of race. To date there does not appear to be any differences among minority or majority set of people for the RRASOR or the STATIC. Not that there have been direct tests; I do not mean to suggest that. I mean in different jurisdictions where there are different types of demographics, it is replicating. Therefore, in that sense there does not appear to be differences.

The underlying research to the RRASOR, ultimately coming from the meta-analysis by Drs. Hanson and Bussiere, found that race did not seem to matter. For the MnSOST Revised the original work did not find race to matter. A replication by Dr. Epperson found some degree of

difference. If one were to take that in consideration you find that it matters by expanding in a sense -- using this loosely, expanding the confidence interval. By moving the interpretation, in other words, a little bit. One can take that into consideration. One doesn't have to ignore that piece of information.

If we're talking about a different characteristic, whether the person is pedophilic, I have every reason to believe that in a whole variety of the studies for these instruments there are also a substantial number of pedophiles, while not everyone was. So that would not be a differentiating characteristic.

If the person were female, I would immediately say the instruments don't apply -- at least we don't know them to apply. And so it would depend on what the characteristic is or characteristics are that differentiates that individual, how far I would go in taking the instruments to apply them or say right up front, they don't apply, and then how I interpret the information. If it's a mixed review I have to take that in consideration.

MR. THETFORD: I'll pass the witness.

MR. BAL: I'll ask a couple of follow up questions.

FURTHER EXAMINATION

BY MR. BAL:

Q. You talked about some differences between races in the MnSOST-R, or at least what Dr. Epperson found.

A. In two different studies. One study where he found differences, one study he did not.

Q. The study where he found differences, he actually found that minorities re-offend at a lower rate than non-minorities; correct?

A. Are you talking about the overall base rate, now, or per certain score categories? I don't know the answer to the first part of that. Actually, I'm not certain of the answer overall.

Q. Okay. If there are differences between minorities and non-minorities, isn't that something that should be looked at by the evaluator, as well as the developer of these actuarials?

A. To the extent that there is reason to believe

that any characteristic, including race, race just being an example, is of relevance to the specific instrument in its interpretation, then all evaluators should be looking at that issue when applying it to someone where that matters. So for instance, if I were applying the MnSOST Revised to a person of a minority race, then I would need to take that finding into consideration in my interpretation. On the other hand, if I were using that same characteristic for the RRASOR or the STATIC, then I don't know that this would have any applicable meaning, the issue of race.

If I were taking a different characteristic, someone's being homosexual, I don't have reason to believe that the simple fact someone is homosexual affects the out come of the instruments one way or another so that would not be a characteristic that would move my interpretation.

Q. The base rates for different types of sex offenders, for example rapists versus extra-familial, are those different depending on the type of sex offense you're talking about?

A. In the article that I published in 1998, I did

put together an analysis that resulted in the suggestion that extra-familial child molesters have higher lifetime re-offense rates than rapists of adult women. The research that's come out since, from Dr. Thornton, of the sixteen to nineteen year follow up of re-conviction rates indicates that if there's a difference at all, it may be in the other direction where rapists of adult women may have higher recidivism rates -- at least they did in that sample of higher re-conviction rates; that's just factual -- than extra-familial child molesters. And both those groups, by the way, are much higher than incest offenders. And it may very well be that we don't have enough data to draw a clear conclusion in that regard, and it may be that Dr. Kim English, E-N-G-L-I-S-H, has the proper interpretation of all of this. Her statement about this is that the categories of rapist versus child molesters are very misleading because a number of sex offenders actually cross over in the age of victimization -- age of victims that they have and so these are simply categories for what we've caught them for, but not necessarily of

what they do. And therefore there would be no difference because they are overlapping categories. I don't know the answer beyond what I just told you.

Q. So there may be differences or there may not be?

A. In the long term lifetime re-offense rates there may be differences and there may not be.

Q. And if there are differences then that's another error factor that you may have to account for.

A. I would not call it an error factor, but it is something that would need to be taken into consideration in -- to the extent that one is doing a clinical adjustment beyond the actuarials. The actuarials were developed with a certain type of measurement of sexual re-offending and ultimately certain base rate for that. Certain average rate for that occurring. When you expand beyond what they're measuring you're expanding the base rate. You're increasing it. That's where the question would come is, By how much should I do that? And that's where that question comes in and ultimately there is some degree of not -- not known about that. Unknown about that.

MR. BAL: Okay, that's all I have.

MR. THETFORD: That's it.

(At the hour of 4:26 p.m. the deposition was concluded.)

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Dennis Doren, Ph.D.

STATE OF)
) SS.
WISCONSIN)

BE IT KNOWN that the foregoing deposition was taken before me, KAREN BLAIR, a Notary Public in and for the State of Wisconsin; that the witness before testifying was duly sworn by me to testify to the whole truth; that the questions propounded to the witness and the answers of the witness thereto were taken down by me in shorthand and thereafter reduced to typewriting under my direction; that the transcript was presented to the witness to read and sign; that the foregoing 230 pages constitute a true and accurate transcript of all proceedings had upon the taking of said deposition, all done to the best of my skill and ability.

I FURTHER CERTIFY that I am in no way related to any of the parties hereto nor am I in any way interested in the outcome hereof.

DATED at Madison, Wisconsin, this 27th day of February, 2001.

_____
KAREN BLAIR, CSR, RPR
Court Reporter

My commission expires:
August 6, 2004