I would like to thank Professor Huffman for providing a little background on *Daubert v. Merrill Dow.* It is certainly an interesting decision which brought into question something known as the *Frye* rule. The *Frye* rule, established by *Frye v. United States,* provides that admissible expert testimony that is based upon scientific principles is admissible only if it is based on scientific studies that have gained general acceptance in the particular scientific field to which it belongs. Thus, under the *Frye* rule, expert testimony must be based on results that are published in the scientific literature, or the refereed literature.

The *Daubert* case commanded so much attention because it was widely believed that the Court might reexamine the *Frye* rule. Blackmun's opinion in *Daubert* began by concluding that *Frye* had been superseded by the Federal Rules of Evidence. The Court then laid down the law of admissibility of scientific evidence as an interpretation of Rule 702 of the Federal Rules of Evidence. Rather than the *Frye* rule requiring "general acceptance," the Court established a more flexible approach for trial courts to employ in deciding whether scientific evidence is admissible. The Court held that trial courts should make a case-by-case determination of the reliability and validity of offered scientific testimony in deciding its admissibility. In particular, the Court expressly held that publication in a refereed scientific journal is not the sine qua non of admissibility of scientific testimony. Further, general acceptance of a scientific theory is a factor in the determination of its admissibility, but it is not a necessary precondition.

*Daubert* is going to cause a great deal of consternation, I do not doubt, but there is a sound basis for the decision, and I think it stems from a more enlightened view of what, in fact, the refereed scientific
literature is. There is a general perception that the peer review process consists of a judgment of a submission by an independent panel of reviewers. In essence, that would be the case if this were a wonderful world. In fact, many of the major articles in *Science* and *Nature*, and even in the more specialized journals in my field (I am a climatologist), such as *Climatic Change*, are invited submissions, and the editor, I assure you, has a vested interest in invited submissions. Further, the reviewers for publications like *Science* and *Nature* are not blind, and a broad majority, in fact, send the name of the author around with the paper, which introduces some unavoidable personal elements into the science. This induces a lot of serious problems.

Now, I want to digress for a moment because the scientific literature as it stands establishes something that we know as a scientific paradigm. The original appellate opinion in *Daubert* stated: "The best test of certainty we have is good science—the science of publication, replication, and verification, the science of consensus and peer review." In other words, the appellate court felt that peer review and consensus define the best science. This would be true in a slowly changing or noncontentious field. But, unfortunately, most of the scientific intersections with the judicial system are by their very nature contentious and very possibly rapidly changing.

The scientific paradigms are overarching views that explain a majority of the data, but they are embedded in a philosophic milieu that we cannot avoid. I will give you an example of the terracentric versus the heliocentric solar system. The *Frye* rule would have kept Galileo's testimony out of court because it was not approved by a consensus of Jesuits. The paradigms are large, medium, and small. Paradigms—such as Newtonian physics, global warming, and global cooling (this last in 1975), stability versus diversity in agriculture, ecosystems, and genetic systems—change, but they do not change very easily. The reason is that scientists are human beings.

New interpretations, new models, and new paradigm shifts ideally would replace old ones as soon as anomalous results come to light. The time for the refereed literature—the definer of the paradigm—to self-correct errors would be brief and limited to the length of time it takes for reviewers to judge the new data. But scientists are people, too. When observations come along that do not agree with the scientist's belief system, scientists behave just like people. They tend, as a rule, to ignore such contrary new theories.

Thomas Kuhn, the great historian of science, wrote a book that everyone involved in this issue should read, *The Structure of Scientific
Revolutions, which was originally published in 1962 by the University of Chicago Press. In his book, Kuhn wrote: "In science novelty emerges only with difficulty manifested by resistance against a background provided by expectation. Initially only the anticipated and the usual are experienced even under circumstances where anomaly is later observed."¹⁰

Scientific paradigms do not replace one another because people become convinced. In fact, what Kuhn is saying is that it is because the older believers die with their paradigms on. (Laughter.)

He did not exactly say that; I said that. But that is what he really meant. Now, what has happened with the Daubert decision? Well, in fact, this opinion established that when faced with a proffer of expert scientific testimony, the trial court must decide that the testimony's underlying reasoning and methodology is scientifically valid and can be properly applied to the facts at issue.¹¹ To guide this determination, trial courts should consider the following factors: testability, or whether the theory or technique in question can be and has been tested;¹² whether the science has been subjected to peer review and publication;¹³ and whether the science has gained widespread acceptance.¹⁴

But as I noted earlier, whether research has been subject to peer review and publication is just one element of the trial court's inquiry, and publication is not a sine qua non for admissibility.¹⁵ The Court reasoned that publication does not necessarily correlate with reliability, and, in some instances, well-grounded but innovative ideas will not have been published in time for a trial.¹⁶ Further, some propositions are too new or too limited to be reviewed and approved by the scientific community at large. The Court held that widespread acceptance is important, but cannot be the determining factor.¹⁷

Let me finish off here with a brief digression on the political nature of science because this also underlies these issues. I would like to talk about my favorite issue, global warming. This issue has been around for ninety-eight years since Svante Arrhenius published a paper on it,¹⁸ but it has only been in the public venue since 1980, when

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¹¹ Daubert, 113 S. Ct. at 2796.
¹² Id. at 2796-97.
¹³ Id. at 2797.
¹⁴ Id.
¹⁵ Id.
¹⁶ Id.
¹⁷ Id.
politicians recognized the scope and nature of its potential to assist them in their cause. (Laughter.)

One can very easily define a scientific paradigm by supporting and feeding it. Then Senator Gore chaired the Senate’s Subcommittee on Science, Space and Technology, the overseers of the National Science Foundation and NASA. Having Gore in charge of such a subcommittee is guaranteed to define the paradigm. We cannot realistically expect the program managers to come in there and say: “Senator, we are not so sure this global warming is such a big deal even though it is going to elect you president if you play your cards right.” It is not going to happen. Politics greatly influences the scientific paradigms. When one proposes a theory that goes against powerful interests, there will be great resistance.

My friend, Robert Balling, from Arizona State University, is one of the most published climatologists in the world. He never had a problem publishing a paper, he told me, until he tried to publish one that said that the Earth was not warming up like it was supposed to. He could not believe the reviews. He said: “You would have thought I killed my mother.” (Laughter.)

I finish here by saying that it is apparent that on issues like global warming—which is not the only one where politics has intruded upon science—there will be a profound resistance in the scientific literature to counterparadigm publications, especially when large amounts of federal dollars have generated all the research that is going into that literature. It should be obvious that it must be the same wherever dollars, politics, and science collide, as with research on AIDS, cancer, nutrition, deforestation, and acid rain.

In summary, I think the Frye rule rested upon rather naive beliefs in scientific credibility, consensus peer review, and an apolitical nature of the scientific paradigm. These beliefs are simply not true. Admissibility rules have now been broadened to allow for innovation in the process of science. Perhaps admissibility rules should consider consistent publication in a second tier of literature, for example, the conference proceedings from our scientific societies. They are not specifically peer reviewed; the publication of the abstracts is peer reviewed. For what it’s worth, the Washington Post and the New York Times treat these proceedings as gospel.

At any rate, I think we have a problem with the intersection of big science and big government, which can enforce a scientific paradigm with more effectiveness and voracity than even a journal editor. I welcome the broadening of the admissibility rule for scientific evidence, and I look forward to its application. I think it is going to be especially interesting to see how this affects litigation regarding the
most important environmental issue of our time, the issue of global warming. I look forward to our ensuing discussion.

Thank you.